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Popularity shocks and political selection: The effects of anti-corruption audits on candidates' quality ☆

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Abstract

We show that the disclosure of information about a government's conduct affects the types of candidates who stand for election. Our empirical test focuses on Brazilian city council elections in 2004 and 2008. The identification strategy exploits the randomness of the timing of the release of audit reports on the (mis)use of federal funds by local governments. We observe that when the audit finds low levels of corruption (i.e., when it represents a positive popularity shock), the parties supporting the incumbent select less-educated candidates. On the contrary, parties pick, on average, more-educated candidates when the audit reveals a high level of corruption (i.e., when it represents a negative popularity shock). These effects are stronger in municipalities that have easier access to local media. Our evidence confirms that parties are strategic players: their decisions are affected by shocks that influence the electoral race.

JEL Classification: D70, D72, D73

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1. Introduction

Electoral accountability is a crucial mechanism that helps guarantee the sustainability of modern democracies. It allows sufficiently informed voters to assess the government's performance and hold politicians accountable for their actions (Barro, 1973; Mayhew, 1974). An extensive empirical literature shows that voters reduce their support of parties and officials involved in political scandals (Ashworth, 2012; Ferraz and Finan, 2008; Snyder and Hirano, 2012; Chong et al., 2015; Costas-Pérez et al., 2012) and reward politicians who are perceived to perform better (Bagues and Esteve-Volart, 2016).

In this paper, we follow up on these findings and account for the possibility that political parties might anticipate voters' punishment, or reward, and change the composition of the pool of candidates selected to run for office accordingly. Specifically, we show that the release of information about government corruption affects the quality of candidates of the incumbent coalition. Intuitively, one might expect that if a political party (or coalition of parties) supports a government that faces a negative popularity shock, it might react by selecting more appealing and competent candidates to compensate for the expected poor results in the following election. If there is a positive popularity shock, one might expect the party to behave strategically by reducing the share of costly, high-ability candidates given that the election will be less competitive.[1] Symmetrical intuitions could hold for the decisions made by the parties supporting opposition candidates. These ideas are closely related to a recent strand of literature showing that political parties are strategic players that take into account specific features of the electoral competition when making decisions (Galasso and Nannicini, 2011; Mattozzi and Merlo, 2015).[2]

We address this issue in the Brazilian context by looking at the effect of popularity shocks resulting from the disclosure of reports about potential misconduct in local

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[1] We follow recent studies by considering education level as a proxy for politicians' ability. See, for example, Ferraz and Finan (2009), Galasso and Nannicini (2011), Gagliarducci and Nannicini (2013), Besley et al. (2011) or Daniele and Geys (2015).

[2] In general, voters like high-ability politicians, but the lack of incentives might limit the number of *good* citizens who choose to enter politics (Caselli and Morelli, 2004; Messner and Polborn, 2004). Similarly, parties benefit from the selection of low-ability politicians, for example when low-ability candidates provide higher rents for the party or/and have a lower reservation wage (Besley, 2005; Dal Bó et al., 2006). However, parties might select better candidates when an election is more competitive in order to maximize their chances of winning (Galasso and Nannicini, 2011; Mattozzi and Merlo, 2015).

governments on the ability of candidates for city councilor in the 2004 and 2008 local elections. Hence, we relate the education level of candidates from the coalition supporting (or opposing) the incumbent government with the release of information about its (dis)honesty. This information was made available to voters through an anti-corruption program introduced in 2003 by the Brazilian central government that monitors how municipalities spend federal funds. The primary task is performed by auditors who examine municipalities' accounts to verify the correct use of federal transfers.

Municipalities are randomly selected into the anti-corruption program, and the timing of the release of the report is also random. These characteristics are crucial for our identification strategy, as they help to assess the multi-faceted endogeneity issue of linking government conduct and the general quality of local politicians. Another important aspect for our purposes is that the process of selecting candidates for the city council has a similar schedule in all Brazilian municipalities. This makes it possible to define when, during the term, the selection process is taking place. Further, although city councilors are elected using an open-list proportional system, parties play a central role in selecting candidates. In practice, a citizen is eligible to stand for election if he has been affiliated with the supporting party for at least 12 months before election day. Formally, the candidate list is decided during party conventions that, by law, are held in June of each election year. This list must be submitted at least 3 months before election day.

We exploit the randomness of the timing of corruption disclosures and, conditioning on the level of corruption, compare the ability of the pool of candidates in municipalities where the audits were released during the candidate *selection period* (i.e., from 12 to 3 months before the election) with that of municipalities in which the audits were released either before or after the selection period.[3]

Our findings, which are based on 1,321 municipalities that were audited in the period 2003–2010, show that when reports are disclosed during the political selection period, they lead to a significant change in the average ability of the candidates running for the city council who belong to the party (or coalition) of the incumbent mayor.[4] This effect crucially depends on the level of corruption reported. Indeed, the spread of information

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[3]It is worth noting that our identification strategy closely follows the one used in Ferraz and Finan (2008), however, given the different research question, our definition of treatment and the way we split the sample between treated and control municipalities are rather different.

[4]We reach a similar conclusion when studying mayoral candidates, though with less statistical power.

about government conduct can provide either a positive or negative shock to the expected electoral results, depending on citizens' prior beliefs about the quality of the government (Besley, 2007). On the one hand, we find that there is a decrease in the candidates' average education of nearly 4.5 months of schooling when low levels of corruption are reported (i.e., lower than the median). On the other hand, we find an increase of nearly 4 months of schooling when substantial corruption is reported (i.e., higher than the median). In other words, there is a difference of 8.5 months of schooling depending on the results of the audit, which corresponds to about 30–35% of one standard deviation. These symmetric results are not surprising in a context where trust in politics is very low, and releasing information about the absence of corruption could be more unexpected than the opposite.[5] The effect is of a similar magnitude when considering the sample of freshmen candidates or when we look at the median level of education. Interestingly, the effect is larger when we constrain our analysis to municipalities that have easier access to information due to the existence of local radio stations. However, we do not find any change in other observable characteristics such as age and gender. Moreover, we show that the closer the release of the audit report is to the election, the higher the ability of the candidates selected, regardless of the level of corruption detected. Finally, we find that changing the composition of the candidate pool does not have a significant effect on electoral outcomes. In fact, voters still punish corrupt parties, in particular where local radio stations are available, and the elected candidates do not appear to have different characteristics compared to those elected in municipalities where the report was released outside the selection period.

To our knowledge, we are the first to estimate the effect of popularity shocks about the incumbent government on the quality of political candidates. Our finding is closely related to the recent literature that emphasizes that the characteristics of the electoral race affect candidate selection. This could be because of a change in either individuals' incentives to enter politics or parties' incentives to select particular types of candidates (Caselli and Morelli, 2004; Galasso and Nannicini, 2011). Our results are more in line with studies that emphasize the role of the *demand* for politicians (i.e., party selection) than those that focus on the *supply* of politicians (i.e., self-selection of individuals).

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[5]Brazilians have very low confidence in political parties. More than 85% of the individuals interviewed in the 6th wave of the World Values Survey (2010–2014) responded that they have little or no confidence at all in political parties.

For instance, Galasso and Nannicini (2011) focus on the role of parties in the selection of candidates. Studying the Italian parliamentarian elections, they show that better candidates (i.e., those with more years of schooling) are selected in districts where the electoral race is expected to be more competitive. Mattozzi and Merlo (2015) further show that the incentives to select high-ability candidates crucially depend on the electoral system. Specifically, high-ability candidates are less likely to be chosen in proportional than in majoritarian systems, as the latter are characterized by a higher level of electoral competitiveness. Likewise, Esteve-Volart and Bagues (2012), studying Senate and House elections in Spain, find that parties tend to select female candidates where they are less likely to be elected.

Further, we complement the literature on political scandals. For instance, Ferraz and Finan (2008) focus on random audits in Brazilian municipalities to show that publicly exposed corrupt incumbents are less likely to be re-elected in the next election. In addition, Snyder and Hirano (2012) show that in US House elections, incumbents involved in scandals have a higher probability of losing their primary elections, and receive less votes in general elections compared to non-scandal incumbents. Chong et al. (2015) show that voters punish both incumbents and challengers after receiving information about the inefficient use of public funds in Mexico.

Our results also contribute to the recent literature on the consequences of Brazil's randomized auditing policy. While Litschig and Zamboni (2013) show that increasing the audit risk reduces rent extraction in public procurement, Ferraz and Finan (2008), Brollo et al. (2013) and Muço (2016) confirm that the release of the audit reports indeed has an effect on electoral outcomes.[6] Interestingly, Brollo et al. (2013) hint that auditing policy affects political selection: they show that the disclosure of the reports affects not only the mayor's likelihood of re-election but also the probability that he or she will run for re-election. Muço (2016) finds that municipal audits influence federal elections. Voters also reduce their support for the party of the incumbent mayor when voting in presidential elections. This result emphasizes that party labels matter in Brazil, despite being recognized as a weak party system.[7]

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[6]These results are relevant to our study, as they provide additional support to the critical assumption that auditing disclosure affects voters' decisions.

[7]Samuels and Zucco (2014) reach a similar conclusion as they find Brazilians conform their opinions on public policy to those of the parties they support.

The paper is organized as follows. Section 2 describes the institutional setting. In Sections 3 and 4, we present the data and estimation strategy, respectively. The main results and additional tests are reported in Sections 5 and 6. We conclude in Section 7.

2. Institutional setting

2.1. Local institutions

Brazil is a federation with three levels of government. In addition to the federal government, there are 26 states and 5,565 municipalities. Citizens choose the executive and the legislative branches of each jurisdiction through direct elections. Local governments have a central role in the provision of a variety of public goods (e.g., primary education, culture, health care, housing, transportation and municipal infrastructure). Transfers from upper levels of government cover a significant proportion of these expenditures.

At the municipal level, the mayor (*Prefeito*) has executive power while the city council (*Câmara de Vereadores*) exercises legislative power. The number of seats for councilors in each city council is based on population size.[8] Mayors and city councilors are both elected for a term of 4 years, but while mayors are limited to two terms, city councilors have no term limits. The mayor plays a central role in defining the expenditure programs, and the city council is responsible for enacting municipality laws and overseeing the mayor's usage of public resources. Indeed, councilors have influence over the allocation of funds, for instance by proposing petitions, amendments and voting on the municipal budget proposal.

2.2. Electoral rules and the candidate selection process

In municipal elections, voters are provided with a list of candidates running for mayor, at most one for each party (or coalition of parties), and a list of candidates for city councilor indicating which party they support.[9] Voters can cast one vote for mayor and one vote for councilor. Alternatively, voters can select a party in the city council election without specifying a candidate. While mayors are elected with a majority of

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[8]Local laws define the exact number of seats, but they have to follow federal laws that set the limit of seats according to the population in the municipality (Art. 29 of 1988 Brazilian Federal Constitution.). The number of seats ranges from a minimum of 9 in municipalities with less than fifteen thousand inhabitants, and a maximum of 55 in those with more than eight million people.

[9]Municipal elections are held in different years than federal and state elections.

votes, councilor elections use an open-list proportional system, and the distribution of seats follows the d'Hondt method. This means that the number of seats assigned to a party in the city council depends on how many votes all candidates from that party received. However, for each party, only the candidates with the most votes will become city councilors.

Importantly for our study, political parties play a significant role in selecting candidates to run in local elections. Only parties that have been registered with the Tribunal Superior Eleitoral (TSE) within one year of the elections can present candidates. Almost 30 different parties presented candidates in the 2004 and 2008 elections. However, the five biggest parties accounted for more than 65% of all mayors elected in the 2004 and 2008 elections. A similar time constraint applies to citizens who want to register to run in the mayoral or city councilor elections. They need to have their voting residency in the district they would like to represent and to be affiliated with their supporting political party for at least one year before the election. The electoral law requires parties to nominate candidates during local conventions, but it does not define how these conventions need to be organized or how candidates should be selected. Although conventions are usually open to all party affiliates, party activists who belong to the municipal directorate and professional politicians have a strong influence on the selection process (Mainwaring, 1999).

In the 2004 and 2008 municipal elections[10], parties had to choose their candidates and coalitions in a party caucus from June 10–30 of the election year. Final candidate lists had to be registered with the TSE before July 5.[11] The maximum number of candidates a coalition can put forward is twice the total number of seats it holds in the city council.[12] The parties must put forward a minimum of 30% female candidates.

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[10]These elections were held on October 3, 2004 and October 5, 2008

[11]Changes to the electoral law (Law No. 9504, of 30 September 1997) in 2015 affected some of the elements we exploit in our analysis. Beginning with the 2016 municipal elections: (1) party conventions are required to take place from July 20 to August 5 in election years; (2) candidate lists must be submitted by August 14 and (3) candidates must be affiliated with a political party for at least 6 months before election day.

[12]If a party is not in a coalition, the number of candidates cannot be more than 150% of the total number of seats it holds in the city council. Further, if not all candidates are selected during the party convention, party leaders may fill the remaining vacancies within 60 days of the election.

2.3. The Brazilian anti-corruption program

In 2003, the Brazilian national government, led by Luís Inácio Lula da Silva, established an innovative anti-corruption program to improve the transparency of public spending and to tackle corruption in local governments. The Controladoria Geral da União (CGU), a federal agency, was made responsible for auditing local spending that has been funded with federal transfers.[13] Importantly for our analysis, municipalities are randomly selected to the auditing process. Lotteries are held every two or three months in the Caixa Econômica Federal, in Brasília, in the presence of the media and members of civil society. Only municipalities with more than 500,000 inhabitants are exempt from the lottery. Further, since lotteries are run independently for each state, the probability of being selected for an audit in a given year varies by state. The first lottery selected 26 municipalities. From the second lottery to the eighth, 50 municipalities were selected each time; 60 municipalities have been chosen since the ninth lottery.

For each selected municipality the CGU compiles a list of all federal transfers received since 2001. Typically, 10 to 15 auditors spend around two weeks in the municipal offices searching for potential anomalies. Once the auditors have completed the inspections, they prepare a report listing any irregularities and malpractices. The report is then sent to competent authorities for prosecution and made publicly available on the CGU website about 3 months later. As shown by Ferraz and Finan (2008), the results of the audits generally receive media attention, especially at the local level.

3. Data

To estimate the effect of corruption disclosure on the quality of the pool of candidates, we collect information about Brazilian municipalities from a variety of sources for the period 2001–2010, which covers two full municipal terms (i.e., 2001–2004 and 2005–2008).[14]

Information about municipal-level corruption is taken from Brollo et al. (2013). Their data contain different measures of corruption for all 1,481 municipalities selected in the first 29 lotteries of the anti-corruption program (i.e., audits disclosed from July 2003 to March 2010). We use a broad definition of corruption that includes also irregularities that could be interpreted as government mismanagement rather than true corruption

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[13]CGU Decree No. 247, June 20, 2003.
[14]Table 1 reports summary statistics of the variables described below.

events.[15] This definition includes illegal procurement practices and the diversion of funds – not justifiable payments.[16] To account for the different levels of corruption across municipalities, our analysis uses the variable *Corruption*, which represents the amount of misappropriated funds as a proportion of the total amount audited.[17] This information is available for only 1,422 municipalities, as it was not possible to compute the amount of resources involved in some irregularities. The level of observation is the municipality-term, meaning that we can clearly identify the term during which electoral irregularities took place. In other words, if a municipality was selected in a lottery in 2007, it will likely appear in the sample twice, as the auditors checked spending that occurred in both the 2001–2004 term and the first part of the 2005–2008 term.

We use candidates' level of education as a proxy for quality. Specifically, we consider the minimum number of years necessary to attain a certain degree.[18] Alternatively, we use the share of candidates that completed *Mandatory School*. The TSE provides data on the level of education of candidates for the city council. For each municipality, we distinguish between the *Average Education* level of candidates in the incumbent's and the challengers' coalitions. The incumbent's coalition includes candidates from all the parties that run in the same coalition as the incumbent mayor's party and who belonged to the winning coalition in the previous municipal election.[19] The challengers' coalition includes candidates running for all other parties. Further, we compute the same variable considering *new* candidates (i.e., those who were not city councilors in the previous term). We also take into account the *Median Education* level of candidates and two other characteristics relevant for political selection – the *Share of Female* candidates

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[15]Therefore, in our analysis we do not distinguish between active and passive waste (Bandiera et al., 2009), as we rely on the fact that both types of misbehaviours might be salient for electoral accountability.

[16]The dataset and full details on how the measure is constructed are available at `https://sites.google.com/site/fernandabrollo/home/data`.

[17]In the original dataset this variable is called *Broad Fraction of the Amount*. Note that we do not consider a binary measure of corruption as in the Brazilian context, the presence of corruption might not be informative per se, conversely we exploit the severity of the phenomenon. See Section 1 and the footnote 5.

[18]For candidates who started a degree but eventually dropped out, we assign half the number of years that would be needed to complete it.

[19]We also show in Appendix Table A.3 our main results by defining the incumbent coalition as composed of all parties that belonged to the winning coalition in the previous municipal election.

and *Average Age*. We also construct the two latter variables and the *Average Education* level for the sample of elected candidates.

We compute similar measures for mayoral candidates, but focus only on candidates belonging to the same party as the incumbent. Finally, we consider the *Share of Seats Won* by candidates elected from the incumbent's coalition.

We control for local political preferences by creating a dummy variable that equals 1 if the incumbent government was led by a mayor from Partido dos Trabalhadores, and 0 otherwise. This variable also controls for whether the municipalities is ruled by the same party in power at the federal government. The 2000 Brazilian Census provides data about the *Population* of the municipality, monthly per capita *Income*, *Share of Population Employed*, the *Gini Coefficient* of income, the *Share of population with a Secondary Degree*, the *Share of Population in Urban* areas, the share of population working in different job sectors (*Services, Transport, Public and Commerce*). We also construct a dummy variable *Media*, which accounts for the presence of local radio stations in the municipal area. This information is taken from the 2006 municipality survey *Perfil dos Municípios Brasileiros: Cultura*.

To provide homogeneous results in all our estimates, we apply a sequence of restrictions to the original dataset on corruption provided by Brollo et al. (2013). First, we remove eight municipalities, as they did not yet exist in 2000 when the population census was conducted. Second, we consider only municipalities in which at least some parties support the incumbent's coalition. Therefore, we remove 88 more municipalities. Finally, we exclude five municipalities in which there were no new candidates for city councilor (i.e., they were all incumbents). Clearly, by applying these restrictions the sample of municipalities we use in our analysis is no longer random. We address this issue in Section 6.

4. Estimation strategy

The main objective of this study is to test whether information shocks that change citizens' voting behavior affect the quality of electoral candidates selected by parties. Specifically, we want to compare (1) the ability of candidates running for city council when the local government experiences an informative shock during the party selection period to (2) the ability of candidates running for city council in local governments that experience a similar informative shock at other points in time. In order to provide a

reliable counterfactual analysis, we exploit the randomness of the timing of the disclosure of the audit reports to determine the group of *Treated* vs. *Control* municipalities.

To precisely determine the *selection period* (and hence the composition) of the two different groups, we identify two important dates in the electoral process. First, we account for the deadline for parties to provide their candidate lists (i.e., July 5, 2004 for the elections held in October 2004 and July 5, 2008 for the October 2008 elections). Second, we consider the cutoff date for citizens to be affiliated with a party in order to run as a candidate for that party (i.e., October 3, 2003 for the October 2004 elections and October 5, 2007 for the elections held in October 2008). Thus, a municipality is considered to be *treated* if the disclosure of the report occurred between 12 to 3 months before election day. We think our definition makes clear that the focus on parties' role in the selection process as the group of citizens willing to run for public office is mostly predetermined by the time the reports are disclosed. Indeed, depending on the lottery results, the communication of the audit reports might occur during, before or after what we identify as the treatment period. It is worth noting that our strategy closely follows the one used in Ferraz and Finan (2008). However, as we are exploring a distinct research question, our definition of *treatment* and the way we split the sample between treated and control municipalities are rather different. Indeed, Ferraz and Finan (2008) define *treated* municipalities as those that received an auditing disclosure anytime before the election.

Another aspect to take into account is that auditing reports reveal a misuse of public funds that could cover both terms we include in our analysis. This could be a problem if it were not possible to determine the exact timing of the misbehavior. In this were the case, citizens would be limited in their judgment as it would be less clear who was accountable for the discoveries made during the audit. Luckily, the corruption data we use categorize the misuse of public funds by term and municipality, which allows us to identify the treated and control groups for each term as reported in Figure 1.

Table 2 summarizes the sample composition, differentiating between treated and control municipalities depending on the electoral term. Over the two terms, we have 1,695 observations: 327 are treated (19.3% of the sample) and 1,368 are controls (80.7% of the sample). We have 182 treated and 960 control municipalities for the 2001–2004 term. Here, the treated municipalities are the ones drawn from the four lotteries disclosed between October 2003 and April 2004, while the control group includes all municipalities selected in the other 25 lotteries that had audit reports released in the 2001–2004 term.

For the 2005–2008 term we have 145 treated and 408 control municipalities. The treated municipalities were drawn from the three lotteries disclosed between January 2008 and June 2008, while the control group comprises all municipalities selected from the six lotteries disclosed between February 2006 and July 2007 plus the four lotteries disclosed between December 2008 and March 2010 that were audited for federal funds released in the 2005–2008 term.[20] Finally, 374 municipalities appear in the sample twice, as they are part of the control group for the first term and part of either the control or treated group in the second term.[21]

Table 3 reports the summary statistics for the two groups of municipalities. Indeed, although our sample is a sub-sample of those selected within the randomized lotteries, it seems well balanced. Only the share of the population working in the industrial sector is different between the two groups at the 5% significance level. Note that including this variable in our specifications does not affect our findings.

Formally, we start the analysis by estimating the following ordinary least squares (OLS) model:

$$Y_{ist} = \beta \, T_{ist} + \delta \mathbf{X}_i + \gamma_s + \lambda_t + \epsilon_{ist}, \tag{1}$$

where i denotes the municipality, s the state and t the term. Y_{ist} can be the *Average Year of Schooling* of either all or freshmen candidates for city councilor, from either the incumbent or the challenger coalition. T_{ist} is a dummy taking a value of 1 for municipalities with audit reports released during the selection period (i.e., from 12 to 3 months before the election), while $\mathbf{X}_i$ is a set of time-invariant municipal controls. Finally, γ_s are state-fixed effects, λ_t are term-fixed effects and ϵ_{is} is the error term. We use an OLS model with standard errors clustered at the municipality level.

Thanks to the random assignment of the auditing among municipalities, the coefficient β is the causal parameter of interest. In other words, it represents the average effect of the release of the auditing reports on candidates' education levels. State-fixed

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[20]The reports disclosed before February 2006 did not account for funds released in the 2005–2008 term.

[21]Municipalities that were audited more than once appear in our sample only with reference to the first draw. In this way, we avoid the possibility that the potential long-term effects of the audit would bias our estimates (Avis et al., 2016).

effects are included in all specifications, since the random assignment was stratified at the state level. Therefore, we ensure that our identification accounts for the heterogeneous probability of selection on the treatment faced by municipalities from different states. In addition, term-fixed effects account for other unobservable characteristics that might have changed from one term to the next. We include municipal controls in order to provide more precise estimates in case the randomization still produces a selection of treated and controlled municipalities with unbalanced characteristics.

We expect the estimates from Equation (1) to produce significant results if the auditing disclosure *per se* matters in the candidate selection process, regardless of the actual information provided. However, this would only be the case if the information disclosed differs from what the voters or parties expect (Ferraz and Finan, 2008) – i.e., if there is a systematic under- or over-estimation of a municipality's level of corruption.

Therefore, we refine our baseline analysis by taking into account the results of the auditing process. We produce different estimations to test whether the effect of the disclosure on the selection of candidates depends on the kind of information reported. Specifically, we estimate the following equation:

$$Y_{ist} = \beta_1 \ T_{ist} \times \ Corruption_{ist} + \beta_2 \ T_{ist} + \beta_3 \ Corruption_{ist} + \delta \mathbf{X}_i + \gamma_s + \lambda_t + \epsilon_{ist}, \quad (2)$$

where $Corruption_{ist}$ is the share of corrupted resources. Moreover, to provide more flexible estimations and to account for potential non-linearity of the effects, we interact T with a variable identifying municipalities that belong to different quartiles of the distribution of corruption. In other words, we compare the effect of the disclosure of different levels of corruption on the level of education of the pool of candidates. This is a crucial point because, depending on the level of corruption revealed, the information shock could send either a positive or negative message to citizens. However, using this approach could raise important issues if our measure of corruption simply serves as a proxy for other municipal conditions. We check for this possibility in Section 6.

5. Results

5.1. Baseline analysis

In this section, we report our central results. Specifically, Columns (1) and (2) of Table 4 show the results from the estimates of Equation (1), in which the dependent

variable measures the level of education of all candidates in the incumbent's coalition. As expected, the disclosure of reports *per se* has no direct effect on candidates' quality. Moreover, the inclusion of municipal covariates does not seem to have a sizable impact on the main coefficient, which is in line with a balanced sample thanks to the randomization of the treatment. The remaining columns of Table 4 report estimates of Equation (2). Columns (3) to (5) report the effect of the auditing, interacted with the level of corruption, on the education of all candidates selected by the incumbent coalition's parties. Column (3) provides the first indication that reporting high corruption boosts the quality of candidates put forward by the incumbent's party. The interaction term has a positive sign, but it is not statistically significantly different from 0. Column (4) reports the estimate where the treatment status variable interacts with a dummy that identifies whether a municipality is in the top 50th percentile of the distribution of our measure of corruption.[22] This result, coherently with Column (3), suggests that when the audit report is disclosed during the selection period, in municipalities with low levels of corruption candidates from the incumbent's coalition have around 4.5 months less of schooling (coeff. -0.378). However, if high levels of corruption are exposed, we observe an increase in the average education of slightly more than 4 months of schooling (coeff. 0.724-0.378 = 0.346). Both coefficients are statistically significant. Therefore, there are more than 8.5 months of education difference depending on the signal provided by the audit. This difference corresponds to about 30–35% of one standard deviation. The coefficients in Column (5) indicate the quartile of corruption and emphasize that

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[22]The level of corruption in the median municipality was 0.5%.

the effect is stronger with higher levels of reported corruption.[23] The last three columns suggest similar results once the focus is only on new candidates (i.e., those who were not previously on the city council). This is a crucial finding, as using new candidates rules out the possibility that the estimates rely on a mechanical effect coming from the direct consequences of the audit (e.g., if the audit led to the incarceration of involved councilors who were mostly low-ability individuals).[24]

Table 5 shows that the challenging coalition, which is not directly accountable for the outcome of the audit, does not change the education levels of its pool of candidates: the coefficients of interest are not statistically significant in any of the regressions.[25]

Thus, our main result points to the effect of information on shaping the selection of political candidates as parties do react to the expected electoral shocks. On the one hand, they increase the quality of candidates in municipalities where elections become more difficult (i.e., where a severe report has been released). This is in line with previous research showing that parties select better candidates when they need them, namely during more competitive elections (Galasso and Nannicini, 2011; Mattozzi and Merlo, 2015). This intuition is supported by other studies showing that educated individuals are more likely to be elected (Dal Bó et al., 2016). Within our sample, individual candidates' level of education is also strongly positively correlated with the number of

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[23]The results are similar if we only use as control those municipalities that have the auditing disclosure after the selection period. Appendix Table A.2 includes an alternative measure of schooling as a dependent variable – the share of candidates that completed *Mandatory School*. Our findings are confirmed. Specifically, the share of candidates that completed mandatory school decreases by 4.3 percentage points where low levels of corruption are found, and increases by about 3.5 percentage points with levels of corruption above the median. Additionally, Appendix Table A.3 provides the results of changing the definition of the incumbent's coalition to include all parties that supported the mayor in the previous election, regardless of whether they do so in the following election. The results are similar, though the positive effect on the education of candidates running for office in highly corrupt municipalities is smaller than the one found with our standard definition of incumbent's coalition. This does not change if we consider the extended sample which includes all the 1396 municipalities where there is at least one candidate representing the old incumbent's coalition. Finally, in Appendix Table A.1, we show that our results are mainly driven by the candidates' selection for the 2004 elections.

[24]Moreover, there is no supportive evidence of the fact that the audits leaded to an increase in the incarceration of local politicians.

[25]This may be because the challenging party expects the incumbent to react to the shock, and therefore has no incentive to change its own pool of candidates.

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votes they receive (and with their probability of being elected).[26] On the other hand, elections become less risky when the local government experiences a positive shock, such as reporting no or little corruption. In this case, parties might decide to reduce the number of high-ability individuals if they are *costly*. For example, this is possible if the party shares rents with selected candidates, and this rent is higher the lower the public motivation (Besley, 2007), or if the party has to supplement candidates' salaries, as high-ability individuals have a higher reservation wage (Mattozzi and Merlo, 2008).

Although we believe our results can likely be explained by parties' strategic behaviors, we cannot rule out the possibility that they are also influenced by changes in the *supply* of politicians (i.e., the pool of individuals willing to run for office). This potential effect is already partially reduced because our treated municipalities cannot select individuals who are external to the party. In other words, the sample of individuals from which candidates can be chosen is pre-determined with respect to the treatment. Still, even within this sample of individuals, an effect could be expected. However, in principle, citizen–candidate models would predict results that are opposite to our findings (Caselli and Morelli, 2004; Osborne and Slivinski, 1996). For instance, one might expect high-ability individuals to have even lower incentives to enter politics after a political scandal. Similarly, it is hard to explain why high-ability candidates would refuse to stand for election in a municipality that appears to have a functioning administration where it would be easier to be elected. Therefore, the effect of the disclosure of the audit report on individuals' willingness to enter politics will, if anything, adjust the size of our coefficients downward.

Below, we propose additional analyses that complement the previous findings. We only report results that focus on parties that support the incumbent, as we have already shown the absence of effects for the challengers.

5.2. Additional analysis

5.2.1. Alternative dependent variables

We first check whether the effect of the disclosure on candidates' average level of education was driven by a general increase (or decrease) in the quality of the pool of

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[26]This correlation holds when controlling for other individual characteristics, as well as city-fixed effects. On average, an additional year of education significantly increases by 8% the number of votes received by a candidate. Results are available upon request.

candidates or whether it came from the selection of a few very good (or bad) candidates. Therefore, in Columns (1) and (5) of Table 6 we estimate Equation (2) by using the median level of education of candidates for city councilor as a dependent variable. The estimates reveal that the median level of education is also significantly affected, in the same direction as in the main analysis. In particular, the disclosure of a positive report decreases the level of median education by 5 months (coeff -0.431), while it increases by 3 months (coeff 0.690-0.431=0.259) when the report is negative. The point estimates are similar when looking at freshmen candidates. This is consistent with a general change in the composition of the pool of candidates.

Second, we replicate the principal analysis looking at the education of mayoral candidates. Therefore, the regression reported in Column (2) of Table 6 considers only municipalities in which the party of the incumbent mayor decided to present a candidate (either the incumbent mayor or a new candidate) in the next election. In Column (6) we focus on the sub-sample of new candidates. The main coefficients are not statistically significantly different from 0, but their direction is consistent with the results for candidates for city councilors.[27] The insignificant effect could be driven by two characteristics of the mayoral race that make the statistical test weaker. First, there is low variability in the pool of candidates from one term to the next, as many mayors can run for re-election. This is not an issue if we look at the results in Column (6), which pertain to new candidates. Indeed, the effects are larger than those reported in Column (2) for incumbent mayors. The second characteristic that could be driving the insignificant effect is the limited variability in the level of education of mayoral candidates: they are usually highly educated, particularly compared to the general population and city councilors (see Table 1).

Finally, we explore the possibility that the disclosure might also affect other candidate characteristics – namely age and gender, which have been analyzed in previous studies on political selection (Esteve-Volart and Bagues, 2012; Daniele and Geys, 2015; Paola et al., 2010). We consider the average age of candidates and the share of female candidates as dependent variables. Our findings, reported in Columns (3 and 7) and (4 and 8) of Table 6, do not highlight any substantial change concerning these characteristics.

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[27]Recall that for mayor, instead of looking at the coalition, we focus on party affiliation.

5.2.2. Timing of the disclosure

In this section, we test the heterogeneity of the effect of the disclosure depending on when it occurred to determine whether the effect was mainly due to municipalities in which the audit report was released close to the end of the selection process. However, it could also be that audits released earlier have the strongest effect because the incumbent would have longer to change the selection of candidates.

For this analysis we estimate the following regression separately for the sub-samples of municipalities with high and low corruption:

$$Y_{ist} = \sum_{\tau=1}^{\tau=3} \beta_\tau \, T_{ist} \times 1(t = \tau) + \delta \mathbf{X}_i + \gamma_s + \lambda_t + \epsilon_{ist}, \tag{3}$$

where T_{ist} interacts with a set of dummies for each trimester of the treatment period. We report the results of these estimates in Figure 2. In the top panel we display point estimates and (95%) confidence intervals from a regression limited to the sample of municipalities in which audit reports revealed high levels of corruption, while in the bottom panel the analysis is constrained to municipalities with low levels of corruption reported in their audit. While all coefficients are borderline insignificant, we can draw two interesting implications. First, the difference between the point estimates from the two groups of municipalities remains relatively stable over time. Second, the level of education of the selected candidates is higher the closer to the election the report is disclosed. This is true regardless of the report's severity. Thus, for instance, auditing disclosures that took place from 3 months to 1 month before the candidate list deadline had no effect on education when they revealed low levels of corruption, but had a large positive effect in municipalities with high levels of corruption.

5.2.3. Electoral results and local media

The mass media are the main channel through which citizens are informed about politicians' behavior. Indeed, the effect of a popularity shock on the electoral results varies depending on the availability and accessibility of sources of information (Ferraz and Finan, 2008; Costas-Pérez et al., 2012). If the results shown so far come from parties that act in anticipation of the potential impact of the audit report on the electoral results, we should also find that parties' reactions depend on the presence of local media. Therefore, we expect audit reports to have a greater effect on the quality of candidates where citizens have easier access to information. To test this hypothesis, we follow Ferraz and Finan

(2008) and account for the presence of local radio stations to characterize the different degrees of media penetration across municipalities. Therefore, we provide estimates by separately considering (1) municipalities that have at least one local AM/FM radio station and (2) municipalities with no local radio stations. It is worth highlighting that media presence in a given municipality is not randomly assigned, thus the following results are not intended to establish causal relationships.

Initially, we look at the impact of the audit reports on electoral outcomes. It is important to emphasize that this analysis adds to previous findings about the effect of the Brazilian auditing policy on electoral results since, to our knowledge, we are the first to examine how the disclosure of corruption might also effect city council elections.[28] Hence, we replicate the baseline model using the *Share of Seats* won by the incumbent's coalition as a dependent variable. We first study the whole sample and then split it depending on whether municipalities have local radio stations. The results are reported in Columns (1), (2) and (3) of Table 7. The coefficients of interest are only significant when looking at municipalities where citizens have greater access to information. On the one hand, Column (2) shows that in municipalities where the disclosure of a positive report occurs during the selection period, the parties supporting the incumbent mayor win 5% more seats, while if the report is negative they win 5% fewer seats.[29] On the other hand, Columns (1) and (3) suggest that, on average (and in the absence of a media presence) audits have very little effect on the electoral results. We can draw three important implications from these results. First, we can confirm that local media play an essential role in the accountability process. Second, the publication of the audit reports has a real effect on the election. This is a crucial element as in order for parties to react to the results of the audit report, they have to expect that voters care enough about the contents of the report to change how they vote. Third, voters still punish corrupt parties in elections even if they could select better candidates. Indeed, the electoral reward of a positive report is not affected by a potential decrease in the quality of candidates.

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[28]Ferraz and Finan (2008) show that corrupt mayors have a lower probability of being re-elected, while Muço (2016) finds that voters also reduce their support of a corrupt incumbent mayor's party in presidential elections.

[29]If we replicate this analysis and add to our treated municipalities those in which the report is disclosed between the end of the selection period and election day, we have slightly different results. While the punishment for having a negative report is significant and of a similar magnitude, there is no electoral reward associated with receiving a positive report.

We then apply the same procedure to our baseline estimation. In Column (4) we report the same results as Column (4) from Table 4, while Columns (5) and (6) report the results for the samples of municipalities with and without local radio, respectively. In municipalities with radio stations, when the disclosure of the report provides a positive signal (i.e., low corruption) there is a decrease in the average years of schooling of all candidates of a bit less than 7 months (coeff. -0.582), while there is an increase in education of 3 months when the report provides a negative signal (coeff. 0.845-0.582 = 0.263). The coefficients are of a similar size when we consider either all candidates or only new candidates (Column 2). When we focus on municipalities with no local radio stations, Columns (3) and (4), the effect is either not significant or borderline significant. The reported coefficients are also smaller.

Overall, the results seem to be coherent with our intuition, as the effect of the audit report appears to be larger, and statistically significantly different from 0 at the 1% level, only in municipalities where citizens are likely to be more exposed to the media (i.e., those with local radio stations).[30] Interestingly, the substantial change in the quality of the pool of candidates does not seem to be enough to significantly change the electoral results.[31]

5.2.4. Elected candidates

As outlined above, city councilors are elected using an open-list proportional system, and citizens still have a say over who will eventually be chosen. In this last section we analyze whether the disclosure of the report, affecting candidates' selection, has also an effect on the elected candidates. This may happen because selecting more- (or less-) educated candidates could lead to more- (or less-) educated elected politicians (see footnote 26). To do so, we use the usual specification and consider the average years of schooling of candidates who were elected from the incumbent coalition as a dependent variable. We also provide alternative results that examine the share of females and the

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[30] We apply the same strategy by replicating the estimates in Table 6. The main findings do not seem to be dependent on the presence of the media, except if the dependent variable is the median level of education; in that case, similarly to the results of this section, the effect appears to be more pronounced where local radio stations are available.

[31] Also note that our main findings do not vary depending on the level of electoral competitiveness, i.e., the change in candidates' ability is not significantly higher in close elections (results available upon request).

average age of the elected candidates. We report the estimated coefficients in Table 8. The main coefficients in all three columns are not significantly different from 0, though the signs of the first column are in line with the once from the main analysis. These results, together with the fact that voters punish parties that support the incumbent in the election, despite the changes in the candidates' selection, are consistent with recent studies providing evidence that voters do not seem to be affected by parties' reactions to popularity shocks (Adam, 2012).[32]

6. Identification checks

6.1. Sample selection

In our analysis we always constrain the sample to municipalities in which the incumbent's coalition decides to run for election. In other words, our sample is not random. This might produce a self-selection bias if the disclosure of the audit report affects the stability of the coalition and hence the decision to run for office. To account for this potential issue, we test whether there is any difference in the probability of being part of our sample between municipalities in which the auditing report was disclosed during the selection period (i.e., the treated group) or at other times (i.e., the control group). Therefore, we take the sample provided by Brollo et al. (2013), which includes all municipalities selected in the first 29 lotteries. From this larger sample we keep the municipalities used so far in the paper as well as those that were only excluded from the analysis because the incumbent party did not present any candidates. We then create a dummy variable equal to 1 if the municipality was part of our sample, and 0 otherwise. We replicate the same procedure for the sample used in the mayoral election analysis. The whole sample would be composed of 1,858 municipality-term observations, which excludes only 163 observations from the analysis. We report in Table 9 the formal test of the potential presence of self-selection bias, including a number of t-tests showing that being treated does not affect the average probability of being part of our sample. This is also true when we look separately at the samples with high and low levels corruption. In conclusion, this analysis suggests that a coalition's decision to run for re-election is uncorrelated with the treatment, and that this is true for different levels of corruption.

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[32]It is worth to emphasize that these results are not meant to be a direct test on whether voters prefer either high or low educated candidates.

6.2. Robustness of the corruption measure

Another potential concern about our identification is that our corruption measure may be serving as a proxy for other municipal features. In fact, while municipalities are randomly assigned to either a treated or control group, the level of corruption is not random and might depend on specific municipal conditions. For instance, corruption is potentially higher where there is extensive use of public investments, which usually occurs in bigger and richer cities. If this is true, we may be estimating how the release of an audit (regardless of its severity) has a differential effect on candidates' education, for example in small vs. big or poor vs. rich cities. To help assess this potential issue, we replicate our baseline estimations and include additional interaction terms, where we multiply the treatment status dummy by a set of covariates that could be expected to be correlated with the level of corruption: *Population*, *Income*, *Education* and *Share of Pop. in Public Administration*. The results, presented in Table 10, reduce our concerns: in all the estimates, the interaction between the treatment status variable and the level of corruption is always significantly different from 0 and the coefficient is relatively stable across the different specifications. Moreover, the interaction terms that include the covariates never reach the conventional level of significance, whether analyzed in separate estimations (Columns 1 to 4) or jointly (Column 5).[33] Overall, this suggests that our measure of corruption is unlikely to be proxying for other municipal characteristics.

7. Conclusion

This paper provides some of the first evidence on the effect of information about government behavior on the selection of political candidates. Using city council election data from Brazil, we find that an unexpected positive shock regarding the government's honesty has a detrimental effect on the quality of candidates put forward by its coalition in the next election. By contrast, it selects better candidates when there is a negative shock. Indeed, we show that these effects are present whether we use the average or median years of candidates' schooling. Importantly, the results of our analysis are of similar size when focusing only on freshmen candidates. Our findings also show that the accessibility of information plays a role, as our results are clearer in municipalities that

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[33]We find similar results when the interaction term instead uses a dummy for the top 50% percentile or dummies for the different quartiles.

have radio stations. However, other candidate characteristics, such as the share of female candidates and the average age of the pool of candidates, are not affected. We also find that, despite the changes in the quality of candidates, neither the electoral results nor the types of elected representatives seem to be significantly affected.

Overall, our analysis first provides one of the few causal estimates supporting the predictions of recent studies showing that political parties react to specific characteristics of electoral competition (Galasso and Nannicini, 2011; Mattozzi and Merlo, 2015). Second, we show that information releases might have indirect effects on electoral outcomes. In light of our results, it could be plausible that studies showing a change in support for the incumbent after a popularity shock might underestimate the shock's *pure* effect on voters' preferences, as their voting decisions might also have been affected by changes to the quality of the pool of candidates. Finally, we find that the Brazilian policy analyzed in this study does not help improve the quality of elected politicians. On the contrary, in the absence of corruption we observe that the incumbent party selects lower-ability candidates. This puzzling result highlights a potential unintended effect of anti-corruption measures on the dynamics of political accountability.
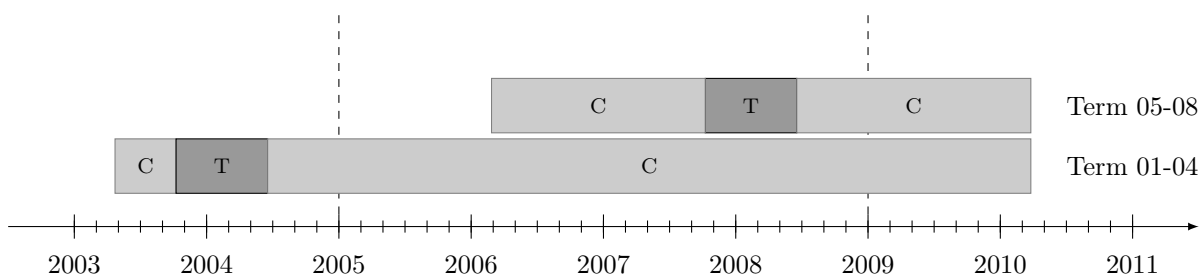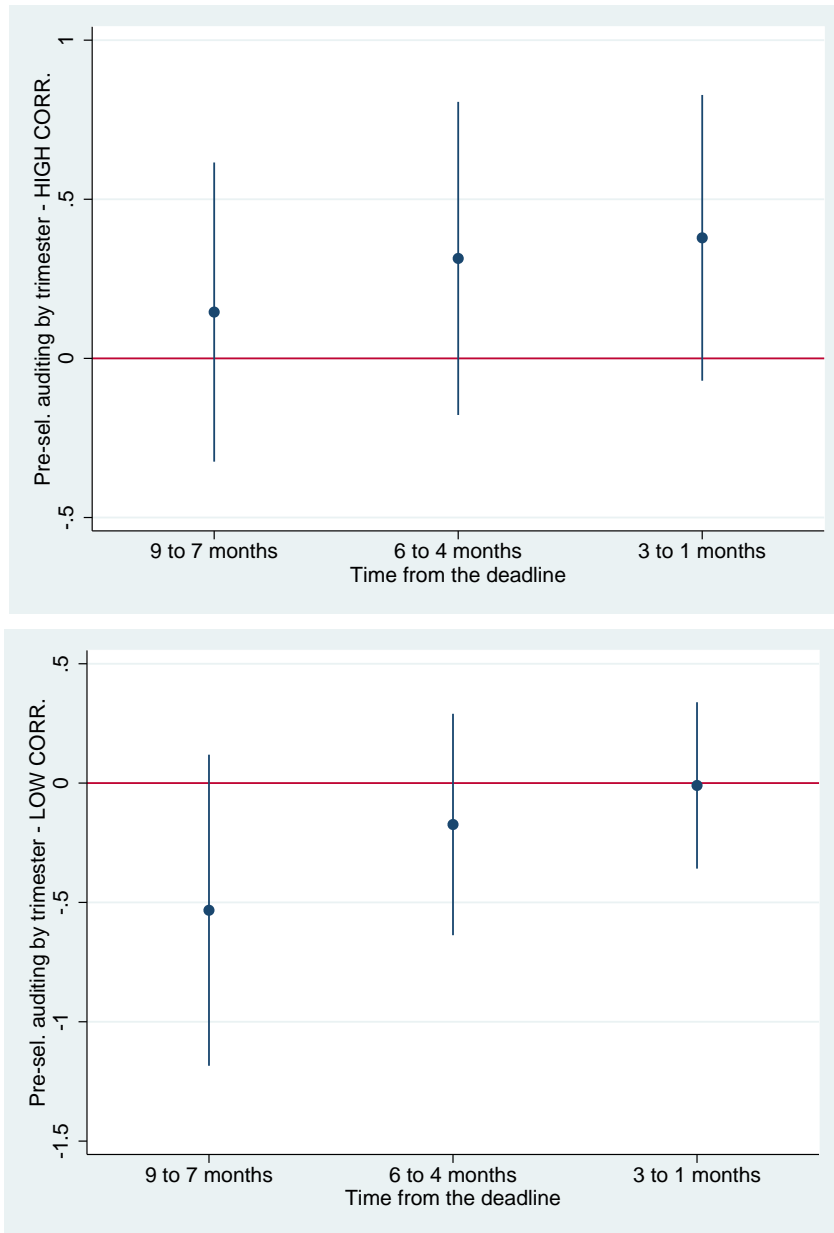
Figure 1: Treated and control definition

Figure 2: Timing of the release - High and low corruption sub samples

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Table 1: Summary statistics

Variable	Mean	Std. Dev.	Min	Max	N
Average education - n. of years (incumbent)	9.938	2.186	1	17	1695
Average education - n. of years (challenger)	9.805	1.675	3.571	14.258	1695
Average education of freshmen - n. of years (incumbent)	9.919	2.331	1	17	1695
Average education of freshmen - n. of years (challenger)	9.734	1.687	3	14.3	1695
Mandatory school (incumbent)	0.678	0.231	0	1	1695
Mandatory school of freshmen (incumbent)	0.679	0.247	0	1	1695
Median education - n. of years (incumbent)	9.882	2.891	1	17	1695
Average age (incumbent)	43.542	4.092	23.5	59	1694
Share of female (incumbent)	0.211	0.133	0	1	1695
Median education of freshmen - n. of years (incumbent)	9.822	3.058	1	17	1695
Average age of freshmen (incumbent)	42.871	4.662	22	67	1694
Share of female of freshmen (incumbent)	0.237	0.166	0	1	1695
Average education elected - n. of years (incumbent)	10.472	3.007	1	17	1540
Average age elected (incumbent)	42.964	6.348	22	69	1540
Share of female elected (incumbent)	0.132	0.212	0	1	1540
Mayor average education - n. of years (incumbent)	13.257	4.013	1	17	1195
Mayor average education freshmen - n. of years (incumbent)	13.206	4.015	1	17	663
Share of seats won (incumbent)	0.330	0.196	0	1	1695
Corruption	0.048	0.1	0	0.905	1695
Media	0.431	0.495	0	1	1695
Dummy party incumbent PT	0.212	0.409	0	1	1695
Population	25936.923	48667.258	795	461534	1695
Income	580.174	317.944	80.967	3062.481	1695
Share of pop. employed	37.641	8.202	11.499	75.59	1695
Gini coefficient	0.560	0.068	0.344	0.796	1695
Municipality average education	3.552	1.088	0.746	7.711	1695
Share of pop. in public administration	2.126	1.202	0.122	9.147	1695
Share of pop. in agriculture	16.467	9.856	0.041	64.043	1695
Share of pop. in industry	3.952	3.69	0	34.637	1695
Share of pop. in service	6.675	2.763	0.257	18.756	1695
Share of pop. in commerce	7.535	3.848	0.26	27.764	1695
Share of pop. in transport	1.167	0.695	0	5.593	1695
Share of pop. in service	6.675	2.763	0.257	18.756	1695

Table 2: Sample details

	Term 2001	Term 2005	Total
Treated	182	145 (101)	327
Control	960 (374)	408 (273)	1368
All sample	1142	553	1695

Notes: The table reports details on the sample of 1321 municipalities considered in our analysis. The level of observation is municipality-term. In parenthesis we report the numbers of municipalities that appear in more than one term.

Table 3: Differences in observable characteristics

	Control group (1)	Treated group (2)	Difference (3)
Average education in $t-1$ (incumbent)	9.324	9.348	-0.024
Average education in $t-1$ (challenger)	9.262	9.165	0.097
Dummy party incumbent PT	0.215	0.202	0.013
Population	26516	23511	3004
Income	581.668	573.920	7.749
Share of pop. employed	37.720	37.313	0.408
Gini coefficient	0.560	0.558	0.002
Avg. municipal number of years of education	3.568	3.483	0.085
Share of pop. in public administration	2.088	2.196	-0.108
Share of pop. in agriculture	16.390	16.792	-0.403
Share of pop. in industry	4.038	3.592	0.446**
Share of pop. in service	6.705	6.551	0.154
Share of pop. in commerce	7.584	7.327	0.257
Share of pop. in transport	1.175	1.137	0.038
Share of pop. in service	6.705	6.551	0.154

Notes: This table displays the mean characteristics of 1321 municipalities (1695 municipality-term observations) that were audited by the *Controladoria Geral da União* (CGU) in the period 2003-2009 (i.e., from the 2nd to the 29th lottery). The control group (column 1) is composed by 994 municipalities which had disclosed the results of the auditing concerning the term 2001-2004 either before the 5th of Oct 2003 or after the 5th of Jul 2004, or had disclosed the results of the auditing concerning the term 2005-2008 after the 5th of Jul 2008 or before the 3rd of Oct 2007. Instead, the treated group (column 2) is composed by 327 municipalities which had disclosed the results of the auditing from 12 to 3 months before the elections (i.e., from one year before elections to the 5th of July 2004 or 2008). Column (3) shows the difference of the means and the level of significance. * p < 0.1, ** p < 0.05 and *** p < 0.01.

Table 4: Audit releases and the quality of candidates - incumbent coalition

| | All candidates | | | | | New candidates | | |
	(1)	(2)	Linear (3)	Median (4)	Quartile (5)	Linear (6)	Median (7)	Quartile (8)
Pre-selection auditing	0.016	0.041	-0.073	-0.378**	-0.429**	-0.160	-0.448**	-0.531**
	(0.134)	(0.115)	(0.131)	(0.172)	(0.194)	(0.141)	(0.191)	(0.212)
Pre-selection auditing ×			1.567			1.796*		
corruption			(0.993)			(1.080)		
Pre-selection auditing ×				0.724***			0.723***	
above median corruption				(0.235)			(0.258)	
Pre-selection auditing ×					0.281			0.448
second quartile of corruption					(0.393)			(0.459)
Pre-selection auditing ×					0.629**			0.691**
third quartile of corruption					(0.315)			(0.333)
Pre-selection auditing ×					0.899***			0.906***
fourth quartile of corruption					(0.280)			(0.314)
Corruption			0.297			0.377		
			(0.452)			(0.496)		
Above median corruption				-0.017			-0.004	
				(0.097)			(0.109)	
Second quartile					-0.005			0.139
					(0.202)			(0.207)
Third quartile					0.002			0.040
					(0.115)			(0.132)
Fourth quartile					-0.036			0.005
					(0.126)			(0.143)
F-test inter			0.181	0.012	0.078	0.142	0.025	0.107
R^2	0.117	0.374	0.376	0.379	0.379	0.319	0.320	0.322
N observations	1695	1695	1695	1695	1695	1695	1695	1695
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Average years of education of candidates - as a city councilor - from the incumbent coalition. Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT*, ln(*population*), *income, gini coefficient, share of population with a secondary degree, share of population employed, share of population working in agriculture, share of population working in industry share of population working in commerce, share of population working in transport, share of population working in service* and *share of population working in the public administration.* Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Table 5: Audit releases and the quality of candidates - challenger coalitions

	All candidates					New candidates		
	(1)	(2)	Linear (3)	Median (4)	Quartile (5)	Linear (6)	Median (7)	Quartile (8)
Pre-selection auditing	-0.043	-0.012	0.014	-0.034	-0.048	-0.026	-0.091	-0.114
	(0.095)	(0.075)	(0.088)	(0.112)	(0.125)	(0.090)	(0.118)	(0.133)
Pre-selection auditing × corruption			-0.452 (0.886)			-0.602 (0.845)		
Pre-selection auditing × above median corruption				0.038 (0.150)			0.052 (0.156)	
Pre-selection auditing × second quartile of corruption					0.075 (0.266)			0.138 (0.277)
Pre-selection auditing × third quartile of corruption					0.174 (0.191)			0.220 (0.200)
Pre-selection auditing × fourth quartile of corruption					-0.058 (0.185)			-0.054 (0.193)
Corruption			0.246 (0.283)			0.309 (0.297)		
Above median corruption				0.010 (0.068)			0.013 (0.069)	
Second quartile					0.077 (0.127)			0.005 (0.125)
Third quartile					-0.017 (0.084)			-0.036 (0.086)
Fourth quartile					0.067 (0.086)			0.069 (0.088)
F-test inter			0.839	0.983	0.932	0.618	0.846	0.857
R^2	0.172	0.515	0.515	0.515	0.515	0.499	0.499	0.500
N observations	1695	1695	1695	1695	1695	1695	1695	1695
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Average years of education of candidates - as a city councilor - from the challenger coalitions*. *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT*, ln(*population*), *income*, *Gini coefficient*, *share of population with a secondary degree*, *share of population employed*, *share of population working in agriculture*, *share of population working in industry*, *share of population working in commerce*, *share of population working in transport*, *share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Table 6: Audit releases and candidates' characteristics

	All candidates				New candidates			
	Median edu. (1)	Mayor edu. (2)	Female (3)	Age (4)	Median edu. (5)	Mayor edu. (6)	Female (7)	Age (8)
Pre-selection auditing	-0.431*	-0.415	-0.012	-0.239	-0.474*	-0.615	-0.021	-0.250
	(0.225)	(0.687)	(0.013)	(0.377)	(0.253)	(0.687)	(0.015)	(0.426)
Pre-selection auditing ×	0.690**	0.309	0.010	0.449	0.600*	0.434	0.010	0.558
above median corruption	(0.316)	(0.594)	(0.017)	(0.508)	(0.346)	(0.852)	(0.020)	(0.566)
Above median corruption	-0.018	0.177	-0.008	0.035	0.058	-0.023	-0.009	0.117
	(0.137)	(0.256)	(0.007)	(0.218)	(0.151)	(0.339)	(0.009)	(0.252)
F-test inter	0.125	0.646	0.578	0.765	0.173	0.821	0.309	0.563
R^2	0.301	0.138	0.065	0.143	0.267	0.151	0.053	0.139
N observations	1695	1195	1695	1694	1695	663	1695	1694
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *median years of education* in Columns (1) and (5), *mayor cand. avg. number of years of education* in Columns (2) and (6) *Share of female* in Columns (3) and (7), *average age* in columns (4) and (8). *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT, population, income, Gini coefficient, share of population with a secondary degree, share of population employed, share of population working in agriculture, share of population working in industry, share of population working in commerce, share of population working in transport, share of population working in service* and *share of population working in the public administration.* Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

Table 7: Audit releases, electoral results and the quality of candidates by media presence

	Share of seats won			Education of candidates		
	All (1)	Local Radio (2)	No Local Radio (3)	All (4)	Local Radio (5)	No Local Radio (6)
Pre-selection auditing	0.011	0.053**	-0.019	-0.378**	-0.582***	-0.275
	(0.018)	(0.026)	(0.025)	(0.172)	(0.182)	(0.266)
Pre-selection auditing ×	-0.031	-0.110***	0.024	0.724***	0.845***	0.578*
above median corruption	(0.024)	(0.034)	(0.033)	(0.235)	(0.304)	(0.340)
Above median corruption	0.008	0.013	0.008	-0.017	-0.055	0.029
	(0.010)	(0.016)	(0.014)	(0.097)	(0.134)	(0.141)
F-test inter	0.520	0.006	0.707	0.012	0.006	0.240
R^2	0.146	0.188	0.134	0.379	0.427	0.289
N observations	1695	730	965	1695	730	965
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Share of seats won by the incumbent coalition* in columns (1-3) and *Average years of education of candidates - as a city councilor - from the incumbent coalition* in columns (4-6). *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT, population, income, Gini coefficient, share of population with a secondary degree, share of population employed, share of population working in agriculture, share of population working in industry, share of population working in commerce, share of population working in transport, share of population working in service* and *share of population working in the public administration.* Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

Table 8: Audit releases and elected candidates

	Education (1)	Age (2)	Female
Pre-selection auditing	-0.102	-1.031*	0.018
	(0.263)	(0.609)	(0.020)
Pre-selection auditing ×	0.064	0.866	-0.030
above median corruption	(0.351)	(0.818)	(0.028)
Above median corruption	0.033	0.082	0.010
	(0.160)	(0.367)	(0.013)
F-test inter	0.965	0.342	0.696
R^2	0.256	0.086	0.047
N observations	1540	1540	1540
State FE	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes

Notes: The dependent variable is *average years of education of elected candidates* in column (1), *average age of elected candidates* in column (2) and *share of female of the elected candidates* in column (3). *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT, population, income, Gini coefficient, share of population with a secondary degree, share of population employed, share of population working in agriculture, share of population working in industry share of population working in commerce, share of population working in transport, share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Table 9: Sample selection

	Control group	Treated group	Difference
	(1)	(2)	(3)
All			
Re-run city council (inc)	0.911	0.919	-0.008
Re-run mayor all (inc)	0.648	0.621	0.028
Re-run mayor new (inc)	0.362	0.334	0.028
High corruption			
Re-run city council (inc)	0.890	0.913	-0.023
Re-run mayor all (inc)	0.670	0.649	0.021
Re-run mayor new (inc)	0.367	0.351	0.016
Low corruption			
Re-run city council (inc)	0.930	0.919	0.004
Re-run mayor all (inc)	0.626	0.601	0.025
Re-run mayor new (inc)	0.356	0.322	0.034

Notes: This table reports differences between the control (1502 observations) and the treated (356 observations) group about the mean probability of being part of the sample of municipalities we use in the different sections of the paper. * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Table 10: Robustness of the corruption measure

	(1)	(2)	(3)	(4)	(5)
Pre-selection auditing	-0.227	-0.686**	-0.969*	-0.318	0.128
	(0.870)	(0.291)	(0.499)	(0.265)	(1.014)
Pre-selection auditing ×	0.725***	0.781***	0.798***	0.716***	0.806***
above median corruption	(0.234)	(0.239)	(0.248)	(0.236)	(0.246)
Pre-selection auditing ×	-0.016				-0.108
ln(population)	(0.087)				(0.100)
Pre-selection auditing ×		0.000			0.000
income		(0.000)			(0.000)
Pre-selection auditing ×			0.157		0.130
education			(0.110)		(0.161)
Pre-selection auditing ×				-0.025	-0.081
pop. in pub. administration				(0.077)	(0.083)
Above median corruption	-0.017	-0.025	-0.026	-0.016	-0.029
	(0.097)	(0.097)	(0.097)	(0.097)	(0.097)
F-test inter	0.008	0.007	0.008	0.012	0.002
R^2	0.379	0.379	0.379	0.379	0.380
N observations	1695	1695	1695	1695	1695
State FE	Yes	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the *average years of education of candidates - as a city councilor - from the incumbent coalitions*. Municipality controls include: *dummy party incumbent PT*, ln(*population*), *income*, *Gini coefficient*, *share of population with a secondary degree*, *share of population employed*, *share of population working in agriculture*, *share of population working in industry share of population working in commerce*, *share of population working in transport*, *share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

A. Appendix

Table A.1: Audit releases and the quality of candidates by electoral term

	Term 2001-2004				Term 2005-2008			
	Incumbent		Challenger		Incumbent		Challenger	
	All	New	All	New	All	New	All	New
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-selection auditing	-0.612***	-0.711***	-0.088	-0.148	0.157	0.112	0.052	-0.007
	(0.227)	(0.256)	(0.152)	(0.162)	(0.254)	(0.275)	(0.148)	(0.153)
Pre-selection auditing ×	0.783**	0.819**	0.153	0.175	0.331	0.280	-0.197	-0.202
above median corruption	(0.304)	(0.339)	(0.204)	(0.212)	(0.368)	(0.405)	(0.213)	(0.219)
Above median corruption	0.007	0.030	-0.040	-0.074	-0.213	-0.164	0.111	0.183
	(0.115)	(0.127)	(0.081)	(0.083)	(0.182)	(0.209)	(0.119)	(0.120)
F-test inter	0.032	0.028	0.891	0.721	0.237	0.546	0.718	0.337
R^2	0.389	0.332	0.518	0.496	0.375	0.321	0.515	0.508
N observations	1142	1142	1142	1142	553	553	553	553
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Average years of education of candidates as a city councilor*. *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT*, *population*, *income*, *Gini coefficient*, *share of population with a secondary degree*, *share of population employed*, *share of population working in agriculture*, *share of population working in industry*, *share of population working in commerce*, *share of population working in transport*, *share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Table A.2: Audit releases and the quality of candidates (mandatory school)

| | All candidates | | | | | New candidates | | |
	(1)	(2)	Linear (3)	Median (4)	Quartile (5)	Linear (6)	Median (7)	Quartile (8)
Pre-selection auditing	0.000	0.002	-0.007	-0.043**	-0.053**	-0.018	-0.054**	-0.060**
	(0.014)	(0.013)	(0.015)	(0.021)	(0.024)	(0.016)	(0.023)	(0.026)
Pre-selection auditing × corruption			0.124			0.189		
			(0.107)			(0.119)		
Pre-selection auditing × above median corruption				0.078***			0.086***	
				(0.027)			(0.029)	
Pre-selection auditing × second quartile of corruption					0.056			0.035
					(0.045)			(0.053)
Pre-selection auditing × third quartile of corruption					0.071**			0.080**
					(0.034)			(0.035)
Pre-selection auditing × fourth quartile of corruption					0.102***			0.102***
					(0.034)			(0.037)
Corruption			0.036			0.048		
			(0.052)			(0.057)		
Above median corruption				-0.003			0.000	
				(0.011)			(0.012)	
Second quartile					-0.015			0.005
					(0.021)			(0.022)
Third quartile					-0.001			0.003
					(0.013)			(0.015)
Fourth quartile					-0.010			-0.000
					(0.015)			(0.016)
F-test inter			0.320	0.023	0.146	0.123	0.016	0.155
R^2	0.111	0.286	0.288	0.291	0.292	0.251	0.253	0.254
N observations	1695	1695	1695	1695	1695	1695	1695	1695
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Share of candidates which finished mandatory school - as a city councilor - from the incumbent coalition. Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT*, ln(*population*), *income, gini coefficient, share of population with a secondary degree, share of population employed, share of population working in agriculture, share of population working in industry share of population working in commerce, share of population working in transport, share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Table A.3: Audit releases and the quality of candidates - old incumbent coalition

	All candidates					New candidates		
	(1)	(2)	Linear (3)	Median (4)	Quartile (5)	Linear (6)	Median (7)	Quartile (8)
Pre-selection auditing	-0.102	-0.083	-0.169	-0.323**	-0.358**	-0.182*	-0.370***	-0.402***
	(0.112)	(0.093)	(0.105)	(0.137)	(0.154)	(0.106)	(0.139)	(0.155)
Pre-selection auditing × corruption			1.224			1.311		
			(0.878)			(0.905)		
Pre-selection auditing × above median corruption				0.414**			0.482**	
				(0.187)			(0.189)	
Pre-selection auditing × second quartile of corruption					0.179			0.164
					(0.320)			(0.334)
Pre-selection auditing × third quartile of corruption					0.253			0.319
					(0.245)			(0.247)
Pre-selection auditing × fourth quartile of corruption					0.622***			0.686***
					(0.229)			(0.231)
Corruption			0.097			0.009		
			(0.409)			(0.416)		
Above median corruption				-0.006			-0.028	
				(0.086)			(0.086)	
Second quartile					0.092			0.080
					(0.171)			(0.172)
Third quartile					0.058			0.038
					(0.104)			(0.103)
Fourth quartile					-0.040			-0.066
					(0.110)			(0.111)
F-test inter			0.279	0.073	0.190	0.278	0.037	0.124
R^2	0.126	0.419	0.420	0.421	0.422	0.417	0.418	0.419
N observations	1695	1695	1695	1695	1695	1695	1695	1695
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Average years of education of candidates - as a city councilor - from the old incumbent coalition*. *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT*, ln(*population*), *income*, *gini coefficient*, *share of population with a secondary degree*, *share of population employed*, *share of population working in agriculture*, *share of population working in industry share of population working in commerce*, *share of population working in transport*, *share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

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