Does Fighting Corruption Affect the Private Gains from Political Office? Evidence from Brazilian Mayoral Elections

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Abstract

This paper studies how an exogenous reduction in corruption promoted by a randomized audit program of Brazilian municipalities affects the gains from political office. Among mayors running for reelection, I compare those that have been audited during their first term to those that did not. I find evidence that audited mayors' reported wealth tended to be *higher* than that of non-audited mayors following the audit. Did a corruption crackdown make them richer? Most likely yes, but for two reasons that may be seen as perhaps unintended consequences of the audit. Municipal GDP grows faster in audited municipalities, explaining as much as 94% of the change in mayors' wealth; moreover, I find suggestive evidence that mayors are induced to disclose previously-omitted assets, increasing transparency in the electoral process.

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1 Introduction

Do politicians have private gains from political office? Conventional wisdom and a small empirical literature alike seems to suggest that returns from politics are substantial. However, most findings center on highly influential politicians in established democracies, where politicians gain through the well-known "revolving doors of politics," *after* leaving office (Eggers and Hainmueller, 2009; Palmer and Schneer, 2016).

This leads to question of whether the same applies to politicians in developing countries, where corruption usually permeates public offices. The little we know comes from Fisman and Svensson (2007), who study Indian politicians of varying degrees of influence, and for which they find a 3-5% annual return premium to political office. While they also find that returns are somewhat larger for politicians in more corrupt states, they rely on a perception-based measure of corruption, which are known to potentially cause reporting biases (e.g. Cordis and Milyo, 2016, 2021). Moreover, it leaves unanswered whether fighting corruption can reduce these abnormal gains.

In this paper, I study how an exogenous reduction in corruption, stemming from an anti-corruption program, impacts the gains from office for municipal mayors in Brazil. Following selection through a lottery, a team of federal auditors is sent to investigate how municipal mayors allocate the application of constitutionally-mandated federal transfers (Ferraz and Finan, 2011; Avis et al., 2018). The findings of these audits is then released in a publicly-accessible report by the General Comptroller's Office (*Controladoria Geral da União* - CGU). Importantly, while data from this audit program has been widely used in previous research (e.g. Ferraz and Finan, 2008, 2011; Ferraz et al., 2012; Bologna and Ross, 2015; Bologna, 2016; Avis et al., 2018; Zamboni and Litschig, 2018; Colonnelli and Prem, 2022; Bastos et al., 2024), none of them analyzes how corruption affects the gains from political office.

I explore this gap by matching data from this audit program to rich election data for the 2004, 2008 and 2012 local elections. The electoral data includes several demographic characteristics of all candidates, including age, education, occupation, and marital status. Crucially, since 2008, it also reports candidates' wealth which, although ultimately self-reported, is based on their latest tax return and can be cross-checked by electoral authorities. Finally, electoral data also includes information about every donation their campaigns received, including the sources (individuals, corporations, or public funds), type (e.g. cash, in-kind), and their amount.

My first goal is to estimate whether audited mayors experience different growth in their reported wealth. Because these audits introduced an exogenous reduction in corruption of about 8% (Avis et al., 2018), effectively reducing available rents, audited mayors should perceive lesser asset growth if they were benefiting from illicit enrichment. I start with a larger sample comparing the wealth of mayors at the end of their terms in 2008 and 2012, among municipalities with population below 500,000 – those eligible for treatment. While I do not observe the initial wealth of those elected in 2004, the randomization in treatment assignment – being audited at some point during their first term – suggests that treated and control groups should have the same average initial wealth. Then, I consider a more restrictive sample: I compare mayors first elected in 2008 who run for reelection in 2012 in municipalities that experienced an audit sometime within the 2008-2012 period with those that did not, thus observing reported wealth at the beginning and end of their first term. In both cases, I weight the data using entropy balancing (Hainmueller, 2012; Hainmueller and Xu, 2013) to further increase similarity between treatment and control groups.

I find that, at the end of their first term, audited mayors are richer than their counterparts that have not been audited by 15.3 to 18.3%. Within the stricter sample, I find more modest evidence that audited mayors report their wealth growing by 33.5 to 48.5 percentage points (over four years) in excess of that of the control group, with these average treatment effects being somewhat sensitive to specification. At face value, these findings are puzzling: they would suggest that a corruption crackdown made mayors report greater increases in wealth. Did they actually get richer? My results suggest that they did, but for two reasons that may be seen as perhaps unintended consequences of the audit. First, as suggested by Colonnelli and Prem (2022), the local economies benefited from the audit, and the greater change in municipal GDP explains as much of 94% of the change in mayors' wealth.

Second, I provide suggestive evidence that the audits induced mayors to report previously-undisclosed assets, increasing transparency in the electoral process. I find that several mayors reported new bank accounts and stock-holdings in small businesses. By searching for reported company names and their tax numbers on the Federal Revenue System (*Receita Federal*) database, I show that 2/3 of newly reported company ownerships that I could identify refer to businesses that already existed in 2008. To check whether these do not reflect changes in reporting practices, I train a simple stacked-ensemble machine-learning model to match mayor's assets into general categories such as real estate, vehicles, cash balances, and business assets. I find that reporting of business ownership more than doubles across the treated group, while it decreased by 6% in the control group. However, I focus on the reporting of bank accounts because they are easily identifiable in the data and mayors tend to under-report bank assets (Souto-Maior and Borba, 2019), but it is unreasonable to assume that a given mayor actually has zero bank accounts. The share of mayors reporting at least one bank account more than doubles among audited mayors, but increases by only 68% in the control group.

Hence, the results suggest that audited mayors do at least partially get richer relative to their non-audited counterparts, but this increase in wealth mostly stems from municipal growth. The fact that these audits seemed to have increased reporting amongst mayors suggests that they have some impact on electoral transparency and, as a result, may have actually reduced true asset wealth even if we cannot directly observe it.

As a final test of this hypothesis, I consider a simple model of rent-seeking assuming that mayors normally extract rents from their time in office. Thus, a reduction in the amount of rents available caused by the audit should lead to a smaller willingness to "pay for their seat", as in Tullock (2008) and Weaver (2021). I argue that this would be captured by the amount of funds they invest in their own campaign. While these results are never significant, they are always negative, weighting more in favor of this hypothesis than the alternative.

This paper contributes, first and foremost, to the empirical literature estimating the private gains from political office. This literature indicates that returns from political office are quite large for high-profile politicians. For instance, a study of members of the British parliament suggest that Conservatives almost doubled their wealth, especially by serving as a director of a publicly traded firm (Eggers and Hainmueller, 2009). This is also common practice in the United States, greatly increasing the incomes of former US Senators and Governors (Palmer and Schneer, 2016). However, they are ambiguous for less influential political offices. Lenz and Lim (2009) find very mixed evidence for U.S. House members between 1995 and 2005, and Querubin and Snyder (2009) finds no evidence of large returns for U.S. House members between 1845 and 1875, except for the Civil War years, during which federal expending sky-rocketed.[1]

In this literature, Querubin and Snyder (2009) and Fisman et al. (2014) are the only studies to exploit the impact of variation in available rents on the private returns from politics. I extend this literature by looking at the private gains of local level politicians in the context of a developing country, where corruption may be an important determinant. Closest to the present study is that of Fisman et al. (2014), who analyzes how variation in corruption affects private gains of politicians in India. I differ from their study by evaluating how an exogenous reduction in corruption affects such returns. Crucially, by looking exclusively at elected officials, I overcome some of the most difficult methodological challenges in this literature, which involve finding both robust counterfactuals for

[1]Berg (2020) also find that local politicians in Sweden had no extra monetary gain at all, but this can simply reflect the very low corruption environment of Sweden. As highlighted in Fisman et al. (2014), this literature is also connected to an emergent literature attempting to identify hidden earnings of politicians and public servants (e.g. Braguinsky et al., 2010; Di Tella, 2007), and use of privileged information and preferential access to assets promoted by political connections or committee-serving (e.g. Chen and Kung, 2019; Ziobrowski et al., 2004, 2011). Although this literature is still incipient for the political returns of politicians themselves, a large literature has studied the political connections of firms (e.g. Grier et al., 1991, 1994; Faccio, 2006). See Claessens et al. (2008); Bandeira-de Mello et al. (2012); Boas et al. (2014); Guerra (2023) for firms' political connections in the context of Brazil.

elected politicians and reliable measures of corruption.

It also relates to several strands of a large literature evaluating the effects of transparency and government accountability on the quality of government (Besley and Prat, 2006; Adsera et al., 2003), political competitiveness and electoral outcomes (Ferraz and Finan, 2008; De Vries and Solaz, 2017) and on reducing corruption directly (e.g. Ferraz and Finan, 2011; Di Tella, 2007; Reinikka and Svensson, 2005; Brunetti and Weder, 2003) – see also Djankov et al. (2010) for an excellent review of disclosure requirements around the globe. While this literature emphasizes that transparency is an important mechanism for reducing corruption, my contribution shows that the reverse is also true: fighting corruption may lead to (perhaps unintended) increases in transparency. This can suggest that under accusations of corruption, politicians attempt to signal "cleanliness" and prevent blame attribution.[2]

This paper proceeds as follows. The next section details the data on corruption and electoral data. Section 3 introduces the empirical strategy and the main results. The following section studies the mechanisms driving the increase in mayors reported wealth. The final section concludes.

2 Data

2.1 Election and Mayor Data

Local elections in Brazil occur every four years to elect a mayor and a local council. Every candidate must be registered under a political party to run. In municipalities with fewer than 200,000 registered voters, the mayoral candidate with the most votes wins. In those above this threshold, a second-round runoff is held unless a mayor gets 50 percent plus one of the votes in the first round. Mayors may serve a maximum of two consecutive terms.

[2]See De Vries and Solaz (2017) for such a framework. Also see Acemoglu et al. (2013) and the references therein for a literature concerning signaling games by politicians under limited voter information.

Following each election, the electoral authorities publish information about the candidates, their wealth, and the sources of campaign finance.[3] From this data, I collect candidates demographic characteristics, including age, marital status, gender, education, occupation, party affiliation, and whether they were running for reelection or not.

Within this extensive data, I focus on mayors' declared wealth as outcomes, and consider campaign donations as a robustness check. For our purposes, data on mayor's reported wealth is available only for the 2008 and 2012 elections.[4] I focus exclusively on mayors running for reelection because mayors are only required to report their wealth when registering their candidacy, and not at the end of their term. Thus, I can only observe the wealth of elected mayors at the end of their term for those that run again for reelection. For the same reason, I look exclusively at first-term mayors: observing the wealth at the end of their second term would require them to run for a third time, but there is a limit on two consecutive terms.[5]

Because candidates have to be registered under a party to run, a candidate will typically hand their latest tax return to their party, and party officials will use create a self-reported declaration of assets and submit it the Regional Electoral Court. In principle, they have little incentive to cheat because the Electoral Court itself can cross-reference the declaration with the Federal Revenue (*Receita Federal*) data base, and significant mismatches can lead to their candidacy being denied by the Electoral Court. Moreover, it is also considered electoral false identity, a crime punishable by up to five years in prison (Article 350 of the Electoral Code - Law 4,737/1965).

Yet, electoral law does not specifically establish *which* assets must be declared.[6] Instead, this has been decided over time by the jurisprudence in electoral courts. Real

[3]Appendix B provides details about campaign finance rules.

[4]To be clear, mayors already had to report their wealth since the 1990s, but this information only became digitally available beginning in the 2006 elections after the introduction of a computer software for registering candidacies, the CANDex. Data has been available since then, but as mentioned earlier, later elections are not directly comparable due significant changes in campaign finance rules.

[5]Incidentally, this becomes an important feature of the identification strategy, because there is strong evidence that first term are substantially less corrupt due to reelection incentives (Ferraz and Finan, 2011).

[6]Law 9,504/1997, Art. 11, §1°, IV, simply requires a "declaration of assets, signed by the candidate."

estate assets and vehicles are extremely well reported, and represent a large majority of candidates wealth both in counts and in terms of value. Testament to the accuracy in reporting, their real estate and vehicles tend to be copied *ipsis literis* from their tax return, even including addresses of the house they owned and lived, and the make, model, year, and license plates number of the cars they drove.[7] However, as highlighted by Souto-Maior and Borba (2019), one important gap is in the reporting of bank accounts. They show that while 60% of general population have a bank account, only around 25% of the candidates reported having one. The same applies to my sample, with 25% control and 26.6% of treated mayors (not statistically different) reporting at least one bank account at the beginning of their first term, i.e. before treatment.[8]

From the campaign finance data, I calculate the amount that is self-financed – how much mayors invested in their own campaigns. I also calculate the total value of donations received by each mayoral candidate, and the shares political party, from individuals, from companies, which I use for covariate balance, as detailed below. Within my sample, the average mayoral candidate invested a total of R$ 13,906.89 his or her own campaign in 2008, and R$ 32,757.60 in 2012. Respectively, these amount to roughly 6,700 USD, and 15,500 USD at the time, and represented 24.5 and 34.3% of their total campaign funds. However, there is huge variance, ranging from 0 to 1,130,000 *reais* across both elections.

2.2 Corruption Data

In turn, the treatment variable comes from a anti-corruption program launched in 2003 by the General Comptroller's Office (*Controladoria Geral da União*, CGU). Following a selection through a public lottery, a team of federal auditors is sent to each municipality, to audit how mayors have spent resources from constitutionally-mandated transfers from the federal government in the last three to four years (Ferraz and Finan, 2008; Avis et al.,

[7] In more recent elections, the electoral jurisprudence has come to the understanding that such fine details are no longer necessary because they can generate security and privacy concerns. Today, the standard is to list "a house in city X worth Y" or "a sedan vehicle valued at Z."

[8] In Section 4.2, I leverage this information about bank accounts to study the mechanisms driving the main results.

2018). In the first and second lotteries, still in 2003, CGU audited 5 and 26 municipalities, respectively. Lotteries 3 through 9 audited 50 municipalities each. Beginning with lottery 10, in mid-2004, all lotteries until number 37, in 2012, audited 60 municipalities each, totaling 2,061 audited municipalities. Importantly, the lottery is done with replacement, meaning that a given municipality may be audited more than once.

My main results focus on the audit as a randomized binary treatment. As mentioned above, data on reported wealth is only available for 2008 and 2012. Thus, for the purposes of this paper, I observe mayors elected for the first term in either 2004 and 2008, who run for reelection in 2008 or 2012, respectively, effectively creating two treatment windows between two pairs of elections. The mayors who got audited during their first term are considered treated, while their pairs that did not serve as the control group. I call this my "large" sample, for which I only observe final wealth. I also consider the second election window (2008-2012) my "strict" sample, for which I observe both final and initial wealth.

In studying the mechanisms explaining the main results, I also use a continuous measure of corruption as a robustness check. I focus on lotteries 22 to 37, for which I have a quantitative measure of corruption. Specifically, I use the data from Avis et al. (2018), who code the (log) number of instances of corruption found in each audited municipality, taken directly from CGU's reports. Their sample covers 1,020 audits and 967 municipalities from 2006 through 2013. However, because I only observe mayors that run for reelection, I end up using only a subsample of 559 mayors from their data.

I create two scaled measures of corruption. First, because the municipalities vary substantially in termf population, from as little as 1,494 to as much 474,596 inhabitants, I use the (log) of corruption *per capita*. Second, because the amount of corruption found will be proportional to the scope of the audit, I also use the log of corruption instances *per service order*, as in Avis et al. (2018). Although I believe these are scaled versions are preferable, I always consider their original variable as well. Summary statistics for all variables are reported in Table 1.

Importantly, I exclude later elections because although the audit program continues to exist, selection is no longer done by lottery (Avis et al., 2018). Further, a campaign expending limit (Avis et al., 2022) and a ban on corporate donations (Aparicio and Avenancio-León, 2022) were introduced after 2014, which makes later elections not directly comparable to earlier ones.

3 Empirical Strategy and Results

3.1 Identification Strategy

The main results rely on the randomness of the audit for causal identification. The units of observation are mayors elected for the first time in either the 2004 or 2008 elections (pre-audit period) who run again for reelection (post-audit period) in 2008 or 2012, respectively, creating two election windows. For mayors elected in 2004, I observe their wealth at the end of their first term, when they run for reelection in 2008. For mayors running in 2008, I observe both their initial wealth in 2008, and their final wealth when they run for reelection in 2012. The treatment group includes mayors that have been audited at some point along their first term, and the control group are their pairs that have not.

Previous studies on the returns to political office have faced challenges to find a reliable counterfactual for elected mayors, often relying on a regression discontinuity along close election races to address the potential differences in "talent," which are not unobservable (e.g. Fisman et al., 2014; Berg, 2020). Instead, I follow Ferraz and Finan (2008, 2011) and only compare mayors to mayors. In this case, first-term elected mayors running for reelection.

First, I start with a larger sample that provides a much larger number of observations, but in which I do not observe mayor's initial wealth. However, the randomization in treatment assigned suggests that treated and control groups should have no significant differences in wealth. This seems to be the case if we can extrapolate from the observable initial wealth in the second election window. Table 1 also shows that this is true for all other observable characteristics of mayors and their municipalities, except for GDP per capita, which is slightly higher in treated municipalities. This sample yields 2,593 control units and 385 treated units elected in 2004 (final wealth observed in 2008), and 501 observations, of which 459 are controls and 45 are treated, for mayors elected in 2008 (final wealth observed in 2012). The equation to be estimate is the following:

$$Log(FinalWealth)_{ise} = \alpha + \beta Audited_i + \delta X_i + \lambda_e + \phi_s + \epsilon_{se} \tag{1}$$

where $FinalWealth$ is the (log) wealth of mayor i from in election e and state s, observed at the end of their first term. The coefficient of interest, β, captures the difference in final wealth among audited and non-audited mayors, measured in percentage terms. I always include election fixed effects (λ_e) in unweighted specifications, and subsequently include state fixed effects (ϕ_s) and a vector controls, X, containing mayor and municipality characteristics observed at the time of their election. Robust standard errors (ϵ_{se}) are clustered at the state-election level.

Nonetheless, I also report results for the 2008-2012 election window alone, which I call the "strict" sample, for which I observe mayors wealth at the beginning and end of their terms. In this case, I set the problem as a canonical (2×2) differences-in-differences, with a single pre- and post- treatment period. In both cases, to further increase similarity between treatment and control, I always consider specifications in which I ensure identical mean values along a series of covariates by weighting the data using entropy balancing (Hainmueller, 2012; Hainmueller and Xu, 2013). The summary statistics after balancing are reported in Appendix A. In this case, the equation is estimated as follows:

$$Log(Wealth)_i = \alpha + \beta(Audited \times Post) + \lambda Audited + \delta Post + \epsilon_s \tag{2}$$

Because my results have a logged outcome variable, two caveats are in order. First, the coefficient of interest, β, captures the differential growth rate in mayors' wealth (from 2008 to 2012), measured in terms of percentage points. Second, it is important to notice that I rely on a slightly modified identification assumption for my difference-in-differences. Namely that, absent treatment, treated units would perceive *growth rates* for the outcome variable identical to those of control units. Thus, balancing also helps to mitigate any concerns that the logged values of the dependent variables may introduce a bias in our difference-in-differences of growth rates because their initial values are substantially different, or because wealth may increase at different rates for different starting levels (see e.g. McConnell, 2024; Roth and Sant'Anna, 2023). Finally, because there is a single election window in this case, robust standard errors are clustered at the state level alone.

For both cases, while previous research suggests that these audits have persistent effects in reducing corruption in the municipality, it is not clear whether the effects are persistent for new *mayors*. Thus, I always report results both including and excluding municipalities audited in the past, though it is important to highlight that the mayors themselves have never been audited.

3.2 Results

First, I report the main results on the effect of the corruption audit on mayor's reported wealth, measured at the end of their first term, using the larger sample in Table 2. I start by simply comparing the mean wealth of mayors in the treated and control groups of the same election window (2004-2008 or 2008-2012). The two subsequent columns add state fixed effects, and two sets of controls.[9] This process is done for the full set of municipalities (columns 1-3) and then continue with only never-treated municipalities in columns 4-6. The last two columns are again simple differences in means using only never-treated municipalities, but ensuring identical pre-treament covariate balance using the same list of "baseline" and "full" controls in the entropy-balancing process, instead of including them as covariates in the regression.

[9] "Baseline" controls include mayor's age, education, gender, and marital status, as well as their margin of victory, the municipal GDP per capita, population, whether it has a state court branch, and the number of *Bolsa Família* recipients (a cash transfer program for low-income population), used as a measure of poverty. "Full" controls add voter turnout, log donations per capita, the share of total donations from companies, individuals, from the mayor's party, and the share self-financed.

In all cases, I find that mayors in audited municipalities report greater wealth at the end of their first term, and the effect is always significant at the 5% level. They suggest that the average audited mayor's wealth is around 15.3% to 18.3% ($\approx exp(0.168) - 1$) larger than their counterparts that have not been audited.

Next, I report analogous results for the stricter sample looking exclusively at the second election window, for which I also observe mayor's initial wealth (see Table 3). Thus, in this case, I am able to estimate the *change* in reported wealth. As it can be seen across all specifications, there are no significant differences in pre-treatment wealth between treated and untreated mayors. Similarly to previous results, I start with all municipalities in the first column, and continue with only never-treated municipalities in subsequent ones. Columns 3 to 5 include baseline, full, and full with initial wealth in the entropy balance weighting process.

The results indicate that audited mayors report that their wealth growing by 33.51 ($\approx exp(0.289) - 1$) to 44.9 percentage points faster than mayors in the control group over four years. Overall, these results are somewhat larger in magnitude because treated mayors start from a lower (although not significantly different) initial wealth. However, these results become insignificant without the inclusion of measures of campaign finance (column 3), suggesting that this effect may be only observed conditional on municipalities having identical voter turnout and campaign donations characteristics. It is also insignificant with the inclusion of the dependent variable (pre-treatment) in the entropy-balance weighting (column 5), thus assuring identical initial wealth between treatment and control. Since they are also less precisely estimated and not always significant, they provide more nuanced evidence that audited mayors report greater changes in their reported wealth.

Indeed, these results seem puzzling, at least at face value. Previous research has emphasized that these audits effectively reduce corruption in audited municipalities (Avis et al., 2018); if mayors were getting richer due to corruption, this exogenous change in corruption should have made then less rich. Yet, they suggest that audited mayors became richer following the audit. Hence, a natural question to ask is what potential mechanisms are driving this increase in reported wealth.

4 Mechanisms and Alternative Explanations

I consider two potential mechanisms. First, that the increase in wealth is driven by positive multipliers of audit program in the local economies, which may lead to mayor's wealth increasing along the municipality GDP as a whole. Most closely to this idea is the work of Colonnelli and Prem (2022), who find an increase in the number of establishments, especially in government-reliant sectors, following one of these corruption audits.[10] Second, one may ask whether the main results reflect a true increase in wealth at all. Indeed, a potential explanation for an increase in *reported* wealth is reporting itself, whereby mayors choose to disclose further assets following the audit. I provide several tests for both hypothesis.

4.1 GDP and Wealth Effects

Fortunately, this hypothesis can be directly tested by measuring wealth relative to GDP. To be precise, I divide the log of reported wealth by the log of municipal GDP. To re-estimate the results for the complete sample, I regress this new variable on the treatment dummy, while controlling for the initial (log) GDP. For the balanced results, I include log GDP in the list of covariates used in the balancing process. For the strict sample, the difference-in-differences (DiD) follow the structure of Table 3. In this case, the DiD coefficient will capture the change in wealth relative to the change in GDP.

The results for the full and strict samples are reported in Tables 4 and 5, respectively. In both cases, they are significant less often, and with much smaller point-estimates. Table 4 reports magnitudes of either 1.1 or 1.2% greater wealth at the end of first term for audited mayors. Likewise, Table 5 reports coefficients suggesting audited mayors'

[10]More generally, Pavlik et al. (2023) provides robust evidence that reductions in corruption lead to a 20 to 25 percent increase in living standards over 10 years, in a panel of 122 countries from 1980 to 2015.

wealth growing by 2.5 to 3.2 percentage points in excess of that of control mayors over four years.[11] By comparing each to their respective initial estimates, the change in GDP explains anything from 92.8% of the difference in reported wealth as the end of their first term (Table 2 vs. Table 4), to as much as 94% difference in wealth growth rates (Table 3 vs. Table 5) among significant coefficients. However, it even potentially discards any abnormal change at all, given the insignificant ones.

4.2 Reporting Effects

Another natural question to ask given the main results above is whether they reflect a true increase in wealth or simply stem from a reporting effect, whereby mayors disclose further assets following the audit. Ideally, to answer this question, one would need a set of units with observable variation in corruption, but that did not face an audit. This would allow one to directly compare changes in wealth in low-and high-corruption municipalities, separated from the effect of the audit. A significant challenge is that we do not observe corruption in control municipalities.

Fortunately, however, I can exploit variance in treatment *timing*. That is, I look at municipalities audited in lottery 37, which happened on October 8th – the day immediately following the 2012 elections. Thus, mayors in this lottery could not use information about the audit to choose how much information to disclose, and thus can serve as a placebo test.

I directly compare the audited municipalities audited before the election to those audited after. Because in this case we observe variation in corruption across both treatment (audited after the election) and control (audited before the election), I test this in two ways. First, I simply regress the final wealth on the amount of corruption found in the audit and a dummy for whether the audit happened *after* the election. Second, I focus on the "strict" sample and perform the placebo test on the change in wealth. In the

[11]The "large" sample coefficients capture differences in percentage terms. The "strict" sample DiD coefficients capture differences in growth rates, expressed in terms of percentage points.

former, the dummy itself is the coefficient of interest, while in the former we focus on the DiD coefficient. In both cases, a negative value would suggest that those municipalities audited *after* the election has less incentive to report gains in wealth.

The first results are reported in Table 6. An advantage of this method is that we can control for the amount of corruption found in the audit. This is done in Panels A through C with for different measures of corruption. In all cases, despite never being significant – likely due to the small number of observations in lottery 37 – the point estimates are almost always negative.

In turn, the DiD results focusing on the second window are reported in Table 7. Here the advantage is that I can control for the baseline change in wealth in municipalities audited before the election, because their initial wealth is observable. The findings are similar to those for the large sample, with all specifications again containing negative point estimates. The only main difference is that one specification (column 2) is now significant. Together, these results weight in favor of the hypothesis that without the correct timing of the audit, mayors have no incentive to disclose wealth.

There is also anecdotal evidence supporting this practice. Here, I focus on the second window in which I observe mayors assets both before and after the audit. One audited mayor in the state of Minas Gerais declared nine bank accounts, investment portfolios and shareholdings following the audit. Another mayor in Santa Catarina state reported seven investments and bank accounts balanced that had not been reported before. Similarly, a mayor in Rio Grande do Sul disclosed checking and savings accounts, ownership of treasury bonds, and a 50% ownership in his wife's company that had not been disclosed prior to the audit.

Similar evidence arises for their reported stock-holdings in companies, mostly local businesses. I manually identify each and every stock-holding reported by the mayors. This process reveals treated mayors reporting 24 new stock-holdings. Eight of them could not be identified. Ten of them are minor stock-holdings in popular credit cooperatives. I searched the remaining company names and their tax numbers on the Federal Revenue

System (*Receita Federal*) database. Two-thirds of these refer to companies that already existed prior to 2008, but had not been reported then. The best example is that of a mayor in the state of Pará, who declared owning zero companies in 2008, but that he owned three in 2012. One such company that takes his name was founded in 1994 according to the *Receita Federal* database. A recent article in the local press also describes him as a "successful businessman, with more than 20 footwear stores" in three different states.[12] At least anecdotally, audited mayors are now reporting assets that can be proven to be already owned when they reported their initial wealth.

To increase the confidence that this evidence is not driven by changes in reporting practices, I take a more systematic look across treated and control mayors. I implement a simple, yet effective machine-learning model to identify nine categories of assets in the data. The model is able to correctly classify assets as either (i) real estate assets, (ii) vehicles, or (iii) cash and liquid assets 99, 97, and 93% percent of the time. These are by far the three most important categories, representing 87% of the observations and 85.8% of the candidate's portfolio value. I detail this process in Appendix C and discuss the results using this data here.

I focus on bank accounts because they are easily identifiable in the data and is unrealistic to assume that a given mayor has zero bank accounts. In 2008, 25% of the control group mayors and 26.66% of the treated mayors declared at least one bank account. However, in the 2012 elections the share of mayors declaring bank accounts rose to 49.44% in the control group, and to 60% among treated mayors. Likewise, the average number of bank accounts stayed exactly the same in the control group (at 3.12), but increased by some 40%, from 2.25 to 3.16, among treated mayors. I report these results more formally in Table 7. Panel A considers a dummy for whether mayors reported a bank account, Panel B looks at the (log) number of bank accounts, and Panel C explores the (log) of total value reported in bank accounts. Results are mostly significant in Panels A and B, but never in Panel C, although always positive in all panels. The point estimates suggest

[12]See https://www.seculodiario.com.br/politica/ex-prefeito-faz-confissao-de-pobreza em-recurso-para-se-livrar-da-justica/. Accessed on November 14, 2024.

that audited mayors were more 8.4 to 21.7 percentage points more likely to report a bank account, and the number of reported bank accounts they reported grew by 19.7 to 23.5 percentage points in excess of the change among non-audited mayors.

While the amounts associated with these accounts cannot fully explain the change in wealth over four years, a back-of-the-envelope calculation suggest that they can explain the residual not explained by GDP. As reported in Table 1, audited mayors' mean wealth rose from R$ 422,829.5 to R$ 812,725.0, or by R$ 389,895.5. If we subtract the baseline change implied by the difference-in-differences,[13] audited mayors' wealth grew by R$ 276,881.6 in excess of control mayors. As reported in Section 4.1, the change in GDP in audited municipalities explains around 90% of this change. Thus, some R$ 27,688 (or 10%) still need some explaining. A difference-in-differences of the amount reported in bank accounts suggests the that audited mayors grew by R$ 83,685.5 $= \Delta T - \Delta C =$ (122,143.8 – 23,557.05) – (37,647.56 – 24,367.79), which would be sufficient to explain the remaining. Of course, this evidence is tentative, at most. But it is nevertheless consistent with the idea that the audits induce mayors to report more assets.

4.3 A Simple Framework of Rent-Seeking

Finally, I suggest interpreting mayors' behavior using a simple model of rent-seeking, in the spirit of (e.g.) Tullock (2008) and Weaver (2021). Such model predicts that if mayors normally extract rents from their time in office, any change in the amount of rents available caused by an exogenous reduction in corruption should induce changes in the politicians willingness to pay for their seat. I argue that this would be captured by the amount of funds they invest in their own campaign. All else equal, given that the audits reduce overall corruption, we should expect their willingness to pay to decrease when running for reelection; otherwise, if they actually got richer, they should be willing to pay more for their seat.

[13] Control mayors grew from R$ 587,511.4 to R$ 700,525.3, or R$ 389,895.5. Thus, DiD $\approx$ 389,895.5 – 113,013.9 = R$ 276,881.6

In Table 8, I estimate a reduced-form specification of this model set up as difference-in-differences of the percentage of total campaign funds that were self-financed (Panel A) and the log of the amount self-financed (Panel B). Because in this case I have two windows for the difference-in-differences estimation, I set the problem as two canonical (2×2) DiDs which are then "stacked" to reach an unbiased estimate of average treatment effect, as in Cengiz et al. (2019). Crucially, this method avoids any problematic comparisons created by staggered treatment (Goodman-Bacon, 2021; Baker et al., 2022).[14] While the results are never significant, the point estimates are unanimously negative. At least, they partially favor the prediction that mayors would become less willing to "pay" for their position over the alternative, given that the audits effectively reduced the amount of rents available (Avis et al., 2018).

5 Conclusion

Earlier studies have found that high profile politicians tend to profit from political office. However, most of the evidence supporting this finding comes from establish democracies, where politicians profit from appointments in corporate boards *after* leaving office. Little is known from politicians of lower ranks, and even less for developing countries, where corruption often permeates public administration, potentially increase the returns for office by substantial margins.

I study how a reduction in corruption led by a randomized audit program can affect the evolution of wealth of elected mayors in Brazil. This empirical strategy relies on a exogenous source of variation in corruption, and compares only mayors to mayors, thus overcoming two key challenges from previous studies involving the identification plausible counterfactuals and concerns about endogeneity.

I find that audited mayors reported being richer following the audits. Given this

[14] This implies that municipalities treated in the second window may potentially serve as controls in the first window, but no municipality treated in the first appears in second window. This is also assured by the fact that mayors face a two-term limit, such that no mayor running for the first time in 2004 could run for a third time in 2012.

initially-puzzling result, one will naturally ask whether they actually got richer. My results suggest indeed they mostly did. However, this can be explained by the local economy benefiting from the audit (Colonnelli and Prem, 2022), and mayors benefited along the greater change. Municipal GDP explain as much of 94% of the excess change in audited mayors' wealth.

Additionally, I find a perhaps unintended consequence of the audit. Other than greater accountability in municipal budgets, I report suggestive evidence that the audits also induced greater accountability of mayors' personal finances, whereby mayors are more likely to report previously-undisclosed assets. I focus on the reporting of bank accounts, because it is unreasonable to assume that a given mayor had exactly none. In support of this idea, I find that the share of mayors reporting at least one bank account more than double among audited mayors, while increasing by only 68% in the control group, which discards mere changes in reporting practices. Overall, this study extends a large literature on the benefits of fighting corruption, and sheds new light on the determinants of private gains from political office in developing countries.

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Tables and Figures

Table 1: Summary Statistics

	All Elections		$\|Margin\| \leq 5$		$\|Margin\| \leq 2.5$		Diff.
	Winner	Runner-Up	Winner	Runner-Up	Winner	Runner-Up	(W–R-Up)
	8107	5914	2167	1195	1128	668	[t-stat.]
Wealth Variables							
% Wealth Growth$_{t \to t+4}$	9.029	8.297	8.311	7.608	6.187	8.599	2.41
	(60.719)	(60.349)	(43.855)	(34.388)	(34.709)	(37.806)	(1.27)
Initial Reported Wealth$_t$	1.156M	0.869M	1.455M	0.872M	1.620M	0.948M	-0.67M
	(8.861)	(4.691)	(14.188)	(2.765)	(14.155)	(3.414)	(-1.48)
Mayor Demographics$_t$							
Age	47.361	48.332	47.445	48.912	47.512	48.954	1.44***
	(14.528)	(10.593)	(10.327)	(10.523)	(10.587)	(10.556)	(2.79)
College Degree	0.523	0.521	0.532	0.490	0.535	0.478	-0.06**
	(0.499)	(0.500)	(0.499)	(0.500)	(0.499)	(0.500)	(-2.37)
Female	0.119	0.120	0.130	0.131	0.137	0.133	-0.00
	(0.323)	(0.325)	(0.336)	(0.337)	(0.344)	(0.340)	(-0.25)
Married	0.733	0.695	0.730	0.723	0.740	0.743	0.00
	(0.443)	(0.460)	(0.444)	(0.448)	(0.439)	(0.438)	(0.11)
Municipality$_t$							
Has State Court	0.309	0.408	0.329	0.326	0.352	0.323	-0.03
	(0.462)	(0.491)	(0.470)	(0.469)	(0.478)	(0.468)	(-1.24)
Transfers per Capita	2735.588	2548.693	2682.515	2670.423	2664.454	2745.256	80.80
	(1653.656)	(1665.669)	(1642.229)	(1631.529)	(1689.764)	(1673.364)	(0.96)
Political and Election Characteristics$_t$							
Same Party of Governor	0.081	0.065	0.075	0.080	0.080	0.081	0.00
	(0.273)	(0.246)	(0.264)	(0.272)	(0.271)	(0.273)	(0.08)
Share Seats from Party	0.233	0.228	0.211	0.227	0.203	0.226	0.02***
	(0.158)	(0.150)	(0.146)	(0.152)	(0.142)	(0.152)	(3.20)
Party concentration (HHI)	0.235	0.217	0.233	0.234	0.227	0.238	0.01**
	(0.106)	(0.099)	(0.106)	(0.104)	(0.104)	(0.103)	(2.19)
Cost per vote (R)	22.603	22.114	23.971	22.151	24.224	22.596	-1.63
	(18.259)	(17.658)	(19.725)	(16.023)	(20.695)	(16.447)	(-1.57)

Notes: ***, **, * denote p<0.01, 0.05, 0.10. All variables measured in the election year (t).

Table 2: The effect of audit on reported wealth at the end of first term (2008 or 2012)

Dependent Variable:	Log of Reported Wealth (End of First Term)							
Sample:	All Municipalities			Never Treated				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Audited	0.142**	0.153**	0.149**	0.157**	0.159**	0.149**	0.148**	0.168**
	(0.0623)	(0.0627)	(0.0701)	(0.0695)	(0.0699)	(0.0701)	(0.0716)	(0.0676)
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No
State FE	No	Yes	Yes	No	Yes	Yes	No	No
Controls	No	Baseline	Full	No	Baseline	Full	No	No
Balanced	No	No	No	No	No	No	Baseline	Full
N	3,501	3,484	3,020	3,028	3,012	3,020	3,010	2,549
$N_{W1}[C,T]$	[2,592, 385]	[2,577, 383]	[2,218, 337]	[2,260, 357]	[2,246, 355]	[1,943, 313]	[2,243, 356]	[1,892, 301]
$N_{W2}[C,T]$	[479, 45]	[479, 45]	[426, 39]	[381, 30]	[381, 30]	[339, 26]	[381, 30]	[328, 28]
R^2	0.005	0.167	0.207	0.005	0.158	0.207	0.003	0.004

Notes: Robust standard errors clustered at the state-window level in parenthesis. The units of observation are mayors elected for the first time in either 2004 or 2008 (pre-audit period) who run again for reelection in 2008 or 2012 (post-audit period), creating two election windows. The treatment group includes mayors that have been audited at some point along their first term, and the control group are their pairs that have not. See Table A3 for a list of variables included in each entropy balancing process.

Table 3: The effect of corruption audit on mayors' reported wealth growth, difference-in-differences estimates (2008 to 2012)

Dependent Variable:	Log of Reported Wealth				
	(1)	(2)	(3)	(4)	(5)
Audited	-0.412	-0.452	-0.416	-0.299	0.000
	(0.244)	(0.310)	(0.308)	(0.357)	(0.384)
Post	0.452***	0.491***	0.515***	0.485***	0.657***
	(0.0524)	(0.0542)	(0.0630)	(0.0769)	(0.0989)
Audited × Post	0.289**	0.328*	0.305	0.371*	0.199
	(0.130)	(0.181)	(0.183)	(0.182)	(0.203)
Entropy-Balanced	No	No	Baseline	Full	Full, with Initial Wealth
Sample	All Munic.	Never-Treat.	Never-Treat.	Never-Treat.	Never-Treat.
N	1,008	790	790	650	650
N [Control, Treated]	[459, 45]	[365, 30]	[365, 30]	[299, 26]	[299, 26]
R^2	0.034	0.041	0.079	0.072	0.078

Notes: Robust standard errors clustered at the state-window level in parenthesis. The units of observation are mayors elected for the first time in either 2004 or 2008 (pre-audit period) who run again for reelection in 2008 or 2012 (post-audit period), creating two election windows. The treatment group includes mayors that have been audited at some point along their first term, and the control group are their pairs that have not. See Table A3 for a list of variables included in each entropy balancing process.

Table 4: The effect of audit on reported wealth at the end of first term as % of GDP (2008 or 2012)

Dependent Variable:	Ratio of Log of Reported Wealth (End of First Term) to Log GDP							
Sample:	All Municipalities			Never Treated				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Audited	0.012**	0.013**	0.012*	0.012*	0.012*	0.011	0.011	0.012
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.009)
Log GDP_{t-4}	-0.064***	-0.071***	-0.068***	-0.065***	-0.072***	-0.067***		
	(0.002)	(0.005)	(0.008)	(0.002)	(0.005)	(0.008)		
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No
State FE	No	Yes	Yes	No	Yes	Yes	No	No
Controls	No	Baseline	Full	No	Baseline	Full	No	No
Balanced	No	No	No	No	No	No	Baseline	Full
N	3,498	3,484	3,020	3025	3,012	2,621	3,010	2,549
$N_{W1}[C,T]$	[2,592, 385]	[2,577, 383]	[2,218, 337]	[2,260, 357]	[2,246, 355]	[1,943, 313]	[2,243, 356]	[1,892, 301]
$N_{W2}[C,T]$	[479, 45]	[479, 45]	[426, 39]	[381, 30]	[381, 30]	[339, 26]	[381, 30]	[328, 28]
R^2	0.360	0.421	0.466	0.369	0.426	0.477	0.001	

Notes: Robust standard errors clustered at the state-window level in parenthesis. The units of observation are mayors elected for the first time in either 2004 or 2008 (pre-audit period) who run again for reelection in 2008 or 2012 (post-audit period), creating two election windows. The treatment group includes mayors that have been audited at some point along their first term, and the control group are their pairs that have not. See Table A3 for a list of variables included in each entropy balancing process.

Table 5: The effect of corruption audit on mayors' reported wealth growth, difference-in-differences estimates (2008 to 2012)

Dependent Variable:	Ratio of Log of Reported Wealth to Log of GDP				
	(1)	(2)	(3)	(4)	(5)
Audited	-0.037*	-0.051	-0.033	-0.022	-0.000
	(0.022)	(0.032)	(0.031)	(0.032)	(0.035)
Post	0.028***	0.032***	0.032***	0.031***	0.046***
	(0.004)	(0.004)	(0.005)	(0.006)	(0.008)
Audited × Post	0.025**	0.026	0.026	0.032*	0.017
	(0.012)	(0.017)	(0.017)	(0.016)	(0.018)
Entropy-Balanced	No	No	Baseline	Full	Full, with Initial Wealth
Sample	All Munic.	Never-Treat.	Never-Treat.	Never-Treat.	Never-Treat.
N	1,008	790	790	650	650
N [Control, Treated]	[459, 45]	[365, 30]	[365, 30]	[299, 26]	[299, 26]
R^2	0.014	0.019	0.031	0.030	0.036

Notes: Robust standard errors clustered at the state level in parenthesis. See Table A3 for a list of variables included in each entropy balancing process.

Table 6: Placebo test: The "reporting effect" of the audit on reported wealth at the end of first term (2008 or 2012), audited before vs. after the election

Dep. Variable:	Log of Reported Wealth (End of First Term)					
Sample:	All Municipalities			Never Treated		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log Instances of Corruption per Capita						
Audited After	-0.226	-0.0236	-0.330	-0.168	0.168	-0.0977
	(0.317)	(0.266)	(0.348)	(0.323)	(0.228)	(0.381)
Log Corruption per cap.	-0.307***	-0.116	-0.122	-0.251***	-0.0647	-0.113
	(0.087)	(0.132)	(0.150)	(0.0885)	(0.151)	(0.164)
R^2	0.002	0.302	0.385	0.004	0.313	0.384
Panel B: Log Instances of Corruption per Service Order						
Audited After	-0.125	0.026	-0.282	-0.131	0.190	-0.049
	(0.283)	(0.269)	(0.339)	(0.288)	(0.205)	(0.374)
Log Corruption per S.O.	0.102	0.016	-0.052	0.178	0.057	0.013
	(0.092)	(0.090)	(0.117)	(0.108)	(0.107)	(0.126)
R^2	0.007	0.300	0.385	0.021	0.313	0.383
Panel C: Log Instances of Corruption						
Audited After	-0.150	-0.031	-0.327	-0.165	0.162	-0.098
	(0.284)	(0.265)	(0.357)	(0.297)	(0.230)	(0.385)
Log Corruption	-0.036	-0.096	-0.075	-0.015	-0.060	-0.084
	(0.092)	(0.116)	(0.138)	(0.110)	(0.131)	(0.151)
R^2	0.058	0.302	0.386	0.045	0.313	0.385
Election FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	No	Yes	Yes
Controls	No	Baseline	Full	No	Baseline	Full
N	242	241	211	203	202	178

Notes: Robust standard errors clustered at the state-window level in parenthesis. The units of observation are mayors elected for the first time in either 2004 or 2008 (pre-audit period) who run again for reelection in 2008 or 2012 (post-audit period), creating two election windows. The treatment group (Audited After) includes mayors that have been audited in lottery 37, after the elections, and the control group are their that have been audited in earlier audits before the election. See Table A3 for a list of variables included in each entropy balancing process.

Table 7: Placebo test: The "reporting effect" of the audit on reported change in wealth (2008 and 2012), audited before vs. after the election

Dep. Variable:	Log of Reported Wealth (End of First Term)					
Sample:	All Municipalities			Never Treated		
	(1)	(2)	(3)	(4)	(5)	(6)
Audited After	0.751	0.851*	0.090	0.213	0.599**	-0.000
	(0.461)	(0.460)	(0.513)	(0.311)	(0.265)	(0.219)
Post	0.741***	0.824***	0.614***	0.819***	1.000***	0.514*
	(0.134)	(0.137)	(0.173)	(0.176)	(0.181)	(0.276)
Audited After × Post	-0.843	-0.926*	-0.716	-0.316	-0.496	-0.010
	(0.510)	(0.509)	(0.566)	(0.311)	(0.321)	(0.382)
Entropy-Balanced	No	Baseline	Baseline w/ Init. Wealth	No	Baseline	Baseline w/ Init. Wealth
N	106	106	106	72	72	72
R^2	0.076	0.114	0.051	0.102	0.231	0.097

Notes: Robust standard errors clustered at the state level in parenthesis. The units of observation are mayors elected for the first time in 2008 (pre-audit period) who run again for reelection in 2012 (post-audit period). The treatment group (Audited After) includes mayors that have been audited in lottery 37, after the elections, and the control group are their that have been audited in earlier audits before the election. See Table A3 for a list of variables included in each entropy balancing process.

Table 8: The effect of corruption audit on mayors' reported bank accounts, difference-in-differences estimates (2008 to 2012)

Dependent Variable:	Reported Bank Account				
Panel A:	(1)	(2)	(3)	(4)	(5)
Audited	0.007	-0.028	-0.056	-0.086	-0.000
	(0.038)	(0.065)	(0.063)	(0.072)	(0.060)
Post	0.094***	0.090***	0.102***	0.091**	0.129***
	(0.024)	(0.029)	(0.036)	(0.042)	(0.040)
Audited × Post	0.084	0.176*	0.164	0.217**	0.179*
	(0.060)	(0.100)	(0.096)	(0.094)	(0.093)
R^2	0.020	0.024	0.060	0.076	0.105
Dependent Variable:	Log Number of Reported Bank Accounts				
Panel B:	(1)	(2)	(3)	(4)	(5)
Audited	0.008	-0.036	-0.064	-0.106	-0.005
	(0.056)	(0.074)	(0.074)	(0.073)	(0.056)
Post	0.114***	0.110***	0.126***	0.133**	0.164***
	(0.026)	(0.032)	(0.042)	(0.052)	(0.047)
Audited × Post	0.108	0.196*	0.180*	0.211**	0.180*
	(0.085)	(0.100)	(0.097)	(0.096)	(0.097)
R^2	0.008	0.010	0.054	0.054	0.087
Dependent Variable:	Log of Total Amount Reported in Bank Accounts				
Panel C:	(1)	(2)	(3)	(4)	(5)
Audited	0.018	0.357	0.058	-0.957	0.151
	(0.615)	(0.880)	(0.857)	(1.024)	(0.956)
Post	1.999***	2.090***	2.238***	2.264***	2.819***
	(0.261)	(0.346)	(0.392)	(0.437)	(0.542)
Audited × Post	0.304	0.482	0.334	1.067	0.511
	(0.841)	(1.256)	(1.190)	(1.092)	(1.164)
R^2	0.042	0.048	0.057	0.082	0.103
Entropy-Balanced	No	No	Baseline	Full	Full, with Dep. Var.
Sample	All Munic.	Never-Treat.	Never-Treat.	Never-Treat.	Never-Treat.
N	1,008	790	790	650	650
N [Control, Treated]	[459, 45]	[365, 30]	[365, 30]	[299, 26]	[299, 26]
R^2	0.014	0.019	0.031	0.030	0.036

Notes: Standard errors clustered at the state level in parenthesis. See Table A3 for a list of variables included in each entropy balancing process.

Table 9: The effect of corruption audit on mayors' reported bank accounts, difference-in-differences estimates (2008 to 2012)

Dependent Variable:	% of Campaign Funds that were Self-Financed			
Panel A:	(1)	(2)	(3)	(4)
Audited	-0.005	-0.012	-0.007	-0.003
	(0.013)	(0.013)	(0.012)	(0.012)
Post	0.065***	0.066***	0.070***	0.071***
	(0.011)	(0.011)	(0.011)	(0.012)
Audited × Post	-0.014	-0.014	-0.018	-0.009
	(0.020)	(0.022)	(0.022)	(0.021)
R^2	0.009	0.009	0.009	0.010
Dependent Variable:	Log of Self-Financed Funds			
Panel B:	(1)	(2)	(3)	(4)
Audited	0.232	0.110	0.069	-0.035
	(0.207)	(0.212)	(0.216)	(0.200)
Post	2.182***	2.209***	2.233***	2.155***
	(0.149)	(0.160)	(0.172)	(0.183)
Audited × Post	-0.313	-0.257	-0.302	-0.014
	(0.222)	(0.241)	(0.246)	(0.233)
R^2	0.066	0.068	0.062	0.067
Entropy-Balanced	No	No	Baseline	Full, with Dep. Var.
Sample	All Munic.	Never-Treat.	Never-Treat.	Never-Treat.
N	6,509	5,567	5,534	4,952
N_{W1} [Control, Treated]	[2,462, 374]	[2,148, 346]	[2,132, 345]	[1,811, 293]
N_{W2} [Control, Treated]	[480, 46]	[353, 29]	[353, 29]	[304, 27]

Notes: Standard errors clustered at the state level in parenthesis. See Table A3 for a list of variables included in each entropy balancing process.

Appendix

A Summary Statistics

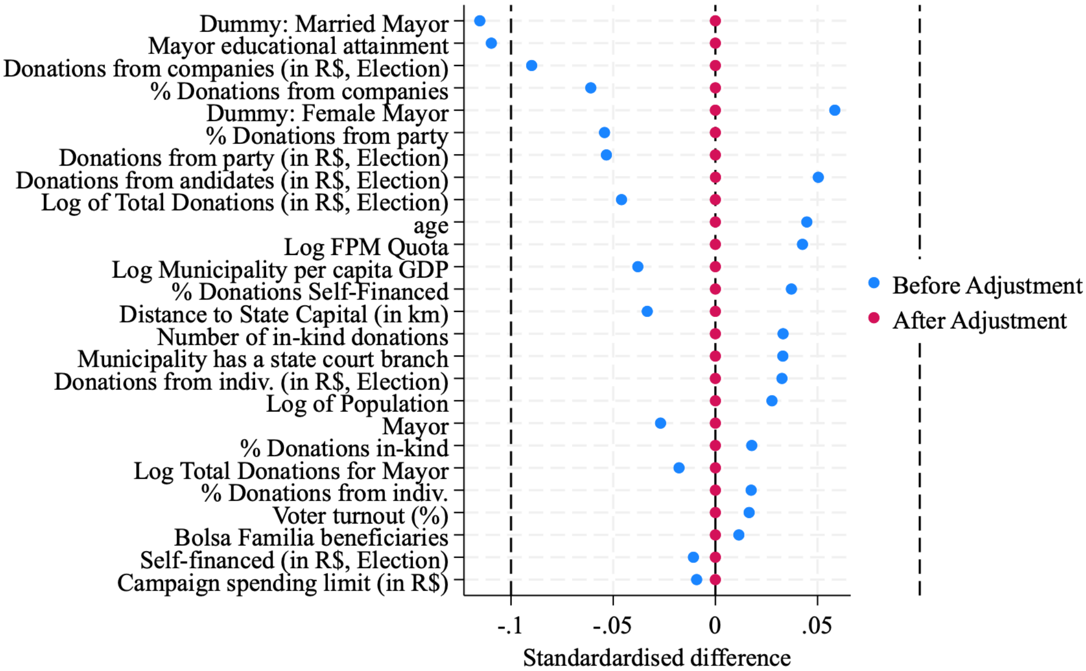

Figure A1: Covariate balance before and effort entropy balancing (2004-2012 sample)

Table A1: Pre-Treatment Summary Statistics after Entropy Balancing (2004-2012)

Variable	Mean (Treated) (N = 353)	Mean (Control) (N = 2,395)	p-value ($\Delta \neq 0$)
Mayor Characteristics			
Age	45.760	45.760	1.000
Education	2.120	2.120	1.000
Female	0.080	0.080	1.000
Married	0.820	0.820	1.000
Municipality Characteristics			
GDP per capita (R$)	7,319.590	7,319.590	1.000
Population	12,001.481	12,001.481	1.000
Has State Court Branch	0.340	0.340	1.000
Bolsa Familia Recipients (per 1,000)	1.220	1.220	1.000
Distance to State Capital (km)	250.910	250.910	1.000
Election and Donation Characteristics			
Voter Turnout (%)	0.880	0.880	1.000
Total Donations	23,386.906	23,386.906	0.999
Share Donations Self-Financed	0.270	0.270	1.000
Share Donations form Individuals	0.340	0.340	1.000
Share Donations from Companies	0.170	0.170	1.000
Share Donations from Party	0.010	0.010	1.000

Notes: Education is a discrete indicator = 0 if mayor is literate with no formal education or has incomplete elementary school; = 1 if complete elementary or incomplete high school; = 2 if complete high school or incomplete college; and = 3 if mayor has college degree.

Table A2: Pre-Treatment Summary Statistics after Entropy Balancing, by treatment window

Window 1: 2004-2008 Cohort Variable	Mean (Treated) (N = 323)	Mean (Control) (N = 2,053)	p-value ($\Delta \neq 0$)
Mayor Characteristics			
Age	45.840	45.840	1.000
Education	2.100	2.100	1.000
Female	0.080	0.080	1.000
Married	0.820	0.820	1.000
Municipality Characteristics			
GDP per capita (R$)	7,018.974	7,018.974	1.000
Population	11,755.791	11,755.791	1.000
Has State Court Branch	0.320	0.320	1.000
Bolsa Familia Recipients (per 1,000)	1.100	1.100	1.000
Distance to State Capital (km)	253.830	253.830	1.000
Election and Donation Characteristics			
Voter Turnout (%)	0.880	0.880	1.000
Total Donations	22,277.813	22,277.813	0.999
Share Donations Self-Financed	0.270	0.270	1.000
Share Donations form Individuals	0.340	0.340	1.000
Share Donations from Companies	0.170	0.170	1.000
Share Donations from Party	0.000	0.000	1.000

Window 2: 2008-2012 Cohort Variable	Mean (Treated) (N = 30)	Mean (Control) (N = 342)	p-value ($\Delta \neq 0$)
Mayor Characteristics			
Age	45.000	45.000	1.000
Education	2.330	2.330	1.000
Female	0.130	0.130	1.000
Married	0.730	0.730	1.000
Municipality Characteristics			
GDP per capita (R$)	10571.569	10571.569	1.000
Population	15169.589	15169.589	1.000
Has State Court Branch	0.570	0.570	1.000
Bolsa Familia Recipients	2.430	2.430	1.000
Distance to State Capital (km)	219.480	219.480	1.000
Election and Donation Characteristics			
Voter Turnout (%)	0.890	0.890	1.000
Total Donations	35,496.348	35,496.348	1.000
Share Donations Self-Financed	0.280	0.280	1.000
Share Donations form Individuals	0.420	0.420	1.000
Share Donations from Companies	0.150	0.150	1.000
Share Donations from Party	0.100	0.100	1.000

Notes: Education is a discrete indicator = 0 if mayor is literate with no formal education or has incomplete elementary school; = 1 if complete elementary or incomplete high school; = 2 if complete high school or incomplete college; and = 3 if mayor has college degree.

Table A3: Declared Wealth Results: Pre-Treatment Summary Statistics after Entropy Balancing (2008-2012)

Baseline Specification Variable	Mean (Treated) ($N = 30$)	Mean (Control) ($N = 360$)	p-value ($\Delta \neq 0$)
Mayor Characteristics			
Age	45.700	45.700	1.000
Education	2.370	2.370	1.000
Female	0.130	0.130	1.000
Married	0.730	0.730	1.000
Municipality and Election Characteristics			
Margin of Victory	19.530	19.530	1.000
GDP per capita (R$)	11,956.14	11,956.14	1.000
Population	14,162.073	14,162.073	1.000
Has State Court	0.470	0.470	1.000
Bolsa Familia Recipients (per 1,000)	2.330	2.330	1.000

Full Controls Variable	Mean (Treated) ($N = 28$)	Mean (Control) ($N = 334$)	p-value ($\Delta \neq 0$)
Mayor Characteristics			
Age	45.320	45.320	1.000
Education	2.430	2.430	0.999
Female	0.070	0.070	1.000
Married	0.710	0.710	1.000
Election and Donation Characteristics			
Voter Turnout (%)	0.890	0.890	1.000
Margin of Victory	20.250	20.250	1.000
Donations per capita	3.095	3.095	1.000
Share Donations from Companies	0.160	0.160	1.000
Share Donations Self-Financed	0.230	0.230	1.000
Share Donations form Individuals	0.450	0.450	0.999
Share Donations from Party	0.100	0.100	1.000
Municipality Characteristics			
GDP per capita (R$)	9.774	9.774	1.000
Population	15123.825	15123.825	1.000
Has State Court	0.500	0.500	1.000
Bolsa Familia Recipients (per 1,000)	2.460	2.460	1.000
Distance to State Capital (km)	227.870	227.890	1.000

Notes: Education is a discrete indicator = 0 if mayor is literate with no formal education or has incomplete elementary school; = 1 if complete elementary or incomplete high school; = 2 if complete high school or incomplete college; and = 3 if mayor has college degree.

B Election and Campaign Finance Rules

Within my period of analysis (2004-2012), elections in Brazil were financed through a mix of donations from individuals, companies, and public funds. Individuals could contribute up to 10 percent of their annual income, but there was no limit on donations to one's own campaign (Avis et al., 2022). Companies could contribute up to 2 percent of their gross annual revenues. Parties also have access to public funding (during this period, from the *Fundo Partidário*), which they can then allocate to candidates, especially in Executive races, but during this time it amounted to a small share of overall contributions (Bourdoukan, 2010).[15]

There was no limit on campaign spending. Only candidates themselves (and parties on their behalf) can spend resources for their campaigns. They must create a specific bank account that will serve exclusively for campaign spending, and every donation and expenditure must be reported within 72 hours. Candidates can receive donations starting in August 15, which gives them roughly 45 days to spend their funds until the election day on the first weekend of October. After election day, any unpaid bills are converted into electoral debts, and mayors can raise additional funds only to pay these debts.

Donations from companies were banned by the Supreme Court in 2015, following a unconstitutionality complaint by the Federal Council of the National Bar Association (*Conselho Federal da Ordem dos Advogados do Brasil*), with the argument that they promote corruption among election officials. In the winning argument leading to a 8-to-3 vote, Judge Luiz Fux argued that:

> "Donations by [private] legal entities to election campaigns, rather than reflecting any political preferences, denote a strategic action by these major donors in their eagerness to strengthen relations with public authorities, often forming alliances that lack a republican spirit."[16]

[15] In addition, these are coded inconsistently across elections in the donation data, which is why I chose to omit it from the analysis. However, donations from public funds have increased substantially over the years, especially with the creation of another fund (*Fundo Especial de Financiamento de Campanha*, FEFC) in 2017. It increased from R$ 1.7 billion in 2018 to R$ 4.9 billions (close to 1 billion USD) in the 2022 and 2024 elections.

[16] Direct Action of Unconstitutionality/ADI n. 4.650/DF – see Guerra Filho (2017) for an analysis of the decision.

C Reporting Assets

Ideally the assets of mayors would be individually identifiable. This would allow one to compare lists of assets and how they change in value – but this approach is not feasible given the data. A second-best approach is to group them into categories (e.g. real estate assets) and compare changes in relevant categories. A key difficulty with this approach is that categories are not readily available for the 2008 data.

To overcome this problem, I use a simple machine-learning algorithm to classify the 2008 assets into 10 categories. First, I group the 50 original categories provided by the 2012 data into ten general categories based on similarity. This aggregation procedure is provided in Table C2. I start with the data for all candidates in the 2012 local elections containing 895,663 assets, 90% of which I use to train the model and the remaning 10% for validation.

I use a stacked-ensemble (SE) machine-learning model consisting of a first-stage Multinomial Naive Bayes (MNB) model, followed by a Random Forest (RF). The MNB is an easy to implement model and widely used for text classification (e.g. Aggarwal and Zhai, 2012). While the RF model is common in stacked-ensemble strategies (e.g. Priya Varshini et al., 2021). The idea is simple: the NB model first predicts a category for an asset and then the RF model uses not only the underlying description of the asset but also the classification of suggested by the NB model to generate a final classification. The process of combining models is useful because each model compensates for particular weaknesses of each other and generates better predictions (e.g. Bajari et al., 2015).[17] This is precisely the case here, where the SE model performs better than any of them separately.

The final model is able to correctly predict 92% of assets correctly within the validation data. But crucially, it is especially able to predict the most relevant categories, both in terms of number of assets and of total value. The model reaches a 99% precision for real state assets, 97% for vehicles, and 93% of cash and liquid assets. These are the three largest categories in the sample, totaling 87% of the observations, and 85.8% of the candidate's portfolio value. While the model performs somewhat worse (85%) for "Business Interests and Shares," which is more important than "Cash and Liquid Assets" in terms of share of total value (6.54%), randomization of treatment assignment should suggest that prediction errors are evenly distributed across our treatment and control

[17] See also Athey and Imbens (2019) for applications in economics.

groups. This information is summarized in Table C1.

Table C1: Model Performance by Category, Ordered by Observation Share

Category	**Precision**	**Recall**	**F1-score**	**Support**	**% Sample**
Real Estate Assets	0.99	0.96	0.97	39,854	44.49
Vehicles	0.97	0.96	0.96	29,520	32.97
Cash and Liquid Assets	0.93	0.87	0.90	8,523	9.52
Business Interests & Shares	0.85	0.87	0.86	4,891	5.46
Investment and Financial Assets	0.65	0.55	0.60	3,131	3.49
Other Assets & Credits	0.49	0.56	0.52	2,604	2.91
Loans and Credit	0.52	0.74	0.61	565	0.63
Uncategorized	0.24	0.58	0.34	303	0.34
Precious Assets	0.39	0.50	0.44	147	0.16
Rights and Licenses	0.01	0.41	0.02	29	0.03
Overall Accuracy			0.92		

Notes: The F1-score is the harmonic mean of precision and recall.

Table C2: Grouped Category Mapping from Original to Aggregated Groups (with English Translation)

Real Estate Assets	**Cash and Liquid Assets**
House	Bank deposit in domestic account
Apartment	Bank deposit in foreign account
Commercial building	Cash in national currency
Residential building	Cash in foreign currency
Other real estate	Other demand deposits and cash
Office or suite	Other linked credits and savings
Store	Savings account
Land	
Bare land	
Construction	
Improvements	
Savings for construction or acquisition of real estate	
Investments and Financial Assets	**Business Interests & Shares**
Fixed income application (CDB, RDB)	Other business shares
Other investments	Capital shares
Futures market, options and term	Shares (including those from telephone lines)
Capitalization fund	Telephone line
Investment fund quotas	
Financial investment fund (FIF)	
Other funds	
VGBL - Free Benefit Generator Life	
Vehicles	**Loans and Credit**
Ground motor vehicle (car, truck, motorcycle, etc.)	Unawarded consortium
Vessel	Loan-derived credit
Aircraft	Alienation-derived credit
Precious Assets	**Rights and Licenses**
Gold (financial asset)	Author,Äôs rights and patents
Jewelry, paintings, antiques, etc.	Mining rights and similar
Club membership and similar	Special licenses and concessions
	PAIT plan and savings account
Other Assets & Credits	
Asset related to self-employment	
Other assets and rights	
Other movable assets	
Leasing	