The Private Gains from Political Office: Evidence from Close Mayoral Elections in Brazil

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Abstract

Conventional wisdom suggests that the returns from politics are substantial, but empirical evidence tends to center on highly influential politicians in developed countries. I study whether low-level politicians obtain private returns from political office in the context of a developing country, using data on disclosures of wealth for Brazilian mayors and a regression discontinuity design along close election races. While I find only modest evidence of private returns from political office for the *average* mayor, I find substantial returns for mayors (relative to runner-ups) in municipalities with low institutional quality (7.5 to 10.5 p.p. a year) and without the presence of a local radio news station (6 to 8 p.p. a year). In contrast, municipalities with local oversight councils and high financial and managerial capacity, as well as those with local media presence, are effective in curbing political enrichment, leading to normal, or even negative, returns relative to runner-ups.

JEL Codes: D72, D73, K42, K16, J45.

Keywords: Private Gains, Political Office, Mayors, Brazil, Corruption

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1 Introduction

"A politician who is poor, is a poor politician."[1] This phrase, coined by the late Mexican politician and businessman, Carlos Hank Gonzáles, summarizes the idea that political careers can be lucrative. The literature has supported this notion, focusing on two plausible explanations for political enrichment. First, politicians gain through the well-known "revolving doors" of politics, *after* leaving office; the second is simply the large official salaries that come with the job.[2] However, these explanations describe the enrichment of highly-influential politicians in established democracies.[3] They describe the political career of someone like Bill Clinton, the former president of the United States who accumulated 90 million dollars through lucrative speeches (CNN, 2013), rather than the corruption, patronage, and rent-seeking that Hank Gonzáles, the former mayor of Toluca, presumably had in mind. This paper studies the latter — the wealth accumulation of local politicians in the context of a developing country, Brazil.

Local politicians may not get the attention of national headlines, but they still control a wealth of resources and often have discretion in how to use them. Within the context of a developing country, and in particular one with a history of corruption, this combination likely enables political enrichment. Due to the decentralized nature of public spending in Brazil, mayors have substantial local influence and are responsible for allocating large sums of resources, in conjunction with city councilors (Ferraz and Finan, 2011; Bologna and Ross, 2015; Avis et al., 2018). Over a single term, the mayor of an average municipality controls approximately 25 million dollars in federal transfers alone. These local politicians are often personally in charge of hiring, closely oversee public contracts, and may be subject to lax levels of public scrutiny, inviting malfeasance.

[1] See The New York Times (2001).

[2] The first has been primarily documented in the United States (Palmer and Schneer, 2016; Diermeier et al., 2005) and the United Kingdom (Eggers and Hainmueller, 2009), with returns for gubernatorial or national-level legislative seats in the magnitude of 400,000 to 1,700,000 USD. The latter channel, characteristic of continental European parliaments, is consistent with findings for MPs in Finland (Kotakorpi et al., 2017), Sweden (Berg, 2020b), and Germany (Peichl et al., 2013), but much smaller in magnitude.

[3] In part, this is due to data availability, as these democracies are more likely to enforce asset disclosure laws for politicians. For an excellent review, see Djankov et al. (2010).

The literature concerning private returns for local-level political offices is sparse. Kotakorpi et al. (2017) find a small return, of about 1,000 euros a year, for Finnish local councilors, and Berg (2020a) finds no returns for Swedish ones.[4] Yet both studies examine returns in the low-corruption settings of Nordic countries. Corruption in Brazil, on the other hand, often permeates public offices. A federal audit program of a random sample of 983 municipalities found an average of 55 instances of corruption per municipality, and none were completely free of corruption (Avis et al., 2018). An instructive example comes from a mayor in the state of Rio Grande do Sul, who was convicted of illicit enrichment for constructing a public building in a parcel of land he owned and leased to the municipality.[5] In line with this idea, Fisman et al. (2014) find that the returns to political office for state-level officials India are larger in states with higher perceived corruption. This suggests that all levels of political office may offer lucrative returns in countries that have considerable corruption, yet we have no systematic evidence of abnormal returns for local-level politicians in these environments – this is my focus.

To investigate whether local politicians experience (above-normal) private returns in Brazil, I rely on official electoral data from over 10,000 mayoral elections between 2008 and 2024. Since 2008, this data includes information on all candidates' wealth. Although self-reported, it is based on the candidate's latest tax return and can be cross-checked against official tax data by electoral authorities. Information about the candidates becomes publicly available, including their asset disclosure, several demographic characteristics (e.g., age, education, occupation, and marital status), and financing information regarding their campaign.

If mayors were randomly assigned into office and if their wealth was observable at the beginning and end of their terms, estimating their returns would be an easy task. Randomization would assure that the average mayor had the same initial wealth as the

[4]Instead of measuring the immediate gains like these studies, Bertoni et al. (2023) estimate the effect the long-run net present value of winning a mayoral election in Italy at 35,000 euros over 15 years, which they interpret as a modest effect.

[5]See https://gepam.adm.br/ead/mprs-ex-prefeito-e-condenado-por-enriquecimento-ilicito-ao-beneficiar-se-de-esquema-para-construcao-de-predio-publico-em-terreno-proprio-2/

average candidate, and thus we could simply compare the wealth of mayors at the end of their term to that of non-selected candidates. Of course, voters do not randomly choose mayors. The results of election races presumably depend on voter preferences about candidate qualities (e.g. charisma, integrity, etc.)[6] and several other factors, including campaign funding and political connections. If these factors also correlate with wealth growth, or if elected candidates differ substantially from non-elected ones along these characteristics, the estimates would be biased. Thus, to estimate the returns from political office, I exploit variation from close election races using a regression discontinuity design. This identification strategy posits that within a small enough margin of victory, being elected is not due to any observable or unobservable difference, offering as-good-as-random variation around the cutoff (Eggers et al., 2015; Cattaneo et al., 2015). Thus, on average, runner-ups who barely lost provide a plausible counterfactual for what would have happened to the wealth of winning candidates had they not won, allowing one to estimate the causal effect of winning mayoral office on wealth accumulation.

I find modest evidence that being elected to office generates above-normal private returns for the *average* winner of a close election. In my full sample, the wealth of both mayors and runner-ups grows at around 11-12% a year, leading to marginal differences that are neither statistically nor economically significant across several specifications. A stricter sample of matched winner and runner-up pairs from the same municipality and election shows some evidence of positive returns, around 4-5 percentage points a year, but these effects are not particularly robust. However, these are average effects, including mayors and runner-ups across all municipalities. Yet, municipalities in Brazil have significant heterogeneity and, of most importance to this paper, this heterogeneity can result in very different accountability environments at the local level. I argue that this local accountability plays an important role in enabling or curbing political enrichment. Thus, the modest average effect may not be representative of the returns in municipalities with low (or high) levels of accountability.

[6] For voter preferences in Brazil, see de Albuquerque et al. (2025).

I test for heterogeneity by splitting the sample using two different measures of accountability. My first indicator of accountability comes from an index of municipal institutional quality that tracks municipal oversight councils, financial capacity, and management capacity. This measure captures the ability of constituents to monitor local policy implementation and implement checks on the mayor's behavior. The second accountability measure is an binary indicator of whether the municipality has a local radio station, a potential proxy for media oversight. Local media presence has been found to be an important oversight mechanism in Brazil, especially by exposing corrupt politicians (Ferraz and Finan, 2008; Avis et al., 2018). I find that both measures of accountability are important determinants of the returns to public office.

Specifically, in municipalities with low institutional quality, namely those lacking mechanisms of public oversight and low financial and managerial capacity, the returns from office are substantial: mayors' wealth grows by 7.5 to 10.5 percentage points in excess of that experienced by runner-ups. Similarly, I find that mayors' wealth growth is 6 to 8 p.p. higher relative to that of runner-ups in municipalities without the presence of local radio news stations. In sharp contrast to these municipalities with low accountability, the returns from office in municipalities with high accountability – high institutional quality or having a local news station – are either negligible or negative, suggesting an effective check to politicians' wealth accumulation. Moreover, there is little difference in observable characteristics of mayors *across* the two sub-samples (low versus high accountability environments), indicating that this result is not driven by a selection effect through voter preferences, whereby voters only select "good" mayors in accountable environments, and "bad" mayors otherwise.

This paper proceeds as follows. The next section reviews previous research on the private returns from political office and highlights the contributions of this paper. Section 3 provides the background context of Brazilian mayoral elections and details the data. Section 4 describes my identification strategy. Section 5 reports the main results. Section 6 conducts a series of robustness checks. Section 7 concludes.

2 Previous Research

This paper contributes first and foremost to the literature on the private returns to the political office. This empirical question is embedded in a much broader literature aimed at understanding what leads candidates to run for office, which provides important context for why this is a crucial political problem. Indeed, financial incentives are a chief concern because they affect which candidates run for office (Besley, 2005; Caselli and Morelli, 2004; Messner and Polborn, 2004; Dal Bó et al., 2013; Kotakorpi and Poutvaara, 2011; Fedele and Giannoccolo, 2020), and also because they affect politicians' performance by shaping their incentives once in office (Barro, 1973; Buchanan, 1983; Ferraz and Finan, 2009; Messner and Polborn, 2004; Gagliarducci et al., 2010; Gagliarducci and Nannicini, 2013).

Subsequent work in this literature has recognized that official wages are only part of politicians' financial motivation, and multiple studies attempted to estimate the private returns to political office more broadly.[7] This literature has documented substantial financial gains from holding political positions, highlighting two primary explanations of enrichment. First, politicians benefit from post-office employment opportunities through the "revolving doors" of politics.[8] Second, politicians receive high official salaries during their tenure.[9] However, this literature has focused predominantly on national-level or senior political positions and highlighted mechanisms that are particularly relevant to

[7] As highlighted in Fisman et al. (2014), this literature connects to an emerging body of work attempting to identify hidden earnings of politicians and public servants (e.g. Braguinsky et al., 2014; Di Tella, 2007; Kim and Neshat, 2025), and the use of privileged information and preferential access through political connections (e.g. Chen and Kung, 2019; Ziobrowski et al., 2004, 2011).

[8] Palmer and Schneer (2016) find that almost half of former governors and senators serve on at least one board of publicly-traded corporations after leaving office. These board positions have an average compensation of US$250,000. Similarly, conservative members of parliament (MPs) earned an extra £250,000 through board positions (Eggers and Hainmueller, 2009). Using a career model, Diermeier et al. (2005) estimate that a seat in US House is worth US$600,000 and a seat in the US Senate is worth US$1,700,000. Estimates for returns during office are mixed, such as Lenz and Lim (2009) among U.S. House members between 1995 and 2005, and Querubin and Snyder (2009) between 1845 and 1875, except for the Civil War years.

[9] Kotakorpi et al. (2017) find that being elected to parliament in Finland increases earnings by 20,000 euros a year, or a 25% increase relative to not-elected candidates. Berg (2020b) reports comparable findings for Sweden, around 20%. Peichl et al. (2013) suggests an increase in earnings around 40-60% for German politicians, though relative to citizens with an executive positions instead of not-elected candidates as the previous two studies.

developed democracies. I expand this literature by examining returns to office for local-level politicians in the context of developing country, uncovering alternative mechanisms for political wealth accumulation consistent with rent-seeking and corruption that operate during, rather than after, political tenure.

Existing research on the returns to office for local-level politicians has found either very small or no effects at all, as the studies of city councilors in Finland (Kotakorpi et al., 2017) and Sweden (Berg, 2020a) mentioned above. However, these are among the least corrupt countries in the world.[10] My focus on rent-seeking and corruption as mechanisms of political enrichment highlights why returns to office may occur even for low-level politicians in developing countries, and connects this paper to the extensive literature on the value of political connections.[11]

Related research has shown that mayors can secure employment and business opportunities for relatives in the private sector (Amore and Bennedsen, 2013; Fafchamps and Labonne, 2017; Gagliarducci and Manacorda, 2020). However, as discussed in Eggers and Hainmueller (2009), this literature has predominantly examined the benefits that accrue to firms from political relationships (Faccio, 2006; Fisman, 2001), rather than the direct financial gains to politicians themselves, although notable exceptions have studied the returns of firms owned by politicians (e.g., Truex, 2014; Jones et al., 2025). This paper addresses this gap by measuring the direct financial returns to holding political office for local level politicians in contexts where corruption and rent-seeking are relevant institutional factors.

[10] For instance, in one of the few known corruption cases in Nordic countries, Peter Brixtofte was found guilty of improper use of public funds while serving as the mayor of Farum, Denmark. He bought personal computers for every children in the city and spent more than 300,000 USD in travel and food expenses, including bottles of wine surpassing 1,250 USD. Although a court found that his actions were not for pure personal gain, the mere fact that he mismanaged public funds was sufficient to send him to prison. The court further ruled that Brixtofte had to pay back all the money he spent, as well as court costs, reaching a total of 2 million USD. Among other things, his house had to be sold to cover the amount due (Langsted, 2012). The clear message is that corruption (or even "mismanagement" in this case) is strictly punished according to the law.

[11] Studies have documented substantial returns to political connections for firms in a wide variety of countries, including Brazil (Claessens et al., 2008; Lazzarini and Musacchio, 2010; Bonomo et al., 2015; Guerra, 2023), Indonesia (Fisman, 2001), Italy (Akcigit et al., 2023; Cingano and Pinotti, 2013; Gagliarducci and Manacorda, 2020), China (Truex, 2014; Chen and Kung, 2019). See also Faccio (2006) for cross-country evidence.

There are two studies that are closely related to this paper in examining returns from political office in the context of developing countries. Fisman et al. (2014) find that state-level politicians in India experience wealth growth 3-5% higher than runner-ups, with larger returns in states with higher corruption levels and for politicians with greater political experience. My study complements these findings by providing additional evidence that rent-seeking serves as a common mechanism of political enrichment in developing countries. Notably, I find similar magnitudes for the role of rent-seeking, despite significant differences between Brazil's and India's political systems. In turn, Alston et al. (2009) was, to my knowledge, the first to study the returns to political office in Brazil. They construct a state-level index of checks and balances and show that the average wealth increase of state legislators is higher in states with in states that lack institutional constraints using a random-effects model. Nevertheless, my study differs from both in several important aspects.

First, I focus on local rather than state-level politicians, demonstrating that positive returns from office exist even at the lowest levels of the political hierarchy. Second, relative to Alston et al. (2009), I employ individual-level data and a regression discontinuity design to provide credible causal estimates of returns from office.[12] Most importantly, my study offers new evidence on how local accountability affects political enrichment. Recent work has examined the role of media in shaping electoral accountability (Myers, 2025), including in Brazil (Ferraz and Finan, 2008, 2011; Boas and Hidalgo, 2011; Avis et al., 2018), my paper establishes an explicit connection between local media presence and politicians' wealth accumulation.[13] This contribution bridges the literatures on private returns to office, media accountability, and political selection, showing how local institutional features are instrumental in curbing or enabling political enrichment, regardless of selection mechanisms highlighted in previous literature.

[12]Their study, while pioneering, relies on two repeated cross-sections pooling average wealth variation within 27 states, limiting causal interpretation.

[13]This also echoes broader cross-country evidence on role of accountability (e.g., Lederman et al., 2005) and the press (e.g., Mahmood and Pavlik, 2025), showing that it also holds at subnational levels.

3 Background and Data

3.1 Mayoral Elections and Reported Wealth

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 (simple plurality). In municipalities above this threshold, a second-round runoff is held unless a mayor gets 50 percent plus one of the votes in the first round (majority). Mayors may serve a maximum of two consecutive terms.[14]

Brazilian mayors are in charge of local public spending, having substantial discretion of how to allocate funds across several local initiatives, which invites misuse and, more critically, corruption (Ferraz and Finan, 2011; Bologna and Ross, 2015; Avis et al., 2018). From 2008 to 2024, the period covered in my sample, municipalities received R$ 207 billion a year in federal transfers (in 2024 *reais* $\approx$ 35 billion USD), or an average of roughly 6.3 million USD a year for each of the 5,570 munipalities. Put differently, over their four-year term, the average mayor in Brazil oversees the allocation of some 25 million dollars in federal transfers alone. Ferraz and Finan (2011, p. 1281-82) provides clear examples of how mayors misuse these resources. In the municipality of Paranhos, around 70,000 reais were used to implement a rural electrification project that directly benefited the mayor's farm. In Itapetinga, the mayor's brother won 12 out 16 school lunch contracts because the calls for bids were posted one hour before the deadline.[15]

Since the 1990s, candidates in Brazil must report their wealth to the Superior Electoral Tribunal (*Tribunal Superior Eleitoral*, henceforth TSE). However, for the purposes of this paper, data on mayor's reported wealth is available from 2008 onward, when wealth data became digitally recorded. 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

[14] Appendix C provides additional details about election and campaign finance rules.

[15] At the federal level, Boas et al. (2014) finds that companies focused on public-works projects stand to gain significantly from political donations–receiving, on average, at least 14 times the value of their donations in contracts.

will create a self-reported declaration of assets and submit it through a software program called CANDex, which enabled the digitization of these records. The user must input the assets following the standard asset classification codes from a tax return, and the software is designed to mimic a tax filling. Figure C1 shows the user interface of the tab for reporting wealth. In principle, they have little incentive to cheat because TSE can cross-reference the declaration with the Federal Revenue (*Receita Federal*) database, and significant mismatches can lead to a rejection of their candidacy and criminal charges for electoral false identity, punishable by up to five years in prison (Articles 348 and 350 of the Electoral Code - Law 4,737/1965).[16] After candidates are registered, information on their wealth and several other characteristics – including age, marital status, gender, education, and occupation – becomes publicly available in a user-friendly website accessible to all voters and the press. There is even an unofficial application that converts this data into a convenient mobile app called *DivulgaCand*.[17]

Table 1 reports the characteristics of candidates and of their municipalities. The average candidate's wealth in my sample is R$ 877,473 (around USD 160,000), but winners tend to be 18% richer than runner-ups (around 932,000 vs. 789,000). Among elections decided by a margin of victory of less than 5%, this difference falls to 5.3%, which is not significant (p=0.614).

Because candidates are only required to report their wealth when registering their candidacy (i.e., before the election), and not at the end of their term, I can only observe the wealth of candidates at the end of an election window for those that run again. This has two implications. One is that my sample includes more winners than runner-ups because I include all candidates that run again, regardless of whether their direct opponent also runs again, and incumbents are more likely to run for reelections.[18] The

[16] For instance, President Lula da Silva's asset disclosure to TSE was used to convict him for corruption in the famous Carwash Operation, in a case related to the construction company OAS that led to his arrest in 2018 (O Globo, 2018).

[17] The official website is `https://divulgacandcontas.tse.jus.br/`, and Figure C2 shows an example of a candidate. Figure C3 shows the unofficial mobile app

[18] In addition to this sample, I report results for a sample of matched pairs (Section 5.1), in which both winner and runner-up necessarily come from the same election-municipality.

other is that among those elected, I look exclusively at first-term mayors. Observing wealth at the end of their second term would require them to run for a third consecutive time, but they cannot run as there is a limit of two *consecutive* terms.[19] Incidentally, this becomes an important feature of the identification strategy (described in detail below) because it rules out alternative explanations given by reelection incentives (e.g. Ferraz and Finan, 2011) or incumbent advantage (De la Cuesta and Imai, 2016).[20]

Table 1: Summary Statistics

	All Elections		|Margin| ≤ 5		|Margin| ≤ 1		Diff. (1)
	Winner	Runner-Up	Winner	Runner-Up	Winner	Runner-Up	(W–R-Up)
N	6324	3888	1674	1399	330	323	[*t*-stat.]
Wealth Variables							
% Wealth Growth	10.764	10.396	10.990	10.927	11.288	12.188	-2.14
	(26.680)	(30.268)	(27.194)	(28.793)	(27.046)	(30.862)	[-0.26]
Initial Wealth	0.934M	0.793M	0.902M	0.856M	1.019M	0.827M	0.68M
	(2.446M)	(2.235M)	(2.423M)	(2.664M)	(2.855M)	(1.889M)	[0.61]
% Adj. Wealth Growth	3.923	6.277	4.117	6.280	6.213	7.942	0.22
	(35.417)	(38.079)	(34.814)	(37.953)	(33.824)	(37.751)	[0.02]
Adj. Initial Wealth	0.625M	0.561M	0.578M	0.556M	0.597M	0.579M	0.20M
	(1.634M)	(1.373M)	(1.434M)	(1.420M)	(1.154M)	(1.371M)	[0.61]
Mayor Characteristics$_t$							
Age	47.905	48.460	48.020	48.596	48.006	48.579	-0.68
	(15.424)	(10.291)	(10.233)	(10.170)	(10.361)	(10.037)	[-0.26]
College Degree	0.530	0.510	0.539	0.508	0.521	0.533	0.04
	(0.499)	(0.500)	(0.499)	(0.500)	(0.500)	(0.500)	[0.27]
Female	0.091	0.108	0.105	0.096	0.106	0.099	0.09
	(0.288)	(0.310)	(0.306)	(0.295)	(0.308)	(0.299)	[1.05]
Married	0.753	0.738	0.749	0.756	0.800	0.786	-0.06
	(0.431)	(0.440)	(0.434)	(0.430)	(0.401)	(0.410)	[-0.72]
Rookie candidate	0.680	0.610	0.675	0.599	0.679	0.594	0.11
	(0.466)	(0.488)	(0.469)	(0.490)	(0.468)	(0.492)	[0.88]
Municipality$_t$							
IQIM Score	3.067	3.029	3.100	3.033	3.111	3.057	0.11
	(0.546)	(0.536)	(0.568)	(0.560)	(0.611)	(0.570)	[0.85]
Has Local Radio	0.218	0.223	0.263	0.214	0.306	0.245	0.13
	(0.413)	(0.416)	(0.440)	(0.410)	(0.462)	(0.431)	[1.19]
Has State Court	0.324	0.346	0.345	0.335	0.391	0.344	0.05
	(0.468)	(0.476)	(0.475)	(0.472)	(0.489)	(0.476)	[0.40]
Transfers per Capita	2663.711	2476.989	2609.318	2444.550	2648.286	2466.484	-53.55
	(1621.715)	(1569.012)	(1624.348)	(1436.382)	(1689.965)	(1497.181)	[-0.14]
Political and Election Characteristics$_t$							
Share seats mayor party	0.235	0.236	0.213	0.223	0.201	0.222	-0.05
	(0.156)	(0.149)	(0.145)	(0.143)	(0.140)	(0.142)	[-1.47]
Party concentration	0.235	0.224	0.232	0.226	0.227	0.224	-0.03
	(0.103)	(0.094)	(0.103)	(0.094)	(0.104)	(0.087)	[-1.19]
Invest. in own campaign	31.144K	30.563K	35.285K	34.367K	45.666K	35.667K	28.50K
	(91.276)	(84.097)	(103.687)	(108.903)	(134.311)	(98.092)	[0.97]

Notes: All variables measured in the election year (t), except for wealth growth, which measures the annualized return rate from t to t+4. Adjusted wealth accounts for reporting omissions – see Section 6.1 for details. The *Diff.* column reports RD estimates as covariate balance tests around the cutoff. ***, **, * respectively denote $p \leq 0.01, 0.05, 0.10$.

[19] Section A.5 describes this issue, and Figure A8 provides a detailed timeline.

[20] However, it is crucial to highlight that this does not mean that they are *first-time* mayors. They could have been mayors in a previous election, provided this was not the election immediately before, or have political experience in other elected offices. In Section 6.2, I leverage prior political office-holding status to identify "seasoned" vs. "rookie" politicians and investigate the role of political experience.

An important caveat is that electoral law does not specifically establish *which* assets must be declared.[21] Instead, this has been decided over time by jurisprudence from electoral courts. Real estate assets and vehicles became required and 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 houses they own, and the make, model, year, and license plate numbers of the cars they drive.[22] 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 has a bank account, only around 25% of the candidates report having one. In Section 6.1, I leverage this information to look exclusively at wealth from well-reported assets as a robustness check.

3.2 Local Accountability

To understand the role of local accountability in shaping the returns from office, I collect additional data on municipal characteristics. It may be that in environments lacking oversight mechanisms and public scrutiny, mayors can successfully increase their wealth. I consider the role of institutional and media accountability. Specifically, I define municipalities with poor institutional accountability according to an index of municipal institutional quality (*Indicador de Qualidade Institutional Municipal*, IQIM) developed by the Ministry of Planning, Budget, and Management (MPOG) in 1999. It has three areas of equal weight, covering the degree political participation (especially through municipal oversight councils),[23] financial capacity, and management capacity, offering a broad

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

[22] 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 $" or "a sedan vehicle valued at $."

[23] Municipal councils (*conselhos municipais*) are committees created by law to advise on and propose policy initiatives in specific areas, such as health or education. They also oversee the implementation of these policies and may deliberate on the local budget. These councils usually include representatives from both the local government and civil society organizations. They are not the same as the city councils (*câmaras municipais*), which are the local legislative branch (Bastos and Bologna Pavlik, 2025).

measure political oversight.[24] I detail the specific variables in each area and their specific weights in Appendix A.

Additionally, I use a survey of municipal characteristics conducted by the Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística*, IBGE) to collect data on the presence of local news, defined by whether the municipality has a local radio station.[25] In Brazil, local radio has played a central role in local politics by serving as the primary medium for informing voters about politician behavior Ferraz and Finan (2008, 2011).[26] When a local radio station is absent, voters will only have access to regional news stations, which may not provide enough information about the candidates or political events in their municipality. Thus, the presence of a local media improves accountability by decreasing voters' monitoring costs. In my sample, 22% of the municipalities have a local radio news station.

4 Empirical Strategy

4.1 The Regression Discontinuity Design

The main contribution of this paper is identifying the causal impact of holding political office on private financial returns. The outcome of interest is the declared change in wealth over a four-year election window, in percentage terms, which I convert to yearly rates for convenience. Since we do not directly observe what would have happened to a winning mayor's wealth had they lost, the fundamental challenge with identifying this causal effect on wealth accumulation by politicians is finding a proper counterfactual. Electoral outcomes are not randomly determined, which means that politicians who win elections

[24]In the robustness checks, I also consider a state-level index of checks and balances (Alston et al., 2009), as well as municipalities with low judicial oversight (municipalities with no judge), low political competition (high concentration of votes across political parties), and high political rents (large sums windfall revenues per capita). In the context of Brazil, these institutional characteristics have been argued to be strong determinants of municipal corruption (Ferraz and Finan, 2011; Brollo et al., 2013), or wealth accumulation by politicians (Alston et al., 2009).

[25]This survey also provides the underlying data used to construct the IQIM.

[26]Not surprisingly, politicians in Brazil may attempt to gain control of radio stations to work in their favor (Boas and Hidalgo, 2011).

may differ systematically from those who lose, for example, in attributes like personal connections, entrepreneurial skills, or inherent financial advantages. These factors might simultaneously enhance their likelihood of electoral victory and independently contribute to their asset growth, leading to biased estimates if a simple comparison across the full sample is made.

To overcome this issue, I exploit variation within close elections using two regression discontinuity (RD) designs, which I detail below.[27] By far, RD designs are the most common strategy to deal with potential biases arising from differences in candidates characteristics, and nearly all previous studies on the private returns from political office rely on some type of RD design for their identification strategy (e.g., Eggers and Hainmueller, 2009; Palmer and Schneer, 2016; Kotakorpi et al., 2017; Berg, 2020a).[28]

The RD design relies on a running variable (X) – in this case, *margin of victory*, the vote-share difference between the winning candidate and the runner-up – with the treatment assigned at the cutoff ($c = 0$). To account for both simple plurality and majority votes, the margin of victory is defined as the difference in votes between the winner and the runner-up as a share of total votes; the cutoff is re-centered at zero, meaning that winners have positive margins and losers have negative ones. Candidates whose vote margin is just below zero constitute the *control group*, the runner-ups who narrowly lose and do not hold office. Conversely, those just above zero comprise the *treatment group*, which accede to office after narrowly winning. Treatment assignment thus follows a deterministic rule, where only candidates with margins equal to or greater than zero gain political office. Formally, identification of a local average treatment effect in the context of a RD design is given by:

[27] For formal reviews of the method and the literature, see Hahn et al. (2001), Imbens and Lemieux (2008), Lee and Lemieux (2010), Cattaneo et al. (2020), Cattaneo and Titiunik (2022).

[28] RD designs around close elections have also been used to study a wide array of research questions, including incumbent advantage (Lee et al., 2004; Butler and Butler, 2006; Lee, 2008; Cattaneo et al., 2015; Eggers et al., 2015), the effect of Islamic rule (Meyersson, 2014), and gendered differences in policy (Brollo and Troiano, 2016). A number of excellent reviews of the method have attested its validity relative to experimental benchmarks (Green et al., 2009; Chaplin et al., 2018), including in the context of close elections (Eggers et al., 2015; De la Cuesta and Imai, 2016; Hyytinen et al., 2018; De Magalhães et al., 2025).

$$\hat{\tau} = \mathbb{E}[Y_i(1) - Y_i(0)|X_i = c] = \lim_{x \downarrow c} \mathbb{E}[Y_i(1)|X_i = x] - \lim_{x \uparrow c} \mathbb{E}[Y_i(0)|X_i = x] \quad (1)$$

where Y_i denotes the outcome variable, wealth growth during an election period, and X_i is the running variable (margin of victory). Intuitively, RD designs of close elections leverage the idea that candidates who narrowly win elections are likely very similar to those who narrowly lose. By focusing exclusively on contests decided by a slim margin, the RD design poses that differences in unobserved characteristics between winners and losers become negligible.

While the nature of RD designs prevent direct testing of covariate imbalances – because we only observe control units below the cutoff and treated units above it – a common indirect test is to check for potential discontinuities along other predetermined variables. Intuitively, if we find that other variables also change sharply around the cutoff (e.g., winners are much richer than runner-ups) we would be less confident that changes in the outcome variable are being caused by the treatment assignment rather than by these other discrete changes.

More precisely, there are two potential identifying assumptions for RD designs (Cattaneo et al., 2017). The most common one, the *continuity assumption*, states that while treatment status (elected vs. non-elected) changes abruptly at the cutoff, conditional expectations of the average characteristics of the candidates must only change smoothly (Hahn et al., 2001; Cattaneo et al., 2017). Thus, strictly speaking, treatment and control units may have different values for a range of predetermined covariates; what matters is that the conditional expectations change continuously. I call this my *continuity design*.

Another assumption, proposed by Lee (2008), frames the RD design as a local randomized experiment. This framework proposes that, within a small enough window, who wins and who loses – the treatment assignment – is determined *as if random*. In this case, for the RD design to be valid, treatment must be unrelated to predetermined variables (Z_i) within the chosen window; in other words, this amounts to achieving covariate balance across both sides of the cut-off.[29] For a formal treatment, see Cattaneo et al. (2015, 2017). I call this my *local randomization design*. Importantly, since both the continuity and local randomization approaches can be seen as complementary to each other, they also serve as robustness checks to one another (Cattaneo et al., 2024).

In practice, estimating the treatment effect requires a few interrelated steps. First, one must choose the relevant bandwidth (h) around the cutoff (c). Second, one must fit the data separately on either side of the cutoff, which will depend on the bandwidth size and polynomial choice. Finally, one must choose the appropriate inference procedure.[30]

Below, in Figure 1, the left panel plots the distribution of margins of victory for the 10,212 candidates included in the main sample, of which 6,324 are winners, and 3,888 are runner ups.[31] The right panel plots a global fourth-degree polynomial as a first approximation to visually check for discontinuities in the outcome variable. While my formal results will zoom into a narrow window to obtain proper estimates, this informal exercise suggests that the treatment effect for the average mayor will be small, if any.[32]

4.1.1 Bandwidth Choice and Estimation

Bandwidth choice is widely recognized as the most important feature of a RD design (Cattaneo and Vazquez-Bare, 2017), and it involves a bias-variance trade-off. A large bandwidth (h) has lower variance, but it depends on the correct parametric specification of $\mathbb{E}[\ \cdot\ ]$. It also requires higher order polynomials, which have been shown to have

[29]Importantly, this does not mean that these predetermined covariates are entirely unrelated to treatment assignment. For example, it is clear that a candidate who spends ten times more has, all else equal, a greater change to win. Rather, the assumption states that the relationship ceases within the local randomization window: spending \$10,000 or \$10,001 will not affect the probability or winning (Cattaneo et al., 2024).

[30]As discussed in Cattaneo et al. (2020), one can also choose different kernels that weight the observations based on their distance to the cutoff. Aside from the uniform kernel (equal weights), two other common choices are the triangular and Epanechnikov kernels, which give less weight to observations farther from c, the first with a linear and the second with a quadratic decay. Typically, however, estimation and inference are not very sensitive to the particular choice of kernel used (Cattaneo et al., 2020).

[31]I exclude 8 observations that had implausibly low initial wealth (as low as zero), that yield implausibly high increases of wealth in percentage terms, of as much as 4,129% a year for wealth, and 17,329% a year for adjusted wealth; I also exclude another 18 mayors who had extremely high wealth ($\geq$ R\$ 50,000,000) which were outside of common support of the data.

[32]Figure A5 provides a similar plot for a sample of matched pairs. It suggests a small, but positive, return for winning mayors.

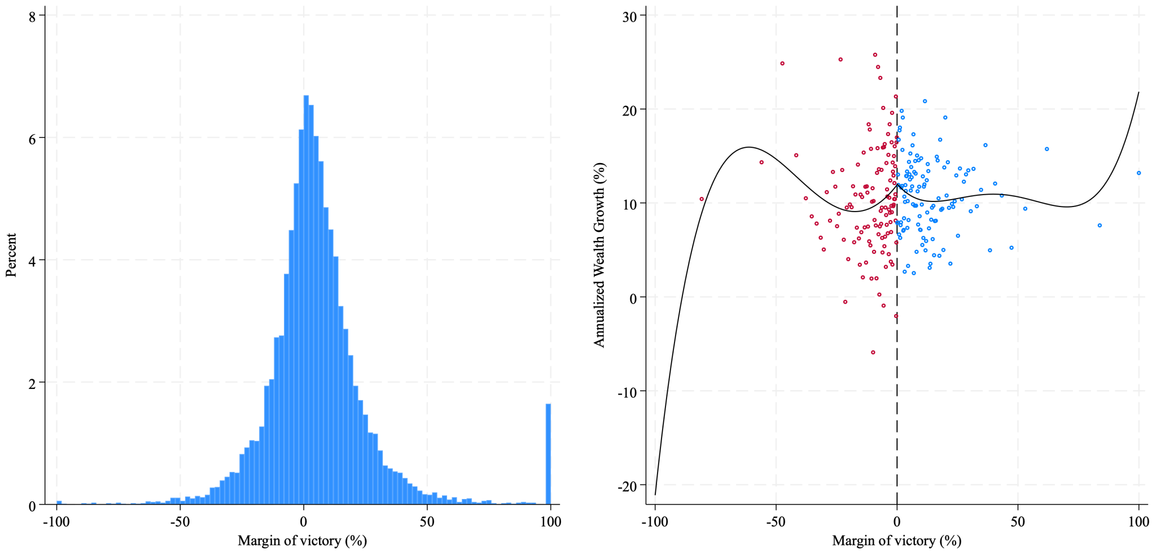

Figure 1: Sample distribution and global polynomial fit

Notes: Left panel: Distribution of margins of victory. Right panel: distribution of the outcome variable, annualized wealth growth (in %) on either side of the margin of victory. Each dot represent the mean of the outcome variable within a quantile-spaced bin. Number of bins ($J_- = 119; J_+ = 120$) chosen using a data-driven algorithm in order to "mimick" the variance of the overall sample (Calonico et al., 2015). Solid black line is a fourth-order global polynomial fit. Number of observations: $N_- = 3,886$; $N_+ = 6,326$.

unattractive properties for point estimation (Gelman and Imbens, 2019). Conversely, smaller bandwidths decrease misspecification error (or "smoothing bias"), as they require fewer parametric assumptions about $\mathbb{E}[\ \cdot\ ]$ in the local polynomial approximation. In the limit, one can estimate a simple difference in means on either side of the cutoff (as in, e.g., Angrist and Lavy, 1999; Fisman et al., 2014). However, they also increase the variance due to the small number of observations used in the estimation (Cattaneo et al., 2020). Optimal bandwidth methods have been proposed to provide a data-driven procedure to arrive at a bandwidth that minimize the bias-variance trade-off (Imbens and Kalyanaraman, 2012; Calonico et al., 2014, 2020).

For my *continuity design*, I start with a minimum squared-error (MSE) optimal bandwidth (Calonico et al., 2014, 2020), which provides relatively large windows that minimize the bias-variance trade-off. In this case, for the design to be valid, we simply need continuity in the conditional expectations; I provide discontinuity tests along a series of predetermined covariates. For the local polynomial estimation, I use both linear and quadratic fits, which have been shown to be more robust, and less sensitive to overfitting and boundary problems (Cattaneo et al., 2020; Gelman and Imbens, 2019). For my *local randomization design*, which requires as-good-as-random covariate balance, I use bandwidths of 5 and 1. Given the smaller bandwidth size, in this case I use a linear fit and a simple difference in means.

Figures 2 and 3 report the results of discontinuity tests for key demographic characteristics of candidates (initial wealth, age, and dummies for female, white, and college degree) within MSE-optimal bandwidth and a margin $\leq \pm 5$. The last column of Table 1 reports tests for all variables at $\leq \pm 1$ (also see Figure A1). I find no evidence of significant discontinuities around the cutoff, which strengthens the confidence in my identification strategy. Additionally, I estimate my results in a sample of matched pairs, which assures that both mayor and runner-up come from the same municipality, holding constant all unobservable municipal-level characteristics (Section 5.1).

4.1.2 Inference

Standard inference procedures from an OLS estimate will lead to invalid conclusions in the context of RD designs. In the standard RD design, where researchers rely on a local polynomial estimate, it treats the estimate as parametric, assuming that there is no misspecification error. As discussed in Calonico et al. (2014, 2020), this is especially troubling when using minimum squared error-optimal bandwidths are used, as this method explicitly recognizes that there is some bias while trying to minimize it. Thus, for MSE-optimal bandwidths, I use robust bias-corrected errors for inference, proposed by these authors to account for the bias term in the variance estimation.

In contrast, since the *local randomization* design is implemented in a much smaller window that restricts the number of observations, large-sample approximations from standard inference may behave poorly and lead to incorrect conclusions. Hence, for the smaller bandwidths, it is more appropriate to use finite-sample randomization inference methods (Cattaneo et al., 2015, 2016, 2017). As a rule of thumb, I report finite-sample Fisher's permutation p-values whenever the sample is smaller than 1,000 observations.

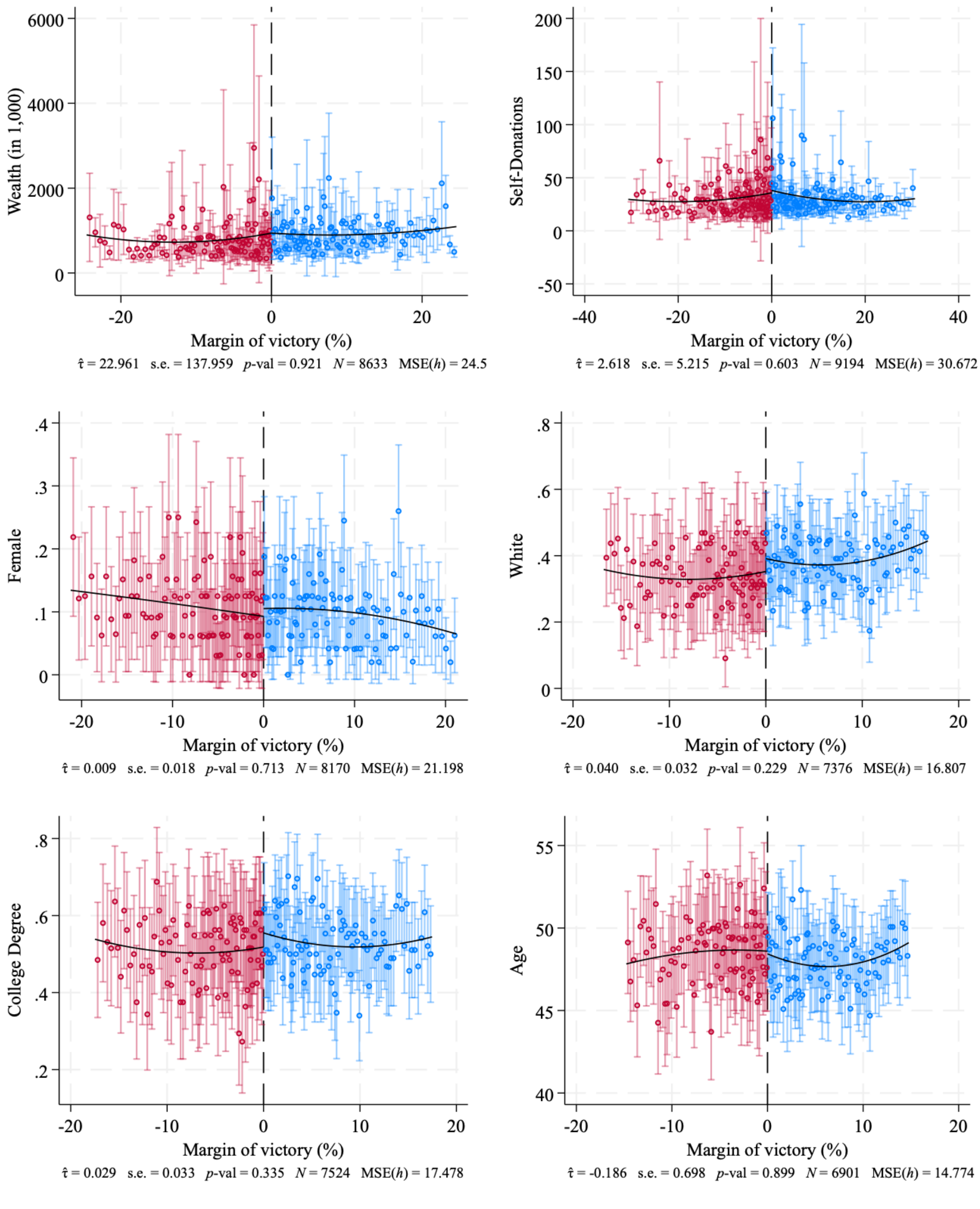

Figure 2: Covariate balance checks (MSE-Optimal bandwidth)

Note: This figure plots RD estimates for the six key candidate characteristics using local quadratic polynomial fit and a mean-squared error (MSE) optimal bandwidth (Calonico et al., 2014, 2020) as covariate balance checks for columns 1-2 of Table 2. Each dot represent binned outcome quantile means and vertical lines report 95% confidence intervals. Number of quantiles chosen using a data-driven algorithm in order to "mimick" the variance of the overall sample (Calonico et al., 2015).

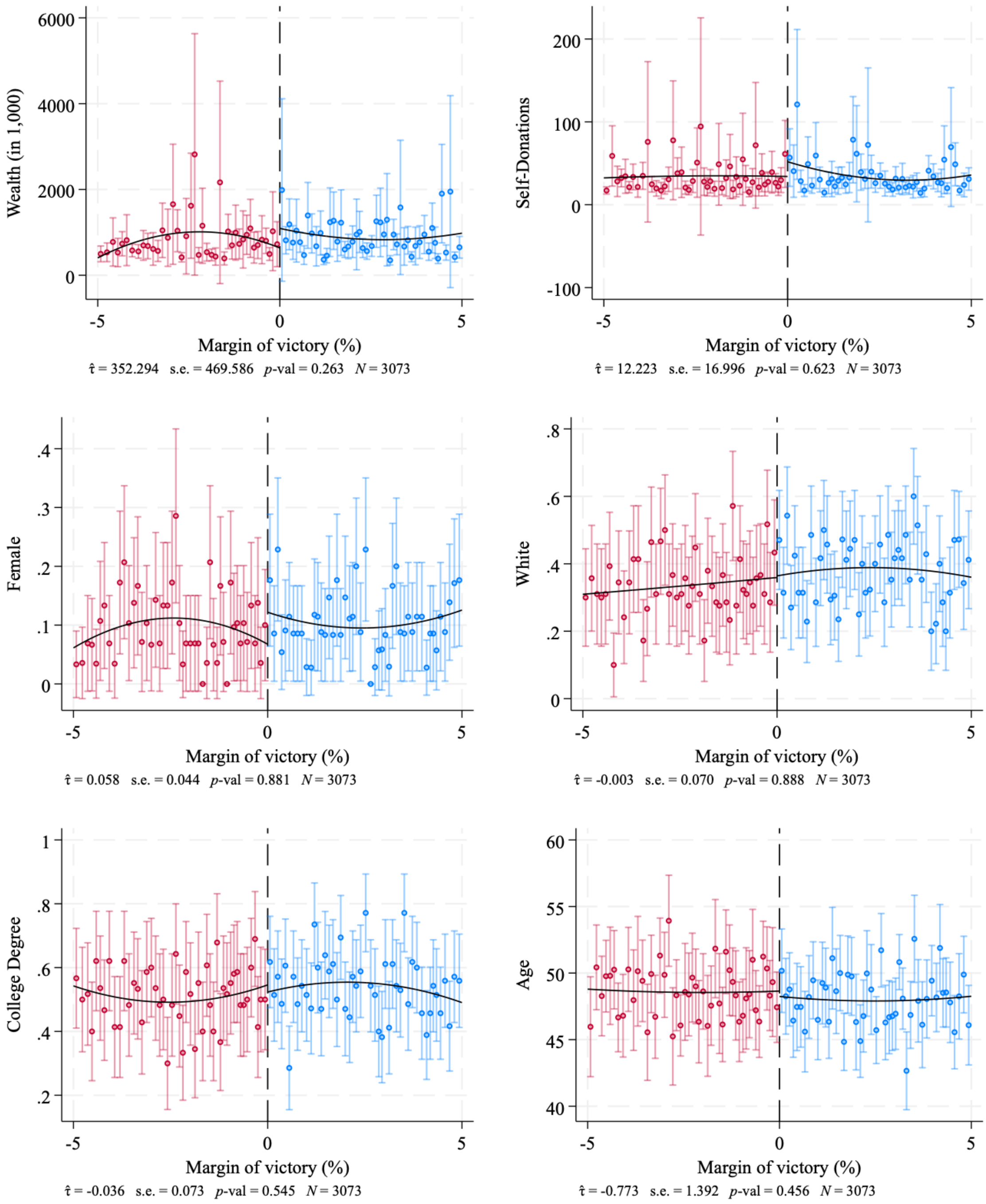

Figure 3: Covariate balance checks ($|\text{Margin}| \leq 5\%$)

Notes: This figure plots RD estimates for the six key candidate characteristics using local quadratic polynomial fit and bandwidth $\leq \pm 5$ as covariate balance checks. Each dot represent binned outcome quantile means and vertical lines report 95% confidence intervals. Number of quantiles chosen using a data-driven algorithm in order to “mimick” the variance of the overall sample (Calonico et al., 2015).

5 Results

I begin by estimating the private returns from political office. The outcome variable is the annualized wealth growth rate between the two election years ($t \to t+4$), when candidates that run again report their wealth once more. The coefficient of interest ($\hat{\tau}$) captures the local difference in growth rates between first-term elected mayors seeking reelection (treatment) and of runner-ups that decide to run again (control), measured in percentage points. Recall that because I only observe units that run again in the next election, this sample compares winners and runner-ups that may come from different municipalities. Importantly, by using a differenced outcome – changes in wealth – we can parse out unobservable time-invariant factors (An and Winship, 2017).

The main results are plotted in Figure 4, and also reported in Table 2. Columns 1-2 of Table 2 report the continuity-based design, which uses the data-driven mean squared error (MSE) optimal bandwidth described earlier. Given the larger bandwidths, I report estimates with both linear and quadratic fits. Columns 3-6 implement my local randomization design, at bandwidths of 5 and 1. Given their smaller size, I report estimates with a local linear polynomial fit and a simple difference in means. Unless otherwise noted, I retain this structure in subsequent tables. Additionally, all columns report robust bias-corrected standard errors (Calonico et al., 2014, 2020) in parenthesis. Columns 5-6 use Fisher's p-values (from 1,000 permutations) for inference, reported in brackets. Given their smaller sample, finite-sample randomization inference methods have been shown to be more appropriate (Cattaneo et al., 2015, 2016, 2017).

Regardless of design, bandwidth, or polynomial specifications, I find no evidence of positive returns from political office. The wealth of mayors who barely won grows at around 9-12% a year, depending on the bandwidth, but the wealth of runner-ups who barely lost increased at nearly identical pace. Naturally, the point estimates of their difference are small. They are often slightly negative, but always statistically indistinguishable from zero. In the following sections, I provide extensions and robustness checks.

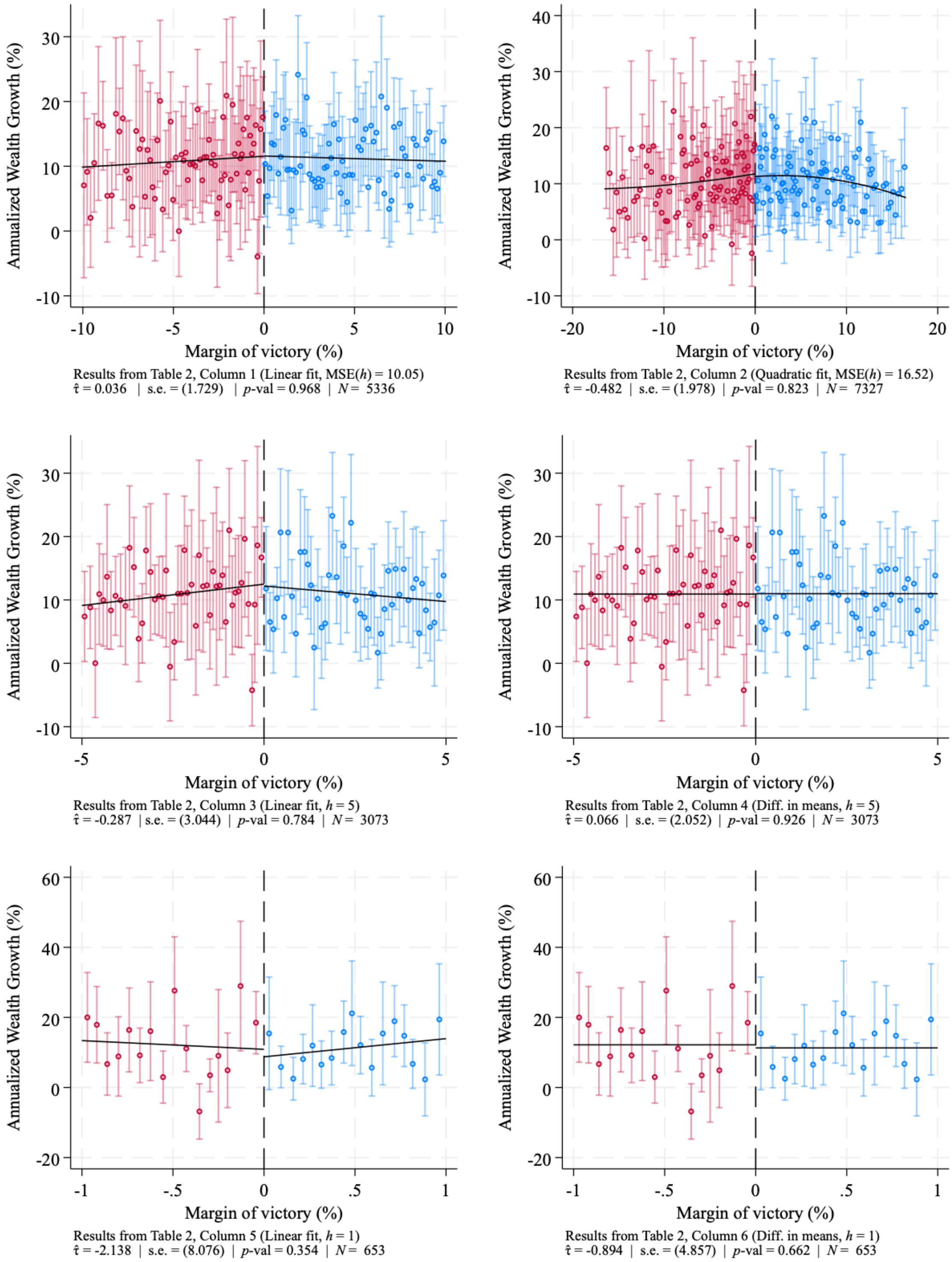

Figure 4: Main Results: the private returns from political office

Note: This figure plots the RD estimates from Table 2. Each dot represent binned outcome quantile means and vertical lines report 90% confidence intervals. Number of quantiles chosen using a data-driven algorithm in order to "mimick" the variance of the overall sample (Calonico et al., 2015). See Table 2 for further details.

Table 2: The private returns to political office

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(10.05)	MSE(16.52)	5	5	1	1
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	0.036	-0.482	-0.287	0.066	-2.138	-0.894
	(1.729)	(1.978)	(3.044)	(2.052)	(8.076)	(4.857)
Fisher's p-values					[0.336]	[0.680]
Polynomial:	Linear	Quadrat.	Linear	Diff. Means	Linear	Diff. Means.
Return Winner:	11.588	11.277	12.201	10.991	8.762	11.294
Return Runner-Up:	11.552	11.759	12.488	10.926	10.900	12.188
N	5336	7327	3073	3073	653	653
$[N_-; N_+]$	[2294;3042]	[3017;4310]	[1397;1676]	[1397;1676]	[321;332]	[321;332]

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control). MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Columns 1-6 report robust bias-corrected standard errors (Calonico et al., 2014) in parenthesis. Columns 5-6 use Fisher's p-values (1,000 permutations) for inference, reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, and $[N_-;N_+]$ report the number of observations left and right of the cutoff. See Figure 4 for the respective plots.

5.1 Matched pairs

As reported in Table 1, the RD design does a good job of sorting winners and runner-ups across political and institutional variables, as there are no discontinuities on these variables across the cutoff. One concern is that while treatment and control units, which may come from difference municipalities in the main sample, may be similar along these observable characteristics, there may be additional unobservable municipality- or election-level factors that affect wealth but remain unaccounted for. For this exercise, I follow Fisman et al. (2014) and rely exclusively on matched pairs. In the main specification, the use of a differenced outcome – changes in wealth – parses out all time-invariant factors that affect the *level* of wealth (An and Winship, 2017), and is equivalent to including individual fixed effects. However, because winners and runner-ups may come from different municipalities, average unobservable municipality-level characteristics may differ across treatment and controls. Using matched pairs solves this problem by requiring that both units come from the same municipality-election, thus holding all municipality-level characteristics constant. That is, this specification parses out municipality-election shocks that affect both treatment and control units separately from individual fixed effects, and is equivalent to including both individual and municipality-election fixed effects.[33]

Indeed, unlike the global polynomial fit for the full sample (Figure 1), Figure A5 suggests a small positive return. To estimate the local treatment effect more precisely, I replicate Table 2 with this sample of matched pairs. Within this restricted sample, reported in Table 3, I find mostly positive point estimates, although they are significant in only two specifications, suggesting a positive return from office around 4.3 to 4.8 percentage points a year, similar in magnitude to the findings of Fisman (2001).

Table 3: The private returns to political office, matched pairs

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(7.60)	MSE(12.06)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	3.372	3.155	4.354**	1.548	-3.517	4.863**
Fisher's p-values	[0.572]	[0.594]	[0.014]	[0.404]	[0.194]	[0.048]
Polynomial:	Linear	Quadrat.	Linear	Diff. Means	Linear	Diff. Means.
Return Winner:	9.555	9.353	11.151	9.876	6.339	12.298
Return Runner-Up:	6.183	6.199	6.797	8.328	9.856	7.435
N	1076	1436	774	774	434	434
Matched Pairs	538	718	387	387	217	217

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control), both from the same municipality (matched pairs sample). MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Fisher's p-values (1,000 permutations) used for inference, reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, and [N_-;N_+] report the number of observations left and right of the cutoff. See Figure 4 for the respective plots.

5.2 Returns under different political environments

While as-good-as-random sorting is ultimately what we want, I also aim to leverage variation in political environments to explore potential heterogeneity in the the returns

[33]Figures A2-A4 report the covariate balance checks for this sample.

from political office. This is a crucial step, because while returns from office seem to be small (or null) in the *average* municipality, there may still be municipalities, notably those with low accountability, that can be particularly fertile environments for political enrichment.[34] Namely, I consider two dimensions of accountability: institutional quality and the presence of a local radio news stations. Within my sample of matched pairs, I define municipalities with low and high institutional quality as those below and above median score in an index of municipal institutional quality (IQIM), respectively. The index covers the degree of political participation (especially through municipal oversight councils), financial and management capacity, as described in detail in Section 3.2. In turn, local radio has been instrumental in Brazilian local politics, functioning as the primary vehicle for disseminating information about politicians' conduct to voters (Ferraz and Finan, 2008, 2011). I use an indicator for the presence of a local radio news station as the second source of variation in accountability.

Table 4: The private returns to political office in municipalities with low institutional quality

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(8.82)	MSE(12.75)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	7.517***	10.102***	10.510***	3.119	10.180***	8.593**
Fisher's p-values	[0.004]	[0.000]	[0.000]	[0.194]	[0.004]	[0.014]
Polynomial:	Linear	Quadratic	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	10.709	10.808	12.443	9.863	12.624	12.654
Return of Runner-Up:	3.192	0.705	1.932	6.744	2.444	4.062
N	534	690	364	364	208	208
Matched Pairs	267	345	182	182	104	104

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control), both from the same municipality with low institutional quality. MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, with $N/2$ units on either side of the cutoff.

[34]This is similar to previous empirical settings such as Klašnja (2015) and Granzier et al. (2023), where there are heterogeneous treatment effects in the context of a RD design. Calonico et al. (2025) shows that estimating this as an interaction between a dichotomous subgroup variable and the treatment indicator or by sample splits leads to the same results.

I start by reporting the role of institutional quality. Tables 4 and 5 show the results for municipalities with low and high institutional quality, respectively. I find that, in municipalities with low institutional quality, the returns from office are substantial, around 7.5 to 10.5 percentage over that of runner-ups. By the end of a four-year term, elected mayors are 48% richer than the counterfactual would suggest. In contrast, in municipalities with high institutional quality, there is no evidence of positive returns. If anything, columns 2 and 5 show mayors underperforming their matched runner-ups.

Table 5: The private returns to political office in municipalities with high institutional quality

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(6.63)	MSE(10.46)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	-3.565	-4.608**	-0.838	0.154	-14.492***	1.431
Fisher's p-values	[0.172]	[0.022]	[0.710]	[0.966]	[0.000]	[0.678]
Polynomial:	Linear	Quadratic	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	7.041	7.199	10.097	9.888	1.281	11.970
Return of Runner-Up:	10.607	11.807	10.935	9.734	15.773	10.539
N	546	708	410	410	226	226
Matched Pairs	273	354	205	205	113	113

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control), both from the same municipality with high institutional quality. MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, with $N/2$ units on either side of the cutoff.

In turn, Table 6 analyzes the returns from office in municipalities without the presence of a local radio news station. Returns are again substantial, around 6 to 8 percentage points in excess of the returns of matched runner-ups, indicating that elected mayors are 33.6% richer by the end of a four-year term. Conversely, Table 7 shows that in municipalities with a local radio news stations, the returns from office are either zero or negative. In Appendix A, I show similar findings for municipalities lacking a local TV station (Tables A2-A3).

Table 6: The private returns to political office in municipalities without local AM radio news station

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(7.94)	MSE(12.45)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	6.076***	6.788***	7.945***	2.012	-0.949	7.256**
Fisher's p-values	[0.000]	[0.000]	[0.000]	[0.348]	[0.734]	[0.020]
Polynomial:	Linear	Quadratic	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	12.197	12.409	13.758	10.915	7.745	14.388
Return of Runner-Up:	6.121	5.621	5.813	8.903	8.694	7.132
N	830	1122	598	598	324	324
Matched Pairs	415	561	299	299	162	162

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control), both from the same municipality without local AM radio news station. MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, with $N/2$ units on either side of the cutoff.

Table 7: The private returns to political office in municipalities with local AM radio news station

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(7.24)	MSE(12.86)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	-2.342	-5.446*	-5.623	-0.025	-9.281**	-2.184
Fisher's p-values	[0.564]	[0.090]	[0.108]	[0.982]	[0.036]	[0.618]
Polynomial:	Linear	Quadratic	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	3.188	1.347	4.157	6.347	3.372	6.142
Return of Runner-Up:	5.530	6.793	9.781	6.372	12.652	8.326
N	256	346	176	176	110	110
Matched Pairs	128	173	88	88	55	55

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control), both from the same municipality with local AM radio news station. MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, with $N/2$ units on either side of the cutoff.

These findings show markedly different returns from office according to level of local accountability, but they do not precisely indicate *how* these institutional factors operate. To attribute these differences to variation in local accountability, we must rule out alternative explanations. For instance, suppose that voters who demand and support good institutions are also more likely to vote for "honest" candidates. This would suggest the different outcomes are not so much the result of these institutions functioning, but rather from underlying differences in voter preferences. It could also be that voters in both types of municipalities have the same preference for "honest" candidates, but there is a random noise that prevents them from properly assessing desirable traits like honesty. In this case, the presence of local media channels help voters make informed decisions about their candidates by revealing desirable qualities like honesty.[35]

These scenarios suggest technically different channels – voter preferences and information about candidates, respectively – but ultimately they would suggest that the observed pattern is the result of an endogenous selection mechanism, whereby voters in high accountability municipalities elect good candidates *ex ante*. An analogous relationship is possible from candidates' perspective. Suppose "honest" candidates – say, those without aspirations to political enrichment – are more likely to implement policies improving local accountability. Then, instead of a causal mechanism whereby accountability effectively constrains mayors of different qualities, we would be capturing reverse causality: accountability as an *ex post* result of candidate honesty. Put simply, honest mayors implement good policies in the first place.

Although plausible, I find little evidence for these alternative explanations. First, recall that my measure of institutional quality, the IQIM, was measured in 1999, more than a decade before any of the mayors in my sample reached office. Thus, the level of institutional quality these mayors face is not the direct result of their own policy

[35] This relates to a large literature on candidate selection under imperfect voter information (see, e.g., Duggan, 2000; Laslier and Straeten, 2004; Besley, 2005; Kartik and McAfee, 2007; Matějka and Tabellini, 2021). For a broader review of political selection, see Dal Bó and Finan (2018). For the specific role of the media in voter information and behavior, see Gentzkow and Shapiro (2011), Dziuda and Howell (2021), and Myers (2025).

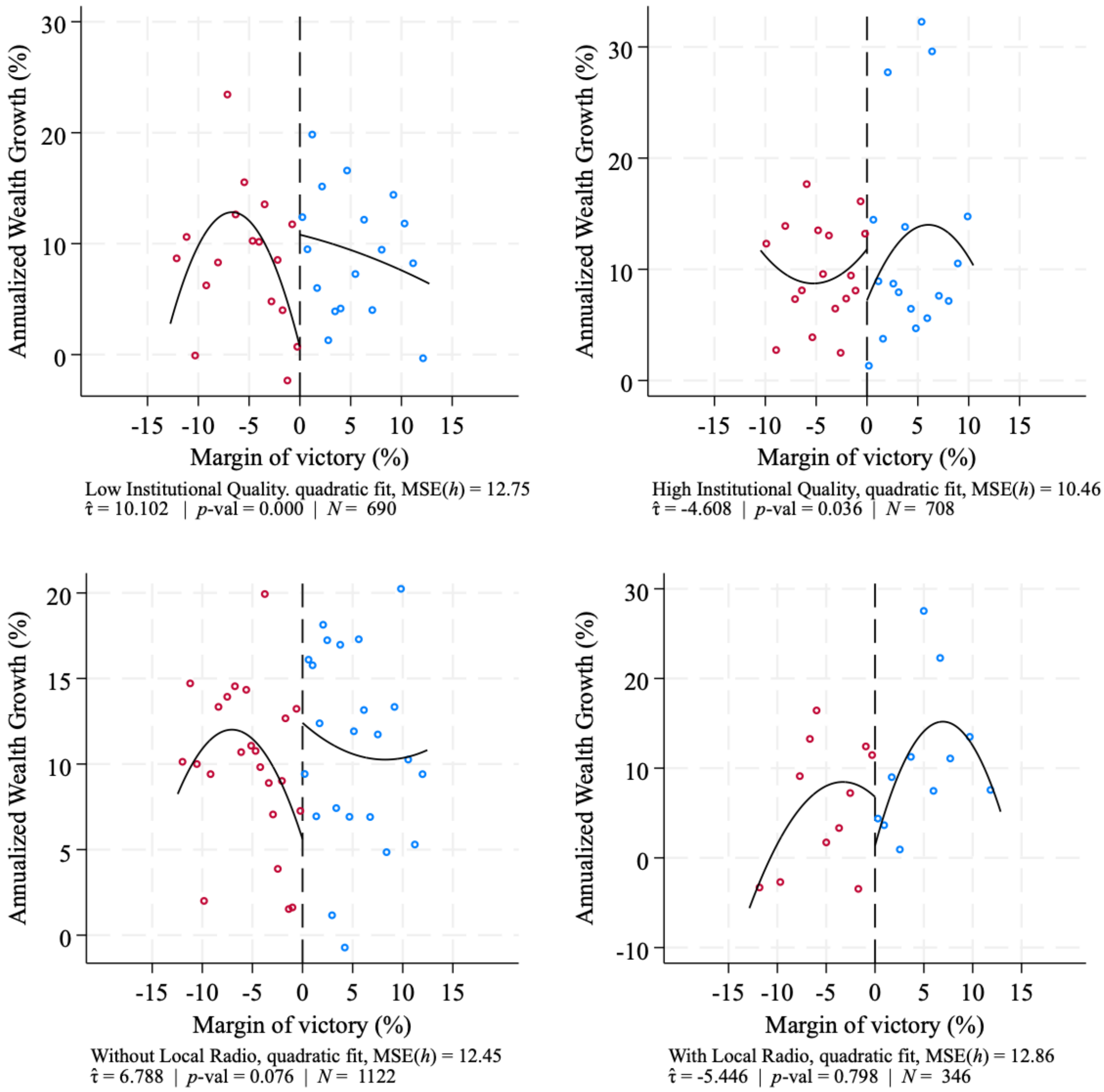

Figure 5: The private returns from political office, low and high accountability

Note: This figure plots the RD estimates from column 2 (MSE-optimal bandwidth) of Tables 4-7. Each dot represent a binned outcome quantile means, and the solid black line shows quadratic fit. Number of quantiles chosen using a data-driven algorithm in order to "mimick" the variance of the overall sample (Calonico et al., 2015). See Tables 4-7 for further details.

choices, ruling reverse causality concerns.[36] To investigate the role of voter preferences and candidate selection channels, I compare the characteristics of winning mayors in municipalities with low and high accountability.

[36]Moreover, in Section A.3.2, I leverage four types of exogenous (i.e. not determined by a mayor's own policy choices) institutional variation: low judicial oversight (municipalities with no judge), low political competition (high concentration of votes across political parties), high political rents (large sums windfall revenues per capita), and a state-level index of checks and balances (Alston et al., 2009), all leading to similar results.

Crucially, even though traits like honesty are likely unobservable, strong selection effects would suggest that mayors will differ systematically across these two types of municipalities. To account for the fact that municipalities in either group may also differ in other characteristics, I weight the observations using an entropy balance algorithm (Hainmueller, 2012; Hainmueller and Xu, 2013). The intuition is simple: we want that our municipalities with high and low accountability to have the same candidate pool given a series of observable characteristics, and also to be similar on overall municipal characteristics, leaving accountability as the only source of variation; then, we may observe how does the typical mayor on either type of municipality, conditional on similar candidate pools. In Appendix A, I detail this procedure and show (Figures A6-A7) the average municipal characteristics before and after balancing.

Table 8 reports the characteristics of mayors across my sample splits. I find that after accounting for differences in candidate pools, elected mayors of both types of municipalities have largely the same characteristics, with a few exceptions. Candidates in municipalities with a local radio tend to be richer, and about 0.7 years older, and these municipalities are two percentage points more likely to elect a female mayor. No such differences occur between high- and low-institutional quality municipalities. In turn, there is mixed evidence regarding previous political experience. In Appendix A, I show that political experience is unlikely to play a major role the determining the returns from office in this case.[37] In special, I find no evidence of differences in educational attainment, the main proxy for candidate "quality" in previous literature (see, e.g., Brollo et al., 2013; Avis et al., 2022; Gamalerio and Trombetta, 2025). These results suggest that, conditional on candidate pool, either type of municipality is just as likely to elect high- or low-"quality" candidates.

[37] Fisman et al. (2014) finds that returns from office for experienced politicians is much larger, about 12%, while "rookies" have little, or even negative, returns.

Table 8: Characteristics of winning mayors in high- and low-accountability municipalities

	Institutional Quality			Presence of Local Radio Station		
Municipality Type	High	Low	(Diff.)	Yes	No	(Diff.)
N	354	345		868	2743	
Initial Wealth	1.035M	0.829M	0.21M	1.460M	0.904M	0.56M***
	(3.515M)	(1.740M)	[0.98]	(3.813M)	(2.442M)	[5.18]
Age	48.200	49.226	-1.03	49.344	48.637	0.71**
	(10.253)	(11.860)	[-1.22]	(10.756)	(10.623)	[1.98]
College Degree	0.571	0.624	-0.05	0.672	0.675	-0.00
	(0.496)	(0.485)	[-1.40]	(0.470)	(0.468)	[-0.19]
Secondary degree or less	0.151	0.162	-0.01	0.098	0.091	0.01
	(0.359)	(0.369)	[-0.37]	(0.298)	(0.288)	[0.68]
Female	0.106	0.082	0.02	0.104	0.085	0.02*
	(0.308)	(0.275)	[1.06]	(0.305)	(0.279)	[1.94]
Married	0.703	0.753	-0.05	0.741	0.740	0.00
	(0.458)	(0.432)	[-1.48]	(0.438)	(0.439)	[0.09]
Rookie candidate	0.654	0.701	-0.05	0.588	0.664	-0.08***
	(0.476)	(0.458)	[-1.33]	(0.493)	(0.472)	[-4.73]
Former city councilor	0.194	0.111	0.08***	0.189	0.170	0.02
	(0.396)	(0.314)	[3.09]	(0.392)	(0.375)	[1.54]
Former mayor	0.160	0.172	-0.01	0.125	0.156	-0.03***
	(0.367)	(0.378)	[-0.43]	(0.331)	(0.363)	[-2.71]
Former vice-mayor	0.063	0.027	0.04**	0.046	0.054	-0.01
	(0.243)	(0.162)	[2.30]	(0.210)	(0.227)	[-1.10]
Former state deputy	0.023	0.015	0.01	0.100	0.033	0.07***
	(0.150)	(0.122)	[0.76]	(0.301)	(0.179)	[8.11]
Former federal deputy	0.011	0.044	-0.03***	0.044	0.007	0.04***
	(0.106)	(0.206)	[-2.63]	(0.205)	(0.083)	[7.07]

Notes: Includes mayors in the sample of column 2 (MSE-optimal bandwidth, quadratic fit) of each table. All variables measured in the election year (t), except for wealth growth, which measures the annualized return rate from t to t+4. Standard deviation reported in parenthesis. The *Diff.* column reports unconditional differences in means and t-statistics in brackets. ***, **, * respectively denote $p \leq 0.01, 0.05, 0.10$.

6 Robustness Checks

To increase the confidence in the main results, in this section I perform a series of robustness checks addressing potential concerns and evaluating alternative explanations, which are briefly discussed here. Appendix B provides additional details and present the full results.

6.1 Accounting for Reporting Omissions

As discussed earlier, data on candidates' wealth may suffer from discrepancies because candidates are not explicitly required to report assets such as bank accounts (Souto-Maior and Borba, 2019). However, it is also well-known that two classes of assets – vehicles and real estate assets – are extremely well reported. Based on data between 2012 and 2024, we know that that these categories account for about 76% of candidates reported wealth (Figure B1).[38] Additionally, Trevisan (2003) also contends that personal property (e.g., cash, jewelry, art) and livestock are more commonly used for wealth accumulation through corrupt means, as they are generally less visible and harder to trace than real estate, potentially making them more conducive to corruption. Thus, I also consider changes in wealth from vehicles and real estate alone. Focusing on these assets minimizes measurement error stemming from both simple omissions or more severe manipulations in reporting,[39] but also has difficulties of its own.

A key difficulty with this approach is that assets are not labeled into categories for the 2008 data. To overcome this problem, I use a simple machine-learning algorithm to classify the 2008 assets into 10 categories, and then use the wealth from vehicles and real estate alone. I start with the asset data for all candidates in the 2012 to 2024 local elections. This data contains 3,798,577 assets, 95% of which I use to train the model and the remaining 5% for validation. The final model is able to correctly identify 97% of real estate assets and vehicles in the validation data. This process is detailed in Section B.3 in Appendix B. While omissions do occur, they seem mostly uncorrelated to election outcomes – elected candidates are just as likely as runner-ups to omit part of their assets – and thus do not seem to systematically bias the results.

6.2 Political Experience

Mayors tend to be at the beginning of their political careers. "Rookie" politicians may be unable to capture private returns if they require skills acquired on the job, and thus only come with greater political experience. For instance, Fisman et al. (2014) find that,

[38] Although the shares become inflated because candidates under-report their bank accounts, they are in line with national estimates for the general population in terms of real estate and car ownership rates (Souto-Maior and Borba, 2019).

[39] This approach is close in spirit to Braguinsky et al. (2014), who infer hidden increases in wealth among Russian officials from administrative data on car ownership (also see Di Tella and Weinschelbaum, 2008; Kim and Neshat, 2025).

among state-level legislators in India, the returns for "seasoned" politicians is around 12% percent, while for "rookie" politicians they find zero, or even negative, returns. In Tables B6-B7, I estimate the returns from office for mayors with prior political experience (either as mayors or in other elected offices, respectively). I find only modest evidence that they are more likely to benefit from office, with only two out of twelve specifications being positive and significant. See Section B.4 for a detailed discussion.

6.3 Excluding extreme outliers

A particular concern given the nature of the wealth data is that extreme outliers may be driving the results. On the one hand, candidates with very low initial wealth may yield implausibly high returns in percentage terms. On the other, it will be hard to find plausible counterfactuals to a few extremely wealthy candidates. Both of them can potentially affect the results, either by forcing inflection points for the polynomial fits or by generating extremely large confidence intervals that would lead to over-rejection of our point estimates.

I replicate the main results while dropping candidates that are on the top (≥ 99) and bottom (≤ 1) percentiles of either the distribution of initial wealth or changes in wealth. This process excludes candidates with wealth over R$ 6 million or under R$ 9,000. In terms of changes in wealth, it excludes candidates whose wealth rose by more than 86% or shrunk by more than 49% every year (on average), or equivalent to a 1097% rise or -94.2% decline over all four years of an election cycle. As shown in Table B8, I find similar estimates after excluding these outliers.

7 Conclusion

Across the globe, the conventional wisdom is that politics does pay off. Large and positive financial returns from political office have been empirically documented in a series of rigorous studies in the United States and in several European democracies. However, by focusing on influential politicians in high-income, established democracies, these studies attribute the private gains to mechanisms that may not be relevant to developing countries. In environments where corruption and rent-seeking still dominate the political game, it is likely that the mechanisms explaining political enrichment will be substantially different, and may lead to substantial returns, even for low-level politicians.

This papers aims to fill this gap by providing estimates of the private returns to public office for local level politicians in the context of Brazil. I find that, in municipalities lacking instruments of oversight or without a local radio station – mayors' wealth grows much faster than their matched runner-ups, even though the typical mayor may not benefit. My findings are broadly in line with Fisman et al. (2014) and their study of India, insofar as both studies highlight corruption and rent-seeking as important mechanisms of political enrichment in developing countries.

I provide two novel contributions to the understanding of financial benefits of political office. First, my findings suggest that politics can be a very lucrative business when politicians govern unchecked, even at the local level. Previous evidence has found no meaningful returns to local office (Berg, 2020a; Kotakorpi et al., 2017), but this can be plausibly attributed to the low-corruption environment of their empirical settings. Most importantly, this paper provides new evidence on the role local institutions in shaping the returns from office. While it may cause dismay that politicians have large returns in environments with low accountability given their ubiquity in developing countries, my findings also highlight that improving local institutions and the presence of local media may effectively curb political enrichment, offering a direct connection to the seminal theoretical contributions of Barro (1973) and Buchanan (1983). Improved accountability makes even the "wrong" politicians to do the "right" thing, offering a pathway for improved local governance.

However, one important caveat concerns the key characteristic of RD designs. Namely, that it provides a causal effect estimate *at the cutoff point*. This is inherently a local estimate, and can hardly be generalized to all winning mayors. Even if we cannot provide reliable causal estimates, it does not rule out that the returns from office for mayors winning with large margins of victories can be substantial.[40] Additionally, and as a direct consequence of data availability, all winners in my sample are potentially seeking reelection. Recall that because there is a two consecutive term limit and I only observe wealth of mayors when registering their candidacy, I can only observe mayors in the first or their two consecutive term limit. This is crucial because reelection incentives may play an important role in constraining mayors' accumulation of wealth. For instance, using data from a federal audit program, Ferraz and Finan (2011) finds that Brazilian mayors in their first-term misappropriate 27% fewer resources than those not facing reelection. Hence, it is plausible to assume that any estimate of the returns from office for mayors facing reelection incentives is likely closer to a lower-bound estimate. Extending these findings to account for the role of electoral incentives is a fruitful avenue for future research.

[40]The mean annual return of mayors who won unopposed (i.e. in a single-candidate race) was 13.18%, about 2.5 percentage points higher than that of the average mayor. Of course, such comparison rely on unconditional means, and each group may differ substantially in many other factors, which prevents any causal interpretation from such comparisons alone.

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Appendix

A Additional Tables and Figures

A.1 Covariance balance checks

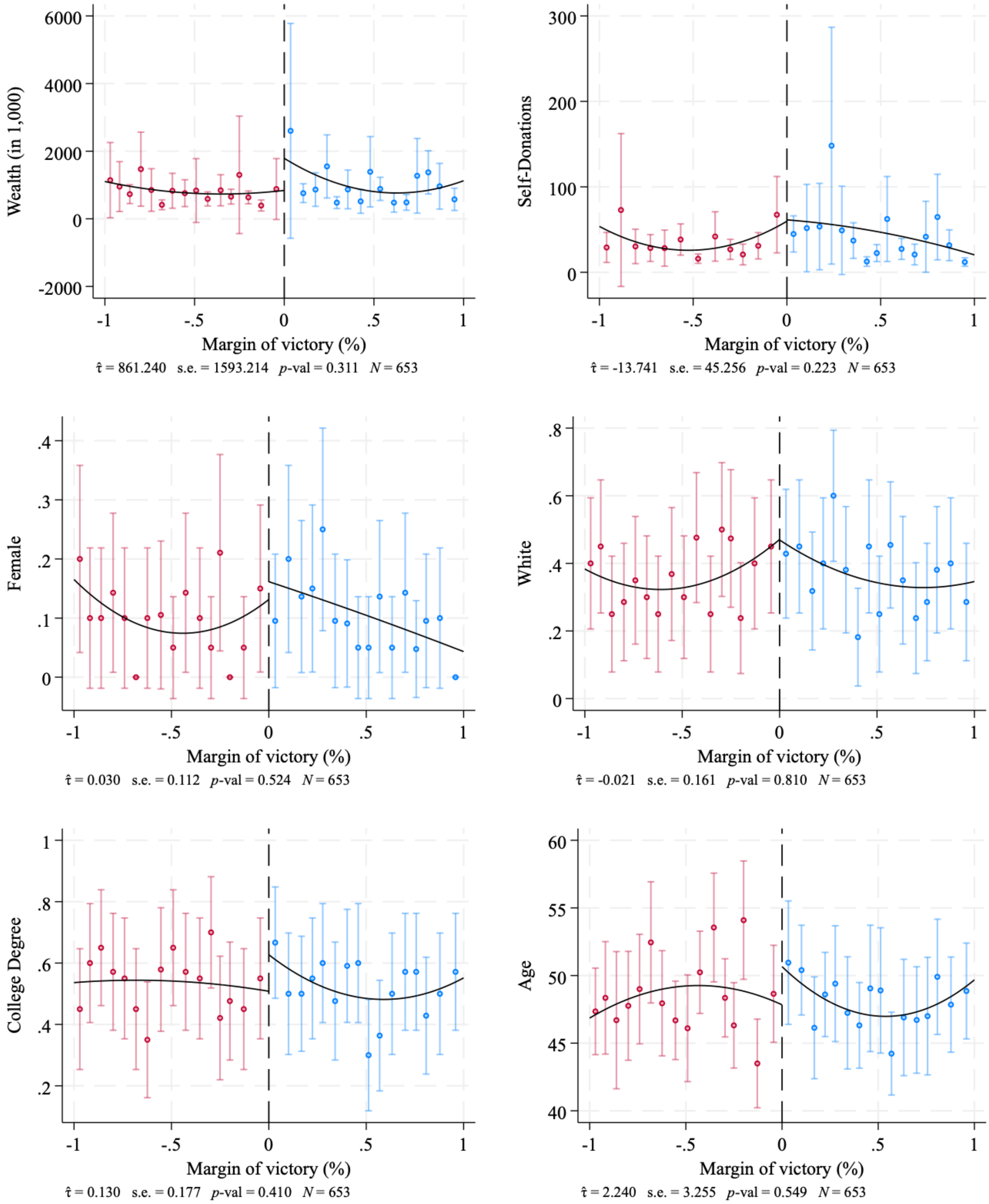


Figure A1: Covariate balance checks ($|\text{Margin}| \leq 1\%$)

Note: This figure plots RD estimates for the six key candidate characteristics using local quadratic polynomial fit and bandwidth $\leq \pm 1$ as covariate balance checks for columns 5-6 of Table 2. Each dot represent binned outcome quantile means and vertical lines report 95% confidence intervals. Number of quantiles chosen using a data-driven algorithm in order to "mimick" the variance of the overall sample (Calonico et al., 2015).

Table A1: Summary Statistics

	MSE-Optimal Bandwidth		Diff. (MSE)	|Margin| ≤ 5		Diff. (5)
	Winner	Runner-Up	(W–R-Up)	Winner	Runner-Up	(W–R-Up)
N	4308	3019	[*t*-stat.]	1674	1399	[*t*-stat.]
Wealth Variables						
% Wealth Growth	10.624	10.450	0.18	10.990	10.927	-0.29
	(26.831)	(30.011)	[0.14]	(27.194)	(28.793)	[-0.09]
Initial Wealth	0.917M	0.812M	0.10	0.902M	0.856M	0.02
	(2.457M)	(2.386M)	[0.98]	(2.423M)	(2.664M)	[0.06]
% Adj. Wealth Growth	3.350	6.151	-2.80*	4.117	6.280	-1.84
	(35.379)	(38.233)	[-1.81]	(34.814)	(37.953)	[-0.48]
Adj. Initial Wealth	0.615M	0.560M	0.05	0.578M	0.556M	-0.03
	(1.725M)	(1.384M)	[0.90]	(1.434M)	(1.420M)	[-0.18]
Mayor Characteristics$_t$						
Age	48.024	48.496	-0.47**	48.020	48.596	-0.52
	(10.258)	(10.322)	[-1.10]	(10.233)	(10.170)	[-0.49]
College Degree	0.529	0.509	0.02	0.539	0.508	0.04
	(0.499)	(0.500)	[0.91]	(0.499)	(0.500)	[0.75]
Female	0.100	0.106	-0.01	0.105	0.096	0.01*
	(0.300)	(0.308)	[-0.48]	(0.306)	(0.295)	[0.18]
Married	0.751	0.742	0.01	0.749	0.756	-0.01
	(0.432)	(0.437)	[0.52]	(0.434)	(0.430)	[-0.15]
Rookie candidate	0.679	0.600	0.08***	0.675	0.599	0.06
	(0.467)	(0.490)	[3.87]	(0.469)	(0.490)	[1.17]
Municipality$_t$						
IQIM Score	3.069	3.027	0.04**	3.100	3.033	0.07
	(0.553)	(0.544)	[1.80]	(0.568)	(0.560)	[1.12]
Has Local Radio	0.229	0.220	0.01***	0.263	0.214	0.09
	(0.420)	(0.414)	[0.51]	(0.440)	(0.410)	[1.95]
Has State Court	0.323	0.336	-0.01	0.345	0.335	0.06
	(0.468)	(0.472)	[-0.64]	(0.475)	(0.472)	[1.25]
Transfers per Capita	2623.294	2472.827	155.06*	2609.318	2444.550	177.67
	(1572.770)	(1546.531)	[2.43]	(1624.348)	(1436.382)	[1.06]
Political and Election Characteristics$_t$						
Share seats mayor party	0.223	0.229	-0.01	0.213	0.223	-0.02*
	(0.147)	(0.147)	[-1.01]	(0.145)	(0.143)	[-1.63]
Party concentration	0.233	0.224	0.01	0.232	0.226	-0.01
	(0.099)	(0.092)	[2.16]	(0.103)	(0.094)	[-0.61]
Invest. in own campaign	32.445K	31.819K	0.61	35.285K	34.367K	8.33
	(99.116)	(87.638)	[0.14]	(103.687)	(108.903)	[0.69]

Notes: All variables measured in the election year (t), except for wealth growth, which measures the annualized return rate from t to t+4. Adjusted wealth accounts for reporting omissions – see Section 6.1 for details. The *Diff.* column reports RD estimates as covariate balance tests around the cutoff. ***, **, * respectively denote $p \leq$ 0.01, 0.05, 0.10.

A.2 Matched Pairs

A.2.1 Covariate Balance checks

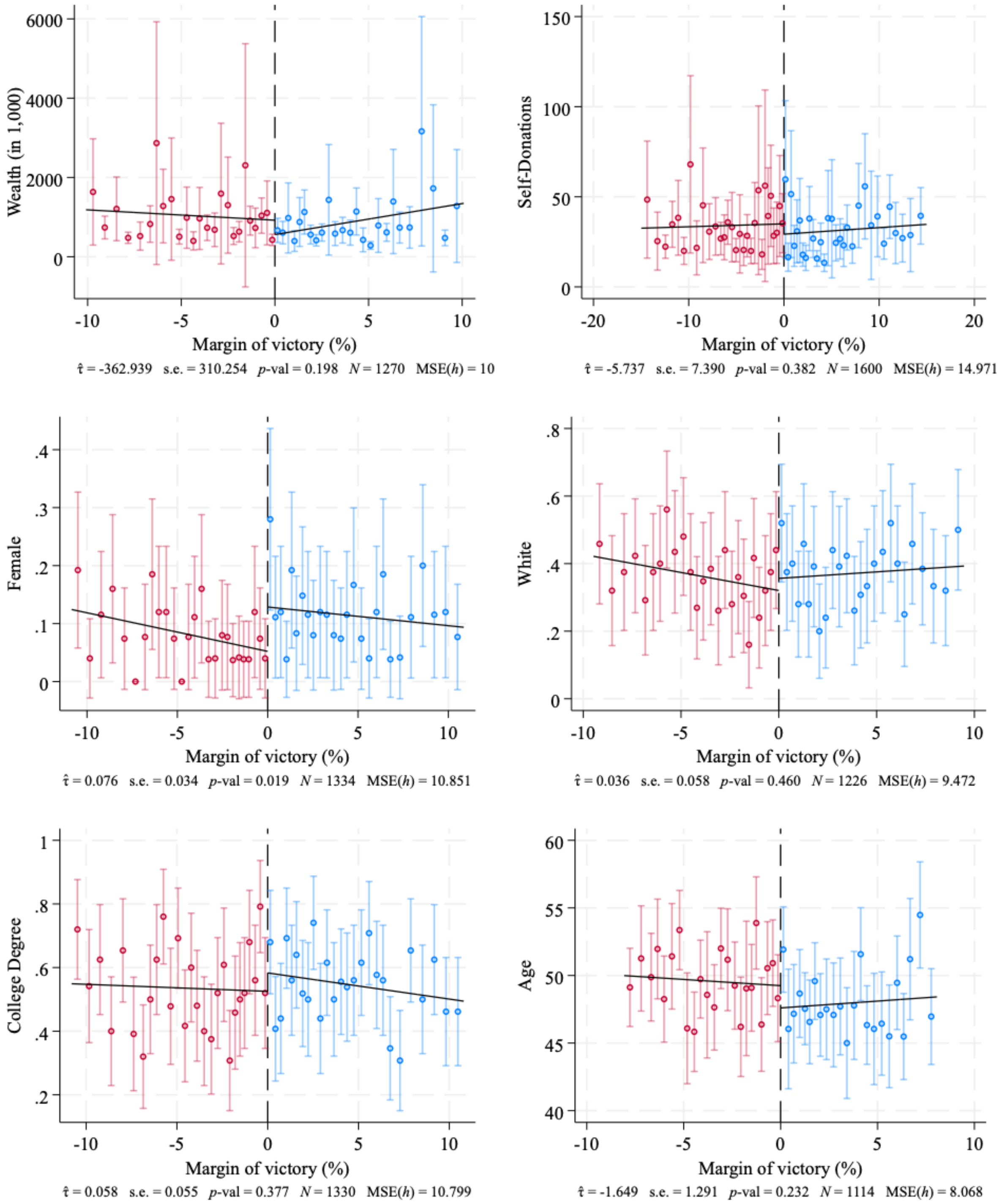


Figure A2: Covariate balance checks (MSE-optimal bandwidth)

Note: This figure plots RD estimates for the six key candidate characteristics using local quadratic polynomial fit and bandwidth $\leq \pm 1$ as covariate balance checks for 1-2 of Tables 3-7, matched pairs sample. Each dot represent binned outcome quantile means and vertical lines report 95% confidence intervals. Number of quantiles chosen using a data-driven algorithm in order to "mimick" the variance of the overall sample (Calonico et al., 2015).

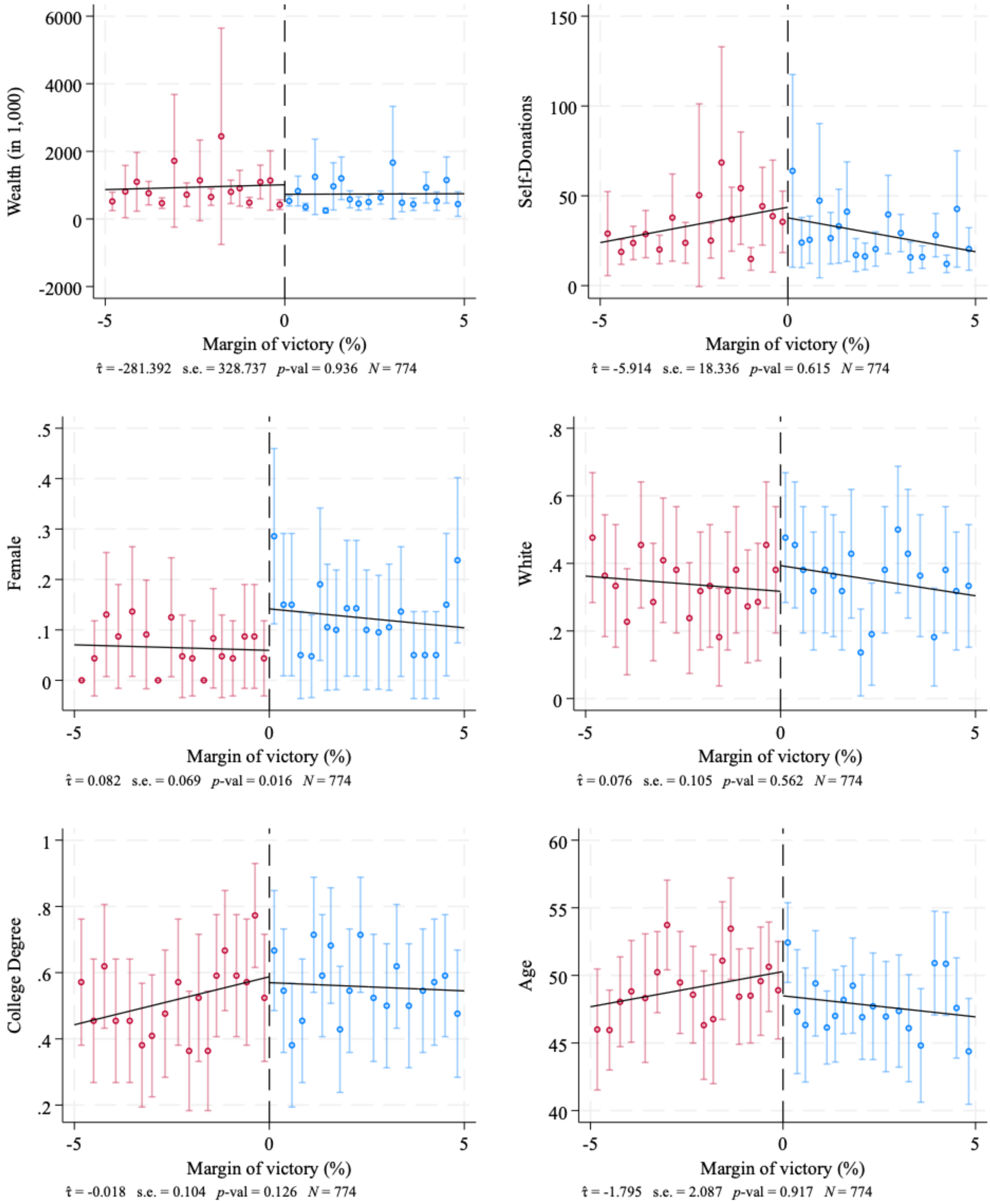


Figure A3: Covariate balance checks (|Margin| $\leq$ 5%)

Note: This figure plots RD estimates for the six key candidate characteristics using local quadratic polynomial fit and bandwidth $\leq \pm 1$ as covariate balance checks for 3-4 of Tables 3-7, matched pairs sample. Each dot represent binned outcome quantile means and vertical lines report 95% confidence intervals. Number of quantiles chosen using a data-driven algorithm in order to "mimick" the variance of the overall sample (Calonico et al., 2015).

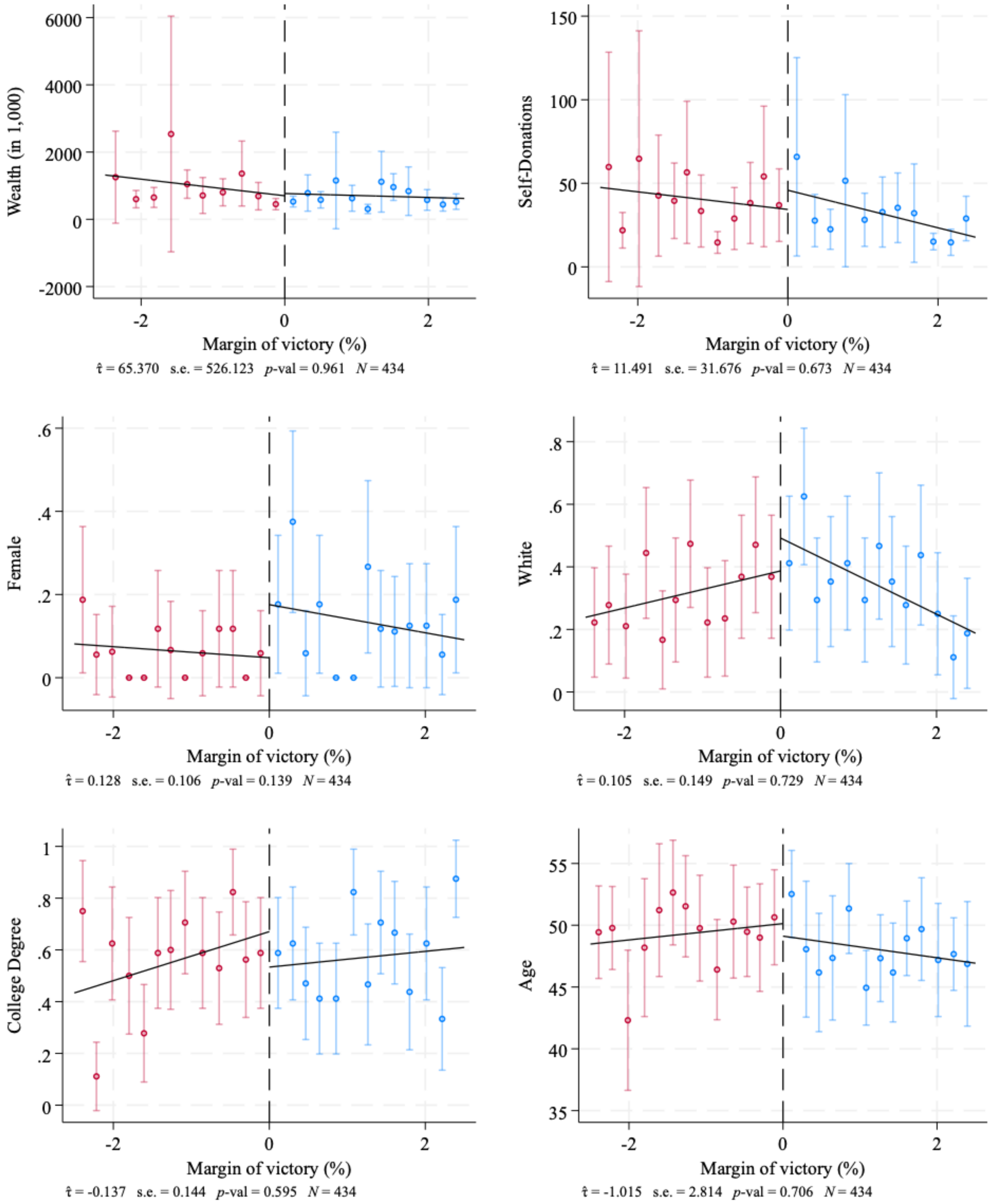


Figure A4: Covariate balance checks (|Margin| $\leq$ 2.5%)

Note: This figure plots RD estimates for the six key candidate characteristics using local quadratic polynomial fit and bandwidth $\leq \pm 1$ as covariate balance checks for 5-6 of Tables 3-7, matched pairs sample. Each dot represent binned outcome quantile means and vertical lines report 95% confidence intervals. Number of quantiles chosen using a data-driven algorithm in order to "mimick" the variance of the overall sample (Calonico et al., 2015).

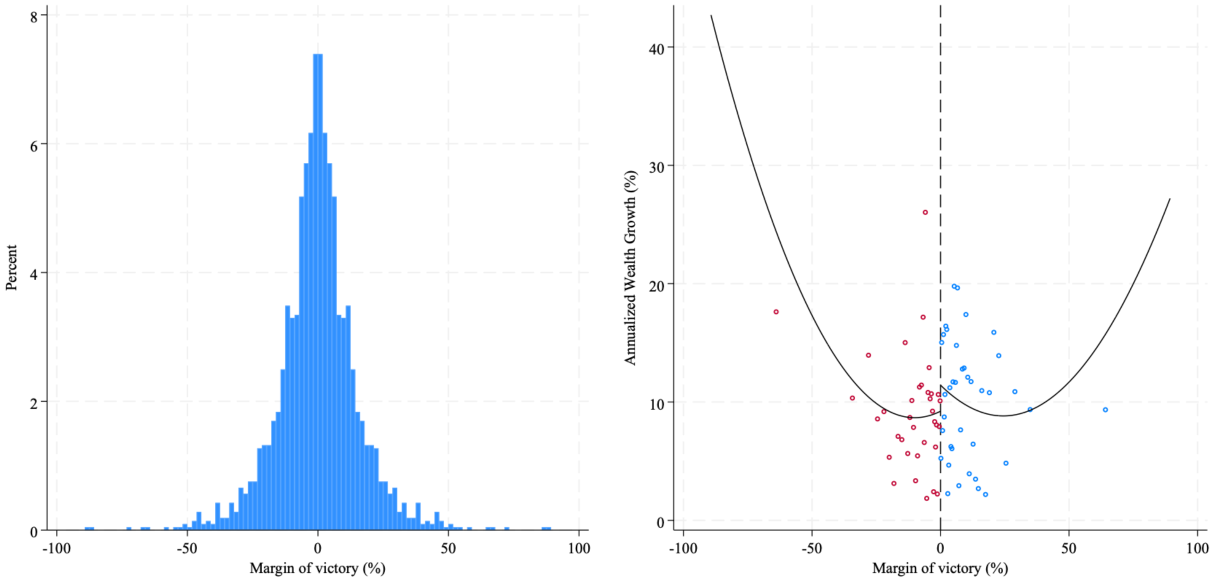


Figure A5: Sample distribution and global polynomial fit, matched pairs sample

Notes: Left panel: Distribution of margins of victory. Right panel: distribution of the outcome variable, annualized wealth growth (in %) on either side of the margin of victory. Each dot represent the mean of the outcome variable within a quantile-spaced bin. Number of bins ($J_{-} = 35$; $J_{+} = 38$) chosen using a data-driven algorithm in order to "mimick" the variance of the overall sample (Calonico et al., 2015). Solid black line is a second-order global polynomial fit. This sample contains 1,062 matched pairs.

A.3 Accountability and Political Environment

A.3.1 Indicador de Qualidade Institucional Municipal - IQIM

As described in Section 3.2, the IQIM has three areas that receive equal weight. The first area "Degree of Participation" tracks five variables (% of total index weight): number of municipal councils (4%), number of active councils (4%), the number of councils with parity – has equal number of seats across elected officials, public employees, and civil society – (4%), the number of deliberative (as opposed to mere advisory) councils (7%), and number of councils that manage earmarked funds (11%).

The second area "Financial Capacity" has three variables, each contributing 11.1% to the overall index. The existence of local consortia, the ratio of current revenues over local debt, and the stock of saving per capita. A local consortium is an inter-governmental regional agreement to manage a public good, such as water and sewage system in a metro area.

Finally, the "Management Capacity" area has four variables, each receiving 8.325% of the total index weight. The track the year of last survey that assess the tax base for the land tax, the land tax compliance rate, and whether the municipality implements management and planning instruments, such as a local development plan.

A.3.2 Additional Accountability and Political Environment Variables

As robustness checks for the my main results using the IQIM and the presence of local radio stations, here I consider additional sample splits. First, I Tables A2-A3 show that the results on the presence of local radio news are similar (albeit with slightly smaller magnitudes) when I consider an indicator for the presence of a local TV station instead.

Table A2: The private returns to political office in municipalities without local TV news station

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(7.96)	MSE(11.86)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	4.713**	3.507**	4.096**	2.195	-3.423	5.264*
Fisher's p-values	[0.012]	[0.030]	[0.038]	[0.256]	[0.214]	[0.082]
Polynomial:	Linear	Quadratic	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	10.483	9.150	10.661	10.421	5.933	12.349
Return of Runner-Up:	5.770	5.643	6.565	8.226	9.356	7.085
N	992	1292	696	696	388	388
Matched Pairs	496	646	348	348	194	194

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t{+}4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control), both from the same municipality without local TV news station. MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, with $N/2$ units on either side of the cutoff.

Table A3: The private returns to political office in municipalities with local TV news station

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(8.90)	MSE(10.31)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	-1.577	5.942	4.589	-4.217	-2.767	1.486
Fisher's p-values	[0.680]	[0.152]	[0.350]	[0.400]	[0.662]	[0.828]
Polynomial:	Linear	Quadratic	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	8.442	15.101	13.142	5.016	10.294	11.869
Return of Runner-Up:	10.019	9.159	8.553	9.233	13.061	10.383
N	122	130	78	78	46	46
Matched Pairs	61	65	39	39	23	23

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t{+}4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control), both from the same municipality with local TV news station. MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, with $N/2$ units on either side of the cutoff.

Additionally, I leverage four types of exogenous (i.e. not determined by a mayor's own policy choices) institutional variation: low judicial oversight (municipalities with no judge), low political competition (high concentration of votes across political parties), high political rents (large sums windfall revenues per capita), and a state-level index of checks and balances (Alston et al., 2009) – I discuss their implementation below. In the context of Brazil, these institutional characteristics have been argued to be strong determinants of municipal corruption (Ferraz and Finan, 2011; Brollo et al., 2013), or wealth accumulation by politicians (Melo and Pereira, 2013). The results are reported in Tables A4-A6; given their smaller samples, I use a minimum bandwidth of 2.5, and rely exclusively on Fisher's finite sample p-values for inference.

Table A4 estimates the returns from political office in municipalities with low judicial oversight, defined as those that are not judiciary districts (*comarcas*) and thus lack a judge serving in a local branch of a state court. Ferraz and Finan (2011) argues that municipalities with a local judge increase the probability of being prosecuted for wrongdoing, and thus increases the costs for engaging in corruption.[41] All estimates are positive, but significant in only half of the specifications, suggesting elected mayors' wealth growing by 4.6 to 6.8 percentage points faster than that of runner-ups.

Table A4: The private returns to political office in municipalities with low judicial oversight

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(8.73)	MSE(13.72)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	4.694**	2.670	6.802***	1.218	0.421	5.666*
Fisher's p-values	[0.014]	[0.116]	[0.002]	[0.564]	[0.928]	[0.058]
Polynomial:	Linear	Quadratic	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	11.969	10.523	12.995	10.441	8.481	13.287
Return of Runner-Up:	7.275	7.853	6.193	9.223	8.060	7.621
N	720	990	496	496	280	280
Matched Pairs	360	495	248	248	140	140

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control), both from the same municipality with low judicial oversight, i.e. not a judiciary district (*comarca*). MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, with $N/2$ units on either side of the cutoff.

In turn, Table A5 shows the returns from mayoral offices in municipalities with low political competition, where votes are concentrated in just a few parties. Specifically, it comprises municipalities with above median scores for a Herfindahl–Hirschman Index (HHI) of vote concentration across parties.[42] The results mostly point to a positive and significant return to office, although column 5 reports a negative and significant estimate, and thus should be interpreted with caution.

[41] Bastos and Bologna Pavlik (2025) also use this variable as an instrument for corruption, and Melo and Pereira (2013) use similar measures of judicial activity at the state level as proxies for checks and balances.

[42] Brazil has a multi-party system, and there has been as many as 35 parties during the period of analysis; currently, there are 29 registered parties.

Table A5: The private returns to political office in municipalities with low political competition

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(7.93)	MSE(10.29)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	4.029*	4.393**	6.273***	1.916	-7.820**	6.799*
Fisher's p-values	[0.060]	[0.018]	[0.004]	[0.458]	[0.028]	[0.068]
Polynomial:	Linear	Quadratic	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	12.422	12.550	13.816	12.073	5.936	14.846
Return of Runner-Up:	8.393	8.158	7.542	10.158	13.756	8.046
N	570	658	416	416	228	228
Matched Pairs	285	329	208	208	114	114

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control), both from the same municipality with low political competition (above median scores for a Herfindahl–Hirschman Index (HHI) of vote concentration across parties). MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, with $N/2$ units on either side of the cutoff.

Table A6 reports the results for municipalities with high available rents. These are defined as municipalities with above median state and federal transfers per capita. Brollo et al. (2013) find that municipalities with larger windfall revenues from federal transfers have higher corruption levels, and similar evidence exists from municipalities receiving windfall royalties from oil exploration (e.g., Monteiro and Ferraz, 2012; Caselli and Michaels, 2013; Bhavnani and Lupu, 2016). Again, all significant coefficients are positive, suggesting the returns of elected mayors are around 4 to 8 percentage points larger than that of runner ups.

Table A6: The private returns to political office in municipalities with high available rents

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(12.22)	MSE(15.61)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	4.278**	2.578	4.865*	4.303	-5.980	8.082**
Fisher's p-values	[0.048]	[0.168]	[0.064]	[0.110]	[0.128]	[0.032]
Polynomial:	Linear	Quadratic	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	13.482	11.005	12.593	12.217	5.896	15.298
Return of Runner-Up:	9.204	8.426	7.728	7.914	11.876	7.216
N	710	792	370	370	200	200
Matched Pairs	355	396	185	185	100	100

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control), both from the same municipality with high available rents (above median state and federal transfers per capita). MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, with $N/2$ units on either side of the cutoff.

Finally, Table A7 uses a state-level index of "checks and balances" developed by Alston et al. (2009). I consider municipalities from the 13 (out of 26) states in the bottom-half of the ranking. The index captures the operational capacity of the judiciary and independent regulatory and oversight agencies. For instance, it uses the number of prosecutors, and their supporting staff and budget, per 100,000 residents, and the presence of an independent budget audit office. It also captures measures of media presence and of civic engagement through media concessions, voter turnout, and nonprofit sector workers per capita. Although less significant – presumably because it ignores within-state variation – these results are broadly in line with the main findings.

Table A7: The private returns to political office in municipalities with high institutional quality

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(7.13)	MSE(15.80)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	7.122*	5.530**	4.671	2.729	5.717	5.447
Fisher's p-values	[0.066]	[0.046]	[0.242]	[0.486]	[0.334]	[0.370]
Polynomial:	Linear	Quadratic	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	9.974	8.790	10.068	10.070	10.411	11.479
Return of Runner-Up:	2.853	3.260	5.397	7.340	4.694	6.032
N	246	404	186	186	106	106
Matched Pairs	123	202	93	93	53	53

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control), both from the same municipality with high institutional quality. MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, with $N/2$ units on either side of the cutoff.

A.4 Selection and Voter Preferences

Here I detail the entropy balance (Hainmueller, 2012; Hainmueller and Xu, 2013) process used to create comparable municipalities. I first collect data on all mayoral candidates and additional municipal-level characteristics for the municipalities included in each sample. Mayoral candidate characteristics include the mean age and the percentage of candidates (i) with a secondary degree or less, (ii) with a college degree, (iii) that married, and (iv) that are female. Additional variables include the distance to the state capital, GDP per capita, voter turnout, and the share of population that are Bolsa Família recipients, a direct cash-transfer program, used as a proxy for poverty.

The weighting procedure aims to balance these variables along municipalities with high accountability (high IQIM or with a local radio) and low accountability (low IQIM or without a local radio). Intuitively, we want that municipalities of either group to be similar on average. Figure A6 reports the standardized difference between municipalities with below- and above-median IQIM scores before and after weighting; Figure A7 does the same for municipalities with and without a local radio station. While the first group differs mostly in terms of income levels (hence the large gap in GDP per capita and the proxy for poverty), the differences are much more significant on the latter. It provides an important adjustment, since candidates in municipalities without a local radio are much less educated, on average.

To the extent that my goal is to investigate candidate choice *conditional on* the candidate pool, the entropy balance procedure does a great job in reducing these differences to zero. Still, it is important to recognize that candidates presumably consider their odds of winning when deciding whether to run. Thus, not only the winning but the whole candidate pool itself may reflect voter preferences because unqualified candidates may choose not to run if they expect they will not attract sufficient votes.

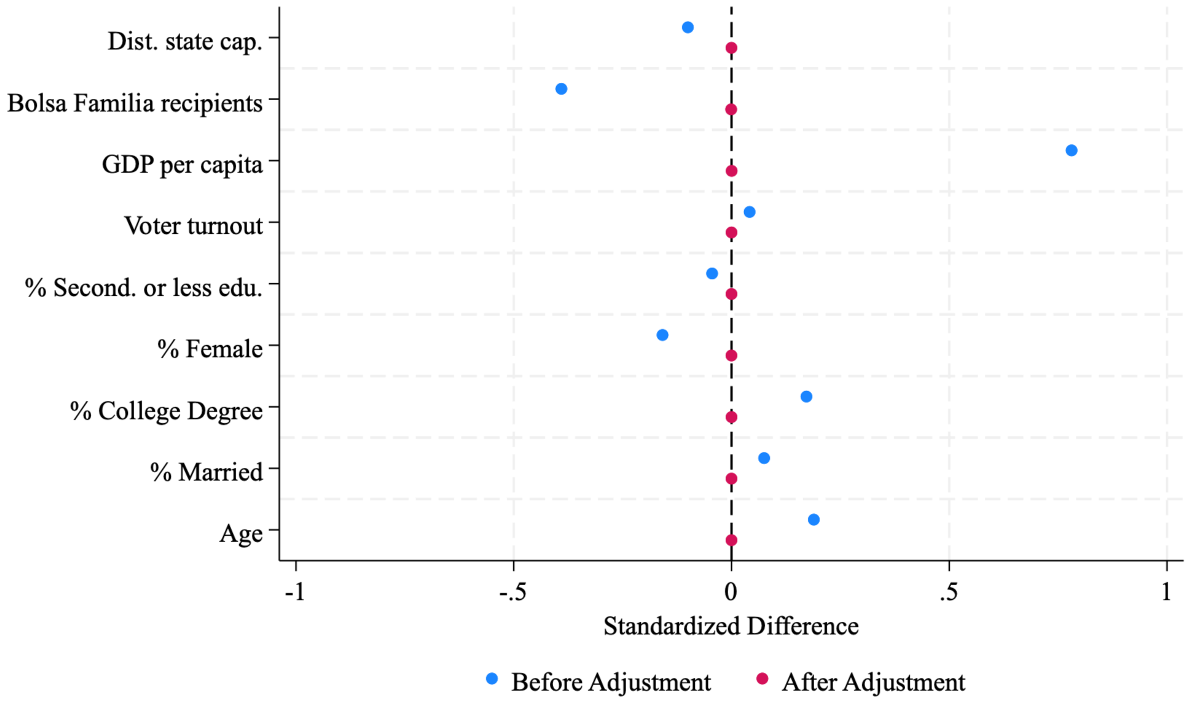


Figure A6: Municipality and Candidate Pool Characteristics (IQIM Sample)

Notes: This figure plots the standardized differences in municipality and candidate pool characteristics between municipalities with low and high institutional quality, before and after adjustment using entropy balance weighting (Hainmueller, 2012; Hainmueller and Xu, 2013). The weights are used in the estimates of Table 8.

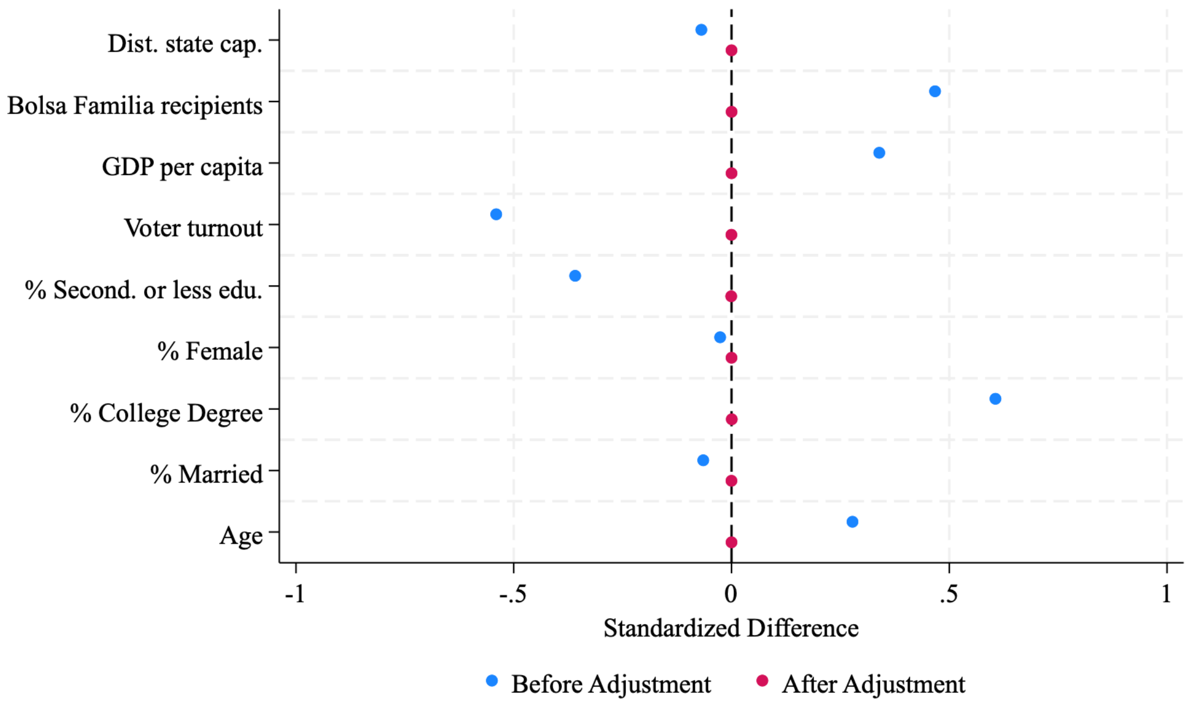


Figure A7: Municipality and Candidate Pool Characteristics (Local Radio Sample)

Notes: This figure plots the standardized differences in municipality and candidate pool characteristics between municipalities with and without a local radio news station, before and after adjustment using entropy balance weighting (Hainmueller, 2012; Hainmueller and Xu, 2013). The weights are used in the estimates of Table 8.

A.5 Reelection

As explained in the main text, I rely exclusive on mayors in their first term, due to two consecutive term limit. Since candidates are only required to report their wealth when registering the candidacy (t=0), I can only observe the wealth of those that run again (either for reelection as incumbent or as contestant) in $t = 4$. To observe the wealth of second-term mayors at some point during or after their second term, it would require them to run again in one of the four points highlighted in Figure A8.

Case 1 ($t = 6$) is a federal election in the middle of their second term. A reelected mayor would have to resign from their second term, six months before the election to be able to register as a candidate – only 6 mayors did this. Case 2 ($t = 8$) would be subsequent local election following their second term. Here, they can only run as city councilors, because they would face term limits as mayors or vice-mayor – only three mayors did this. In cases 3 ($t = 10$) and 4 ($t = 12$), mayors reelected in $t = 4$ would be free to run to any position[43] in a federal and local election, respectively. Among mayors elected first elected in 2008 (and reelected in 2012), there are 277 mayors that ran in 2020 ($t = 10$), and 40 that ran iun 2022 ($t = 12$).

Although the sample significantly increases for these elections, estimating any causal effects would require very strong assumptions about their returns during their time off. That is, whatever is the growth between $t = 4$ and $t = 10$ or $t = 12$, it encompasses their second term ($t = 4 \rightarrow t = 8$) and time out of office ($t = 8 \rightarrow t = \{10; 12\}$). Assigning part or all of it to the second term leads to implicit arbitrary assumptions about their relative importance. Additionally, it is unclear who would be the appropriate counterfactual for such mayor; even if we could find a matching candidate who barely lost across all elections, it becomes hard to argue that the variation is random after several losses.

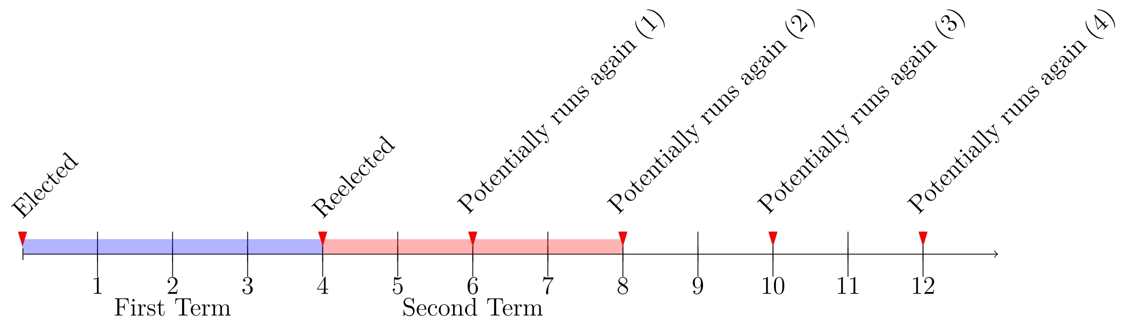


Figure A8: Stylized timeline of elections

[43]Recall that they face *consecutive* term limits, so they can be mayors again.

B Robustness Checks

B.1 Including covariates

Table B1: The private returns to political office, including covariates

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(9.68)	MSE(16.14)	5	5	1	1
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	0.106	-0.331	-0.353	-0.002	-2.000	-0.939
	(1.734)	(1.953)	(3.010)	(2.025)	(7.923)	(4.775)
Covariates:	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial:	Linear	Quadrat.	Linear	Diff. Means	Linear	Diff. Means.
N	5199	7240	3073	3073	653	653
$[N_-; N_+]$	[2234;2965]	[2987;4253]	[1397;1676]	[1397;1676]	[321;332]	[321;332]

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t$+4), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control). All columns include covariates (initial wealth, age, and dummies for female, white, and college degree) following the method proposed by Calonico et al. (2019). MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Columns 1-6 report robust bias-corrected standard errors (Calonico et al., 2014) in parenthesis. Columns 3-6 also report Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, and $[N_-;N_+]$ report the number of observations left and right of the cutoff.

B.2 Using entropy balance weighting

Table B2: The private returns to political office, using entropy balance weighting

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(10.05)	MSE(16.52)	5	5	1	1
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	-0.164	0.468	-0.248	-0.120	-2.887	-0.706
	(2.270)	(1.814)	(3.109)	(2.087)	(8.251)	(5.011)
Fisher's p-values			[0.151]	[0.473]	[0.569]	[0.746]
Entropy Weights:	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial:	Linear	Quadrat.	Linear	Diff. Means	Linear	Diff. Means.
N	5336	7327	3073	3073	653	653
$[N_-; N_+]$	[2294;3042]	[3017;4310]	[1397;1676]	[1397;1676]	[321;332]	[321;332]

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t$+4), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control). All columns use entropy balance weighting (Hainmueller, 2012) to achieve perfect covariate balance. MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Columns 1-6 report robust bias-corrected standard errors (Calonico et al., 2014) in parenthesis. Columns 3-6 also report Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, and $[N_-;N_+]$ report the number of observations left and right of the cutoff.

B.3 Accounting for Reporting Omissions

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-onward data into ten general categories based on similarity. This aggregation procedure is provided in Table B4. I start with the data for all candidates in the 2012-2024 local elections containing 3,798,577 assets, 95% of which I use to train the model and the remaining 5% 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), and 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 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).[44] 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. Crucially, it is especially effective in classifying 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 groups. This information is summarized in Table B3.

Figure B1 plots the distribution of wealth across these categories for all candidates and the general population, using income tax data from *Receita Federal*. As highlighted by Souto-Maior and Borba (2019), candidates tend to under report their cash balances and financial assets, which inflates the share of their wealth coming from real estate. Still, Figure B2 shows that their wealth has grown in line with the general population.

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

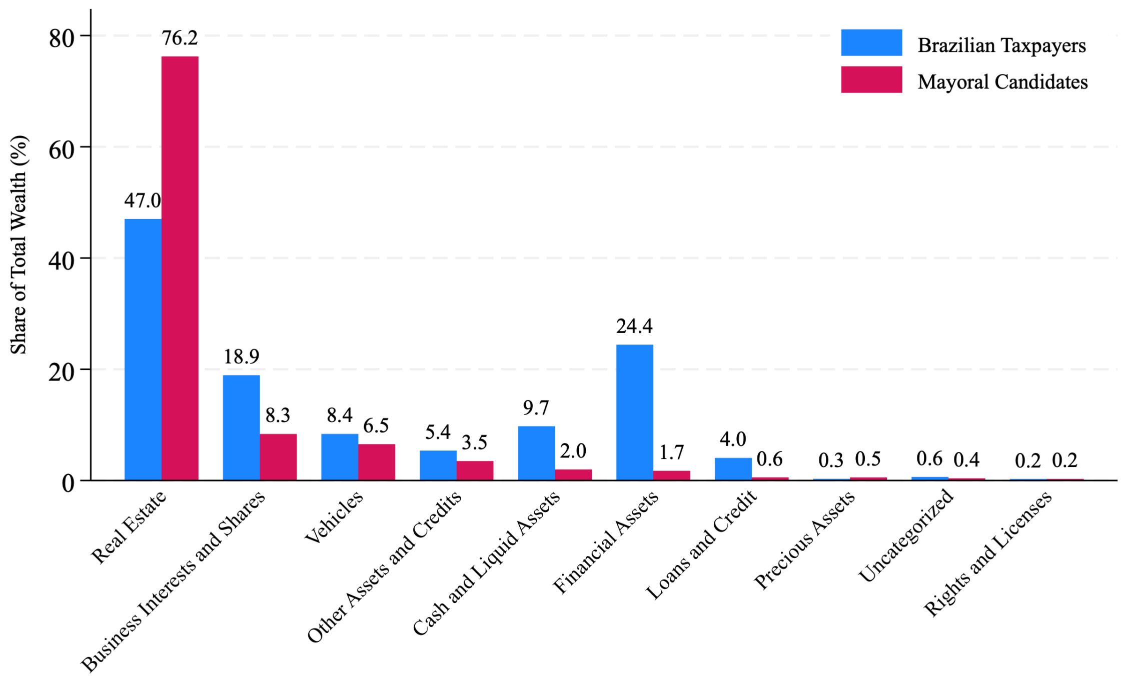


Figure B1: Distribution of wealth across key asset categories, candidates vs. all taxpayers

Note: This figure plots the relative shares of different asset categories (see Table B3 for grouping details), for mayoral candidates and the general population, averaged for election years (2008, 2012, 2016, and 2020). Data for the general population is drawn from income tax data (*Receita Federal*); not available for 2021 onward.

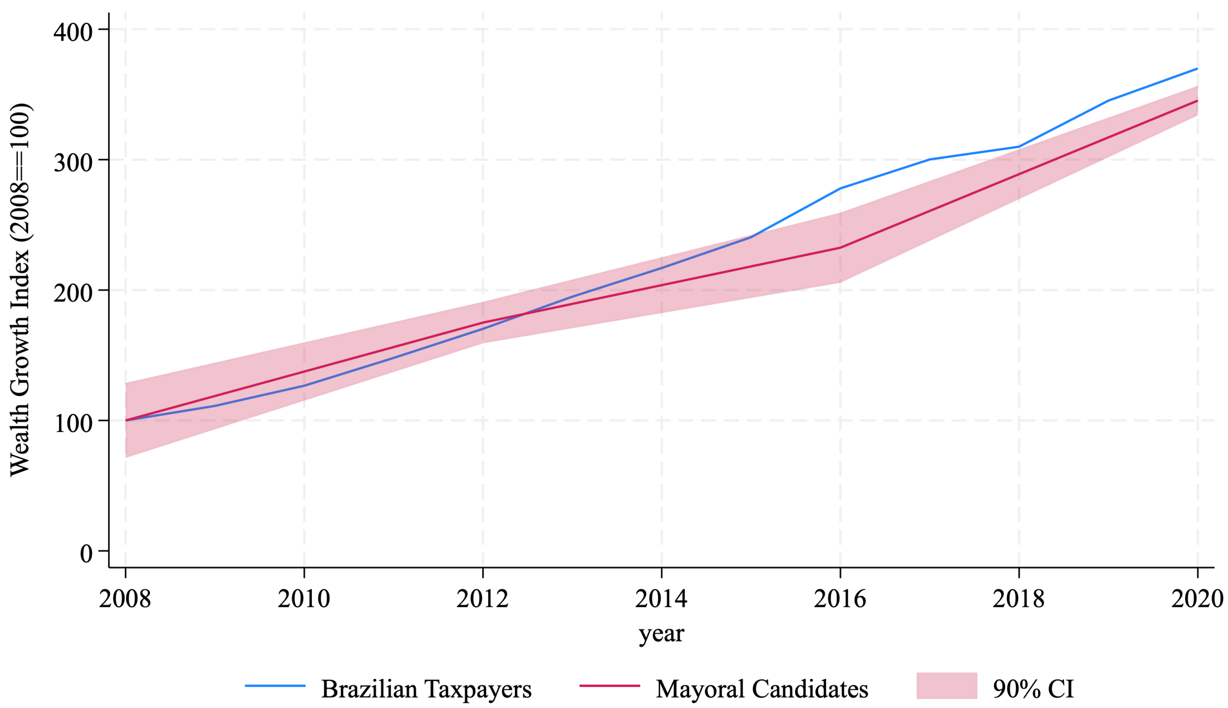


Figure B2: Wealth growth, candidates vs. all taxpayers

Note: This figure plots an index of average wealth growth (2008=100) for mayoral candidates and the general population. Values are computed yearly for the general population and every four years for candidates (in 2008, 2012, 2016, and 2020). Data for the general population is drawn from income tax data (*Receita Federal*); not available for 2021 onward. Shaded 90% confidence interval for candidates.

Table B3: Model performance by category

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 B4: Category mapping

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	**Other Assets & Credits**
Construction	Asset related to self-employment
Improvements	Other assets and rights
Savings for construction or acquisition of real estate	Other movable assets
	Leasing
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 (Private pension plan)	
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's rights and patents
Jewelry, paintings, antiques, etc.	Mining rights and similar
Club membership and similar	Special licenses and concessions
	PAIT plan and savings account

Table B5: The private returns to political office, accounting for reporting omissions

Outcome Variable:	Annualized Adjusted Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(8.99)	MSE(17.17)	5	5	1	1
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	-1.012	-2.093	-1.844	-2.153	0.222	-1.702
	(2.322)	(2.406)	(3.808)	(2.593)	(9.601)	(5.844)
Fisher's p-values					[0.904]	[0.532]
Polynomial:	Linear	Quadrat.	Linear	Diff. Means	Linear	Diff. Means.
Return Winner:	5.742	4.731	6.442	4.123	5.622	6.232
Return Runner-Up:	6.754	6.824	8.286	6.276	5.399	7.934
N	4809	7321	3009	3009	634	634
$[N_{-}; N_{+}]$	[2093;2716]	[2995;4326]	[1372;1637]	[1372;1637]	[311;323]	[311;323]

Notes: This tables replicates Table 2 while accounting for wealth reporting omissions. Effect is the annualized difference in growth rates ($t \rightarrow t+4$) of wealth derived from real estate and vehicles ("adjusted" wealth), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control). MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Columns 1-6 report robust bias-corrected standard errors (Calonico et al., 2014) in parenthesis. Columns 3-6 also report Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, and $[N_{-};N_{+}]$ report the number of observations left and right of the cutoff.

B.4 Political Experience

To test the role of political experience, I narrow my sample to investigate the returns of current elected mayors that have served as mayors or in other elected offices in the in the past. I collect data on 30 years of local and federal elections, between 1994 and 2024.[45] For any given year of my main sample, I obtain information on whether the winning candidates have been elected to office before. About 32% of the winning mayors and 39% of the runner ups have prior political experience. Among winning mayors (runner-ups), 16.3% (16.7%) have served as city councilors, 5.6% (3.3%) as vice-mayors, 14.9% (23.4%) as mayors, 2.3% (2%) as state deputies, 0.8% (0.3%) as federal deputies, 2 (0) as senators, and one (none) as governors.

First, I focus on those with previous experience as mayors. These are compared to their matched pairs, a runner-up who narrowly lost in the same municipality-election, regardless of whether the runner-up has had prior political experience. Critically, the implicit assumption is that prior political experience in an elected office offers no meaningful advantage in the private sector. I report the results in Table B6. Column 1 shows the only significant estimate, suggesting that winning former mayors accrue returns approximately 7 percentage points higher than runner-ups with any political experience. However, since the point estimates are highly dependent on specification, there is little evidence to support the hypothesis that mayors with prior experience have systematically larger returns.

[45] I start in 1994 because it is first year for which I can obtain a list of candidates that can be matched to the candidates in my main sample (2008-2024).

Table B6: The private returns to political office, winners with prior experience as mayors

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(11.81)	MSE(15.96)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	6.923**	1.525	-1.920	4.382	-6.440	1.864
Fisher's p-values	[0.012]	[0.530]	[0.686]	[0.300]	[0.282]	[0.776]
Polynomial:	Linear	Quadrat.	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	13.288	9.882	10.379	11.379	2.524	12.436
Return of Runner-Up:	6.365	8.356	12.299	6.997	8.964	10.571
N	226	264	104	104	58	58
Matched Pairs	113	132	52	52	29	29

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t$+4), measured in percentage points, between first-term mayors with prior experience as mayors that run for reelection (treatment) to runner-ups that decide to run again, regardless of their political experience (control). MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Columns 1-6 report robust bias-corrected standard errors (Calonico et al., 2014) in parenthesis. Columns 3-6 also report Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, with $N/2$ units on either side of the cutoff.

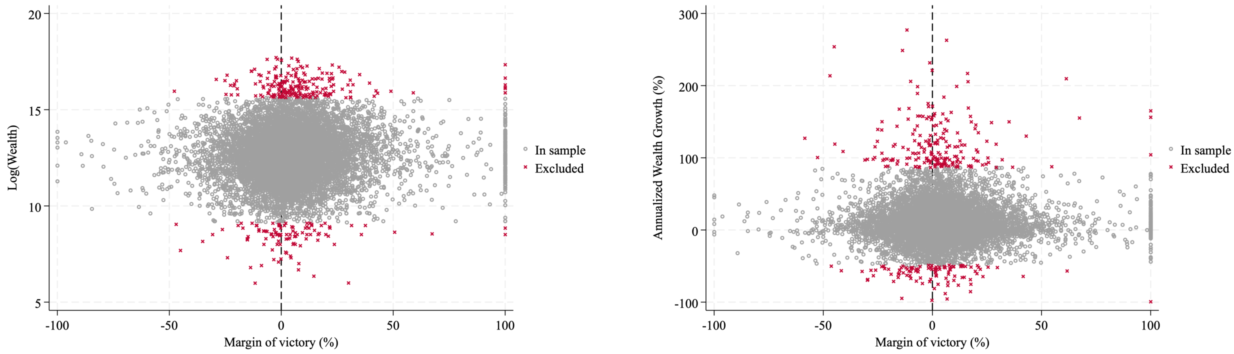
Table B7 uses an expanded sample in which the treated group includes elected mayors who have previously served in *any* of the elected political offices listed above, and compares them to matched controls regardless of political experience – results are substantially the same. I have also considered similar estimates in which matched control units have no prior political experience ("rookies"). However, this leads to a quite small samples, with as little as 14 matched pairs in the 2.5 bandwidth. In all cases, however, results are not significant. These results are available upon request.

Table B7: The private returns to political office, winners with prior experience in any elected office

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(11.07)	MSE(12.93)	5	5	2.5	2.5
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	3.785	3.348	1.021	5.690*	-5.522	5.242
Fisher's p-values	[0.118]	[0.132]	[0.786]	[0.070]	[0.250]	[0.296]
Polynomial:	Linear	Quadrat.	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	13.344	12.323	12.906	13.420	5.570	15.232
Return of Runner-Up:	9.559	8.976	11.885	7.730	11.091	9.990
N	430	476	218	218	128	128
Matched Pairs	215	238	109	109	64	64

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t$+4), measured in percentage points, between first-term mayors with prior experience in any elected office that run for reelection (treatment) to runner-ups that decide to run again, regardless of their political experience (control). MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Columns 1-6 report robust bias-corrected standard errors (Calonico et al., 2014) in parenthesis. Columns 3-6 also report Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, with $N/2$ units on either side of the cutoff.

B.5 Excluding extreme outliers



(a) Outliers in Initial Wealth (b) Outliers in Annualized Wealth Growth

Figure B3: Excluding extreme outliers

Notes: Figures report distribution of values in and out of sample after the exclusion of the top (≥ 99) and bottom (≤ 1) percentiles of each measure.

Table B8: The private returns to political office, excluding outliers

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
Bandwidth:	MSE(9.89)	MSE(16.52)	5	5	1	1
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	0.246	-0.482	1.229	0.281	-2.146	-0.673
	(1.306)	(1.978)	(2.126)	(1.493)	(5.066)	(3.076)
Fisher's p-values					[0.206]	[0.672]
Polynomial:	Linear	Quadrat.	Linear	Diff. Means	Linear	Diff. Means.
Return of Winner:	9.354	11.277	10.734	9.176	5.967	9.388
Return of Runner-Up:	9.108	11.759	9.504	8.895	8.113	10.061
N	4983	7327	2902	2902	612	612
$[N_-; N_+]$	[2141;2842]	[3017;4310]	[1317;1585]	[1317;1585]	[295;317]	[295;317]

Notes: This sample excludes the top and bottom percentiles in terms of initial wealth and wealth growth. Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control). MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Columns 1-6 report robust bias-corrected standard errors (Calonico et al., 2014) in parenthesis. Columns 5-6 use Fisher's p-values (1,000 permutations) for inference, reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, and $[N_-;N_+]$ report the number of observations left and right of the cutoff.

B.6 Alternative designs and identification strategies

Previous studies in the literature on private returns to office have used different additional methods as complements to their main identification strategy. Here, I follow previous literature (e.g., Lenz and Lim, 2009; Eggers and Hainmueller, 2009; Boas and Hidalgo, 2011; Keele et al., 2015) and consider matching as a complementary specification to my RD design. The Mahalanobis Distance Matching procedure attempts to minimize the Euclidean distance along a set of covariates, and compares a given treatment unit to its n closest controls (nearest neighbors). Table B9 shows the results for 1-3 nearest neighbors with Abadie and Imbens (2006) standard errors in parenthesis, which are highly in line with the main findings in Table 2.

Table B9: The private returns to political office, Mahalanobis Distance Matching

Outcome Variable:	Annualized Wealth Growth (in %)					
Bandwidth:	5			1		
Nearest Neighbors:	1	2	3	1	2	3
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	-0.476	0.228	0.532	-3.254	-2.182	-0.809
	(1.439)	(1.243)	(1.157)	(2.722)	(2.565)	(2.387)
Fisher's p-values	[0.266]	[0.254]	[0.270]	[0.268]	[0.260]	[0.234]
N	3073	3073	3073	653	653	653
$[C;T]$	[1399;1674]	[1399;1674]	[1399;1674]	[323;330]	[323;330]	[323;330]

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control). Abadie and Imbens (2006) standard errors in parenthesis. Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, and $[C;T]$ report the number of control (runner-ups) and treated (winners) units.

B.7 By cohort

Table B10: The private returns to political office, by election cohort

Outcome Variable:	Annualized Wealth Growth (in %)					
Design:	Continuity		Local Randomization			
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2008-2012 Cohort						
Effect	1.694	1.955	-1.617	2.045	1.193	-5.623
	(3.643)	(4.856)	(8.103)	(5.133)	(32.385)	(15.999)
Fisher's p-values	[0.312]	[0.198]	[0.470]	[0.346]	[0.884]	[0.306]
Bandwidth:	MSE(12.86)	MSE(16.66)	5	5	1	1
Polynomial:	Linear	Quadrat.	Linear	Diff. Means	Linear	Diff. Means.
N	1378	1577	668	668	135	135
$[N_-; N_+]$	[602;776]	[697;880]	[309;359]	[309;359]	[68;67]	[68;67]
Panel B: 2012-2016 Cohort						
Effect	0.639	1.643	0.934	0.140	3.829**	2.499
	(3.208)	(3.702)	(5.040)	(3.501)	(10.749)	(6.627)
Fisher's p-values	[0.686]	[0.200]	[0.604]	[0.936]	[0.382]	[0.548]
Bandwidth:	MSE(8.87)	MSE(13.22)	5	5	1	1
Polynomial:	Linear	Quadrat.	Linear	Diff. Means	Linear	Diff. Means.
N	1278	1711	832	832	185	185
$[N_-; N_+]$	[599;679]	[766;945]	[409;423]	[409;423]	[93;92]	[93;92]
Panel C: 2016-2020 Cohort						
Effect	-2.711	-3.762	-2.104	-1.375	3.267	-0.159
	(2.875)	(3.032)	(4.232)	(2.934)	(8.806)	(6.205)
Fisher's p-values	[0.056]*	[0.000]***	[0.204]	[0.398]	[0.314]	[0.944]
Bandwidth:	MSE(7.85)	MSE(14.18)	5	5	1	1
Polynomial:	Linear	Quadrat.	Linear	Diff. Means	Linear	Diff. Means.
N	1285	1985	898	898	197	197
$[N_-; N_+]$	[556;729]	[796;1189]	[395;503]	[395;503]	[97;100]	[97;100]
Panel D: 2020-2024 Cohort						
Effect	3.410	2.266	3.008	0.350	-19.578	-1.846
	(4.037)	(4.927)	(8.400)	(5.398)	(19.807)	(14.304)
Fisher's p-values	[0.044]**	[0.120]	[0.176]	[0.888]	[0.000]***	[0.748]
Bandwidth:	MSE(11.05)	MSE(16.99)	5	5	1	1
Polynomial:	Linear	Quadrat.	Linear	Diff. Means	Linear	Diff. Means.
N	1295	1671	675	675	136	136
$[N_-; N_+]$	[505;790]	[622;1049]	[284;391]	[284;391]	[63;73]	[63;73]

Notes: Effect is the annualized difference in wealth growth rates ($t \rightarrow t+4$), measured in percentage points, between first-term mayors that run for reelection (treatment) to runner-ups that decide to run again (control). MSE(h) is a data-driven optimal bandwidth choice using the Mean Squared Error method (Calonico et al., 2014, 2020). Columns 1-6 report robust bias-corrected standard errors (Calonico et al., 2014) in parenthesis. Columns 3-6 also report Fisher's p-values (1,000 permutations) in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. N reports the total number of observations, and $[N_-;N_+]$ report the number of observations left and right of the cutoff.

C Election and Campaign Finance Rules

During the period of analysis (2008-2024), elections in Brazil had different campaign financing rules. Until 2014, campaigns were financed through a mix of donations from individuals, companies, and public funds. There was no limit on campaign spending, but individuals could only contribute up to 10 percent of their annual income (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. At the beginning of my period, specially in 2008-2012, it amounted to a small share of overall contributions (Bourdoukan, 2010). With the ban on donations from private companies, public election funds have increased substantially over the years both in shares , 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 one billion USD) in the 2022 and 2024 elections.

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 could only receive donations (and spend their funds) within the electoral period, which was of roughly 90 days in 2008 and 2012, and of roughly 45 days since 2016. The election day is always 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.

Beginning in the 2016 elections, an campaign spending limit was introduced and the campaign period was shortened from 90 to 45 days; additionally, donations from companies were banned. The spending limit for mayors varied by municipality. Mayors could only spend 70% of the highest amount spend by a candidate of that municipality in the 2012 elections; however, it also established a floor of R$ 100,000 (around 30,000 USD at the time), in case the 70%-rule generated a lower value. Since them, these maxima are only corrected for inflation (Avis et al., 2022).

The ban on donations from companies stemmed from a Supreme Court decision, following an 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."[46]

[46] Direct Action of Unconstitutionality/ADI n. 4.650/DF. For an analysis of the decision, see Guerra Filho (2017).

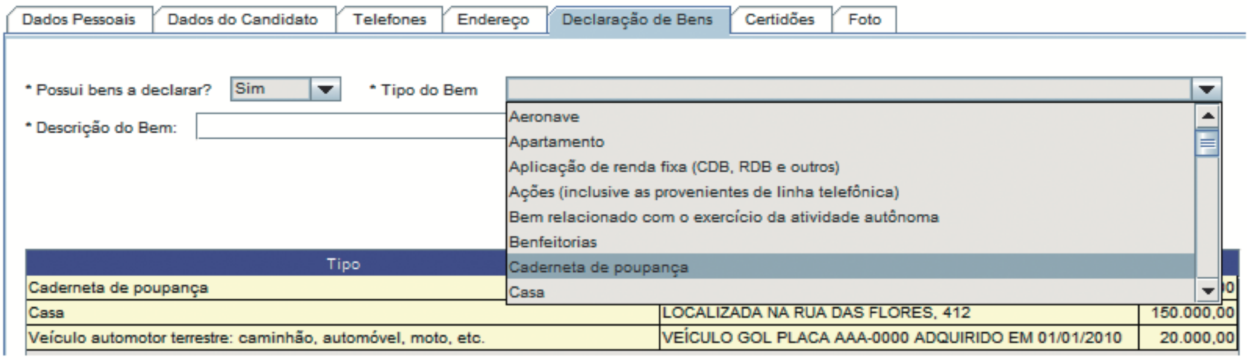


Figure C1: Official TSE software for registering candidates (CANDex)

Note: Screenshot of TSE's CANDex software, used by the parties to register their candidates. This specific screen shows the tab where candidates input their assets, following the income tax classification codes.

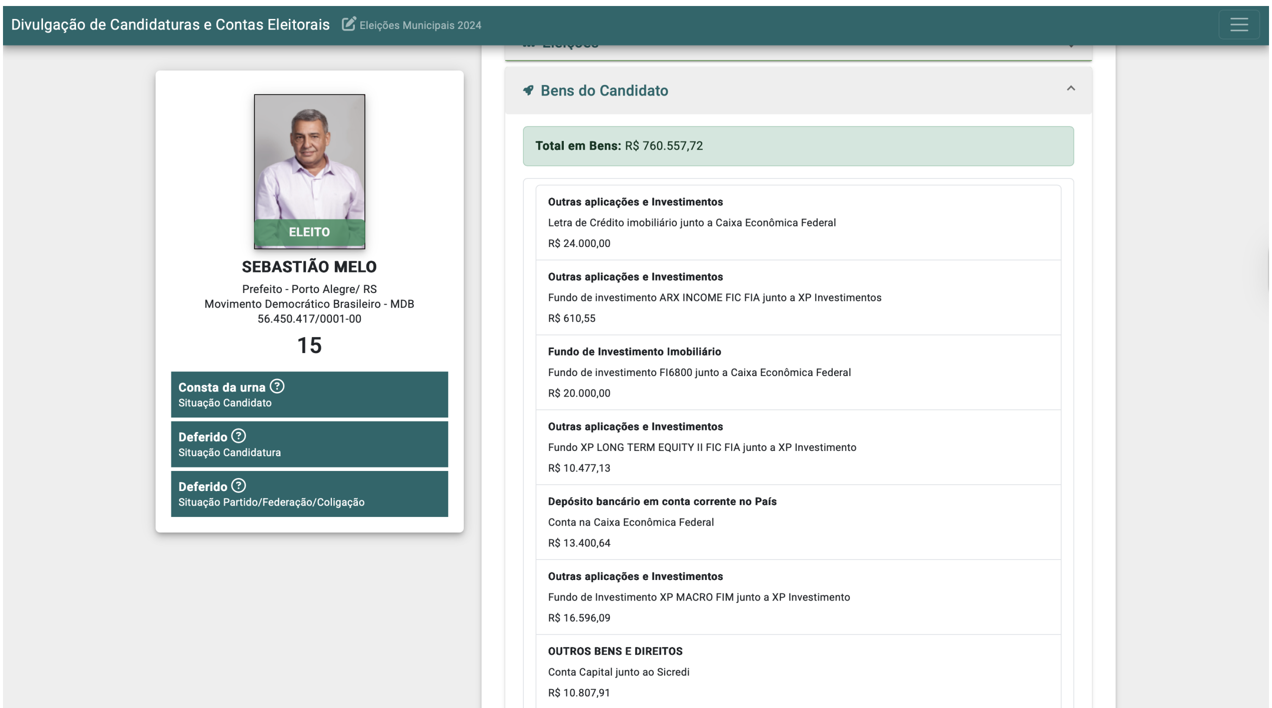


Figure C2: Official TSE candidate information platform

Note: Screenshot for the elected mayor of Porto Alegre, RS, in TSE's official website (`https://divulgacandcontas.tse.jus.br/`). Accessed on July 26th, 2025.

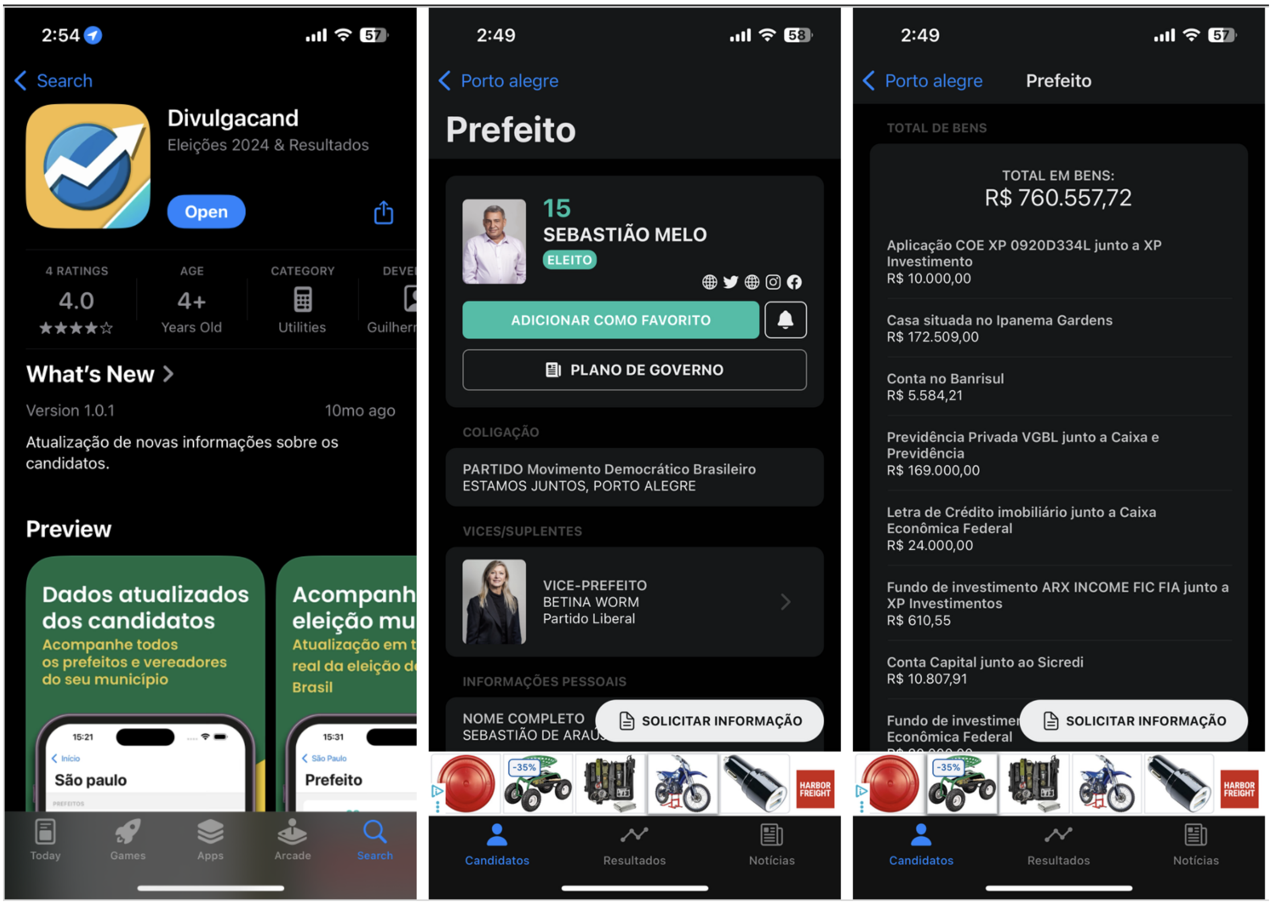


Figure C3: Unofficial mobile app (*DivulgaCand*)

Note: Unofficial mobile app in Apple AppStore and screenshots for the elected mayor of Porto Alegre, RS. Accessed on July 26th, 2025.