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ABSTRACT

A Few Bad Apples? Criminal Charges, Political Careers, and Policy Outcomes*

We study the prevalence and effects of individuals with past criminal charges among candidates and elected politicians in Brazil. Individuals with past criminal charges are twice as likely to both run for office and be elected compared to other individuals. This pattern persists across political parties and government levels, even when controlling for a broad set of observable characteristics. Randomized anti-corruption audits reduce the share of mayors with criminal records, but only when conducted in election years. Using a regression discontinuity design focusing on close elections, we demonstrate that the election of mayors with criminal backgrounds leads to higher rates of underweight births and infant mortality. Additionally, there is an increase in political patronage, particularly in the health sector, which is consistent with the negative impacts on local public health outcomes.

JEL Classification: K42, J45, P16

Keywords: politicians, crime, audits, policies, patronage

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1 Introduction

The personal traits of politicians are crucial to good governance in modern democracies, with honesty being an essential attribute (Caselli and Morelli, 2004; Besley, 2005; Dal Bó and Finan, 2018). The integrity of politicians is vital for building trust in political institutions, which in turn fosters greater political participation (Daniele et al., 2023). Moreover, a politician's honesty can significantly impact the quality of policy-making, exemplified by Benjamin Franklin's famous adage "Honesty is the best policy." Indeed, many countries have implemented rules regarding ineligibility for elected office based on criminal convictions (see, e.g. Transparency International, 2016). Brazil, Colombia, India, Italy, the UK, and several U.S. states, among others, have introduced measures to prevent individuals with a criminal history from running for office. Some proposals, such as those in India and Italy, even suggest banning individuals merely charged with a crime, in addition to those with definitive convictions.[1] These regulations aim to promote good governance and uphold ethical standards among political leaders.

In this paper, we study the prevalence and effects of individuals with previous criminal charges among electoral candidates and appointed politicians using rich individual-level data on criminal prosecutions for the entire Brazilian population. We first establish that the proportion of individuals with criminal charges among (first-time) candidates and politicians at all levels of Brazilian politics is twice as high as in the general population. We then estimate how anti-corruption audits affect the entry of individuals with prior criminal charges into politics, and the impact of the latter on the quality of policy making and on policy outcomes at the local level.

Our analysis focuses on criminal charges rather than convictions for two primary reasons. First, under Brazilian law, individuals convicted of certain crimes are prohibited from running for office, but there is no such ban on those who have only been charged. Second, focusing on criminal charges reduces the risk of false negatives – instances where individuals committed crimes but were discharged, for example, due to statutes of limitation.[2] Of course, our approach increases the risk of false positives – where individuals were prosecuted without sufficient cause. However, we believe that false negatives pose a greater risk than false positives when assessing criminal involvement among high-status individuals, such as

[1] See, for instance, https://www.theguardian.com/world/2014/apr/02/fifth-india-election-candidate-face-criminal-charges and https://thevision.com/politica/impresentabili-m5s/.

[2] According to sentence data for São Paulo – the largest Brazilian state – 34% of cases are dropped due to statutes of limitation, and only 17% of defendants going to trial are acquitted.

politicians. Additionally, we focus on criminal charges initiated against first-time candidates before they ran for office, as we are primarily interested in studying selection into politics based on "inherent" honesty and its effects on governance and policy-making. Opportunities to commit crimes and the likelihood of prosecution may differ between politicians and non-politicians, and these differences could bias in several ways the comparison between the two groups. Politicians may be more closely monitored while in office or during campaigns, but they could also use their influence to avoid investigation and prosecution. Politicians may also have more opportunities to commit certain crimes (e.g., embezzlement of public funds) compared to non-politicians. By focusing on criminal charges before entering politics, we aim to minimize these differences in crime opportunities and the likelihood of criminal prosecution.

The incidence of individuals with previous criminal charges among first-time candidates who ran for elections during the 2012-2020 period is 4.4%, increasing to 5.1% among those who were eventually elected, compared to only 2.3% in the general population. Although the relative difference in prosecution rates between politicians and non-politicians does not imply causality, it is unaffected when controlling for municipality-year fixed effects along with a wide array of observable characteristics such as age, race, education, and previous occupation. Furthermore, this pattern is consistent across political parties, levels of government, and types of crime, with a more pronounced effect for white-collar crimes, such as fraud and environmental offenses. The occurrence of other, "minor" crimes, such as traffic violations, also shows significant differences between politicians and non-politicians. In turn, politicians with a criminal background are more likely to be male, white, and older compared to other politicians without criminal charges. Interestingly, they are also less educated and less likely to have held formal employment or worked in public administration before their political careers.

This descriptive evidence aligns with several models of negative selection, where dishonest individuals are more likely to pursue political office (e.g., Caselli and Morelli, 2004; Besley, 2005). Consistent with these models' predictions, we show that a negative shock to rent-seeking opportunities, namely randomized anti-corruption audits, reduces the entry of "bad" actors into politics. Prior research has demonstrated that these audits are effective in lowering the re-election rates of corrupt mayors (Ferraz and Finan, 2008) and in reducing overall corruption levels (Avis et al., 2018).

Our analysis shows that anti-corruption audits temporarily decrease the proportion of first-time candidates and elected mayors with criminal charges, though no significant effects are seen for councilors. Importantly, this effect is limited to audits conducted during election years, and it fades within one year after the election. These results are in line with Avis et al. (2018), who suggest that anti-corruption audits reduce corruption mainly by raising the perceived legal risks of engaging in corrupt activities.

We then investigate the impact of electing mayors with prior criminal charges. Using a Regression Discontinuity (RD) design around the electoral margin for winning elections, we compare a variety of outcomes between municipalities where candidates with prior criminal charges narrowly won or lost the municipal elections in 2012 and 2016. We find evidence of adverse effects on several municipal indicators related to basic health care, a core public service managed by local governments in Brazil (Ferraz and Finan, 2011). Specifically, we observe an increase in the incidence of underweight births and a rise in infant mortality rates at various ages (+8% and +21%, respectively, compared to baseline averages in areas with non-criminal mayors). In contrast, we find no significant impact on education, another important policy domain influenced by local government.

These findings align with the fact that health expenditure is the budget item most frequently highlighted in federal corruption audits in Brazilian municipalities (Ash et al., forthcoming).[3] To better understand the mechanisms driving the effect on basic health, we examine the influence of criminally charged politicians on public expenditure but find no significant effects. We therefore hypothesize that the primary issue lies in the quality, rather than the quantity, of public spending. In fact, we find that criminally prosecuted mayors are more likely to engage in political patronage, particularly the systematic recruitment of their own party members into public sector positions (Colonnelli et al., 2020). On average, supporters of these politicians gain 3.3 additional public sector jobs annually, compared to just 2.3 jobs for marginal winners without criminal charges. These patronage practices likely contribute to the negative effects on public health, as politicians facing criminal charges tend to prioritize hiring political supporters over more qualified candidates. Supporting this hypothesis, we find that the patronage effect is concentrated in public health sector jobs, aligning with the observed decline in public health outcomes. In contrast, we find no evidence of patronage or adverse policy effects in public education. Thus, patronage may be a key mechanism through which mayors with criminal records undermine social welfare.

[3]Health expenditures are cited 190 thousand times in audits reports for 1,481 municipalities from 2003 and 2009. Spending in labor and education, ranking second and third, are cited 69 thousand and 59 thousand times, respectively.

This study contributes to the literature on political selection and its implications for governance and policy-making quality. As highlighted in comprehensive surveys by Besley (2005), Dal Bó and Finan (2018), Gulzar (2021), most theoretical studies in this field focus on two key dimensions of political selection: competence and honesty. For instance, Caselli and Morelli (2004) suggest that less competent and less honest individuals are more inclined to pursue political careers because they have fewer opportunities in the labor market and a higher propensity to exploit rent-seeking opportunities in political office. Besley (2005) argues that the proportion of bad politicians increases when rent-seeking opportunities are greater and decreases in political settings where public service motivation is stronger.

Testing these theoretical predictions requires individual-level data on the entire population, which is available only in a few (primarily wealthier) countries. Dal Bó et al. (2017) use Swedish registry data to document patterns consistent with the positive selection of politicians based on intelligence and leadership, measured through military enlistment tests. To our knowledge, this study, along with a follow-up paper by the same authors (Dal Bó et al., 2018), are the only works that examine selection into candidacy at the population level, whereas other research typically focuses on the characteristics of appointed politicians (e.g., Besley et al., 2011; Ferraz and Finan, 2009; Gagliarducci and Nannicini, 2013). Most importantly, none of these previous studies investigates the role of candidates' criminal history. Our study contributes to this body of research by showing that, in Brazil, individuals with a criminal record are more likely to run for office and be elected than the general population.

We also contribute to prior research examining the factors that influence the selection of 'good' and 'bad' candidates, such as the presence of organized crime (Daniele and Geys, 2015), transparency (Mattozzi and Merlo, 2007), salary (Ferraz and Finan, 2009; Gagliarducci and Nannicini, 2013), electoral laws (Mattozzi and Merlo, 2015; Beath et al., 2016; Arora, 2022) and electoral competition (Galasso and Nannicini, 2011). In contrast to these earlier studies, we do not assess politicians' quality based on income, education, or previous political experience; instead, we focus on criminal charges, a measure of honesty that is arguably relevant and salient to voters.

More broadly, our study relates to prior research on the selection of specific groups into politics, such as by educational background (Besley and Reynal-Querol, 2011; Cavalcanti et al., 2018), gender (Chattopadhyay and Duflo, 2004; Baltrunaite et al., 2014; Gagliarducci and Paserman, 2012), age (Alesina et al., 2019), leadership (Jones and Olken, 2005), personality traits (Dal Bó et al., 2017), and political and family connections (Dal Bó et al., 2009; Jia et al., 2015; Querubin et al., 2016; Daniele et al., 2021), and their consequences for policy-making and performance.

Our paper is most closely related to some recent papers on politicians in India. Asher and Novosad (2018) show that a positive shock to rent-seeking opportunities – namely, mining booms – attracts more individuals with criminal records into politics. Chemin (2012), Kim and Lee (2022), and Prakash et al. (2022, 2019) find that electing candidates with criminal records to State Legislative Assemblies negatively affects economic growth, crime, and the welfare of vulnerable groups. Compared to these earlier studies, we leverage data on the entire population, which allows us to more accurately characterize the selection into politics of individuals with past criminal charges. Additionally, we examine how the entry of such individuals is influenced by anti-corruption policies and how their election affects novel outcomes, such as political patronage and public health, showing that patronage is a key mechanism driving these public health effects.

The remainder of the paper is organized as follows. Section 2 provides the institutional background, while Section 3 describes the data. In Section 4, we compare the presence of individuals with past criminal charges among politicians and non-politicians in Brazil, and we investigate the effects of anti-corruption audits on political selection. Section 5 studies the consequences of electing criminally charged individuals. Section 6 concludes the paper.

2 Institutional background

2.1 The administrative and political system

Brazil is a federation with three administrative levels: the federal government, 27 states, and 5,570 municipalities. Citizens choose the executive and legislative branches through direct elections.

At the municipal level, the mayor (*Prefeito*) holds executive power while the city council (*Câmara de Vereadores*) has legislative power. The mandate of both mayors and city councilors lasts four years. The mayor has considerable power over the provision of a variety of public goods such as basic health care, primary education, culture, housing, transportation, and municipal infrastructure. The allocation of the budget across different areas is ultimately approved by the city council and is largely financed by transfers from the central government. The council also legislates in various areas that are under the municipality's responsibility, such as basic health care and primary education, and oversees the mayor's usage of public resources.

In addition, mayors retain considerable discretion over hiring in the public sector. Although selection in permanent public sector jobs is based on a formal civil service examination, other job positions are exempted: commissioned posts (*cargos comissionados*), positions of trust (*cargos de confiança*), and temporary jobs (*emprego temporário*). Colonnelli et al. (2020) provide evidence of political favoritism in hiring of public employees.

Citizens can run for office in the district where they reside and must be affiliated with their supporting political party for at least one year before the election. The president, state governors, and mayors lead the executive power in the country, states, and municipalities, respectively, and are elected by majority rule. In turn, legislative elections for councilors, state and federal deputies, and senators follow an open-list proportional system.[4] Finally, federal elections of governors, state and federal deputies, senators, and the president occur every 4 years, alternating with municipal elections in 2-year gaps.

2.2 Justice and politics in Brazil

The Brazilian judicial system is composed of federal, electoral, and 27 state courts, composed of 2,697 tribunals with jurisdiction over one or more municipalities.[5] Corruption cases involving the Federal government run in Federal courts, while the remaining cases run in State courts. Criminal cases against the president, governors, mayors, and federal legislators are handled by higher courts, as mandated by the Brazilian Constitution.

The judiciary system is largely independent of other government branches. Judges are highly paid public officials and are appointed for life through competitive public examinations. Nevertheless, there is some evidence that candidates winning the mayoral election by a narrow margin receive special treatment in courts (Lambais and Sigstad, 2023), which may constitute an additional benefit for individuals with criminal charges to enter politics.

[4] The distribution of legislative seats follows the d'Hondt method. Citizens can either vote for a candidate or vote for a party without specifying a candidate. The number of seats assigned to a party (or a pre-established party coalition) depends on how many votes the party received either through its candidates or directly. In turn, the allocation of seats within a party is based on the most voted candidates.

[5] In addition, there are Labor and Military specialized in such matters.

In 2010, following popular pressure against corruption scandals, federal legislators enacted a law designed to prevent corrupt politicians from staying in politics – "*Lei da Ficha Limpa*". This law comprises several measures. First, impeached politicians cannot be candidates for eight years in elections at any level. Second, individuals convicted for certain types of offenses are excluded from candidacy. These offenses include corruption and electoral crimes, a wide set of white-collar crimes, and other offenses such as crimes against life, drug trafficking, and sexual crimes. Although specific aspects of the law have been subject to discussion in higher courts, they have been widely applied since the 2012 elections. Therefore, only individuals with an ongoing criminal trials matter for our analysis, whereas convicted individuals should be excluded from electoral competitions. We will further discuss this distinction in the empirical analysis.

3 Data

Our empirical analysis leverages multiple administrative datasets tracking electoral outcomes and individual-level information on criminal judicial cases for politicians and the general population. To this purpose, we first combine population employment records for the period 2002-2019 (*Relaçao Anual de Informaçoes Sociais, RAIS*) to a welfare registry maintained by the Federal government (*Cadunico*) that covers two-thirds of the Brazilian population.[6] Both datasets contain a wide array of demographic characteristics (birth date, gender, race, education, municipality, among others) as well as individuals' (full) names and their unique person codes (*CPF*). *Cadunico* is primarily used for the administration of Federal social programs, so it mainly covers the low and middle part of the income distribution, whereas *RAIS* mainly includes formal workers in the middle and upper part of the distribution. Taken together, the two registries cover 95% of the adult Brazilian population, allowing us to compare politicians to the general population along several dimensions.

We also use electoral data provided by the Brazilian Superior Electoral Tribunal (*TSE*), which identify all individuals running for office during the period 2000-2020. In particular, the data report the name and unique person code of each candidate, in addition to rich information on party affiliation, campaign spending and revenues, and demographic characteristics (birth date, gender, race, education, among others).

[6] We combine yearly snapshots of *Cadunico* during 2011-2020, for a total of 135 million individuals.

The third dataset we use is the universe of criminal prosecutions in state courts initiated in the 2009-2020 period – about 18 million cases – as available from the public court diaries of 27 Brazilian state courts.[7] These records identify each defendant by their name and contain information on case filing dates, court location, and subjects, which allow us to classify crime types. Importantly, these prosecutions may or may not lead to a final conviction. Information on (first-degree) sentences is available only for the state of São Paulo during the 2009-2018; we will use such information for robustness analysis (Section 4.2).[8] Alternatively, we map definitive convictions in all states by mapping sentence execution cases which are initiated after definitive sentences to prison, which we also use for robustness analyses as a proxy for definitive convictions. However, execution cases cover only a small share of all cases in our analysis, because reaching a definitive sentence typically takes several years and numerous appeals to higher courts.[9]

The final dataset tracks criminal justice charges for the entire Brazilian population during the period 2009-2020 along with electoral outcomes for all individuals running for election during the same period. To improve computational speed, we compare the universe of politicians with a two-percent random sample of the general population stratified by municipality.[10] We link the different data sources by unique person codes whenever possible, and by individual name and state of residence when the unique person code is not available – notably, in the judicial data. As in Britto et al. (2022), we thus restrict the sample to individuals who have unique names in each Brazilian state, which is very large (about 70% of the population) due to the fact that Brazilians typically carry multiple surnames from their parents.[11] Importantly, individuals with unique names within each state do not differ significantly from the general population in terms of a wide array of socio-demographic characteristics (see Britto et al., 2022).

Given that criminal prosecution data are available only from 2009, we restrict attention to elections taking place from 2012 onward, so as to have information on prosecutions for at least three years before each election.[12]

[7]These data are collected by a private firm offering services for legal enterprises. The Brazilian constitution establishes that all judicial cases are public, with very few exceptions for cases concerning sexual crimes.

[8]First-degree sentences by the State Tribunal of São Paulo are available at link.

[9]Based on the 2018 report by the National Council of Justice, the average time for a second degree decision, which may not even be definitive, is 4.5 years. Therefore, we observe sentence execution cases for only 3.6% of individuals facing any criminal charges.

[10]We also limit the sample to individuals born until 1999.

[11]We use the population registry to identify individuals with unique names in each state.

[12]We exclude the 2010 elections from the analysis because we would have information on prosecutions only for a single year before elections (2009).

We complement the individual-level data on judicial prosecutions and political careers with municipality-level data on several socio-economic indicators, including (but not limited to) infant mortality and other birth outcomes, educational attainment, municipal spending by area, female labor force participation, fertility, digital divide, and electricity supply. These data, described in detail in Appendix Section A.2, will allow us to investigate the impact of electing candidates with past criminal charges on politicians on local outcomes.

Finally, we obtained information on federal anti-corruption audits for the period 2003-2015 – notably, audit dates and the set of audited municipalities – from the *Controladoria Geral da União* (CGU).

4 Criminal Charges and Entry into Politics

In this section, we investigate the entry of criminally prosecuted individuals into Brazilian politics. We compare the prevalence of criminal prosecutions between first-time candidates running for (and possibly winning) each election during the period 2012-2020 and the general population. We consider criminal prosecutions filed between 2009 (the first year for which our judicial data are available) and the year before each election. Therefore, for the 2012 elections we consider criminal prosecutions during the period 2009-2011, for the 2014 elections we consider prosecutions during the period 2009-2013, and so on. We then stack these datasets for all elections.

Prosecutions may lead to convictions or acquittals, or they may be dropped for having reached statutes of limitation. Sentence data for the state of São Paulo during the 2009-2019 period indicate that only 17% of cases going to trial result in an acquittal.[13] The same data show that 34% of all cases never reach the trial stage because they exceed the statutes of limitation.

Importantly, focusing on candidates running for the first time ensures that any difference in prosecution rates between politicians and the general population are not driven by politicians being more under the law enforcement spotlight or, conversely, being able to avoid prosecutions thanks to their influence.

[13] We will provide a robustness test restricting the sample to the state of Sao Paulo and excluding acquitted individuals (details in the next section).

4.1 Stylized Facts

Figure 1 plots the share of individuals with previous criminal charges in the general population and among first-time candidates and elected politicians during the period 2012-2020. Strikingly, the incidence of individuals with previous criminal charges is twice as high among first-time candidates and politicians (4.4% and 5.1%, respectively) than in the general population (2.3%). This pattern arises in spite of the fact that a (small) share of criminally prosecuted individuals receiving a definitive conviction before the elections cannot run due to the *Ficha Limpa* law.[14]

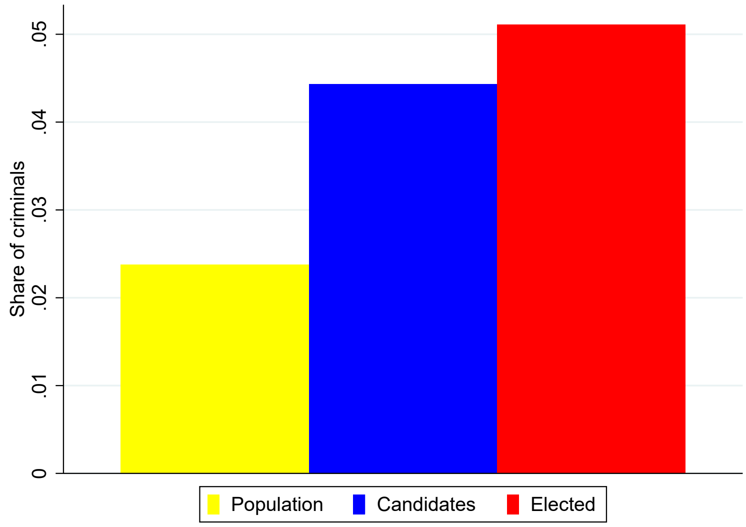
The high prevalence of individuals with previous criminal prosecutions among first-time politicians holds across genders, offices, political alignment, and types of crime – with the notable exception of environmental and violent crimes, which are equally prevalent among politicians and the general population, respectively. Appendix Figures A.1, A.2, A.3 and A.4 document these patterns, and how the over-representation of individuals with past prosecutions in politics varies across these dimensions.[15] The main conclusion we draw from this evidence is that our central finding in Figure 1 is not driven by a specific sub-population, nor by politicians being exposed to a higher risk of committing specific types of crime. For instance, there is a higher share of (past) public employees among politicians than in the general population, which could explain a higher rate of corruption charges among the former. However, we find that politicians are more likely to be charged for several other types of crime, such as driving-related offenses, which should be orthogonal to their career.

In Appendix Figure A.5, we document how first-time candidates and elected politicians with and without past criminal charges differ from the general population along several other characteristics. Men are substantially over-represented among candidates and elected politicians with past criminal charges – 75% and 91%, respectively, compared to 65% and 77% among candidates and elected politicians without past criminal charges. Differences exist also along other dimensions, although smaller in magnitude. In particular, first-time politicians with a criminal prosecution are more likely to be white, old, and more educated than the average individual in the population; they are also less likely to have a formal job, but more likely to have a job in the public sector.

[14]In our sample, 3.6% of defendants are part of criminal execution cases, meaning that they have been convicted. Excluding these individuals from the sample does not affect our main results (see Table A.6).

[15]For instance, the share of criminally charged politicians is higher for men than for women,

Figure 1: Incidence of individuals with prior criminal charges in the general population and among candidates and elected politicians



Note: This figure shows the share of individuals with prior criminal charges in the entire Brazilian population (yellow bar) and among candidate and elected individuals (blue and red bars). We consider candidates and elected individuals who only run for the first time since 2012 onward.

4.2 Other Dimensions of Political Selection and Heterogeneity

In this section, we investigate whether the over-representation of individuals with past criminal charges among candidates and elected politicians still holds when other dimensions of political selection are accounted for. To this purpose, we re-weight the 2% random sample of the population registry by a factor of 50 in order to recover the actual size of the reference population, and we estimate the following linear probability model:

$$Y_{it} = \alpha_1 + \beta\, Crime_{it} + \mathbf{X}_{it}^{'}\, \gamma + \eta_{mt} + \epsilon_{it} \qquad (1)$$

where Y_{it} is a dummy indicating either that individual i runs for the first time as a candidate for any office in electoral year t, or that the same individual won that election.[16] Our main explanatory variable, $Crime_{it}$, is a dummy for individual i having been prosecuted for at least one crime by the year before each election (following the same definition as in Section 4.1). The vector of covariates $\mathbf{X}_{it}$

and for politicians in centrist parties relative to either left- or right-wing parties.

[16] To ease the interpretation of estimated coefficients, we multiply the indicator variable Y_{it} by 100.

includes several individual-level characteristics, such as gender, race (white vs non-white), age (above or below 40 years old), educational level (college education vs lower levels), as well as dummies for formal and public sector jobs in the year of the election. Finally, η_{mt} are municipality-election year fixed effects, and standard errors are clustered at the same level.

The coefficient of main interest is β, which measures the excess probability that criminally prosecuted individuals run for and win elections, controlling for other observable characteristics. Clearly, we do not attribute a causal interpretation to this coefficient. Nevertheless, it is interesting to see that the higher incidence of criminals among candidates and elected individuals persists when we include in the regression a wide array of observable characteristics along with municipality-year fixed effects; see Table 1. At the bottom of each column, we also report the effects relative to the baseline rates in the general population for each outcome. Individuals with previous criminal charges are 84% more likely to be first-time candidates and 97% more likely to be elected (columns 3 and 6), in line with the simple descriptive evidence presented in Figure 1.

In Table A.2, we show that these patterns hold across the entire ideological spectrum; indeed, the relative effect over the baseline is very similar for politicians of left, center, and right-wing parties.[17] The relative effect is also very similar when comparing candidates and elected politicians for different positions and at different levels of government, with the notable exception of politicians elected as state legislators (+29%, compared to +72% to +182% for other politicians); see Table A.3.

In Table A.4, we distinguish between different types of crime. To this end, we replace the dummy $Crime_i$ with a full set of dummies for specific types of crimes on the right-hand side of Equation (1); to facilitate comparability between coefficients, in Figure A.6 we plot them as relative effects over the baseline for the general population. Previous charges for most categories of crimes are positive predictors of entering politics and winning elections; these categories include violent, threatening and driving-related offenses, but the relative increase is strongest for corruption, fraud, and environmental crimes. The fact that individuals charged for white-collar crimes are most likely to enter politics is consistent with the idea that individuals with rent-seeking motives may be more willing (and able) to enter politics. Instead, property crimes and illegal trafficking (of drugs, guns, and stolen goods) are negative predictors of entry into politics, perhaps because these

[17] We follow classification in Zucco and Power (2021) to group Brazilian parties into left, center, and right-wing.

Table 1: Prior criminal charges and probability of becoming a politician

	(1) Candidate	(2) Candidate	(3) Candidate	(4) Elected	(5) Elected	(6) Elected
Any crime	0.122***	0.114***	0.112***	0.011***	0.008***	0.009***
	(0.008)	(0.007)	(0.005)	(0.001)	(0.001)	(0.001)
Observations	9,951,591	9,383,949	9,383,949	9,951,591	9,383,949	9,383,949
R-squared	0.000	0.001	0.004	0.000	0.000	0.001
Controls	NO	YES	YES	NO	YES	YES
Mun. X Year FE	NO	NO	YES	NO	NO	YES
Baseline prob.	0.134%	0.134%	0.134%	0.009%	0.009%	0.009%
%Δ over baseline	+91%	+85%	+84%	+124%	+88%	+97%

Note: This table shows the relationship between prior criminal charges and the probability of entering politics at any level – municipal, state, and federal. In particular, the table shows the OLS-estimated coefficient of a regression of dummies for running for election for the first time (col. 1-3) or being elected (col. 4-6) from 2012 onward on a dummy for having been charged for any type of crime in the past (*Any crime*). For those never running for a public office we consider whether they have been charged for any crime by the year before each electoral round. To ease the interpretation of the estimated coefficients, the dependent variable is multiplied by 100. Columns 2-3 and 5-6 control for individuals' gender, skin color, age, education, and previous job (formal vs informal and Public vs no Public), while columns 3 and 6 also include municipality-year fixed effects. Standard errors are robust to heteroskedasticity and clustered at the municipality-year level. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. The last row of the table re-scales the estimated coefficient of *Any crime* by the baseline probability that a non-criminal individual within the Brazilian population either runs for the first time as a candidate or is elected for any type of office (also reported in the second to last row).

offenses are more salient in the media.

Finally, we examine how selection on past criminal charges varies across different groups by interacting the dummy $Crime_{it}$ in Equation (1) with a vector of individual characteristics $\mathbf{X}_{it}$. Figures A.7 and A.8 show the excess probability that individuals with previous criminal charges are candidate or elected, respectively, by gender, race, age, education, and employment. Criminal selection into politics is particularly strong for older individuals and for those with higher education and that held public jobs.

4.3 Robustness

In our main analysis, we consider candidates that are eventually acquitted or whose charges are dropped due to statutes of limitation as having a criminal background. This approach minimizes the risk of false negatives, but increases the risk of false positives. Although we believe that the former risk is more relevant for the specific case of political candidates (or individuals with high socio-economic status more generally), we assess the robustness of the results to using an alternative measure based on first-degree sentence data available for the state of São Paulo.

Using these data, we no longer define individuals that are subsequently acquitted or whose charges are dropped due to statutes of limitation as having a criminal background. The results of this analysis are presented in Table A.5. In columns 1 and 3, we first show that our main results for "criminally charged" candidates (i.e., the baseline definition of $Crime_{it} = 1$) hold within the state of São Paulo.[18] In columns 2 and 4, we show that the same conclusions hold when excluding acquitted defendants from the analysis; in fact, the effect size becomes even larger. In Table A.6 we present some additional results. First, we estimate Equation (1) considering as "criminals" only individuals that were sentenced to jail, rather than all individuals that had been prosecuted. As discussed in Section 2.2, the *"Ficha Limpia"* law bans from politics individuals convicted for several types of crime. It does not come as a surprise, then, that previous convictions are associated with a strong reduction in the probability of running for election and being elected (columns 1 and 2).[19] Conversely, when considering as criminals individuals who have been prosecuted but not convicted, the results are similar to our baseline specification (columns 3 and 4). Therefore, our main findings are robust alternative definitions of candidates' criminal background.

4.4 The effect of anti-corruption audits

A natural explanation for our results, which is consistent with previous models such as Caselli and Morelli (2004), is that individuals with a higher propensity to illicit behavior may be attracted to politics by the rent-seeking opportunities. To provide suggestive evidence in this direction, we examine the impact of a negative shock to rent-seeking opportunities, which should reduce the proportion of politicians facing criminal charges. Specifically, we investigate the effects of a policy designed to mitigate corruption, which has the potential to increase the perceived level of monitoring (Chalfin and McCrary, 2017). By doing so, we also gain insights into the effectiveness of anti-corruption policies in preventing the entry of criminally charged individuals into politics.

In 2003, the Brazilian national government entrusted the federal agency *Controladoria Geral da União* (CGU) with a major audit program to tackle corruption in local governments. Municipalities were randomly selected to be audited by lotteries held at the *Caixa Econômica Federal* in Brasilia, in the presence of media

[18]The smaller magnitude of the effects may be related the characteristics of São Paulo, which is the largest and richest Brazilian state.

[19]Since this law applies for specific types of crime, we do not expect that perfect enforcement would lead to an exclusion of all individuals in our data from participating in elections.

and members of civil society. All municipalities with less than 500,000 inhabitants were eligible for the audits. Since the lotteries were run independently for each state, the probability of being selected for an audit in a given year varied by state. Typically, 10 to 15 auditors spend around two weeks in the municipalities selected by the lottery, looking for irregularities and malpractices that are then reported to the enforcement authorities and are also made publicly available on the CGU website. Previous studies documented the fairness of the auditing process, the relevance of the results for local accountability and corruption levels, and the media coverage of the results (Ferraz and Finan, 2008; Avis et al., 2018; Cavalcanti et al., 2018).

To analyze the effects of audits on the entry of individuals with a criminal background into politics, we exploit the randomization of audits across municipalities and their timing relative to municipal electoral cycles. The analysis is conducted at the municipal level and combines three municipal election rounds in the period 2012-2020. We regress the share of criminally prosecuted individuals among candidates and elected politicians in each electoral round on a set of dummies for whether the municipality received an audit and, in case, the timing of the latter relative to the election. Specifically, we estimate the following equation:

$$Y_{it} = \alpha + \beta_0 Years^0_{it} + \beta_1 Years^1_{it} + \beta_2 Years^{2+}_{it} + \eta_{st} + \epsilon_i. \quad (2)$$

The variable Y_{it} indicates the share of criminally prosecuted individuals among first-time candidates or elected individuals (councillors and mayors) in municipality i and electoral year t.[20] The dummy $Years^0_{it}$ equals 1 when municipality i was audited up to July of the election year when parties submit their lists of candidates (Cavalcanti et al., 2018). The other two dummy variables, $Years^1_{it}$ and $Years^{2+}_{it}$ are equal to 1 when municipality i was audited 1 year and 2+ years before the elections, respectively, and municipalities that never received an audit by electoral year t serve as the reference category. η_{st} are state-year fixed effects and the standard errors are clustered at the municipal level.

The estimated coefficients β's have a causal interpretation because, conditional on state fixed effects, municipalities are randomly selected for the auditing process and, therefore, the time distance between audits and elections is also random. This is confirmed in Table A.7, which shows that in year 2000 (that is, before the first

[20] To facilitate the interpretation of the results, we multiply all outcomes by 100. For elected mayors, the outcome is not a share but a dummy equal to 100 when the elected mayor has been prosecuted in the past, and 0 otherwise.

audits were carried out), municipalities later receiving at least one audit are very similar to those that never received an audit.[21]

Table 2 presents the estimates for the main coefficients of Equation (2). Columns (1) and (2) show that audits do not have a statistically significant impact on the probability that individuals with a criminal history enter politics as councilors. Regarding mayors, columns (3) and (4) show that audits taking place right before the election decrease the share of criminally charged candidates and elected mayors, while audits taking place in previous years have no significant effect. Therefore, audits seem to discourage individuals with past criminal charges from running for political offices that are most likely to be monitored (i.e., mayors), though this effect is short-lived. These results are in line with Avis et al. (2018), who argue that increased perception of legal punishment reduces corruption, and with Gonzalez-Navarro et al. (2023), who find a similar short-term effect on clientelism.[22]

Table 2: Anti-Corruption Audits and entry of individuals with prior criminal charges into politics

	(1) Candidates	(2) Elected	(3) Candidates	(4) Elected
Anti-corruption audit, electoral year	0.465	0.328	-1.634***	-0.762**
	(0.516)	(1.766)	(0.437)	(0.296)
Anti-corruption audit, 1 year before elections	0.334	0.048	0.651	1.598
	(0.265)	(0.774)	(1.093)	(1.149)
Anti-corruption audit, 2+ years before elections	0.072	0.159	-0.125	-0.023
	(0.088)	(0.245)	(0.267)	(0.154)
Observations	16,703	16,703	16,231	16,231
R-squared	0.427	0.080	0.026	0.008
Office	Counc.	Counc.	Mayor	Mayor
State X Year FE	YES	YES	YES	YES
Baseline share (Never audited)	3.88%	4.41%	2.9%	0.79%

Note: This table shows the effect of anti-corruption audit conducted in election years and in the two preceding years on the probability that individuals with a prior criminal record running for election (odd columns) or being elected (even columns) as municipal councilors (col. 1 and 2) or as mayor (col. 3 and 4) in all electoral rounds between 2012 and 2020. To ease the interpretation of coefficients, the dependent variable is multiplied by 100. The reference category includes municipalities that have never received an audit in the electoral round considered. All specifications control for state-year fixed effects. Standard errors are robust to heteroskedasticity and clustered at the municipality level. The last row of the table reports the share of individuals with prior criminal records among first-runner candidates and elected individuals in municipalities that are never audited. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

[21] All the standardized differences are below the threshold of 0.25 recommended by Imbens and Wooldridge (2009).

[22] We found little heterogeneity in the effect of audits between municipalities in which higher or lower levels of corruption were revealed by the audit.

5 The effects of electing politicians with prior criminal charge

Next, we analyze the effects of electing mayors with past criminal charges on local socioeconomic outcomes that depend on the activity of municipal governments – notably basic health and schooling. To better understand any effect on such outcomes, we will also look at the effects on local public expenditure allocation and on patronage in the allocation of public jobs.

5.1 Policy Outcomes

Basic health and schooling are the main public services provided by Brazilian local governments, representing 24% and 31% of municipal expenditures, respectively.[23] In addition, health and municipal expenditures are the first and third most cited budget items in the federal random audits reports (Ash et al., forthcoming). We measure the quality of the public health system by (i) the share of children born underweight and (ii) mortality of children under five years of age. Turning to schooling, we measure the quality of local public schools by pupils' performance in standardized test scores.

To estimate the impact of electing politicians with a criminal background on these outcomes, we compare municipalities in which criminally charged politicians won and lost by a narrow margin, respectively. We thus estimate the following equation:

$$Y_{ie} = \alpha + \beta Criminal_{ie} + \delta MV_{ie} + \gamma(Criminal_{ie} \cdot MV_{ie}) + FE_s + FE_e + \epsilon_{ie}, \quad (3)$$

where Y_{ie} is an outcome for municipality i in the four-year political term after election e. $Criminal_{ie}$ is a dummy indicating that the politician with prior criminal charges won the election, while MV_{ie} is the margin of victory ($MV_{ie} < 0$ for candidates that lost the elections); in particular, we consider the margin of victory of the winner against the most voted loser. The specification also controls for state (FE_s) and electoral year (FE_e) fixed effects. We estimate Equation 3 using a local linear regression with an optimal bandwidth defined according to the criterion of Calonico et al. (2014) and a triangular kernel.

Unlike the analysis in the previous section, in which we restricted the sample to first-time candidates in order to minimize biases from the differential reporting

[23]Statistics based on municipal expenditure data (FINBRA) for the period 2013-2019.

of crimes committed by politicians and non-politicians, there is no reason to apply the same restriction when comparing candidates than won or lost the elections. We thus extend the definition to include in the analysis all politicians who have been prosecuted by the year before each election.[24]

Appendix Figure A.9 shows no discontinuity in the number of observations around the cutoff, supporting the main identifying assumption that, within a narrow bandwidth of the cutoff, the outcome of the election is as good as random. Also in line with this assumption, a wide array of municipal characteristics in the 2000 and 2010 Population Censuses and electoral data are balanced around the cutoff; see Appendix Table A.8. In addition, the same table shows that individual candidates' characteristics are also balanced at the cutoff, meaning that prosecuted politicians barely winning the elections are not dissimilar from other candidates barely losing the elections.

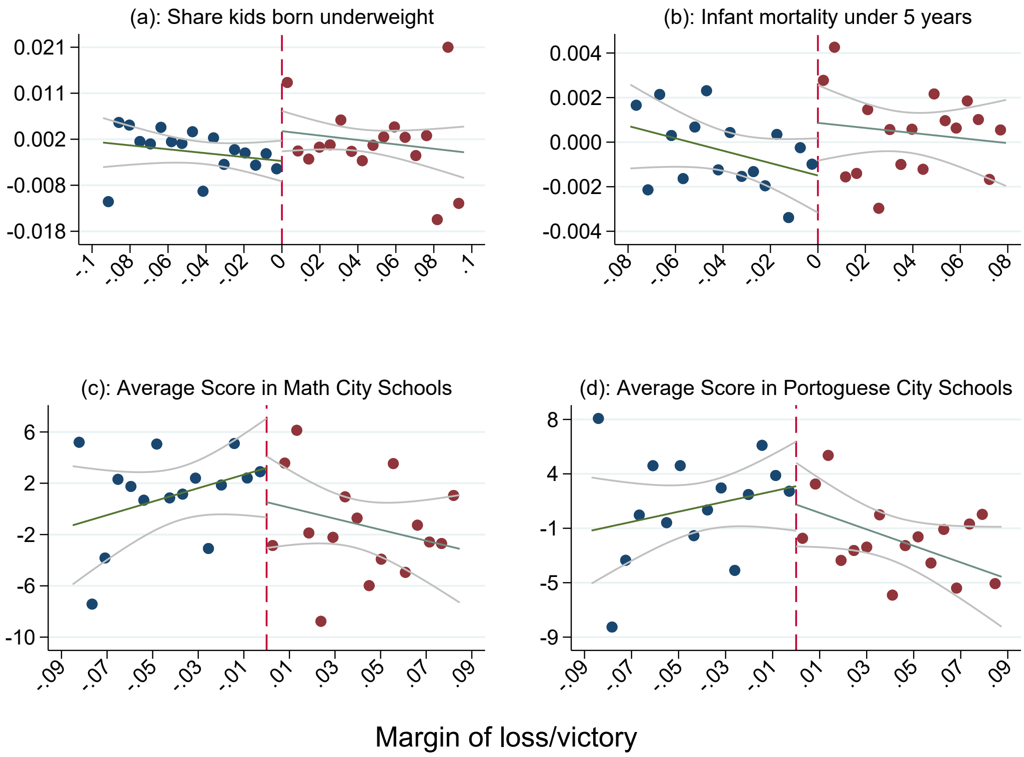
Figure 2 and Table 3 turn to the main outcomes of interest, namely basic health and schooling. Figure 2 suggests a worsening of infant health when criminally prosecuted politicians (barely) win elections, while we do not detect any significant effect on schooling-related outcomes. Estimates of Equation (3), reported in Table 3, confirm that the estimated increases in the share of babies born underweight (+8% compared to the mean) and mortality rates (+21%) are statistically significant. These estimates are robust to the use of a different degree of polynomial specification for the running variable, different kernels, and different definitions of mortality rates (see Appendix Table A.10 and A.9). Placebo estimates for the same outcomes in the year before elections, presented in Appendix Table A.11, confirm that the effect appears only after the election of a prosecuted mayor. Finally, Appendix Figure A.10 compares the estimated effects on health outcomes at the true RD cutoff with the distribution of estimates obtained at 10,000 placebo cutoffs. The results show that the coefficients estimated at the true electoral cutoffs are abnormal compared to the distribution of placebo cutoffs.

To test whether these effects on health are driven by a different allocation of public spending, we replicate the same analysis using as a dependent variable the allocation of municipal budget across different spending areas.[25] Public spending on health and education declines when mayors with prior criminal charges win the elections, but the effect is not significantly different from zero. More generally, we do not find systematic differences in spending patterns between municipalities

[24]We exclude municipalities with a population greater than 200,000 inhabitants by the year before each election, because these municipalities follow a two-round rather the one-round electoral system.

[25]A complete list of all expenditure items is provided in Appendix Section A.2.

Figure 2: The effects of electing mayors with prior criminal charges on basic health and education, RD plots



Note: This figure shows the effect of electing a mayor with prior criminal charges on basic health and education in the municipality, as estimated from the RD design in Equation 3. Basic health is measured by the share of babies born underweight (panel a) and infant mortality under 5 years of age (panel b), while schooling is measured by average math and literacy scores (panels c and d, respectively). The data refer to the 2012 and 2016 elections, and all outcomes are averaged over the four-year period after elections. The running variable is the margin of victory between the winner and the most-voted loser in each election. Averages of the outcome variable within equally-spaced bins are plotted along with linear regressions on each side of the cutoff and with 95% confidence intervals.

Table 3: The effects of electing mayors with prior criminal charges on basic health and education, RD estimates

	(1) Babies under.	(2) Mortality 5Y	(3) Math score	(4) Literacy score
Criminal	0.007**	0.003**	-3.417	-3.107
	(0.003)	(0.001)	(2.790)	(2.554)
Observations	1,094	1,094	1,007	1,007
Bandwidth	.1	.08	.09	.09
Base Value	.081	.014	217.934	204.283
Robust P-val.	.031	.018	.343	.3

Note: This figure shows the effect of electing a mayor with prior criminal charges on basic health and education, as estimated from the RD design in Equation 3. Basic health is measured by the share of babies born underweight (col. 1) and infant mortality under 5 years of age (col. 2), while schooling is measured by average math and literacy scores (cols. 3 and 4, respectively). The data refer to the 2012 and 2016 electoral rounds, and all outcomes are averaged over the four-year period after elections. The estimation sample is restricted to municipalities in which one candidate between the winner and the most voted loser has prior criminal charges while the other one has a clean criminal record. The main explanatory variable, *Criminal* is a dummy equal to 1 if the candidate with prior criminal charges is elected as mayor, and 0 otherwise. The running variable is the margin of victory between the winner and the most-voted loser in each election, and we include in the regression observations within a symmetric bandwidth around the cutoff, computed according to the criteria of Calonico et al. (2014, 2020). All regressions include a linear RD specification, fixed effects for Brazilian states and electoral rounds, and weight observations by a triangular kernel in the distance from the cutoff. The last two rows of each panel report the average value of the dependent variable in municipalities electing a non-criminal mayor within the estimation bandwidth, and the robust p-value for each estimate. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

in which criminally charged mayors won or lost the elections, respectively. Tables A.12 and A.13 show that out of 25 sector-specific coefficients, only one (spending on energy) is significant at the 10 percent level, and another one (spending on sport) is significant at the 5 percent level. Therefore, the negative effect on basic health does not seem to reflect a different allocation of public spending by mayors with prior criminal charges. For this reason, we next turn to investigate (one dimension of) the quality of public spending.

5.2 Political Patronage

Colonnelli et al. (2020) show that the mayors of Brazil allocate a substantial portion of public jobs to their political supporters. Building on their RD analysis estimating the public employment premium for individuals supporting candidates barely winning and losing mayoral elections, we examine how this premium varies when the elected mayor does and does not have a criminal charge, respectively. In this way, we shed light on whether criminally charged politicians are associated with higher levels of patronage and, more generally, rent extraction. In addition, this analysis may help rationalize the findings of the previous section. To the extent that mayors with prior criminal charges have a greater tendency to appoint political supporters rather than competent public workers and managers, the quality of basic health may worsen even in the absence of any marked decline in public expenditure on local health.

We measure patronage by the number of public jobs and the associated earnings that are allocated to the candidate's supporters. Specifically, we consider three alternative measures: the total number of jobs in the public sector; total number of months worked in the public sector; and total income earned in these jobs. Following Colonnelli et al. (2020), we focus on supporters who are city council candidates in any of the parties that make up the mayor's supporting coalition during the elections.

Our RD design is based on the following equation:

$$Y_{cit} = \alpha + \beta Winner_{cie} + \delta MV_{ie} + \gamma(Winner_{cie} \cdot MV_{cie}) + FE_{it} + \epsilon_{cit}\,, \qquad (4)$$

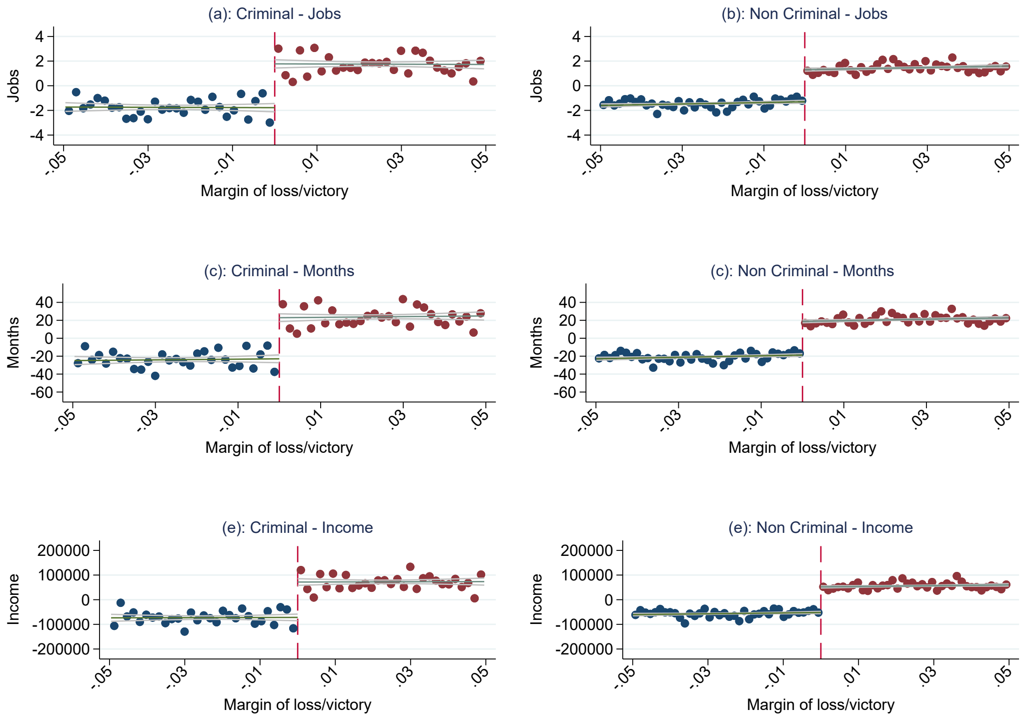
where Y_{cit} is an outcome for (the supporters of) candidate c (i.e., the winner or the most voted loser) in municipality i in the year t; $Winner_{cie}$ is an indicator variable equal to 1 if candidate c won the election e in municipality i, and MV_{ie} is the margin of victory relative to the most voted loser; and FE_{it} are fixed effects by municipality-year. This analysis differs from the RD design in Section 5.1, in that outcomes are defined at the candidate level rather than at the municipal level, so the RD equation (4) leverages variation within (rather than between) municipalities.[26] Following Colonnelli et al. (2020), we restrict the comparison to municipalities where the margin of victory between the winner and the loser does not exceed 5%.

We estimate the model in Equation (4) separately in two different samples: municipalities electing a criminally charged mayor and the remaining ones. We provide evidence of the validity of the design in each of the two samples. Figure A.11 shows that the density of observations is continuous at the cutoff, and Table A.14 shows that other characteristics are also balanced at the cutoff between winners and losers in the sample of municipality-elections in which a criminally prosecuted mayor won the election. In the sample of municipality-elections in which mayors without past criminal charges won the elections, formal employment and employment in the public sector are not balanced; however, both differences remain very small in magnitude (below half percentage point).

Figure 3 and Table 4 shows how patronage varies between municipalities where criminally charged mayors win or lose elections, respectively. In line with Colonnelli et al. (2020), employment opportunities in the public sector improve considerably for the political supporters of marginal winners, relative to the political supporters of marginal losers. Interestingly, these benefits are larger for supporters of winners with prior criminal charges. On average, political supporters in this group gain 3.3 additional public jobs each year relative to only 2.3 jobs for marginal winners without a criminal charge (Table 4, columns 1-2). In addition, each year they gain R$137k in additional public labor income and work 43 additional months, while supporters of a mayor without a criminal charge only gain R$96k and work 34 additional months (Table 4, columns 1-2). This evidence suggests that criminally charged politicians engage more strongly in patronage practices compared to other politicians.

[26] As in equation (3), our main specification includes only a first-order polynomial in MV_{ie}, but results obtained using a second-order polynomial are very similar and are available upon request. We weight observations according to a triangular kernel and use standard errors robust to heteroskedasticity.

Figure 3: Political patronage under mayors with and without prior criminal charges



Note: This figure shows the extent of political patronage after electing mayors with prior criminal charges (panel a, c, and e) and mayors with clean criminal records (panel b, d, and f) in the electoral rounds of 2012 and 2016. Patronage is measured by the excess number of public jobs filled by individuals belonging to the same political party as the elected mayor (panel a and b), the total amount of months they work (panel c and d), and the income they received in such positions (panel e and f). The excess number of jobs, hours worked, and income is estimated, in turn, by the discontinuity (if any) at the RD cutoff for winning the elections, where the running variable is the margin of victory between the winner and the most-voted loser in each election; see Equation (4). Averages of the outcome variable within equally-spaced bins are plotted along with linear regressions on each side of the cutoff and with 95% confidence intervals.

Table 4: Political patronage under mayors with and without prior criminal charges

	(1) Jobs	(2) Jobs	(3) Months	(4) Months	(5) Income	(6) Income
Winner	3.343***	2.328***	42.725***	34.114***	137,090.337***	95,687.835***
	(0.365)	(0.121)	(4.425)	(1.553)	(14,307.972)	(4,122.190)
Observations	1,944	14,586	1,944	14,586	1,944	14,586
Specification	Criminal	Non Criminal	Criminal	Non Criminal	Criminal	Non Criminal
Pol. degree	1st	1st	1st	1st	1st	1st
Bandwidth	0.05	0.05	0.05	0.05	0.05	0.05
Mun X Elec. FE	YES	YES	YES	YES	YES	YES
Crim. - Non Crim.	1.015***		8.610*		41,402.502**	
	(0.385)		(4.687)		(14,879.502)	

Note: This table shows the extent of political patronage after electing mayors with prior criminal charges (odd columns) and mayors with clean criminal records (even columns) in the electoral rounds of 2012 and 2016. Patronage is measured by the excess number of public jobs filled by individuals belonging to the same political party as the elected mayor (cols. 1-2), the total amount of months they work (cols. 3-4), and the income they received in such positions (cols. 5-6). The excess number of jobs, hours worked, and income is estimated, in turn, by the discontinuity (if any) at the RD cutoff for winning the elections, where the running variable is the margin of victory between the winner and the most-voted loser in each election; see Equation (4). The sample is restricted to observations within a symmetric bandwidth of 5 percentage points around the cutoff. All columns include a linear RD specification, as well as fixed effects for municipality-election, and weight observations by a triangular kernel in distance from the cutoff. The last two rows of the table report the estimated difference (along with its standard error, in parentheses) between the estimated RD coefficient in municipalities in which mayors with prior criminal charges won and lost the elections, respectively. Standard errors are robust to heteroskedasticity. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

Our heterogeneity analysis compares the effects of winning mayoral elections on patronage in two samples – namely, municipalities where politicians with a criminal charge win vs. other municipalities. Appendix Table A.15 shows that the two samples are fairly similar across a wide range of characteristics; standardized differences are generally around or below the threshold of 0.20, indicating that any differences in the underlying distributions are small (Cohen, 2013). Moreover, municipality-year fixed effects should account for any residual difference between the two groups.

We bolster this evidence by estimating the impact of electing criminal mayors on within-municipality changes in patronage. In particular, we compare the effect of winning the 2016 mayoral elections on patronage when the winner has or does not have a criminal record, relative to the same differential effect in the same municipality in 2012. For this analysis, we restrict the sample to (i) municipalities where politicians with a criminal charge win in 2016 but not in 2012 and (ii) municipalities where politicians with a criminal charge win neither in 2016 nor in 2012. We then add to the right hand side of Equation 4 the dummy $Criminal_{cie}$ equal to 1 when candidates with a criminal background win the election; a dummy $Post_e$ for the 2016 elections ($Post = 1$ in 2016 and $= 0$ in 2012);

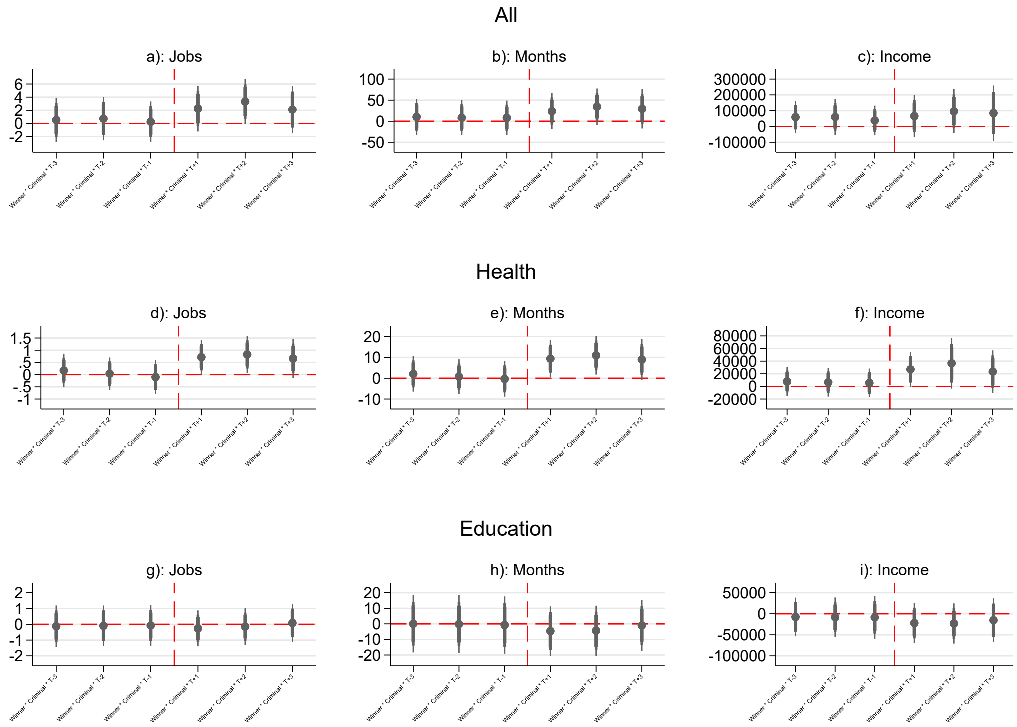
and all possible interactions among all right-hand side variables ($Winner$, MV, $Criminal$, $Post$). We are interested in the coefficient of the triple interaction $Winner_{cie} \times Criminal_{cie} \times Post_e$, which captures the *change* in the extent of patronage after 2016 in municipalities where a mayor with prior criminal charges (barely) wins the 2016 elections relative to the change during the same period in municipalities where a candidate without criminal charges wins the elections. The estimate of such coefficient is presented in Table 5. Column 1-3 present the estimates pooling together all sectors of the public administration. The results align well with our previous findings showing higher impacts on patronage when politicians with a criminal background are elected, although the effect on income (while virtually identical in magnitude) is less precisely estimated.

Interestingly, the heterogeneity in the effects on patronage between the health sector and education (columns 7-9) mirrors the heterogeneity in the effect on policy outcomes. In particular, the effect of electing mayors with prior criminal charges on patronage is positive and statistically significant in the health sector (columns 4-6 of Table 5), which is also where we observe the most negative policy effects (in terms of infant mortality and share of children born underweight, see Panels a-b of Figure 2). By contrast, when focusing on education, the other main area of public policy under municipal control, there is no effect on either patronage (columns 4-6 of Table 5) or policy outcomes (Panels c-d of Figure 2).

Figure 4 shows the dynamics of the effect on patronage in the years around the 2016 election. In particular, we plot the coefficients (and associated confidence intervals) of year-specific dummy variables replacing the dummy $Post_t$ in the interaction specification described above. The graphs confirm that patronage increases after a mayor with prior criminal charges wins the 2016 elections relative to other municipalities (panels a-c), and that such effect is driven by patronage in the health sector (panels d-f). In addition, the graphs in Figure 4 also validate the assumption that differences in patronage emerge only after the election of mayors with prior criminal charges, while there is no difference in the years before elections.

Overall, the results on patronage presented in Tables 4-5 and in Figures 3-4 along with the evidence on policy effects in Figure 2 are consistent with the idea that political patronage may be an important mechanism driving the negative effects of mayors with a criminal background on public health. Indeed, Colonnelli et al. (2020) show that patronage leads to the selection of less competent officials. Furthermore, patronage may be correlated with other types of political malpractice while in office.

Figure 4: Political patronage under mayors with and without prior criminal charges, triple interaction estimates



Note: This figure shows the differential effect of appointing mayors with prior criminal charges compared to mayor with clean criminal records on political patronage after the 2016 elections. Top panels refer to patronage in all sectors of the public administration, while middle and bottom panels refer to patronage in public health and education. Patronage is measured by the excess number of public jobs filled by individuals belonging to the same political party as the elected mayor (left panels), the total amount of months they work (center panels), and the income they received in such positions (right panels). The excess number of jobs, hours worked, and income is estimated, in turn, by the discontinuity (if any) at the RD cutoff for winning the elections, where the running variable is the margin of victory between the winner and the most-voted loser in each election. The graphs plot time-specific effects and confidence intervals for the three years before and after the 2016 election. The sample is restricted to observations within a symmetric bandwidth of 5 percentage points around the cutoff. All regressions include a linear RD specification and fixed effects for municipality-year pairs, and weight observations by a triangular kernel in distance from the cutoff.

Table 5: Political patronage under mayors with and without prior criminal charges, triple interaction estimates

	(1) Jobs	(2) Months	(3) Income	(4) Jobs	(5) Months	(6) Income	(7) Jobs	(8) Months	(9) Income
Winner X Criminal X Post	2.208***	22.511**	42,767.700	0.709***	9.201***	24,291.112***	-0.040	-3.205	-14,169.001
	(0.798)	(9.644)	(33,525.443)	(0.163)	(1.969)	(7,343.648)	(0.229)	(3.206)	(9,599.180)
Observations	15,416	15,416	15,416	15,416	15,416	15,416	15,416	15,416	15,416
R-squared	0.677	0.696	0.708	0.571	0.575	0.601	0.659	0.654	0.668
Patronage	All	All	All	Health	Health	Health	Educ.	Educ.	Educ.

Note: This table shows the differential effect of appointing mayors with prior criminal charges compared to mayor with clean criminal records on political patronage after the 2016 elections. Patronage is measured by the excess number of public jobs filled by individuals belonging to the same political party as the elected mayor (col. 1), the total amount of months they work (col. 2), the income they received in such positions (col. 3), the same three variables but only related to the health sector (col. 4-6), and only to the educational sector (col. 7-9). The excess number of jobs, hours worked, and income is estimated, in turn, by any discontinuous change in such variables at the RD cutoff for winning the elections, where the running variable is the margin of victory between the winner and the most-voted loser in each election. *Criminal* is a dummy equal to 1 if a candidate with prior criminal charges won the election in 2016 while a candidate with a clean record won the election in 2012 electoral round, and 0 otherwise. *Post* is a dummy equal to 1 for years between 2017 and 2019, and 0 for those between 2013-2016. The sample is restricted to observations within a symmetric bandwidth of 5 percentage points around the cutoff. All columns include a linear RD specification, as well as fixed effects for municipality-year pairs, and weight observations by a triangular kernel in distance from the cutoff. Standard errors are robust to heteroskedasticity. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

6 Conclusions

We investigate the prevalence and effects of individuals with prior criminal charges in Brazilian politics. Combining nationwide data on judicial prosecutions and other rich individual-level information, we document that individuals with a criminal history are over-represented in the political arena, particularly among first-time candidates and elected politicians. Although purely correlational, this evidence is concerning, as the selection of the political class is of utmost importance for the performance of democracies. In fact, these concerns are confirmed when looking at the (causal) effect of electing mayors with prior criminal charges on local public policies. Leveraging the RD design around the cutoff for winnign the elections, we find that electing a mayor with prior criminal charges worsens public health at the municipal level, as measured by the infant mortality and share of children born underweight. This negative effect seems to reflect (among other potential mechanisms) a greater tendency of mayors with prior prosecutions to engage in political patronage, whereby they hire their political supporters in the public sector, and particularly in the health sector.

These results call for stricter background checks and screening processes for political candidates. Enhanced transparency and voter education initiatives could empower citizens to make more informed choices, thereby limiting the electoral success of individuals with criminal backgrounds. Also, anti-corruption audits seem to deter the entry of such individuals, but the effect is very short-lived.

Reforms in electoral laws may ultimately be needed to simply ban individuals with prior criminal charges from entering or remaining in political office. For example, expanding the "Ficha Limpa" law to include a wider array of criminal offenses could mitigate the negative impacts observed in this study. Further, public policies focused on depoliticizing the allocation of public sector jobs, especially in critical areas like health and education, could attenuate the patronage practices that seem to be an important mechanism driving the negative effects on local public health. Strengthening institutional checks and balances would ensure that political appointments are based on merit rather than loyalty, ultimately improving the quality of public service delivery.

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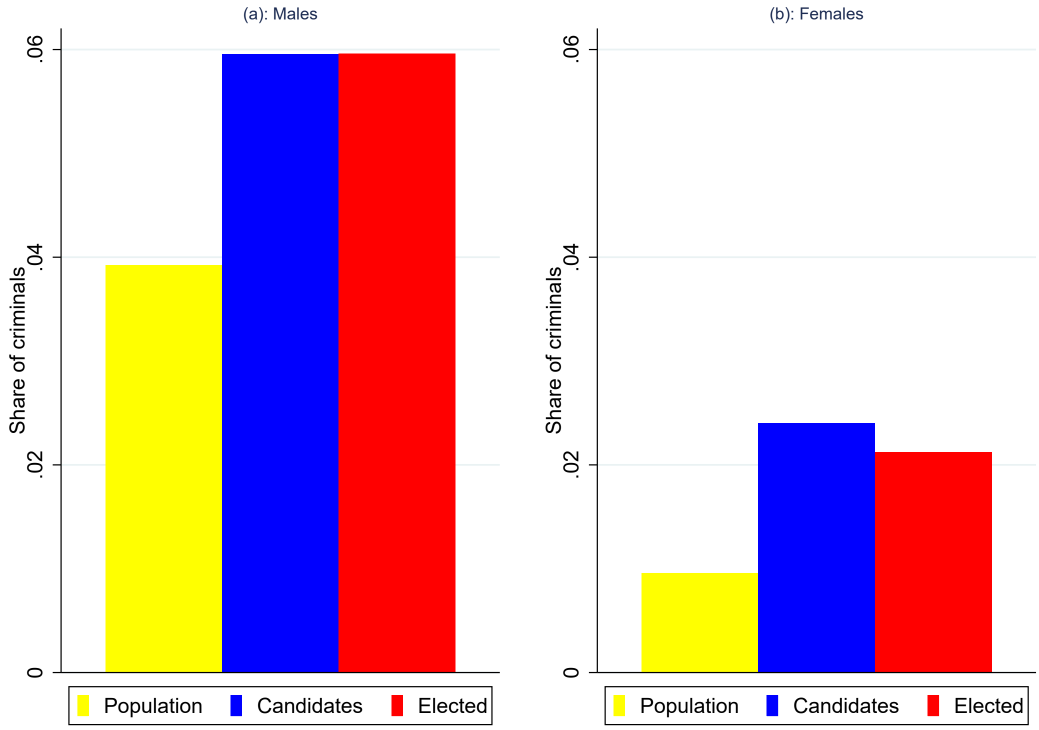
A.1 Appendix: Additional tables and figures

Figure A.1: Incidence of individuals with prior criminal charges in the general population and among candidates and elected politicians, by gender



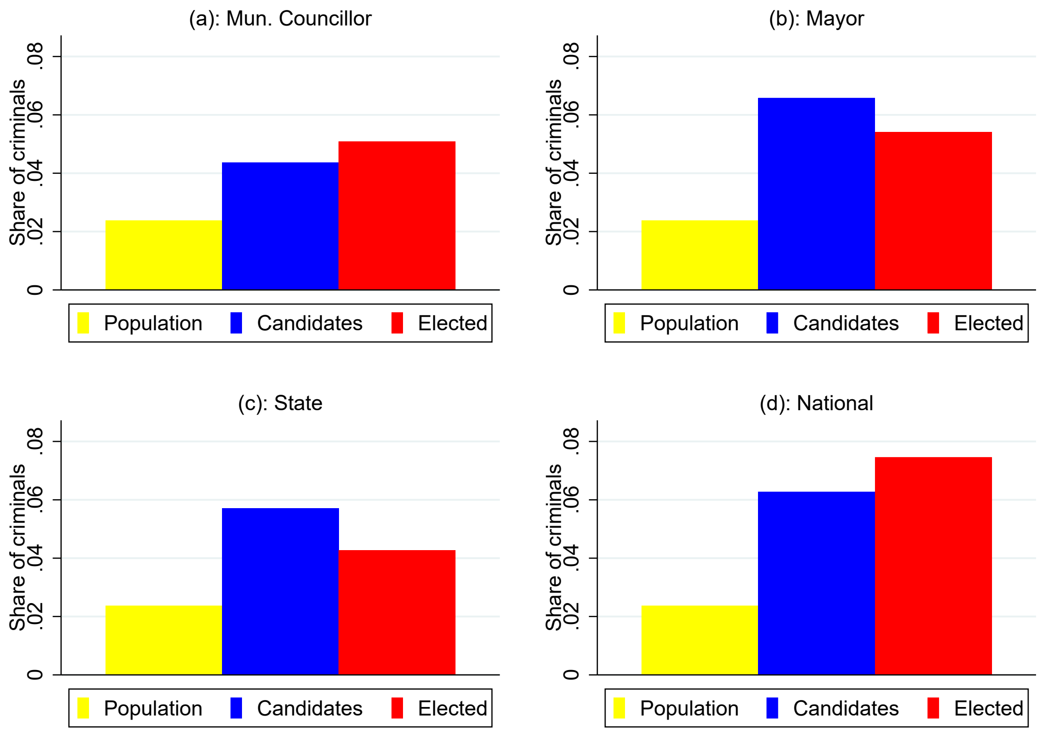
Note: This figure shows the share of individuals with prior criminal charges in the entire Brazilian population (yellow bar) and among candidate and elected individuals (blue and red bars), by gender. We consider candidates and elected individuals who only run for the first time since 2012 onward.

Table A.1: Summary Statistics

	(1) Mean	(2) SD	(3) Min.	(4) Max.
Panel (a): Criminals in politics				
Candidates (%)	6.436	24.540	0	100
Elected (%)	0.446	6.661	0	100
Criminal Candidates (Counc., %)	3.954	6.127	0	60
Criminal Elected (Counc.,%)	4.520	14.817	0	100
Criminal Candidates (Mayor, %)	2.937	15.698	0	100
Criminal Elected (Mayor, %)	0.807	8.948	0	100
Panel (b): Social Outcomes				
Babies underweight (%)	0.079	0.021	0	0.255
Mortality 5Y (%)	0.016	0.008	0	0.101
Math score	214.169	24.450	146.310	303.365
Portuguese score	200.397	22.700	138.650	275.415
Mortality 3M (%)	0.011	0.007	0	0.070
Mortality 6M (%)	0.012	0.007	0	0.070
Mortality 1Y (%)	0.014	0.008	0	0.083
Panel (c): Expenditures				
Total exp. (per capita, 1,000s reales)	50145	75339	0	1208558
Administration (per capita, 1,000s reales)	6958	11330	0	191778
Agriculture (per capita, 1,000s reales)	568	908	0	27722
Social Assistance (per capita, 1,000s reales)	1625	2301	0	54093
Sport (per capita, 1,000s reales)	380	933	0	25057
Education (per capita, 1,000s reales)	15645	21279	0	381905
Special (per capita, 1,000s reales)	1137	3023	0	75171
Legislative (per capita, 1,000s reales)	1517	3867	0	229950
Sanitation (per capita, 1,000s reales)	1065	12774	0	978538
Health (per capita, 1,000s reales)	11857	18645	0	279948
Transportation (per capita, 1,000s reales)	950	2196	0	74068
Urbanization (per capita, 1,000s reales)	3813	7504	0	154781
Culture (per capita, 1,000s reales)(per capita, 1,000s reales)	470	846	0	14088
Commerce (per capita, 1,000s reales)	163	790	0	31173
Energy (per capita, 1,000s reales)	165	695	0	26421
Environment (per capita, 1,000s reales)	369	1578	0	58549
Judiciary (per capita, 1,000s reales)	121	1024	0	44194
Social Security (per capita, 1,000s reales)	1675	4763	0	110316
Public Security (per capita, 1,000s reales)	256	1418	0	31354
Justice (per capita, 1,000s reales)	46	438	0	20445
Communication (per capita, 1,000s reales)	16	121	0	4184
Housing (per capita, 1,000s reales)	103	1376	0	102586
Industry (per capita, 1,000s reales)	47	244	0	9244
Labor (per capita, 1,000s reales)	71	493	0	18012
Citizens' rights (per capita, 1,000s reales)	28	387	0	23069
Panel (d): Political Patronage				
Number of Jobs	4.426	5.162	0	93
Number of Working Months	58.208	67.258	0	1125.267
Income (per 1,000s reales)	129	208	0	5306

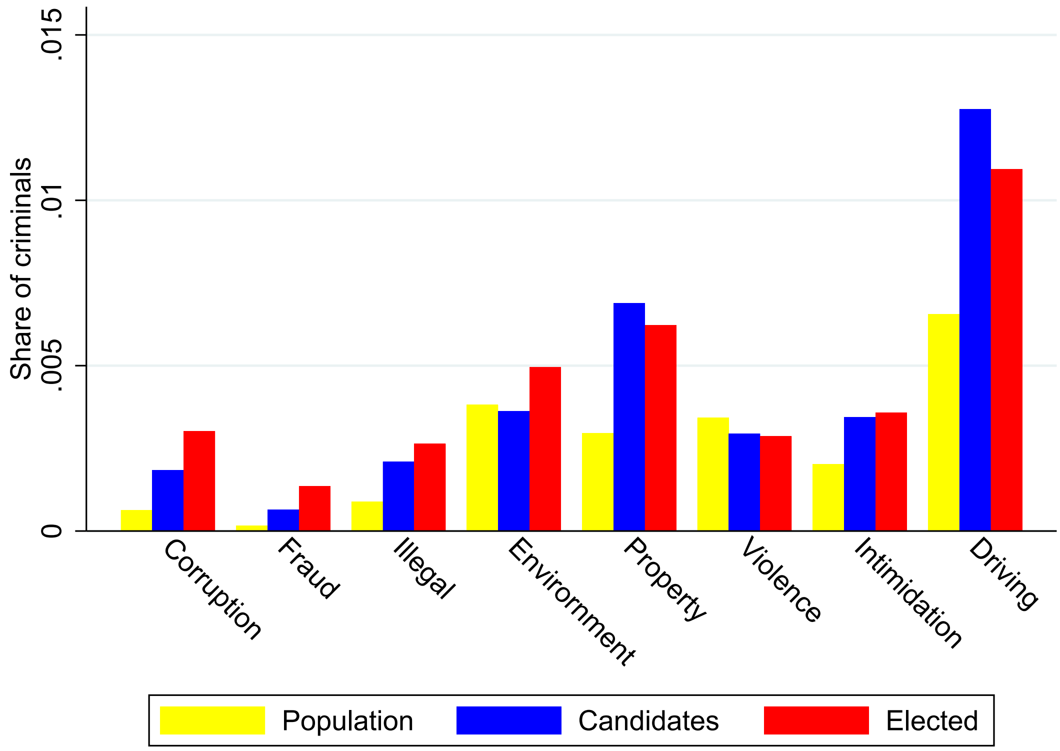
Note: The table reports summary statistics for all the dependent variables employed throughout the empirical analysis.

Figure A.2: Incidence of individuals with prior criminal charges in the general population and among candidates and elected politicians, by type of political office



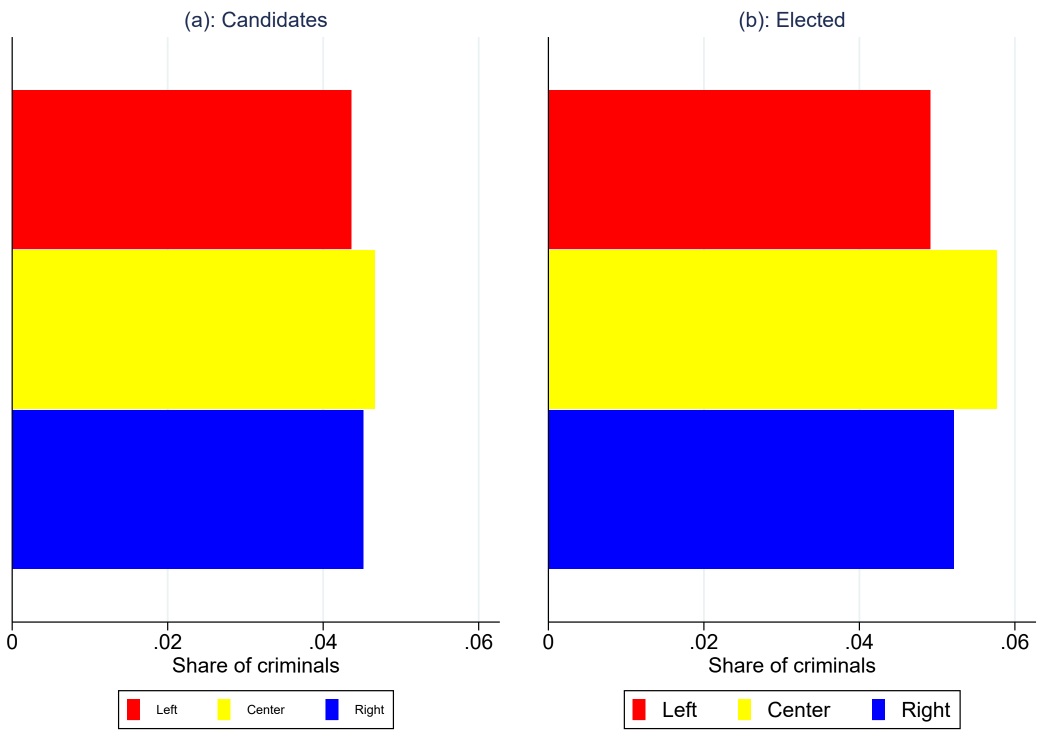
Note: This figure shows the share of individuals with prior criminal charges in the entire Brazilian population (yellow bar) and among candidate and elected individuals (blue and red bars), by type of political office. We consider candidates and elected individuals who only run for the first time since 2012 onward.

Figure A.3: Incidence of individuals with prior criminal charges in the general population and among candidates and elected politicians, by type of crime



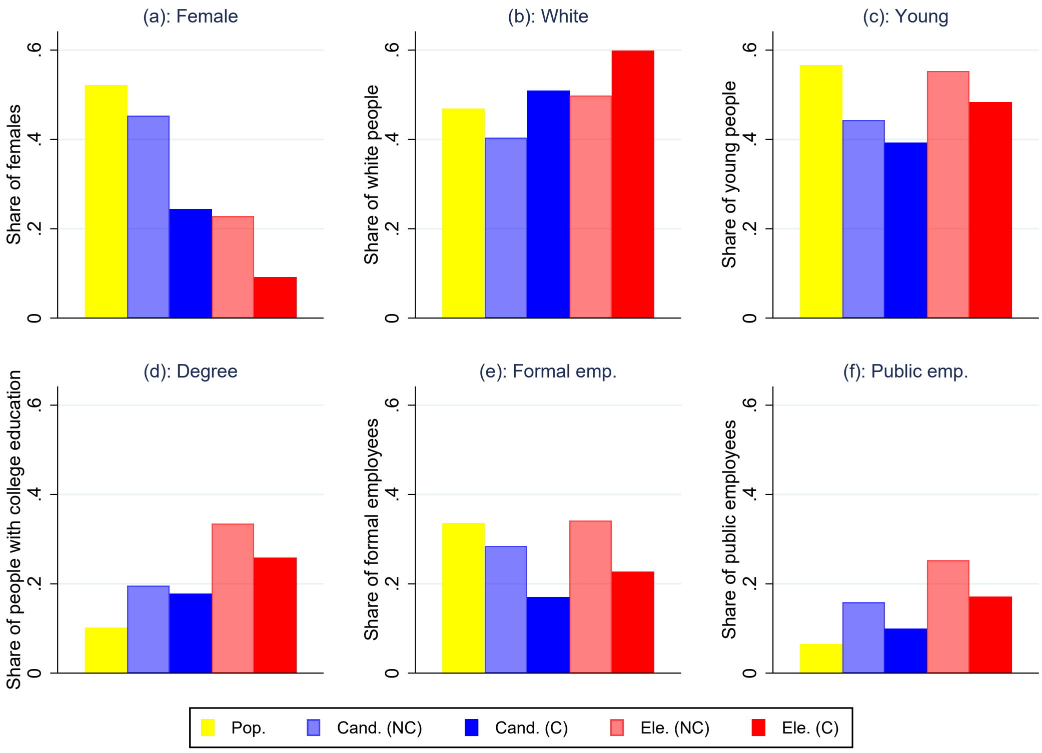
Note: This figure shows the share of individuals with prior criminal charges in the entire Brazilian population (yellow bar) and among candidate and elected individuals (blue and red bars), by type of crime. We consider candidates and elected individuals who only run for the first time since 2012 onward.

Figure A.4: Incidence of individuals with prior criminal charges among candidates and elected politicians, by ideology



Note: This figure shows the share of individuals with prior criminal charges among candidates (panel a) and elected politicians (panel b), by party ideology. We consider candidates and elected individuals who only run for the first time since 2012 onward.

Figure A.5: Average characteristics in the general population and among candidates and elected politicians with and without prior criminal chanrges



Note: This figure shows the share of females (Panel a), white people (Panel b), people under 40 years old (Panel c), people with tertiary education (Panel d), people working in formal employment (Panel e), and people working in the public sector (Panel f) in the general population (yellow bars); among candidates with and without prior criminal charges (dark- and light-blue bars); and among elected politicians with and without prior criminal charges (light- and dark-red bars). We only consider candidates and elected individuals who run for the first time since 2012 onward.

Table A.2: Prior criminal charges and the probability of becoming a politician, by ideology

	(1) Candidate	(2) Elected	(3) Candidate	(4) Elected	(5) Candidate	(6) Elected
Any crime	0.042***	0.003***	0.013***	0.001***	0.053***	0.004***
	(0.002)	(0.000)	(0.001)	(0.000)	(0.002)	(0.000)
Observations	9,383,949	9,383,949	9,383,949	9,383,949	9,383,949	9,383,949
R-squared	0.002	0.001	0.001	0.001	0.002	0.001
Ideology	Left	Left	Center	Center	Right	Right
Baseline prob.	0.052%	0.003%	0.014%	0.001%	0.062%	0.004%
%Δ over baseline	+80%	+94%	+93%	+149%	+86%	+104%

Note: This table shows the relationship between prior criminal charges and the probability of entering politics, by party ideology. In particular, the table shows the OLS-estimated coefficient of a regression of dummies for running for election for the first time (odd columns) or being elected (even columns) from 2012 onward on a dummy for having been charged for any type of crime in the past (*Any crime).* For those never running for a public office we consider whether they have been charged for any crime by the year before each electoral round. To ease the interpretation of the estimated coefficients, dependent variables are multiplied by 100. All specifications control for individual gender, skin color, age, education, previous job (formal vs informal and Public vs no Public), and municipality-year fixed effects. Separate regressions are estimated for politicians running and being elected with Left-wing parties (cols. 1-2), Center parties (cols. 3-4), and Right-wing parties (cols. 5-6). Standard errors are robust to heteroskedasticity and clustered at the municipality-year level. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. The last row of the table re-scales the estimated coefficient of *Any crime* by the baseline probability that a non-criminal individual within the Brazilian population either runs for the first time as a candidate or is elected for any type of office (also reported in the second to last row).

Table A.3: Prior criminal charges and probability of becoming a politician, by type of political office

	(1) Candidate	(2) Elected	(3) Candidate	(4) Elected	(5) Candidate	(6) Elected	(7) Candidate	(8) Elected
Any crime	0.171***	0.013***	0.006***	0.001***	0.008***	0.00003	0.004***	0.00012**
	(0.009)	(0.001)	(0.001)	(0.000)	(0.001)	(0.00006)	(0.001)	(0.00006)
Observations	5,783,914	5,783,914	5,303,495	5,303,495	3,588,359	3,588,359	3,582,917	3,582,917
R-squared	0.003	0.001	0.000	0.000	0.000	0.000	0.000	0.000
Office	Counc.	Counc.	Mayor	Mayor	State	State	National	National
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Mun. X Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Baseline prob.	0.217%	0.015%	0.003%	0.001%	0.005%	0.0001%	0.002%	0.00007%
%Δ over baseline	**+79%**	**+88%**	**+169%**	**+115%**	**+151%**	**+29%**	**+175%**	**+182%**

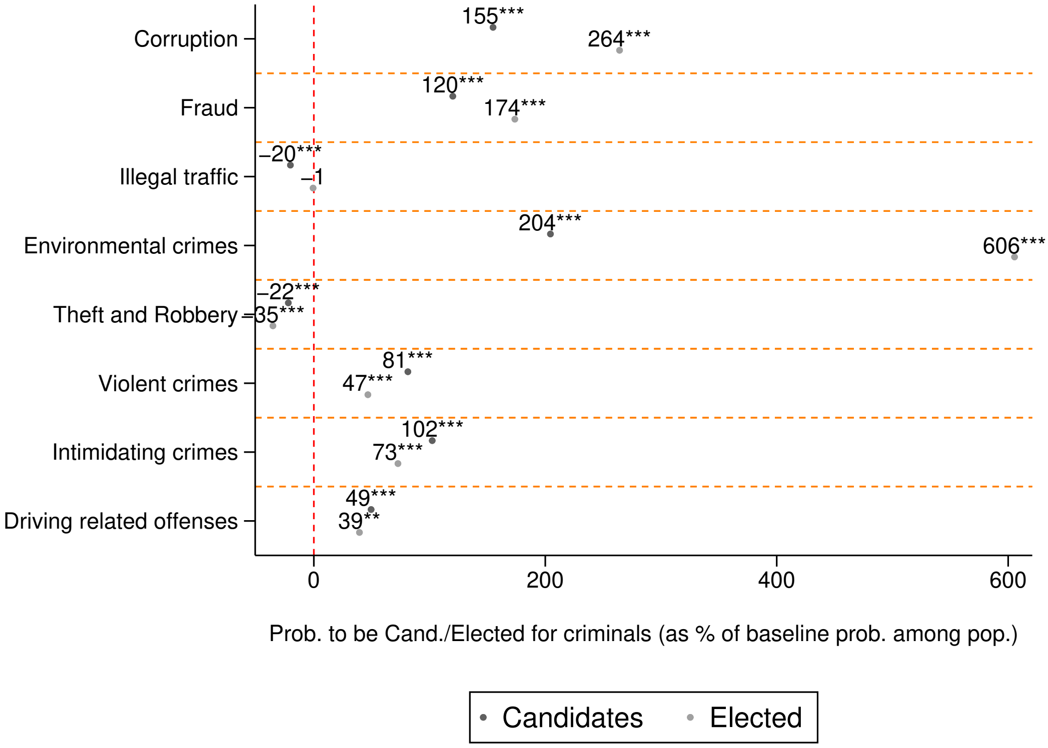
Note: This table shows the relationship between prior criminal charges and the probability of entering politics, by type of political office. In particular, the table shows the OLS-estimated coefficient of a regression of dummies for running for election for the first time (odd columns) or being elected (even columns) from 2012 onward on a dummy for having been charged for any type of crime in the past (*Any crime).* For those never running for a public office we consider whether they have committed any crime by the year before each electoral round. To ease the interpretation of the estimated coefficients, dependent variables are multiplied by 100. All specifications control for individual gender, skin color, age, education, previous job (formal vs informal and Public vs no Public), and municipality-year fixed effects. Separate regressions are estimated for politicians running and being elected as municipal councillors (cols. 1-2), Mayors (cols. 3-4), State representatives (cols. 5-6), and members of the national parliament (cols. 7-8). Standard errors are robust to heteroskedasticity and clustered at the municipality-year level. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. The last row of the table re-scales the estimated coefficient of *Any crime* by the baseline probability that a non-criminal individual within the Brazilian population either runs for the first time as a candidate or is elected for any type of office (also reported in the second to last row).

Table A.4: Prior criminal charges and probability of becoming a politician, by type of crime

	(1) Candidate	(2) Elected
Corruption	0.208***	0.024***
	(0.019)	(0.004)
Fraud	0.161***	0.016***
	(0.012)	(0.003)
Illegal traffic	-0.027***	-0.000
	(0.003)	(0.001)
Environmental crimes	0.274***	0.054***
	(0.031)	(0.010)
Theft and Robbery	-0.030***	-0.003***
	(0.004)	(0.001)
Violent crimes	0.109***	0.004***
	(0.008)	(0.001)
Intimidating crimes	0.137***	0.007***
	(0.009)	(0.001)
Driving related offenses	0.066***	0.004**
	(0.007)	(0.001)
Observations	9,383,949	9,383,949
R-squared	0.004	0.001
Baseline prob.	0.009%	0.134%

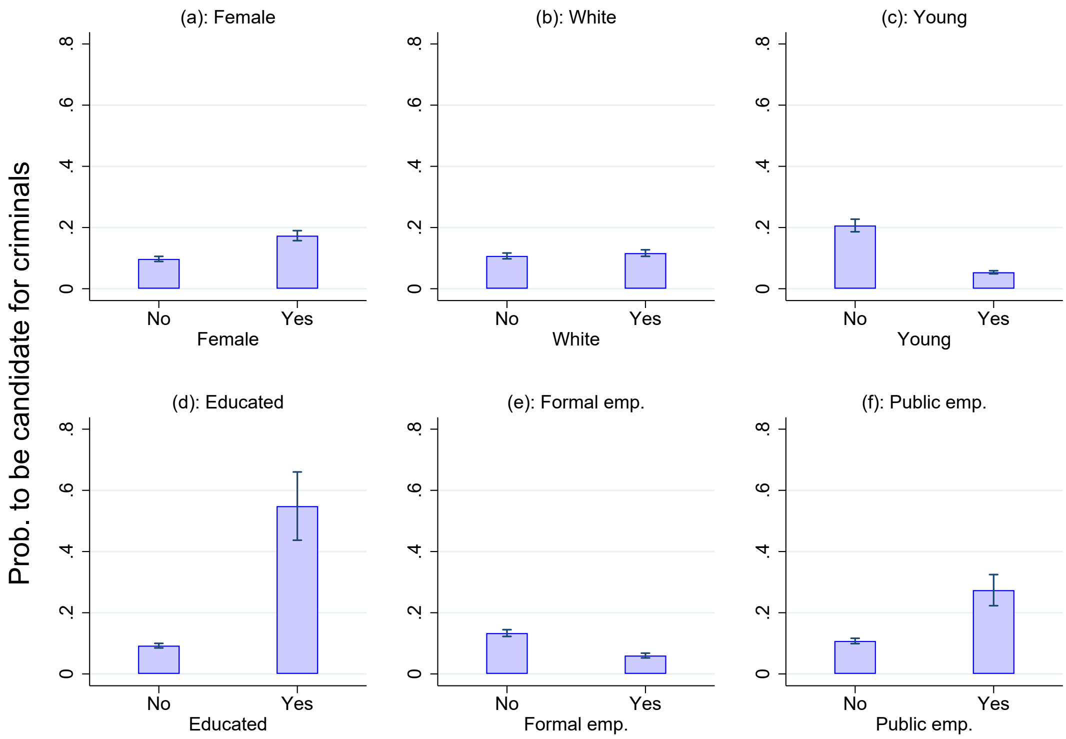
Note: This table shows the relationship between prior criminal charges and the probability of entering politics, by type of crime. In particular, the table shows the OLS-estimated coefficient of a regression of dummies for running for election for the first time (col. 1) or being elected (col. 2) from 2012 onward on a set of dummies for having been charged for different types of crime in the past. For those never running for a public office we consider whether they have been charged for any crime by the year before each electoral round. To ease the interpretation of the estimated coefficients, dependent variables are multiplied by 100. All specifications control for individual gender, skin color, age, education, previous job (formal vs informal and Public vs no Public), and municipality-year fixed effects. Standard errors are robust to heteroskedasticity and clustered at the municipality-year level. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. The last row of the table re-scales the estimated coefficient of *Any crime* by the baseline probability that a non-criminal individual within the Brazilian population either runs for the first time as a candidate or is elected for any type of office (also reported in the second to last row).

Figure A.6: Prior criminal charges and probability of becoming a politician, by type of crime



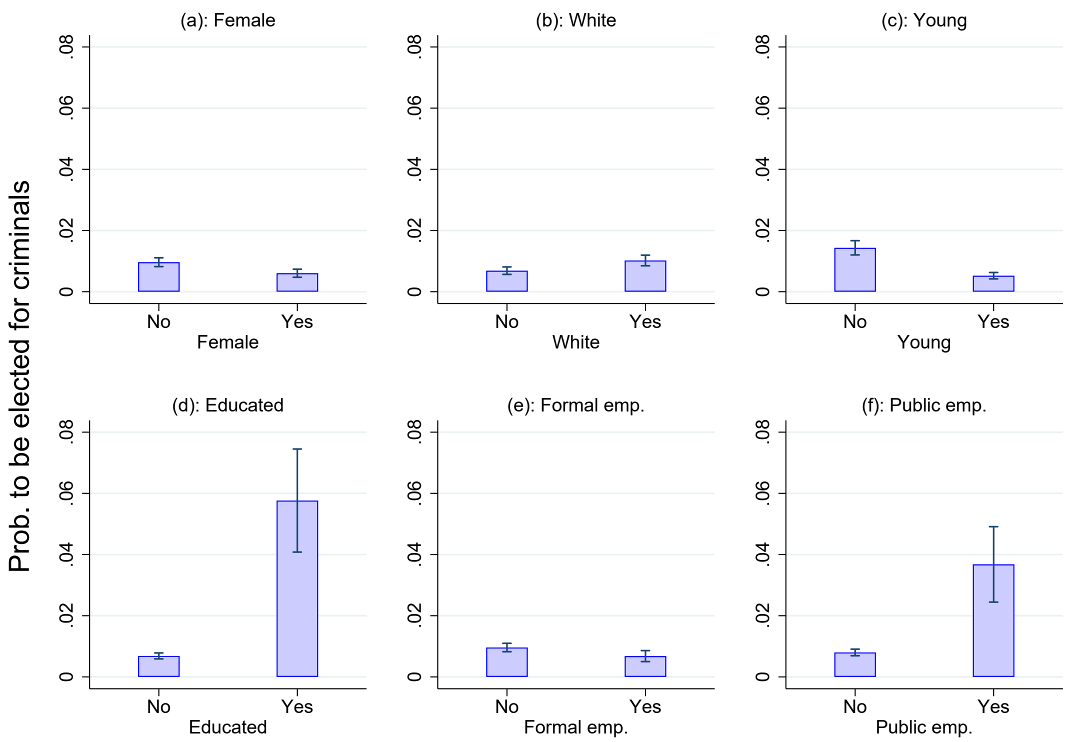
Note: This figure shows the estimated coefficients in Table A.4, expressed as relative changes over the baseline probability that a non-criminal individual either runs for the first time as a candidate (light-gray points) or is elected for any type of office (dark-gray points).

Figure A.7: Prior criminal charges and probability of running for election, heterogeneity by individual characteristics



Note: This table shows the relationship between prior criminal charges and the probability of running for election, separately by gender (Panel a), skin color (Panel b), age (Panel c), education (Panel d), and previous job (Panels e and f). All the effects are expressed as percentage points (i.e., probability multiplied by 100).

Figure A.8: Prior criminal charges and probability of being elected, heterogeneity by individual characteristics



Note: This table shows the relationship between prior criminal charges and the probability of being elected, separately by gender (Panel a), skin color (Panel b), age (Panel c), education (Panel d), and previous job (Panels e and f). All the effects are expressed as percentage points (i.e., probability multiplied by 100).

Table A.5: Prior criminal charges and probability of becoming a politician, subsample for the state of Sao Paulo

	(1) Candidate	(2) Candidate	(3) Elected	(4) Elected
Any crime	0.067***	0.084***	0.004***	0.005***
	(0.008)	(0.010)	(0.001)	(0.001)
Observations	1,864,739	1,862,455	1,864,739	1,862,455
R-squared	0.011	0.011	0.005	0.005
Innocents	YES	NO	YES	NO
Baseline prob.	0.106%	0.106%	0.005%	0.005%
%Δ over baseline	+63%	+80%	+82%	+103%

Note: This table shows the relationship between prior criminal charges and the probability of entering politics, restricting the sample to the state of Sao Paulo for which additional information on the outcome of first-degree sentence available. In particular, the table shows the OLS-estimated coefficient of a regression of dummies for running for election for the first time (cols. 1-2) or being elected (cols. 3-4) from 2012 onward on a dummy for having been charged for any type of crime in the past (*Any crime*). For those never running for a public office we consider whether they have been charged for any crime by the year before each electoral round. Columns 1 and 3 include all individuals in this sample regardless of the first-degree sentence, while columns 2 and 4 exclude individuals that were subsequently acquitted. To ease the interpretation of the estimated coefficients, dependent variables are multiplied by 100. All specifications control for individual gender, skin color, age, education, previous job (formal vs informal and Public vs no Public), and municipality-year fixed effects. Standard errors are robust to heteroskedasticity and clustered at the municipality-year level. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. The last row of the table re-scales the estimated coefficient of *Any crime* by the baseline probability that a non-criminal individual within the Brazilian population either runs for the first time as a candidate or is elected for any type of office (also reported in the second to last row).

Table A.6: Prior criminal charges and probability of becoming a politician, robustness

	(1) Candidate	(2) Elected	(3) Candidate	(4) Elected	(5) Candidate	(6) Elected
Any crime	-0.064***	-0.006***	0.124***	0.010***	0.112***	0.009***
	(0.006)	(0.001)	(0.005)	(0.001)	(0.002)	(0.000)
Observations	9,164,342	9,164,342	9,373,463	9,373,463	9,383,949	9,383,949
R-squared	0.004	0.001	0.004	0.001	0.004	0.001
Trials	Concluded	Concluded	Ongoing	Ongoing	All	All
Baseline prob.	0.135	0.009	0.135	0.009	0.134	0.010
%Δ over baseline	-48%	-65%	+92%	+107%	+84%	+99%

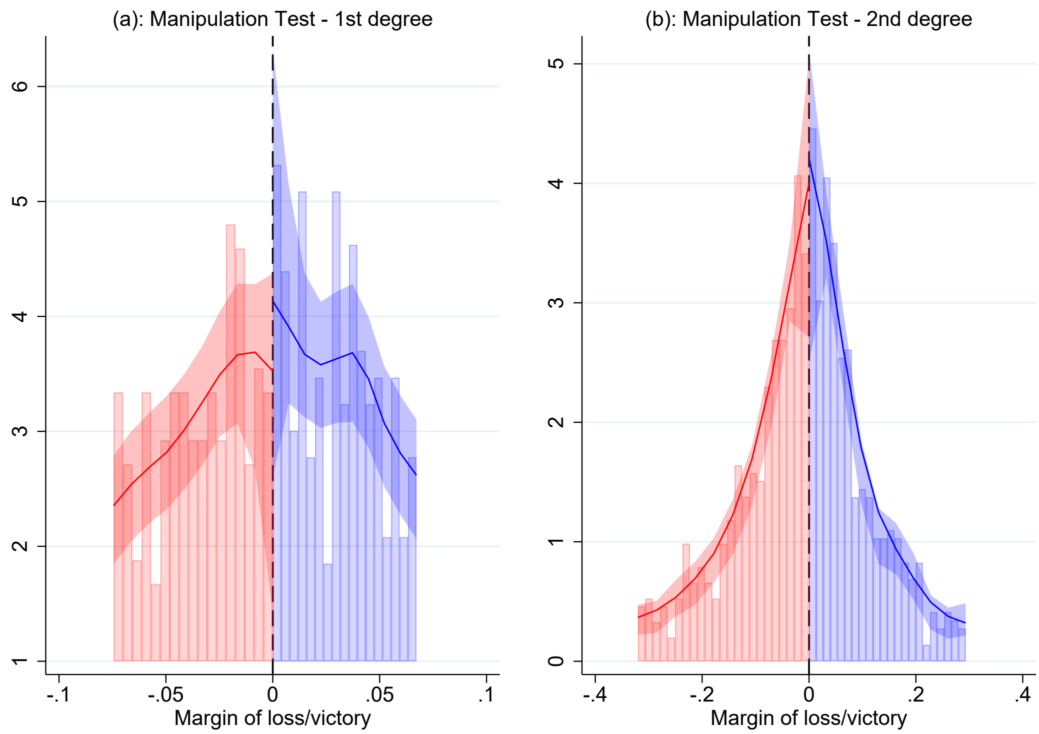
Note: This table shows the relationship between prior criminal charges and the probability of entering politics, distinguishing prior charges by the outcome of the trial. In particular, the table shows the OLS-estimated coefficient of a regression of dummies for running for election for the first time (odd columns) or being elected (even columns) from 2012 onward on a dummy for having been charged for any type of crime in the past (*Any crime*). For those never running for a public office we consider whether they have been charged for any crime by the year before each electoral round. In columns 1-2 we include in the sample only trials which reached a final sentence; in columns 3-4 we include ongoing trials; while columns 5-6 include all trials. To ease the interpretation of the estimated coefficients, dependent variables are multiplied by 100. All specifications control for individual gender, skin color, age, education, previous job (formal vs informal and Public vs no Public), and municipality-year fixed effects. Standard errors are robust to heteroskedasticity and clustered at the municipality-year level. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. The last row of the table re-scales the estimated coefficient of *Any crime* by the baseline probability that a non-criminal individual within the Brazilian population either runs for the first time as a candidate or is elected for any type of office (also reported in the second to last row).

Table A.7: Differences between audited and non-audited municipalities

	Non Audited		Audited		
	Mean	SD	Mean	SD	Std. Diff.
Avg. years of educ. (2000)	3.934	1.252	4.097	1.303	-0.127
Dropout (%, 2000)	0.065	0.051	0.059	0.045	0.140
Illiteracy (2000)	0.282	0.151	0.259	0.152	0.153
Active women (2000)	0.383	0.103	0.395	0.110	-0.113
Fertility (2000)	2.96	0.802	2.816	0.710	0.189
Poverty (2000)	0.268	0.183	0.238	0.179	0.166
Population (2000)	23,508	42,049	34,586	227,807	-0.068
Electricity (2000)	0.852	0.174	0.874	0.164	-0.131
Digital divide (2000)	2.906	3.454	3.464	4.043	-0.149

Note: This table shows the average characteristics in 2000 of municipalities never receiving an audit (first two columns) and receiving at least one audit (second two columns), together with the standardized differences between the two groups (last column).

Figure A.9: RD design at the electoral cutoff, manipulation test



Note: This figure shows the density of the running variable in the RD design in Equation 3. The estimation sample is restricted to municipalities in which one candidate between the winner and the most voted loser has prior criminal charges while the other one has a clean criminal record in the elections of 2012 and 2016. The running variable is the margin of victory between the winner and the most-voted loser in each election. Local polynomial density estimates and confidence intervals computed according to Cattaneo et al. (2020) are also reported in the graph.

Table A.8: RD design for the effect of electing mayors with prior criminal charges, balance test at the cutoff

	Linear polynomial		Quadratic polynomial	
	RD Effect	Robust p-val	RD Effect	Robust p-val
	(1)	(2)	(3)	(4)
No education (%, 2010)	-0.011	0.062	-0.012	0.091
Elementary educ. (%, 2010)	-0.004	0.691	-0.007	0.537
Secondary educ. (%, 2010)	0.019	0.275	0.027	0.206
Dropout (%, 2010)	0.796	0.195	0.876	0.225
Illiteracy (%, 2010)	0.453	0.266	0.289	0.599
Active women (%, 2000)	0.005	0.806	0.006	0.785
Fertility (2000)	-0.098	0.359	-0.093	0.441
Poverty (%, 2000)	0.029	0.142	0.040	0.082
Population (2011)	3,145	0.489	2,708	0.628
Electricity (2000)	-0.013	0.506	-0.033	0.163
Digital divide (2000)	0.130	0.800	-0.025	0.968
Turnout	-0.006	0.555	-0.010	0.422
Registered votes	1,836	0.511	1,035	0.759
Abstention	347	0.568	322	0.671
Gender (%, winner)	-0.016	0.821	-0.010	0.917
Young (%, winner)	-0.073	0.409	0.014	0.900
Educated (%, winner)	-0.093	0.306	-0.061	0.588
Incumbent (%, winner)	0.105	0.263	0.095	0.408
Incumbent party (%, winner)	0.066	0.515	0.073	0.531

Note: This table shows the difference in means (col. 1 and 3) and their robust p-value (col. 2 and 4) between municipalities just to the right and to the left of the cutoff in the RD design in Equation (3). The sample is restricted to municipalities in which one candidate between the winner and the most voted loser has prior criminal charges while the other one has a clean criminal record in the elections of 2012 and 2016. The running variable is the margin of victory between the winner and the most-voted loser in each election, and we include in the regression observations within a symmetric bandwidth around the cutoff, computed according to the criteria of Calonico et al. (2014, 2020). Columns 1 and 2 include a linear RD specification, while columns 3 and 4 include a quadratic specification; all regressions include fixed effects for Brazilian states and electoral rounds and weight observations by a triangular kernel in distance from the cutoff. Standard errors are robust to heteroskedasticity. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

Table A.9: The effects of electing mayors with prior criminal charges on alternative indicators of basic health, RD estimates

	(1) Mortality 3M	(2) Mortality 6M	(3) Mortality 1Y
Criminal	0.002**	0.003**	0.002**
	(0.001)	(0.001)	(0.001)
Observations	1,094	1,094	1,094
Bandwidth	.08	.07	.09
Base Value	.01	.011	.012
Robust P-val.	.018	.011	.03

Note: This figure shows the effect of electing a mayor with prior criminal charges in the electoral rounds of 2012 and 2016 on alternative indicaotrs of basic health, as estimated from RD design in Equation 3. In particular, basic health is measured by the infant mortality rate under 3 months (col. 1), 6 months (col. 2), and 1 year (col. 3). All outcomes are averaged over the four-year period after elections. The estimation sample is restricted to municipalities in which one candidate between the winner and the most voted loser has prior criminal charges while the other one has a clean criminal record, and the main explanatory variable, *Criminal* is a dummy equal to 1 if the candidate with prior criminal a criminal is elected as mayor, and 0 otherwise. The running variable is the margin of victory between the winner and the most-voted loser in each election, and we include in the regression observations within a symmetric bandwidth around the cutoff, computed according to the criteria of Calonico et al. (2014, 2020). All regressions include a linear RD specification, fixed effects for Brazilian states and electoral rounds, and weight observations by a triangular kernel in the distance from the cutoff. The last two rows of each panel report the average value of the dependent variable in municipalities electing a non-criminal mayor within the estimation bandwidth, and the robust p-value for each estimate. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

Table A.10: The effect of electing mayors with prior criminal charges on basic health, alternative RD specifications

	(1) Babies under.	(2) Mortality 5Y	(3) Babies under.	(4) Babies under.	(5) Mortality 5Y	(6) Mortality 5Y
Criminal	0.008**	0.004**	0.007**	0.008**	0.002*	0.004**
	(0.004)	(0.002)	(0.003)	(0.004)	(0.001)	(0.002)
Observations	1,094	1,094	1,094	1,094	1,094	1,094
Kernel	Triangular	Triangular	Uniform	Uniform	Uniform	Uniform
Pol. degree	2	2	1	2	1	2
Bandwidth	.17	.09	.08	.13	.07	.08
Obs. within band. L/R	403/412	295/314	264/286	346/362	247/274	276/299
Base Value	.081	.014	.081	.081	.014	.014
Robust P-val.	.028	.01	.031	.043	.056	.021

Note: This figure shows the effect of electing a mayor with prior criminal charges in the electoral rounds of 2012 and 2016 on basic health, as estimated from RD design in Equation 3. Basic health is measured by the share of babies born underweight (odd columns) and the infant mortality rate under 5 years (even columns). All outcomes are averaged over the four-year period after elections. The estimation sample is restricted to municipalities in which one candidate between the winner and the most voted loser has prior criminal charges while the other one has a clean criminal record, and the main explanatory variable, *Criminal* is a dummy equal to 1 if the candidate with prior criminal a criminal is elected as mayor, and 0 otherwise. The running variable is the margin of victory between the winner and the most-voted loser in each election, and we include in the regression observations within a symmetric bandwidth around the cutoff, computed according to the criteria of Calonico et al. (2014, 2020). Columns 1, 2, 4 and 6 include a quadratic polynomial in the running variable, while columns 3 and 5 use a linear specification; all regressions also include fixed effects for Brazilian states and electoral rounds. Columns 1 and 2 weight observations by a triangular kernel in the distance from the cutoff, while columns 3-6 use a uniform kernel. The last two rows of each panel report the average value of the dependent variable in municipalities electing a non-criminal mayor within the estimation bandwidth, and the robust p-value for each estimate. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

Table A.11: The effect of electing mayors with prior criminal charges on basic health, placebo RD estimates for the year before elections

	(1) Babies under.	(2) Mortality 5Y
Criminal	0.007	-0.002
	(0.006)	(0.002)
Observations	1,094	1,094
Pol. degree	1st	1st
Bandwidth L/R	.1/.1	.08/.08
State FE	YES	YES
Base Value	.081	.014
Robust P-val.	.379	.305

Note: This figure shows the effect of electing a mayor with prior criminal charges in the electoral rounds of 2012 and 2016 on basic health in the year before elections, as estimated from RD design in Equation 3. Basic health is measured by the share of babies born underweight (col. 1) and the infant mortality rate under 5 years (col. 2). Both outcomes are measured in the year before elections. The estimation sample is restricted to municipalities in which one candidate between the winner and the most voted loser has prior criminal charges while the other one has a clean criminal record, and the main explanatory variable, *Criminal* is a dummy equal to 1 if the candidate with prior criminal a criminal is elected as mayor, and 0 otherwise. The running variable is the margin of victory between the winner and the most-voted loser in each election, and we include in the regression observations within a symmetric bandwidth around the cutoff, computed according to the criteria of Calonico et al. (2014, 2020). All regressions include a linear RD specification, fixed effects for Brazilian states and electoral rounds, and weight observations by a triangular kernel in the distance from the cutoff. The last two rows of each panel report the average value of the dependent variable in municipalities electing a non-criminal mayor within the estimation bandwidth, and the robust p-value for each estimate. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

Table A.12: Criminally Charged Mayors and Policy Making: RD Estimates for Public Expenditures - Part 1

	(1) Tot. exp	(2) Adm.	(3) Agr.	(4) Soc.	(5) Sport	(6) Educ.	(7) Special.	(8) Leg.	(9) San.	(10) Health	(11) Trans.	(12) Urb.
Criminal	3.747	-31.097	-9.617	-5.545	-10.639**	25.587	2.740	2.011	-7.042	-17.654	-11.832	15.853
	(205.937)	(43.535)	(16.253)	(7.726)	(4.963)	(46.753)	(9.384)	(7.518)	(9.331)	(38.002)	(27.915)	(22.170)
Observations	1,337	1,337	1,337	1,337	1,337	1,337	1,337	1,337	1,337	1,337	1,337	1,337
Pol. degree	1st	1st	1st	1st	1st	1st	1st	1st	1st	1st	1st	1st
Bandwidth L/R	.15/.15	.2/.2	.19/.19	.19/.19	.1/.1	.12/.12	.13/.13	.21/.21	.12/.12	.15/.15	.19/.19	.12/.12
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Base Value (millions)	60.8	8.9	.8	1.7	.3	18.2	1.5	1.8	.8	14.5	1.2	4.5
Robust P-val.	.931	.506	.626	.543	.022	.514	.772	.829	.335	.694	.729	.479

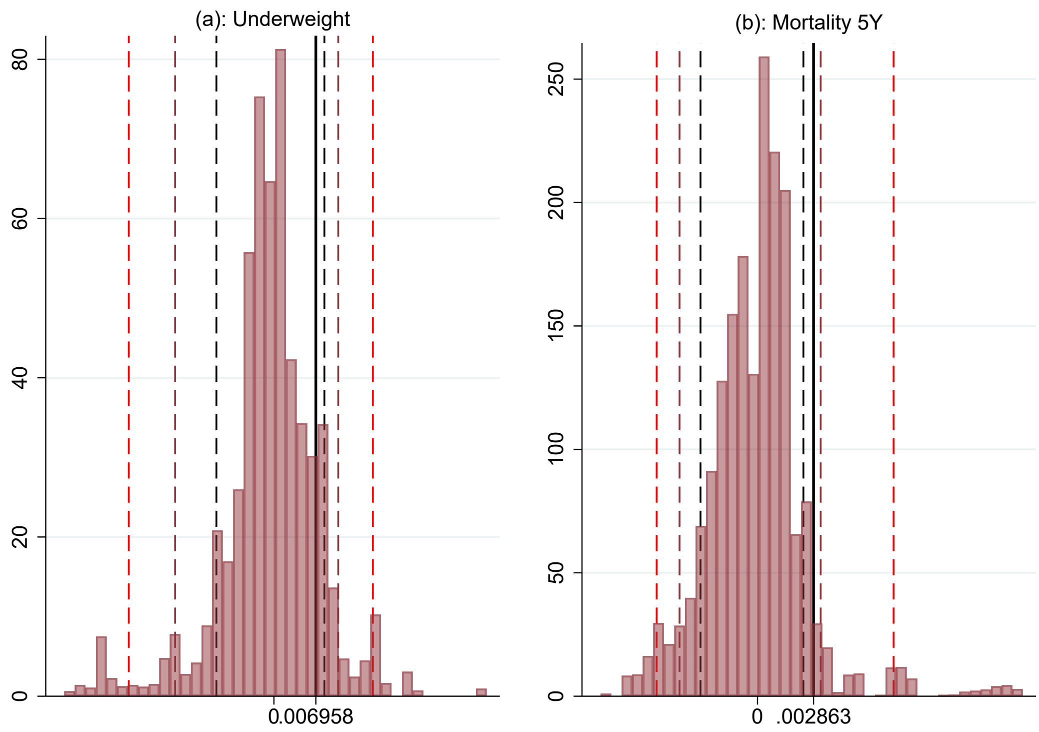
Note: This table shows the coefficients of the RD regression, as estimated from Equation 3, on different items of public expenditure per capita at the municipal level, which is averaged over the four years-period following the mayoral electoral rounds of 2012 and 2016. The items considered are total expenditures (col. 1), administration (col. 2), agriculture (col. 3), social assistance (col. 4), sport (col. 5), education (col. 6), special assignments (col. 7), legislative (col. 8), sanification (col. 9), health (col. 10), transports (col. 11), urbanization (col. 12). The running variable is the margin of victory between the winner and the two most-voted losers in the mayoral electoral rounds of 2012 and 2016, normalized to 0, and excluding margins greater in absolute value than 50%. *Criminal* is a dummy equal to 1 if a criminal is elected as mayor, and 0 otherwise. The estimation sample is restricted to municipalities where a criminal candidate runs against a non-criminal one and to observations within a symmetric bandwidth around the cutoff, computed according to the criteria of Calonico et al. (2014, 2020). All columns include a linear RD specification, fixed effects for Brazilian states and electoral rounds, and weight observations by a triangular kernel in the distance from the cutoff. The last two rows of each panel report the average value of the dependent variable in municipalities electing a non-criminal mayor within the estimation bandwidth (in millions of Reais), and the robust p-value for each estimate. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

Table A.13: Criminally Charged Mayors and Policy Making: RD Estimates for Public Expenditures - Part 2r

	(1) Cul.	(2) Comm.	(3) Energy	(4) Env.	(5) Jud.	(6) Soc. sec.	(7) Sec	(8) Jus.	(9) Comu.	(10) House	(11) Ind.	(12) Lab.	(13) Rights
Criminal	-2.163	3.178	4.359*	1.607	-0.525	-4.933	7.138	2.273**	-0.061	5.523	-1.825	0.104	1.158
	(4.656)	(7.641)	(2.422)	(5.581)	(1.886)	(15.317)	(4.846)	(0.977)	(0.462)	(4.049)	(2.596)	(2.674)	(1.025)
Observations	1,337	1,337	1,337	1,337	1,337	1,337	1,337	1,337	1,337	1,337	1,337	1,337	1,337
Pol. degree	1st	1st	1st	1st	1st	1st	1st	1st	1st	1st	1st	1st	1st
Bandwidth L/R	.12/.12	.17/.17	.1/.1	.17/.17	.15/.15	.13/.13	.15/.15	.14/.14	.2/.2	.14/.14	.25/.25	.13/.13	.17/.17
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Robust P-val.	.464	.623	.048	.794	.949	.707	.132	.017	.856	.186	.458	.938	.275
Base Value (millions)	.4	.2	.2	.4	.1	2.1	.3	.04	.02	.1	.07	.05	.03

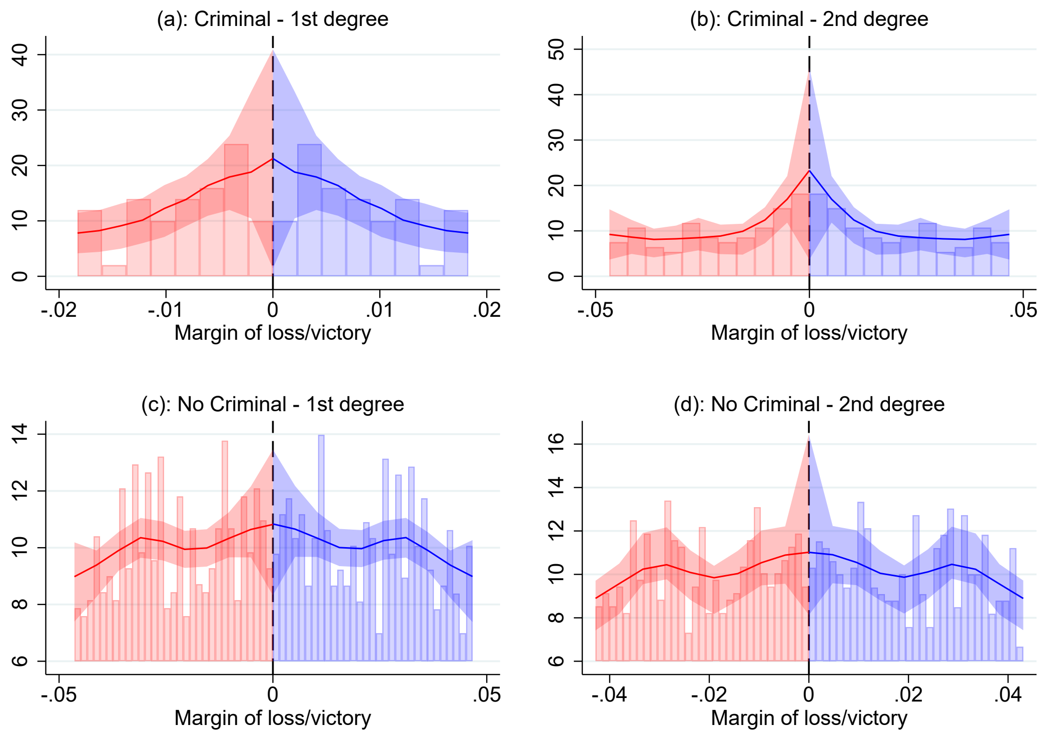
Note: This table shows the coefficients of the RD regression, as estimated from Equation 3, on different items of public expenditure per capita at the municipal level, which is averaged over the four years-period following the mayoral electoral rounds of 2012 and 2016. The items considered are culture (col. 1), commerce (col. 2), energy (col. 3), environment (col. 4), judiciary (col. 5), social security (col. 6), public security (col. 7), justice (col. 8), comunications (col. 9), housing (col. 10), industry (col. 11), labor (col. 12), and citizens' rights. The running variable is the margin of victory between the winner and the two most-voted losers in the mayoral electoral rounds of 2012 and 2016, normalized to 0, and excluding margins greater in absolute value than 50%. *Criminal* is a dummy equal to 1 if a criminal is elected as mayor, and 0 otherwise. The estimation sample is restricted to municipalities where a criminal candidate runs against a non-criminal one and to observations within a symmetric bandwidth around the cutoff, computed according to the criteria of Calonico et al. (2014, 2020). All columns include a linear RD specification, fixed effects for Brazilian states and electoral rounds, and weight observations by a triangular kernel in the distance from the cutoff. The last two rows of each panel report the average value of the dependent variable in municipalities electing a non-criminal mayor within the estimation bandwidth (in millions of Reais), and the robust p-value for each estimate. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

Figure A.10: The effect of electing mayors with prior criminal charges on basic health, distribution of placebo RD estimates



Note: These graphs show distributions of placebo estimates of the effect of electing a mayor with prior criminal charges on basic health obtained by estimating the RD equation (3) at 10,000 equally-spaced placebo cutoffs between the 10^{th} and 90^{th} percentiles of the distribution of the margin of victory of candidates with prior criminal charges. Basic health is measured by the share of babies born underweight (left graph) and the infant mortality rate under 5 years (right graph). The estimation sample is restricted to municipalities in which one candidate between the winner and the most voted loser has prior criminal charges while the other one has a clean criminal record, and the main explanatory variable is a dummy equal to 1 if the candidate with prior criminal a criminal is elected as mayor, and 0 otherwise. We include in each placebo the regression observations within a symmetric bandwidth around the cutoff, computed according to the criteria of Calonico et al. (2014, 2020). All regressions include a linear RD specification, fixed effects for Brazilian states and electoral rounds, and weight observations by a triangular kernel in the distance from the cutoff. Black, brown, and red dashed lines report 90^{th}, 95^{th}, and 97.5^{th} percentiles of distribution of estimates. Solid black line reports RDD estimate at the true cutoff.

Figure A.11: Criminally Charged Mayors and Private Returns: Manipulation Test



Note: This figure shows the density of the running variable in the RD design in Equation 4 estimated separately for municipalities in which candidates with or without prior criminal charges won the elections (top and bottom graphs, respectively). The running variable is the margin of victory between the winner and the most-voted loser in each election. Local polynomial density estimates and confidence intervals computed according to Cattaneo et al. (2020) for linear and quadratic approximations (left and right graphs, respectively) are also reported in the figure.

Table A.14: RD design for the effect of appointing candidates with or without prior criminal charges on patronage, balance test and the electoral cutoff

	Linear polynomial		Quadratic polynomial	
	RD Effect	Robust p-val	RD Effect	Robust p-val
	(1)	(2)	(3)	(4)
Panel A: Candidate with prior criminal charges winning elections				
Female (%)	0.014	0.832	-0.013	0.889
White (%)	0.175	0.274	0.131	0.420
Young (%)	-0.017	0.802	-0.009	0.922
Formal Emp. (%)	-0.197	0.099*	-0.264	0.114
Educated (%)	-0.001	0.996	-0.021	0.903
Public emp. (%)	-0.160	0.142	-0.146	0.338
Panel B: Candidate without prior criminal charges winning elections				
Female (%)	0.054	0.065*	0.066	0.109
White (%)	0.082	0.385	0.154	0.242
Young (%)	0.006	0.824	0.008	0.831
Formal Emp. (%)	-0.339	0.000***	-0.394	0.000***
Educated (%)	-0.032	0.456	-0.062	0.333
Public emp. (%)	-0.257	0.000***	-0.296	0.000***

Note: This table shows the difference in means (cols. 1 and 3) and their robust p-value (cols. 2 and 4) between municipalities just to the right and to the left of the cutoff in the RD design in Equation (4) estimated separately for candidates with prior criminal charges (top panel) and candidates with clean criminal records (bottom panel). The sample is restricted to municipalities in which one candidate between the winner and the most voted loser has prior criminal charges while the other one has a clean criminal record in the elections of 2012 and 2016. The running variable is the margin of victory between the winner and the most-voted loser in each election, and we include in the regression observations within a symmetric bandwidth of 5 percentage points around the cutoff. Columns 1 and 2 include a linear RD specification, while columns 3 and 4 include a quadratic specification; all regressions include fixed effects for Brazilian states and electoral rounds and weight observations by a triangular kernel in distance from the cutoff. Standard errors are robust to heteroskedasticity. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

Table A.15: Difference in means between municipalities in which candidates with or without prior criminal charges won the elections

	Non Criminal		Criminal		
	Mean	SD	Mean	SD	Std. Diff.
No education (%, 2010)	0.117	0.035	0.123	0.036	-0.187
Elementary educ. (%, 2010)	0.151	0.052	0.154	0.055	-0.047
Secondary educ. (%, 2010)	0.681	0.095	0.666	0.102	0.149
Dropout (%, 2010)	0.089	0.036	0.086	0.033	0.079
Illiteracy (%, 2010)	0.038	0.039	0.033	0.030	0.138
Active women (%, 2000)	0.394	0.120	0.427	0.130	-0.260
Fertility (2000)	2.902	0.772	2.746	0.627	0.223
Poverty (%, 2000)	0.267	0.185	0.228	0.170	0.217
Population (2011)	15,851	19,963	19,963	27,594	-0.171
Electricity (2000)	0.854	0.171	0.882	0.151	-0.171
Digital divide (2000)	2.608	2.897	3.345	3.615	-0.225
Turnout	0.871	0.058	0.878	0.058	-0.125
Registered votes	10,242	11,885	13,034	17,223	-0.189
Abstention	1,881	2,659	2,353	3,674	-0.147
Gender (%, winner)	0.156	0.363	0.051	0.220	0.352
Young (%, winner)	0.161	0.368	0.104	0.305	0.170
Educated (%, winner)	0.467	0.499	0.471	0.499	-0.008
Incumbent (%, winner)	0.209	0.407	0.212	0.409	-0.008
Incumbent party (%, winner)	0.169	0.375	0.180	0.384	-0.030

Note: This table shows the average characteristics of municipalities in which candidates with prior criminal charges (first two columns) or candidates with clean criminal records (second two columns) won the elections in the electoral round of 2012 and 2016, together with the standardized difference between the two groups (last column). The sample is restricted to municipalities in which the margin of victory remained below 5 percentage points.

A.2 Appendix: Municipal-Level Data

We complement our data with several pieces of information at the municipal level for the period 2009-2020. From *Sistema de Informaçoes de Mortalidade* (SIM) provided by the Brazilian Ministry of Health, we retrieve information on infant mortality under three months, one year, and five years, while from *Sistema de Informação sobre Nascidos Vivos* (SINASC) we get the share of babies born underweight, the share of pregnancy ended prematurely and the share of babies born dead. Concerning educational performance, we use *Sistema de Avaliação da Educação Básica* (SAEB) provided by the Ministry of Education to recover the yearly average score for municipal schools in each municipality, as well as the average score in Math and Portuguese.

We turn to FINBRA (*Finanças Municipais*), provided by the Brazilian Secretary of Treasury, to collect data on public finance at the municipal-year level. We construct two sets of variables on the yearly expenditures of each municipality. Expenditures are classified by spending item[A.1]

From the census waves of 2000 and 2010, we collect additional municipal-level information regarding educational attainment, female labor force participation, fertility, poverty, digital divide, and electricity supply. In turn, we obtain the total turnout at the municipal elections of 2016, as well as the number of registered voters and the abstention rate, from *Tribunal Superior Eleitoral*.

[A.1]The items considered are: administration, agriculture, social insurance, sport, education, special assignments, legislative, sanification, health, transports, urbanization, culture, commerce, energy, environment, judiciary, social security, public security, justice, communication, housing, industry, labor, and citizen's rights. All of them are expressed in millions of Reales.