Universidade de Brasília
Faculdade de Economia, Administração, Contabilidade e Gestão Pública
Departamento de Economia

Ricardo Koubik Saldanha

Race and governance: the case of local governments in Brazil

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Dissertação apresentada ao Departamento de Economia da Universidade de Brasília como requisito parcial à obtenção do título de Mestre em Economia.

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Rafael Terra de Menezes, D.Sc.
Orientador

Michael Christian Lehmann, Phd.
Universidade de Brasília

Sergio Naruhiko Sakurai, D.Sc.
Universidade de São Paulo

Brasília - DF
2022

Abstract

Brazil has a Nonwhite majority that faces disadvantages in terms of social-economic indicators and is politically underrepresented. When Nonwhite politicians come to power, does race influence their policies? Since 2016, electoral authority provides information of mayor's self-declared color/race. I use regression-discontinuity design exploring this information in close elections to examine the relation between race and other mayoral characteristics, as well as chance of re-election, style of governance and performance. I find no evidence of either electoral advantages or different governance style, including expenditures composition and implementation of programs aimed to minorities and inclusion. Apart from few exceptions attributed to chance, under Nonwhite mayors' terms, performance variables are indistinguishable as well.

Keywords: color/race, political selection, style of governance, government performance.

Resumo

O Brasil tem uma maioria não-branca que enfrenta desvantagens em termos de indicadores sócio-econômicos e é politicamente subrepresentada. Quando um político não-branco chega ao poder, sua raça influencia suas políticas? Desde 2016, a autoridade eleitoral fornece dados da cor/raça auto-declarada dos prefeitos. Eu utilizo um desenho de regressão descontínua em eleições apertadas para examinar a relação entre raça e outras características dos prefeitos, bem como chances de reeleição, estilo de governança e performance. Eu não encontro evidências de vantagens eleitorais ou de diferenças em estilo de governança, inclusive na composição de gastos e na implementação de políticas voltadas a minorias e inclusão. A não ser por poucas exceções atribuídas ao acaso, sob mandados de prefeitos não-brancos, indicadores de performance também são indistinguíveis.

Palavras-chave: cor/raça, seleção política, estilo de governança, performance do governo.

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IBGE	Instituto Brasileiro de Geografia e Estatística (Brazilian statistical office)
MNU	Movimento Negro Unificado (Unified Black Movement)
Siconfi	Sistema de Informações Contábeis e Fiscais do Setor Público Brasileiro (Fiscal Information System on the Brazilian Public Budget)
SIDRA	Sistema IBGE de Recuperação Automática (IBGE Automatic Recovery System)
TEN	Teatro Experimental Negro (Experimental Black Theater)
TSE	Tribunal Superior Eleitoral (Federal Electoral Court)
UHC	União dos Homens de Cor (Union of Men of Color)
WHO	World Health Organization

Contents

1 Introduction

Black population in Brazil exhibits worse conditions than the Whites in any socio-economic indicator[1]. Black people are also politically underrepresented[2]. Item 3.1 shows that these indicators stem from a history of racial inequalities, which goes back to the slavery period, and, more importantly, is perpetuated through generations. As presented in item 3.2, the literature suggests that these racial inequalities in the economic field explain most of the lack of representation. The same subsection shows, however, that the Black movement has a history of struggle, for example, for the integration of Black people into the labor market and was responsible for recent changes that encourage parties to launch Black candidates. In such a scenario, it is reasonable to admit that a Black policy-maker may have different engagements. Moreover, the literature indicates race feature cannot be absorbed by ideology, which my tests on party ideology corroborates. I thus argue that a politician's race may be a meaningful treatment for governance style and performance indicators, specially those related to the Black population or minorities. And, as discussed ahead, this dissertation also include some hypotheses regarding political selection.

In section 2, I briefly review the related literature. The studies reviewed in item 2.1 exemplify the base of economic theory on analyzing elections. They show there are two opposite results of election models, convergence of policies (toward the preference of the median voter) or divergence of policies (politicians' preferences guide policy outcomes). Inferring whether a personal characteristic of a politician such as race can affect policies carries an implicit divergence hypothesis. Item 2.1 tells both convergence and divergence find support in the theory. While item 2.2 shows both are reasonable under the applied literature. Item 2.3 states all the hypotheses of this dissertation, which are organized into two groups, one regarding political selection and other concerning governance. They are handled by multiple testes, as explained below. That subsection also shows the contributions of this work for the literature, which are in the database built, in the empirical strategy and in the novelty in testing the effects of race on governance in Brazilian context.

Federal Electoral Court (*Tribunal Superior Eleitoral - TSE*) provides information of self-declared color/race, following the same categories used by Brazilian statistical office (*Instituto Brasileiro de Geografia e Estatística - IBGE*), which are Asian, "Pretos", "Pardos", Indigenous and White. The sum of "Pretos" and "Pardos" can be understood as Black (the first one indicates a darker skin tone)[3]. I grouped "Pretos", "Pardos" and Indigenous in a Non-white category and used a regression-discontinuity (RD) design, in which treatment variable

[1] See the report on racial differences Desigualdades Sociais por Cor ou Raça no Brasil.
[2] See Table 1, in item 3.2.
[3] Whites include Hispanic whites.

is Nonwhite mayor victory. So, Whites and Asians make up the other block. For simplicity, I will refer to this group as Whites[4]. Cutting the categories in this way is consistent with socio-economic indicators and the Brazilian racial issue[5]. Color/race variable started to be reported in 2014 for general elections and in 2016 for local elections. Thus, there are currently data to assess outcomes of one mayoral term, 2017-2020, and chance of re-election in one poll, 2020. From a sample of more than five thousand municipalities, there were 1596 interracial (White vs. Nonwhite) mayoral disputes in 2016, 775 won by Nonwhites.

There is a vast literature on the characteristics of people who become political leaders, Bó et al. (2017) is one example. And there are a few works that specifically study the contribution of race, such as Vogl (2014), in the case of United States, and Campos and Machado (2017) and Bueno and Dunning (2017), in Brazilian experience. I will briefly comment on them in subsection 3.2. I do not intend to explain the causes of that underrepresentation, such as inferring whether Black politicians experience racial prejudice, to which those studies are already dedicated. However, this work includes some questions regarding political selection. I infer about the relationship between race and party ideology. And I check for contrasts in tangible and intangible resources between races, as well as for electoral advantages. These problems are handled by testing a broad set of baseline variables, which also serve as balance checks. Besides basic personal characteristics, like age and educational level, all the attributes of most interest to the literature are represented: party ideology, incumbency status, dynastic traditions and gender. I mention examples of that literature in item 2.2. In addition, I examine self-declared assets and campaign funding, that play an important role in Campos and Machado (2017) and in Bueno and Dunning (2017). Balance checks also include municipal pre-treatment characteristics, such as usual variables related to housing and education and lagged outcomes, when available. It is also worth mentioning the use of proxies to racial and gender salary gap, inspired in Brollo and Troiano (2016). The construction of all proxies is described in subsection 4.2.

As a matter of equity, it is desirable that each race plays all roles in society in proportion to its share of the population. In a full democracy, this includes holding positions of political power. Therefore, pursuing to fill the racial representation gap is justifiable per se. Nevertheless, it would be even more interesting if we observe that Black politicians are more sensible to policies aimed at minorities or poor people, or perform comparatively well in some field. This relationship between race and governance is the main interest of this work. I aim to evaluate both policy-making style and performance. Formal hypotheses are stated in 2.3. My selection of post-treatment variables is specially inspired by Bragança, Ferraz and Rios

[4] Except in section 3, where the works cited and the discussion refers to White as a race, not as the control group.

[5] Average income data broken down by color/race illustrate this division. See SIDRA - IBGE, Table 3600.

(2015). Expenditures composition, existence of minority-oriented policies and proportion of temporary public employees purport to characterize style of governance. Expenditures are broken down into investments and current expenditures and into categories, such as housing and social assistance. I also checked 2020 expenditures separately in an attempt to identify opportunistic spending in an election year, as Sakurai and Menezes-Filho (2011) did in more detail, for mayors in general. Policies focused at minorities or poverty include family health programs oriented to black population and labor inclusion plans. In addition, although cash transfer programs in Brazil are federal, city halls are responsible for the registration of families to request the benefit. Thus, the amount of beneficiaries is affected by the mayor and may be associated with a commitment to reduce poverty. The proportion of temporary public employees is a proxy for patronage, as used by Brollo and Troiano (2016). Performance evaluation intends to cover economy growth, health, education and violence. It is necessary to find indicators that may be affected by a mayor in short-term. I will follow Fujiwara (2015) in using prenatal visits and low birth rate as health indicators. Education variables include class size and Portuguese and math scores and violence is measured by deaths from aggression.

I find no evidence of sorting around the threshold. McCrary's test does not suggest manipulation at the cutoff and pre-treatment mayoral and municipal variables show good balance. The exceptions are not sufficient evidence of precise control over the victory margin. These results both validate the RD strategy and answer questions concerning political selection. They will be discussed in item 6.1. Re-election chances do not change significantly with the race. And, with a few negligible exceptions, variables covering governance style and the accomplishments of the two racial groups appear to be indistinguishable as well. These results are presented in item 6.2. An appraisal of heterogeneity effects of "Pretos" and Indigenous separated from "Pardos" and of geographic regions gives inappreciable results, mainly due to sample restrictions. They are performed in item 6.3.

2 Related works and hypotheses

2.1 Policy convergence and divergence when modeling elections

Two opposite possible results of models that analyze political behavior and elections outcomes are convergence and divergence of policies. Convergence means, no matter the differences in personal positions on the political spectrum that candidates occupy, to win over the electorate, the implemented policies will converge towards the median voter's preference. Note that if we were sure that convergence always happens, hypotheses of personal preferences affecting governance would make no sense. And then, in whatever way a politician's race might affect his or her personal policy preferences, it would not be reflected in governance. Nevertheless, this is an unsolved issue in the literature. Although I do not intend to exhaustively review the literature on the subject[6], I will show in this subsection both tendencies, policy convergence and divergence, can find support in sophisticated models. We will see how sensitive to model assumptions the results are.

The tendency towards policy convergence was first suggested by Hotelling (1929). He presents his well known example of two grocery stores in a street, where buyers transport their purchase home at a cost proportional to the distance to the store. The location equilibrium for both stores is the middle point. The author notices the application of this model on politics, as in the case of the Republican and Democratic parties in the USA. According to him, rather than offering the electorate two contrasting positions, each party attempts to make its platform as similar to the other's as it can. Analogously to shoppers choosing the shortest distance, the reasoning is simple: while, e.g., a leftward move by Democrats may increase engagement from more radical left-wing voters, if the party adopts a more central position it can win over moderate voters without losing radical ones, who would still prefer Democrat's platform to a slightly more right-wing position of the Republican party. This idea is further explored and formalized as a theorem by Black (1948). He depicts voters preference curves in a plane, where the horizontal axis is the political spectrum and the vertical axis is the order of preference. Voters rank motions according to their preference curves. If the Condorcet criterion is satisfied, than the motion chosen by the group is the one closest to the median voter's optimum. This result is known as the **Median voter theorem**. Naturally, motions can be seen as candidates and the one-dimensional political space as a scale from left-wing to right-wing. He also states that, if preference curves are single-peaked[7], there is a Condorcet winner and so the Condorcet criterion is satisfied by a simple system such as starting with an election between any two motions, eliminate the beaten one, take any other motion to challenge the winner and so on, until the final winner remains[8].

Downs (1957) more broadly analyzes democratic government using the tools of economic theory, in an effort to identify general rules of political behavior and their results. His model also predicts policy convergence. He assumes that political leaders reap benefits of holding office, such as income, prestige and satiation of desires for power and conflict. Since the

[6] For a less detailed in each work, but broader review, see Osborne and Slivinski (1996, sec.5).

[7] Single-peaked means the preferences curves, depicted as explained above, exhibit only one local maximum, hence global maximum. Curves can be strictly increasing, strictly decreasing or ∩-shaped. See Black (1948, p.24) and the lecture, p. 6, 17-19.

[8] The author does not explicitly mention the Condorcet criterion or Condorcet winner, but describes processes that match with these definitions. If the number of motions is even and there is a tie, it is assumed to be a person with the power to cast a deciding vote.

only way to attain those rewards is to be elected, from the self-interest axiom, the author defines that the only objective of a government is voting-maximizing. It implies that policies are solely means of pursuing private interests, never a motivation by themselves[9]. Similarly, voters gain from government activity, such as security and roads maintenance, which yield utility in the same way as services obtained in the market. Hence, voters' political decisions are oriented by utility-maximizing. A key feature of Down's model is that politicians and voters go after their objectives rationally, considering the concept of rationality as it is conceived in economics - although agents are not calculating machines with perfect information, they do not systematically and deliberately allocate scarce resources in an inefficient way. He thus constructs a *homo politicus*, analogous to the *homo economicus*. Downs discuss several potential political scenarios, for example, bimodal or multimodal distribution of the electorate preferences, inter-temporal preferences (radical voters could credibly threaten not to vote for a candidate who wants to move to the center if future elections are considered), overlapped positions (a party incorporates part of the opposite side's agenda) and multi-party system. In some of these situations policies would not converge. However, his main result, under more restricted assumptions, is probably the one that most seeded future discussions in the literature. Downs asserts that, in a two-party system with a unimodal distribution of voters preferences, parties converge ideologically upon the center. There is the caveat that, although ambiguous, the parties do not become identical, because they still need to show themselves to be distinguishable to voters.

Feddersen, Sened and Wright (1990) contributes to the literature that predicts policy convergence by encompassing multi-party system and endogenous entry of candidates. In the first stage of the model, politicians decide whether to enter and pick a position in a policy space represented by the unit interval. Political leaders go after the benefits of holding office b, and they pay a fix fee c if they decide to enter. So, when deciding to enter, a candidate's expected payoff is $\lambda b - c$, where λ is the probability of winning. Because c is supposed to be positive, so will be b and λ, otherwise the politician would not enter in the first place. In the second stage, voters choose between candidates under plurality rule. Voters are strategically rational, maximizing over expected payoffs, which are a function of the candidates' position. In addition, they assume voters have single peaked-preferences, there is a unique median voter's optimum and candidates have complete information on the preferences of all the other candidates and voters. Then the only pure strategy Nash equilibria are the ones in which: candidates enter solely at the median voter's optimum position; the

[9] The author recognizes the change of ideology for electoral purposes may be perceived by the public as a lack of integrity. Notwithstanding, he argues such situation is less prevalent, the goal of getting and keeping power would be more important in real experiences than serving a social group that values a certain doctrine.

number of politicians who enter is less than or equal to the benefit-cost ratio b/c[10]; voters vote sincerely, i.e., they select the representative closest to their own position on political spectrum[11].

Alesina (1988) finds divergent policies in a bipartisan dynamic model. Although he assimilates the argument that converging to the center can increase the changes of victory by capturing middle voters, there is a fundamental difference between his model and those discussed so far: candidates attribute utility not only to the benefits of holding office, but also to the proximity between the implemented policy and their own political position. He assumes voters know the objective function of the two parties and that policy space is one-dimensional. A period before the election, candidates declare the policy they will apply if elected. Voters choose their most preferred policy based upon rational expectations of those declared positions. In addition to admitting utility from the implemented policy, the crucial element of the model that may lead to policy divergence is considering the possibility of non-commitment, that is, the winner may enact a policy different from the one he or she has announced. Alesina's work then brings the distinction, between policy announced by the candidate, expected by the electorate and implemented. For a one-shot game, even when imposing commitment assumption, the model predicts some degree of divergence. Nevertheless, in such game, the political leader has no incentive to keep the campaign position. If commitment is not imposed, for any amount of office benefits, there will be a full divergence of policies, i.e., the elected representative will carry out exactly his or her favorite policy. Once rational voters anticipate this behavior, they will vote sincerely. In an infinitely repeated game, on the other hand, imposing commitment yields perfect convergence and, even without such assumption, equilibrium with high degree of convergence may arise, due to reputation costs. Deviating from the announced policy cause voters to predict non-cooperation in the following periods and to vote accordingly. An announced policy is credible if the incentive to deviate from it is smaller than the deviation cost. Therefore, besides the benefit-cost ratio of being in office and the discount factor, the credibility of a declared policy depends on the distance between it and the median voter's position[12].

Coining the term "citizen-candidate", Osborne and Slivinski (1996) predicts policy diver-

[10] Given that they also assume $b > c$, the number of candidates in the dispute is also greater than or equal to 1.

[11] Note that voting sincerely is a result of the model, not an assumption. The author highlights the importance of this feature of the model mentioning that, in multi-candidate poll, it is possible to expect better outcomes choosing the second choice than the first one.

[12] More precisely, the author shows that the credibility of a position depends on the probability of being elected given that voters expect each candidate to implement his or her bliss-point. If this probability is low for one party, it has little bargaining power and the most popular policy is distant from its preferred one. So, if such party announces a popular policy and is elected, the incentive to deviate is high. Cooperation is easier to sustain when those probabilities are balanced.

gence as well. In their model all citizens can choose to enter in the electoral competition. The number of candidates is, therefore, endogenous. After all citizens have simultaneously chosen whether or not to enter, they cast their vote. Citizens care about the policy executed. Policy space is the set of real numbers, agents have complete information and single-peaked preferences and there is an unique median position. Also by assumption, voting is sincere and the winner is committed to implement his or her preferred objective function (subject to the constraints of the office). Let b be the office benefits, c the costs of running and w and x be the favorite positions of the winner and of a citizen, respectively. Then the payoff of this citizen is: $-|w-x|$, if he or she does not enter, $b-c$, if wins, and $-|w-x|-c$, if looses. Their work explore results under plurality and run-off systems, I will focus on some results of the former[13]. The authors show, in any two-candidate equilibrium, politicians occupy symmetric positions around the median voter and they are neither identical nor too dispersed. If both candidates are identical, they share their supporters with equal probability. But then a third contender can enter, capture all the rest of the electorate and win for sure. Moreover, if the office benefit-cost ratio b/c is lower than 2, the divergence degree must be appreciable, otherwise either politician would withdraw, to avoid entry costs and still enjoy a policy similar to his or her favorite. In the case the two candidates are too dispersed, a third one positioned between then would obtain sufficiently many votes to win. Another interesting result involving divergence is that in any equilibrium, at most two politicians share a position. Finally, unlike Feddersen, Sened and Wright (1990), their model finds an equilibrium in which an agent with no chance of winning decides to enter. It occurs if he or she is capable of (and better off when) turning a certain victory of his or her lest preferred candidate into a lottery over the two original competitors.

Also bringing policy divergence results, Besley and Coate (1997) presents the last and more general model I comment here. Further, they investigate whether the equilibrium are socially efficient. The model is developed independently from the work of Osborne and Slivinski (1996), but they have in common the idea of citizens that value policies and can decide to enter in the electoral competition. Naturally, entering is costly. There is a first stage when entering decisions (possibly following mixed strategies) take place, a second one when citizens cast their votes and a third stage when the winner enacts his or her preferred policy (the authors explain that, in such scheme, the representative could not credibly commit to do otherwise). They bring new elements, though. The model accounts for differences in competence. Each candidate has a set of feasible policy, given his or her ability, besides institutional and technological constraints. Moreover, the model adds another dimension to the political spectrum, that is idiosyncratic taste for the identity of the political leader

[13] As seen in item 4.1, plurality rule is in force for the vast majority of Brazilian municipalities.

(e.g., utility from being in office oneself or admiring the charisma of other). Voters then maximize utility not solely over policies, but also over policy-makers. In their pure strategy two-candidate result, if office intrinsic benefits are zero, the two candidates ought to have sufficiently distinct positions. This divergence is though attenuated as those benefits grow. And, just as in Osborne and Slivinski (1996), a certainly loosing candidate may enter to affect poll results. Regarding efficiency, the authors show that, under identical policy-making abilities, the only dimension over which to choose is policy. And, since the winner applies his or her favorite policy, it is not possible to make anyone better off without making this representative worse off. Such scenario is, therefore, Pareto efficient. The same is not true for heterogeneous policy-making abilities, where social inefficiency can arise.

2.2 Personal characteristics affecting selection and governance

As with theory, applied literature does not have a definitive answer to the question of policy convergence or divergence. It is safe to say, nevertheless, that the degree of support for full convergence is far from sufficient to render the hypotheses of this work meaningless. Many studies find non-null impacts of party ideology or personal characteristics on political selection and policy-making. I present below a small fraction of the vast literature on the subject, trying to cover the features of greatest interest to researches, which seems to be ideology, dynastic traditions and gender. I also comment on educational level and, of course, race. Unlike the other characteristics, applied economics literature on race and political leadership is scarce. In addition to the papers on race that I cite below, I am only aware of works with a very limited sample and which are mainly interested in criminal justice[14].

According to the model of Alesina (1988), seen in the previews subsection, when there are incentives (reputation costs) to commit to the campaign position, the equilibrium will depend on the distance between the candidate's preferences and those of the median voter. When there are no commitment incentives, policy divergence is complete. Exploring Alesina's reasoning, Lee, Moretti and Butler (2004) argue that if a change in electoral strength (i.e., a distance variation between the candidate and the median voter) has no effect on policies, then there is evidence of complete divergence. In this case, an increase in strength of Democratic party in a district, as they exemplifies, should leave legislator's policies unchanged. In fact, that is what they observe. RD estimations on data from US House of Representatives (1946-1995) show no impact of electoral strength on policies. Miller (2008) aims to provide empirical evidence of the historical link between American women enfranchisement, in late 19th and early 20th centuries, and child survival. He applies difference-in-difference method on state-

[14] See Hopkins and McCabe (2012) for a review.

level data from 1900 to 1936 with rich spatial and temporal variation of women's suffrage enactments. He finds an increase by more than a third of public health expenditures and a decrease of 8% to 15% in infant mortality. The diseases that showed a decline were specifically those responsive to hygiene campaigns, such as diphtheria, diarrheal diseases and meningitis. This phenomenon would explain around 10% of the drop in infant mortality in the period. The year fixed-effect allows to capture responses within a term period. The fact that these responses are immediate, perceptible within a year, reveals the legislators reacts to the shift of the median voter. If this alone is not sufficient support of the median voter theorem, it is at least evidence against predictions of full policy divergence.

Pettersson-Lidbom (2008) looks for evidences of causal impact of party ideology on governance, in 288 Swedish municipalities over a 21-year period as of 2005. According to him, despite of the multiparty system, Sweden experiences a clear two-bloc division, into socialists and non-socialists. With a RD approach, he tests government revenues, current and capital expenditures, and, regarding performance, unemployment rate. They find left-wing parties spend between 2% and 3% more, as share of income, and exhibit 7% less unemployment then their right-wing counterparts. Part of the lower unemployment rate is due to 4% more public employees hired by left-wing parties. Ferreira and Gyourko (2009) evaluate party ideology influence on mayor's policy-making in the US, from a sample of 413 municipalities between 1950 and 2005. Their tests cover size of government (revenues and expenditures), categories of resource allocation and crime indexes. Unlike what is observed at the federal and state level, RD estimations do not return significant differences between Democrats and Republicans for any of these features. There is heterogeneity in that result, however. Cities with few geographically close municipalities exhibit non-null impacts. While in municipalities more likely to experience migration leaders use to assume more moderate positions. That being the case, the moderation of local leaders would follow a different dynamic from the competition for the city's median voter, as one would think under a Downsean model. On the contrary, more homogeneity among cities could be a result of the need to credibly commit to non-extremist positions, which matches citizen-candidate model[15].

Political skills and charisma can be transmitted from incumbents to their relatives and these attributes are independent from incumbency. The first to isolate the causal effect of holding office on the probability of having an elected relative in the future is Bó, Bó and Snyder (2009). They use RD design[16] on data of US legislative elections. Results show that longer tenure contributes to self-perpetuation in the form of political dynasties. Assets

[15] Recall that, according to model of Osborne and Slivinski (1996), discussed in the previews item, in a two-candidate equilibrium, policies are neither identical nor too dispersed.

[16] Alternatively, the authors use a regular instrumental variable. The instrument is the reelection probability (i.e., the rate of reelection) of the other legislators of the same party.

like name recognition and contacts may explain this relation. Bragança, Ferraz and Rios (2015) investigate the effect of dynasties on both selection and governance. They apply RD on elections from 2000 to 2012 in Brazil. The results indicate that holding office increases the probability of having an elected relative in the future by almost 60%. On governance, they find that dynastic political leaders spend more resources per capita, particularly in capital expenditures. Dynastic mayors focus on investment in urban infrastructure, health and sanitation. In these municipalities, however, greater GDP growth or better indicators in education, health and infrastructure are not observed. That is, higher expenses do not translate into better performance. When testing balances of the dynastic variable, there is no discontinuity in terms of age, education or occupation patterns; however, a dynastic politician would be much more likely to be a woman than a non-dynastic one. Daniele, Romarri and Vertier (2021) study the case of local dynasties in Italy, from 1985 to 2012. They got consistent results from panel fixed-effects and RD design: dynastic politicians are more likely to re-run and to win local and provincial elections; on average, expenditures and revenues are the same; although both dynastic and non-dynastic increase spending in pre-electoral year, the former rises to a greater degree, particularly capital spending.

Chattopadhyay and Duflo (2004) look for contrasts between men's and women's political preferences. They explore the random assignment of women to political office in two Indian districts, which aimed to increase female share in political positions. They constructed a proxy for the preferences of women and men from formal complaints brought by the population to the councils of these villages. In both districts, sanitation stood out among the main demands of women, for example. The authors show that women elected as leaders on the councils invest more in public goods directly related to the preferences associated with women. Brollo and Troiano (2016) contribute to this literature by examining the case of Brazilian mayors elected in 2000 and 2004. They use temporary employment in the local public administration as a proxy for patronage and a measure of corruption obtained from tax audits carried out by Brazilian authorities on a small random sample of municipalities. Their work suggests female mayors hire fewer temporary staff, engage less in corruption cases, are able to collect more discretionary transfers and perform better on health indicators. Notwithstanding, women tend to raise fewer campaign resources and are less likely to be reelected. Meier and Funk (2017) account not only for the election of women, but for the gender composition of the municipal civil service. They show having a woman mayor or a large percentage of women on the city council rise the presence of women in top-level public management positions, which, in turn, enlarge the overall number of women in public administration. This representation, at first symbolic, becomes active representation through the implementation of "women-friendly" policies, such as services to women victims of violence, daycare services and birthing centers.

Based on Archigos data set, Besley, Montalvo and Reynal-Querol (2011) built a database of world leaders from 1875 and 2004. With a fixed-effect estimation, they test the impact on economic growth of the identity of a politician, as well his or her educational level. Then, from 217 cases of power transitions due to natural death or terminal disease (i.e., supposed to be random), they check for heterogeneity effects on economic growth of the former leader's educational level. Estimations suggests the identity of the political leader matters, by significant leader fixed-effects and impressive negative impact of a random exit. Also, highly educated chiefs of governments are associated with larger economic growth, as both methods indicate. Rocha, Orellano and Bugarin (2018) assess the effect on fiscal indicators of mayors' education, past public experience and gender. They adopt RD strategy on Brazilian elections of 2000, 2004 and 2008. There is evidence that more educated and experienced political leaders spend a smaller fraction of the budget on current expenditures and, in particular, on personnel. In addition, more educated mayors are better at negotiating or are in a better position to obtain discretionary transfers. The authors do not continue the investigation regarding gender, because they find a significant imbalance between the educational levels of men and women.

Vogl (2014) investigate systematic advantages of Black candidates in close mayoral elections in the US, from 1965 to 2010. The country, especially the South, has experienced a historical process in which the black population is excluded from political participation. With the expansion of franchising in the 1960s, this Black population becomes a contingent of voters potentially engaged in Black candidacies, thus having a low mobilization cost. This constitutes an asset that rivals conventional resources in which White contenders usually have advantage, like wealth and clout. This faithful engagement makes Black turnout, which is observable, highly predictive of voting decisions. Thus, Black votes could be subject to strategic manipulation. From panel fixed-effects and RD estimations, the authors conclude that, in the South, close Black victories are associated to higher turnout and narrower winning margins and are considerably more likely than close White victories. Other regions do no exhibit these effects. Their results may cast uncertainty over the RD strategy, especially over works that explore the same context. That may be the case of Hopkins and McCabe (2012). They are interested in the relation between race and style of governance, just as one of the hypotheses I bring in this work. They also use RD design (in addition to a "differences-in-differences" approach), but in the US context, from 1972 to 2005. In agreement with Vogl (2014), the authors depict US interracial elections as highly polarized. In a systematic disconnect with such a scenario, however, campaign commitments are bad predictors of policies. Their set of variables covers basically expenditures categories and hiring. Except for the larger share of Black employees hired in security sector by Black mayors, policy-making does not differ. I explain in item 5.1 why the findings of Vogl (2014) do not concern me about the validity of the empirical strategy and in item 6.1 I compare my results with theirs and with those of Hopkins and McCabe (2012). As mentioned, Campos and Machado (2017) and Bueno and Dunning (2017) research the impact of race on political selection in Brazil. I comment on them in item 3.2, about race and political participation.

2.3 Hypotheses

The hypotheses tested in this work are organized into two groups, the ones regarding political selection and the ones related to governance. The first group is as following:

H-S1. Color/race Nonwhite is not a proxy for ideology.

H-S2. Nonwhite and White mayors are similar in terms of tangible and intangible personal resources.

H-S3. Nonwhite and White mayors obtain equal amounts of campaign resources.

H-S4. Nonwhite and White mayor candidates had the same rate of success in winning interracial elections in 2016.

H-S5. (Internal validation of RDD). Nonwhite and White mayors have imprecise control over their margin of victory.

H-S6. Nonwhite and White mayors had equal chances of reelection in 2020.

The second group of tested hypotheses, regarding governance, are given by:

H-G1. Nonwhite and White mayors are indistinguishable in terms of style of governance.

H-G2. Nonwhite and White mayors exhibit equal performances.

Note these statements can be seen as null hypotheses for the treatment Nonwhite color/race. In practice, however, some of them will be broken down into several null hypotheses. These tests are performed in section 6. I access, for example, whether Nonwhite and White mayors have different personal resources (H-S2) by looking for non-null treatment effects of a wide set of variables, including educational level, declared assets and occupation. Style of governance as well is portrayed by many measures, regarding expenditures, municipal programs and other variables subject to mayor's decision. As is characteristic in RD design, therefore, the hypotheses will be handled by multiple two-dimensional analyses. Figures 1 and 2, described further on, display the hypotheses and part of the relationship between them in diagram form. At the end of this subsection I present the contributions of this work to the literature.

If H-S1 is rejected, it means color/race and ideology may be too overlapped, in a way it would be difficult to know for sure whether any treatment effect attributed to race is actually due to ideology. Although this logic also applies to any other pre-treatment characteristic, since the left-wing spectrum is generally known for undertaking minorities demands, ideology is particularly important in this case. Some degree of correlation between race and ideology measures alone would not undermine our results. Hopkins and McCabe (2012), for example, who also use RD design, say Black candidates in the US are more likely to be Democrats than their White counterparts. However, if ideology strongly predicts race in Brazilian context, the results could be confounded with ideology, on which there is already abundant literature. Accessing differences in resources, by H-S2 and H-S3, is a way to investigate whether race can be associated to electoral advantage or disadvantage. H-S4 addresses to this issue directly and is answered by the McCrary's test. Item 5.1 discuss how extreme advantage in resources of one candidate may be related to control over the margin of victory. If that control is precise, i.e., if this candidate self-selects to treatment, RD design can not properly identify treatment effects. In such a scenario, we shall see a discontinuity in the density of the winning margin variable. Hence, rejecting H-S4 is a strong sign that H-S5 must be rejected. On that account, hypotheses H-S2 to H-S4 are relevant questions on their own right and, at the same time, they serve to validate the empirical strategy, together with balance checks of several mayoral and personal characteristics.

After validating the RD strategy, I move on to the hypotheses based on post-treatment variables, which are H-S6, H-G1 and H-G2. By exposing the racial issue in Brazil in section 3, I intend to show Nonwhite color/race is a meaningful treatment. That being so, finding non-null treatment effects when accessing those hypotheses (i.e., rejecting them) is tenable. A mechanism by which Nonwhite mayors could have different styles of governance (H-G1) is special commitment to the causes of Blacks, Indigenous, other minorities or poor people. Other than specific programs, these causes can guide expenditures. As a matter of course, divergent policies can lead to contrasting outcomes. But even if I find no distinguishable governance styles, I will check on performance indicators (H-G2) anyway, for two reasons: it is possible that my analysis of governance style was incomplete, having missed some pertinent feature on the topic, and there may be differences in competence. Explaining where differences in competence would arise or distinguishing the effects of competence from those of political preference are beyond the scope of this work[17]. Yet, I need not to assume ho-

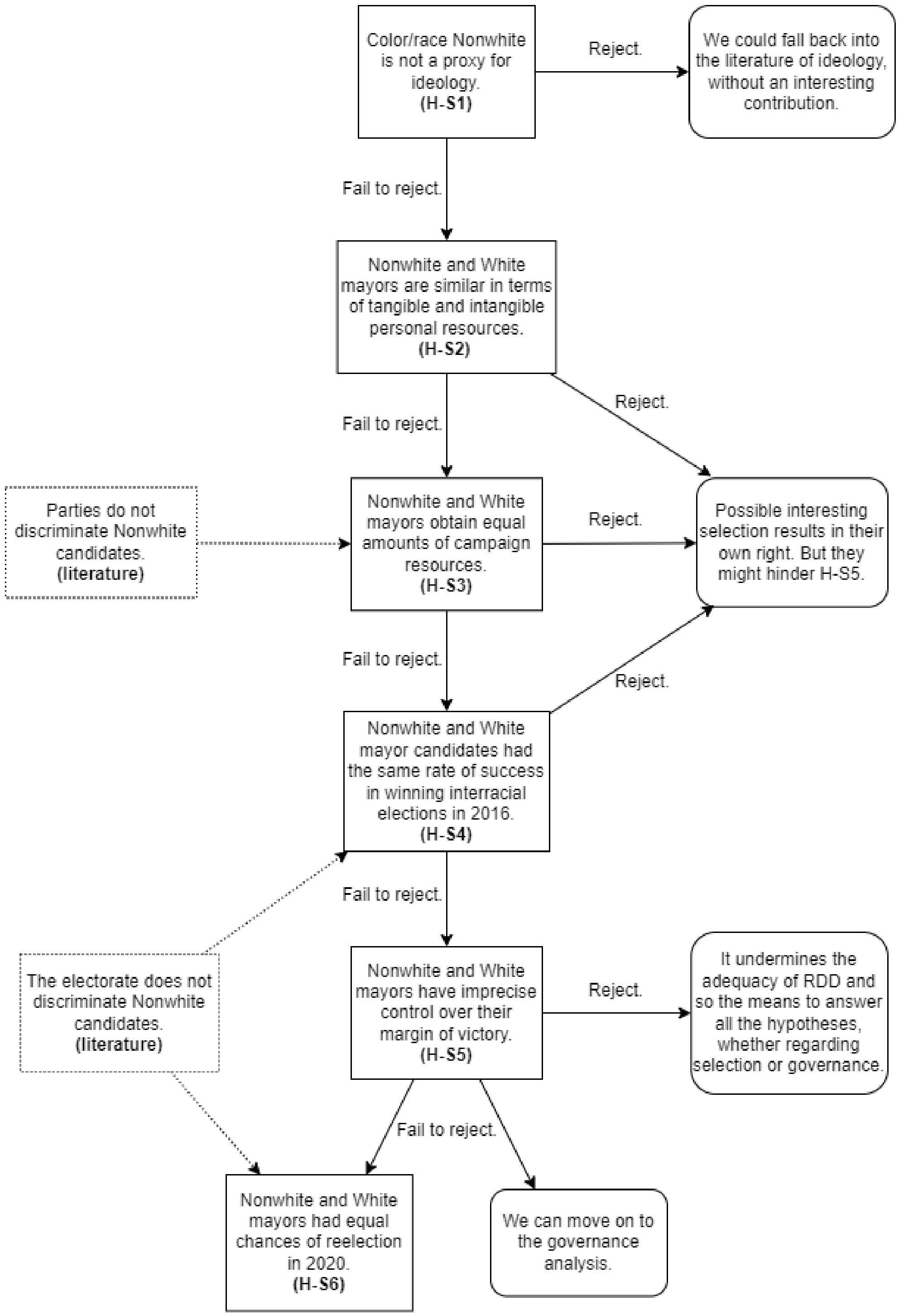
[17] I could, at most, speculate on the subject. A hypothetical better performance of a Black politicians could

mogeneous policy-making ability. Adhering to the assumption of Besley and Coate (1997), covered in item 2.1, I admit mayors are constrained not only by the obstacles inherent to the office, but by their political abilities. Thus each mayor has a different policy set. This is consistent, for example, with the case where two politicians have identical expenditures on education, but one achieves better indicators by managing to implement a more efficient policy. In respect of reelection chances (H-S6), it is safe to assume the influence of the electorate's perception of the results of the current term. In addition to the new campaign commitments, this perception is built into the expectation of future performance. Furthermore, I resort to Besley and Coate (1997) again to consider voters may have idiosyncratic taste for the identity of the candidate. It includes the race, which can motivate both racial prejudice and support from the Black community. According to Vogl (2014) and Hopkins and McCabe (2012) the latter occurs in the US. I will not directly test these factors.

The diagram of Figure 1 shows connections between the selection hypotheses. Depending on the evaluation of the hypothesis, it can lead to a possible result or open the way to proposing new problems. For instance, personal assets, related to H-S2, can be earmarked for campaign resources, referring to H-S3. Campaign resources, in turn, may bring electoral advantages, explored in H-S4. In addition, accessing H-S5 depends on these three previews assumptions. So it is natural the way the diagram arranges hypotheses H-S2 to H-S5. It is worth mentioning that, although these links illustrate relationships between the hypotheses, they do not necessarily mean direct implications. By the discussion in items 5.1 and 6.1, e.g., we see it is not enough to identify some difference in resources to conclude politicians have precise control over their margin of victory. Moreover, as mentioned above, the appreciation of H-S5 goes through balance checks of a wider set of variables than the one involving H-S2 and H-S3. The diagram also brings some hypotheses present in the literature, discussed in this section, that I will not test or not directly test. The diagram of Figure 2 follows the same logic of Figure 1 to show the hypotheses regarding governance.

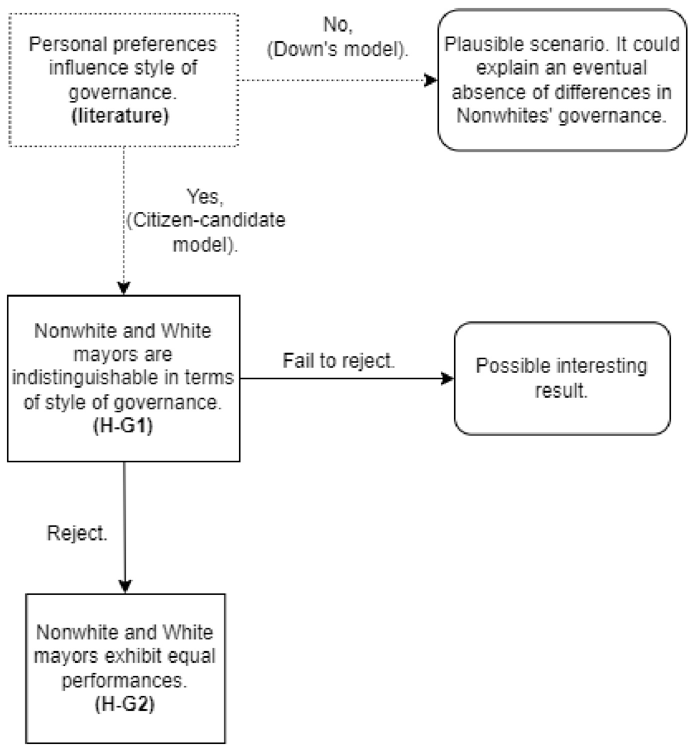
Except for campaign funding (H-S3), Vogl (2014) already address to my hypotheses regarding political selection. Hopkins and McCabe (2012) discuss race's ideological tendency (H-S1) and the balancing of winning margins (H-S4) and is mainly interested in style of governance (H-G1). Both works refer to the US context, though. In item 5.1, I defend political selection in Brazil probably exhibits different traits, which is confirmed by estimations in item 6.1. Moreover, my investigation on policy-making style considers features not covered by Hopkins and McCabe (2012), such as actions oriented to minorities. Last, although these

be that they suffer selection prejudice, whether to run or to be elected. So, on average, they would need to be more capable than Whites to acquire the same position. This is an interesting hypothesis I cannot test. Anyway, in item 3.2, we see the literature finds no evidence of racial prejudice in political selection in Brazil.



Note: Solid lines squares contain hypotheses that will be tested. Arrows indicate a hypothesis can lead to another hypothesis or to a possible result. Results are depicted by curved squares. Dotted lines squares contain hypotheses present in the literature which I discuss but not directly test; these hypotheses could also influence the phenomena represented by the squares to which they are linked.

Figure 1 – Reasoning of the hypotheses regarding selection



Note: Solid lines squares contain hypotheses that will be tested. Arrows indicate a hypothesis can lead to another hypothesis or to a possible result. Results are depicted by curved squares. Dotted lines squares contain hypotheses present in the literature which I discuss but not directly test; these hypotheses could also influence the phenomena represented by the squares to which they are linked.

Figure 2 – Reasoning of the hypotheses regarding governance

studies measure causal effect with RD estimation, they have smaller samples. Vogl's sample, the larger of the two, has 87 interracial elections, 46 within the bandwidth. Campos and Machado (2017) and Bueno and Dunning (2017) perform a broad analysis on Black political underrepresentation in Brazil, addressing all my hypotheses on political selection. However, there are two important distinctions between these works and mine when dealing with differences in ideology, resources and electoral competitiveness. First, they do so by methods not suited to causal inference. The former estimate by OLS with controls and the latter use mean differences[18]. Second, they explore the legislative branch. The racial composition of candidates, mentioned in item 3.2, suggests legislative and executive branches may have their peculiarities. From above, my work contributes to the literature concerning the effect of race

[18] Bueno and Dunning (2017) use an experimental design, but to check for electorate's racial prejudice. As well as a RD approach on mayors' terms, but to evaluate a possible mechanism of underrepresentation, which is institutional entry barriers, represented by the application or not of runoff rule in 88 municipalities.

on political selection and policy-making style by employing a method capable of capturing causal effect in a considerable sample of 1596 elections in Brazilian context. Additionally, as shown in item 4.2, I explore a wide range of variables, including important potential confounders (like ideology and dynastic status) and, notably, variables related to politicians' sensitivity to minorities and poor people. Finally, I am not aware of other applied economics study that appraise the impact of politicians' race on performance indicators (H-G2).

3 A glimpse into the racial issue in Brazil

3.1 Race relations and their paradigms of interpretation

Between 1560 and 1852 Brazil received about five million enslaved Africans. With the end of the slave trade, the manpower needed to export agriculture sector and the nascent industry comes from Europe, jettisoning the Nonwhite population. Between 1850 and 1932, Sao Paulo, the southern states and Rio de Janeiro received almost all of the approximately four million European immigrants who arrived in the country, predominantly Iberians and Italians. Among the motivations for this immigration policy were racist[19] principles reinforced by pseudoscientific theories of the 1870s generation. Bringing the European involved the desire to "whiten" the population, in order to dilute the African and Indigenous heritages, which were seen as under-civilized and associated with indolence for work. Such policy resulted in a pattern of geographic distribution in which a Nonwhite majority of the population remains in economically less dynamic regions and led to the marginalization of Nonwhites living in the Southeast. In the 1930s, with the end of the immigration process, the demand for labor in Southeastern cities that were experiencing greater economic growth generated a migratory flow of Nonwhite Brazilians to these cities, mainly from Minas Gerais, interior of Sao Paulo and Northeastern states. To a lesser extent, the South also received these workers. In such a scenario, there was fear of ethnic tensions that could threaten national unity and, guided by the aforementioned racist ideas, for the uniformity and the racial and cultural "quality" of the population. (GUIMARÃES, 2001; HASENBALG, 2005; OSÓRIO, 2021).

As described by Guimarães (2001), to solve this ethnic dilemma, an idea of nation was built based on what is called racial democracy or ethnic democracy. That line of thought believed Brazil had overcome the racial differences created in its formation process. They not only denied the existence of differences in biological capabilities between races (an advance in relation to pseudoscientific racism), but they also gainsaid the existence of segregation or prejudice between ethnicities. Observed inequalities would essentially be class inequalities, thus solely an effective competition of Nonwhites against Whites could cause prejudice and discrimination. Descendants of Europeans, Blacks, Indigenous and mestizos would form a

[19] See Osório (2009, ch. 2) for precise definitions of race, prejudice, racism and discrimination.

hybrid and mixed nation. The country would be able to "Brazilianize" cultural traditions of these different groups; only cultural manifestations that were not compatible with modernity, such as superstitions and animisms, were rejected (note how arbitrary it can be). Moreover, the very idea of "whitening" would reveal the strategy of incorporation (and not segregation) through the mixing of successful mestizos into the dominant stratum. Already in the 1920s, one can see a relationship between this imaginary and the modernist artistic experience of the meeting between high and popular culture. In the following decade, the concept of racial democracy would find support in the material field by the labor policy of Getúlio Vargas, which guaranteed rights to the industrial proletariat, irrespective of race. But it is above all in the Brazilian social thought of academia that this idealized Brazil were built. Apart from pseudoscientific racism, that school of thought constitutes the first paradigm of the racial issue. The following excerpt by Gilberto Freyre, one of its exponents, illustrates that paradigm:

> "The secret of Brazil's success in building a humane, Christian, and modern civilization in tropical America has been her genius for compromise. While the British, as no other people, have had this genius for compromise in the political sphere (...) the Brazilians have been successful in using this same power of compromise in the cultural and social spheres. Hence their ethnic democracy, the almost perfect equality of opportunity for all men regardless of race or color." (FREYRE, 1959, p. 7).

The idea of racial democracy served to partially alleviate the ethnic tensions of its time and contributed to a limited progress in the integration of the Nonwhite population into labor market. The Black movement at the time supported Vargas and, in the 1940s, the tradition of racial democracy was accepted as the model of anti-racism by a portion Black leaders (generally leaders in Rio de Janeiro, where interaction with the community of White intellectuals was greater). In the 1960s, however, prejudice began to be unmasked. Theorists have came to see prejudice would serve to legitimize the social order. The "myth of racial democracy" began to be overturned, giving place to this second paradigm of thought on the racial issue in Brazil. (GUIMARÃES, 2019; OSÓRIO, 2009). Among these theorists was Florestan Fernandes.

> "Manifestations of racial prejudice and discrimination have nothing to do with threats that may have been created by competition between Blacks and Whites, nor with the real or potential aggravation of racial tensions. They are pure and simple expressions of mechanisms that, literally, kept the past in the present, preserving racial inequality in the style that prevailed in the caste regime." (FERNANDES, 2013, p. 122, my translation)[20].

Note, however, that although this new conceiving denounces the existence of racism and racial prejudice, these are perceived as the "past in the present"; that is, as a legacy of

[20] From the original: "As manifestações de preconceito e de discriminação raciais nada têm que ver com ameaças porventura criadas pela concorrência ou pela competição do negro com o branco, nem com o agravamento real ou potencial das tensões raciais. Elas são expressões puras e simples de mecanismos que mantiveram, literalmente, o passado no presente, preservando a desigualdade racial ao estilo da que imperava no regime de castas."

the differences in the initial conditions of Nonwhite population's ancestors at the time of the abolition of slavery. They shared with the previous steam of thought the view that modernity and economic growth would bring about a natural transition from a caste society to a class society. In this sense, racism and prejudice would be archaisms. (OSÓRIO, 2009; MOTTA, 2000).

If the opportunities were in fact the same or practically the same for everyone, as predicted, economic growth would gradually make the inequalities between races to disappear. The overcoming of the interpretation of prejudice and racism as archaisms was achieved in the late 1970s, due to the observation that such a process had not occurred and by separating the effects of race from the contribution of other variables. Authors such as Valle Silva and Carlos Hasenbalg have shown that class inequalities are reinforced by racial discrimination. A third paradigm was formed on the racial issue, which is the current one. Controlling for characteristics such as residence in urban or rural area, urban or rural background and marital status, Silva (1978, p. 215) concludes "Whites are much more efficient in their conversion of experience and educational investments into monetary returns while Nonwhites suffer increasing disadvantages as they try to go up the social ladder." And, examining indicators of concentration of occupations, Hasenbalg finds that:

> "In the Southeast (...) the efforts made by Nonwhite people to cover a certain social distance were significantly greater than the efforts required of a white person (...). Whereas in manual industrial occupations, for example, qualifications seem to be more important than color as a criterion for admission to employment, in occupations that demand direct contact with the public or consumers, Black and Mestizos were excluded, not only owing to their lack of qualifications, but because they were seen as aesthetically undesirables." (HASENBALG, 2005, p. 183-184, my translation, my underline)[21].

In addition to inequalities in occupations, Hasenbalg (2005) also finds inequalities of opportunities in education. He estimates that about a third of racial inequalities at the basic level stem from the geographical division of racial groups[22], but the remaining two-thirds can be attributed to the effects of racial discrimination. Furthermore, it is clear that the exclusion of the Nonwhite population grows at higher educational levels.

Osório (2021) provides an overview of the evolution between 1986 and 2019 of racial inequality in income, probably the type of racial inequality that best identifies differences between groups. He shows that Blacks' income remains at about half that of Whites. There is

[21] From original: "No Sudeste (...) os esforços feitos por pessoas não-brancas para cobrir uma certa distância social fossem significativamente maiores que os esforços exigidos de uma pessoa branca (...). Enquanto nas ocupações industriais manuais, por exemplo, as qualificações parecem ser mais importantes que a cor como critério de admissão ao emprego, em ocupações que exigem contato direto com o público ou consumidores, os negros e mulatos foram excluídos, não apenas por sua falta de qualificações, mas porque eram vistos como esteticamente indesejáveis."

[22] Given the historic formation of the country, even after the internal migratory flow from the 1930s onwards, the Nonwhite population is still preponderant outside the South and Southeast, where access to education is worse.

only a slight reduction in income difference and this phenomenon may be due to the increase in the number of people from higher income groups who declared themselves Black (i.e., they previously declared themselves White, but started to perceive themselves as Black); this phenomenon transfers to Blacks part of the inequality previously associated with Whites. Without deviating from the current paradigm described above, he identifies two main factors that cause the maintenance of racial income inequality. First, social mobility, which is very dynamic, but short-ranged (that is, it is unlikely that people are far from their starting point of income, when they were between 15 and 19 years old). The author finds approximately 70% of intergenerational persistence of income. Hence, even if racial discrimination suddenly disappeared, such persistence would hold back racial inequalities in income for several decades. Notwithstanding, even slowed down by little social mobility, racial differences between incomes should disappear over time and they do not. It remains that the second factor is racial discrimination. Given the high intergenerational persistence, Osório recommends that policies aimed at the Nonwhite population must be associated with combating socioeconomic inequalities.

3.2 Race and political participation

In addition to the racial scenario described so far, to characterize the context in which a Nonwhite political leader could have different chances of re-election or different engagements than a White one, it is still interesting to briefly portray the evolution of the Black movement in Brazil. After the abolition of slavery until the beginning of the Vargas dictatorship (1889-1937), the Black movement was mainly concerned with guaranteeing material gains that would reverse the situation of marginalization in which the Black population found itself; thus, it was predominantly aligned with Vargas' fascist-like laborism. The creation in 1931 of the Brazilian Black Front (*Frente Negra Brasileira*) represents a leap in terms of political organization of the movement, but it was disbanded in 1937, as well as all political organizations. Together with the return of civil liberties, in 1945, a period of greater economic growth began, which gave a better economic insertion to Blacks. However, there was still a large part of the Black population marginalized and the fiercer competition in the markets intensified prejudices and stereotypes. Therefore, even though it did not have the binding capacity of the previous phase and those that would come after, in the period 1945-1964, the Black movement stayed alive and matured. The Union of Men of Color (*União dos Homens de Cor - UHC*), which had branches in ten states, and the Experimental Black Theater (*Teatro Experimental do Negro - TEN*), led by Abdias do Nascimento, stand out in this phase. Among the actions of such organizations were legal and health assistance services, publishing their own newspapers and participating in electoral campaigns. (GUIMARÃES,

2001; DOMINGUES, 2007).

A new military coup, in 1964, threw social movements, including Black movement, into clandestinity, or semi-clandestinity. When speaking about racism, Black leaders were accused of creating a supposedly non-existent problem. The political opening in 1978 marks the rebirth of the Black movement, with the Unified Black Movement (*Movimento Negro Unificado - MNU*). This time anti-racism was clearly identified with the left-wing and more connected to the student movement and other popular movements. In the internal field, it was influenced by authors such as Florestan Fernandes and in the external field, MNU was inspired by the Black's struggle for civil rights in the United States and by the processes of liberation in southern Africa. The keynote was to fight racism while challenging the current order in general, alongside the other oppressed sections of society. And, in an unprecedented way, the motto was preached: “Black power!”. Africans aesthetics standards (hair, clothes) was revived, as well as religions of African origin. Finally, another stage in the history of the Black movement from 2000 onwards can be pointed out, characterized mainly by the phenomenon of *hip-hop*. Although it does not have a defined political agenda and is not strictly racial, this movement speaks the language of the outskirts, differing from the vanguardism of traditional Black organizations. (IDEM).

Despite of Black mobilization, as mentioned in the Introduction, Nonwhite population is underrepresented in political office. Table 1 illustrates this phenomenon. Campos and Machado (2017) examines the underrepresentation of Nonwhite federal deputies in Brazil in 2014. The racial composition of deputies candidates are similar to racial population's composition. Hence, differently from what was observed in Table 1, the greater filter of deputies representation is located between candidacy and election, not between population data and candidacies. In an OLS model, controlling for social class[23], gender and education, there is no significant effect of the race variable on votes; gender and class would be more relevant than race. When using as dependent variable the resources obtained for campaign[24], he finds Nonwhite candidates are disadvantaged, averaging 8.9% less revenue. A correlation is found between the size of the party and the race of candidates; larger parties select or attract more White candidates than smaller parties. One explanation would be that larger parties can select supposedly more competitive candidates. One could not assert nevertheless that there is a bias in the selection of parties; some parties even show a preference for upper-class Nonwhites. The author concludes that selection criteria by party and class are the main determinants of underrepresentation.

By different methods, including an experiment in which subjects watched videotaped

[23] The author builds a proxy for the candidate's social class based on their professional occupation.

[24] According to the author, the campaign resources should not be included among the regressors of the previous model due to the high correlation of this variable with votes.

Table 1 – Comparing the distribution of color/race

	% brazilian population 2010	% candidates for mayor 2016	% elected mayors 2016
Asian	1.10	0.57	0.51
Pretos	7.52	3.17	1.68
Pardos	43.42	29.57	27.39
Indigenous	0.43	0.18	0.11
White	47.51	66.49	70.29
Not declared	0.02	0.02	0.02

Source: *IBGE*; *TSE*.

speeches performed by actors, Bueno and Dunning (2017) comes to some conclusions similar to Campos and Machado (2017). They find no effects of race over electorate preferences. It seems to be no systematic racial discrimination of deputies candidates from party elites. And institutional barriers, represented by electoral rules, cannot explain the smaller proportion of Nonwhite mayors. White deputies candidates, nonetheless, own a three times greater value in assets and acquire three and a half more campaign funds. The authors attribute such fact to elites' propensity to allocate resources to those who are already part of the economic elite, predominantly White. Socioeconomic inequities would explain underrepresentation persistence in a country without strong racial cleavages.

There are recent changes in rules that affects representativeness. In August 2020, the TSE analyzed the results of a public consultation organized by federal deputy Benedita da Silva and entities of the Black movement, such as the NGO Educafro. The court then ruled that, as of the 2022 elections, the division of the Party Fund and the Electoral Fund[25] made by a party, as well as free time on radio and TV, will have to be proportional to the number of Black candidates from that party. (BRASIL. Tribunal Superior Eleitoral, 2020). Although there is concern that the rule will be manipulated by parties, especially those on the right, to leverage candidacies from Black people not committed to minority agendas[26], that rule represents an advance in the representativeness in the legislative branch. Another important recent affirmative policy is Constitutional Amendment No. 111/2021, which establishes that votes for the Chamber of Deputies given to female or Black candidates will be counted double for the purpose of resource distribution of Party Fund and Electoral Fund in the elections

[25] For notions about the functioning of the Party Fund and the Electoral Fund, see TSE.

[26] The party that most elected congressmen who declare themselves Black in 2018 was the PSL, on the right and which typically takes a stand against affirmative racial policies. Such example matches Campos and Machado (2017), who asserts racial distribution of politicians appear not to follow party ideology orientations.

from 2022 to 2030. (BRASIL, 2021, art. 2)

4 Institutions and data

4.1 Brazilian political system

Brazil is a democratic presidential republic. The federal units, states, are headed by the governor and municipalities by the mayor. The election of a politician to an executive branch office automatically implies the election of the vice registered with him or her. The National Congress is bicameral, composed of the Federal Chamber and the Senate. There are also an state legislative, Legislative Assembly, and a Municipal Council. Executive branch politicians and senators are elected by simple majority of the valid votes, while federal deputies, state deputies and councilors are chosen by proportional system[27]. With the exception of senators, with an eight-year term, all these representatives serve a four-year term. However, whereas legislative representatives can be re-elected indefinitely, in the executive power only one consecutive re-election is allowed. President and governors are elected in the first round if the most voted obtained more than 50% of the votes; otherwise, a few weeks after the first poll, there is a second round between the two most voted candidates. The same second round rule (know as run-off system) is valid to candidates for mayor in cities with more than 200,000 voters. From the electoral data available, we can see that there have been local elections every four years regularly since 1992, alternating with general elections since 1994. There are elections every two years, therefore. Voting is mandatory for literate people between eighteen and seventy years old and it is optional for people over sixteen and under eighteen, for people over seventy and for illiterate. (BRASIL, 1988, arts. 14,27,28,29,34,44,46,77,82).

Throughout the electoral process, there are mechanisms to guarantee the security of the vote. Among these, stands out the use of the electronic ballot box, which has allowed, since 2000, elections to be fully computerized. The ballot box carries around 90 built-in security systems, which constitute several interlinked barriers, making fraud highly unlikely, especially in the very short time of data transmission. There are physical barriers, such as specific security components, as well as digital barriers, including, for instance, internal data scrambling, that eliminates the possibility of discovering which candidates a person voted for. Security is verified by a recorded auditing process with the presence of party representatives and by public security tests, which expose the ballot boxes, in a controlled environment, to any specialists who might want to test them. In addition, since 2008, the Electoral Court has been gradually implementing the biometric identification system. By October 2021, about

[27] Electoral Code (*Lei nº 4.737, de 15 de julho de 1965*) regulates the proportional system.

80% of the total of more than 146 million voters had their fingerprints registered through this system. (TSE, 2016; TSE, 2021a; TSE, 2021b). Besides the security issue, Fujiwara (2015) provides evidence that the ballot box has increased effective access to voting of people with lower levels of education, who had trouble writing on the paper ballot.

Brazilian Federal Constitution of 1988 granted municipalities better conditions to finance themselves while increasing the responsibilities of local administration. Municipalities collect taxes within their jurisdiction whereas they need to render a sort of essential services: mass-transportation, directly or by concession; programs of infant and elementary school education as well as health services, with technical and financial cooperation of the Union or the state; protection of local historic and cultural heritage, under the supervision of federal and state governments. (BRASIL, 1988, art. 30). However, although municipalities have own tax revenues, its amount is not sufficient to cover the need for expenses. Municipalities are highly dependent on federal and state transfers. According to the National Treasury database, commented in the next subsection, on average, municipal own tax revenue, mandatory transfers[28] and discretionary transfers represent, respectively, about %18, %46 and %36 of the total revenue in 2020. Larger cities have more revenue autonomy. Still, even if we select the 46 cities with more than 500,000 inhabitants, those percentages are around %45, %25 and %30.

4.2 Data sources and constructed proxies

Data of candidates' characteristics, election results, assets possession and campaign accountability are obtained from TSE. I classify as an interracial (White vs. Nonwhite) election the one in which there is one Nonwhite and one White between the two most voted candidates candidates, regardless of the existence of other candidates. Following Vogl (2014, p.104), the margin of victory is defined as the absolute difference between the votes of the top-two candidates divided by their sum[29]. Beside color/race, mayoral characteristics include age, gender, educational level and a dummy for second term. Adding up by candidate self-declared assets and campaign donations of respective databases provide total amounts of assets and cam-

[28] Mandatory transfers are mainly determined by the population, the inverse of state's per capita income and whether the municipality is state capital or not.

[29] In polls involving three or more candidates and where the votes are not highly concentrated on the top-two, this denominator is considerably less than 1, which contributes to an increase in the margin. This correction makes sense, though, because in theses cases the absolute differences (numerator) are expected to be smaller. Either way, opting for weighted or absolute margins in this work does not meaningfully affect regression results. With the exception of the variables Class size (in the model without covariates) and Metropolitan area (in the model with covariates), which have their p-value decreased to about 4.5% when using absolute margins, the statistically significant variables are exactly the same.

paign revenues. I also used measures constructed based on electoral data that aim to capture two features: ideology and whether or not the mayor belongs to a political dynasty.

Adapting the methodology of Campos and Machado (2017, p.131), I created a proxy for social class based on occupations categories. The goal is to enrich the characterization of the mayor's background, in addition to features such as personal assets and educational level. Those authors built five categories, managing to distinguish, for instance, small and large entrepreneurs. As the TSE data I was able to access does not have this level of detail, I have divided occupations into three categories: (i) "Low-class occupation" (rural, manual and domestic workers, artisans and office technicians with little training); (ii) "Middle-class occupation" (technicians with high specialization, artists, low and middle level public agents and merchants); (iii) "High-class occupation" (professionals with college degrees, high-ranking public officials, businessmen and politicians).

Power and Rodrigues-Silveira (2019) build a party's ideology score system derived mainly from Brazilian Legislative Survey. Each party receives a score ranging from -1 (most leftist) to 1 (most rightist). Using their scale, I assigned to the mayor his or her 2016 party score and called this variable "Party ideology". As political orientation may be an important confounding variable, I added an alternative ideology measure, based on data provided by Parlametria[30]. I utilized a survey of the times each federal deputy voted on the agendas put forward by the federal government according to the government's recommendation. It reflects how pro-government the parliamentarian has been. The average of the adherence of each party was assigned to each mayor elected by that party and served as a proxy for mayor's alignment with the federal government. I named this variable "Adherence rate". Although congressmen's votes are certainly related to the government's ability to obtain support and to the merits of each agenda, the adherence may represent not only agreement with the specific matters proposed by the government, but also the party's ideological alignment. This makes sense, above all, in a strongly polarized scenario and with a far right-wing government, like the current one[31]. Usual dummies for being part of the same coalition of the governor and president[32] were also tested[33].

[30] A project powered by private institutions and foundations that aims to furnish to civil society virtually inaccessible legislative data, commonly published in complex structures or in PDF-like formats.

[31] The distribution of adherence rate variable is basically characterized by two clusters near the extremes and nothing in the center. And those clusters match with the position in the left/right spectrum the parties are generally known to occupy.

[32] Except for President Bolsonaro's coalition, that was formed only by his own party and the vice president's party, which certainly did not sum up his support base. In fact, the president himself left the party from which he was elected. That variable would therefore make little sense.

[33] Some parties have changed their name and/or acronym and others were incorporated into other parties. Those cases were updated to 2020 names, which were necessary to make party names from 2014, 2016 and 2018 elections comparable.

My proxy for political dynasties, called "Dynastic", consists of searching for intersections between the surnames of the person elected in 2016 with the names of mayors elected in the four previous elections. It is in line with the literature. Brazilian names generally have the following structure: [Forename (eventually compound)] + [Surname of mother's family] + [Surname of father's or husband's family][34] + [An eventual indication the person has the same name of his father or grandfather]. I firstly try to extract the surname. For example, in "José Maria Rodrigues da Rocha Júnior", only "Rodrigues" and "Rocha" are surnames. "Maria" is the second part of a compound name, "da" is a connective and "Júnior" means he has the same name as his father. Eliminating those indicators and connectives is easy, there are a few possibilities. The second part of the compound forename, however, may be basically any word that could be used as forename. So, after cutting the first word, I eliminated any match with a list of 368 forenames. That is the list of all first words in the names of candidates' database itself with frequency greater than four, added with an arbitrary short list I thought might be common second parts of compound forenames. Next, I perform the search for intersections. Evidently, perfectly matching names are treated as "same person" and are disregarded[35]. Finally, in an attempt to avoid matching people who are not actually related, I replace with omissions all observations with any of the three most frequent surnames, "Silva", "Oliveira" and "Santos", which is about one fifth of the data.

Area and population estimates of municipalities are provided by IBGE, the last one through the system *SIDRA*. The same institution is responsible for the Demographic Census. Except for the area, all pre-treatment municipal characteristics indicated in Table A.2 as data from 2010 are obtained from microdata of individuals database and households database of 2010 edition of the Census. The sample is expanded to municipal level according to an integer weight they provide. The Gini index and the measures of salary gap are derived from Census' data. Gini index is computed for each municipality based on per capita household income. And racial and gender salary gaps are the respective coefficients in the regression of log salary on Nonwhite, female, educational level, occupation and region of the country[36]. Other than population estimates, municipal baseline variables of 2015 or 2016 are lagged outcomes, whose sources are presented below.

[34] When a woman marries, she can choose to adopt her husband's family surname as last name. A men can adopt his wife's surname, but it is uncommon.

[35] In fact, the condition I set for accusing "same person" is perfect match or same birth date. I decided that way because small differences between names of the same person in different years, due to registration errors, are not so rare. Such errors are certainly much more likely than the mayor and his or her predecessor being born in the same day.

[36] Under 5% significance level, Nonwhite coefficient is significant for about 67% of the municipalities and female coefficient is significant for 94% of them. By counting the number of distinct codes of occupation by municipality (which is at most 497), we see this categorical variable represent, on average, a loss of 5% of the degrees of freedom and never more than 23%.

GDP data is also provided by *IBGE*. Nominal municipal GDP is corrected by national GDP deflator. Municipal revenues and expenditures information is furnished by Secretary of National Treasury (*Secretaria do Tesouro Nacional*), through the system *Siconfi*. Infant mortality and live births, as well violence/security[37] data, are given by Ministry of Health (*Ministério da Saúde*), through the system *Datasus*. Class size, age delay and test scores are provided by Ministry of Education. The variables of privatization initiatives, proportion of temporary public employees and all variables related to minorities and inclusion (Table 7) are obtained from the research Municipalities Profile (*Perfil dos Municípios*), published by *IBGE*. Information about municipal registration for federal cash transfer programs are given by Ministry of Citizenship (*Ministério da Cidadania*).

5 Methodology

5.1 Identification strategy

The victory of a Nonwhite candidate is evidently an endogenous phenomenon. Poll's results are associated to a sort of baseline characteristics that may influence policy outcomes. As an exercise, suppose two interracial elections. One takes place in a municipality whose voters reject the Nonwhite candidate owing to his or her race, i.e., by racial prejudice. The second occurs in a municipality that elects the Nonwhite competitor. The second municipality probably have more racial equality and is more progressive than the former. And its racial equality and progressivism may be related to a better business environment and thus a better response to a mayor's investment. An eventual increase in municipal GDP might then be attributed to the quality of the investment decision, when is in fact due to the quality of the response, which is prior to treatment assignment and not fully observable[38]. Although in the real world those phenomena maintain a more complex relationship than the describe above, the example makes it clear that more than direct comparison between White and Nonwhite terms is necessary. I rely on RD design to attempt to identify the effect of a Nonwhite mayor's governance.

RD approach can be characterized by a quasi-experimental design in which the probability of treatment assignment varies discontinuously as a function of one or more underlying variables (HAHN; TODD; KLAAUW, 2001). The assignment variable is named forcing vari-

[37] Causes of death were selected in the "aggression" category, that adds the causes of death X85 to Y09 in the base, as well as "legal intervention and war operations", represented by causes Y35 and Y36. Those categories are "large groups" of the ICD (WHO's International Classification of Diseases).

[38] I tried to construct proxies to racial equality and political orientation, as described in the previews section, but they are, of course, imperfect measures and they would be far from covering all aspects that determine the response.

able (also running var. or rating var.). The known point of discontinuity is called cutoff (also threshold or cut-point). Lee and Lemieux (2010, p. 5) present a perspective that is different from the one of simple discontinuity at a threshold: "the RD design should perhaps be viewed as more of a description of a particular data generating process". As discussed below, under certain conditions, such process involves stochastic error of the assignment variable, which leads to local randomization. I will favor this last interpretation because it is more interesting when deliberating about internal and external validity of RD strategy.

In Sharp RD Design (SRD) individuals are assigned to treatment in a deterministic way, based on the threshold of the forcing variable. That is the case of elections. Municipalities that receive treatment are those whose Nonwhite mayor candidate obtained a positive winning margin. The forcing variable is the margin of victory and the cutoff is the zero difference in that margin.

Let $i = 1, ..., n$ be the index of municipalities of our sample, MV_i the margin of victory of Nonwhite candidate (in which 0 is the cutoff), $Y_i(1)$ the potential outcome with treatment and $Y_i(0)$ the potential outcome in the absence of treatment. In sharp RD design, treatment assignment rule is $Nonwhite_i = \mathbf{1}(MV_i{\geq}0)$, where $\mathbf{1}(\cdot)$ denotes the indicator function. The measure of interest is the difference of potential outcomes $E[Y_i(1){-}Y_i(0)]$. As seen in Lee and Lemieux (2010), since it is impossible to observe an individual in both scenarios, with and without treatment, $Y_i(0)$ and $Y_i(1)$ are never simultaneously available. RD approach then resorts to the idea of randomization near the threshold and looks for averages of $Y_i(1){-}Y_i(0)$ over a sub-sample in that interval. Formally, it wants to estimate the average treatment effect (ATE) at the cut-point, given by $\tau = E[Y_i(1){-}Y_i(0)|MV_i{=}0]$.

Consider the following hypotheses, which are equivalent to those stated by Hahn, Todd and Klaauw (2001): (i) $\lim_{MV\to 0^-} Nonwhite_i$ and $\lim_{MV\to 0^+} Nonwhite_i$ exist and are distinct (discontinuity of treatment variable); (ii) $E[Y_i(0)|MV_i{=}mv]$ and $E[Y_i(1)|MV_i{=}mv]$ (average expectations over potential outcomes) are continuous at the cut-point; (iii) $E[Nonwhite \cdot (Y_i(1){-}Y_i(0))|MV_i{=}mv] = E[Y_i(1){-}Y_i(0)|MV_i{=}mv] \cdot E[Nonwhite|MV_i{=}mv]$ with MV_i in a neighborhood $mv \pm h$ (near a given MV, differences between potential outcomes are independent of treatment dummy). The authors then show that τ is nonparametrically identified by

$$\tau = \mu^+ - \mu^- \,, \tag{1}$$

where $\mu(MV) := E[Y_i|MV_i{=}mv]$, $\mu^+ := \lim_{mv\to 0^+} \mu(MV)$ and $\mu^- := \lim_{mv\to 0^-} \mu(MV)$.

The first hypothesis is trivially satisfied in sharp design. The last one is termed conditional independence and, as explained by Hahn, Todd and Klaauw (2001), it means that individuals do not select into treatment by anticipating gains. If it is violated, we would see, in a neighborhood of the cutoff, average potential outcome curves closer together on one side of the cutoff than on the other side. The second hypothesis, continuity of potential outcomes, is emphasized by the authors as essential to the results. Second and third assumptions are associated with the control over assignment, discussed below.

Lee (2008) derives a bit different expression for average treatment effect τ than the presented in equation (1). He weights individuals treatment effect by the term $f(0|w)/f(0)$, where f denotes a density function and 0 refers to the threshold. Rather than just applying to the cut-point or to a tiny neighborhood of it, τ is interpreted as a **weighted** average treatment effect for the whole population, in which the weight is proportional to the likelihood of being at the cutoff[39]. Despite distinct expressions, in practice, Hahn, Todd and Klaauw (2001) propose a weighted local regression, exposed in the next subsection.

The forcing variable is affected by individual's features and behavior, as well as by random chance. So each subject will have a different and unobserved chance of being treated. Let W be the observable variables that can influence both, the outcome variable Y and the assignment variable MV, while U and V refer to the omitted factors that affect, respectively, Y and MV. Thus Y is completely determined by the pair (W, U) and V dictate how much control subjects will have over their winning margin.

> **Imprecise Control:** Individuals are said to have *imprecise control* over MV when, conditional on $(W{=}w, U{=}u)$, the density of V (and so of MV) is continuous.

By Bayes's Rule, it can be shown that, if subjects have imprecise control over MV, then $P(W{=}w, U{=}u|MV{=}mv)$ is continuous in mv. In other words, all variables prior to assignment, observable or not, will have identical distributions on either side of $MV{=}0$ in the limit, as smaller and smaller bandwidths around the cut-point are accessed. Consequently, in a neighborhood of the cutoff, all baseline variables will be independent of treatment status, their differences will not be confounded by omitted factors. This attribute characterizes **local randomization**. In short, RD design adds variation in the forcing variable, in a way that, near the threshold, RD is as good as randomized experiments. (LEE, 2008; LEE; LEMIEUX, 2010).

Moreover, once potential outcomes $Y(0)$ and $Y(1)$ are functions of $(W{=}w, U{=}u)$, the continuity of the density of MV conditional on $(W{=}w, U{=}u)$ implies that the densities of $Y(0)$ and $Y(1)$ conditional on MV are continuous in mv; that is, imprecise control also guarantees the continuity assumption essential to derive the treatment effect in equation (1). (LEE; LEMIEUX, 2010).

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[39] As put by Imbens and Lemieux (2008), expected outcome given MV would be: $\mu(MV) = E[Y_i|MV_i{=}mv] = E[Y_i|Nonwhite_i{=}0, MV_i{=}mv] \cdot P(Nonwhite_i{=}0|MV_i{=}mv) + E[Y_i|Nonwhite_i{=}1, MV_i{=}mv] \cdot P(Nonwhite_i{=}1|MV_i{=}mv)$.

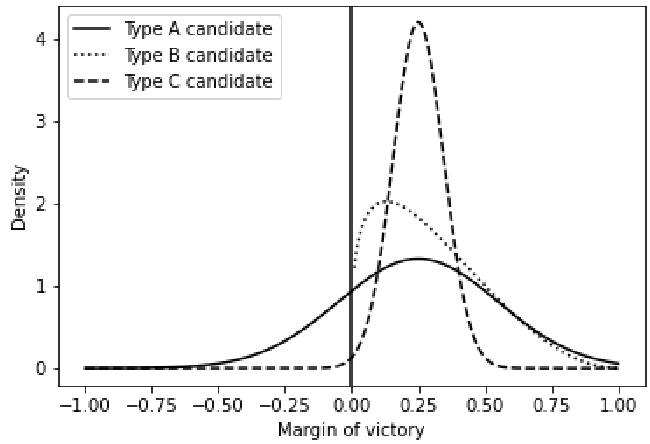

Figure 3 – Precise and imprecise control over the assignment variable

Let us briefly explore this key hypothesis of imprecise control by examining Figure 3, inspired by Jacob et al. (2012) and Lee and Lemieux (2010). It depicts three hypothetical types of Nonwhite candidates, with distinct distributions conditional on $(W = w, U = u)$ of MV. Type A has imprecise control over MV and has considerable probability of being near the cut-point, on either side. In order to apply RD, we expect this type of candidate to be the most representative of the population. Type B, although not having have much control over the margin he or she wins, manages to perfectly avoid defeat. That means self-selection to treatment and compromises the **internal validity** of RD approach. The distribution of Type C has the same mean of Type A, but with far less variation. While Type C meets imprecise control, as defined above, there is a much smaller degree of random error and he or she will hardly loose. Imagine a population with half of individuals similar to Type A and the other half similar to Type C. In this case, despite equal proportions of types, the population of a small vicinity of the cutoff will be composed almost entirely of Type A. Such deceptive homogeneity hinders the generalization of RD results, i.e., its **external validity**. The chance to include in the analyses a candidate with low density at the threshold, like Type C[40], or assign to him or her a meaningful weight, will depend on estimation parameters, investigated in the next subsection.

Together with a theoretical essay, it happens that Lee (2008) examines the applicability of RD design to the case of elections. He claims that, even though political actors naturally exert influence over their chance of victory, in a large poll the exact number of votes is affected by factors beyond control of those actors. Uncertainties remain over the vote count

[40] Note that Type C's likelihood at the cut-point could be equally low if his or her distribution had less variation but higher mean.

even on election day. If results are hard to predict, they are even harder to manipulate. For this reason, Type C candidate depicted in Figure 3 might not be much representative. A highly accurate control over final result would demand either violation of voter's liberty to choose or, assuming that this is a work of persuasion of each voter, a degree of coordination of agents that is impracticable in large groups. Type B candidate, the one that can assure victory, may represent this case of coercion of voters or fraud. From what was exposed in item 4.1, it appears to be an implausible scenario in Brazil.

Eggers et al. (2015) examine incumbency effects and reinforce the idea that imprecise control over the winning margin is not a strong assumption. According to them, a casual relation between an unbalance of incumbent victory and strategic campaigning requires the incumbent to react to small variations across the expected election result. The incumbent would need to have access to accurate polling intend. When foreseeing a close defeat, and only then, he or she would deploy extra resources capable of reversing election outcomes (the opponent, of course, would have less information or resources). If information or actions were not precise we would not see a discontinuity. Note how unlikely it seems to be the scenario proposed by the authors of a politician who manages to reverse results if he or she expects to get 49.9% of the votes, but does not apply any extra effort at 49.7%, because he or she precisely knows such outcome is irreversible. The authors explore long period databases of several countries, including Brazil, and find significant results only for the post-war US, which they then attribute to chance. Hyytinen et al. (2018) also access RD design validity by testing incumbency effects and comparing its results with the outcomes of an experimental design. They utilize Finnish data of voting ties, that were decided by lottery. As expected, the experiment shows no influence of incumbency status over winning chance. Although conventional local polynomial regression exhibits significant positive incumbency effects, bias-corrected and robust estimation developed by Calonico, Cattaneo and Titiunik (2014b) matches experimental results. As discussed in the next item, that is the algorithm I will employ.

The evidences of sorting in interracial elections in the US found by Vogl (2014), commented in item 2.3, do not seem to be generalizable for Brazil. The mechanism of manipulation the author suggests depends on the observability of turnout and disproportionately low mobilization cost of the Black electorate. As discussed in item 3.2, the literature (Campos and Machado (2017), Bueno and Dunning (2017)) suggests such mobilization cost advantage is not present in Brazil, because neither prejudice nor special support from voters are associated to Black candidates. Second and more importantly, given that voting is mandatory and absenteeism is low[41], the absence of the turnout factor in Brazil is almost certain.

[41] On the report of TSE (2021a), the average abstention in the 2014, 2016 and 2018 elections is 19.1%.

From above, imprecise control over polls, which leads to local randomization, seems to be a reasonable assumption and hence RD must be an appropriate technique. Furthermore, RD design exploring elections is widely adopted by the literature, as most of the works cited in item 2.2, including for Brazilian context. Still, empirical tests of imprecise control hypotheses are needed to sustain any causal inference. Whereas extreme cases such as Type B candidate probably do not exist, there is no guarantee we could not find a less extreme discontinuity in assignment variable. In item 6.1, I will examine the density of MV, seeking for signs that Type B-esque or C-esque individuals represent the population. I will also defy local randomization by performing balance checks of a myriad of baseline characteristics.

5.2 Estimation

The first choice to be made in RD design is between a parametric and nonparametric regression. The first one aim to find the right model (e.g., the best degree of a polynomial) to fit a given data set, in general using all available data[42], which is why it is also called global strategy. Nonparametric approach seeks, in a data-driven fashion, for the best subsample (bandwidth around a data point) to fit a given model, most commonly a linear one. Because it restricts the analysis to this range, it is also named local strategy. Since, of course, RD design intends to infer relations out-of-sample, it faces the trade-off between bias and variance. Provided that parametric estimation covers all observations, it will probably be more precise, that represents greater statistical power. However, the difficulty to specify over the whole data a correct functional form potentially gives rise to a larger bias. On the other hand, the smaller sample covered by local strategy will presumably offer less bias, at the cost of greater variance. (JACOB et al., 2012).

I choose for main specification a local linear regression (LLR) performed by the algorithm developed by Calonico, Cattaneo and Titiunik (2014a), Calonico et al. (2017). In using LLR method for RD design, these authors adopt a tradition initiated by Hahn, Todd and Klaauw (2001) and later seen in Imbens and Lemieux (2008) and Imbens and Kalyanaraman (2012). For RD applications, a regression is performed on each side of the threshold, in a way that the good boundary properties of the LLR proposed by Fan and Gijbels (1996, p. 4)[43] are specially opportune. They apply to the quadratic errors a kernel function, in order to down-weight the contributions of data point away from the center. In RD case, the center

Whereas, according to US Census Bureau (2022), 61.4% of American citizen voting-age population reported voting in 2016, 61.8% in 2012.

[42] Bragança, Ferraz and Rios (2015), for example, restrict their alternative parametric specification to observations with margins of victory between -0.5 and 0.5.

[43] Fan and Gijbels (1996, p. 19) also generalizes the linear model to any class of function that can be locally approximated by Taylor's expansion and build a polynomial regression.

is the threshold (here, in particular, the expressions are centered at cutoff equal 0). The conventional estimates for μ^+ and μ^- in equation (1) are the estimated intercepts below:

$$\begin{aligned} \hat{\tau} &= \hat{\mu}^+ - \hat{\mu}^- = \hat{\beta}_0^+ - \hat{\beta}_0^- \,, \\ \hat{\beta}^+ &= (\hat{\beta}_0^+, \hat{\beta}_1^+)' = \operatorname*{argmin}_{\beta_0,\beta_1 \in \mathbb{R}} \sum_{i=1}^{n} \mathbf{1}(MV_i \geq 0)(Y_i - \beta_0 - X_i\beta_1)^2 K(MV_i/h_n)\,, \\ \hat{\beta}^- &= (\hat{\beta}_0^-, \hat{\beta}_1^-)' = \operatorname*{argmin}_{\beta_0,\beta_1 \in \mathbb{R}} \sum_{i=1}^{n} \mathbf{1}(MV_i < 0)(Y_i - \beta_0 - X_i\beta_1)^2 K(MV_i/h_n)\,, \end{aligned} \tag{2}$$

where h_n is a sequence of positive bandwidths, X_i is the matrix with MV_i eventually added with columns of other covariates and K is an unimodal non-negative kernel function with support $[-1, 1]$ [44]. A local regression with a non-uniform kernel is pretty much aligned to the view of the gap at the threshold as a weighted average treatment effect, covered in the previous subsection. Fan and Gijbels (1996, p. 90) shows triangular kernels are optimal to boundery estimation. Calonico, Cattaneo and Titiunik (2014b) utilize triangular kernel as the default one. In addition , among other mild hypotheses, they constrain the application of (2) by the assumption of continuity of the density function of MV at the threshold. This property ensures that there will be observations in an arbitrarily small vicinity of the threshold. By the discussion in the previews subsection, we know it is reasonable if the population have, in general, imprecise control over MV.

Both need to be accomplished: dealing with the trade-off between bias and variance needed to bandwidth selection and constructing a confidence interval. The validity of the conventional distributional approximation $(\hat{\tau} - \tau) \cdot V^{-1/2} \xrightarrow{d} N(0, 1)$, where V is the conditional variance, depends on the condition $nh_n^5 \to 0$, which removes the effect of the leading bias. If that approximation is invalid, resulting confidence intervals will not have correct empirical coverage. Even in the simplified case of known V, common methods such as plug-in and cross-validation produce too large bandwidths, that means h_n is not small enough to hold $nh_n^5 \to 0$. Minimizing the asymptotic mean square error (MSE) of $\hat{\tau}$ generates an optimal plug-in bandwidth that, by construction, yields $nh^5 \to C > 0$ and the low convergence rate of cross-validation usually gives too large h_n as well. Those too large h_n may systematically lead to over-rejecting a null ATE. A starting point to overcome those issues is a bias-correction procedure. (CALONICO; CATTANEO; TITIUNIK, 2014b).

For now, consider Y_i and X_i such that $MV_i \geq 0$. (2) is a weighted lest squares problem, whose well-known solution is $\hat{\beta}^+ = (X'WX)^{-1}X'WY$, where W=$diag\{K(X_i/h_n)\}$. Theoretical condition bias is $E[\hat{\beta}^+|X] = \beta^+ + (X'WX)^{-1}X'Wr^+$, with unknown quantity

[44] Since all parameters $(\hat{\tau}, \hat{\mu}^+, \beta_0^+, ...)$ depend on the bandwidth choice, they could be represented as functions of h_n, which I omitted for simplicity.

$r^+ = \mu(x) - X\hat{\beta}^+$, where $\mu(x)$ is the regression function, i.e., $\mu(x) = E[Y|X{=}x]$. Fan and Gijbels (1996, p. 113-5) approximate r^+ using a Taylor's expansion of degree two around the center. i^{th} term of r^+ is $r_i^+ \approx \beta_2^2 X_i^2 + \beta_3^3 X_i^3$, where the coefficients β_2 and β_3 refer, respectively, to the second and third degree terms of a polynomial model. That means the authors consider the contribution of two more degrees of complexity, omitted in the original model, as a suitable approximation of r^+. β_2 and β_3 are estimated by fitting locally a third degree polynomial[45]. Provided that the kernel depends on the bandwidth, such fitting request a pilot bandwidth; it is selected by a Residual Squares Criterion, that includes and estimation of residual sum of squares[46]. Finally, replacing estimated r^+ in conditional bias expression gives the estimator for conditional bias of $\hat{\beta}^+$. Calculations for $\hat{\beta}^-$ are analogous.

Calonico, Cattaneo and Titiunik (2014b) follow the technique of Fan and Gijbels (1996) to find a bias-corrected estimator term for $\hat{\tau}$, denoted by $\hat{\tau}_{bc}$. With a bias estimator in hand, it still remains to obtain a proper conditional variance estimator to be able to select MSE-optimal h_n for the local linear estimator and to construct a robust[47] confidence interval. Nevertheless, since the bias estimator involves finding some higher order derivatives of the regression function[48], variance estimation procedure must appreciate that in general both $\hat{\tau}$ and its bias estimator contribute to the asymptotic variance. That is where the contribution of Calonico, Cattaneo and Titiunik (2014b) comes in. They propose combining bias-correction with a new expression for the conditional variance, that accounts for this additional variability.

Denote the pilot bandwidth by b_n and define $V_{bc} \coloneqq V + C_{bc}$, where V is the infeasible conditional variance and C_{bc} a correction term regarding the additional variability introduced by the bias estimator. Calonico, Cattaneo and Titiunik (2014b) show that, under $h_n/b_n \to \rho \in [0, \infty]$ (that implies bias-correction term may be asymptotically non-negligible) and other regularity conditions, the large-sample distributional approximation $(\hat{\tau}_{bc} - \tau) \cdot V_{bc}^{-1/2} \xrightarrow{d} N(0,1)$ applies. The authors explain that, to be Gaussian, distributional approximations that count on $\hat{\tau}_{bc}$ but does not consider C_{bc} depend on the more restrictive condition $h_n/b_n \to 0$, which makes the variability associated to bias-correction to disappear asymptotically. However, since that assumption is never satisfied in finite samples, bias-correction alone delivers poor performance in applications.

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45 Actually, author's presentation is general for a q^{th} degree expansion of r^+ and a polynomial fit of degree $p{+}q$. They say $q{=}2$ is appropriate, because it is nearly $\sqrt{n}$-consistent, while entailing moderate computational cost. And I set $p{=}1$, once here we are dealing with a linear model. When working with polynomial models with $p{\geq}2$, the authors also promote a small modification, which aims to eliminate the collinearity effect between terms such as X^2 and X^4.

46 See Fan and Gijbels (1996, p. 118-20).

47 Robust to large h_n choices, which, as mentioned above, is not the case of conventional CI.

48 Recall that r^+ is estimated using a higher degree Taylor's expansion.

Their estimator of V_{bc}, denoted by $\hat{V}_{bc}$, is built on nearest-neighbor estimators. Define

$$\begin{aligned}
\hat{\Psi}_{+,\,p,\,q} &= \sum_{i=1}^{n} \mathbf{1}(MV_i \geq 0) K(MV_i/h_n) K(MV_i/b_n) \\
&\quad \times r_p(X_i/h_n) r_q(X_i/h_n)' \hat{\sigma}_+^2(X_i)/n\,, \\
\hat{\Psi}_{-,\,p,\,q} &= \sum_{i=1}^{n} \mathbf{1}(MV_i < 0) K(MV_i/h_n) K(MV_i/b_n) \\
&\quad \times r_p(X_i/h_n) r_q(X_i/h_n)' \hat{\sigma}_-^2(X_i)/n\,,
\end{aligned} \tag{3}$$

with

$$\begin{aligned}
\hat{\sigma}_+^2(X_i) &= \mathbf{1}(MV_i \geq 0) \frac{J}{J+1} \left(Y_i - \sum_{j=1}^{J} Y_{\ell_{+,\,j}(i)}/J \right)^2, \\
\hat{\sigma}_-^2(X_i) &= \mathbf{1}(MV_i < 0) \frac{J}{J+1} \left(Y_i - \sum_{j=1}^{J} Y_{\ell_{-,\,j}(i)}/J \right)^2,
\end{aligned}$$

where $r_p(x) = (1, x, ..., x^p)'$ (i.e., the transpose of the vector resulting from transformations of x into a constant and higher order degree terms), $\ell_{+,j}(i)$ is the jth nearest observation to the observation i such that $MV_i \geq 0$ and $\ell_{-,j}(i)$ is the analogous for $MV_i < 0$. $\hat{V}_{bc}$ is then obtained using $\hat{\Psi}_{+,1,1}$, $\hat{\Psi}_{+,1,2}$, $\hat{\Psi}_{+,2,1}$, $\hat{\Psi}_{+,2,2}$, $\hat{\Psi}_{-,1,1}$, $\hat{\Psi}_{-,1,2}$, $\hat{\Psi}_{-,2,1}$ and $\hat{\Psi}_{-,2,2}$. I will omit the exact expressions due to their cumbersome notation. Following the parameter of authors' simulations, I will set $J = 3$. Finally, the bias-corrected robust confidence interval is given by $\hat{\tau}_{bc} \pm \Phi^{-1}_{1-\alpha/2}\sqrt{\hat{V}_{bc}}$. From a visual perspective, the conventional point estimate is recentered by the bias estimator and its confidence interval is rescaled by the variability correction that $\hat{V}_{bc}$ yields.

Calonico, Cattaneo and Farrell (2020) show that, although valid, the MSE-optimal procedure described above is suboptimal for the construction of robust bias-corrected confidence intervals. They propose CE-optimal robust bias-corrected estimators, where "CE" stands for coverage error. Its design provide a faster coverage error decay rate. The authors recommend using CE-optimal for interval estimation and MSE-optimal for point estimation. Since the two methods give almost the same statistically significant outcomes for my database[49], I employ MSE-optimal for both interval and point estimation.

Jacob et al. (2012) and Lee and Lemieux (2010) assert that, although RD design does not require covariates, introducing them improves estimation precision. In my database, however, omissions in covariates cause the sample to be reduced by about a third, potentially

[49] Apart from Growth GDPpc, that is not is not significant with CE-optimal estimator, all the others significant variables at 5% significance are the same for both methods. They are shown in the next section.

decreasing precision. Rather than choosing between using covariates or not, I will present the main specification, LLR, with and without covariates. Furthermore, following those authors' recommendations, I will conduct sensitivity analysis with a parametric approach. They suggest a F-test when choosing the degree of a parametric model. Based on their orientation, I divide the margin of victory in K parts according to a given binwidth[50] and create indicators for each bin. I build a "restricted" model adding these indicators to the original "unrestricted" model and perform the F-test. A not statistically significant F-statistics mean that the bins do not provide additional information and suggests the original model is not underspecified. I start with the linear model and repeat the procedures increasing the complexity (so the variance) of the specification. The simplest model whose F-statistic is not significantly is chosen.

The McCrary's test, introduced by McCrary (2008), is performed by the algorithm of Calonico, Cattaneo and Farrell (2018). Finally, I check on the capability of the sample to detect treatment effects by calculating the power of the test. To do so, I use the package provided by Cattaneo, Titiunik and Vazquez-Bare (2019), which performs the same bias correction and robust estimation of Calonico, Cattaneo and Titiunik (2014a). The algorithm requires as argument the treatment effect under the alternative at which the power function is evaluated. I obtained such argument by averaging the magnitude (in terms of standard deviation) of the significant coefficients in similar works of the literature and multiplying it by the standard deviation of each dependent variable.

6 Results

Before going for balance checks and outcomes let us discuss the suitability of using linear models for this dataset. Table 2 shows the F-tests for choosing model's complexity in the parametric alternative approach, as explained in the previews section. As put by Lee and Lemieux (2010), LLR is supported by the argument that, around the cutoff the linear approximation is less likely to deliver large biases. Nevertheless, I have also availed this F-test to speculate about using polynomial local models in place of a linear local model. I run the same tests, but restricting the data to the bandwidth that was chosen by the non-parametric approach. That is not directly testing LLR, which involves weighing by a kernel function, but it may shed some light over the relationship between the running variable and the dependent variables near the threshold. Table 2 suggests that the linear approximation, both over the whole data and locally, is the most adequate. Such interpretation is reinforced by observing

[50] Binwidth seleciton is presented by Jacob et al. (2012, p. 13-15). As it will be shown in Table 2, I perform the F-test for the binwidth I obtained from their procedure, which is 1/32=0.03125 (the range of Margin of victory over 32), and two more, 1/16 and 1/64.

the graphs presented ahead. Therefore, although I show sensitivity results of third degree parametric and nonparametric models in Tables A.6 and A.7, I argue they are overspecified.

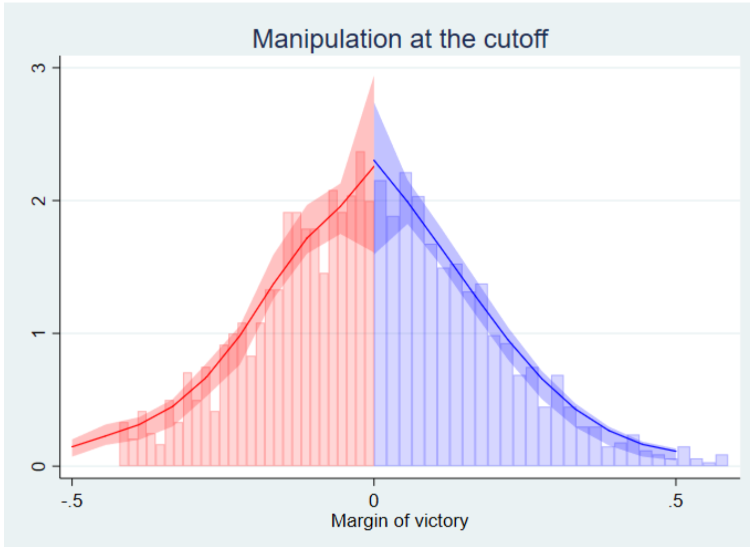
Table 2 – F-tests of model specification

	No covariates			With covariates		
Binwidth	0.0625	0.03125	0.015625	0.0625	0.03125	0.015625
Whole data						
Linear	75.3%	77.6%	81.2%	84.7%	87.1%	83.5%
Linear and interaction	12.9%	10.6%	7.1%	1.2%	3.5%	
Quadratic	1.2%			1.2%		
Quadratic and interaction	1.2%	1.2%		3.5%		2.4%
Cubic						
Cubic and interaction		1.2%	1.2%	1.2%	1.2%	3.5%
None of the previews	9.4%	9.4%	10.6%	8.2%	8.2%	10.6%
Locallly						
Linear	90.6%	92.9%	98.8%	90.6%	92.9%	98.8%
Quadratic	8.2%	5.9%		8.2%	5.9%	
Cubic	1.2%	1.2%		1.2%	1.2%	
None of the previews			1.2%			1.2%

Notes: Proportions of dependent variables according to the model chosen by the F-tests proposed by Lee and Lemieux (2010). They include three different binwidths and both options, with and without adding covariates to the models (covariates list is indicated in Table A.2). "Whole data" means the tests include all obs., while "Locally" means the test of each dependent variable is constrained to its local regression bandwidth. Considered 5% significance level for all tests. Treatment variable is color/race Nonwhite. Tests consider the whole set of dependent variables of this work, listed in Tables A.2 and A.3.

6.1 Political selection outcomes and search for evidences of sorting

This subsection aims to answer all the hypotheses related to political selection, listed in item 2.3, aside from the evaluation of reelection chances, shown in the next item. It explores, therefore, all pre-treatment variables. As presented in that item, hypotheses H-S2 to H-S4, which deal with differences in resources, are, at the same time, interest questions by their own right and balance checks. And hypothesis H-S5 addresses directly to the possibility of precise control over the winning margin, that means, self-assignment to treatment, called sorting. Thus, hypothesis H-S5 involves the variables related to hypotheses H-S2 to H-S4 and the balance checks of all the other baseline variables. Given this dual function of most tests, answering relevant questions about political selection and supporting the empirical strategy, section 6 follows an unusual arrangement. Many results related to robustness are shown in this subsection, before the presentation of the results on governance, in the next item. This



McCrary's test of manipulation of the forcing variable at the cutoff, performed by the algorithm of Cattaneo, Jansson and Ma (2018). The shadows represent 95% confidence intervals. Treatment variable is color/race Nonwhite.

Figure 4 – McCrary's test

arrangement is consistent with the order in which the hypotheses were stated and allows us to follow the evaluation of the hypotheses under the reasoning proposed by the diagrams in figures 1 and 2.

In 2016, from a sample of 5398[51] municipalities, there were 1596 interracial (White vs. Nonwhite) elections, 48.5% of them won by Nonwhites. And Nonwhites win 49.3% of 2016 elections within the bandwidth 0.2, which encompasses the bandwidths computed for about 95% of the dependent variables. In 2020, Nonwhites win 47.9% of the disputes and 50.7% of the ones within the bandwidth 0.2. There appears to be little or no advantage for Whites. McCrary's test of manipulation at the cut-point, in Figure 4, corroborates it. The test shows no meaningful discontinuity in the density. So I fail to reject hypothesis H-S4.

I mentioned in item 5.1 that baseline variables will be independent of treatment status, as a consequence of imprecise control over the assignment variable. However, as seen in Lee and Lemieux (2010, p. 296), discontinuities of observable baseline characteristics weaken the identification hypothesis. I checked a wide set of pre-treatment features, covering mayoral and municipal attributes. This section contains visual inspection of the main ones. Full results are shown in Table A.6. The list with description of baseline variables is in Table A.2 and their summary statistics in Table A.4. The list of baseline variables used as covariates

[51] Brazil has 5570 municipalities, but I dropped those where there were by-elections between 2017 and 2020, which means that the 2016 winner did not complete his or her term.

is indicated in Table A.2 [52].

The baseline variables whose robust p-value of LLR estimation are under 5% are[53,54]:

- no covariates: Metropolitan Area ($\hat{\tau}_{bc}$=0.102, p-value=0.026);

- with covariates: Log declared assets ($\hat{\tau}_{bc}=-0.471$, p-value=0.040).

Although Metropolitan Area results suggest Nonwhite mayors are more associated with larger cities, such variable may suffer from the fact that there are not many observations equal 1. Table A.1 shows that, from 723 observations within the LLR bandwith, 78 belong to metropolitan areas, 40 with Nonwhite mayors. In addition, Log population and Urbanization rate, associated with Metropolitan Area, are balanced.

The most worrying result regarding manipulation at the cutoff is the one for Log declared assets. Nonwhites are associated with approximately 38% less assets, which is in line with the description of legislative representatives in Campos and Machado (2017) and Bueno and Dunning (2017). Such a difference of personal resources could represent a systematic disadvantage for Nonwhites. Unlike what those authors found for deputies, however, if we analyze Log campaign revenue, which probably has a much more direct relationship to campaign advantages, we see it is pretty balanced. I thus fail to reject hypothesis H-S3. Apparently Whites are not able to convert this difference in personal assets into tangible campaign resources and so into expressive electoral advantage or, even less, into precise control over the winning margin[55]. All other measures concerning tangible or intangible resources are balanced, notably College degree and the proxies for social class based on occupations. Hence, even thought I cannot entirely fail to reject hypothesis H-S2, it should not be a problem.

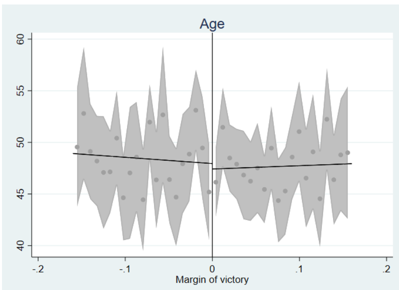
As I point out in item 2.3, given that left-wing parties tend to assimilate minority causes more than right-wing parties, more than other mayoral characteristics, it is particularly interesting to check if ideology and race overlap too much. As presented in item 4.2, I utilize two measures of ideology, based on completely different methodologies. Neither of the two proxies is unbalanced, one of them is illustrated in Figure 5g. It is in agreement with Campos

[52] If there is obvious potential correlation with the dependent variable, the covariate is excluded. For example, the covariate Portuguese score 2015 is excluded if the dependent variable is any of the following: Class size, Age delay 2016, Math score 2015, Δ% class size, Δ% age delay, Δ% portuguese score, Δ% math score.

[53] At 10% significance level, Log population and Class size (no covariates) and President 1 coalition and Social prgm. registration 2016 (with covariates) would also be significant.

[54] Pre-treatment variables are available also in the 2020 period. Considering both 2016 and 2020 for the estimates, at 5% significance level, significant variables are: Log declared (with and without covariates) and Female and Metropolitan area (without covariates). Once it would not fundamentally change the conclusions of this subsection, I kept with 2016 estimates, which are consistent with the sample used to evaluate estimations of post-treatment variables.

[55] The rendering of accounts for the campaign includes revenues from candidate's own resources.



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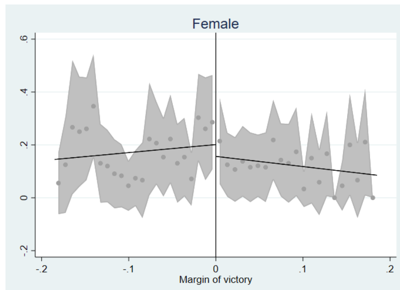

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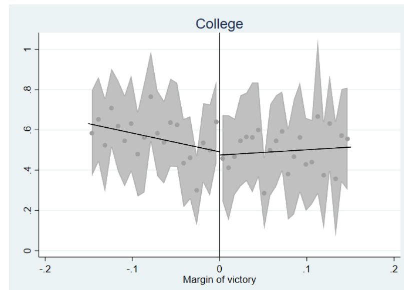

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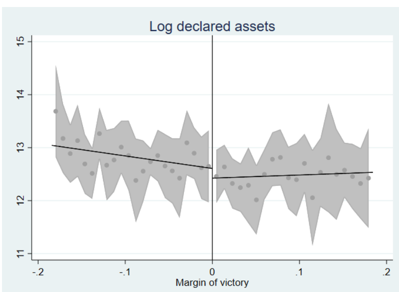

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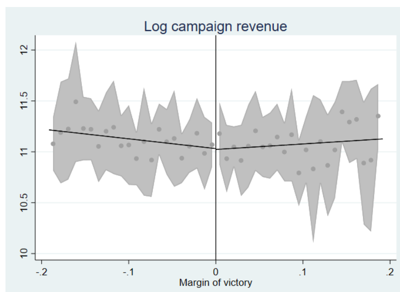

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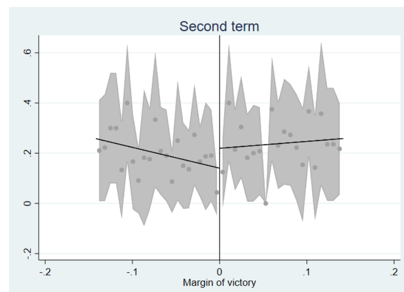

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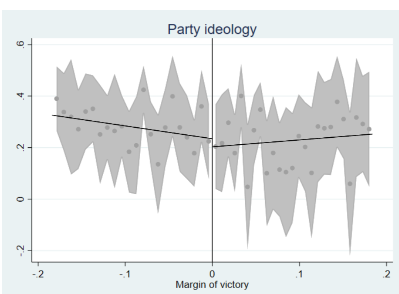

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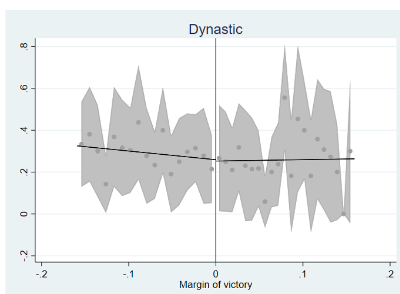


(h)

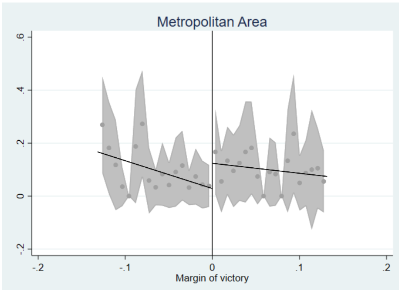
Plot of bias-correct and robust local linear RD estimates, performed by the algorithm of Calonico, Cattaneo and Titiunik (2015). The shadows represent 95% confidence intervals. Covariates are not included. Treatment variable is color/race Nonwhite. The dependent variable of each graph is reported at the top of the graph (detailed definitions of variables are given in Table A.2).

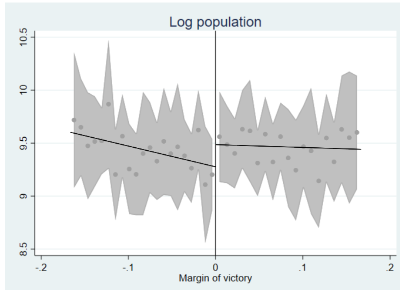
Figure 5 – Balance check of main baseline mayoral characteristics

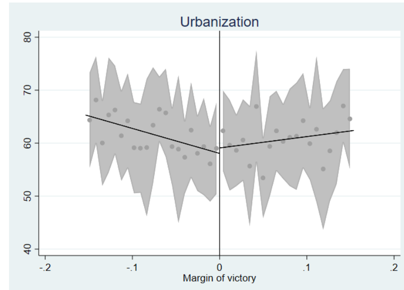
and Machado (2017), who observes no party ideology tendency on the race distribution of deputies. Withal, the qualitative analysis of subsection 3.2 shows the political participation of the Black movement does not consistently follow a position on the political spectrum. In the 1940s the Black movement identified itself with the right, in the 1970s it aligned with the left and nowadays one of its facets is the hip-hop movement, which has no defined political agenda. I conclude that, at least in the Brazilian context, measures of ideology are not capable of representing eventual race-related preferences. So I fail to reject hypothesis H-S1.

In addition to ideology, most of politicians' features of greatest interest to the literature, mentioned in the Introduction, are shown in Figure 5. Dynastic variable, which is not commonly tested in works that are not dedicated to it, stands out. The common census expects, for example, a young woman to have a more progressive governance style. This style may be notwithstanding more influenced by a dynastic tradition she represents than by gender or age. Gender, incumbency status, and dynastic traditions are all balanced. A potentially important confounding municipal variable is Racial salary gap. A smaller racial income gap, for example, might be correlated with a better corporate environment and a more prosperous city. Just as the proxy for patronage, Racial salary gap shows no signs of unbalancing. Figure 6 depicts these two and others municipal characteristics. Figure 7 indicate no impressive Nonwhite advantage in any of the Brazilian regions.

Besides to winning margin density examination and the above arguments, note that none of the variables are consistently significant in both situations, without and with covariates. Finally, it should be considered that when analyzing a broad set of dependent variables (in this case, 51 baseline characteristics plus 38 post-treatment variables), the probability of obtaining some low p-values by chance is high. I conclude there is no substantial evidence of precise control over the margin of victory near the threshold; that is, I failed at rejecting hypothesis H-S5. By the discussion in item 5.1, we know it implies failing to reject the continuity hypothesis necessary to the internal validity of RD strategy. The evaluation of hypotheses H-S1 to H-S5 is complete.







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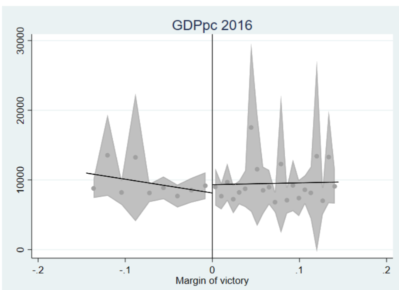

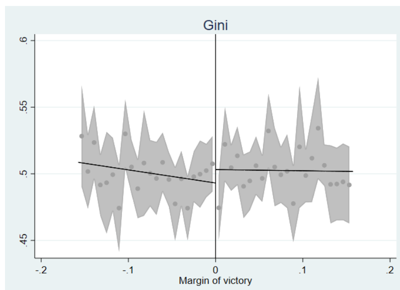

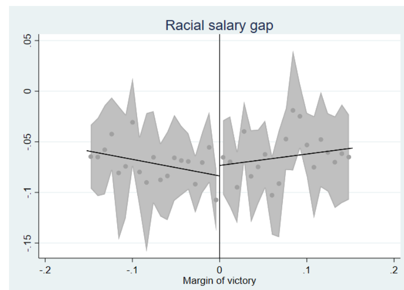

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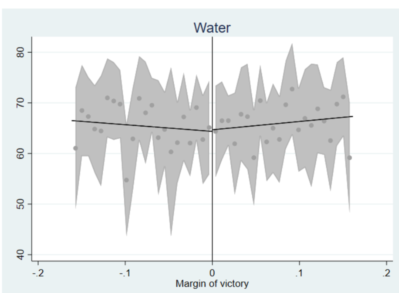
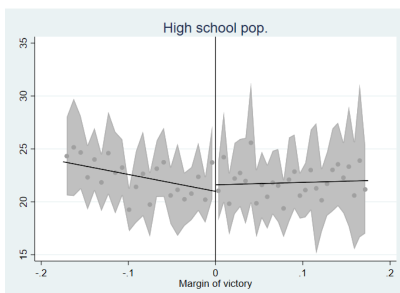
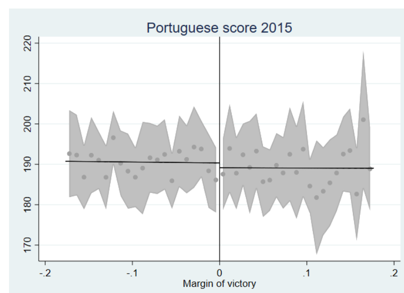
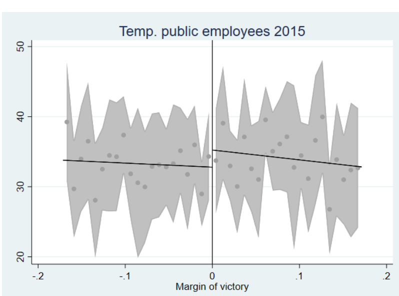
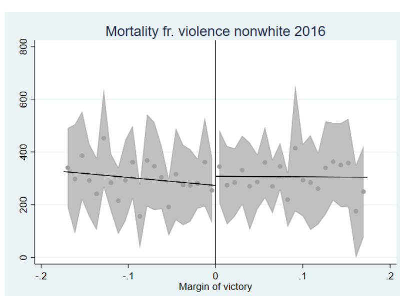






(g) (h) (i)

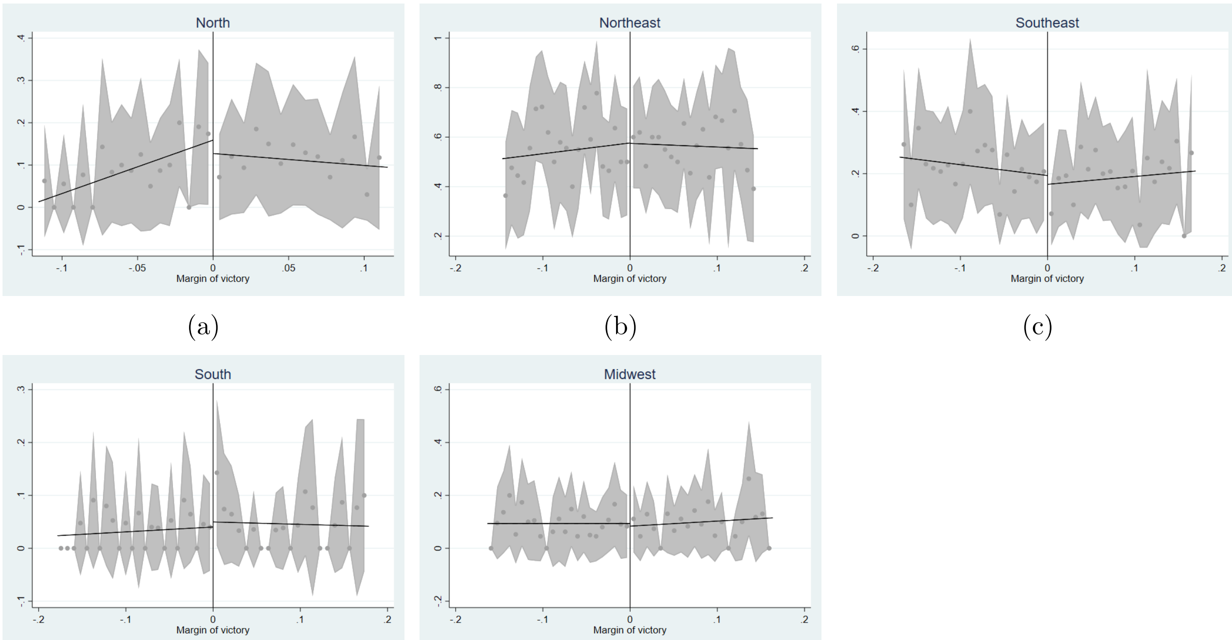




(j) (k)

Plot of bias-correct and robust local linear RD estimates, performed by the algorithm of Calonico, Cattaneo and Titiunik (2015). The shadows represent 95% confidence intervals. Covariates are not included. Treatment variable is color/race Nonwhite. The dependent variable of each graph is reported at the top of the graph (detailed definitions of variables are given in Table A.2).

Figure 6 – Balance check of main baseline municipal characteristics



(a) (b) (c)

(d) (e)

Plot of bias-correct and robust local linear RD estimates, performed by the algorithm of Calonico, Cattaneo and Titiunik (2015). The shadows represent 95% confidence intervals. Covariates are not included. Treatment variable is color/race Nonwhite. The dependent variable of each graph is reported at the top of the graph (detailed definitions of variables are given in Table A.2).

Figure 7 – Balance check of country's Regions

6.2 Reelection and governance outcomes

The goal of this subsection is to examine the hypotheses, stated in item 2.3, about the effect of mayor's race on chance of reelection (H-S6), governance style (H-G1) and performance (H-G2). These hypotheses are covered by the post-treatment variables. This subsection presents tables with LLR results for all variables and graphs for a selection of them, in Figure 8. Full results, including the conventional p-value in LLR[56], the alternative parametric specification and third polynomial degree models are shown in Table A.7. Table A.3 contains the description of all post-treatment variables and Table A.5 brings their summary statistics.

The variables significant in LLR estimation at 5% significance level are[57]:

- no covariates: Privatization ($\hat{\tau}$=-0.076, p-value=0.006), Mortality fr. covid ($\hat{\tau}$=130.358, p-value=0.038);

[56] That means, conventional LLR confidence intervals, without rescaling procedure explained in item 5.2.
[57] At 10% significance level one more variable is significant, Δ% math score, with and without covariates.

- with covariates: Privatization ($\hat{\tau}$=-0.104, p-value=0.002), Growth GDPpc ($\hat{\tau}$=12.229, p-value=0.036).

Privatization (dummy=1 if there was a privatization initiative in the previous 24 months) and Growth GDPpc are shown in Table 3 and Mortality fr. covid in Table 6.

Table 3 – Reelection, economic performance and hiring

	Reelection	Economy		Hiring
	(1)	(2)	(3)	(4)
	Re-elected in 2020	Growth GDPpc	Privatization	Δ% temp. public employees
No covariates				
Bias-corr. RD est.	-0.023	5.390	-0.076	-4.775
Robust std. err.	0.113	5.753	0.028	11.709
Robust p-value	0.840	0.349	0.006	0.683
Bandwidth	0.177	0.180	0.135	0.130
Eff. obs. left	288	525	412	397
Eff. obs. right	261	503	403	390
With covariates				
Bias-corr. RD est.	-0.216	12.229	-0.104	-13.111
Robust std. err.	0.145	5.848	0.034	11.086
Robust p-value	0.135	0.036	0.002	0.237
Bandwidth	0.143	0.203	0.127	0.108
Eff. obs. left	172	379	269	230
Eff. obs. right	142	335	231	206

Notes: Bias-correct and robust local linear RD estimates, performed by the algorithm of Calonico, Cattaneo and Titiunik (2014a). Both models are included, with and without adding covariates (covariates list is indicated in Table A.2). Treatment variable is color/race Nonwhite. The dependent variable of each regression column is reported at the top of the column (detailed definitions of variables are given in Table A.3).

Table 3 shows re-election rates of Whites and Nonwhites are statistically equal, unlike Southern United States, where Nonwhite electorate engagement gives Nonwhites an advantage, as depicted by Vogl (2014). I, therefore, fail to reject hypothesis H-S6. I do not directly access racial prejudice or support from the Black community. Thus, failing in rejecting H-S4 and H-S6, at most, suggest my database corroborates Campos and Machado (2017) and Bueno and Dunning (2017), who finds no evidence of political selection prejudice. It could be, for instance, that both factors are present and cancel each other out.

Table 3 also may indicate Nonwhite mayors delivered greater GDP growth (p-value=3.6% when adding covariates) and are less prone to privatization initiatives. In a hasty analysis, one could think that the conclusion of this work is set: Nonwhite mayors perform better in

Table 4 – Government revenues and expenditures

	Term average					Election year	
	(1) Log own tax revenues pc	(2) Log discretionary transfers pc	(3) Log current expenditures pc	(4) Log investments pc	(5) Budget balance pc	(6) Log current exp. pc 2020	(7) Log investments pc 2020
No covariates							
Bias-corr. RD est.	0.124	0.014	-0.065	-0.092	-542.840	-0.109	-0.207
Robust std. err.	0.124	0.053	0.055	0.117	848.145	0.417	0.319
Robust p-value	0.320	0.787	0.235	0.434	0.522	0.794	0.515
Bandwidth	0.156	0.161	0.152	0.177	0.203	0.147	0.147
Eff. obs. left	473	487	467	517	565	450	449
Eff. obs. right	458	465	449	497	548	434	433
With covariates							
Bias-corr. RD est.	0.137	-0.018	-0.046	-0.011	140.622	0.034	-0.057
Robust std. err.	0.110	0.047	0.039	0.120	831.712	0.451	0.343
Robust p-value	0.212	0.707	0.242	0.929	0.866	0.941	0.869
Bandwidth	0.174	0.210	0.149	0.156	0.163	0.139	0.148
Eff. obs. left	349	384	314	326	336	296	314
Eff. obs. right	296	337	266	274	280	250	264

Notes: Bias-correct and robust local linear RD estimates, performed by the algorithm of Calonico, Cattaneo and Titiunik (2014a). Both models are included, with and without adding covariates (covariates list is indicated in Table A.2). Treatment variable is color/race Nonwhite. The dependent variable of each regression column at reported in the top of the column (detailed definitions of variables are given in Table A.3).

the economy and the success mechanism is a less privatizing policy. However, the results of Privatization can be disregarded. As shown in Table A.1, it is a more serious case of sample restriction than the one mentioned for the Metropolitan area, in the previous subsection. Within LLR bandwidths, there are only 23 observation with Privatization equal 1 for White mayors and 17 for Nonwhite mayors. When adding covariates, there are respectively 13 and 9[58]. And in Figure 8c we see many spots equal 0, that means in many binwidths there is not even a single observation equal 1.

The analysis of the proportion of temporary public employees, that is proxy for patronage (Table 3), expenditures composition (Tables 4 and 5), minority-oriented policies (Table 7) and cash transfer beneficiaries (Table 8) indicates no evidence of distinct styles of governance between Whites and Nonwhite mayors. Table 4 intends to inform about ability to catch discretionary transfers, proportion of current/capital expenditures, fiscal responsibility and opportunistic expenditures in an election year. Table 5 could indicate a priority sector to the administration and explain greater accomplishments in such sectors. Table 7 could show special sensibility of Nonwhite mayors to minorities causes, through the promotion of education, health or productive inclusion policies aimed at groups such as Indigenous, Quilombolas or Black population in general. Those variables related to minorities are from 2019 and unfortunately there are no previews periods available for comparison. Once municipalities are responsible for registering families in federal cash transfer programs, a variation

[58] Apart maybe from Gender program, other dummies depicted in Table A.1, which are related to minorities and inclusion, do not exhibit such a severe problem.

Table 5 – Government expenditures by category

	(1) Log security exp. pc	(2) Log health and sanitation exp. pc	(3) Log education and culture exp. pc	(4) Log urban and housing exp. pc	(5) Log administrative exp. pc	(6) Log social assistance exp. pc	(7) Log environmental mgmt. exp. pc	(8) Log sport and leisure exp. pc
No covariates								
Bias-corr. RD est.	0.315	-0.050	-0.047	-0.044	-0.016	-0.113	0.014	-0.269
Robust std. err.	0.433	0.062	0.047	0.119	0.092	0.085	0.376	0.216
Robust p-value	0.467	0.418	0.318	0.715	0.857	0.183	0.971	0.213
Bandwidth	0.180	0.168	0.193	0.223	0.164	0.161	0.164	0.195
Eff. obs. left	223	501	551	587	492	489	366	533
Eff. obs. right	189	477	528	574	471	466	364	516
With covariates								
Bias-corr. RD est.	-0.486	0.014	-0.042	-0.096	0.015	0.003	0.051	-0.153
Robust std. err.	0.543	0.049	0.046	0.135	0.081	0.061	0.415	0.230
Robust p-value	0.370	0.769	0.354	0.476	0.858	0.967	0.903	0.505
Bandwidth	0.131	0.191	0.158	0.218	0.192	0.197	0.179	0.207
Eff. obs. left	117	368	333	391	369	371	267	371
Eff. obs. right	82	320	277	344	321	326	232	326

Notes: Bias-correct and robust local linear RD estimates, performed by the algorithm of Calonico, Cattaneo and Titiunik (2014a). Both models are included, with and without adding covariates (covariates list is indicated in Table A.2). Treatment variable is color/race Nonwhite. The dependent variable of each regression column is reported at the top of the column (detailed definitions of variables are given in Table A.3).

Table 6 – Health and education

	Health				Education			
	(1) Δ infant mortality	(2) Δ 7+ prenatal visits	(3) Δ low birth weight	(4) Covid-19 mortality	(5) Δ% class size	(6) Δ% age delay	(7) Δ% Portuguese score	(8) Δ% math score
No covariates								
Bias-corr. RD est.	1.564	-0.877	0.824	130.358	-0.028	0.026	-0.010	-0.019
Robust std. err.	18.488	5.166	3.310	62.847	0.034	0.079	0.010	0.010
Robust p-value	0.933	0.865	0.803	0.038	0.411	0.738	0.307	0.059
Bandwidth	0.156	0.205	0.183	0.174	0.133	0.173	0.239	0.225
Eff. obs. left	298	352	331	515	401	491	562	542
Eff. obs. right	298	369	333	489	392	471	539	524
With covariates								
Bias-corr. RD est.	3.166	-5.830	1.790	52.246	-0.027	0.069	-0.009	-0.021
Robust std. err.	21.572	5.604	4.364	64.841	0.042	0.098	0.012	0.011
Robust p-value	0.883	0.298	0.682	0.420	0.511	0.482	0.485	0.063
Bandwidth	0.128	0.183	0.157	0.177	0.136	0.143	0.212	0.197
Eff. obs. left	181	236	219	353	306	310	383	367
Eff. obs. right	153	201	177	303	263	269	334	322

Notes: Bias-correct and robust local linear RD estimates, performed by the algorithm of Calonico, Cattaneo and Titiunik (2014a). Both models are included, with and without adding covariates (covariates list is indicated in Table A.2). Treatment variable is color/race Nonwhite. The dependent variable of each regression column is reported at the top of the column (detailed definitions of variables are given in Table A.3).

of beneficiaries in Table 8 might suggest mayor's commitment to poverty alleviation.

Since, given the arguments mentioned above, I will not support the results of Privatization and none of the other variables representing policy-making initiatives proved to be statistically significant, I consider there is no evidence that the Nonwhite color/race of Brazilian mayors affects governance style. That is, I fail to reject hypothesis H-G1. These findings match with Hopkins and McCabe (2012), who, other than measures related to hiring police staff, observe no meaningful policy contrasts between Black and White mayors in the US. Further, one could interpret these results as a support to the theoretic literature that predicts policy converge. However, this is nothing more than a suggestion. It could be that the Citizen-candidate model is the most adequate to the Brazilian context, but color/race in particular has no effect on governance. The works that find effects of personal characteristics on governance in Brazil, mentioned in item 2.2, favor this second interpretation.

Table 7 – Minorities and inclusion

	(1) Education pgrm. quilombolas	(2) Family health pgrm. nonwhite	(3) Vaccination pgrm. nonwhite	(4) Social assistance quilombolas	(5) Labor inclusion pgrm.	(6) Gender program
No covariates						
Bias-corr. RD est.	0.026	-0.032	-0.053	-0.042	0.011	0.052
Robust std. err.	0.083	0.079	0.154	0.090	0.071	0.032
Robust p-value	0.758	0.687	0.732	0.643	0.880	0.105
Bandwidth	0.195	0.168	0.121	0.158	0.166	0.229
Eff. obs. left	340	503	127	330	467	601
Eff. obs. right	321	478	118	321	437	586
With covariates						
Bias-corr. RD est.	0.033	-0.058	0.010	-0.019	0.072	0.025
Robust std. err.	0.112	0.090	0.171	0.101	0.082	0.040
Robust p-value	0.766	0.516	0.955	0.851	0.380	0.527
Bandwidth	0.162	0.161	0.128	0.148	0.172	0.158
Eff. obs. left	216	335	94	222	327	330
Eff. obs. right	169	279	74	188	274	275

Notes: Bias-correct and robust local linear RD estimates, performed by the algorithm of Calonico, Cattaneo and Titiunik (2014a). Both models are included, with and without adding covariates (covariates list is indicated in Table A.2). Treatment variable is color/race Nonwhite. The dependent variable of each regression column is reported at the top of the column (detailed definitions of variables are given in Table A.3).

Besides GDP growth, the performance evaluation consists of indicators of health and education (Table 6) and violence/security (Table 8). As explained in item 4.1, Brazilian municipalities have constitutional responsibility to provide health and elementary education services. Table 6 brings infant health and infant education indicators that are sensitive to the coverage and efficiency of such services. Probably, Covid-19 mortality is an exception. Despite of the fact that the Supreme Court assured the autonomy of mayors to promote

Table 8 – Cash transfer and security

	Cash transfer		Security		
	(1)	(2)	(3)	(4)	(5)
	Δ% social prgm. registration	Δ% cash transf. beneficiaries	Δ mortality fr. violence	Δ mortality fr. violence nonwhite	Municipal guard staff
<u>No covariates</u>					
Bias-corr. RD est.	0.027	0.032	8.990	20.355	-6.805
Robust std. err.	0.028	0.043	41.874	54.506	19.717
Robust p-value	0.344	0.462	0.830	0.709	0.730
Bandwidth	0.160	0.237	0.147	0.152	0.146
Eff. obs. left	487	617	450	468	101
Eff. obs. right	465	595	434	448	102
<u>With covariates</u>					
Bias-corr. RD est.	0.029	0.106	-43.040	-29.906	0.798
Robust std. err.	0.032	0.071	53.257	67.594	20.252
Robust p-value	0.365	0.138	0.419	0.658	0.969
Bandwidth	0.205	0.198	0.162	0.169	0.176
Eff. obs. left	380	371	335	342	80
Eff. obs. right	337	327	281	288	79

Notes: Bias-correct and robust local linear RD estimates, performed by the algorithm of Calonico, Cattaneo and Titiunik (2014a). Both models are included, with and without adding covariates (covariates list is indicated in Table A.2). Treatment variable is color/race Nonwhite. The dependent variable of each regression column is reported at the top of the column (detailed definitions of variables are given in Table A.3).

isolation policies in April 2020 [59], the power constraints of the office, such as not being able to restrict movement of people across municipal borders, implies that mayors likely have a minor role in fighting the pandemic. Similarly, public safety in Brazil is primarily responsibility of the States. A minority of city halls has a municipal guard. And these guards have attributions restricted to the protection of the Municipality's assets and facilities. Even so, the possibility of municipal administration influencing both phenomena must be considered. Table 8 assesses the deaths from aggression in the population and, in particular, in the Nonwhite population.

As discussed in item 2.3, observing differences in performance without significant results of policy initiatives could be justified by missed features in the analysis of governance or by heterogeneity in competence. Still, the absence of distinct observable policies that could be pointed out as mechanisms weakens results related to performance variables. More importantly, the two significant results regarding performance, Growth GDPpc (with covariates) and Covid-19 mortality (no covariates), are not enough evidence of the effect of Nonwhite color/race on the performance of Brazilian mayors. From 31 post-treatment variables, at 5%

[59] SUPREMO FEDERAL TRIBUNAL - ADI 6341.

significance level, we expect each model to return one or two significant variables simply by chance and, including Privatization, this is what we get. On that account, I fail to reject hypothesis H-G2. This completes the evaluation of all the hypotheses.

6.3 Alternative specification, power of test and heterogeneity effects

The variables significant at 5% significance level for the alternative linear parametric approach are shown above. As mentioned, Tables A.6 and A.7 brings full results, including parametric models.

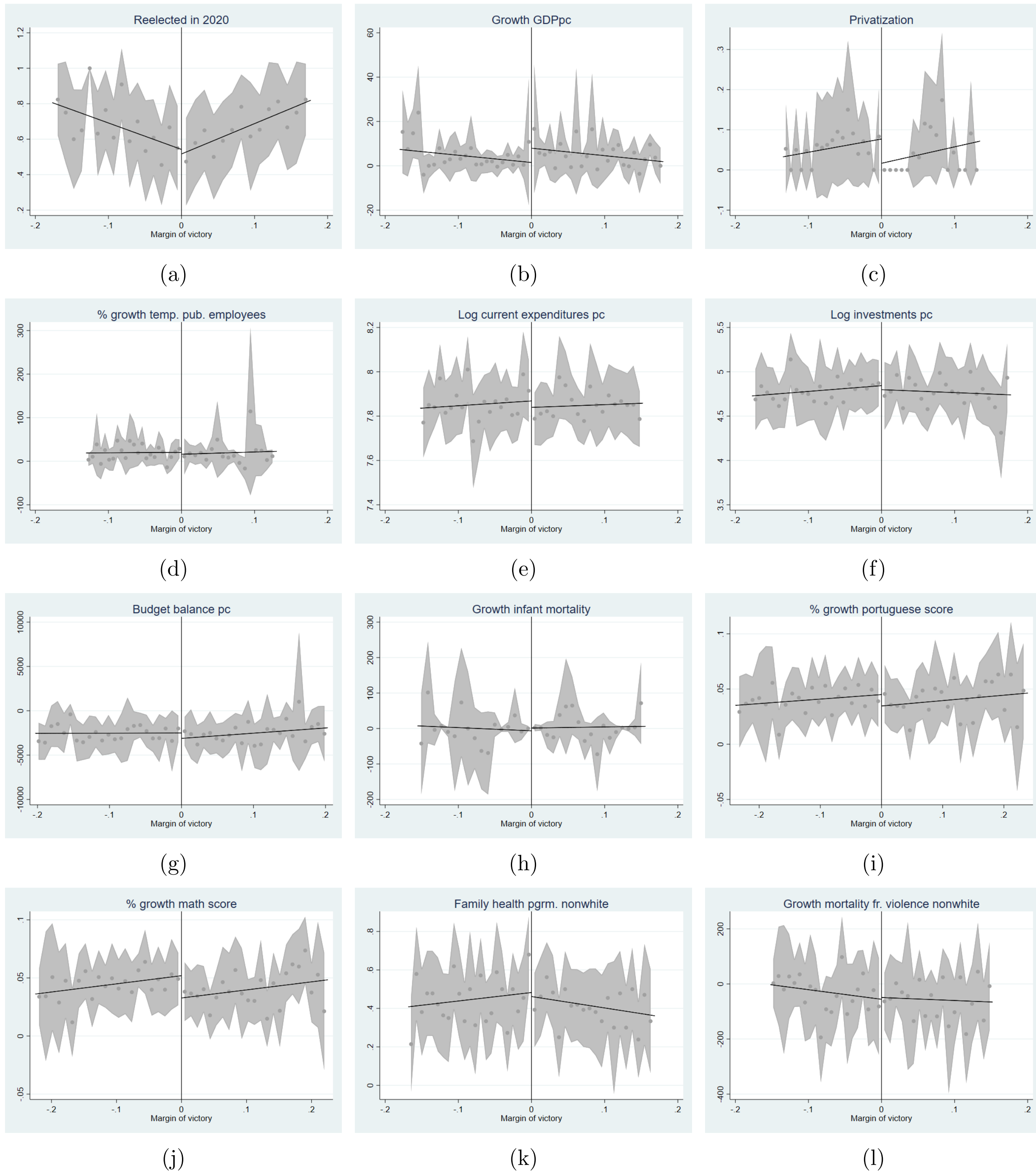
- Baseline variables
  - no covariates: Female ($\hat{\tau}=-0.072$, p-value=0.005), College ($\hat{\tau}=-0.085$, p-value=0.018), Log declared assets ($\hat{\tau}=-0.329$, p-value=0.003);
  - with covariates: College ($\hat{\tau}=-0.126$, p-value=0.003), Log declared assets ($\hat{\tau}=-0.327$, p-value=0.006).
- Post-treatment variables
  - no covariates: Δ low birth weight ($\hat{\tau}=-3.252$, p-value=0.028), Δ% age delay ($\hat{\tau}=-0.109$, p-value=0.015);
  - with covariates: Δ% age delay ($\hat{\tau}=-0.115$, p-value=0.018).

Table 9 – Power of the test

	No covariates	With covariates
$\text{power}_{rbc}>0.8$	23.6%	37.0%
$0.7<\text{power}_{rbc}<0.8$	47.2%	15.7%
$0.6<\text{power}_{rbc}<0.7$	15.7%	21.4%
$0.5<\text{power}_{rbc}<0.6$	5.6%	11.2%
$\text{power}_{rbc}<0.5$	7.9%	14.6%

Notes: Proportions of dependent variables according to the power-value for robust bias-corrected local linear estimates. They are performed by the algorithm of Cattaneo, Titiunik and Vazquez-Bare (2019). Both models are included, with and without adding covariates (covariates list is indicated in Table A.2). Treatment variable is color/race Nonwhite. Testes consider the whole set of dependent variables of this work, listed in Tables A.2 and A.3.

To check on the sample's capacity to identify treatment effects, I computed the power of the tests, as commented in item 5.2. The algorithm requires an input that is the treatment effect under the alternative at which the power function is evaluated, which I got by averaging



Plot of bias-correct and robust local linear RD estimates, performed by the algorithm of Calonico, Cattaneo and Titiunik (2015). The shadows represent 95% confidence intervals. Covariates are not included. Treatment variable is color/race Nonwhite. The dependent variable of each graph is reported at the top of the graph (detailed definitions of variables are given in Table A.3).

Figure 8 – Results of a selection of outcome variables

significant treatment effects of the literature. Once works regarding race are not abundant, I also resort to articles that test other personal characteristics in Brazil. I used Hopkins and McCabe (2012) (race, US), Rocha, Orellano and Bugarin (2018) (education and experience, Brazil) and Brollo and Troiano (2016) (gender, Brazil)[60]. I came to the average treatment effect (in terms of standard deviation) equal 0.413. A summary of results is given in Table 9. The list of variables of the model without covariates whose power of test is under 0.6 is: Dynastic, Light, Low birth weight 2016, Reelected in 2020, Log security exp. pc, Log environmental mgmt. exp. pc, Growth GDPpc, Δ 7+ prenatal visits, Δ low birth weight, Education pgrm. quilombolas, Vaccination pgrm. nonwhite, Δ% cash transf. beneficiaries[61].

I tested heterogeneity effects of each of the five Brazilian regions. Just Northeast may have a sample large enough to make such an analysis feasible. Yet, gender is unbalanced and, more importantly, the lagged versions of deaths from violence of the whole population and the Nonwhite population are unbalanced. Municipalities govern by Nonwhite mayors show worse initial indicators of violence. These variables are precisely the ones for which negative effects (decreasing in violence) have been found. One might think that starting from an unfavorable initial situation and still getting greater improvements at the end of the term reinforces a diagnosis of good performance. However, it should be considered, for example, that moving from a very critical situation to a less critical one is perhaps easier than starting from a situation of medium severity and achieving good levels of safety. When covariates are added, other pre-treatment characteristics, notably ideology, and outcome variables become significant. It is a smaller sample, though. These results are not shown.

Disaggregating ”Pretos” and ”Pardos” is loosing the sociological meaning these groups have together. Osório (2009) explains that ”Pretos” and ”Pardos” are subjected to discrimination for the same reason, they do not meet the socially valued ideal of whiteness. And they hold similar socioeconomic features, such as access to basic services, educational level and income. Examining the average amount of assets declared in my database reinforces the relevance of the Nonwhite category, as it was designed. ”Pretos” and ”Pardos” do indeed show heterogeneity. They report, respectively, 416 and 746 thousand reais in assets on average. The contrast with Whites is much greater, though. They declare approximately 1681 thousand reais[62]. Still, I checked for heterogeneity effects in race. Since considering the polls

[60] I tried to include the attribute dynastic with Bragança, Ferraz and Rios (2015), but the authors do not inform the standard deviation of outcome variables and I did not find supplementary material.

[61] As comment in item 6.1, pre-treatment variables are available also in the 2020 period. Naturally, power-values are greater when 2020 is considered. For the model without covariates, 76.5% of the baseline variables exhibit bias corrected robust power-value greater than or equal to 0.8, and 96.1% greater than or equal to 0.7. Recall, however, considering 2020 does not fundamentally change the set of statistically significant pre-treatment variables.

[62] Asians declare on average around 2352 thousand reais and Indigenous 173, but these groups have only 11 and 3 obs., respectively. ”Pretos”, ”Pardos” and Whites have, in that order, 47, 670 and 737 obs..

of ”Pretos” against ”Pardos” would not be admissible, I dropped all elections where one of the top-two candidates is ”Pardo”. So that interracial elections have a candidate ”Preto” or Indigenous against a White or Asian. The sample then faces a severe reduction. There were only 98 such elections in 2016, 54 of which won by ”Pretos” or Indigenous. On that account, I used only the parametric (global) approach. Full results are not shown. Above, I list all the outcomes of the linear model that are significant at 5% significance level:

- Baseline variables
  - no covariates: Party ideology ($\hat{\tau} = -0.237$, p-value=0.048, $Obs.$=98), Dynastic ($\hat{\tau} = -0.455$, p-value=0.001, $Obs.$=70);
  - with covariates: Log declared assets ($\hat{\tau} = -1463$, p-value=0.034, $Obs.$=58), Dynastic ($\hat{\tau} = -0.342$, p-value=0.038, $Obs.$=58).
- Post-treatment variables
  - no covariates: Social assistence quilombolas ($\hat{\tau} = -0.473$, p-value=0.015, $Obs.$=68);
  - with covariates: Social prgm. registration 2016 ($\hat{\tau} = 0.035$, p-value=0.032, $Obs.$=58), Δ low birth weight ($\hat{\tau} = -22611$, p-value=0.001, $Obs.$=38).

7 Conclusion

Brazil’s racial inequalities are not only the result of a history of slavery and low income mobility, but the persistence of prejudice. Overcoming this context of inequalities includes occupying positions of political power. In this work I investigated whether, more than one of the dimensions of the inequalities, political representation can be a direct mechanism for overcoming them. It could be achieved by Nonwhite mayors through policies more sensible to the racial issue or poverty. I utilized regression-discontinuity approach with a bias corrected and robust local linear regression, exploring local elections data and, in particular, the color/race variable started to be report in 2016. From a sample of more than five thousand municipalities, there were 1596 interracial elections. In respect of political selection, I investigated the relation between color/race and ideology, eventual differences in tangible and intangible resources, as well as advantages in being elected and reelected. Regarding governance, I accessed measures related to policy-making style and performance. Those subjects demanded the examination of a wide range of pre-treatment variables (including lagged outcomes, when available) and governance indicators.

Together with the examination of victory margin’s density, balance checks suggest mayors have, at best, imprecise control over the margin of victory. Even though White candidates are substantially richer in assets, they are not better funded than Nonwhites. Variables potentially associated with intangible resources, such as educational level and relatives predecessors are balanced, along with other features of great interested in the literature, like gender and incumbency advantage. And, in particular, ideology is not a good proxy for color/race, i.e., cannot satisfactorily represent color/race. None of the racial groups has higher election or re-election rate. Expenditures composition, a proxy for patronage, minority-oriented policies and registration to cash transfers programs evince that Nonwhite and White mayors styles of governance are not discernible. Health, education and violence indicators show no greater accomplishments of either racial group. The statistical significance of few indicators, among the wide range of variables tested, is attributed to chance. I conclude the inauguration of a Nonwhite mayor have no expressive impact on municipal policies.

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1 Appendix

Table A.1 – Tabulation of a selection of dummies

		Whole data		Local no covs.		Local with covs.	
		White	Nonwhite	White	Nonwhite	White	Nonwhite
Metropolitan area	0	730	690	365	358	278	237
	1	91	85	38	40	34	26
Privatization	0	766	731	389	386	256	222
	1	53	44	23	17	13	9
Education pgrm. quilombolas	0	365	343	248	233	152	115
	1	130	121	92	88	64	54
Family health pgrm. nonwhite	0	449	456	278	279	176	161
	1	371	319	225	199	159	118
Vaccination pgrm. nonwhite	0	132	111	60	45	43	29
	1	153	143	67	73	51	45
Social assistence quilombolas	0	336	328	194	199	125	114
	1	228	205	136	122	97	74
Labor inclusion pgrm.	0	211	209	130	127	95	79
	1	551	502	337	310	232	195
Gender program	0	773	721	566	546	313	255
	1	47	53	35	40	17	20

Notes: Tabulation of a selection of dummy variables. The objective is to check for sample restrictions, specially of dummy=1. "White" and "Nonwhite" refers to control and treatment group, respectively. "Whole data" means all observations are considered. "Local" refers to the observations within the bandwidth of a bias-corrected robust local linear regression, computed by the algorithm of Calonico, Cattaneo and Titiunik (2014a). Both models are included, with and without adding covariates (covariates list is indicated in Table A.2). Detailed definitions of variables are given in Tables A.2 and A.3.

Table A.2 – Description of baseline variables

Panel A: Mayoral pre-treatment characteristics	
Adherence rate	Adherence rate to the federal government
Age[c]	Age in years at the time of inauguration
College[c]	Dummy=1 if the mayor has college degree
Dynastic[c]	Dummy=1 if family predecessors were found (dynastic mayor)
Female[c]	Dummy=1 if the mayor is female
Governor 1 coalition	Dummy=1 if same coalition of the governor elect before mayor's term
Governor 2 coalition	Dummy=1 if same coalition of the governor elected during mayor's term
High-class occupation	Dummy=1 if mayor's occupation is classified as high-class.
Log campaign revenue	Log of campaign total revenue
Log declared assets[c]	Log of assets declared to the electoral authority
Low-class occupation	Dummy=1 if mayor's occupation is classified as low-class.
Margin of victory	[forging variable] Winning margin of nonwhite color/race candidate
Middle-class occupation	Dummy=1 if mayor's occupation is classified as middle-class.
Nonhigh	Dummy=1 if the mayor does not have high school degree
Nonwhite	[treatment] Dummy=1 color/race different from white
Party ideology[c]	Ideology of the mayor's party (2016) - Power and Rodrigues-Silveira methodology
President 1 coalition	Dummy=1 if same coalition of the president elected before mayor's term
Run for reelection	Dummy=1 if the mayor will run for reelection in 2020 (it constrains "Re-elected in 2020")
Second term[c]	Dummy=1 if the mayor is in the 2nd term
Panel B: Municipal pre-treatment characteristics	
7+ prenatal visits 2016	Rate of mothers of live births with 7 or more prenatal visits (2016)
Age delay 2016	Age-grade distortion rate in the 5th grade of elementary school (2016)
Area	Area (Km^2) (2010)
Average salary	Average salary income (2010)
Cash transf. beneficiaries 2016	Number of per capita families benefiting from "Bolsa Família" program (2016)
Class size	Average class size in the 5th grade of elementary school (2016)
College pop.	Proportion of people aged 25 or over with complete higher education (2010)
GDPpc 2016[c]	GDP per capita in 2010 values (2016)
Gender salary gap	Gender wage inequality (regression coefficient) (2010)
Gini[c]	Gini index (2010)
High school pop.[c]	Proportion of people aged 25 or over who have completed high school (2010)
Illiteracy	Proportion of illiterate people aged 20 or over (2010)
Infant mortality 2016	Infant mortality [(deaths up to 1 year/live births)*1000] (2016)
Light	Proportion of households connected to a power grid (2010)
Log population[c]	Log of population estimate (2016)
Low birth weight 2016	Rate of live births weighing less than 2500g (2016)
Math score 2015	Average math score on the Saeb test of the 5th grade of elementary school (2015)
Mean age	Average age of the population (2010)
Metropolitan Area	Dummy=1 if it is a metropolitan region (2010)
Midwest	Midwest region of Brazil
Mort. fr. violence nonwhite 2016[c]	Mortality from violence of nonwhite people (per million nonwhite inhabitants) (2016)
Mortality from violence 2016	Mortality from violence (per million inhabitants) (2016)
Nonwhite pop.	Proportion of people of nonwhite race/color in the population (2010)
North[c]	North region of Brazil
Northeast[c]	Northeast region of Brazil
Portuguese score 2015[c]	Average portuguese score in the Saeb test of the 5th year of elementary school (2015)
Public employees pc 2015	Total number of employees per capita in direct administration (2015)
Racial salary gap[c]	Racial wage inequality (regression coefficient) (2010)
Sewer	Proportion of households connected to the general sewage network (2010)
Social prgm. registration 2016	Number of families per capita registered in the "Cadastro Único" system (2016)
South[c]	South region of Brazil
Southeast[c]	Southeast region of Brazil
Temp. pub. employees 2015[c]	Proportion of temporary employees in direct administration (2015)
Urbanization[c]	Proportion of urban population (2010)
Water[c]	Proportion of households connected to the general water network (2010)

Note: Superscript "[c]" indicates the variable is included among the covariates used in the regressions.

Table A.3 – Description of post-treatment variables

Δ 7+ prenatal visits	Change in the rate of mothers of live births with 7 or more prenatal visits (2016-19)
Δ infant mortality	Change in infant mortality (2016-19)
Δ low birth weight	Change in the rate of live births weighing less than 2500g (2016-19)
Δ mortality fr. violence Nonwhite	Change in mortality from violence of Nonwhite people (2016-19)
Δ mortality fr. violence	Change in mortality from violence (2016-19)
Δ% age delay	Percentage change in the age-grade distortion rate in the 5th grade of elementary school (2016-20)
Δ% cash transf. beneficiaries	Percentage change of families benefiting from "Bolsa Família" program (2016-20)
Δ% class size	Percentage change in average class size in the 5th grade of elementary school (2016-20)
Δ% math score	Percentage change in average math score on the Saeb test of the 5th grade of elementary school (2015-19)
Δ% Portuguese score	Percentage change in average Port. score on the Saeb test of the 5th year of elementary school (2015-19)
Δ% social prgm. registration	Percentage change of families registered in the "Cadastro Único" system (2016-20)
Δ% temp. pub. employees	Percentage change in the proportion of temporary employees in direct administration (2015-19)
Budget balance pc	Cumulative balance budget per capita (2017-20)
Covid-19 mortality	Covid-19 mortality (per million inhabitants) (2020)
Education pgrm. quilombolas	Education agency has programs aimed at indigenous people or quilombolas (2018)
Family health pgrm. Nonwhite	PMS includes PNSIPN or there is a family health program aimed at indig. people or quilomb. (2018)
Gender program	There is a municipal policy plan for women (2018)
Growth GDP pc	Growth of real GDP per capita (2016-18)
Labor inclusion pgrm.	There is a productive inclusion program for job and income generation (2018)
Log administrative exp. pc	Log of average per capita paid administrative expenditures (2017-20)
Log current exp. pc 2020	Log of current paid per capita expenditures (2020)
Log current expenditures pc	Log of average current paid per capita expenditures (2017-20)
Log discretionary transfers pc	Log of average gross per capita revenue from discretionary transfers (2017-20)
Log education and culture exp. pc	Log of average per capita paid expenditures in education plus culture (2017-20)
Log environmental mgmt. exp. pc	Log of average per capita paid expenditures in environmental management (2017-20)
Log health and sanitation exp. pc	Log of average per capita paid expenditures in health plus sanitation (2017-20)
Log investments pc	Log of average paid per capita capital expenditures (investments) (2017-20)
Log investments pc 2020	Log of paid per capita capital expenditures (investments) (2020)
Log own tax revenues pc	Log of own gross revenue per capita average (total revenue - transfers) (2017-20)
Log security exp. pc	Log of average per capita paid expenditures in public security excluding civil defense (2017-20)
Log social assistance exp. pc	Log of average per capita paid expenditures in social assistance (2017-20)
Log sport and leisure exp. pc	Log of average per capita paid expenditures in sports plus leisure (2017-20)
Log urban and housing exp. pc	Log of average per capita paid expenditures in urbanism plus housing (2017-20)
Municipal guard staff	Municipal guard staff (2019)
Privatization	There was a privatization initiative in the previous 24 months (2019)
Re-elected in 2020	Dummy=1 if the mayor will be re-elected in 2020
Social assistance quilombolas	Municipality provides social assistance services aimed at people of african origin, indig. or quilomb. (2018)
Vaccination pgrm. Nonwhite	There is a vaccination program aimed at the black, indigenous or quilomb. population (2018)

Table A.4 – Summary statistics of baseline variables

	White mayor			Nonwhite mayor			t-test
	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.	p-value
Panel A: Mayoral pre-treatment characteristics							
Age	821	48.683	11.433	775	47.742	10.193	0.082
Female	821	0.168	0.374	775	0.116	0.321	0.003
Nonhigh	821	0.149	0.356	775	0.150	0.357	0.952
College	821	0.548	0.498	775	0.499	0.500	0.051
Low-class occupation	775	0.137	0.344	721	0.140	0.347	0.853
Middle-class occupation	775	0.230	0.421	721	0.286	0.452	0.013
High-class occupation	775	0.634	0.482	721	0.574	0.495	0.019
Log declared assets	748	12.934	1.398	720	12.503	1.452	0.000
Log campaign revenue	821	11.152	0.914	775	11.114	0.878	0.395
Second term	821	0.212	0.409	775	0.223	0.417	0.585
Party ideology	821	0.268	0.377	775	0.223	0.398	0.022
Adherence rate	806	74.865	25.519	764	72.053	27.204	0.035
Governor 1 coalition	821	0.508	0.500	775	0.548	0.498	0.106
Governor 2 coalition	788	0.406	0.491	750	0.444	0.497	0.133
President 1 coalition	821	0.583	0.493	775	0.577	0.494	0.788
Dynastic	658	0.275	0.447	559	0.263	0.441	0.635
Panel B: Municipal pre-treatment characteristics							
North	821	0.122	0.327	775	0.114	0.317	0.609
Northeast	821	0.477	0.500	775	0.535	0.499	0.020
Southeast	821	0.244	0.430	775	0.204	0.403	0.057
South	821	0.045	0.208	775	0.043	0.202	0.808
Midwest	821	0.112	0.316	775	0.105	0.306	0.628
Area	821	1920.735	7035.927	774	1832.430	6482.129	0.794
Urbanization	821	62.604	20.418	775	62.183	20.381	0.680
Metropolitan Area	821	0.111	0.314	775	0.110	0.313	0.941
Log population	821	9.504	1.115	775	9.516	1.076	0.825
Nonwhite pop.	821	0.664	0.157	775	0.674	0.152	0.162
Mean age	821	30.613	2.840	775	30.503	2.791	0.433
Average salary	821	727.847	268.620	775	703.424	261.196	0.066
Racial salary gap	821	-0.076	0.089	775	-0.072	0.104	0.409
Gender salary gap	821	-0.306	0.125	775	-0.293	0.135	0.052
Gini	821	0.501	0.060	775	0.502	0.058	0.723
Illiteracy	821	22.644	10.656	775	23.402	10.707	0.157
High school pop.	821	22.957	7.898	775	22.508	7.692	0.250
College pop.	821	4.631	2.494	775	4.495	2.411	0.268
Water	821	65.422	20.500	775	66.164	20.302	0.468
Light	821	95.899	6.859	775	95.857	6.246	0.898
Sewer	821	24.959	28.755	775	24.080	28.315	0.538
Public employees pc 2015	820	0.052	0.022	773	0.052	0.020	0.674
Temp. public employees 2015	820	31.653	16.551	773	33.219	17.085	0.063
GDPpc 2016	821	10350.324	11904.015	775	9917.115	11136.107	0.453
Infant mortality 2016	581	49.416	175.384	571	28.842	111.403	0.017
7+ prenatal visits 2016	581	51.362	29.627	571	52.617	28.757	0.466
Low birth weight 2016	581	8.398	16.311	571	8.533	16.045	0.887
Mortality fr. violence 2016	821	244.822	271.605	774	247.865	248.380	0.815
Mortality fr. violence nonwhite 2016	821	302.018	357.640	774	303.705	324.388	0.921
Class size	808	21.403	4.956	764	21.542	4.699	0.568
Age delay 2016	814	24.576	14.956	769	26.410	15.064	0.015
Portuguese score 2015	759	191.851	19.630	717	190.110	21.015	0.101
Math score 2015	759	204.898	20.033	717	203.194	21.017	0.112
Social prgm. registration 2016	821	0.206	0.062	775	0.211	0.061	0.149
Cash transf. beneficiaries 2016	821	0.117	0.050	775	0.122	0.052	0.058

Notes: Summary statistics of pre-treatment variables divided into control group ("White mayors") and treatment group ("Nonwhite mayors") plus a mean-comparison tests. Detailed definitions of variables are given in Table A.2.

Table A.5 – Summary statistics of post-treatment variables

	White mayors			Nonwhite mayors			t-test
	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.	p-value
Reelected in 2020	452	0.670	0.471	413	0.683	0.466	0.696
Growth GDPpc	821	3.838	22.795	775	4.760	27.137	0.464
Privatization	819	0.065	0.246	775	0.057	0.232	0.507
Δ% temp. pub. employees	813	33.413	159.898	766	27.085	135.641	0.396
Log own tax revenues pc	817	5.915	0.798	774	5.877	0.793	0.338
Log discretionary transfers pc	817	7.017	0.415	774	7.032	0.365	0.454
Log current expenditures pc	818	7.858	0.352	774	7.853	0.341	0.733
Log investments pc	818	4.818	0.752	774	4.775	0.746	0.249
Budget balance pc	821	-2610.430	5171.586	774	-2504.462	5650.006	0.697
Log current exp. pc 2020	821	7.180	2.453	774	7.169	2.456	0.929
Log investments pc 2020	821	4.807	1.862	774	4.785	1.823	0.808
Log security exp. pc	357	1.200	1.859	310	1.421	1.771	0.117
Log health and sanitation exp. pc	821	6.559	0.387	774	6.550	0.391	0.659
Log education and culture exp. pc	821	6.788	0.353	774	6.801	0.336	0.433
Log urban and housing exp. pc	816	5.094	0.945	773	5.073	0.973	0.672
Log administrative exp. pc	821	5.926	0.595	774	5.907	0.586	0.507
Log social assistence exp. pc	821	4.640	0.596	774	4.605	0.591	0.252
Log environmental mgmt. exp. pc	637	1.829	1.961	595	1.729	1.992	0.373
Log sport and leisure exp. pc	793	2.185	1.435	746	2.089	1.429	0.189
Δ infant mortality	529	-9.906	186.761	526	-3.008	126.843	0.483
Δ 7+ prenatal visits	529	6.370	27.598	526	5.590	24.196	0.625
Δ low birth weight	529	1.424	19.187	526	-0.269	15.898	0.119
Mortality fr. covid	821	571.424	428.745	775	566.232	418.804	0.807
Δ% class size	805	0.015	0.271	763	0.012	0.267	0.801
Δ% age delay	790	-0.073	0.861	752	-0.167	0.494	0.009
Δ% portuguese score	749	0.041	0.066	706	0.040	0.068	0.765
Δ% math score	749	0.045	0.066	706	0.041	0.065	0.196
Education pgrm. quilombolas	495	0.263	0.441	464	0.261	0.440	0.948
Family health pgrm. nonwhite	820	0.452	0.498	775	0.412	0.492	0.100
Vaccination pgrm. nonwhite	285	0.537	0.500	254	0.563	0.497	0.543
Social assistence quilombolas	564	0.404	0.491	533	0.385	0.487	0.506
Labor inclusion pgrm.	762	0.723	0.448	711	0.706	0.456	0.470
Gender program	820	0.057	0.233	774	0.068	0.253	0.360
Δ% social prgm. registration	821	0.053	0.165	775	0.054	0.156	0.954
Δ% cash transf. beneficiaries	821	0.019	0.172	775	0.032	0.216	0.204
Δ mortality fr. violence	821	-32.858	260.724	774	-42.702	238.473	0.431
Δ mortality fr. violence nonwhite	821	-45.511	332.701	774	-43.828	336.453	0.920
Municipal guard staff	195	69.410	178.829	193	65.238	143.780	0.800

Notes: Summary statistics of post-treatment variables divided into control group ("White mayors") and treatment group ("Nonwhite mayors") plus a mean-comparison tests. Detailed definitions of variables are given in Table A.3.

Table A.6 – Full results of baseline variables

This table presents full results for all baseline variables considered in this work. Nonparametric estimations are the local linear regression (LLR, models (1) and (2)), and the local polynomial regression of third degree (LPR 3^{rd}, models (3) and (4)). They are performed by the algorithm of Calonico, Cattaneo and Titiunik (2014a). Parametric (global) estimations are the ordinary least squares regression (OLS, models (5) and (6)) and the polynomial third degree model with terms of interaction between the treatment and the forcing variable (Pol. 3^{rd} interac., models (7) and (8)). Odd-index models do not include covariates, while even-index models do (covariates list is indicated in Table A.3). For nonparametric models, statistics consider the bias-correction and robust variance estimation, unless they are identified as "conventional". When referring to parametric models, "robust" statistics simply indicate the usual heteroscedasticity correction. Treatment variable is color/race Nonwhite. The dependent variable is indicated in the first column (detailed definitions of variables are given in Table A.2).

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Age	Convl. RD est.	-0.632	-0.620	-0.723	-0.948	-1.265	-0.943	-0.436	-0.655
	Convl. std. err.	1.425	1.675	2.123	2.405				
	Convl. p-value	0.657	0.711	0.734	0.693				
	Bias-corr. RD est.	-0.838	-0.656	-0.691	-1.139				
	Robust std. err.	1.693	1.993	2.315	2.601	0.788	0.944	1.252	1.537
	Robust p-value	0.621	0.742	0.765	0.661	0.109	0.318	0.727	0.670
	Bandwidth	0.160	0.165	0.270	0.306				
	Eff. obs. left	487	339	654	460				
	Eff. obs. right	465	285	636	412				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.001	0.121	0.000	0.121
Female	Convl. RD est.	-0.071	-0.052	-0.147	-0.157	-0.072	-0.046	-0.026	-0.029
	Convl. std. err.	0.050	0.052	0.086	0.089				
	Convl. p-value	0.162	0.321	0.088	0.077				
	Bias-corr. RD est.	-0.085	-0.060	-0.153	-0.166				
	Robust std. err.	0.059	0.061	0.096	0.097	0.026	0.029	0.045	0.050
	Robust p-value	0.154	0.322	0.111	0.086	0.005	0.110	0.562	0.569
	Bandwidth	0.185	0.198	0.276	0.260				
	Eff. obs. left	537	371	659	434				
	Eff. obs. right	515	327	640	379				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.005	0.071	0.006	0.070

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Nonhigh	Convl. RD est.	-0.036	0.005	-0.034	0.022	0.010	0.016	-0.002	0.051
	Convl. std. err.	0.053	0.064	0.072	0.090				
	Convl. p-value	0.500	0.942	0.643	0.812				
	Bias-corr. RD est.	-0.044	-0.008	-0.021	0.037				
	Robust std. err.	0.064	0.076	0.078	0.098	0.025	0.030	0.044	0.051
	Robust p-value	0.489	0.914	0.788	0.708	0.687	0.603	0.960	0.321
	Bandwidth	0.142	0.154	0.308	0.316				
	Eff. obs. left	436	324	691	470				
	Eff. obs. right	425	275	670	416				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.087	-0.003	0.084
College	Convl. RD est.	-0.015	0.010	-0.039	0.015	-0.085	-0.126	-0.023	-0.059
	Convl. std. err.	0.072	0.086	0.102	0.127				
	Convl. p-value	0.836	0.904	0.704	0.908				
	Bias-corr. RD est.	-0.009	0.037	-0.053	0.007				
	Robust std. err.	0.087	0.100	0.110	0.138	0.036	0.043	0.060	0.071
	Robust p-value	0.914	0.710	0.630	0.962	0.018	0.003	0.694	0.404
	Bandwidth	0.150	0.144	0.297	0.268				
	Eff. obs. left	462	308	680	436				
	Eff. obs. right	446	259	655	387				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.002	0.084	0.003	0.081
Low-class occupation	Convl. RD est.	-0.033	0.043	-0.067	-0.003	0.036	0.058	0.006	0.063
	Convl. std. err.	0.050	0.051	0.069	0.084				
	Convl. p-value	0.506	0.402	0.332	0.975				
	Bias-corr. RD est.	-0.051	0.037	-0.077	-0.017				
	Robust std. err.	0.058	0.059	0.074	0.091	0.025	0.030	0.043	0.052
	Robust p-value	0.372	0.530	0.301	0.849	0.154	0.058	0.896	0.224
	Bandwidth	0.134	0.197	0.282	0.254				
	Eff. obs. left	384	351	629	406				
	Eff. obs. right	369	303	599	347				
	Observations	1496	982	1496	982	1496	982	1496	982
	Adj. R-squared					0.001	0.128	-0.001	0.124
Middle-class occupation	Convl. RD est.	0.061	0.033	0.044	0.024	0.029	0.008	0.028	0.013
	Convl. std. err.	0.059	0.066	0.085	0.107				
	Convl. p-value	0.300	0.615	0.602	0.819				
	Bias-corr. RD est.	0.070	0.045	0.032	0.018				
	Robust std. err.	0.070	0.077	0.091	0.116	0.032	0.038	0.054	0.066
	Robust p-value	0.317	0.557	0.726	0.877	0.361	0.838	0.603	0.842
	Bandwidth	0.188	0.222	0.372	0.344				
	Eff. obs. left	508	376	686	457				
	Eff. obs. right	481	322	652	396				
	Observations	1496	982	1496	982	1496	982	1496	982
	Adj. R-squared					0.004	0.091	0.002	0.089

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs.	Covariates	No covs.	Covariates	No covs.	Covariates	No covs.	Covariates
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High-class occupation	Convl. RD est.	-0.039	-0.084	0.026	-0.022	-0.065	-0.065	-0.034	-0.076
	Convl. std. err.	0.068	0.072	0.107	0.118				
	Convl. p-value	0.564	0.244	0.805	0.855				
	Bias-corr. RD est.	-0.028	-0.090	0.048	-0.003				
	Robust std. err.	0.081	0.084	0.116	0.128	0.036	0.041	0.061	0.070
	Robust p-value	0.726	0.286	0.680	0.984	0.071	0.112	0.577	0.280
	Bandwidth	0.175	0.211	0.285	0.265				
	Eff. obs. left	486	367	632	414				
	Eff. obs. right	456	315	601	361				
	Observations	1496	982	1496	982	1496	982	1496	982
	Adj. R-squared					0.002	0.202	0.001	0.199
Log declared assets	Convl. RD est.	-0.296	-0.448	-0.335	-0.607	-0.329	-0.327	-0.061	-0.349
	Convl. std. err.	0.186	0.198	0.298	0.293				
	Convl. p-value	0.112	0.024	0.261	0.039				
	Bias-corr. RD est.	-0.336	-0.471	-0.315	-0.651				
	Robust std. err.	0.218	0.229	0.327	0.314	0.110	0.119	0.178	0.189
	Robust p-value	0.123	0.040	0.335	0.038	0.003	0.006	0.730	0.066
	Bandwidth	0.184	0.174	0.271	0.277				
	Eff. obs. left	488	351	592	441				
	Eff. obs. right	471	297	590	393				
	Observations	1468	1038	1468	1038	1468	1038	1468	1038
	Adj. R-squared					0.022	0.178	0.029	0.179
Log campaign revenue	Convl. RD est.	0.009	-0.145	-0.001	-0.060	-0.059	-0.065	0.030	-0.103
	Convl. std. err.	0.099	0.097	0.155	0.163				
	Convl. p-value	0.931	0.133	0.992	0.714				
	Bias-corr. RD est.	0.016	-0.167	-0.006	-0.031				
	Robust std. err.	0.116	0.113	0.169	0.177	0.063	0.049	0.104	0.084
	Robust p-value	0.892	0.141	0.970	0.860	0.347	0.190	0.772	0.220
	Bandwidth	0.191	0.185	0.266	0.275				
	Eff. obs. left	548	364	653	438				
	Eff. obs. right	526	313	633	391				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.635	0.004	0.634
Second term	Convl. RD est.	0.082	-0.008	0.117	0.057	0.040	-0.002	0.068	0.010
	Convl. std. err.	0.053	0.067	0.071	0.094				
	Convl. p-value	0.124	0.909	0.099	0.543				
	Bias-corr. RD est.	0.093	-0.006	0.128	0.077				
	Robust std. err.	0.062	0.079	0.076	0.100	0.030	0.038	0.048	0.061
	Robust p-value	0.135	0.939	0.091	0.439	0.193	0.963	0.154	0.868
	Bandwidth	0.142	0.148	0.288	0.265				
	Eff. obs. left	433	313	674	436				
	Eff. obs. right	425	265	650	386				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.000	0.010	0.005	0.014

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs.	Covariates	No covs.	Covariates	No covs.	Covariates	No covs.	Covariates
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Party ideology	Convl. RD est.	-0.022	-0.010	0.028	0.076	-0.043	-0.029	-0.036	-0.018
	Convl. std. err.	0.049	0.062	0.076	0.098				
	Convl. p-value	0.656	0.871	0.710	0.437				
	Bias-corr. RD est.	-0.017	-0.002	0.039	0.095				
	Robust std. err.	0.057	0.073	0.082	0.104	0.028	0.035	0.046	0.058
	Robust p-value	0.768	0.978	0.630	0.360	0.133	0.402	0.444	0.754
	Bandwidth	0.184	0.152	0.282	0.231				
	Eff. obs. left	535	322	670	408				
	Eff. obs. right	510	271	645	354				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.002	0.056	0.001	0.054
Adherence rate	Convl. RD est.	-1.675	0.098	1.008	4.912	-2.640	-1.404	-2.382	-0.654
	Convl. std. err.	3.358	4.240	5.196	6.548				
	Convl. p-value	0.618	0.982	0.846	0.453				
	Bias-corr. RD est.	-1.367	0.625	1.564	6.047				
	Robust std. err.	3.959	4.947	5.656	6.997	1.953	2.398	3.200	3.974
	Robust p-value	0.730	0.899	0.782	0.387	0.177	0.558	0.457	0.869
	Bandwidth	0.184	0.153	0.296	0.237				
	Eff. obs. left	524	315	667	405				
	Eff. obs. right	501	270	644	354				
	Observations	1570	1023	1570	1023	1570	1023	1570	1023
	Adj. R-squared					0.002	0.058	0.001	0.057
Governor 1 coalition	Convl. RD est.	-0.052	0.002	-0.143	-0.132	0.053	0.046	-0.030	-0.008
	Convl. std. err.	0.068	0.077	0.099	0.119				
	Convl. p-value	0.440	0.976	0.149	0.268				
	Bias-corr. RD est.	-0.071	-0.006	-0.161	-0.159				
	Robust std. err.	0.079	0.091	0.106	0.126	0.036	0.043	0.060	0.073
	Robust p-value	0.368	0.944	0.129	0.206	0.138	0.289	0.617	0.913
	Bandwidth	0.161	0.161	0.295	0.235				
	Eff. obs. left	489	335	680	410				
	Eff. obs. right	467	279	653	357				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.001	0.073	0.002	0.077
Governor 2 coalition	Convl. RD est.	-0.002	-0.048	-0.063	-0.136	0.055	0.037	0.030	-0.042
	Convl. std. err.	0.065	0.073	0.101	0.116				
	Convl. p-value	0.978	0.513	0.533	0.239				
	Bias-corr. RD est.	-0.019	-0.064	-0.077	-0.157				
	Robust std. err.	0.075	0.084	0.110	0.126	0.036	0.042	0.060	0.071
	Robust p-value	0.803	0.444	0.483	0.211	0.130	0.381	0.614	0.557
	Bandwidth	0.174	0.176	0.272	0.269				
	Eff. obs. left	500	340	630	419				
	Eff. obs. right	476	296	620	379				
	Observations	1538	1000	1538	1000	1538	1000	1538	1000
	Adj. R-squared					0.000	0.138	-0.001	0.140

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs.	Covariates	No covs.	Covariates	No covs.	Covariates	No covs.	Covariates
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
President 1 coalition	Convl. RD est.	-0.104	-0.131	-0.167	-0.203	0.002	-0.032	-0.040	-0.058
	Convl. std. err.	0.071	0.080	0.100	0.109				
	Convl. p-value	0.144	0.104	0.093	0.061				
	Bias-corr. RD est.	-0.130	-0.158	-0.182	-0.220				
	Robust std. err.	0.082	0.091	0.108	0.116	0.035	0.043	0.059	0.072
	Robust p-value	0.110	0.084	0.092	0.057	0.965	0.450	0.492	0.418
	Bandwidth	0.144	0.146	0.296	0.330				
	Eff. obs. left	443	311	680	473				
	Eff. obs. right	430	263	654	422				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.038	-0.001	0.037
Dynastic	Convl. RD est.	-0.034	-0.064	-0.005	-0.104	-0.019	-0.035	-0.031	-0.043
	Convl. std. err.	0.069	0.069	0.101	0.106				
	Convl. p-value	0.627	0.353	0.959	0.330				
	Bias-corr. RD est.	-0.027	-0.079	0.005	-0.108				
	Robust std. err.	0.084	0.081	0.110	0.116	0.037	0.038	0.060	0.063
	Robust p-value	0.746	0.333	0.963	0.350	0.597	0.357	0.601	0.500
	Bandwidth	0.159	0.174	0.308	0.305				
	Eff. obs. left	392	350	551	460				
	Eff. obs. right	334	296	482	412				
	Observations	1217	1038	1217	1038	1217	1038	1217	1038
	Adj. R-squared					-0.001	0.090	-0.004	0.087
North	Convl. RD est.	-0.054	-0.052	-0.094	-0.112	0.007	-0.017	0.016	-0.021
	Convl. std. err.	0.050	0.048	0.061	0.066				
	Convl. p-value	0.284	0.278	0.121	0.091				
	Bias-corr. RD est.	-0.075	-0.069	-0.108	-0.124				
	Robust std. err.	0.057	0.055	0.063	0.070	0.023	0.024	0.039	0.042
	Robust p-value	0.186	0.206	0.087	0.077	0.770	0.494	0.680	0.613
	Bandwidth	0.115	0.149	0.309	0.270				
	Eff. obs. left	356	317	692	436				
	Eff. obs. right	358	268	671	388				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.212	-0.002	0.209
Northeast	Convl. RD est.	0.025	0.059	0.097	0.109	-0.019	0.011	0.013	0.051
	Convl. std. err.	0.071	0.061	0.103	0.084				
	Convl. p-value	0.728	0.331	0.348	0.193				
	Bias-corr. RD est.	0.044	0.072	0.115	0.123				
	Robust std. err.	0.084	0.072	0.112	0.089	0.035	0.034	0.059	0.057
	Robust p-value	0.603	0.317	0.303	0.166	0.594	0.747	0.824	0.367
	Bandwidth	0.146	0.163	0.276	0.336				
	Eff. obs. left	449	336	659	477				
	Eff. obs. right	434	281	639	424				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.008	0.417	0.018	0.415

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Southeast	Convl. RD est.	-0.020	0.005	-0.005	0.001	-0.002	-0.024	-0.073	-0.033
	Convl. std. err.	0.052	0.058	0.071	0.079				
	Convl. p-value	0.702	0.930	0.945	0.990				
	Bias-corr. RD est.	-0.009	0.020	0.001	-0.002				
	Robust std. err.	0.061	0.067	0.076	0.085	0.029	0.030	0.047	0.050
	Robust p-value	0.887	0.767	0.992	0.981	0.949	0.431	0.115	0.512
	Bandwidth	0.169	0.146	0.362	0.327				
	Eff. obs. left	505	312	726	473				
	Eff. obs. right	481	263	703	422				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.003	0.309	0.010	0.310
South	Convl. RD est.	0.030	0.024	0.085	0.094	0.014	0.006	0.038	0.018
	Convl. std. err.	0.031	0.037	0.055	0.071				
	Convl. p-value	0.320	0.518	0.122	0.181				
	Bias-corr. RD est.	0.032	0.028	0.100	0.111				
	Robust std. err.	0.038	0.046	0.060	0.076	0.015	0.018	0.027	0.034
	Robust p-value	0.403	0.540	0.095	0.145	0.344	0.758	0.165	0.598
	Bandwidth	0.178	0.179	0.263	0.247				
	Eff. obs. left	523	356	652	420				
	Eff. obs. right	499	303	631	367				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.000	0.103	0.002	0.109
Midwest	Convl. RD est.	-0.014	-0.044	-0.041	-0.056	-0.001	0.024	0.007	-0.015
	Convl. std. err.	0.040	0.042	0.058	0.066				
	Convl. p-value	0.731	0.288	0.483	0.399				
	Bias-corr. RD est.	-0.023	-0.054	-0.052	-0.056				
	Robust std. err.	0.048	0.048	0.062	0.072	0.021	0.025	0.036	0.039
	Robust p-value	0.630	0.263	0.406	0.442	0.979	0.341	0.852	0.711
	Bandwidth	0.163	0.162	0.328	0.273				
	Eff. obs. left	492	336	708	437				
	Eff. obs. right	470	281	684	388				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.144	-0.000	0.142
Area	Convl. RD est.	214.791	-153.381	-195.862	1174.459	-46.938	-276.954	-320.316	-1107.007
	Convl. std. err.	444.448	748.525	926.096	1065.102				
	Convl. p-value	0.629	0.838	0.833	0.270				
	Bias-corr. RD est.	274.496	12.930	-207.011	1278.767				
	Robust std. err.	499.068	781.821	1024.706	1178.291	460.972	469.904	726.545	910.879
	Robust p-value	0.582	0.987	0.840	0.278	0.919	0.556	0.659	0.225
	Bandwidth	0.128	0.206	0.208	0.209				
	Eff. obs. left	394	382	570	386				
	Eff. obs. right	389	337	556	338				
	Observations	1595	1038	1595	1038	1595	1038	1595	1038
	Adj. R-squared					-0.001	0.156	-0.003	0.154

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Urbanization	Convl. RD est.	2.009	0.702	3.766	1.167	-0.663	0.338	-0.561	-1.190
	Convl. std. err.	2.721	1.833	3.567	2.498				
	Convl. p-value	0.460	0.702	0.291	0.640				
	Bias-corr. RD est.	2.849	1.000	4.074	0.867				
	Robust std. err.	3.145	2.152	3.848	2.662	1.431	1.165	2.390	1.852
	Robust p-value	0.365	0.642	0.290	0.745	0.643	0.771	0.815	0.521
	Bandwidth	0.154	0.164	0.350	0.336				
	Eff. obs. left	470	337	722	477				
	Eff. obs. right	456	282	696	424				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.611	0.010	0.610
Metropolitan Area	Convl. RD est.	0.088	0.076	0.110	0.069	0.013	0.007	0.039	0.025
	Convl. std. err.	0.040	0.047	0.052	0.064				
	Convl. p-value	0.028	0.104	0.035	0.281				
	Bias-corr. RD est.	0.102	0.088	0.105	0.056				
	Robust std. err.	0.046	0.054	0.056	0.068	0.021	0.023	0.035	0.040
	Robust p-value	0.026	0.104	0.060	0.413	0.520	0.752	0.262	0.537
	Bandwidth	0.132	0.147	0.298	0.268				
	Eff. obs. left	403	312	681	436				
	Eff. obs. right	398	263	658	387				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.197	0.005	0.196
Log population	Convl. RD est.	0.241	0.217	0.308	0.422	0.033	0.065	0.249	0.231
	Convl. std. err.	0.133	0.132	0.207	0.203				
	Convl. p-value	0.069	0.100	0.136	0.037				
	Bias-corr. RD est.	0.261	0.240	0.325	0.463				
	Robust std. err.	0.158	0.158	0.227	0.215	0.075	0.071	0.126	0.118
	Robust p-value	0.097	0.128	0.153	0.031	0.660	0.365	0.049	0.051
	Bandwidth	0.166	0.151	0.275	0.247				
	Eff. obs. left	497	321	659	419				
	Eff. obs. right	476	270	638	367				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.485	0.013	0.490
Nonwhite pop.	Convl. RD est.	-0.002	-0.007	-0.047	-0.017	0.001	0.008	0.008	0.001
	Convl. std. err.	0.020	0.016	0.034	0.027				
	Convl. p-value	0.924	0.690	0.168	0.533				
	Bias-corr. RD est.	-0.005	-0.011	-0.055	-0.017				
	Robust std. err.	0.023	0.019	0.037	0.029	0.012	0.010	0.019	0.017
	Robust p-value	0.818	0.552	0.139	0.555	0.937	0.398	0.690	0.945
	Bandwidth	0.210	0.198	0.267	0.299				
	Eff. obs. left	572	371	653	456				
	Eff. obs. right	557	327	633	405				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.001	0.513	0.004	0.514

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Mean age	Convl. RD est.	-0.074	0.081	1.211	0.735	-0.054	-0.134	-0.186	0.047
	Convl. std. err.	0.370	0.291	0.647	0.418				
	Convl. p-value	0.841	0.781	0.061	0.079				
	Bias-corr. RD est.	0.011	0.189	1.374	0.802				
	Robust std. err.	0.432	0.334	0.695	0.443	0.203	0.168	0.342	0.274
	Robust p-value	0.980	0.572	0.048	0.071	0.789	0.424	0.587	0.863
	Bandwidth	0.194	0.159	0.241	0.284				
	Eff. obs. left	552	333	623	449				
	Eff. obs. right	532	278	597	398				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.537	-0.002	0.538
Average salary	Convl. RD est.	14.279	-8.699	-9.427	-10.558	-6.945	-12.664	15.629	-12.141
	Convl. std. err.	34.118	20.790	53.870	34.496				
	Convl. p-value	0.676	0.676	0.861	0.760				
	Bias-corr. RD est.	11.888	-5.760	-17.100	-13.245				
	Robust std. err.	41.004	24.754	59.446	38.325	18.329	11.752	30.697	19.822
	Robust p-value	0.772	0.816	0.774	0.730	0.705	0.281	0.611	0.540
	Bandwidth	0.165	0.205	0.274	0.332				
	Eff. obs. left	494	380	658	474				
	Eff. obs. right	474	337	637	424				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.002	0.785	0.022	0.784
Racial salary gap	Convl. RD est.	0.009	0.005	0.015	0.020	0.009	0.005	0.014	0.004
	Convl. std. err.	0.013	0.013	0.018	0.020				
	Convl. p-value	0.459	0.717	0.431	0.326				
	Bias-corr. RD est.	0.010	0.006	0.017	0.022				
	Robust std. err.	0.015	0.015	0.020	0.021	0.007	0.008	0.011	0.013
	Robust p-value	0.514	0.706	0.398	0.296	0.206	0.504	0.189	0.775
	Bandwidth	0.152	0.184	0.296	0.304				
	Eff. obs. left	468	363	680	459				
	Eff. obs. right	451	308	655	412				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.000	0.022	0.001	0.020
Gender salary gap	Convl. RD est.	0.010	0.029	0.003	0.013	0.009	0.011	0.009	0.023
	Convl. std. err.	0.019	0.023	0.029	0.036				
	Convl. p-value	0.592	0.205	0.915	0.723				
	Bias-corr. RD est.	0.007	0.028	0.003	0.010				
	Robust std. err.	0.023	0.028	0.031	0.039	0.010	0.012	0.017	0.021
	Robust p-value	0.758	0.316	0.922	0.790	0.321	0.348	0.582	0.275
	Bandwidth	0.179	0.182	0.318	0.294				
	Eff. obs. left	523	359	705	456				
	Eff. obs. right	502	305	675	401				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.001	0.026	0.002	0.024

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Gini	Convl. RD est.	0.003	0.002	-0.004	-0.004	0.003	0.002	0.011	0.003
	Convl. std. err.	0.007	0.007	0.010	0.012				
	Convl. p-value	0.637	0.815	0.657	0.751				
	Bias-corr. RD est.	0.002	0.002	-0.006	-0.005				
	Robust std. err.	0.008	0.009	0.010	0.013	0.004	0.004	0.007	0.007
	Robust p-value	0.833	0.792	0.548	0.676	0.523	0.616	0.086	0.700
	Bandwidth	0.157	0.174	0.332	0.307				
	Eff. obs. left	480	351	711	463				
	Eff. obs. right	461	297	688	413				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.325	0.003	0.327
Illiteracy	Convl. RD est.	0.003	1.579	2.107	3.219	-0.213	0.340	-0.260	1.007
	Convl. std. err.	1.474	1.053	2.189	1.525				
	Convl. p-value	0.998	0.134	0.336	0.035				
	Bias-corr. RD est.	0.363	1.884	2.550	3.428				
	Robust std. err.	1.733	1.183	2.358	1.623	0.759	0.549	1.288	0.952
	Robust p-value	0.834	0.111	0.279	0.035	0.779	0.536	0.840	0.291
	Bandwidth	0.150	0.165	0.253	0.281				
	Eff. obs. left	461	339	642	447				
	Eff. obs. right	446	285	614	397				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.002	0.688	0.019	0.687
High school pop.	Convl. RD est.	0.347	-0.915	-0.514	-0.899	-0.497	-0.318	0.421	-0.559
	Convl. std. err.	0.975	0.829	1.487	1.138				
	Convl. p-value	0.721	0.270	0.730	0.430				
	Bias-corr. RD est.	0.352	-1.026	-0.718	-0.823				
	Robust std. err.	1.158	0.971	1.613	1.211	0.555	0.455	0.940	0.783
	Robust p-value	0.761	0.291	0.656	0.497	0.371	0.485	0.655	0.475
	Bandwidth	0.175	0.162	0.283	0.296				
	Eff. obs. left	517	336	672	456				
	Eff. obs. right	492	281	646	402				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.000	0.610	0.020	0.610
College pop.	Convl. RD est.	-0.017	-0.144	-0.253	-0.129	-0.169	-0.207	-0.067	-0.137
	Convl. std. err.	0.300	0.240	0.446	0.355				
	Convl. p-value	0.955	0.549	0.570	0.716				
	Bias-corr. RD est.	0.028	-0.115	-0.327	-0.144				
	Robust std. err.	0.351	0.279	0.479	0.383	0.170	0.150	0.290	0.250
	Robust p-value	0.937	0.680	0.494	0.707	0.320	0.167	0.818	0.583
	Bandwidth	0.178	0.190	0.283	0.294				
	Eff. obs. left	523	367	671	456				
	Eff. obs. right	499	320	646	400				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.000	0.534	0.011	0.532

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs.	Covariates	No covs.	Covariates	No covs.	Covariates	No covs.	Covariates
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Water	Convl. RD est.	0.358	0.876	0.766	-1.246	0.134	0.955	0.246	2.769
	Convl. std. err.	2.634	2.262	3.838	3.527				
	Convl. p-value	0.892	0.698	0.842	0.724				
	Bias-corr. RD est.	0.427	0.473	1.278	-1.661				
	Robust std. err.	3.137	2.624	4.173	3.815	1.496	1.413	2.414	2.296
	Robust p-value	0.892	0.857	0.759	0.663	0.929	0.499	0.919	0.228
	Bandwidth	0.161	0.187	0.304	0.291				
	Eff. obs. left	489	364	687	453				
	Eff. obs. right	468	316	665	400				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.490	-0.000	0.491
Light	Convl. RD est.	0.288	-0.731	0.764	-1.636	-0.023	0.382	0.284	0.240
	Convl. std. err.	1.130	0.955	1.894	1.543				
	Convl. p-value	0.799	0.444	0.686	0.289				
	Bias-corr. RD est.	0.346	-1.036	1.023	-1.796				
	Robust std. err.	1.415	1.109	2.129	1.684	0.474	0.461	0.895	0.837
	Robust p-value	0.807	0.350	0.631	0.286	0.962	0.407	0.751	0.774
	Bandwidth	0.171	0.175	0.293	0.274				
	Eff. obs. left	510	352	678	437				
	Eff. obs. right	485	298	651	389				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.352	-0.002	0.350
Sewer	Convl. RD est.	-2.025	-3.304	-2.346	-8.349	-0.405	0.429	-3.886	-3.206
	Convl. std. err.	3.676	2.877	5.467	4.601				
	Convl. p-value	0.582	0.251	0.668	0.070				
	Bias-corr. RD est.	-1.819	-4.125	-2.748	-9.254				
	Robust std. err.	4.375	3.319	5.952	4.928	1.990	1.695	3.260	2.742
	Robust p-value	0.678	0.214	0.644	0.060	0.839	0.800	0.233	0.243
	Bandwidth	0.162	0.183	0.279	0.267				
	Eff. obs. left	491	361	667	436				
	Eff. obs. right	469	307	643	386				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.563	0.005	0.563
Public employees pc 2015	Convl. RD est.	-0.004	-0.002	-0.006	-0.005	-0.001	-0.000	-0.004	-0.002
	Convl. std. err.	0.003	0.002	0.004	0.003				
	Convl. p-value	0.178	0.301	0.153	0.143				
	Bias-corr. RD est.	-0.004	-0.003	-0.007	-0.006				
	Robust std. err.	0.003	0.003	0.005	0.004	0.001	0.001	0.002	0.002
	Robust p-value	0.169	0.334	0.151	0.135	0.550	0.734	0.089	0.262
	Bandwidth	0.202	0.160	0.321	0.252				
	Eff. obs. left	560	334	706	426				
	Eff. obs. right	543	279	674	368				
	Observations	1593	1038	1593	1038	1593	1038	1593	1038
	Adj. R-squared					-0.000	0.534	0.010	0.535

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Temp. pub. employees 2015	Convl. RD est.	1.482	1.333	1.999	2.629	0.943	1.426	0.585	0.964
	Convl. std. err.	2.227	2.362	3.113	3.759				
	Convl. p-value	0.506	0.573	0.521	0.484				
	Bias-corr. RD est.	1.870	1.381	1.891	2.953				
	Robust std. err.	2.639	2.783	3.415	4.066	1.257	1.400	2.009	2.313
	Robust p-value	0.479	0.620	0.580	0.468	0.453	0.308	0.771	0.677
	Bandwidth	0.171	0.190	0.358	0.278				
	Eff. obs. left	510	367	724	442				
	Eff. obs. right	487	320	701	394				
	Observations	1593	1038	1593	1038	1593	1038	1593	1038
	Adj. R-squared					0.001	0.116	0.013	0.121
GDPpc 2016	Convl. RD est.	1083.295	801.336	258.850	-2211.198	356.961	331.861	825.400	811.523
	Convl. std. err.	1048.206	973.846	1375.206	1486.262				
	Convl. p-value	0.301	0.411	0.851	0.137				
	Bias-corr. RD est.	1206.323	1029.490	-14.741	-2611.982				
	Robust std. err.	1218.482	1070.060	1474.210	1621.419	801.514	788.448	1092.548	1106.276
	Robust p-value	0.322	0.336	0.992	0.107	0.656	0.674	0.450	0.463
	Bandwidth	0.145	0.167	0.328	0.209				
	Eff. obs. left	445	340	708	385				
	Eff. obs. right	430	286	683	338				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.000	0.242	0.005	0.241
Infant mortality 2016	Convl. RD est.	-14.927	-20.875	26.332	22.626	-22.113	-13.593	-33.462	-37.824
	Convl. std. err.	12.984	13.386	13.329	17.458				
	Convl. p-value	0.250	0.119	0.048	0.195				
	Bias-corr. RD est.	-12.881	-18.404	28.007	25.287				
	Robust std. err.	14.529	15.426	14.641	19.259	13.403	16.365	16.722	19.046
	Robust p-value	0.375	0.233	0.056	0.189	0.099	0.406	0.046	0.047
	Bandwidth	0.162	0.154	0.193	0.202				
	Eff. obs. left	342	244	377	275				
	Eff. obs. right	338	196	385	242				
	Observations	1152	769	1152	769	1152	769	1152	769
	Adj. R-squared					0.003	0.022	0.005	0.034
7+ prenatal visits 2016	Convl. RD est.	2.478	1.800	1.799	0.080	3.819	5.251	1.672	2.555
	Convl. std. err.	4.264	4.845	6.855	6.402				
	Convl. p-value	0.561	0.710	0.793	0.990				
	Bias-corr. RD est.	2.357	0.452	1.838	-0.948				
	Robust std. err.	5.091	5.700	7.542	6.990	2.491	2.737	4.109	4.464
	Robust p-value	0.643	0.937	0.807	0.892	0.126	0.055	0.684	0.567
	Bandwidth	0.187	0.139	0.289	0.329				
	Eff. obs. left	371	220	471	355				
	Eff. obs. right	378	179	475	305				
	Observations	1152	769	1152	769	1152	769	1152	769
	Adj. R-squared					0.001	0.176	-0.000	0.174

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Low birth weight 2016	Convl. RD est.	-3.683	0.379	-1.673	6.609	-0.701	0.489	-3.158	-0.073
	Convl. std. err.	2.926	3.514	5.921	7.959				
	Convl. p-value	0.208	0.914	0.777	0.406				
	Bias-corr. RD est.	-4.246	0.577	-0.576	8.244				
	Robust std. err.	3.657	4.326	6.743	8.893	1.501	1.948	2.440	3.167
	Robust p-value	0.246	0.894	0.932	0.354	0.640	0.802	0.196	0.982
	Bandwidth	0.195	0.216	0.281	0.259				
	Eff. obs. left	378	289	467	322				
	Eff. obs. right	389	247	471	273				
	Observations	1152	769	1152	769	1152	769	1152	769
	Adj. R-squared					-0.001	0.058	-0.002	0.054
Mortality fr. violence 2016	Convl. RD est.	21.753	5.772	15.201	1.304	-6.094	-12.471	47.847	17.658
	Convl. std. err.	35.266	40.912	50.257	54.964				
	Convl. p-value	0.537	0.888	0.762	0.981				
	Bias-corr. RD est.	16.218	3.819	16.141	1.003				
	Robust std. err.	41.990	49.410	54.304	58.838	18.452	20.031	31.015	33.781
	Robust p-value	0.699	0.938	0.766	0.986	0.741	0.534	0.123	0.601
	Bandwidth	0.162	0.152	0.307	0.360				
	Eff. obs. left	489	322	691	484				
	Eff. obs. right	468	271	668	430				
	Observations	1595	1038	1595	1038	1595	1038	1595	1038
	Adj. R-squared					-0.001	0.217	0.000	0.217
Mort. fr. violence nonwhite 2016	Convl. RD est.	25.280	-5.234	13.508	4.416	-17.222	-22.099	58.706	19.528
	Convl. std. err.	45.777	53.046	62.458	77.830				
	Convl. p-value	0.581	0.921	0.829	0.955				
	Bias-corr. RD est.	16.923	-7.426	10.982	8.040				
	Robust std. err.	54.244	63.698	67.206	83.668	23.990	26.780	40.802	44.898
	Robust p-value	0.755	0.907	0.870	0.923	0.473	0.409	0.150	0.664
	Bandwidth	0.174	0.158	0.374	0.290				
	Eff. obs. left	516	329	730	453				
	Eff. obs. right	490	276	706	400				
	Observations	1595	1038	1595	1038	1595	1038	1595	1038
	Adj. R-squared					-0.001	0.234	0.001	0.234
Class size	Convl. RD est.	1.152	0.556	1.581	0.906	0.202	0.131	0.248	-0.250
	Convl. std. err.	0.692	0.651	0.861	0.859				
	Convl. p-value	0.096	0.393	0.066	0.292				
	Bias-corr. RD est.	1.389	0.771	1.654	0.993				
	Robust std. err.	0.793	0.767	0.927	0.930	0.344	0.358	0.560	0.574
	Robust p-value	0.080	0.315	0.074	0.286	0.557	0.715	0.657	0.663
	Bandwidth	0.138	0.152	0.357	0.374				
	Eff. obs. left	420	341	715	527				
	Eff. obs. right	405	296	691	462				
	Observations	1572	1108	1572	1108	1572	1108	1572	1108
	Adj. R-squared					-0.001	0.252	-0.002	0.250

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs.	Covariates	No covs.	Covariates	No covs.	Covariates	No covs.	Covariates
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age delay 2016	Convl. RD est.	0.911	-0.202	2.270	0.832	0.611	0.902	1.642	-0.564
	Convl. std. err.	1.734	1.731	3.091	2.662				
	Convl. p-value	0.599	0.907	0.463	0.755				
	Bias-corr. RD est.	0.912	0.005	2.363	0.890				
	Robust std. err.	2.029	2.048	3.402	2.926	1.060	0.994	1.779	1.688
	Robust p-value	0.653	0.998	0.487	0.761	0.564	0.364	0.356	0.738
	Bandwidth	0.240	0.175	0.280	0.302				
	Eff. obs. left	615	376	662	496				
	Eff. obs. right	592	323	638	438				
	Observations	1583	1113	1583	1113	1583	1113	1583	1113
	Adj. R-squared					0.004	0.432	0.003	0.432
Portuguese score 2015	Convl. RD est.	-0.214	-0.249	1.816	0.178	-0.304	-1.027	-1.825	0.121
	Convl. std. err.	2.775	2.664	4.119	4.457				
	Convl. p-value	0.938	0.926	0.659	0.968				
	Bias-corr. RD est.	0.261	-0.365	2.371	0.521				
	Robust std. err.	3.276	3.215	4.454	4.937	1.516	1.467	2.593	2.598
	Robust p-value	0.937	0.910	0.594	0.916	0.841	0.484	0.482	0.963
	Bandwidth	0.177	0.188	0.308	0.299				
	Eff. obs. left	483	365	638	456				
	Eff. obs. right	458	319	618	405				
	Observations	1476	1038	1476	1038	1476	1038	1476	1038
	Adj. R-squared					0.002	0.377	0.006	0.374
Math score 2015	Convl. RD est.	0.291	1.559	3.160	3.269	0.409	0.095	-1.326	1.413
	Convl. std. err.	2.761	2.460	4.398	4.418				
	Convl. p-value	0.916	0.526	0.472	0.459				
	Bias-corr. RD est.	0.770	1.695	3.839	3.685				
	Robust std. err.	3.265	2.923	4.776	4.856	1.524	1.477	2.625	2.624
	Robust p-value	0.814	0.562	0.421	0.448	0.788	0.949	0.613	0.590
	Bandwidth	0.191	0.217	0.293	0.270				
	Eff. obs. left	506	392	627	436				
	Eff. obs. right	483	344	601	388				
	Observations	1476	1038	1476	1038	1476	1038	1476	1038
	Adj. R-squared					0.003	0.371	0.006	0.369
Social prgm. registration 2016	Convl. RD est.	-0.007	-0.010	-0.007	-0.007	-0.003	-0.003	-0.007	-0.010
	Convl. std. err.	0.008	0.006	0.012	0.010				
	Convl. p-value	0.345	0.076	0.548	0.464				
	Bias-corr. RD est.	-0.007	-0.012	-0.009	-0.006				
	Robust std. err.	0.009	0.007	0.013	0.010	0.004	0.003	0.007	0.006
	Robust p-value	0.451	0.068	0.508	0.548	0.480	0.354	0.303	0.087
	Bandwidth	0.179	0.213	0.270	0.303				
	Eff. obs. left	523	387	654	459				
	Eff. obs. right	500	340	636	410				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.004	0.606	0.021	0.605

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Table A.6 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs.	Covariates	No covs.	Covariates	No covs.	Covariates	No covs.	Covariates
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cash transf. beneficiaries 2016	Convl. RD est.	-0.002	-0.004	0.001	-0.001	-0.002	-0.002	-0.001	-0.003
	Convl. std. err.	0.007	0.004	0.010	0.006				
	Convl. p-value	0.760	0.272	0.899	0.830				
	Bias-corr. RD est.	-0.001	-0.004	0.001	-0.001				
	Robust std. err.	0.008	0.005	0.012	0.006	0.004	0.002	0.006	0.004
	Robust p-value	0.877	0.348	0.915	0.845	0.672	0.378	0.813	0.365
	Bandwidth	0.170	0.185	0.275	0.278				
	Eff. obs. left	507	363	659	442				
	Eff. obs. right	484	313	639	394				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.005	0.757	0.026	0.756

Table A.7 – Full results of post-treatment variables

This table presents full results for all post-treatment variables considered in this work. Nonparametric estimations are the local linear regression (LLR, models (1) and (2)), and the local polynomial regression of third degree (LPR 3^{rd}, models (3) and (4)). They are performed by the algorithm of Calonico, Cattaneo and Titiunik (2014a). Parametric (global) estimations are the ordinary least squares regression (OLS, models (5) and (6)) and the polynomial third degree model with terms of interaction between the treatment and the forcing variable (Pol. 3^{rd} interac., models (7) and (8)). Odd-index models do not include covariates, while even-index models do (covariates list is indicated in Table A.3). For nonparametric models, statistics consider the bias-correction and robust variance estimation, unless they are identified as "conventional". When referring to parametric models, "robust" statistics simply indicate the usual heteroscedasticity correction. Treatment variable is color/race Nonwhite. The dependent variable is indicated in the first column (detailed definitions of variables are given in Table A.3).

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Reelected in 2020	Convl. RD est.	-0.027	-0.187	-0.034	-0.258	-0.008	-0.036	-0.037	-0.113
	Convl. std. err.	0.094	0.124	0.133	0.170				
	Convl. p-value	0.772	0.132	0.799	0.129				
	Bias-corr. RD est.	-0.023	-0.216	-0.037	-0.279				
	Robust std. err.	0.113	0.145	0.145	0.183	0.049	0.061	0.083	0.101
	Robust p-value	0.840	0.135	0.800	0.128	0.865	0.558	0.655	0.266
	Bandwidth	0.177	0.143	0.352	0.293				
	Eff. obs. left	288	172	402	254				
	Eff. obs. right	261	142	373	211				
	Observations	865	559	865	559	865	559	865	559
	Adj. R-squared					-0.002	0.014	0.009	0.036
Growth GDPpc	Convl. RD est.	4.934	10.215	3.125	12.174	3.335	2.185	4.624	8.888
	Convl. std. err.	4.602	4.815	8.618	9.320				
	Convl. p-value	0.284	0.034	0.717	0.191				
	Bias-corr. RD est.	5.390	12.229	2.365	12.192				
	Robust std. err.	5.753	5.848	9.824	10.583	2.367	2.249	3.801	4.438
	Robust p-value	0.349	0.036	0.810	0.249	0.159	0.331	0.224	0.045
	Bandwidth	0.180	0.203	0.274	0.313				
	Eff. obs. left	525	379	658	464				
	Eff. obs. right	503	335	636	414				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.001	0.004	0.006	0.007

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Table A.7 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs.	Covariates	No covs.	Covariates	No covs.	Covariates	No covs.	Covariates
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Privatization	Convl. RD est.	-0.069	-0.092	-0.084	-0.109	0.001	-0.008	-0.066	-0.047
	Convl. std. err.	0.025	0.030	0.038	0.047				
	Convl. p-value	0.005	0.002	0.026	0.019				
	Bias-corr. RD est.	-0.076	-0.104	-0.086	-0.109				
	Robust std. err.	0.028	0.034	0.042	0.052	0.017	0.022	0.026	0.032
	Robust p-value	0.006	0.002	0.042	0.034	0.966	0.731	0.011	0.149
	Bandwidth	0.135	0.127	0.253	0.247				
	Eff. obs. left	412	269	640	417				
	Eff. obs. right	403	231	612	366				
	Observations	1594	1036	1594	1036	1594	1036	1594	1036
	Adj. R-squared					-0.001	0.011	0.010	0.018
Δ% temp. pub. employees	Convl. RD est.	-1.785	-11.741	4.129	-7.114	1.772	-6.544	-4.483	-25.869
	Convl. std. err.	10.321	9.415	15.662	15.246				
	Convl. p-value	0.863	0.212	0.792	0.641				
	Bias-corr. RD est.	-4.775	-13.111	6.113	-4.643				
	Robust std. err.	11.709	11.086	17.105	16.456	10.346	12.477	16.238	20.714
	Robust p-value	0.683	0.237	0.721	0.778	0.864	0.600	0.783	0.212
	Bandwidth	0.130	0.108	0.229	0.210				
	Eff. obs. left	397	230	594	380				
	Eff. obs. right	390	206	580	334				
	Observations	1579	1027	1579	1027	1579	1027	1579	1027
	Adj. R-squared					-0.000	-0.004	0.001	-0.002
Log own tax revenues pc	Convl. RD est.	0.119	0.106	0.106	0.188	0.080	0.041	0.106	0.039
	Convl. std. err.	0.104	0.094	0.150	0.137				
	Convl. p-value	0.253	0.262	0.481	0.171				
	Bias-corr. RD est.	0.124	0.137	0.098	0.214				
	Robust std. err.	0.124	0.110	0.164	0.147	0.057	0.051	0.093	0.086
	Robust p-value	0.320	0.212	0.551	0.144	0.159	0.422	0.251	0.652
	Bandwidth	0.156	0.174	0.297	0.336				
	Eff. obs. left	473	349	676	473				
	Eff. obs. right	458	296	656	423				
	Observations	1591	1033	1591	1033	1591	1033	1591	1033
	Adj. R-squared					0.005	0.481	0.019	0.480
Log current expenditures pc	Convl. RD est.	-0.053	-0.038	-0.104	-0.075	0.010	-0.002	-0.042	-0.030
	Convl. std. err.	0.047	0.034	0.067	0.047				
	Convl. p-value	0.256	0.256	0.119	0.112				
	Bias-corr. RD est.	-0.065	-0.046	-0.115	-0.084				
	Robust std. err.	0.055	0.039	0.072	0.051	0.026	0.018	0.041	0.031
	Robust p-value	0.235	0.242	0.109	0.095	0.705	0.906	0.313	0.336
	Bandwidth	0.152	0.149	0.295	0.286				
	Eff. obs. left	467	314	677	446				
	Eff. obs. right	449	266	652	397				
	Observations	1592	1034	1592	1034	1592	1034	1592	1034
	Adj. R-squared					-0.001	0.607	0.003	0.609

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Table A.7 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs.	Covariates	No covs.	Covariates	No covs.	Covariates	No covs.	Covariates
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log investments pc	Convl. RD est.	-0.071	-0.027	-0.130	0.051	0.031	0.004	-0.090	-0.102
	Convl. std. err.	0.099	0.100	0.149	0.149				
	Convl. p-value	0.478	0.785	0.384	0.732				
	Bias-corr. RD est.	-0.092	-0.011	-0.141	0.074				
	Robust std. err.	0.117	0.120	0.162	0.161	0.054	0.052	0.091	0.090
	Robust p-value	0.434	0.929	0.383	0.644	0.562	0.941	0.324	0.256
	Bandwidth	0.177	0.156	0.306	0.286				
	Eff. obs. left	517	326	685	447				
	Eff. obs. right	497	274	666	397				
	Observations	1592	1034	1592	1034	1592	1034	1592	1034
	Adj. R-squared					0.002	0.340	0.006	0.341
Budget balance pc	Convl. RD est.	-504.323	-116.718	2.780	505.883	-282.268	-402.986	-469.988	-881.038
	Convl. std. err.	711.059	707.160	1157.566	1071.868				
	Convl. p-value	0.478	0.869	0.998	0.637				
	Bias-corr. RD est.	-542.840	140.622	144.242	578.587				
	Robust std. err.	848.145	831.712	1244.533	1164.097	388.010	454.461	657.421	686.366
	Robust p-value	0.522	0.866	0.908	0.619	0.467	0.375	0.475	0.200
	Bandwidth	0.203	0.163	0.289	0.296				
	Eff. obs. left	565	336	677	456				
	Eff. obs. right	548	280	649	401				
	Observations	1595	1037	1595	1037	1595	1037	1595	1037
	Adj. R-squared					0.000	0.143	-0.001	0.143
Log current exp. pc 2020	Convl. RD est.	0.001	0.138	-0.382	-0.190	0.125	0.180	0.384	0.561
	Convl. std. err.	0.358	0.390	0.514	0.568				
	Convl. p-value	0.999	0.723	0.458	0.739				
	Bias-corr. RD est.	-0.109	0.034	-0.462	-0.252				
	Robust std. err.	0.417	0.451	0.563	0.613	0.178	0.214	0.299	0.343
	Robust p-value	0.794	0.941	0.411	0.681	0.482	0.400	0.200	0.102
	Bandwidth	0.147	0.139	0.290	0.246				
	Eff. obs. left	450	296	677	419				
	Eff. obs. right	434	250	650	365				
	Observations	1595	1037	1595	1037	1595	1037	1595	1037
	Adj. R-squared					-0.001	0.054	0.001	0.053
Log investments pc 2020	Convl. RD est.	-0.119	0.031	-0.414	-0.224	0.093	0.106	0.129	0.267
	Convl. std. err.	0.274	0.298	0.395	0.439				
	Convl. p-value	0.665	0.916	0.295	0.610				
	Bias-corr. RD est.	-0.207	-0.057	-0.470	-0.243				
	Robust std. err.	0.319	0.343	0.432	0.478	0.133	0.158	0.225	0.259
	Robust p-value	0.515	0.869	0.277	0.610	0.486	0.501	0.567	0.304
	Bandwidth	0.147	0.148	0.285	0.256				
	Eff. obs. left	449	314	672	430				
	Eff. obs. right	433	264	646	374				
	Observations	1595	1037	1595	1037	1595	1037	1595	1037
	Adj. R-squared					-0.000	0.078	-0.000	0.076

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Table A.7 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Log discretionary transfers pc	Convl. RD est.	0.012	-0.014	-0.096	-0.030	0.032	0.011	-0.007	-0.015
	Convl. std. err.	0.045	0.040	0.080	0.069				
	Convl. p-value	0.786	0.733	0.225	0.664				
	Bias-corr. RD est.	0.014	-0.018	-0.118	-0.042				
	Robust std. err.	0.053	0.047	0.085	0.075	0.028	0.027	0.043	0.041
	Robust p-value	0.787	0.707	0.166	0.577	0.265	0.693	0.865	0.712
	Bandwidth	0.161	0.210	0.224	0.250				
	Eff. obs. left	487	384	592	419				
	Eff. obs. right	465	337	576	367				
	Observations	1591	1033	1591	1033	1591	1033	1591	1033
	Adj. R-squared					-0.000	0.267	0.006	0.270
Log security exp. pc	Convl. RD est.	0.295	-0.355	-0.013	-0.883	0.114	0.055	0.237	0.460
	Convl. std. err.	0.367	0.476	0.528	0.656				
	Convl. p-value	0.421	0.456	0.981	0.178				
	Bias-corr. RD est.	0.315	-0.486	-0.098	-0.996				
	Robust std. err.	0.433	0.543	0.568	0.682	0.203	0.246	0.345	0.432
	Robust p-value	0.467	0.370	0.864	0.144	0.575	0.824	0.492	0.288
	Bandwidth	0.180	0.131	0.310	0.215				
	Eff. obs. left	223	117	300	164				
	Eff. obs. right	189	82	266	124				
	Observations	667	422	667	422	667	422	667	422
	Adj. R-squared					0.001	0.081	0.002	0.074
Log health and sanitation exp. pc	Convl. RD est.	-0.045	0.011	-0.059	0.030	0.012	0.019	-0.049	-0.012
	Convl. std. err.	0.052	0.042	0.074	0.065				
	Convl. p-value	0.395	0.801	0.419	0.641				
	Bias-corr. RD est.	-0.050	0.014	-0.057	0.034				
	Robust std. err.	0.062	0.049	0.080	0.070	0.028	0.023	0.047	0.040
	Robust p-value	0.418	0.769	0.474	0.629	0.658	0.421	0.296	0.764
	Bandwidth	0.168	0.191	0.332	0.279				
	Eff. obs. left	501	368	711	447				
	Eff. obs. right	477	320	687	394				
	Observations	1595	1037	1595	1037	1595	1037	1595	1037
	Adj. R-squared					-0.000	0.517	0.003	0.517
Log education and culture exp. pc	Convl. RD est.	-0.036	-0.039	-0.162	-0.083	0.010	-0.012	-0.035	-0.038
	Convl. std. err.	0.041	0.039	0.069	0.054				
	Convl. p-value	0.382	0.328	0.019	0.125				
	Bias-corr. RD est.	-0.047	-0.042	-0.179	-0.090				
	Robust std. err.	0.047	0.046	0.073	0.058	0.025	0.022	0.041	0.038
	Robust p-value	0.318	0.354	0.015	0.117	0.697	0.599	0.391	0.321
	Bandwidth	0.193	0.158	0.245	0.285				
	Eff. obs. left	551	333	629	449				
	Eff. obs. right	528	277	604	397				
	Observations	1595	1037	1595	1037	1595	1037	1595	1037
	Adj. R-squared					-0.001	0.459	0.008	0.460

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Table A.7 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs.	Covariates	No covs.	Covariates	No covs.	Covariates	No covs.	Covariates
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log urban and housing exp. pc	Convl. RD est.	-0.043	-0.089	-0.040	-0.180	-0.020	-0.036	-0.035	-0.034
	Convl. std. err.	0.102	0.115	0.165	0.188				
	Convl. p-value	0.671	0.439	0.807	0.337				
	Bias-corr. RD est.	-0.044	-0.096	-0.046	-0.204				
	Robust std. err.	0.119	0.135	0.178	0.204	0.066	0.071	0.108	0.119
	Robust p-value	0.715	0.476	0.795	0.318	0.759	0.614	0.746	0.777
	Bandwidth	0.223	0.218	0.295	0.349				
	Eff. obs. left	587	391	676	479				
	Eff. obs. right	574	344	651	425				
	Observations	1589	1033	1589	1033	1589	1033	1589	1033
	Adj. R-squared					-0.001	0.209	-0.002	0.208
Log administrative exp. pc	Convl. RD est.	-0.014	0.012	-0.035	0.013	0.026	0.027	-0.073	-0.047
	Convl. std. err.	0.077	0.068	0.104	0.109				
	Convl. p-value	0.855	0.861	0.738	0.905				
	Bias-corr. RD est.	-0.016	0.015	-0.042	0.002				
	Robust std. err.	0.092	0.081	0.112	0.119	0.043	0.037	0.069	0.062
	Robust p-value	0.857	0.858	0.707	0.989	0.546	0.469	0.291	0.451
	Bandwidth	0.164	0.192	0.349	0.287				
	Eff. obs. left	492	369	722	450				
	Eff. obs. right	471	321	694	397				
	Observations	1595	1037	1595	1037	1595	1037	1595	1037
	Adj. R-squared					0.000	0.491	0.005	0.493
Log social assistance exp. pc	Convl. RD est.	-0.099	0.004	-0.182	-0.048	-0.021	-0.009	-0.058	0.019
	Convl. std. err.	0.072	0.052	0.108	0.081				
	Convl. p-value	0.172	0.940	0.092	0.551				
	Bias-corr. RD est.	-0.113	0.003	-0.199	-0.061				
	Robust std. err.	0.085	0.061	0.116	0.086	0.043	0.032	0.070	0.057
	Robust p-value	0.183	0.967	0.087	0.482	0.631	0.771	0.407	0.740
	Bandwidth	0.161	0.197	0.274	0.273				
	Eff. obs. left	489	371	658	437				
	Eff. obs. right	466	326	635	387				
	Observations	1595	1037	1595	1037	1595	1037	1595	1037
	Adj. R-squared					-0.000	0.605	0.003	0.605
Log environmental mgmt. exp. pc	Convl. RD est.	-0.049	-0.040	-0.016	0.295	-0.104	-0.209	-0.110	-0.351
	Convl. std. err.	0.317	0.358	0.425	0.538				
	Convl. p-value	0.877	0.912	0.969	0.584				
	Bias-corr. RD est.	0.014	0.051	-0.001	0.357				
	Robust std. err.	0.376	0.415	0.460	0.576	0.161	0.183	0.277	0.320
	Robust p-value	0.971	0.903	0.998	0.536	0.519	0.254	0.691	0.273
	Bandwidth	0.164	0.179	0.366	0.291				
	Eff. obs. left	366	267	556	344				
	Eff. obs. right	364	232	539	307				
	Observations	1232	803	1232	803	1232	803	1232	803
	Adj. R-squared					-0.001	0.144	0.001	0.147

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Table A.7 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Log sport and leisure exp. pc	Convl. RD est.	-0.235	-0.125	-0.265	0.141	-0.035	0.016	-0.260	-0.152
	Convl. std. err.	0.185	0.197	0.270	0.320				
	Convl. p-value	0.203	0.526	0.328	0.660				
	Bias-corr. RD est.	-0.269	-0.153	-0.261	0.205				
	Robust std. err.	0.216	0.230	0.293	0.342	0.106	0.115	0.177	0.197
	Robust p-value	0.213	0.505	0.373	0.549	0.745	0.886	0.142	0.439
	Bandwidth	0.195	0.207	0.356	0.279				
	Eff. obs. left	533	371	699	431				
	Eff. obs. right	516	326	676	381				
	Observations	1539	1002	1539	1002	1539	1002	1539	1002
	Adj. R-squared					0.000	0.279	0.008	0.282
Δ infant mortality	Convl. RD est.	3.306	5.993	-47.020	-35.836	-8.238	-14.971	15.545	40.102
	Convl. std. err.	16.208	18.652	18.669	22.067				
	Convl. p-value	0.838	0.748	0.012	0.104				
	Bias-corr. RD est.	1.564	3.166	-50.256	-40.022				
	Robust std. err.	18.488	21.572	19.771	23.495	14.010	17.389	19.550	22.812
	Robust p-value	0.933	0.883	0.011	0.088	0.557	0.390	0.427	0.079
	Bandwidth	0.156	0.128	0.191	0.199				
	Eff. obs. left	298	181	340	240				
	Eff. obs. right	298	153	348	217				
	Observations	1055	698	1055	698	1055	698	1055	698
	Adj. R-squared					0.001	0.005	0.002	0.017
Δ 7+ prenatal visits	Convl. RD est.	-0.327	-4.602	1.134	-6.037	-0.734	-1.197	2.225	0.290
	Convl. std. err.	4.375	4.893	6.957	7.486				
	Convl. p-value	0.940	0.347	0.870	0.420				
	Bias-corr. RD est.	-0.877	-5.830	1.740	-6.182				
	Robust std. err.	5.166	5.604	7.515	7.976	2.365	2.906	4.228	4.812
	Robust p-value	0.865	0.298	0.817	0.438	0.756	0.680	0.599	0.952
	Bandwidth	0.205	0.183	0.290	0.274				
	Eff. obs. left	352	236	432	295				
	Eff. obs. right	369	201	435	257				
	Observations	1055	698	1055	698	1055	698	1055	698
	Adj. R-squared					-0.002	-0.007	-0.001	-0.011
Δ low birth weight	Convl. RD est.	-0.069	1.227	2.545	0.730	-3.252	-3.092	-1.573	0.567
	Convl. std. err.	2.867	3.723	4.427	5.688				
	Convl. p-value	0.981	0.742	0.565	0.898				
	Bias-corr. RD est.	0.824	1.790	3.219	0.396				
	Robust std. err.	3.310	4.364	4.913	6.183	1.483	1.930	2.638	3.326
	Robust p-value	0.803	0.682	0.512	0.949	0.029	0.110	0.551	0.865
	Bandwidth	0.183	0.157	0.316	0.336				
	Eff. obs. left	331	219	449	322				
	Eff. obs. right	333	177	449	280				
	Observations	1055	698	1055	698	1055	698	1055	698
	Adj. R-squared					0.002	0.020	0.000	0.017

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Table A.7 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Covid-19 mortality	Convl. RD est.	115.672	45.863	162.798	121.137	12.351	-8.807	94.248	43.857
	Convl. std. err.	54.264	55.524	78.624	83.702				
	Convl. p-value	0.033	0.409	0.038	0.148				
	Bias-corr. RD est.	130.358	52.246	174.250	133.791				
	Robust std. err.	62.847	64.841	84.007	89.116	31.034	35.095	50.415	54.288
	Robust p-value	0.038	0.420	0.038	0.133	0.691	0.802	0.062	0.419
	Bandwidth	0.174	0.177	0.297	0.238				
	Eff. obs. left	515	353	680	414				
	Eff. obs. right	489	303	655	358				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.179	0.001	0.178
Δ% class size	Convl. RD est.	-0.020	-0.021	-0.035	-0.018	-0.008	-0.006	0.010	0.020
	Convl. std. err.	0.029	0.035	0.036	0.046				
	Convl. p-value	0.482	0.557	0.343	0.691				
	Bias-corr. RD est.	-0.028	-0.027	-0.039	-0.019				
	Robust std. err.	0.034	0.042	0.039	0.049	0.020	0.026	0.026	0.032
	Robust p-value	0.411	0.511	0.315	0.700	0.682	0.814	0.699	0.530
	Bandwidth	0.133	0.136	0.311	0.280				
	Eff. obs. left	401	306	680	480				
	Eff. obs. right	392	263	660	422				
	Observations	1568	1104	1568	1104	1568	1104	1568	1104
	Adj. R-squared					-0.001	0.003	-0.003	0.000
Δ% age delay	Convl. RD est.	0.000	0.032	0.059	0.162	-0.109	-0.115	-0.062	-0.117
	Convl. std. err.	0.070	0.086	0.110	0.122				
	Convl. p-value	0.999	0.712	0.593	0.184				
	Bias-corr. RD est.	0.026	0.069	0.064	0.174				
	Robust std. err.	0.079	0.098	0.122	0.132	0.045	0.048	0.067	0.077
	Robust p-value	0.738	0.482	0.599	0.186	0.015	0.018	0.360	0.127
	Bandwidth	0.173	0.143	0.290	0.268				
	Eff. obs. left	491	310	648	455				
	Eff. obs. right	471	269	628	403				
	Observations	1542	1081	1542	1081	1542	1081	1542	1081
	Adj. R-squared					0.003	0.027	0.001	0.025
Δ% portuguese score	Convl. RD est.	-0.009	-0.009	0.001	0.005	-0.002	-0.000	-0.012	-0.013
	Convl. std. err.	0.009	0.011	0.015	0.019				
	Convl. p-value	0.300	0.403	0.963	0.807				
	Bias-corr. RD est.	-0.010	-0.009	0.002	0.007				
	Robust std. err.	0.010	0.012	0.016	0.021	0.005	0.007	0.009	0.010
	Robust p-value	0.307	0.485	0.922	0.741	0.753	0.966	0.173	0.207
	Bandwidth	0.239	0.212	0.295	0.261				
	Eff. obs. left	562	383	618	429				
	Eff. obs. right	539	334	595	376				
	Observations	1455	1024	1455	1024	1455	1024	1455	1024
	Adj. R-squared					-0.001	0.027	0.007	0.036

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Table A.7 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Δ% math score	Convl. RD est.	-0.017	-0.021	-0.009	-0.012	-0.008	-0.010	-0.022	-0.026
	Convl. std. err.	0.008	0.010	0.014	0.016				
	Convl. p-value	0.039	0.033	0.514	0.436				
	Bias-corr. RD est.	-0.019	-0.021	-0.008	-0.011				
	Robust std. err.	0.010	0.011	0.015	0.017	0.005	0.007	0.008	0.010
	Robust p-value	0.059	0.063	0.587	0.509	0.163	0.124	0.009	0.007
	Bandwidth	0.225	0.197	0.311	0.283				
	Eff. obs. left	542	367	632	444				
	Eff. obs. right	524	322	610	392				
	Observations	1455	1024	1455	1024	1455	1024	1455	1024
	Adj. R-squared					0.000	0.021	0.015	0.039
Education pgrm. quilombolas	Convl. RD est.	0.022	0.037	-0.012	0.029	0.022	0.019	-0.000	0.032
	Convl. std. err.	0.070	0.094	0.112	0.153				
	Convl. p-value	0.757	0.694	0.918	0.848				
	Bias-corr. RD est.	0.026	0.033	-0.023	0.028				
	Robust std. err.	0.083	0.112	0.122	0.165	0.041	0.051	0.068	0.084
	Robust p-value	0.758	0.766	0.849	0.867	0.596	0.705	0.998	0.701
	Bandwidth	0.195	0.162	0.273	0.231				
	Eff. obs. left	340	216	404	263				
	Eff. obs. right	321	169	372	208				
	Observations	959	625	959	625	959	625	959	625
	Adj. R-squared					-0.001	0.072	-0.003	0.082
Family health pgrm. nonwhite	Convl. RD est.	-0.028	-0.058	-0.058	-0.132	-0.021	-0.052	-0.049	-0.096
	Convl. std. err.	0.066	0.076	0.095	0.115				
	Convl. p-value	0.668	0.447	0.545	0.250				
	Bias-corr. RD est.	-0.032	-0.058	-0.068	-0.152				
	Robust std. err.	0.079	0.090	0.104	0.124	0.036	0.044	0.059	0.073
	Robust p-value	0.687	0.516	0.512	0.217	0.550	0.230	0.411	0.188
	Bandwidth	0.168	0.161	0.318	0.258				
	Eff. obs. left	503	335	703	432				
	Eff. obs. right	478	279	675	379				
	Observations	1595	1037	1595	1037	1595	1037	1595	1037
	Adj. R-squared					0.001	0.057	-0.000	0.060
Vaccination pgrm. nonwhite	Convl. RD est.	-0.014	0.035	-0.084	-0.090	0.064	-0.033	0.136	0.126
	Convl. std. err.	0.132	0.145	0.157	0.194				
	Convl. p-value	0.916	0.811	0.594	0.641				
	Bias-corr. RD est.	-0.053	0.010	-0.108	-0.120				
	Robust std. err.	0.154	0.171	0.169	0.207	0.062	0.077	0.102	0.130
	Robust p-value	0.732	0.955	0.522	0.564	0.299	0.669	0.185	0.331
	Bandwidth	0.121	0.128	0.336	0.299				
	Eff. obs. left	127	94	242	143				
	Eff. obs. right	118	74	222	136				
	Observations	539	347	539	347	539	347	539	347
	Adj. R-squared					-0.002	0.077	-0.003	0.079

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Table A.7 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs.	Covariates	No covs.	Covariates	No covs.	Covariates	No covs.	Covariates
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Social assistance quilombolas	Convl. RD est.	-0.040	-0.026	-0.099	-0.060	-0.022	-0.041	-0.031	-0.018
	Convl. std. err.	0.075	0.085	0.109	0.124				
	Convl. p-value	0.601	0.761	0.368	0.631				
	Bias-corr. RD est.	-0.042	-0.019	-0.117	-0.076				
	Robust std. err.	0.090	0.101	0.117	0.133	0.042	0.049	0.072	0.081
	Robust p-value	0.643	0.851	0.319	0.568	0.606	0.411	0.662	0.823
	Bandwidth	0.158	0.148	0.261	0.261				
	Eff. obs. left	330	222	449	307				
	Eff. obs. right	321	188	436	275				
	Observations	1097	730	1097	730	1097	730	1097	730
	Adj. R-squared					-0.001	0.124	-0.003	0.127
Labor inclusion pgrm.	Convl. RD est.	0.015	0.068	-0.026	0.066	-0.011	0.010	0.012	0.055
	Convl. std. err.	0.060	0.070	0.087	0.096				
	Convl. p-value	0.810	0.332	0.766	0.494				
	Bias-corr. RD est.	0.011	0.072	-0.037	0.063				
	Robust std. err.	0.071	0.082	0.093	0.103	0.034	0.042	0.056	0.069
	Robust p-value	0.880	0.380	0.693	0.537	0.743	0.820	0.832	0.426
	Bandwidth	0.166	0.172	0.300	0.358				
	Eff. obs. left	467	327	636	450				
	Eff. obs. right	437	274	610	400				
	Observations	1473	960	1473	960	1473	960	1473	960
	Adj. R-squared					-0.001	0.033	-0.002	0.034
Gender program	Convl. RD est.	0.048	0.028	0.053	0.007	0.008	0.009	0.057	0.038
	Convl. std. err.	0.028	0.034	0.049	0.048				
	Convl. p-value	0.083	0.420	0.284	0.880				
	Bias-corr. RD est.	0.052	0.025	0.052	0.007				
	Robust std. err.	0.032	0.040	0.055	0.052	0.017	0.020	0.029	0.033
	Robust p-value	0.105	0.527	0.342	0.898	0.643	0.664	0.048	0.250
	Bandwidth	0.229	0.158	0.273	0.292				
	Eff. obs. left	601	330	656	452				
	Eff. obs. right	586	275	635	399				
	Observations	1594	1036	1594	1036	1594	1036	1594	1036
	Adj. R-squared					-0.001	0.089	-0.001	0.091
Δ% social prgm. registration	Convl. RD est.	0.021	0.019	0.064	0.045	-0.001	-0.005	-0.004	-0.004
	Convl. std. err.	0.024	0.027	0.038	0.047				
	Convl. p-value	0.397	0.484	0.096	0.338				
	Bias-corr. RD est.	0.027	0.029	0.074	0.050				
	Robust std. err.	0.028	0.032	0.041	0.052	0.012	0.015	0.021	0.026
	Robust p-value	0.344	0.365	0.073	0.335	0.928	0.746	0.859	0.874
	Bandwidth	0.160	0.205	0.252	0.311				
	Eff. obs. left	487	380	640	464				
	Eff. obs. right	465	337	611	414				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					-0.001	0.034	0.001	0.036

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Table A.7 – continued from previous page

		Nonparametric				Parametric			
		LLR		LPR 3^{rd}		OLS		Pol. 3^{rd} interac.	
		No covs. (1)	Covariates (2)	No covs. (3)	Covariates (4)	No covs. (5)	Covariates (6)	No covs. (7)	Covariates (8)
Δ% cash transf. beneficiaries	Convl. RD est.	0.025	0.088	0.089	0.157	0.019	0.030	0.013	0.065
	Convl. std. err.	0.034	0.058	0.074	0.112				
	Convl. p-value	0.459	0.126	0.229	0.160				
	Bias-corr. RD est.	0.032	0.106	0.107	0.181				
	Robust std. err.	0.043	0.071	0.082	0.129	0.015	0.021	0.032	0.048
	Robust p-value	0.462	0.138	0.195	0.160	0.212	0.159	0.677	0.179
	Bandwidth	0.237	0.198	0.301	0.324				
	Eff. obs. left	617	371	685	472				
	Eff. obs. right	595	327	660	421				
	Observations	1596	1038	1596	1038	1596	1038	1596	1038
	Adj. R-squared					0.000	0.050	0.005	0.055
Δ mortality fr. violence	Convl. RD est.	-0.173	-42.698	-2.754	-59.133	-12.116	-13.398	-31.227	-62.112
	Convl. std. err.	35.455	44.096	50.761	66.660				
	Convl. p-value	0.996	0.333	0.957	0.375				
	Bias-corr. RD est.	8.990	-43.040	-10.656	-67.965				
	Robust std. err.	41.874	53.257	54.693	71.776	17.562	22.515	29.475	37.795
	Robust p-value	0.830	0.419	0.846	0.344	0.490	0.552	0.290	0.101
	Bandwidth	0.147	0.162	0.287	0.295				
	Eff. obs. left	450	335	673	456				
	Eff. obs. right	434	281	647	401				
	Observations	1595	1038	1595	1038	1595	1038	1595	1038
	Adj. R-squared					-0.001	0.016	0.001	0.018
Δ mortality fr. violence nonwhite	Convl. RD est.	4.799	-31.636	-7.031	-64.570	1.162	-3.060	-36.047	-57.889
	Convl. std. err.	47.216	56.731	69.348	87.744				
	Convl. p-value	0.919	0.577	0.919	0.462				
	Bias-corr. RD est.	20.355	-29.906	-21.224	-80.449				
	Robust std. err.	54.506	67.594	74.456	94.395	23.707	30.897	40.110	52.516
	Robust p-value	0.709	0.658	0.776	0.394	0.961	0.921	0.369	0.271
	Bandwidth	0.152	0.169	0.276	0.281				
	Eff. obs. left	468	342	660	447				
	Eff. obs. right	448	288	639	397				
	Observations	1595	1038	1595	1038	1595	1038	1595	1038
	Adj. R-squared					-0.001	0.013	0.002	0.012
Municipal guard staff	Convl. RD est.	-0.866	-1.548	2.817	-21.924	-16.634	-24.183	35.434	-5.354
	Convl. std. err.	19.026	18.351	22.649	26.453				
	Convl. p-value	0.964	0.933	0.901	0.407				
	Bias-corr. RD est.	-6.805	0.798	7.052	-22.870				
	Robust std. err.	19.717	20.252	25.628	28.939	20.344	22.451	34.987	33.000
	Robust p-value	0.730	0.969	0.783	0.429	0.414	0.282	0.312	0.871
	Bandwidth	0.146	0.176	0.211	0.223				
	Eff. obs. left	101	80	126	95				
	Eff. obs. right	102	79	131	90				
	Observations	388	267	388	267	388	267	388	267
	Adj. R-squared					-0.004	0.467	0.011	0.457