Gender and Electoral Incentives: Evidence from Crisis Response*

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Abstract

While there is evidence of gender differences in leaders' behavior, less is known about what drives these gaps. This paper uncovers the role of electoral incentives. Using a close election RD design in Brazil, we first show that female mayors handled the COVID-19 crisis differently over the year 2020, which ended with new municipal elections. We find that having a female mayor led to more deaths per capita at the beginning of the pandemic – a period characterized by uncertainty about the severity of the threat – but to fewer deaths per capita later in the year – a period where this uncertainty was reduced. We provide additional evidence that female mayors were less likely to close non-essential businesses early on, and more likely to do so at the end, and that residents in female-led municipalities were more likely to stay at home in the weeks surrounding the election. We then show that these results can be rationalized by a simple political agency model where politicians seek re-election and where voters assess female and male politicians' actions differently. Consistent with this interpretation, we show that the gender differences we find are driven exclusively by mayors who were not term-limited and thus allowed to run for re-election and that the effects are stronger in municipalities with greater gender discrimination. Taken together, the results suggest that female and male leaders face different electoral incentives and adapt their policy decisions to voters' gender-biased assessment.

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1 Introduction

A large literature documents gender differences in the behavior of elected officials, which are particularly robust in developing countries. This literature has shown that female leaders are more likely to invest in certain public goods, such as education and health (e.g., Chattopadhyay and Duflo, 2004; Clots-Figueras, 2012), and are less prone to corruption (Brollo and Troiano, 2016). However, less is known about *why* female and male elected leaders make different choices.

Gender differences in leaders' behavior could come from differences in policy preferences. An alternative explanation is that female and male leaders face different electoral incentives. Indeed, there is substantive evidence that voters are gender-biased and evaluate female leaders' actions differently (e.g., Gagliarducci and Paserman, 2012; Bertrand and Duflo, 2017; Le Barbanchon and Sauvagnat, 2022). Female politicians seeking reelection might therefore have incentives to behave differently than male politicians. For instance, if female politicians expect more backlash (or less reward) for a given policy decision than a male politician would receive, it will be strategically less beneficial to adopt it.

This paper provides new evidence that electoral incentives can be important drivers of gender differences in leaders' behavior. We study the response of local leaders to the COVID-19 crisis in Brazil. We investigate whether female and male mayors handled the crisis differently and to what extent their behaviors were driven by electoral incentives.

This setting offers several advantages. First, studying a crisis context enables us to focus on high-stakes policies that are salient to voters and that we can directly link to outcomes. Second, whereas in most countries these decisions were taken at the national or regional level, Brazilian municipalities are federal entities, allowing mayors to independently choose COVID containment policies. Furthermore, Brazil has over 5,000 municipalities, which enables us to use a close election design to assess the causal impact of female leadership. Third, a municipal election was held in November 2020. Hence, from the start of the pandemic, mayors seeking reelection knew they would face their electorate in the near future. Fourth, Brazilian mayors face a two-term limit, meaning that only first-time mayors could run for reelection. This creates variation in electoral incentives across mayors that we exploit to study the underlying mechanisms.

In order to isolate the causal impact of female leadership, we use a regression discontinuity design (RDD) and compare municipalities where a female candidate narrowly won against a male candidate in the 2016 election – the last one before the COVID-19 outbreak –

to those where a male candidate narrowly won against a female candidate. We can thus compare municipalities that are similar in every aspect but the gender of their mayor. To support our identification strategy, we show that municipalities are indeed balanced on a large set of sociodemographic and political characteristics at the threshold. We also explore balance in the individual characteristics of the winner and show that closely elected female and male mayors are similar in incumbency status, age, race, occupation, and political orientation. Female mayors tend to have more education on average, consistent with positive selection, but this difference is not statistically significant at the threshold and our results are robust to controlling for all these observable characteristics. We are therefore confident that we can interpret our findings as driven by the mayor's gender.[1] Using this RD framework together with daily panel data at the municipal level, we explore female and male mayors' responses to the crisis throughout 2020, the last year of their term.

Our main outcome of interest is the number of COVID-19 deaths in the municipality. We find large but opposite effects of female leadership at the beginning and at the end of the year, revealing a very different evolution in the number of deaths in female-led compared to male-led municipalities. At the beginning of the first wave (April-May 2020), having a female mayor led to 0.39 more deaths per 10,000 inhabitants, an effect that corresponds to a three-fold increase compared to the average number of deaths in male-led municipalities at the threshold. Instead, at the end of the year (November-December 2020), female-led municipalities experienced one fewer death per 10,000 inhabitants, corresponding to a 41.1 percent decrease relative to male-led municipalities.

Given that female- and male-led municipalities at the threshold differ only in the gender of their mayor, we interpret these results as reflecting differential responses to the crisis by female and male mayors over time. To further support this interpretation, we next explore the impact of having a female mayor on containment policies and residents' isolation behavior.

Using data collected directly from laws and decrees issued by the municipalities, we find that female and male mayors differ primarily in their use of commerce restrictions. Consistent with the evolution in the number of deaths, female mayors were less likely than male mayors to close non-essential businesses at the beginning of the year, but became more likely than male mayors to do so towards the end. Commerce restrictions were in place 3.0 and 8.6 fewer days in female-led municipalities in March and April 2020, as female mayors

[1] Section 5.2.3 provides additional evidence that selection is unlikely to account for our results.

started closing non-essential businesses 43 days later on average. In contrast, commerce restrictions were in place 7.8 and 7.6 more days in female-led municipalities in September and October 2020, as male mayors started reopening non-essential businesses earlier on average.[2]

Additional evidence shows that residents in female-led municipalities were more likely to stay at home toward the end of the year. Using daily cellphone data, we find that the share of phone users who stayed at home was 10- to 20-percent higher in female-led municipalities in the last days of the electoral campaign and in the few days following the election results. These effects likely reflect a higher propensity of male mayors to organize in-person events around election day.

Overall, our results suggest that female and male mayors handled the COVID-19 crisis differently: while female mayors were less likely to undertake containment efforts at the beginning of the pandemic, male mayors were more likely to relax containment efforts at the end of the year.

The second part of the paper uncovers the role of electoral incentives. We start by using a simple political agency model, in which voters are gender-biased, to illustrate how electoral incentives can give rise to the observed patterns. In this model, voters care about a public good and the politician, who cares about reelection, can implement policies that mitigate the incoming shock to the public good but that also generate a direct cost to voters.[3] Our model assumes that voters evaluate female leaders' actions less favorably than those of male leaders. More specifically, to capture voters' gender bias, we assume that voters believe that crisis containment policies will be less effective if implemented by a female politician, such that female politicians receive less credit for their actions. We analyze politicians' optimal policy choice under two scenarios corresponding to our two periods of analysis. First, when voters believe that there is only a low probability that the shock will materialize – as was arguably the case during the first weeks of the pandemic in Brazil – they are less willing to accept containment policies and it is costly

[2] Assessing the causal impact of policies on COVID-19 deaths is beyond the scope of this paper and mayors' actions likely go beyond the policies we are able to observe. We therefore refrain from making a causal claim on the relationship between commerce restrictions and COVID-19 deaths. However, we do see these results as evidence that the effects we find on COVID-19 deaths reflect the fact that female and male mayors responded differently to the crisis over time.

[3] In our context, the public good is health and the policies are any actions the politicians can take to contain the pandemic and reduce deaths. However, this framework can apply to other crisis contexts featuring policy solutions that can be unpopular and politically costly, such as fiscal policies to curb inflation or environmental policies to limit global warming.

for politicians to close the economy. In this scenario, female politicians implement a lower level of policy than their male counterparts, as voters expect policies to be even more cost-ineffective when implemented by female politicians. Conversely, when voters believe that there is a significant threat to the public good – for example, later in 2020, when the health consequences have become more apparent – they are more willing to accept the disutility associated with the policy in order to preserve the public good. As voters believe that male politicians can achieve the same results with a lower level of policy than female politicians can, male mayors are the ones implementing a lower policy level in this scenario.

We then investigate the role of electoral incentives empirically. Exploiting the two-term limit, we compare mayors who were elected for the first time in 2016 – and were therefore allowed to run again in 2020 – to mayors who were elected for a second term in 2016, and thus could not run again. Consistent with electoral incentives explaining gender differences in leaders' behavior, we find that the gender differences in the evolution of COVID-19 deaths are only driven by mayors eligible to run for reelection. Departing from the RD framework and using an OLS estimation, we also show that the effects are stronger in municipalities where the mayor won with a small victory margin in 2016; that is, in more competitive places where electoral incentives are arguably the strongest.

Lastly, we explore whether the data support the assumption that female and male mayors face different electoral incentives because voters assess their actions differently. Consistent with this interpretation, we find that our results are stronger in municipalities where we expect higher gender discrimination and thus voter bias, as proxied by the gender wage gap in the local labor market. We also show that alternative interpretations of the results – such as selection and gender differences in policy preferences or risk aversion – are unlikely to account for the observed patterns. Taken together, these results support the interpretation that female and male mayors face different electoral incentives and that their responses to the crisis were shaped by voters' gender bias.

Our results have important implications for the way we interpret gender differences in leaders' behavior. These differences might stem not from differences in their preferences but rather from differences in the incentives they face. This makes gender differences in leaders' behavior particularly likely in competitive elections, in contexts with greater gender discrimination, and for policies salient to voters.

Contribution to the Literature

Our paper contributes to three main strands of the literature, which study how the behavior of political leaders varies by gender, the prevalence of gender discrimination in politics, and how electoral incentives shape leaders' behavior. We add novel insights to each of these bodies of work and connect them by showing how the interaction of voter bias and electoral incentives helps explain gender differences in policymaking.

Along with the literature showing that leaders matter for economic outcomes (Jones and Olken, 2005; Besley et al., 2011; Yao and Zhang, 2015; Ottinger and Voigtlander, 2022), a large literature has emerged on gender differences in the behavior of political leaders.[4] Studies in developing countries consistently find divergent policy choices by politician gender. In India and Brazil, researchers have shown that female politicians invest more in infrastructure relevant to women's needs (Chattopadhyay and Duflo, 2004), spend more on education and health (Clots-Figueras, 2011, 2012; Bhalotra and Clots-Figueras, 2014; Funk and Philips, 2019), and are less corrupt (Brollo and Troiano, 2016). The results are less conclusive in high-income countries. While female legislators are more likely to support bills related to family and children's issues (Besley and Case, 2003; Lippmann, 2022), there is mixed evidence of gender differences in legislative effectiveness (Anzia and Berry, 2011; Volden et al., 2013; Battaglini et al., 2020) and several papers find no gender differences in public policies at the municipal level in the US, Spain, or Italy (Ferreira and Gyourko, 2014; Bagues and Campa, 2021; Casarico et al., 2022; Carozzi and Gago, 2023).[5]

Only a few recent papers uncover causal effects of female leadership in crisis contexts. Dube and Harish (2020) find that European queens were historically more likely to be at war than kings, as unmarried queens were perceived as easier to attack, while married queens were better equipped to attack, splitting the work with their husbands. Using data from the Colombian armed internal conflict, Eslava (2021) shows that having a female mayor reduced the number of guerilla attacks, likely due to better negotiation skills. In the context of the COVID-19 crisis, Bruce et al. (2022) find that female-led municipalities in Brazil had fewer deaths in 2020, consistent with the net negative cumulative effect we report.

[4] See Hessami and da Fonseca (2020) for a review.

[5] Recent evidence from Italy suggests that those null effects can mask more subtle differences: Profeta and Woodhouse (2022) find that, while having a female mayor does not impact overall spending, it affects the timing of public expenditures, a finding that points up the importance of investigating gender differences over time.

Our study contributes to the literature on gender differences in leaders' behavior in two main ways. First, we provide causal evidence of gender differences in crisis response over time. We show that these differences can vary and even reverse as the crisis unfolds. Second, and most importantly, we provide evidence on a new mechanism explaining why female leaders make different policy decisions. We show that our results can be explained by female and male leaders adapting their behavior to voters' gender biases. We thus bridge the gap between the literature on female leadership discussed above and the literature on gender discrimination in politics.

A large body of work finds evidence of voter bias against female candidates (e.g., Fréchette et al., 2008; De Paola et al., 2010; Eyméoud and Vertier, 2023; Le Barbanchon and Sauvagnat, 2022).[6] Beyond gender discrimination at the electoral stage, voters also appear gender-biased in evaluating the actions of female leaders once in power. The "role incongruity" theory in the psychology literature posits that these biases arise because traits associated with leadership, such as strength and assertiveness, are perceived as inconsistent with the characteristics that society associates with women, making voters unlikely to perceive women as strong leaders (Eagly and Karau, 2002; Duflo, 2012; Bertrand and Duflo, 2017). Consistent with the incongruity theory, lab and field experiments show that female politicians are evaluated less favorably than male politicians, particularly in circumstances in which traditional male attributes are especially valued – such as issues related to national security and crises – but less so for "feminine" issues such as child care and education (Herrnson et al., 2003; Lawless, 2004; Beaman et al., 2009; Eggers et al., 2018). Using quasi-experimental evidence from Italy, Gagliarducci and Paserman (2012) show that female mayors are more likely to experience an early termination of their mandate in regions where people display less-favorable attitudes towards working women.[7] Similar gender biases in performance evaluation have been found in contexts outside politics, including the manufacturing, financial, and technological sectors (Macchiavello et al., 2020; Egan et al., 2022; Feld et al., 2022), healthcare (Sarsons, 2022), and academia (Sarsons, 2017; Mengel et al., 2018; Ross et al., 2022).

[6] An exception in the quasi-experimental literature is Broockman and Soltas (2020), who find evidence of discrimination based on race but not gender in the election of delegates in US Republican presidential primaries. Recent studies have also highlighted gender discrimination by political parties as a key driver of low female representation in politics (Casas-Arce and Saiz, 2015; Fujiwara et al., 2021; Gonzalez-Eiras and Sanz, 2021).

[7] Given that gender discrimination in elections can lead to positive selection of female leaders (Baltrunaite et al., 2014; Besley et al., 2017), these results suggests that female leaders are assessed more harshly than men even when they are relatively more skilled.

If people are biased against women in leadership positions and assess their actions differently, female and male leaders are likely to face different electoral incentives. Our results show that this mechanism can rationalize why female mayors responded differently to the COVID-19 crisis, building on the literature studying the impact of electoral incentives on leaders' behavior.

Political agency models (Barro, 1973; Ferejohn, 1986) posit that elections work as a disciplining device, creating incentives for leaders to align their decisions with voters' preferences. Researchers have found extensive empirical support for this theory by showing that politicians seeking reelection exert more effort than term-limited ones (Besley and Case, 1995; List and Sturm, 2006; Sieg and Yoon, 2017; Aruoba et al., 2019; Fouirnaies and Hall, 2022).[8] In Brazil, Ferraz and Finan (2011) and de Janvry et al. (2012) find, respectively, that having a non–term-limited mayor decreases resource misappropriation and increases the performance of a large conditional cash transfer program.

The effects of electoral incentives on the behavior of politicians are more pronounced in certain circumstances; in particular, when voters are more aware of leaders' policy decisions and performance (Snyder and Strömberg, 2010; Ashworth, 2012). Crises tend to create such higher-accountability environments. Indeed, there is ample evidence that leaders' responses in a crisis matter for electoral outcomes; for example, during the Ebola pandemic (e.g., Campante et al., 2021; Maffioli, 2021), after a natural disaster (e.g., Healy and Malhotra, 2009; Bechtel and Hainmueller, 2011) or a terrorist attack (e.g., Getmansky and Zeitzoff, 2014), and, more recently, during the COVID-19 pandemic (e.g., Baccini et al., 2021; Giommoni and Loumeau, 2022). Electoral incentives are also stronger close to elections. In the last year of their term, politicians have an incentive to implement short-term, electorally rewarding policies that might ignore long-term consequences, such as monetary expansions and tax reductions (see Alesina, 1988; Drazen, 2001; Alesina and Paradisi, 2017; Aidt et al., 2020; and, in Brazil, Klein and Sakurai, 2015; Orair et al., 2015) or weaker containment policies at the beginning of the COVID-19 pandemic (Pulejo and Querubín, 2021).

Our study contributes to this literature by showing, in a setting in which electoral incentives are likely to be strong – the response to a crisis during an election year – that while electoral incentives affect the behavior of both female and male leaders, female leaders

[8] In the Argentinian context, where there is no term limit, Dal Bó and Rossi (2011) show that longer terms increase politicians' efforts, as the positive effects of their actions are more likely to materialize before the next election.

adopt different electoral strategies when voters are likely to assess their actions differently.

The remainder of the paper is organized as follows. Section 2 presents our setting and data. Section 3 describes our sample and empirical strategy. We present the results showing that female mayors handled the COVID-19 crisis differently in Section 4 and uncover the role of electoral incentives in Section 5. Section 6 concludes.

2 Setting and Data

2.1 Brazilian Local Governments and Elections

Brazil is divided into 5,570 municipalities, the lowest subnational government tier in the country.[9] Municipal governments are in charge of providing public services of local interest, including water and sanitation, transportation, basic education, and – importantly for this paper – public health. Municipalities' expenditures were 18.9 percent of total public spending in 2019. Their revenues come mainly from constitutionally mandated intergovernment transfers (56.7 percent of total municipal revenues in 2019), followed by local taxes and user fees (IBGE, 2020).

The constitution recognizes municipalities as "federal entities," which gives them the status of autonomous governments, with the ability to independently decide local policies. At the onset of the COVID-19 pandemic, the national congress reaffirmed municipalities' power to implement containment policies (Law Nº 13.979). The Brazilian Supreme Court further ruled that the federal government could not overrule the policies of local governments (Decision ADPF 672).

Municipal governments have an executive branch (*prefeitura*) and a legislative branch (*câmara municipal*). The executive branch is presided over by a mayor elected by popular vote every four years. Voter registration and voting are mandatory for adults between the ages of 18 and 70. Municipalities with fewer than 200,000 eligible voters elect their mayors through plurality rule – whereby the candidate with the most votes wins the election – while municipalities with 200,000 eligible voters or more use a two-round system. Mayors are subject to a two-term limit established by the 1988 constitution, meaning that mayors serving a second term cannot run for reelection. Local legislators are elected at the same

[9]The first tier consists of 27 "federative units," made up of 26 states and the Federal District. The Federal District does not contain any municipality; it is divided into administrative regions, including the capital, Brasília, and is therefore excluded from the analysis.

time as mayors, using an open-list proportional system. The legislature analyzes and revises the budget proposed by the mayor, who then decides how much to spend on the different items. The legislators can also propose bills, which can be contested by the mayor, who ultimately retains the most influence over the implementation of laws and decrees.

Our empirical strategy relies on the results of the 2016 municipal election, the last before the COVID-19 outbreak. The term of the mayors elected in 2016 ran from January 1, 2017, through December 31, 2020. The first round of the next local election took place on November 15, 2020,[10] and the new mayors took office on January 1, 2021. Our period of analysis is the last year of the mayor's term, from February 2020 (first registered COVID-19 case in the country) through the end of January 2021.[11]

Participation by female candidates in the last two municipal elections was higher than in prior ones, but remained small. The share of female mayoral candidates in the 2016 (2020) elections was 12.9 (13.5) percent and only 11.5 (12.1) percent of the elected mayors were female. This was a small improvement relative to 2000, when women made up 7.6 percent of mayoral candidates and 5.7 percent of elected mayors (TSE, 2021). This political participation gap is also observed in congressional elections; in 2020, the share of congresswomen in Brazil's parliament was 14.6 percent, less than half of the averages for Latin America (32.8 percent) and the OECD (31.5 percent) (The World Bank, 2021).[12]

2.2 The COVID-19 Pandemic in Brazil

The authorities announced the first COVID-19 case in Brazil on February 26, 2020, and the first death three weeks later, on March 17. The disease expanded exponentially across the country and so did the death toll (Roser et al., 2020).

Our period of analysis includes the first wave of infections (February 2020-October 2020) and the beginning of the second wave (November 2020-January 2021). The first wave

[10]The 2020 municipal election was originally scheduled for October 4 and postponed to November 15 due to the COVID-19 health emergency. While basic safety protocols were put in place at the voting booths, the election took place in person, as had the previous ones. The electoral campaign lasted for 50 days – the usual length in Brazilian municipal elections – and ran up to the day before the election.

[11]We include the first month of the new municipal administration, as COVID-19 deaths tend to materialize a few weeks after infection, implying that people who died from the disease in January likely became infected while the prior mayor was still in office.

[12]The gender gap in leadership positions in Brazil is not restricted to the political world. Among the 343 publicly listed companies in Brazil, only 14.2 percent of board members are female (Teva Índices, 2021). Considering only the CEOs, the share of females is of 8 percent, which is similar to the 2020 *Fortune*-500 share of female CEOs (7.4 percent) (Hinchliffe, 2021).

in Brazil was one of the deadliest worldwide (Appendix Figure A1). On June 10, Brazil's cumulative deaths exceeded those reported by the UK and were then second only to those reported by the US. The second wave started in November and was even deadlier than the first. By the end of our period of analysis, the daily number of deaths was similar to that at the peak of the first wave and the country had accumulated over 224,000 deaths in total.

The federal government responded by implementing social assistance programs and border restrictions, while largely refraining from imposing restrictions on mobility and gatherings within the country. Meanwhile, multiple states and municipal governments declared states of emergency and implemented containment policies such as commerce closures. Only a few local governments decided to go further and implement curfews and lockdowns. Section 4.2 and Appendix B3 provide more details about containment policies across municipalities and over time.

The public's perception of the severity of the threat and the need for containment policies evolved over the year 2020. According to surveys conducted by Ipsos, in March 2020, 56 percent of Brazilian respondents did not believe that social isolation would work and, at the beginning of April, 85 percent expected things to return to normal by June. But by the start of May, with the number of deaths starting to increase, 68 percent of respondents did not want to return to the workplace and, by the end of May, seven out of ten Brazilians did not agree with reopening non-essential businesses (Ipsos, 2020).

2.3 Data

This section describes the main datasets used in the analysis. Appendix Table B1 provides the definition and source of each variable used in the paper.

COVID-19 deaths. Our main outcome, the number of COVID-19 deaths, comes from Brasil.io. This open-data platform collects, cleans, and assembles the COVID-19 information provided by the state health secretaries and makes it publicly available as a daily municipal-level panel (Justen, 2021). We focus on deaths rather than cases. Deaths are less likely to go unrecorded and are thus considered a more reliable measure of the spread of COVID-19 as well as of the spread of other major pandemics (Maugeri et al., 2020; O'Driscoll et al., 2021; Xu, 2023). We observe the daily number of COVID-19 deaths from the first registered death on March 17, 2020, until January 31, 2021. We performed quality checks to identify potential data errors and outliers and we only found unusual spikes in a few municipalities in the state of Mato Grosso. We exclude municipalities in this state – 3.3 percent of the sample – in one of our robustness check (Appendix D) and when presenting the raw data on the number of deaths in Section 3.1.

We validate our main results using data from the Brazilian System of Information and Epidemiological Surveillance of Respiratory Infections (SIVEP-Gripe). The Ministry of Health maintains a patient-level registry of deaths from severe acute respiratory infection (SARI), a broader category that includes COVID-19 and other diseases with similar symptoms. The registry contains data from both public and private hospitals. By looking at overall SARI deaths, we can test the robustness of our results to using a death measure that does not rely on COVID-19 testing and is therefore less vulnerable to diagnostic misclassification. As shown in Appendix D, both data sources are highly consistent during the period of analysis.[13]

Containment policies. We built a novel policy dataset based on publicly available municipal legislation documents, following the procedure of Chauvin et al. (2021). We first accessed multiple online sources, including municipal websites and municipal official gazettes, and collected local laws, decrees, and other mandates issued by municipalities in response to the COVID-19 crisis. Collection took place at the end of 2020; we collected documents released between March 1 and October 31, 2020. We then extracted the text of the legal documents, parsed their individual articles, and used them to construct a daily panel of indicator variables that denote whether a given policy was in place in the municipality on a given day. Finally, we validated the quality of the text algorithm by using a testing dataset built manually for a random subset of municipalities. One challenge is that some municipalities might not have systematically released their laws online, which would limit our ability to capture all policies implemented over our period of interest. In particular, while it was more common to find a dedicated online repository for COVID-19 legislation in larger municipalities, collection turned out to be harder for smaller municipalities, likely due to scarcer resources and lower institutional capacity. We thus focus our policy analysis on municipalities with above 10,000 inhabitants, as in Chauvin et al. (2021), accounting for 50 percent of our sample.[14]

[13]As discussed in more detail in Chauvin (2021), the study of COVID-19 at the municipal level makes it hard to compute the number of deaths using alternative measures. Estimating excess deaths relative to prior years for a given month, for instance, requires historical mortality data with enough variation in each month to accurately predict the number of deaths that would be expected without the pandemic. This is only feasible for a few highly populated municipalities. Likewise, data from seroprevalence surveys collected to infer infection rates from the presence of antibodies are only available for a subset of municipalities.

[14]We could not find *any* document at all for 24 municipalities, among which only four have above 10,000 inhabitants. We consider them as missing.

We consider 10 containment policies, which we defined in line with the international policy data featured in the Oxford COVID-19 Government Response Tracker (Wade et al., 2022): commerce restrictions (closing non-essential businesses), curfews, event cancellations, face mask mandates, restrictions on gathering, lockdowns, school closures, workplace restrictions, and restrictions on transport and travel. Four of these policies (school closures, event cancellations, face masks mandates, and restrictions on gathering) were implemented by the vast majority of municipalities over similar amounts of time (Appendix Tables B2 and Appendix Figure B2), providing little variation with which to identify the effects of interest. We therefore focus our analysis on the remaining six policies. See Appendix B3 for a more extensive discussion of the policy data.

Isolation index. To study residents' isolation behavior, we use the "Social Isolation Index" produced by the private firm InLoco (2021). This index is built using anonymized data from over 60 million cell phones and indicates the share of active phone users who stayed within 450 meters of their residence in a given municipality on a given day. During the pandemic, the company made a daily municipal-level panel available to researchers. To protect users' privacy, the data are not available for days when the number of active users in the municipality was below a given threshold. Furthermore, the number of municipalities included in the sample gradually decreased over the second half of 2020, reflecting a change in the company's business priorities. For consistency, we focus on a balanced panel of municipalities for which we have data for every day over our period of analysis, from February 26, 2020, to January 31, 2021, corresponding to 29 percent of our sample. Appendix B4 provides further details about the construction of this variable.

Electoral data. Municipal electoral data come from the Brazilian elections authority (*Tribunal Superior Eleitoral*, TSE). We performed several data-quality checks using alternative sources such as press articles and municipal official gazettes (see Appendix B5 for further details). For each candidate in each municipality, we know gender, incumbency status, age, race, education level, occupation, party affiliation, and number of votes received. We further attribute to each candidate an ideology score capturing the ideological inclination of their political party, following Power and Rodrigues-Silveira (2019).

Municipalities' characteristics. We use a large set of municipal socio-demographic characteristics to test the validity of our identification strategy and the robustness of our results to the inclusion of controls. Most of these baseline variables are constructed directly from the microdata of the 2010 demographic census (the last one before the 2016 elections). One exception is our measure of density -– the total population living within one kilometer of the average inhabitant of the city – which we compute using 2015 data from the Global Human Settlement Layer (Schiavina et al., 2019), following De la Roca and Puga (2017)'s method. We made sure to include variables that have been shown to predict the geographic variation in COVID-19 deaths, such as population density, the share of residents above 65 years old, proximity to internationally connected airports, the number of nursing home residents, and household income (Chauvin, 2021).[15]

3 Empirical Strategy

3.1 Sample and Descriptive Statistics

To estimate the causal impact of female leadership, we use a regression discontinuity design (RDD) and compare municipalities where a female candidate narrowly defeated a male candidate to those where a male candidate narrowly defeated a female candidate. We thus restrict our sample to the 22.4 percent of Brazilian municipalities where the top two contenders in the 2016 election were one female and one male candidate.[16]

We further exclude municipalities whose COVID-19 outcomes cannot be directly linked to their local government's actions. More precisely, we exclude the 18.6 percent of municipalities that are part of a commuting zone (*arranjos populacionais*), as defined by the Brazilian Institute of Geography and Statistics (IBGE, 2016). A commuting zone is a group of municipalities that are linked through commuting flows and that often coordinate on urban services such as transport. Hence, the number of COVID-19 deaths in a municipality that is part of a commuting zone is tightly linked to the spread of the virus inside the

[15] The 2010 municipal population is also used to normalize the number of deaths so that our main outcome is the number of COVID-19 deaths per 10,000 inhabitants. Between 2010 and our period of analysis, five new municipalities were created from seven parent municipalities. Of these 12 redistricted municipalities, only one qualified for our sample. We removed it to ensure time-consistent geographies throughout our analysis.

[16] In some municipalities, the original election's results were invalidated and a supplementary election took place later. In these cases, we ignore the results of the ordinary election and consider the top two candidates in the supplementary one. This concerns 25 municipalities in our sample and our results are robust to excluding them (see Appendix D). We further identified 40 municipalities where no supplementary election took place but where the votes of one of the original top-two candidates were invalidated by the electoral justice due to irregularities, such as having registered their candidacy after the official deadline. We remove those elections, as the candidates who were eventually assigned first and second place were not the ones who received the most votes. Finally, we exclude one municipality whose supplementary election took place in March 2020, implying that two different mayors were in office during our period of interest, and one municipality where the mayor was removed from office and replaced by the vice-mayor. See Appendix B5 for more details on the electoral data cleaning.

commuting zone and to the policy choices of its neighbors.

Our final sample consists of 981 municipalities. As shown in Appendix Figure A2, municipalities where a female candidate was elected (blue) and municipalities where a male candidate was elected (red) are both evenly spread out across all Brazilian states.

Table 1 presents some descriptive statistics for our sample. The first panel includes socio-demographic characteristics from the 2010 census. The second panel includes political characteristics based on the first round of the 2016 municipal election for turnout and number of candidates[17] and based on the first round of the 2018 presidential election for the vote share of the president at the municipal level. Municipalities in our sample had 13,928 inhabitants on average in 2010; the average monthly median household income per capita was 319 reais (56.2 US dollars at the contemporary exchange rate); and 2.6 candidates ran in the 2016 elections on average. Although municipalities in our sample are on average smaller and less dense than the average Brazilian municipality, 60 percent of the residents in our sample live in urban areas. Moreover, the average municipality in our sample is very similar in all the other socio-demographic and political characteristics to the average Brazilian municipality (Appendix Table A1).

Our sample is also representative of the evolution of COVID-19 in Brazil. Appendix Figure B1 plots the number of COVID-19 deaths over time separately for our sample and for all Brazilian municipalities and shows that the two samples experienced a similar number of deaths per capita throughout the period of analysis. The same is true when looking at the share of phone users staying at home over time (Appendix Figure B3). Finally, Appendix Table B2 presents the share of municipalities that implemented a given containment policy at least once during the period of analysis, separately for our policy sample and for a representative random sample of 20 percent of municipalities with a population of 10,000 or higher, obtained from Chauvin et al. (2021). As in the random sample of municipalities (first two columns), around 90 to 95 percent of municipalities in our sample implemented school closures, event cancellations, and restrictions on gathering and made face masks mandatory. Our analysis therefore focuses on the remaining six policies, for which we have enough variation across municipalities: commerce restrictions, curfews, lockdowns, transport restrictions, travel restrictions, and workplace restrictions.

[17] All municipalities in our sample had fewer than 200,000 eligible voters in 2016 and thus had single-round elections.

3.2 Specification

We define the running variable X as the victory margin of the female candidate (the difference between her vote share and that of the male candidate) and the treatment variable T as an indicator equal to 1 if the winner is a woman $(X > 0)$ and 0 if the winner is a man. We assess the impact of having a female mayor using the following specification:

$$Y_i = \alpha_i + \tau T_i + \beta_1 X_i + \beta_2 X_i T_i + \mu_i \; , \tag{1}$$

where i indexes municipalities.

In the robustness tests, we augment this specification in two ways. First, we add controls for municipality socio-demographic characteristics and for winners' characteristics (other than gender). Second, we include state fixed effects to make sure our results are not affected by state-level politics. All robustness tests are reported in Appendix D.

We use a nonparametric estimation method, which amounts to fitting two linear regressions on each side of the threshold (Imbens and Lemieux, 2008; Calonico et al., 2014). We follow Calonico et al. (2014)'s estimation procedure, which provides robust confidence intervals, and we use the data-driven MSERD bandwidths developed by Calonico et al. (2019). We also show the robustness of the main results to using a second-order polynomial and a wide range of bandwidths. Finally, we follow Calonico et al. (2017) when presenting the RD results graphically: we focus on observations in the estimation bandwidths and we use a linear fit and a triangular kernel, so that the polynomial fit represents the RD point estimator.

As shown in Appendix Table A2, municipalities close to the threshold are very similar to the average municipality in the full sample in terms of both socio-demographic and political characteristics.[18]

3.3 Validity of the Design

3.3.1 Density and Balance Tests

The identification assumption is that all municipalities' characteristics change continuously at the discontinuity, so that the only discrete shift is the change in the mayor's gender. This

[18]For the descriptive statistics, we define municipalities close to the threshold as municipalities where the victory margin is smaller than 4 percentage points, but the estimation bandwidths used in the analysis, being data-driven, vary with the outcomes.

assumption can be violated if candidates are able to sort themselves across the threshold, which would require them to be able to predict and manipulate their vote share with extreme precision.

We perform several tests to support our identification strategy. First, we test for a jump in the density of the running variable, using both McCrary (2008)'s and Cattaneo et al. (2018)'s methods. As shown in Appendix Figures A3 and A4, the victory margin of the female candidate is smooth at the discontinuity.

Second, we test for the balance of municipalities' characteristics at the threshold using a general balance test, following Anagol and Fujiwara (2016). We regress the treatment variable on all 19 baseline variables presented in Table 1, predict the treatment status of each municipality using the regression coefficients, and test for a jump in the predicted value at the discontinuity. As shown in Figure 1 and Appendix Table A3, there is no significant jump at the threshold and the point estimate is small and not significant.

We also test for a jump in each of the baseline characteristics taken individually (see Appendix C). Only one coefficient out of 19 is significant at the 10-percent level. Consistent with Figure A2, municipalities close to the threshold are balanced based on their distance to São Paulo or to the nearest airport, confirming the absence of geographic sorting. They are also balanced on key variables shown to predict the spread of COVID-19, such as density or the share of residents above 65 years old. Turning to political variables, female- and male-led municipalities at the threshold had the same average number of candidates and turnout rate in 2016. Municipalities are also balanced in their political alignment with Jair Bolsonaro, the president in office during the COVID-19 outbreak. They are balanced in characteristics strongly associated with his political base, such as the employment share in agriculture and the share of evangelicals in the population. Moreover, residents of municipalities that closely elected a female or a male mayor in 2016 were equally likely to vote for Bolsonaro in the 2018 presidential election.

Taken together, these results suggest that there is no sorting at the discontinuity. Furthermore, we show that the main results are robust in magnitude and statistical significance to controlling for the whole set of covariates as well as to the inclusion of state fixed effects (Appendix D).

3.3.2 Gender Versus Other Characteristics of the Winner

While using an RDD ensures that the mayor's gender is as good as randomly assigned across municipalities at the threshold, it does not ensure that our results can be interpreted as a gender effect if gender is correlated with other characteristics of the winner.

In order to assess whether female candidates closely defeating male candidates differ from male candidates closely defeating female candidates in attributes other than gender, we test for a jump at the threshold in the following characteristics of the winner: incumbency status, age, race, education, occupation, and political orientation.[19]

In the presence of gender discrimination, a female candidate receiving the same vote share as a male candidate is likely to have attributes that compensate for her initial discrimination-related disadvantage, such as higher ability (Marshall, 2022). While we cannot measure ability directly, we can expect observable characteristics such as education to be at least partly correlated with it. Consistent with positive selection on ability, the coefficient on education suggests that closely elected female mayors are more likely to have completed higher education, even though the effect is not significant (Column 4 of Table 2 and Appendix Figure C2). In contrast, female mayors are not more likely to be the incumbent, to work in the health sector, or to be a business owner and they have similar ideological positions as male mayors.[20]

One concern is that the "compensating attributes" may affect our outcomes of interest independently from gender. To assess the extent to which other characteristics of the winner could bias our findings, we test the robustness of our results to controlling for all winners' characteristics described above, including education. This leaves the point estimates and levels of significance virtually unchanged (Appendix D). We are thus confident that our results can be interpreted as a gender effect, rather than coming from political experience, age, race, education, occupation, or ideology or from other correlated unobserved factors. Section 5.2.3 discusses the selection issue further and provides additional evidence that selection is unlikely to explain our results.

[19] We measure political orientation using an ideological score that summarizes the position of the candidate's political party on a left-right axis (Power and Rodrigues-Silveira, 2019). We also consider indicator variables for the two parties that gathered the most votes during the 2016 elections (PMDB and PSDB, the main center and center-right party, respectively) and for the historical left-wing party (PT).

[20] We observe a similar pattern when looking at *all* 2016 candidates: female candidates are very similar to the average male candidate in terms of age, race, incumbency status, and political orientation, while they are more likely to have completed higher education (72.6 vs. 49.4 percent, on average, Appendix Table A4).

4 Main Results

4.1 Impact of Having a Female Mayor on COVID-19 Deaths

We start by looking at the impact of having a female mayor on the timing of a municipality's first recorded COVID-19 death. We take as outcome the number of days between the last day of 2019 – when the first case of COVID-19 was reported worldwide – and the first death attributed to the disease in the municipality. As shown in Appendix Table A5 and Figure A5, the coefficient is close to zero and nonsignificant.

Given that having a female mayor did not affect the timing at which municipalities started to experience fatalities, we can use the same time frame to study the evolution of COVID-19 deaths in female- and male-led municipalities. We look at the impact on the number of deaths in the four main periods characterizing the evolution of COVID-19 in Brazil (see Appendix Figure B1): beginning of the first wave (April-May 2020), peak of the first wave (June-August 2020), end of the first wave (September-October 2020), and beginning of the second wave (November 2020-January 2021).[21] We normalize the number of deaths by the 2010 population and multiply by 10,000 so that the outcome measures the number of deaths in the municipality per 10,000 inhabitants.

Table 3 shows that, on average, having a female mayor led to a 0.39 increase in the number of deaths per 10,000 inhabitants in the first period, a coefficient significant at the five-percent level. This corresponds to a threefold increase compared to the average number of deaths in male-led municipalities at the threshold. Conversely, we find that female-led municipalities experienced, on average, one fewer death per 10,000 inhabitants in the last period. This effect is significant at the five-percent level and corresponds to a 41.1-percent decrease compared to male-led municipalities. We find no effect during the second and third periods – the middle and end of the first wave. The coefficients are not significant and the point estimates are much smaller, both in absolute terms and compared to the means.

Figure 2 plots the number of deaths against the running variable for each period separately. Consistent with the formal estimation, we see an upward jump at the threshold at the beginning of the first wave, a downward jump at the end of the period of analysis, and no significant jumps for the other two periods.

Appendix Table A6 and Appendix Figure A6 further assess the impact month by month.

[21]We start in April, as no death occurred in municipalities in our sample in March. (Only 201 COVID-19 deaths occurred across the country during this month.)

We see that the positive impact in the first period is mainly driven by a larger number of deaths in female-led municipalities in May 2020, while the negative impact in the last period is driven by a lower number of deaths in female-led municipalities in November and December 2020.

While a higher mortality rate at the beginning could mechanically lead to a lower number of deaths later on (as the affected population develops immunity and a fraction of the most vulnerable residents has already died), such a mechanical effect is unlikely to explain the impact in Period 4. As shown in Appendix Figure A7, there is no correlation between the number of deaths at the beginning and at the end of the year 2020. This is due to the fact that infections were still at very low levels in Period 1, making it implausible that female-led municipalities reached herd immunity earlier in the pandemic.

Finally, we look at how these effects translate into the evolution of the number of cumulative deaths. Figure 3 shows, for each day from April 1 to January 31, the estimated impact of having a female mayor on the number of deaths up to that date. Consistent with female-led municipalities experiencing more deaths at the beginning, the point estimates on the cumulative number of deaths are positive and significant from May to June. They remains positive but not significant up to October, when they approach zero. Next, in line with female-led municipalities experiencing fewer deaths at the end of the year and consistent with the later effect more than compensating for the earlier one, the point estimates become negative starting in November. Looking at the number of deaths over the whole period, we find that having a female mayor reduced cumulative deaths by 0.97 per 10,000 inhabitants as of January 31, 2021 (14.4 percent), on average, but the coefficient is not statistically significant (Appendix Table A7 and Appendix Figure A8).

The impact of female leadership on COVID-19 deaths is robust in both magnitude and significance to the inclusion of municipal baseline characteristics, mayors' characteristics other than gender, and state fixed effects; to the exclusion of unusual observations (Mato Grosso state and supplementary elections); and to specification choices (use of a second polynomial order and different bandwidths). In addition, the same patterns are found if we use as outcome the overall number of SARI deaths (as defined in Section 2.3). Appendix D describes the robustness tests in more detail and presents the corresponding tables and figures.

Female- and male-led municipalities experienced a different evolution in the number of COVID-19 deaths. As municipalities on either side of the threshold have the same characteristics and differ only in the mayor's gender, these results suggest that female and male mayors handled the crisis differently. To further support this interpretation, we now investigate the impact of female leadership on containment policies and on residents' isolation behavior.

4.2 Impact on Policies and Isolation

4.2.1 Impact of Having a Female Mayor on Containment Policies

As discussed in Section 2.3, our policy analysis focuses on municipalities with above 10,000 inhabitants and we consider six policies for which we have enough variation across municipalities: commerce restrictions (closing non-essential businesses), curfews, lockdowns, and workplace, travel, and public transport restrictions.

For each policy, Figure 4 shows the estimated impact of having a female mayor on the probability that the policy was in place in the municipality, for each day from March through October 2020. For most policies, the estimates are nonsignificant and small over most of the period of analysis. In contrast, we see large and significant estimates for commerce restrictions and a stark reversal, showing that female mayors were significantly less likely to close non-essential businesses at the beginning of the pandemic but became significantly more likely to do so at the end of the year.

We next look at the impact of having a female mayor on the adoption of a given policy by calendar month. For each policy and month, we define our dependent variable as the number of days in which the policy was in place in the municipality.

Table 4 presents the results for commerce restrictions. On average, non-essential businesses were closed 3.0 and 8.6 fewer days in female-led municipalities in March and April, respectively, compared to an average of 3.9 and 13.5 days in male-led municipalities at the threshold. Both coefficients are significant at the one-percent level. As shown in Appendix Table A8, these effects are driven by the fact that female mayors started closing non-essential businesses 43 days later on average, an effect that is significant at the one-percent level (Column 1). Given that closing commerce was one of the first policies to be implemented in response to the crisis, this implies that female mayors were more likely to delay the implementation of any containment policy (Column 7).

Instead, non-essential businesses were closed on average 7.8 and 7.6 more days in female-led municipalities in September and October, respectively. The point estimates represent a twofold increase relative to male-led municipalities and are significant at the 5- and 10-percent level, respectively. These effects appear to be driven by male mayors being more likely than female mayors to lift commerce restrictions at the end of the first wave. Indeed, the average number of days with commerce restrictions in male-led municipalities started decreasing in July 2020 and we find that male mayors were 15 percentage points more likely to reopen non-essential businesses between August and October 2020 (Appendix Table A9).

Appendix Figure A9 provides the RDD graphs for each month. While we see a large downward jump in March and April, the discontinuity gradually disappears in subsequent months, before turning into large upward jumps in September and October.

In line with Figure 4, we do not find significant effects for the other five policies, with the exception of the last two coefficients on travel restrictions, which are significant at the 10-percent level (Appendix Tables A10 to A14). Throughout the period of analysis, female mayors appear more likely to implement curfews and less likely to impose lockdowns and travel restrictions (preventing outsiders from coming in), but the coefficients are imprecisely estimated. Female- and male-led municipalities thus differed mainly in their use of commerce restrictions. This could be explained by the fact that commerce closure was a key policy instrument used by Brazilian mayors during the pandemic, with a lot flexibility over time.[22] Other policies, such as curfews and lockdowns, were more extreme and therefore adopted only rarely and only by a small fraction of municipalities, making variation across municipalities difficult to detect (Appendix Table B2).

The results are robust to exploiting within-state variation only, through the inclusion of state fixed effects (Appendix Table A15). This shows that the effects we find are not driven by female and male mayors being subject to different state policies, but are attributable to the mayors' own policy choices.

The timing of the policy results aligns well with the evolution of the number of COVID-19 deaths: female mayors were less likely than male mayors to close commerce in March and April and female-led municipalities experienced more deaths in May; they became more likely than male mayors to close commerce in September and October and their municipalities experienced fewer deaths in November and December. We nevertheless refrain from making a causal claim on the relationship between commerce restrictions and COVID-19 deaths; we only partially observe the mayors' actions and formally assessing the causal impact of policies on COVID-19 deaths is beyond the scope of this paper. Still,

[22]More than 20 percent of the municipalities in our sample that closed non-essential businesses at least once did so several times over the period of analysis. This flexibility was commonly referred to in the media as the "open-close policy" (e.g., in interviews with epidemiologists and private sector leaders).

we see these results as evidence that the effect we find on COVID-19 deaths reflects the fact that female and male mayors responded differently to the crisis over time.

4.2.2 Impact of Having a Female Mayor on Residents' Isolation Behavior

Finally, we measure the impact of having a female mayor on residents' isolation behavior, using InLoco's isolation index. This index is defined as the share of phone users in a municipality who stayed home on a given day. Figure 5 shows daily RD estimates of the effect from February 25, 2020, to January 31, 2021.

For most of the period of study, we find no statistically significant female-mayor effect on residents' isolation behavior. The point estimates are positive in the first few weeks of the pandemic, but the effects are imprecisely estimated and not significant. In the following months – May through October – they remain close to zero. This nonsignificant impact on isolation is consistent with the fact that female and male mayors differ mainly in their use of commerce restrictions. Indeed, closing non-essential businesses does not restrict mobility per se; it mainly reduces the risk of contamination by preventing people from entering closed spaces (Goolsbee and Syverson, 2021).

In sharp contrast with the null effects found over most of the period of interest, Figure 5 shows a large, positive, and statistically significant effect of having a female mayor on the share of residents staying at home around the day of the election. In other words, residents in male-led municipalities were significantly more likely to go out around election day. Appendix Table A16 zooms in on this period, providing separate estimates for each day around Sunday, November 15. We find that the positive effect is driven by the two days prior to the election (Columns 4 and 5) – Friday and Saturday, the last two days in which campaigning was legally allowed – and by a few days in the week immediately after the election. On those days, the impact represents an increase of 10 to 20 percent in the share of residents staying at home in female-led municipalities, compared to male-led municipalities.[23]

These effects are likely the result of male mayors being more likely to organize in-person events during the 2020 electoral campaign and following the election results. Indeed, given that electoral authorities banned the use of mass messaging on social media during the 2020 election period, candidates had incentives to hold in-person events instead, despite

[23]Note that the null effect on election day (Column 6, Appendix Table A16) is consistent with mandatory voting in Brazilian municipal elections.

the social distancing regulations in place. Local media reported multiple breaches of sanitary protocols; in particular, large in-person gatherings violating the social distancing recommendations (Tarouco, 2021).[24]

Together with the impact on commerce closure, these results suggest that male mayors were more likely than female mayors to open up their municipalities at the end of the year.

5 Mechanisms: The Role of Electoral Incentives

Overall, our results show that female and male mayors responded differently to the crisis: while female mayors were less likely to undertake containment efforts at the onset of the pandemic, male mayors were more likely to relax them at the end of the year.

We now explore the role of electoral incentives in explaining these gender differences. Municipal elections were held in November 2020, meaning that, since the start of the pandemic, mayors planning to run for reelection knew they would face their voters in the near future and likely be reelected or voted out based on their response to the crisis. In Section 5.1, we first outline a simple political agency model that illustrates how our results can arise from the optimal choice of reelection-seeking politicians if voters are gender-biased. Section 5.2 then provides further empirical evidence on the role of electoral incentives and gender bias.

5.1 A Simple Model of Political Agency With Voter Bias

Public Good

In our model, society is a representative democracy made up of a mass one of voters and one politician. Voters derive utility from the consumption of a public good g, which, in our application, is health. In normal times, the amount of the public good available to voters is fully predictable and is given by $\overline{g}$.

Society faces an emerging shock that threatens to reduce the public good according to $g = \overline{g}\ \exp(-\psi)$, where ψ represents the severity of the shock, and $\psi > 0$, such that $g < \overline{g}$.

The politician has access to a policy instrument, $0 \leq P \leq 1$, which can mitigate the impact of the shock. In our application, this is any action the mayor can take to contain the spread of the COVID-19 virus and reduce the number of deaths.

[24]Anecdotal evidence includes large in-person campaign events in the week leading to the election and victory parties, parades, and concerts in the week after the election.

The amount of the public good that will be available after the shock depends on the severity of the shock ψ and on the policy intensity P, according to the following production function:

$$g = \overline{g}\ \exp(-\psi\ \exp(-\lambda P)\,)\ , \tag{2}$$

where $\lambda \geq 1$ is a parameter that captures the effectiveness of the politician's actions at mitigating the effects of the shock.[25]

Voters' Utility

Voters observe the policy level enacted by the politician. They draw utility from the amount of public good that they believe will be available after the shock and direct disutility from the policy, according to:

$$U = \tilde{g}\exp(-P)\ , \tag{3}$$

where $\tilde{g}$ is the anticipated amount of public good.

Equation 3 reflects the tradeoff for containment policies: on the one hand, they increase voters' utility by preserving the public good, but on the other, they impose a direct cost on voters by closing the economy and limiting freedom. Importantly, the disutility caused by the policy enters Equation 3 multiplying $\tilde{g}$, such that the larger the anticipated level of public good, the more disutility the policy generates. This captures voters' higher willingness to accept containment policies if the shock is perceived as more severe, in line with recent survey evidence across 15 countries showing that the willingness to sacrifice civil liberties increases with the perception of health insecurity (Alsan et al., 2023). While our model is motivated by the COVID-19 pandemic context, it can apply to other crisis contexts involving policy interventions that can be costly to voters and thus potentially unpopular, such as fiscal austerity policies or environmental policies.

[25]We arrive at this formulation by assuming that the public good is produced according to $g = \overline{g} - \overline{g}\, f_1(\psi_m)$ and that $\psi_m = \psi\,(1 - f_2(P))$, where ψ_m represents the severity of the shock that remains after the policy is implemented. We then define the shock's damage function as $f_1(\psi_m) = 1 - exp(-\psi_m)$ and the policy abatement function as $f_2(P) = 1 - exp(-\lambda P)$. Combining these expressions into the production function and simplifying yields Equation 2. Similar specifications have been used in a long-standing plague control literature on the optimal use of pesticides (e.g., Talpaz and Borosh, 1974; Lichtenberg and Zilberman, 1986; Hall and Moffitt, 2002).

Voters' Beliefs

The level of public good that voters anticipate is based on their subjective beliefs about (a) the extent of the shock and (b) the effectiveness of the policy, such that $\tilde{g}$ can differ from g.[26]

With respect to (a), voters believe that the shock will happen with probability $0 \leq p \leq 1$. Only when $p = 1$ do voters believe that the full potential of the shock (ψ) will materialize.[27] Indeed, opinion surveys suggest that many Brazilian respondents doubted the gravity of the threat at the beginning of the pandemic, when the exposure to COVID-19 deaths was low, and that this perception shifted as the first wave progressed (Ipsos, 2020). We will thus consider the behavior of politicians for different levels of p.

With respect to (b), voters are gender-biased and assess the actions of female politicians less favorably. Specifically, we assume that voters believe that crisis containment policies will be less effective if implemented by a female politician than if implemented by a male politician, such that $\lambda_f < \lambda_m$. This assumption implies that female leaders get less credit for their actions, in line with evidence showing that voters assess female political leaders less favorably than male politicians, particularly during crises, when traits that society associates with males – such as strength and assertiveness – are more valued (Eagly and Karau, 2002; Lawless, 2004; Bertrand and Duflo, 2017).

Under these two assumptions, we obtain the voters' utility:

$$U = \overline{g}\ \exp(-p\,\psi\ \exp(-\lambda_s P)\,)\ \exp(-P)\,, \tag{4}$$

where $s = \{m, f\}$ indexes the politician's gender, with m (f) denoting male (female) politicians.

Elections and Optimal Policy

We assume that politicians seek to maximize their likelihood of reelection. Since that is an unobserved positive function of voters' utility, politicians optimize it by choosing the

[26]Several recent papers in political economy also consider agents who make decisions based on potentially misspecified subjective models (Esponda and Pouzo, 2016); this includes papers on the consequences of competing political narratives (Eliaz and Spiegler, 2020) and the recurrence of populism (Levy et al., 2022).

[27]The probability p could also be interpreted as the share of the electorate that believes that the shock will take place and will have a severe public health impact.

policy level that maximizes voters' utility (Equation 4).[28] This yields the following optimal policy level:

$$P_s^* = \frac{1}{\lambda_s} \log(\lambda_s p \psi) \; . \tag{5}$$

We are interested in how the optimal policy P_s^* varies with voters' gender-biased beliefs about policy effectiveness (λ_s) and their beliefs about the severity of the threat (p). Both are interrelated, as shown by the interaction of these two terms in Equation 5.

Figure 6 plots the optimal policy level as a function of λ_s at two levels of p. Recall that the model assumes that voters attribute a lower level of policy effectiveness to female politicians. For instance, they may assume that $\lambda_f = 2$ and $\lambda_m = 4$. When voters believe that the probability p of a shock is small (left graph), the level of optimal policy P_s^* is increasing in λ_s over much of the support of λ_s, meaning that a female politician would implement a lower level of policy than a male politician would. In contrast, when voters believe that the probability p of a shock is large (right graph), the level of optimal policy P_s^* is decreasing with λ_s over much of the support of λ_s, meaning that a female politician would choose a higher level of policy.

Intuitively, when voters believe that the threat to the public good is low – as at the beginning of the pandemic in Brazil – the marginal cost to them of an additional unit of policy is high. This makes it very costly for politicians to close the economy and even more costly for female mayors, as voters view their policies as less cost-effective than those of male leaders. In this context, the optimal policy – the point at which the marginal benefit of the policy to voters just outweighs the marginal cost – is low and is even lower if the mayor is a woman (as illustrated by Appendix Figure E1).

In contrast, when voters believe there *is* a significant threat – for example, after the consequences of the first wave materialized – they are more willing to bear the disutility associated with the policies in order to preserve the public good.[29] In this case, the marginal cost of the policy to voters is lower. Policies are tolerated up to the point where the

[28]Specifically, we assume that voters will reelect a politician if their utility – a function of the politician's policy choice – is higher than their reservation utility. We assume that politicians know voters' preferences and beliefs, but do not observe the reservation utility that voters have in crisis times. Their best strategy is therefore to choose the policy that delivers the maximum utility possible given voters' beliefs and the severity of the shock.

[29]As discussed in Section 2, the support for containment policies increased during the first wave in Brazil, consistent with people's willingness to sacrifice civil liberties increasing with the perception of health insecurity (Alsan et al., 2023).

anticipated level of public good is deemed high enough such that the marginal benefit no longer justifies the marginal cost. Because containment actions by male mayors are perceived as more effective, they don't need to implement as high a level of policy as female mayors do to reach this point (Appendix Figure E2).

5.2 Empirical Evidence

5.2.1 Gender Differences and Electoral Incentives

If, as in the model, our results are driven by female and male mayors facing different electoral incentives, we should see the effects concentrated among electorally motivated mayors. We consider two measures of electoral incentives to test this prediction. First, we exploit the two-term limit and compare mayors who could run for reelection to those who could not. Second, we depart from the RD framework to test whether our results vary with the margin of victory and thus with the competitiveness of the election.[30]

Term Limits

In Brazil, mayors can hold office for two consecutive terms only, meaning that mayors reelected in 2016 – that is, those who ran in 2016 as incumbents – could not run again in 2020. As stressed by Ferraz et al. (2012), being term-limited is a particularly strong indicator of electoral incentives in the Brazilian context. Indeed, given the absence of incumbency advantage in Brazilian municipal elections (Anagol and Fujiwara, 2016), first-time mayors cannot take reelection for granted. Moreover, only a very small fraction of term-limited mayors return to office – either at the municipal level after a one-term hiatus or in higher-level offices – making them unlikely to be motivated by future political career concerns.

In order to test whether the results are driven by mayors who *can* run for reelection, Table 5 compares municipalities where, in 2016, the mayor was elected for the first time (thus permitted to run in 2020) to municipalities where the mayor was elected as an incumbent (thus not permitted to run in 2020). Note that conditioning on the incumbency status of the

[30]We run all heterogeneity analyses focusing on our main outcome of interest: the number of COVID-19 deaths. Data on deaths are available for our full sample, whereas the policy and isolation results are only derived for a subset of the observations (about 50 and 25 percent, respectively), making subsample analysis difficult. Moreover, given that municipalities differ only in the mayor's gender, differences in deaths capture differences in crisis management by gender, including – but also going beyond – what we can measure using our policy and isolation indicators.

ultimate winner is unlikely to create selection issues in our setting, given the null impact of the treatment on the probability that the 2016 winner is the incumbent (Section 3). We replicate our main analysis on COVID-19 deaths in each subsample separately. Consistent with the results being driven by mayors with electoral incentives, the point estimates are large and significant only when the mayor is not term-limited and can thus run again in 2020 (Columns 3-4 vs. 5-6).

We obtain similar results when conditioning on the incumbency status of the top two candidates rather than that of the ultimate winner. Appendix Tables A17 and A18 compare the results across the following three subsamples: (a) neither of the two front-runners ran as an incumbent in 2016, so that the treatment captures the impact of having a non–term-limited female mayor versus a non–term-limited male mayor (that is, both can run for reelection); (b) only the male candidate ran as an incumbent, so that the treatment captures the impact of having a non–term-limited female mayor versus a term-limited male mayor (that is, only the female mayor can run for reelection); and (c) only the female candidate ran as an incumbent, so that the treatment captures the impact of having a term-limited female mayor versus a non–term-limited male mayor (that is, only the male mayor can run for reelection).

Appendix Table A17 focuses on the first period, when female-led municipalities experienced significantly more COVID-19 deaths than male-led municipalities. The point estimate remains large and positive only in municipalities where female mayors can run for reelection (Columns 2 and 3). Appendix Table A18 looks instead at the last period, when female-led municipalities experienced significantly fewer COVID-19 deaths than male-led municipalities. In this case, the negative impact on deaths is exclusively driven by municipalities where the male mayor can run for reelection (Columns 2 and 4).

These results are consistent with the model intuition and with female and male mayors adopting different electoral strategies: electoral incentives push female mayors to undertake less containment effort than male mayors at the beginning of the year, when the crisis is more uncertain, but push male mayors to undertake less containment effort than female mayors at the end of the year, when the severity of the crisis has become clearer.

One concern could be that term-limited and non–term-limited mayors do not differ only in the fact that they face stronger or weaker electoral incentives. Indeed, second-term mayors have been reelected, meaning that they have more experience in office, and implying that they may have higher abilities (if higher-ability candidates are more likely to get reelected). This is, however, unlikely to explain the patterns we observe. First, the

COVID-19 crisis started in the last year of the mayors' term, meaning that first-time mayors already had three years of experience. Second, when focusing on first-term mayors, we do not see that the effects are systematically weaker for college-educated mayors, older mayors, or mayors who served as municipal legislators during the previous term, suggesting that less-able or less-experienced mayors are not driving the results (Appendix Tables A19 to A21).[31] Third, as shown in the next section, we also find that our results are driven by more-competitive elections, a proxy for electoral incentives that does not rely on term limits.

Election Competitiveness

By construction, the RDD focuses on mayors who won by a small margin in 2016 and who are thus likely to face more competition in the next election than mayors who secured a large victory margin. If electoral incentives are driving our results, we would expect the effects to be larger for the former.

To test this prediction, we run an OLS estimation in which we regress our main COVID-19-death outcomes on the treatment variable (having a female mayor) and include an interaction term between the victory margin and the treatment variable. We also include the victory margin in the regression and control for all municipality characteristics displayed in Table 1 and all winner characteristics displayed in Table 2. While the causal interpretation of the effects is more difficult, this analysis allows us to see how the impact evolves as the victory margin of the mayor increases.

As shown in Table 6, the impact of the treatment goes in the same direction as in the main RD analysis for both periods. In the full sample, the point estimates associated with the treatment are small and nonsignificant (Columns 1 and 4). The effects become large and significant when we restrict the sample to more competitive elections – those with a victory margin smaller than 10 or 5 percentage points (Columns 2 and 4 and Columns 3 and 6, respectively). More interestingly, in all regressions, the coefficient of the interaction term is negative in Period 1 and positive in Period 4. Focusing on elections won by a vote margin smaller than 10 percentage points, we see that the estimates associated with the treatment are very close to those obtained with the RDD (0.37 for Period 1 and -0.95 for

[31] First-time mayors could have served as municipal legislators during the previous term. Elections for municipal legislators and for mayors happen at the same time and involve the same voters. While the subsample becomes very small, Appendix Table A21 shows that the coefficients are not smaller in magnitude for mayors who served as legislators during the 2012-2016 term.

Period 4; Columns 2 and 4). This is reassuring, as these effects can be interpreted as the impact of having a female mayor when the vote margin is zero, which corresponds to the impact at the discontinuity estimated with the RDD. The coefficient of the interaction term further shows that the magnitude of the effect decreases as the victory margin increases, disappearing if we go from a 0- to a 10-percentage-point victory margin.[32]

These results show that the impact is larger in more competitive races, where mayors face stronger electoral incentives. They also suggest that, when gender differences in policymaking are due to female and male leaders facing different electoral incentives, the effects captured by close election designs are likely to dissipate in less contested races.

5.2.2 Gender Differences and Voters' Gender Bias

The model assumes that female and male mayors face different electoral incentives because voters assess their actions differently. We test this assumption by running heterogeneity analyses based on the extent of gender discrimination in the municipality. If the empirical results stem from voters' gender bias, we expect the impact to be larger in municipalities with greater gender discrimination.

To proxy for gender discrimination and thus voters' gender bias at the municipal level, we follow Le Barbanchon and Sauvagnat (2022) and use the gender wage gap. Specifically, we consider all workers living in a given municipality and compute the gap in the wages received by female and male workers, after accounting for age, education, and occupation. Table 7 focuses on municipalities where the mayor can run for reelection, which drive our effects (Section 5.2.1). As shown in Columns 3 to 6, the positive impact on COVID-19 deaths in Period 1 and the negative effect in Period 4 are mainly driven by municipalities above the median gender wage gap. In this subsample (as in the full sample), the coefficients are large and significant at the five-percent level, whereas the coefficients are small and not significant for municipalities below the median. We obtain a similar pattern if we consider the gender gap in labor force participation as an alternative proxy for gender discrimination (Appendix Table A22).

[32] To see that, we divide the point estimate of the interaction term by 10 and add it to the point estimate of the treatment effect.

5.2.3 Alternative Mechanisms

The evidence presented above is consistent with our main results being driven by electoral incentives and gender bias. We now discuss alternative interpretations for our findings.

Selection. As discussed in Section 3, in the presence of voters' gender discrimination, closely elected female mayors are likely to be positively selected compared to closely elected male mayors. For positive selection to affect our results, the attributes that compensate for discrimination against female candidates need to be correlated with our outcomes. While it is unlikely that candidates in the 2016 election were selected based on their health crisis management skills, as the pandemic was not yet in voters' minds, voters could have selected candidates based on general ability, which could then affect health policy decisions. Two pieces of evidence suggest, however, that our results are not driven by such positive selection. First, as shown in Appendix D, while female candidates are indeed more educated on average, the results remain unchanged when controlling for education and for other characteristics likely correlated with ability, such as political experience. Second, we run an alternative RDD analysis in which we focus on male candidates and compare municipalities led by mayors with or without high education; we find no significant differences in the evolution of COVID-19 deaths (Appendix Table A23).

Policy preferences. Gender differences in COVID-19 responses could come from gender differences in policy preferences. Specifically, we could have expected female leaders to prioritize public health and adopt more containment policies, in line with evidence showing that female politicians tend to invest more in health (Bhalotra and Clots-Figueras, 2014; Funk and Philips, 2019) and that women in the population took the COVID-19 risk more seriously than men (Vincenzo et al., 2020). While this interpretation could rationalize the later effect, it does not explain why female mayors delayed their crisis response at the beginning of the pandemic. Moreover, it would not account for the fact that gender differences materialize only when mayors face electoral incentives.[33]

Our results are also unlikely to be driven by female candidates being elected by different groups of voters with different preferences. To account for our findings, voters' preferences would have needed to change over time – and in opposite directions – for voters supporting female versus male candidates. Furthermore, the population composition of female- and male-led municipalities is balanced at the threshold (Section 3.3) and voting is mandatory

[33]The same reasoning holds if we instead expect female mayors to prioritize leaving the economy open: this would rationalize the earlier effect but not the later and would not account for the effects being driven by electoral incentives.

in Brazil, meaning that closely-elected female and male mayors faced the same electorate.

Risk aversion. Alternatively, the effects could be driven by gender differences in risk aversion (Eckel and Grossman, 2008; Croson and Gneezy, 2009). This could have made female mayors more likely to wait and learn, behaving more cautiously at the beginning of the pandemic and reversing course over time as the severity of the shock became more clear. However, if this was the main mechanism behind our results, the same time-varying patterns should hold with or without electoral incentives.

Gender differences in risk aversion could still account for our results if we assume that female mayors are more averse to the risk of losing an election. This would have led them to adopt the least electorally risky moves; that is, delaying containment efforts at the beginning but upholding them towards the end of the first wave. Several pieces of evidence go against this interpretation. First, we show in Section 5.2.2 that the effects are driven by municipalities with greater gender discrimination. This supports the hypothesis that female mayors acted in response to voters' bias rather than being driven by a higher intrinsic risk aversion, which would be independent of voters' assessment.[34] Second, some evidence suggests that gender differences in risk aversion found in the overall population tend to dissipate with education level and for people working as managers or entrepreneurs, which are considered more risk-taking careers (Croson and Gneezy, 2009). The fact that our results are not driven by less-educated mayors (Appendix Table A19) and that we study individuals who self-selected into a highly competitive environment makes risk aversion unlikely to be the main driver of our results. Finally, more risk aversion to losing reelection would lead female mayors to take the actions most aligned with voters' preferences and, absent voter discrimination, should help female incumbents secure a higher vote share in the next election. Instead, we find no gender differences in the probability of running or being reelected or in vote share in the 2020 election (Appendix Table A24), consistent with male and female mayors optimizing their policy choices by factoring in the gender biases in voters' assessments.

[34]Our results suggest that gender differences in crisis response arise in response to voters' gender bias. This interpretation relates to several studies that find that gender differences in risk aversion arise from women expecting negative consequences from not conforming to gender stereotypes, suggesting that gender norms could also be the ultimate driver of gender differences in risk aversion (Larkin and Pines, 2003; Kawakami et al., 2007; Carr and Steele, 2010).

6 Conclusion

This paper provides new evidence that electoral incentives can explain gender differences in the behavior of leaders.

Using a regression discontinuity design in Brazilian municipal elections, we first show that female mayors handled the COVID-19 crisis differently over time than male mayors did. We find that having a female mayor led to three times more deaths at the beginning of the pandemic (May 2020) compared to the average male-led municipality but to 41.1-percent fewer deaths at the end of the year (November-December) compared to the average male-led municipality.

Consistent with mayors' decisions driving these effects, we show that female mayors were less likely than male mayors to impose commerce restrictions early – in February and March – but became more likely than male mayors to do so later – in September and October. Further evidence suggests that a lower share of residents stayed at home in male-led municipalities around election day. Hence, while female mayors were more likely to delay containment efforts at the beginning, male mayors were more likely to relax them at the end of the year.

We then show that the observed gender differences are best explained by female and male mayors facing different electoral incentives. Mayors' behavior can be rationalized by a simple political agency model in which (a) politicians seek reelection and (b) voters evaluate the actions of female and male politicians differently, leading female and male politicians to adopt different policies. Consistent with electoral incentives and voter bias explaining the behavior of mayors, we find that our results are driven exclusively by non–term-limited mayors who can run for reelection and that the effects are stronger in more competitive races and in municipalities where gender discrimination is more prevalent.

All in all, our paper shows that gender differences in leaders' behavior can be explained by leaders' incentives to adapt their policy choices to voters' gender biases.

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Tables

Table 1: Descriptive statistics

	Mean	Sd	Min	Max	N
Panel A	*Socio-demographic characteristics*				
Population	**13,928**	12,724	1,037	91,311	981
Density	**119.5**	186.3	0.0	3,467.9	981
Average persons per room	**0.704**	0.243	0.435	4.282	981
Commuting time	**21.57**	4.57	9.03	44.59	981
Share of population $\geq$ 65 years old	**0.083**	0.023	0.022	0.179	981
Nursing home residents per 10k pop	**3.742**	11.488	0.000	209.939	981
Area	**1,765**	5477	27	84,568	981
Distance to São Paulo	**1,448**	739	49	3,441	981
Km to airport connecting to COVID hot spots	**301.3**	214.6	23.1	1,556.9	981
Median household income p/c	**319.3**	143.9	80.0	836.5	981
Informality rate	**0.169**	0.055	0.036	0.418	981
Unemployment rate	**0.044**	0.021	0.000	0.173	981
College graduate employment share	**0.067**	0.030	0.005	0.192	981
Black and mixed-race population share	**0.600**	0.215	0.019	0.952	981
Agriculture employment share	**0.422**	0.149	0.024	0.814	981
Evangelical share of population	**0.156**	0.091	0.009	0.838	981
Panel B	*Political characteristics*				
Turnout	**0.855**	0.059	0.673	0.980	981
Number of candidates	**2.642**	0.920	2.000	9.000	981
President's vote share	**0.318**	0.186	0.025	0.808	981

Notes: The sample includes municipalities outside of any commuting zones (*arranjos populacionais*) and where one man and one woman were the two front-runners in the 2016 election. Socio-demographic variables come from the 2010 census, except density, which is defined as the total population living within 10 km of the average inhabitant of the municipality and which is computed using the 2015 data from the Global Human Settlement Layer. The political variables are computed using the results of the first round of the 2016 municipal election, except for *President's vote share*, which uses data from the first round of the 2018 presidential election.

Table 2: Balance test: Characteristics of the election winner

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Outcome	Incumbent	Age	White	Higher education	Occupation				Ideology score	PMDB	PSDB	PT
					Politics	Public	Health	Business				
Treatment	-0.040	-0.814	0.132	0.155	-0.021	0.029	-0.049	0.025	0.081	0.021	0.031	0.019
	(0.077)	(1.929)	(0.075)	(0.100)	(0.075)	(0.060)	(0.048)	(0.053)	(0.056)	(0.062)	(0.048)	(0.034)
R. p-value	0.591	0.822	0.123	0.299	0.736	0.734	0.461	0.644	0.246	0.889	0.481	0.563
Observations	604	573	592	482	565	527	630	617	728	565	593	516
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	0.141	0.131	0.138	0.107	0.130	0.122	0.147	0.144	0.191	0.130	0.138	0.119
Mean	0.260	48.972	0.644	0.445	0.212	0.112	0.121	0.102	0.206	0.154	0.053	0.022

Notes: In Column 1 (resp. 3, 4, 5, 6, 7, 8, 10, 11, 12), the outcome is an indicator variable equal to 1 if the winner of the 2016 election is the incumbent (resp. is white; has completed higher education; works in politics, the public sector, or the health sector or is a business owner; runs under the PMDB, PSDB, or PT party label). In Column 2, the outcome is the age of the 2016 winner at the time of the election. In Column 9, the outcome is an ideological score based on the candidate's party and ranging from -1 (most to the left) to 1 (most to the right). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and we use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table 3: Impact on COVID-19 deaths, by period

	(1)	(2)	(3)	(4)
Outcome	# COVID-19 deaths per 10,000 inhabitants			
	Period 1	Period 2	Period 3	Period 4
Treatment	0.391**	-0.057	-0.192	-0.999**
	(0.176)	(0.511)	(0.281)	(0.405)
Robust p-value	0.035	0.846	0.466	0.016
Observations	578	498	677	513
Polyn. order	1	1	1	1
Bandwidth	0.133	0.113	0.163	0.118
Mean, left of threshold	0.203	2.580	1.380	2.432

Notes: Each column takes as outcome the number of deaths per 10,000 inhabitants (using the 2010 population) during the period of interest. Period 1 (resp. 2, 3, and 4) is April-May 2020 (resp. June-August 2020, September-October 2020, and November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table 4: Impact of having a female mayor on commerce restrictions, by month

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Number of days with commerce restrictions in place							
	03/20	04/20	05/20	06/20	07/20	08/20	09/20	10/20
Treatment	-3.044***	-8.620***	-5.920	-2.265	1.590	3.948	7.756**	7.552*
	(0.962)	(2.720)	(3.632)	(3.875)	(3.974)	(3.845)	(4.334)	(4.311)
Robust p-value	0.002	0.002	0.183	0.772	0.486	0.212	0.050	0.055
Observations	240	282	265	244	218	238	238	238
Polyn. order	1	1	1	1	1	1	1	1
Bandwidth	0.102	0.129	0.118	0.103	0.091	0.100	0.099	0.099
Mean, left of threshold	3.859	13.536	13.882	12.432	12.061	8.685	7.522	6.971

Notes: The sample is restricted to municipalities with above 10,000 inhabitants. The outcome is the number of days during which the policy was in place, separately for each month. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table 5: Impact on COVID-19 deaths, by mayor term-limit status

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths					
	Full sample		Mayor can run		Mayor cannot run	
Periods	1	4	1	4	1	4
Treatment	0.391**	-0.999**	0.548**	-1.250**	0.013	-0.557
	(0.176)	(0.405)	(0.224)	(0.523)	(0.273)	(0.679)
Robust p-value	0.035	0.016	0.014	0.019	0.849	0.499
Observations	578	513	387	375	120	149
Polyn. order	1	1	1	1	1	1
Bandwidth	0.133	0.118	0.120	0.111	0.109	0.137
Mean, left of threshold	0.203	2.432	0.197	2.722	0.137	1.755

Notes: In Columns 3 and 4, the sample is restricted to municipalities where the mayor elected in 2016 was not the incumbent and was thus allowed to run again in 2020 (i.e., not term-limited). In Columns 5 and 6, the sample is restricted to municipalities where the mayor elected in 2016 was the incumbent and was thus not allowed to run again in 2020 (i.e., term-limited) The outcome is the total number of deaths per 10,000 inhabitants (using the 2010 population) during the first period (April-May 2020) in Columns 1, 3, and 5 and during the last period (November 2020-January 2021) in Columns 2, 4, and 6. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table 6: OLS estimates of the impact of having a female mayor on COVID-19 deaths

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	# COVID-19 deaths in Period 1			# COVID-19 deaths in Period 4		
VM	All	$\leq$ 10pp	$\leq$ 5pp	All	$\leq$ 10pp	$\leq$ 5pp
Treatment	0.064	0.368**	0.697***	-0.102	-0.954**	-0.982*
	(0.088)	(0.172)	(0.225)	(0.236)	(0.431)	0.567
T*VM	-0.621	-4.906	-18.040***	0.606	15.408**	14.383
	(0.398)	(2.990)	(6.844)	(1.277)	(7.463)	(18.190)
Obs.	981	458	252	981	458	252
Mean	0.402	0.458	0.434	2.343	2.200	1.993

Notes: The outcome is the number of deaths per 10,000 inhabitants (using the 2010 population) during the first period (April-May 2020) in Columns 1-3 and during the last period (November 2020-January 2021) in Columns 4-6. Columns 1 and 4 include all observations, while Columns 2 and 4 (resp. 3 and 6) include only elections won by a victory margin smaller than 10 (resp. 5) percentage points in 2016. The treatment (T) is an indicator variable equal to 1 if the female candidate won. All regressions include the victory margin (VM) and control for municipality and winner characteristics (listed in Tables 1 and 2, respectively). Robust standard errors are in parentheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities.

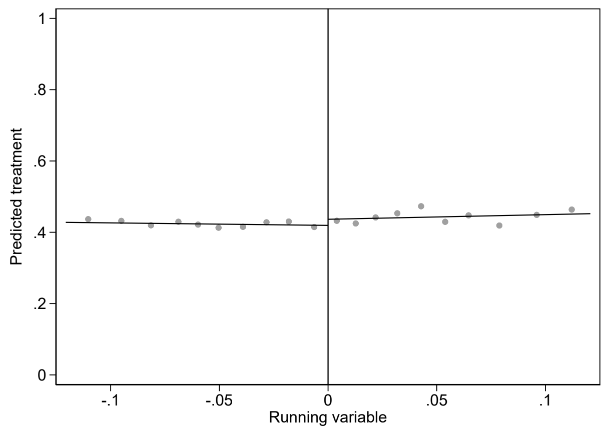
Table 7: Impact on COVID-19 deaths, by municipality's gender wage gap

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths					
	Full sample		Above median		Below median	
Periods	1	4	1	4	1	4
Treatment	0.548**	-1.250**	0.693**	-2.141**	0.306	-0.111
	(0.224)	(0.523)	(0.291)	(0.853)	(0.316)	(0.560)
Robust p-value	0.014	0.019	0.011	0.011	0.432	0.925
Observations	387	375	206	171	207	180
Polyn. order	1	1	1	1	1	1
Bandwidth	0.120	0.111	0.130	0.095	0.126	0.106
Mean	0.197	2.722	0.110	3.515	0.347	1.582

Notes: The sample includes only elections in which the mayor is not term-limited and can run for reelection. Columns 3 and 4 (resp. 5 and 6) further restrict the sample to municipalities in which the gender wage gap is above (resp. below) the median. In Columns 1, 3, and 5 (resp. 2, 4, and 6), the outcome is the number of COVID-19 deaths per 10,000 inhabitants (using the 2010 population) in Period 1 (resp. Period 4), which is April-May 2020 (resp. November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

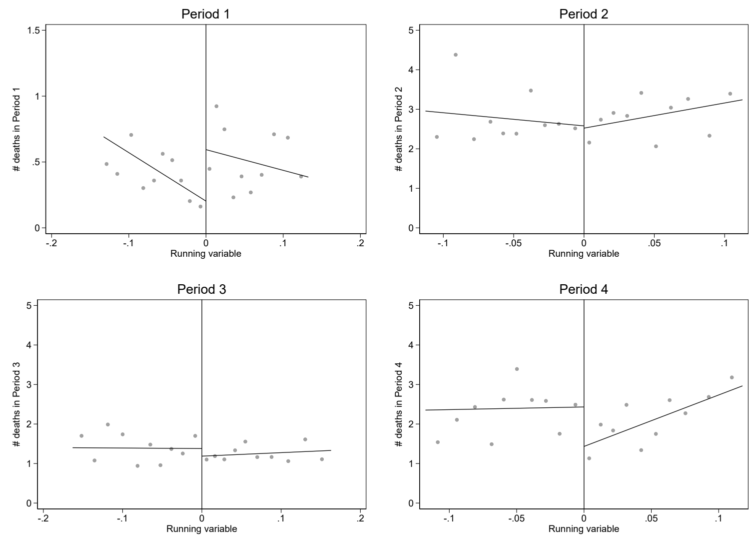
Figures

Figure 1: General balance test



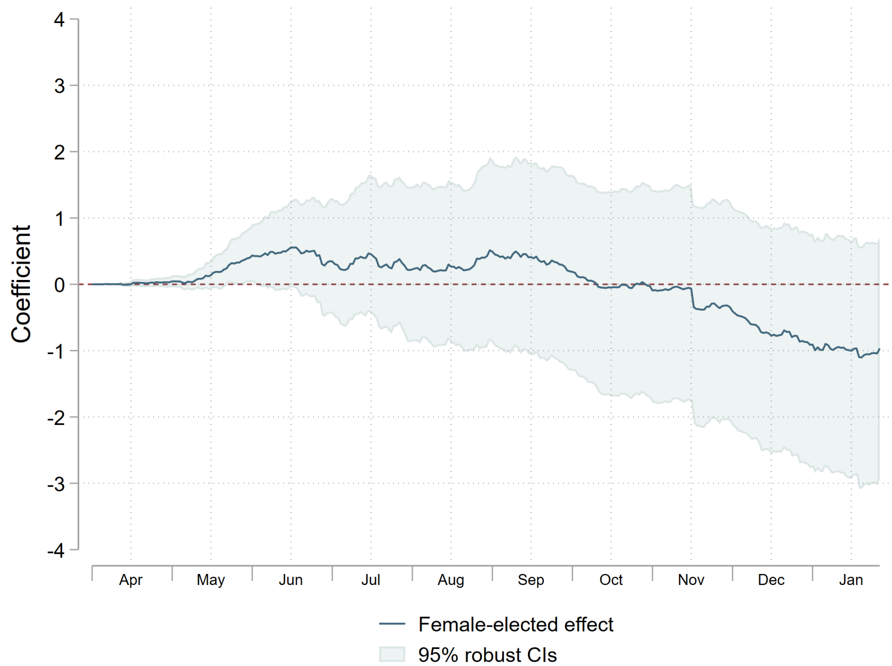
Notes: This figure is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the treatment variable (indicator equal to 1 if the female candidate won in 2016) predicted by a set of 19 municipal characteristics. Averages are calculated within quantile spaced bins of the running variable. The running variable is the margin of victory of the female candidate in the 2016 election (percentage-point difference between the vote shares of the female and male candidates). Positive (negative) values denote that the female (male) candidate won.

Figure 2: Impact on COVID-19 deaths, by period



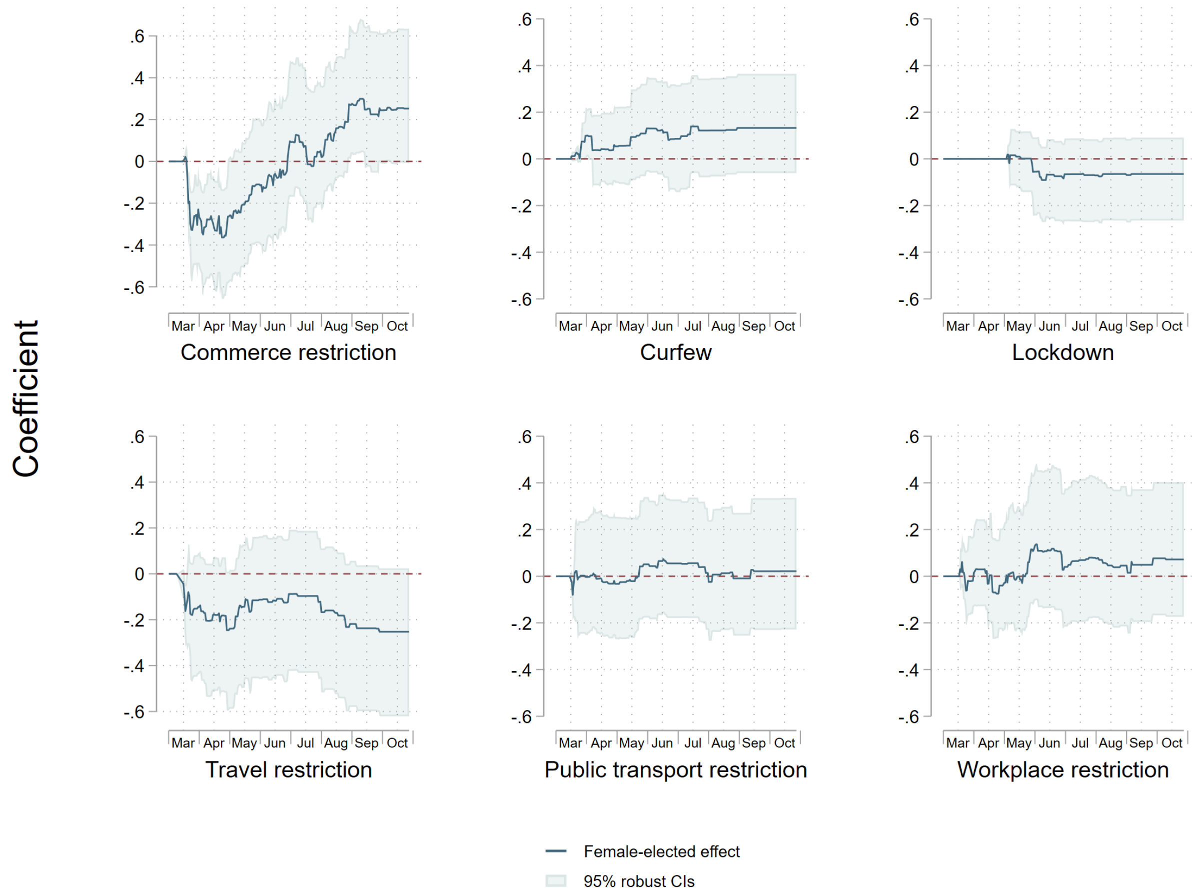
Notes: Each graph is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the number COVID-19 deaths per 10,000 inhabitants in the municipality during the period of interest. Averages are calculated within quantile spaced bins of the running variable. The running variable is the margin of victory of the female candidate in the 2016 election (percentage-point difference between the vote shares of the female and male candidates). Positive (negative) values denote that the female (male) candidate won. The scale of the graph for Period 1 is adapted to reflect the much smaller average number of deaths during this period (see Table 3).

Figure 3: Impact on cumulative number of COVID-19 deaths: Daily estimates



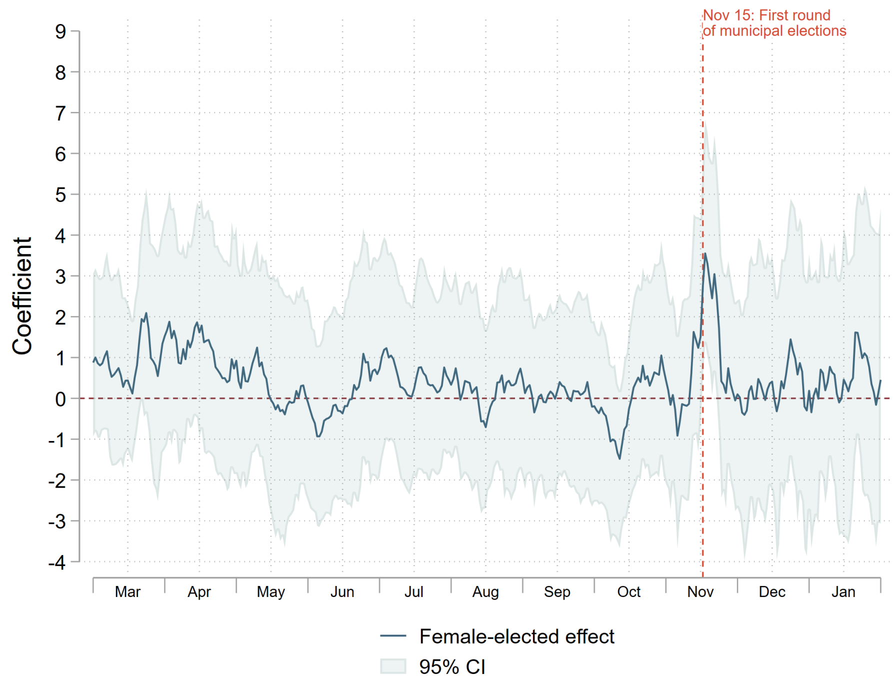
Notes: This figure plots the RD estimates obtained by taking as outcome the cumulative number of COVID-19 deaths per 10,000 inhabitants for each day from April 1, 2020 to January 31, 2021.

Figure 4: Impact of having a female mayor on policies: Daily estimates



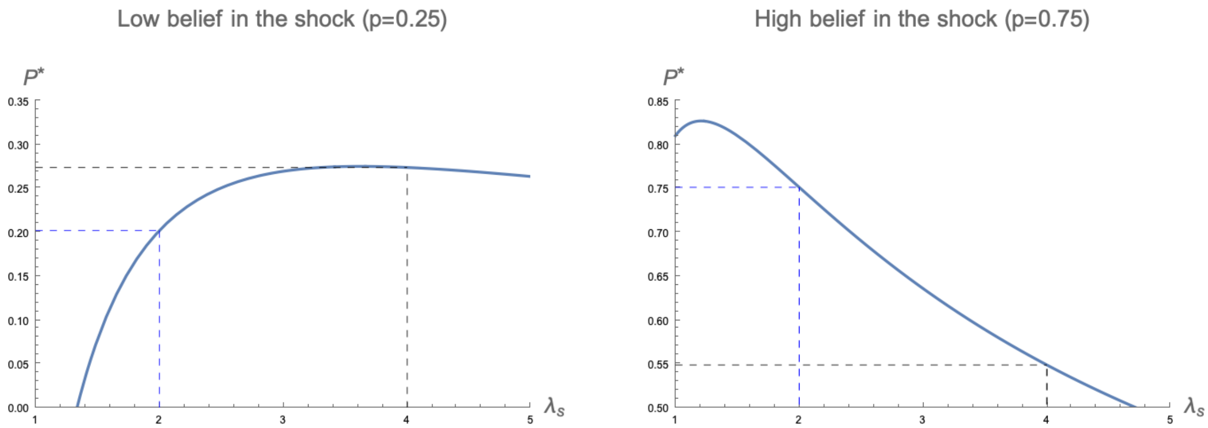
Notes: The sample is restricted to municipalities with above 10,000 inhabitants. This figure plots the estimated daily coefficients of the effect of having a female mayor on an indicator equal to 1 if the policy was implemented on that day.

Figure 5: Impact of having a female mayor on the isolation index: Daily estimates (7-day moving average)



Notes: This figure plots the estimated daily coefficients of the effect of having a female mayor on the seven-day moving average of the isolation index, which measures the share of phone users staying at home on a given day. The moving averages are centred on the current day, so that for each day, it measures the impact on the average share of phone users staying at home over the previous three days, the current day, and the next three days. We restrict the sample to a balanced panel of municipalities, excluding those with missing values between February 25, 2020, and January 31, 2021.

Figure 6: Optimal policy (P^*) as a function of voters' gender-biased beliefs about policy effectiveness (λ_s) at different levels of belief in the likelihood of the shock (p)



Notes: The figure plots the optimal level of policy chosen by the politician as a function of voters' gender-biased beliefs about the effectiveness of containment policies under two scenarios – one in which voters believe that the shock has a low probability of occurring ($p = 0.25$) and one in which voters believe it has a high probability of occurring ($p = 0.75$) – normalizing the pre-crisis amount of the public good to $\tilde{g} = 1$ and assuming a shock of magnitude $\psi = 3$.

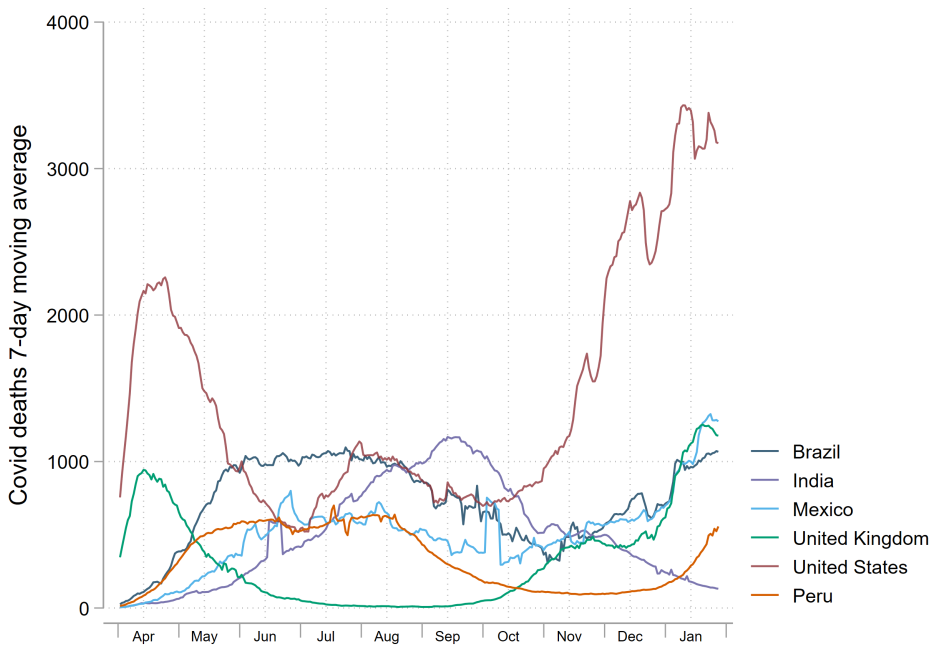
Appendix (For Online Publication)

Table of Contents

A Additional figures and tables

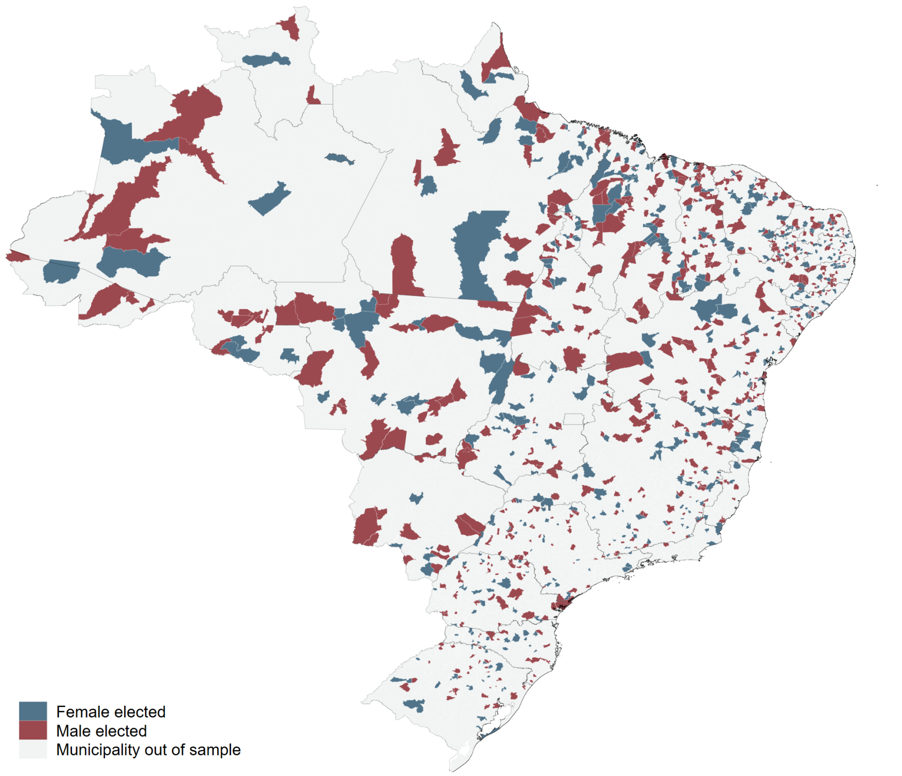
A1 Additional figures

Figure A1: Daily number of COVID-19 deaths in Brazil and in the other five countries with the highest mortality (7-day moving average)



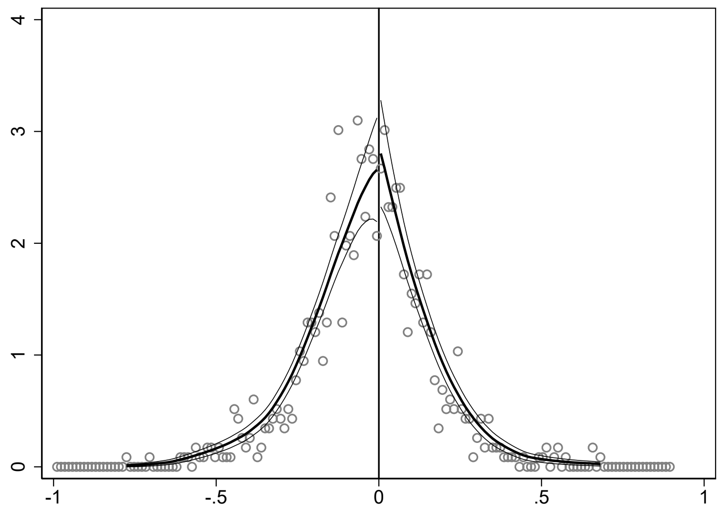
Notes: This figure includes the six countries with the highest number of COVID-19 deaths in the world as of January 31, 2021. It shows the number of COVID-19 deaths, smoothed using a 7-day moving average centered in the current day. Data from Our World in Data, accessed on June 23, 2021.

Figure A2: Municipalities in the analysis sample by gender of the election winner



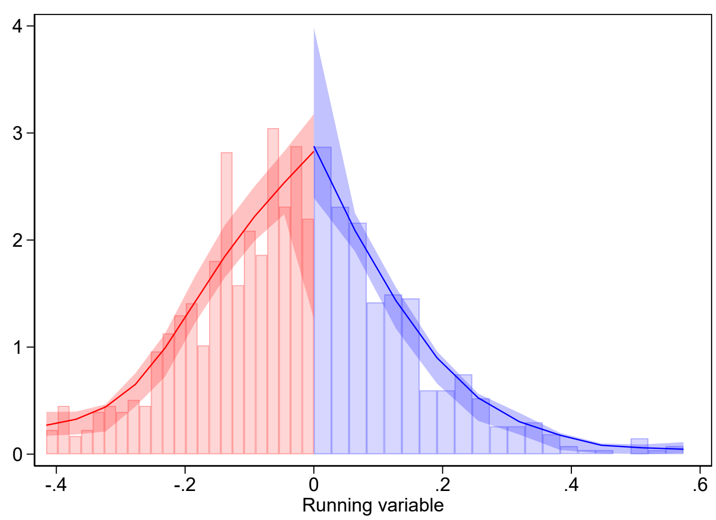
Notes: This figure plots the geographical distribution of municipalities in our sample of analysis. Municipalities in blue (red) are where a female (male) candidate was elected in 2016.

Figure A3: McCrary (2008)'s density test



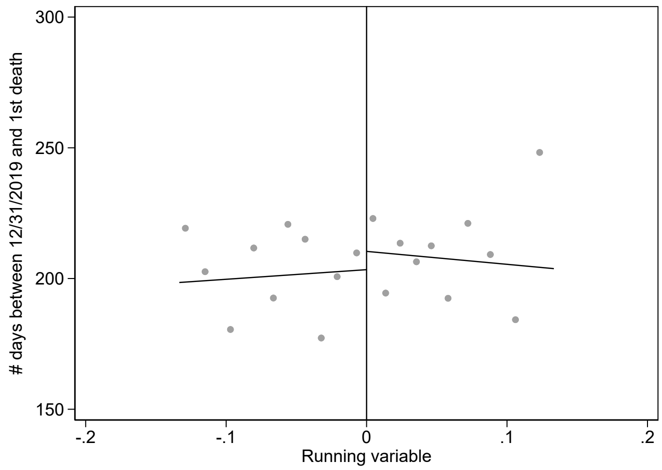
Notes: This Figure tests for a jump in the density of the running variable (the victory margin of the female candidate) at the threshold using the method developed by McCrary (2008). The solid line represents the density of the running variable. Thin lines represent the confidence intervals.

Figure A4: Cattaneo et al. (2018)'s density test



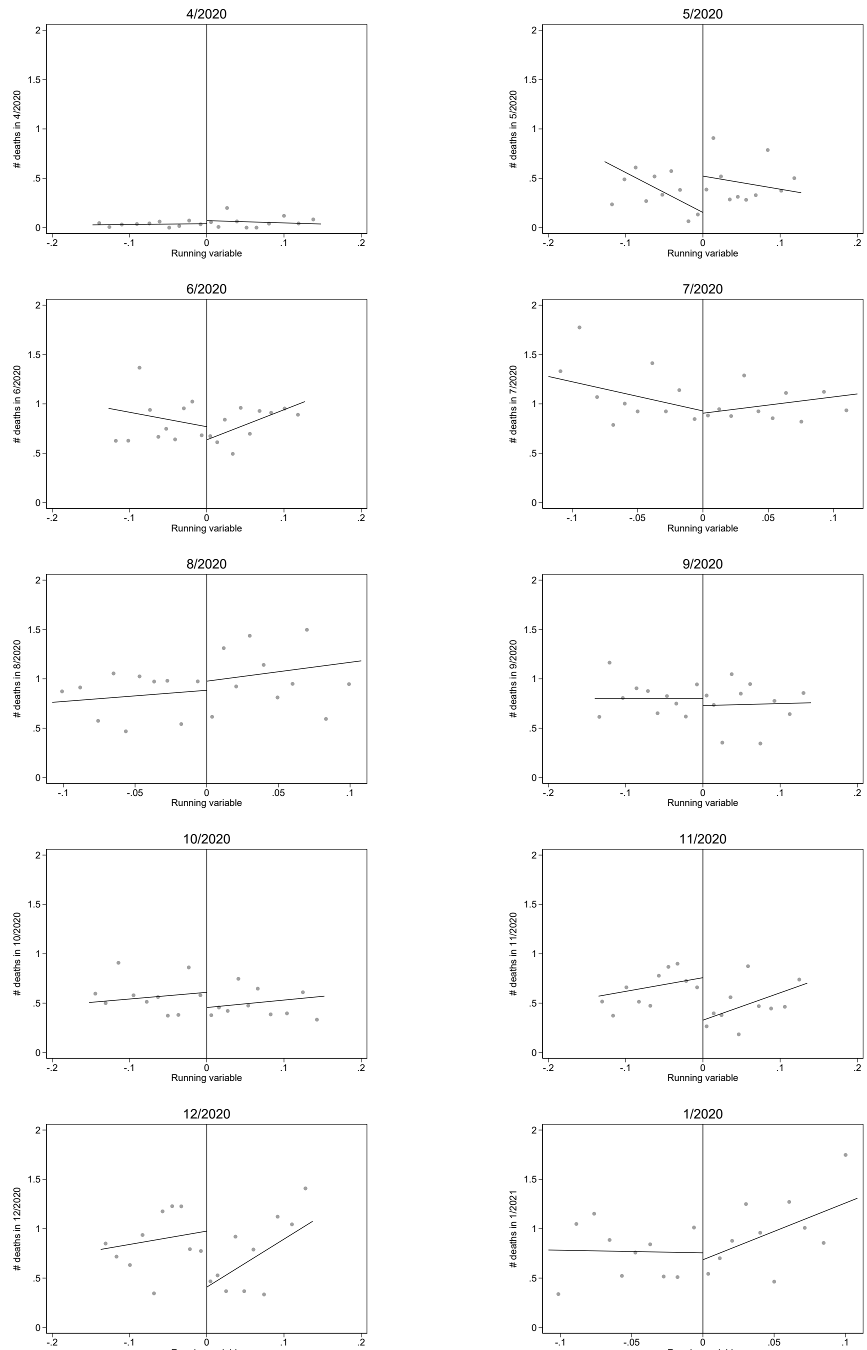
Notes: This Figure tests for a jump in the density of the running variable (the victory margin of the female candidate) at the threshold using the method developed by Cattaneo et al. (2018). The solid line represents the density of the running variable. Thin lines represent the confidence intervals. The p-value associated with the density test is 0.19.

Figure A5: Impact of having a female mayor on the timing of the first COVID-19 death



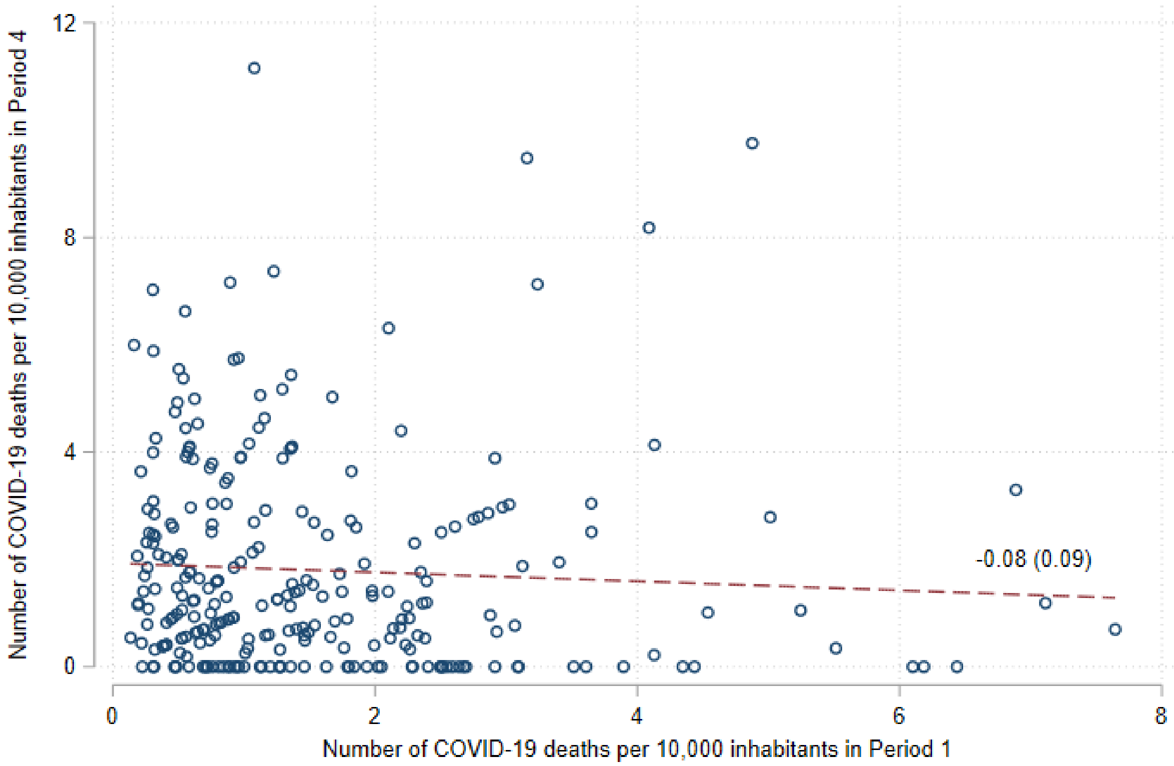
Notes: This figure is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the number of days between December 31, 2019, and the first reported COVID-19 death. Averages are calculated within quantile spaced bins of the running variable. The running variable is the margin of victory of the female candidate in the 2016 election (percentage-point difference between the vote share of the female and male candidates). Positive (negative) values denote that the female (male) candidate won.

Figure A6: Impact of having a female mayor on COVID-19 deaths, by month



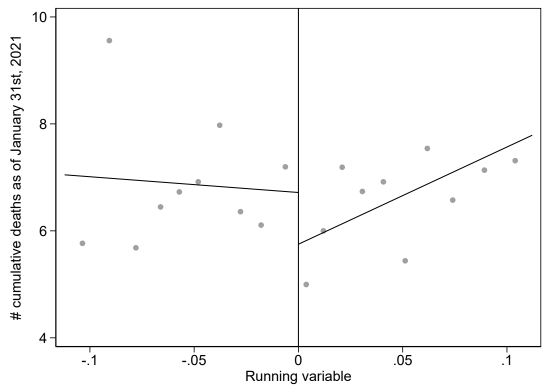
Notes: Each graph is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the total number COVID-19 deaths per 10,000 inhabitants in the municipality during the month of interest. Averages are calculated within quantile spaced bins of the running variable. The running variable is the margin of victory of the female candidate in the 2016 election (percentage-point difference between the vote share of the female and the male candidates). Positive (negative) values denote that the female (male) candidate won.

Figure A7: Correlation between COVID-19 deaths in Period 1 and Period 4



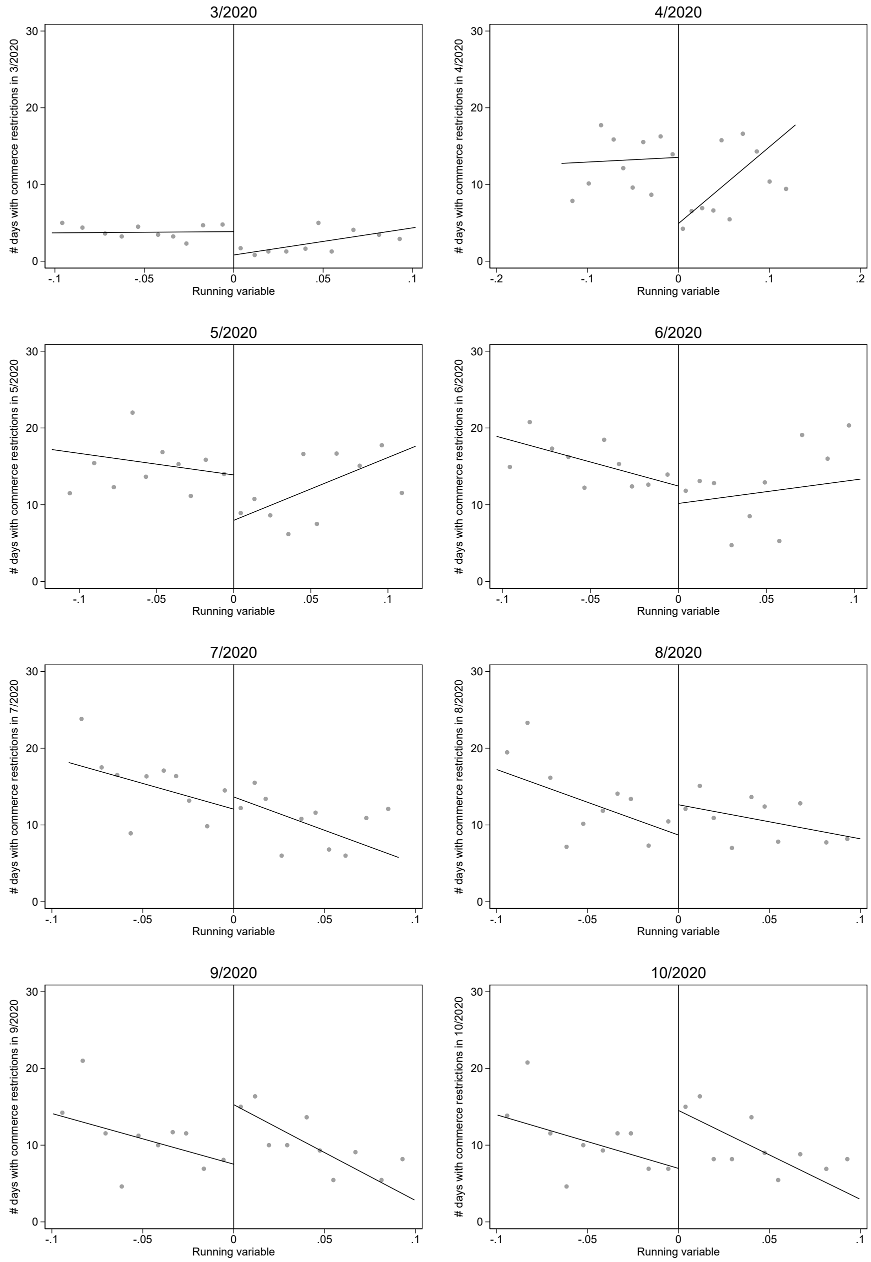
Notes: This scatterplot reports the total number of COVID-19 deaths per 10,000 inhabitants in the first period (April-May 2020) of analysis (x-axis) and in the fourth period (November 2020-January 2021) of analysis (y-axis), restricting the sample to municipalities that had at least one death in the first period.

Figure A8: Impact on the cumulative number of COVID-19 deaths as of January 31, 2021



Notes: This figure is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the cumulative number COVID-19 deaths per 10,000 inhabitants in the municipality as of January 31, 2021. Averages are calculated within quantile spaced bins of the running variable. The running variable is the margin of victory of the female candidate in the 2016 election (percentage-point difference between the vote share of the female and the male candidates). Positive (negative) values denote that the female (male) candidate won.

Figure A9: Impact of having a female mayor on commerce restrictions, by month



Notes: The sample is restricted to municipalities above 10,000 inhabitants. Each graph is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the number of days the policy was implemented in the municipality during the month of interest. Averages are calculated within quantile spaced bins of the running variable. The running variable is the margin of victory of the female candidate in the 2016 election (percentage-point difference between the vote share of the female and the male candidates). Positive (negative) values denote that the female (male) candidate won.

A2 Additional tables

Table A1: Descriptive statistics: Comparison with the average Brazilian municipality

	All (N=5,556)		Sample (N=981)	
	Mean	Sd	**Mean**	Sd
Panel A	*Socio-demographic characteristics*			
Population	**33,706**	199,763	**13,928**	12,724
Density	**501.2**	1667.8	**119.5**	186.3
Average persons per room	**0.664**	0.213	**0.704**	0.243
Commuting time	**22.23**	5.98	**21.57**	4.57
Share of population $\geq$ 65 years old	**0.084**	0.025	**0.083**	0.023
Nursing home residents per 10k pop	**5.876**	12.832	**3.742**	11.488
Area	**1,525**	5,645	**1,765**	5477
Distance to São Paulo	**1,168**	754	**1,448**	739
Km to airport connecting to COVID hot spots	**272.7**	205.6	**301.3**	214.6
Median household income p/c	**388.3**	165.6	**319.3**	143.9
Informality rate	**0.158**	0.055	**0.169**	0.055
Unemployment rate	**0.043**	0.022	**0.044**	0.021
College graduate employment share	**0.076**	0.036	**0.067**	0.030
Black and mixed-race population share	**0.524**	0.238	**0.600**	0.215
Agriculture employment share	**0.364**	0.184	**0.422**	0.149
Evangelical share of population	**0.171**	0.095	**0.156**	0.091
Panel B	*Political characteristics*			
Turnout	**0.855**	0.060	**0.855**	0.059
Number of candidates	**2.748**	1.170	**2.642**	0.920
President's vote share	**0.387**	0.190	**0.318**	0.186

Notes: The sample includes either all Brazilian municipalities (first two columns) or only municipalities in our sample of analysis (last two columns). In Columns 1 and 2, we exclude 12 municipalities that experienced a redistricting between 2010 (census year) and 2020 and two municipalities that do not hold municipal elections (Brasília and Fernando de Noronha). Socio-demographic variables come from the 2010 census, except for density, which is defined as the population living within 10 km of the average inhabitant of the municipality and is computed using the 2015 data from the Global Human Settlement Layer. The political variables are computed using the results of the first round of the 2016 municipal election, except for the last, which uses data from the first round of the 2018 presidential election. The area, distance to São Paulo, and number of kilometers to the closest airport are missing for 5 municipalities in the full sample. All variables are defined in Appendix Table B1.

Table A2: Descriptive statistics: Municipalities close to the threshold

	Full sample (N=981)		Close (N=202)	
	Mean	Sd	**Mean**	Sd
Panel A	*Socio-demographic characteristics*			
Population	**13,928**	12,724	**13,880**	11,254
Density	**119.5**	186.3	**109.7**	117.9
Average persons per room	**0.704**	0.243	**0.708**	0.209
Commuting time	**21.57**	4.57	**21.59**	4.70
Share of population $\geq$ 65 years old	**0.083**	0.023	**0.081**	0.023
Nursing home residents per 10k pop	**3.742**	11.488	**3.215**	7.650
Area	**1,765**	5,477	**1,682**	4,634
Distance to São Paulo	**1,448**	739	**1,492**	730
Km to airport connecting to COVID hot spots	**301.3**	214.6	**294.3**	202.7
Median household income p/c	**319.3**	143.9	**314.4**	148.4
Informality rate	**0.169**	0.055	**0.167**	0.057
Unemployment rate	**0.044**	0.021	**0.044**	0.023
College graduate employment share	**0.067**	0.030	**0.066**	0.031
Black and mixed-race population share	**0.600**	0.215	**0.598**	0.225
Agriculture employment share	**0.422**	0.149	**0.439**	0.156
Evangelical share of population	**0.156**	0.091	**0.149**	0.090
Panel B	*Political characteristics*			
Turnout	**0.855**	0.059	**0.858**	0.057
Number of candidates	**2.642**	0.920	**2.733**	1.092
President's vote share	**0.318**	0.186	**0.307**	0.193

Notes: The sample includes either all municipalities in our analysis sample (first two columns) or only municipalities close to the discontinuity, defined as those where the victory margin is lower than 4 percentage points (last two columns). Socio-demographic variables come from the 2010 census, except for density, which is defined as the population living within 1 km of the average inhabitant of the municipality and is computed using the 2015 data from the Global Human Settlement Layer. The political variables are computed using the results of the first round of the 2016 municipal election, except for the last, which uses data from the first round of the 2018 presidential election. All variables are defined in Appendix Table B1.

Table A3: General balance test

	(1)
Outcome	Predicted treatment
Treatment	0.017
	(0.014)
Robust p-value	0.330
Observations	518
Polyn. order	1
Bandwidth	0.121
Mean, left of threshold	0.419

Notes: The outcome is the treatment variable predicted by a set of 19 municipal characteristics, as described in Section 3.3. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A4: Descriptive statistics: 2016 candidates

Panel A	*All candidates (N=16,065)*							
	Female candidates (N=2,105)				Male candidates (N=13,960)			
	mean	sd	min	max	**mean**	sd	min	max
Incumbency	**0.167**	0.373	0	1	**0.178**	0.383	0	1
Age	**47.7**	10.3	20	90	**49.0**	10.8	20	89
White	**0.675**	0.469	0	1	**0.667**	0.471	0	1
Higher education	**0.726**	0.446	0	1	**0.494**	0.500	0	1
Occ.: Politics	**0.183**	0.387	0	1	**0.187**	0.390	0	1
Occ.: Public	**0.135**	0.341	0	1	**0.089**	0.284	0	1
Occ.: Health	**0.097**	0.297	0	1	**0.068**	0.252	0	1
Occ.: Business owner	**0.082**	0.275	0	1	**0.146**	0.353	0	1
Ideological score	**0.187**	0.436	-0.843	0.760	**0.192**	0.427	-0.843	0.760
PMDB	**0.147**	0.354	0	1	**0.141**	0.348	0	1
PSDB	**0.100**	0.300	0	1	**0.107**	0.309	0	1
PT	**0.066**	0.248	0	1	**0.060**	0.238	0	1
Wins	**0.313**	0.464	0	1	**0.360**	0.480	0	1
Panel B	*Winners (N=5,568)*							
	Female candidates (N=626)				Male candidates (N=4,942)			
	mean	sd	min	max	**mean**	sd	min	max
Incumbency	**0.225**	0.418	0	1	**0.239**	0.427	0	1
Age	**47.3**	10.2	21	82	**48.9**	10.8	21	88
White	**0.709**	0.454	0	1	**0.702**	0.457	0	1
Higher education	**0.717**	0.451	0	1	**0.500**	0.500	0	1
Occ.: Politics	**0.195**	0.396	0	1	**0.206**	0.405	0	1
Occ.: Public	**0.150**	0.358	0	1	**0.083**	0.276	0	1
Occ.: Health	**0.105**	0.307	0	1	**0.077**	0.266	0	1
Occ.: Business owner	**0.101**	0.301	0	1	**0.157**	0.364	0	1
Ideological score	**0.278**	0.365	-0.686	0.760	**0.273**	0.369	-0.843	0.760
PMDB	**0.195**	0.396	0	1	**0.183**	0.386	0	1
PSDB	**0.126**	0.332	0	1	**0.146**	0.353	0	1
PT	**0.048**	0.214	0	1	**0.045**	0.206	0	1

Notes: The sample includes all Brazilian municipalities (except Brasília and Fernando de Noronha, which do not hold municipal elections). The level of observation is the candidate, considering only "effective" candidates (candidates who did not withdraw their candidacy and who were not disqualified for irregularities before the election). In Panel A, we consider all candidates running in the first round (considering candidates running in both supplementary and ordinary elections), whereas in Panel B, we consider only the ultimate winner (the winner of the supplementary election if one took place). The age and education level of the candidate is missing for 5 candidates. All variables are defined in Appendix Table B1.

Table A5: Impact of having a female mayor on the timing of the first COVID-19 death

	(1)
Outcome	Date of the first death
Treatment	6.992
	(13.920)
Robust p-value	0.615
Observations	577
Polyn. order	1
Bandwidth	0.133
Mean, left of threshold	203.394

Notes: The outcome is the the number of days between December 31, 2019, and the first reported COVID-19 death. It is missing for 20 municipalities in which no death occurred up to May 9, 2021 (day on which the data were generated). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A6: Impact of having a female mayor on monthly COVID-19 deaths

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome	Number of COVID-19 deaths per 10,000 inhabitants									
	04/20	05/20	06/20	07/20	08/20	09/20	10/20	11/20	12/20	01/21
Treatment	0.031	0.368**	-0.133	-0.023	0.092	-0.072	-0.154	-0.431**	-0.568**	-0.070
	(0.037)	(0.174)	(0.256)	(0.232)	(0.288)	(0.192)	(0.191)	(0.187)	(0.219)	(0.266)
R. p-value	0.520	0.040	0.665	0.970	0.973	0.754	0.391	0.027	0.018	0.644
Obs.	632	547	546	514	484	603	651	585	591	488
Polyn.	1	1	1	1	1	1	1	1	1	1
Bandwidth	0.148	0.127	0.127	0.118	0.108	0.140	0.152	0.135	0.137	0.108
Mean	0.040	0.154	0.769	0.929	0.884	0.801	0.610	0.758	0.976	0.756

Notes: Each column takes as outcome the number of deaths per 10,000 inhabitants (using the 2010 population) during the month of interest. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A7: Impact on the cumulative number of COVID-19 deaths as of January 31, 2021

Outcome	(1) Cumulative number of COVID-19 deaths as of 01/31/2021
Treatment	-0.967
	(0.789)
Robust p-value	0.229
Observations	497
Polyn. order	1
Bandwidth	0.112
Mean, left of threshold	6.717

Notes: The outcome is the cumulative number of deaths per 10,000 inhabitants (using the 2010 population) as of January 31, 2021. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A8: Impact of having a female mayor on the timing of policies adoption

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Date on which the policy was first implemented						
	Commerce	Workplace	Travel	Transport	Curfew	Lockdown	Any
Treatment	42.876***	13.220	3.383	24.389	-28.944	0.018	17.491*
	(11.972)	(21.480)	(12.723)	(25.148)	(33.388)	(19.235)	(9.375)
Robust p-value	0.000	0.668	0.787	0.321	0.500	0.874	0.067
Observations	184	92	158	88	28	23	245
Polyn. order	1	1	1	1	1	1	1
Bandwidth	0.125	0.134	0.194	0.146	0.117	0.147	0.131
Mean, left of threshold	92.927	121.625	107.130	95.253	142.707	141.211	97.474

Notes: The sample is restricted to municipalities above 10,000 inhabitants. The sample varies by policies as, for each policy, it is restricted to municipalities that implemented the policy at some point during the period of analysis. The outcome is the number of days between December 31, 2019, and the first day on which the municipality implemented the given policy (Columns 1 through 6) or any of the six policies considered (Column 7). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A9: Impact of having a female mayor on commerce reopening

	(1)
Outcome	Probability that commerce restrictions are lifted between August and October 2020
Treatment	-0.157*
	(0.096)
Robust p-value	0.097
Observations	233
Polyn. order	1
Bandwidth	0.096
Mean, left of threshold	0.159

Notes: The sample is restricted to municipalities above 10,000 inhabitants. The outcome is an indicator equal to 1 if the mayor reopened non-essential businesses at some point between August and October 2020. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A10: Impact of having a female mayor on workplace restrictions, by month

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Number of days with workplace restrictions in place							
	03/20	04/20	05/20	06/20	07/20	08/20	09/20	10/20
Treatment	-0.073	-0.298	0.604	3.281	1.971	1.604	1.378	2.222
	(0.795)	(2.476)	(3.342)	(3.972)	(3.965)	(3.902)	(3.741)	(3.787)
Robust p-value	0.873	0.857	0.648	0.289	0.483	0.533	0.561	0.428
Observations	259	254	252	238	251	255	258	259
Polyn. order	1	1	1	1	1	1	1	1
Bandwidth	0.112	0.108	0.107	0.099	0.106	0.109	0.110	0.111
Mean, left of threshold	0.948	3.606	5.918	6.072	6.963	7.361	7.619	7.833

Notes: The sample is restricted to municipalities above 10,000 inhabitants. The outcome is the number of days during which the policy was in place, separately for each month. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A11: Impact of having a female mayor on travel restrictions, by month

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Number of days with travel restrictions in place							
	03/20	04/20	05/20	06/20	07/20	08/20	09/20	10/20
Treatment	-2.175	-5.680	-4.828	-3.470	-2.862	-5.241	-6.999*	-7.571*
	(1.388)	(3.643)	(3.902)	(3.992)	(3.963)	(4.075)	(4.159)	(4.235)
Robust p-value	0.111	0.106	0.187	0.348	0.440	0.182	0.088	0.069
Observations	252	244	258	264	265	258	252	246
Polyn. order	1	1	1	1	1	1	1	1
Bandwidth	0.108	0.104	0.110	0.115	0.118	0.111	0.108	0.104
Mean, left of threshold	3.539	9.375	11.618	12.463	12.565	13.602	14.186	14.290

Notes: The sample is restricted to municipalities above 10,000 inhabitants. The outcome is the number of days during which the policy was in place, separately for each month. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A12: Impact of having a female mayor on transport restrictions, by month

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Number of days with transport restrictions in place							
	03/20	04/20	05/20	06/20	07/20	08/20	09/20	10/20
Treatment	-0.111	-0.499	-0.166	1.609	1.500	0.084	0.182	0.642
	(1.234)	(3.280)	(3.282)	(3.297)	(3.307)	(3.350)	(3.463)	(3.638)
Robust p-value	0.886	0.969	0.916	0.542	0.582	0.879	0.827	0.714
Observations	282	264	265	264	265	265	262	258
Polyn. order	1	1	1	1	1	1	1	1
Bandwidth	0.129	0.117	0.118	0.117	0.118	0.119	0.114	0.110
Mean, left of threshold	2.395	6.244	6.002	5.581	5.710	6.698	6.697	6.797

Notes: The sample is restricted to municipalities above 10,000 inhabitants. The outcome is the number of days during which the policy was in place, separately for each month. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A13: Impact of having a female mayor on curfew, by month

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Number of days with a curfew in place						
	04/20	05/20	06/20	07/20	08/20	09/20	10/20
Treatment	1.538	2.366	3.410	3.425	3.675	3.979	3.979
	(1.775)	(2.214)	(2.580)	(2.694)	(2.685)	(2.725)	(2.725)
Robust p-value	0.442	0.334	0.230	0.278	0.199	0.161	0.161
Observations	287	269	264	259	259	256	256
Polyn. order	1	1	1	1	1	1	1
Bandwidth	0.132	0.123	0.116	0.112	0.113	0.110	0.110
Mean, left of threshold	1.234	1.512	1.657	2.104	1.988	1.921	1.921

Notes: The sample is restricted to municipalities above 10,000 inhabitants. The outcome is the number of days during which the policy was in place, separately for each month. We start in April as no municipality in our sample implemented a curfew in March. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A14: Impact of having a female mayor on lockdown, by month

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of days with a lockdown in place					
	05/20	06/20	07/20	08/20	09/20	10/20
Treatment	0.112	-2.170	-2.004	-2.012	-1.970	-1.941
	(1.283)	(2.118)	(2.306)	(2.288)	(2.302)	(2.299)
Robust p-value	0.912	0.279	0.315	0.323	0.331	0.340
Observations	283	264	259	262	259	259
Polyn. order	1	1	1	1	1	1
Bandwidth	0.129	0.116	0.112	0.114	0.113	0.114
Mean, left of threshold	0.986	3.348	3.549	3.511	3.493	3.452

Notes: The sample is restricted to municipalities above 10,000 inhabitants. The outcome is the number of days during which the policy was in place, separately for each month. We start in May as no municipality in our sample implemented a lockdown in March and April. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A15: Impact on commerce restrictions, adding state fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Number of days with commerce restrictions in place							
	Full sample				Restricted + state FEs			
	03/20	04/20	09/20	10/20	03/20	04/20	09/20	10/20
Treatment	-3.044***	-8.620***	7.756**	7.552*	-3.648***	-6.873***	6.930*	6.971*
	(0.962)	(2.720)	(4.334)	(4.311)	(1.058)	(2.457)	(4.054)	(4.061)
Robust p-value	0.002	0.002	0.050	0.055	0.001	0.007	0.056	0.056
Observations	240	282	238	238	187	246	186	186
Polyn. order	1	1	1	1	1	1	1	1
Bandwidth	0.102	0.129	0.099	0.099	0.088	0.126	0.086	0.087
Mean, left of threshold	3.859	13.536	7.522	6.971	4.359	14.384	9.644	8.991

Notes: The sample is restricted to municipalities above 10,000 inhabitants. In Columns 5 to 8, the estimation includes state fixed effects and we remove municipalities in states with fewer than 20 municipalities in our overall analysis sample (corresponding to 11 percent of municipalities above 10,000 inhabitants). The outcome is the number of days during which the policy was in place, separately for each month. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A16: Impact of having a female mayor on the isolation index around election day

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Outcome	Share of phone users staying at home on each day from November 10 to 20										
	10	11	12	13	14	15	16	17	18	19	20
Treatment	1.048	0.157	0.950	3.849**	5.581**	0.733	-0.573	3.662*	4.152*	6.951***	1.083
	(2.117)	(2.142)	(1.419)	(1.867)	(2.473)	(1.955)	(1.873)	(1.753)	(2.499)	(1.705)	(2.186)
R. p-value	0.593	0.955	0.520	0.038	0.023	0.609	0.747	0.055	0.071	0.000	0.501
Obs	151	158	187	119	142	140	158	162	114	130	166
Polyn. order	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	0.118	0.125	0.154	0.091	0.108	0.106	0.125	0.128	0.085	0.098	0.133
Mean	36.642	36.917	35.504	33.147	32.739	36.406	39.325	36.654	36.084	34.289	36.828

Notes: The sample is restricted to municipalities with no missing value between February 25, 2020, and January 31, 2021. The outcome is the share of phone users staying at home on a given day. We provide the estimated impact for each day from November 10 to November 20. The day of the election was Sunday, November 15 (Column 6). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A17: Impact on COVID-19 deaths in Period 1, by term-limit status

Outcome	(1)	(2)	(3)	(4)
	Number of Covid-19 deaths in Period 1			
	Full sample	Both can run	Female can run; male cannot	Male can run; female cannot
Treatment	0.391**	0.387	0.919**	-0.154
	(0.176)	(0.271)	(0.347)	(0.290)
Robust p-value	0.035	0.133	0.014	0.592
Observations	578	282	140	116
Polyn. order	1	1	1	1
Bandwidth	0.133	0.129	0.126	0.110
Mean	0.203	0.157	0.127	0.305

Notes: In Column 2, the sample is restricted to elections in which neither of the two front-runners ran as an incumbent. In Column 3 (resp. 4), the sample is restricted to elections in which only the male (resp. female) candidate ran as an incumbent. The outcome is the number of deaths per 10,000 inhabitants (using the 2010 population) during the first period (April-May 2020). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A18: Impact on COVID-19 deaths in Period 4, by term-limit status

Outcome	(1)	(2)	(3)	(4)
	Number of Covid-19 deaths in Period 4			
	Full sample	Both can run	Female can run; male cannot	Male can run; female cannot
Treatment	-0.999**	-1.743**	-0.176	-1.228
	(0.405)	(0.671)	(0.609)	(0.750)
Robust p-value	0.016	0.011	0.690	0.108
Observations	513	257	171	142
Polyn. order	1	1	1	1
Bandwidth	0.118	0.115	0.149	0.142
Mean	2.432	3.044	1.872	2.425

Notes: In Column 2, the sample is restricted to elections in which neither of the two front-runners ran as an incumbent. In Column 3 (resp. 4), the sample is restricted to elections in which only the male (resp. female) candidate ran as an incumbent. The outcome is the number of deaths per 10,000 inhabitants (using the 2010 population) during the last period (November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A19: Impact on COVID-19 deaths, by mayor's education level

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths					
	Full sample		No higher education		Higher education	
Periods	1	4	1	4	1	4
Treatment	0.548**	-1.250**	0.839**	-1.545	0.371*	-1.009**
	(0.224)	(0.523)	(0.405)	(0.876)	(0.213)	(0.506)
Robust p-value	0.014	0.019	0.045	0.108	0.068	0.037
Observations	387	375	194	182	204	176
Polyn. order	1	1	1	1	1	1
Bandwidth	0.120	0.111	0.128	0.118	0.117	0.095
Mean	0.197	2.722	0.266	3.182	0.125	2.310

The sample is restricted to municipalities in which the mayor is elected for the first time in 2016 and can therefore run for reelection. In Columns 3 and 4 (resp. 5 and 6), the sample is restricted to municipalities where the mayor has not completed higher education (resp. has completed higher education). In Columns 1, 3, and 5 (resp. 2, 4, and 6), the outcomes is the number of deaths per 10,000 inhabitants during Period 1 (resp. 4) – April-May 2020 (resp. November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A20: Impact on COVID-19 deaths, by mayor's age

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths					
	Full sample		Below age median		Above age median	
Periods	1	4	1	4	1	4
Treatment	0.548**	-1.250**	0.495**	-1.647**	0.534*	-0.810
	(0.224)	(0.523)	(0.230)	(0.720)	(0.310)	(0.760)
Robust p-value	0.014	0.019	0.022	0.025	0.083	0.334
Observations	387	375	183	191	230	187
Polyn. order	1	1	1	1	1	1
Bandwidth	0.120	0.111	0.107	0.116	0.148	0.110
Mean	0.197	2.722	0.137	2.782	0.320	2.602

The sample is restricted to municipalities in which the mayor is elected for the first time in 2016 and thus can run for reelection. In Columns 3 and 4 (resp. 5 and 6), the sample is restricted to municipalities where the mayor is below (resp. above) the median age. In Columns 1, 3, and 5 (resp. 2, 4, and 6), the outcomes is the number of deaths per 10,000 inhabitants during Period 1 (resp. 4) – April-May 2020 (resp. November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A21: Impact on COVID-19 deaths, by mayor's previous legislative office

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths					
	Full sample		Has not served		Has served	
Periods	1	4	1	4	1	4
Treatment	0.548**	-1.250**	0.574**	-0.795	0.470	-4.328**
	(0.224)	(0.523)	(0.250)	(0.510)	(0.369)	(1.827)
Robust p-value	0.014	0.019	0.019	0.108	0.200	0.034
Observations	387	375	341	330	45	46
Polyn. order	1	1	1	1	1	1
Bandwidth	0.120	0.111	0.117	0.112	0.114	0.123
Mean	0.197	2.722	0.225	2.190	-0.037	6.431

The sample is restricted to municipalities in which the mayor is elected for the first time in 2016 and thus can run for reelection. In Columns 3 and 4 (resp. 5 and 6), the sample is restricted to municipalities where the mayor in 2016 has not served as a legislator during the 2012-2016 term (resp. as served as a legislator). In Columns 1, 3, and 5 (resp. 2, 4, and 6), the outcomes is the number of deaths per 10,000 inhabitants during Period 1 (resp. 4) – April-May 2020 (resp. November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold. Note that the means at the threshold are estimated quite imprecisely in Columns 5 and 6 due to the small sample size.

Table A22: Impact on COVID-19 deaths, by municipality's gender gap in labor force participation

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths					
	Full sample		Above median		Below median	
Periods	1	4	1	4	1	4
Treatment	0.548**	-1.250**	0.525*	-2.032***	0.346	-0.525
	(0.224)	(0.523)	(0.309)	(0.768)	(0.261)	(0.687)
Robust p-value	0.014	0.019	0.063	0.008	0.214	0.562
Observations	387	375	246	191	214	214
Polyn. order	1	1	1	1	1	1
Bandwidth	0.120	0.111	0.158	0.111	0.138	0.137
Mean	0.197	2.722	0.322	3.191	0.188	2.264

Notes: The sample is restricted to municipalities in which the mayor is elected for the first time in 2016 and thus can run for reelection. In Columns 3 and 4 (resp. 5 and 6), the sample is further restricted to municipalities where the gap in the labor force participation of female and male residents is above the median (resp. below the median). In Columns 1, 3, and 5 (resp. 2, 4, and 6), the outcomes is the number of deaths per 10,000 inhabitants during Period 1 (resp. 4) – April-May 2020 (resp. November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table A23: Impact of the mayor's level of education on COVID-19 deaths

	(1)	(2)	(3)	(4)
Outcome	# COVID-19 deaths per 10,000 inhabitants			
	Period 1	Period 2	Period 3	Period 4
Treatment	-0.131	0.263	-0.057	-0.038
	(0.129)	(0.345)	(0.240)	(0.410)
Robust p-value	0.339	0.413	0.942	0.968
Observations	873	730	793	823
Polyn. order	1	1	1	1
Bandwidth	0.149	0.115	0.129	0.137
Mean, left of threshold	0.358	1.960	1.316	2.923

Notes: The sample is restricted to municipalities where the two front-runners in 2016 were male candidates and where one had completed higher education while the other had not. The same sample restrictions as for the main analysis also apply (see Section 3) and we end up with a sample of 1,408 municipalities. The independent variable is an indicator equal to 1 if the higher-educated candidate won the election. Each column takes as outcome the number of deaths per 10,000 inhabitants (using the 2010 population) during the period of interest. Period 1 (resp. 2, 3, and 4) is April-May 2020 (resp. June-August 2020, September-October 2020, and November 2020-January 2021). We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We control for municipality and winner characteristics (listed in Tables 1 and 2, respectively). We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for municipalities at the threshold where the mayor did not complete higher education.

Table A24: Impact on the 2020 election

Outcome	(1) Run	(3) Win	(7) Conditional on running Vote share	(8) Win
Treatment	0.024	0.035	0.012	0.042
	(0.084)	(0.075)	(0.040)	(0.115)
Robust p-value	0.799	0.550	0.619	0.563
Observations	655	664	284	386
Polyn. order	1	1	1	1
Bandwidth	0.154	0.157	0.113	0.161
Mean, left of threshold	0.569	0.252	0.445	0.448

Notes: In Column 1, the outcome is an indicator variable equal to 1 if the 2016 mayor runs in the 2020 election. In Columns 2 and 4, the outcome is an indicator variable equal to 1 if the 2016 mayor is reelected in 2020. In Column 3, the outcome is the vote share obtained by the 2016 mayor in the first round of the 2020 election. In Columns 3 and 4, the sample is restricted to mayors who ran again in 2020. Note that this restriction is unlikely to create selection issues due to the null impact on running in Column 1. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

B Data appendix

B1 Definitions and sources of variables

Table B1: Definitions and sources of variables used in the analysis

Variable	Dataset	Date	Description / comments
Panel A: Municipality-level socio-demographic characteristics			
Population	Census	2010	Population of the municipality.
Density	GHSL	2015	Population living within 10 km of the average inhabitant of the municipality. For each municipality, we count the population living in a 10km radius (encompassing areas inside and outside the municipality's perimeter) around each 1-square-km pixel composing the area of the municipality. We then average this count using each pixel's population as weights.
Average persons per room	Census	2010	Number of individuals living in the household, divided by number of rooms.
Commuting time	Census	2010	Average time that the municipality's employed population usually spend in travel from home to work, in minutes.
Share of population $\geq$65 years old	Census	2010	Share of the municipality´s population aged 65 or above.
Nursing home residents per 10k pop	Census	2010	Number of individuals aged 65 or above living in nursings homes or asylums, per 10,000 individuals (considering residents aged 18 or above) living in the municipality.
Area	IBGE	2010	Area of the municipality in square kilometers.
Distance to São Paulo	IBGE	2010	Geographical distance (straight line along earth's surface), in kilometers, between each municipality and the city of São Paulo.
Km to airport connecting to COVID hot spots	ANAC	2010	Geographical distance, in kilometers (straight line along earth's surface), to nearest airport having at least one flight connecting Brazil with the US, UK, France, Spain, Italy, Germany, or China.
Median household income p/c	Census	2010	Municipality's median household income per capita. Total household income includes all sources of income, both labor and non-labor income, and is divided by the number of household members.
Informality rate	Census	2010	Share of the municipality's working age population (18 y.o. or above) that work as employees without a signed work card. Self-employed individuals are not considered informal.
Unemployment rate	Census	2010	Share of the municipality's working age population (18 y.o. or above) that did not work for at least one hour in the week of reference, but actively looked for a job in that month.
Gender wage gap	Census	2010	Gender difference in the municipality's mean residual labor income. Residual income is computed from a linear regression of the individual's total labor income on age, education, and occupation.
Labor force participation gap	Census	2010	Gender difference in the municipality's labor force participation rate. The participation rate is the share of the municipality's working age population (18 y.o. or above) that is employed or unemployed.
College graduate employment share	Census	2010	The share of the municipality's population that had completed college or higher educational level among those employed who reported their educational status in the census.
Black and mixed-race population share	Census	2010	Share of the municipality's population that is black or mixed-race.
Agriculture employment share	Census	2010	Share of employed individuals working in agriculture, based on CNAE - Domiciliar sector definition.
Evangelical share of population	Census	2010	Share of the municipality's population belonging to an evangelical religion.

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Variable	Dataset	Date	Description / comments
			Panel B: Municipality-level electoral variables
Turnout	TSE	2016	Share of registered voters who cast a vote in the first round of the 2016 election.
Number of candidates	TSE	2016	Number of candidates running for mayor in the first round of the 2016 election.
Elected president's vote share	TSE	2018	Share of votes in the first round of the 2018 presidential elections that went to the elected president.
			Panel C: Candidate-level electoral variables
Election winner	TSE	2016	Dummy variable that equals 1 if the candidate has the largest share of valid votes as registered by the electoral justice in the first round, in case there was no second round, or in the second round, if there was one.
Gender of the candidates	TSE	2016	Dummy variable that equals 1 if the candidate is a female, as registered by the electoral justice (not self-declared), and 0 if male. This variable was verified using an algorithm that computes the probability of being a female according to the candidate's first name.
Incumbency status of the candidates	TSE	2016	Dummy variable that equals 1 if the candidate ran as the incumbent – i.e., ran for reelection – and 0 otherwise. This variable was constructed by using the self-declaration of candidates and verified by matching the name of the candidate with the name of the winner of the 2012 election.
Age	TSE	2016	Age of the candidate at the time of the election, computed using the election's date and the candidate's date of birth as registered by the electoral justice. In the case of supplementary elections, we follow the same logic and compute the candidate's age as of the supplementary election date.
Education	TSE	2016	Dummy variable that equals 1 if the candidate has completed tertiary-level education.
Race	TSE	2016	Dummy variable that equals 1 if the candidate is white.
Occupation	TSE	2016	Professional occupation of the candidate. There are 167 occupations declared by the candidates in the 2016 election data. We manually classified these occupations into four relevant areas: politics, public servants, health-related, and business owners.
Political party	TSE	2016	Political party under which the mayoral candidate ran in the 2016 election.
Ideological score	BLS	2019	To each candidate, we assign their party's ideology score from the 2018 wave of the Brazilian Legislative Survey (BLS) (Zucco and Power, 2019). We use data and replications files from Power and Rodrigues-Silveira (2019), who further impute the score for smaller parties. The score is centered around zero and goes from -1 (extreme left) to +1 (extreme right) and is adjusted to take into account party movements across years.

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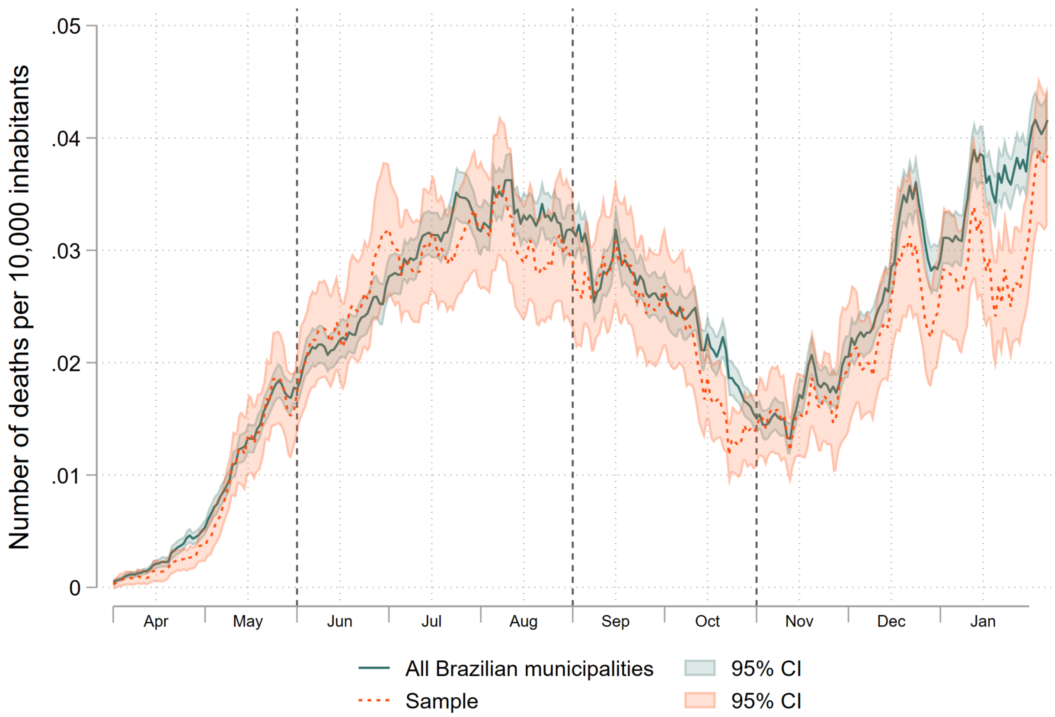
Variable	Dataset	Date	Description / comments
			Panel D: Main outcomes
Deaths per 10k	Brazil IO	2020-21	Number of COVID-19 deaths registered in the municipality for each day, normalized using the 2010 population so that it gives the number of daily deaths per 10,000 inhabitants. We then either use the data day by day or aggregate it by months and periods. Brazil IO collected the data directly from state's health secretaries.
Deaths per 10k	SIVEP-Gripe	2020-21	Number of COVID-19 deaths registered in the municipality for each day, normalized using the 2010 population so that it gives the number of daily deaths per 10,000 inhabitants. We then either use the data day by day or aggregate it by months and periods. SIVEP-Gripe is a registry maintained by the Ministry of Health of deaths from severe acute respiratory infection (SARI), a broader category that includes COVID-19 and other diseases with similar symptoms. The registry contains data from public and private hospitals.
Timing of first confirmed death	Brazil IO	2020-21	Number of days between 12/31/2019 and the first COVID-19 death registered in the municipality.
Policy variables	Own data collection	2020	Policies types: commerce restrictions (closing non-essential businesses), gathering, transport, travel, and workplace restrictions, event cancellations, school closures, curfews, lockdowns, and face mask mandates. Dummy equal to 1 if the policy was in place in the municipality on a given day. We use it daily and also aggregate it by month. Data collection follows Chauvin et al. (2021) (see Appendix B3.
Daily Social Distancing Index	InLoco	2020-21	Share of active phone users staying within 450 meters of their residence in a given municipality on a given day. The index is measured using anonymized geolocalization data from around 30 million cellphones in Brazil (see Appendix B4).

Notes: Census's period of reference is the last week of July 2010, unless otherwise stated.

B2 COVID-19 data

Figure B1: Evolution of COVID-19 deaths across Brazilian municipalities



Notes: This graph plots the 7-day moving average of the number of deaths per 10,000 inhabitants (using the 2010 population) across Brazilian municipalities for each day from April 1, 2020, to January 31, 2021. In blue, we consider all Brazilian municipalities, while in orange we consider only municipalities in our sample of analysis. For both, we exclude municipalities in the state of Mato Grosso (3.3 percent), where we detected misreporting issues. The vertical lines separate the four main periods that characterize the evolution of COVID-19 in Brazil and that we analyze separately in Section 4.1: the beginning of the first wave (April-May 2020), peak of the first wave (June-August 2020), end of the first wave (September-October 2020), and beginning of the second wave (November 2020-January 2021).

B3 Policies data

We constructed our policy data directly from publicly available municipal legislation documents, following Chauvin et al. (2021).

The first step consisted in collecting all publicly available digital documents (laws, decrees, and other mandates) issued by each municipality in response to COVID-19. The documents were primarily found on the municipal government's website and on municipal gazettes (*diários oficiais*) and, in a few cases, in a national online legislation repository ("Leis Municipais").

The data collection took place between November 11 and December 29, 2020. We collected all relevant documents for all municipalities in our analysis sample for the period

March-October 2020. Data availability and accessibility varied across municipalities: while some featured dedicated web pages where COVID-19 laws were systematically posted, in others the documents could be hard to find and download and some documents appeared to be missing altogether. These issues were particularly prevalent in small municipalities, likely due to limited resources and institutional capacity. We address this issue by focusing our analysis on municipalities with a 2010 population of 10,000 or more, as in Chauvin et al. (2021), corresponding to 486 observations (49.5 percent of our sample).[35]

The next step consisted of extracting the full text of the legal documents and parsing it into individual articles, resulting in an article-level dataset. We then identified a series of key expressions associated with the presence (or absence) of each of the policies and used regular expressions to construct variables indicating whether each policy was in place in a given municipality on a given date. Lastly, we chose a random sample of 100 municipalities and read their legal documents to manually construct a "testing" policies dataset, which we used to validate the quality of the regular expressions algorithm.

In order to make our policy variables comparable with international datasets, we followed the policy definitions from the Oxford COVID-19 Government Response Tracker (Wade et al., 2022), focusing on 10 containment policies, defined as follows:

- *Commerce restrictions.* Closure of non-essential businesses. Specifically, the variable equals 1 if, on a given day, the law prevents non-essential businesses that involve in-person contacts from opening. If only delivery and pickup are allowed, businesses are considered closed. The variable also equals 1 if the law mentions a set of essential businesses that can remain open, while everything else must close. For instance, the variable equals 1 if grocery stores are allowed to remain open while commercial establishments, restaurants, and malls are closed. Mandated early closures (before a given time of the day) are not considered business closures. The variable equals 0 if non-essential businesses have not been closed or when a law reopens them, including when it maintains some rules on opening hours.

- *Curfew.* The variable equals 1 if the law imposes a curfew (*toques de recolher*) – a time window during which residents have to stay home (even if it starts at midnight and ends before dusk). This does not include lockdowns (see below).

- *Event cancellations.* The variable equals 1 if the law mandates the cancellation of large in-person events such as music festivals, concerts, sporting events, and June festivals (*Festa Junina*) and/or the closure of nightclubs, museums, and libraries. It equals 0 if the law allows events and parties to take place and/or reopens nightclubs, museums, and libraries to the public.

- *Face mask mandatory.* The variable equals 1 if the law mandates the use of face masks, including if they are mandatory indoors only. It equals 0 after this mandate ends and if the law only "recommends" the use of face masks.

[35]We could not find *any* document at all for 24 municipalities, among which only four have above 10,000 inhabitants. We consider these municipalities as missing in the policy analysis.

- *Gathering restrictions.* The variable equals 1 if the law prohibits gatherings, whether indoors or outdoors, which can include church meetings, municipal events, consumption of alcohol on the sidewalk, visits to parks or beaches, forums (*palestra*), conferences, or visits to residential buildings other than one's own.

- *Lockdown.* The variable equals 1 if the law imposes a lockdown (i.e., a stay-at-home order).

- *School closures.* The variable equals 1 if regular-curriculum schools are closed. This includes mandates to close or keep closed primary, secondary, or tertiary education schools, public or private. We do not consider the closure of other facilities such as dance schools, after-school, driving schools, or art schools.

- *Transport restrictions.* The variable equals 1 if the law shuts public transportation down, and 0 if is allowed to operate.

- *Travel restrictions.* The variable equals 1 if the law imposes a ban on all incoming vehicles.

- *Workplace restrictions.*The variable equals 1 if the law mandates non-public and non-essential workplaces to close. It equals 0 if the text allows non-essential workplaces to reopen or leaves it up to individual employers to decide.

Table B2 reports the number and share of Brazilian municipalities that used each of these policies at some point over the period March-October 2020. The first two columns are computed using a 20 percent random sample of municipalities above 10,000 inhabitants, from Chauvin et al. (2021). The third and fourth columns focus on municipalities in our analysis sample, also restricting to those with a population of at least 10,000. In both samples, four policies stand out as being used by the vast majority of municipalities (around 90 percent and above): event cancellations, face mask mandates, restrictions on gathering, and school closures.

Table B2: Number and share of municipalities that implemented containment policies

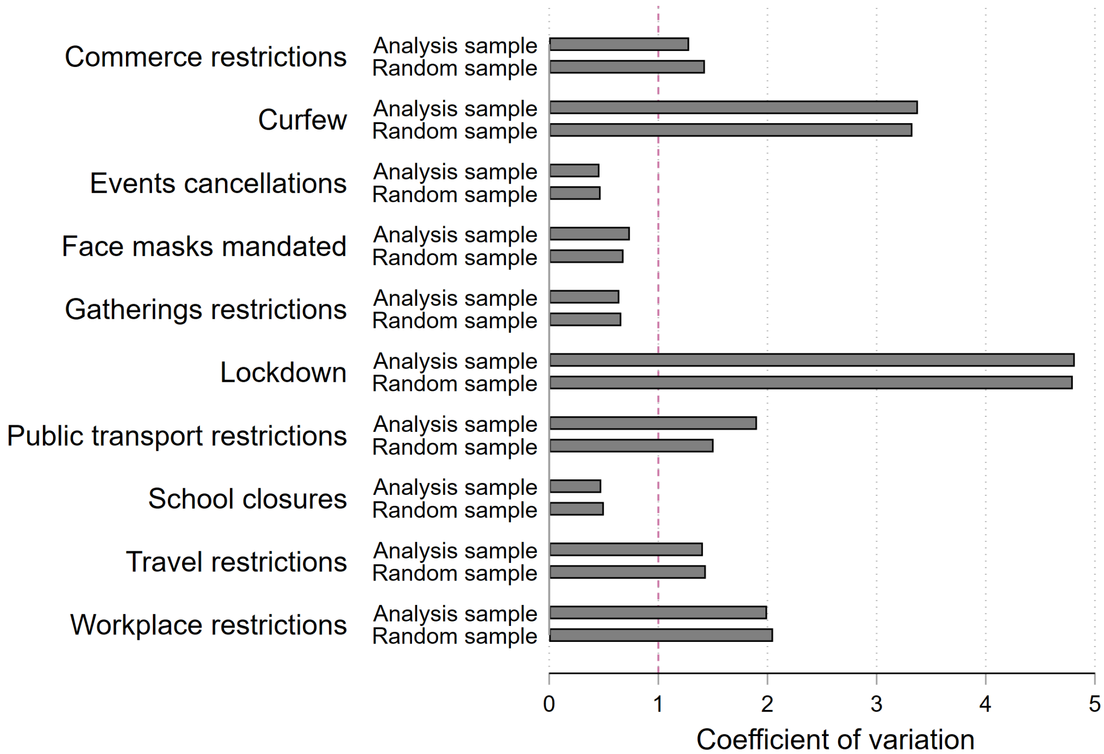
Policy	Representative municipalities	Share of total (%)	Municipalities in sample	Share of total (%)
Commerce restriction	410	69.02	327	67.28
Curfew	68	11.45	59	12.14
Events cancellation	555	93.43	459	94.44
Facemask mandatory	534	89.9	421	86.63
Gatherings restriction	533	89.73	437	89.92
Lockdown	46	7.74	38	7.82
School closure	544	91.58	454	93.42
Transport restriction	237	39.9	147	30.25
Travel restriction	246	41.41	206	42.39
Workplace restriction	169	28.45	148	30.45
Total	594	100	486	100

Notes: This table gives the number and share of municipalities that implemented the policy at least once from March to October 2020. The first two columns consider a 20-percent random sample representative of Brazilian municipalities with a population of 10,000 or larger (from Chauvin et al. (2021)). The last two columns consider municipalities in our sample of analysis with a population of 10,000 or larger.

Figure B2 considers the variation in the use of policies across municipalities and across time, using the same two samples. For each policy and over the period March–October, we computed the average and the standard deviation of the indicator equal to 1 if the policy was in place, across municipalities and days. We then used them to compute the coefficient of variation, equal to the ratio of the standard deviation to the mean. As shown in Figure B2, the coefficient of variation for the four most prevalent policies (event cancellations, face mask mandates, restrictions on gathering, and school closures) are all below 1, indicating limited variance not only across municipalities but also across time. In other words, for those policies, the vast majority of municipalities imposed them and they generally imposed them for similar amounts of time. This was particularly stark for school closure, as schools closed all over Brazil early in the pandemic and mostly remained closed over the year 2020.

We thus focus our analysis on the remaining six policies, for which we have enough variation to identify the effects of interest.

Figure B2: Coefficient of variation of containment policies



Notes: This figure reports the coefficient of variation for each policy, as explained in the text. For each policy, this statistic is reported for a 20-percent random sample representative of Brazilian municipalities with a population of 10,000 or larger (from Chauvin et al. (2021)) and for municipalities in our analysis sample with a population of 10,000 or larger.

B4 Isolation data

Our isolation data come from InLoco, a private technology company specialized in the use of location data.[36] During the COVID-19 pandemic, the company created the InLoco Social Isolation Index, which has already been used in several academic studies (Ajzenman et al., 2022; Peixoto et al., 2020; Brotherhood et al., 2022).

The index is based on de-identified apps location data from over 60 million cell phones in Brazil, around one-fourth of the devices in the country (InLoco, 2021). It is computed as follows. First, the company defines hexagon-shaped polygons of 450 meters radius and sides. Second, it assigns each unique user a "residence" polygon, based on the location where they spend the longest time during the night. Third, a user is defined as having left their residence if they were at least 40 meters away from their residence's polygon. Last, the index is computed at the municipal level as the share of users in the municipality that have not left their assigned residences on a given day.

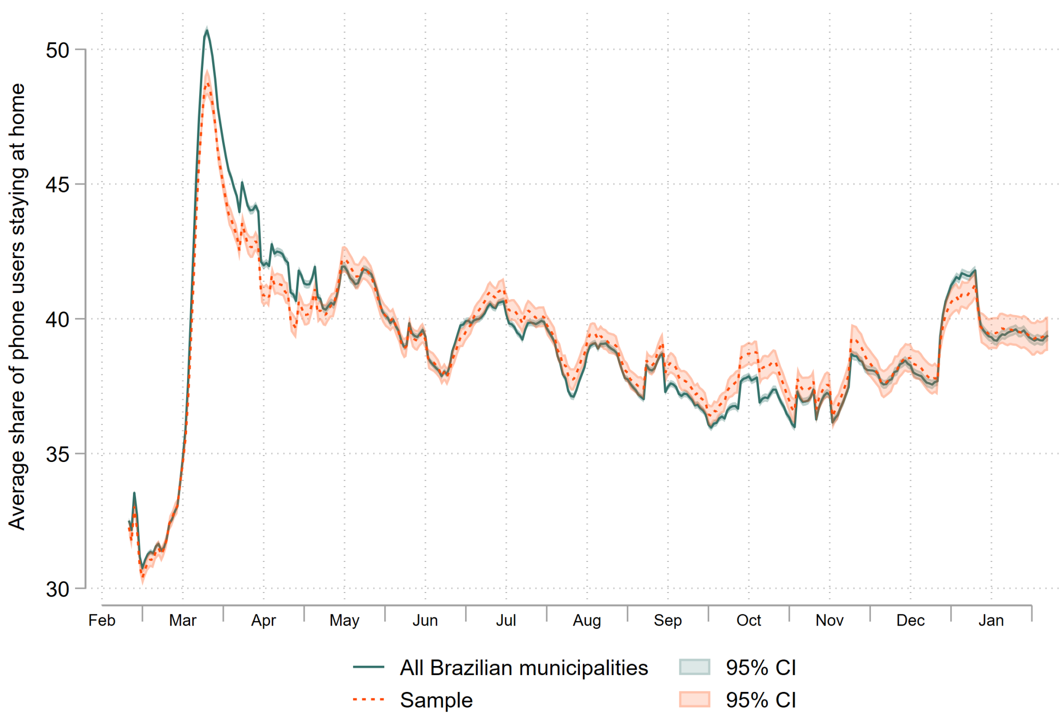
[36] In 2021, the company changed its name to Incognia (`https://www.incognia.com`).

The index is missing for some municipalities on some days. This happens for two main reasons. First, for privacy concerns, the company requires a minimum of 20 active users per polygon to compute the index on a given day. Second, InLoco started phasing out some of its business in Brazil towards the end of 2020. As contracts with data providers expired, some were not renewed, resulting in some polygons lacking the data necessary to compute the index in subsequent periods.

To avoid biases due to changes in the composition of the sample over time, we exclude from the analysis all municipalities in which the isolation index is missing at least one day during our period of analysis (from February 25, 2020, through January 31, 2021). Our isolation results are based on a balanced panel of 285 municipalities.

Figure B3 shows that our sample is representative of the isolation behavior of the Brazilian population. Indeed, the isolation index follows a very similar trajectory in all Brazilian municipalities and in the municipalities in our sample.

Figure B3: Evolution of the isolation index across Brazilian municipalities



Notes: This graph plots the 7-day moving average of the share of phone users staying at home across Brazilian municipalities for each day from February 25, 2020, to January 31, 2021. In blue, we consider all Brazilian municipalities, while in orange we consider only municipalities in our analysis sample. For both, we consider a balanced panel of municipalities, excluding those with missing values during the period of interest.

B5 Elections data

Our electoral data come from the Brazilian National Elections Authority (TSE, 2021). Our empirical strategy relies on the use of the 2016 municipal election results. We describe below the data cleaning and sanity checks we performed to correctly identify close races between female and male candidates and to correctly classify candidates by incumbency status.

B5.1 Supplementary elections

If the original election is invalidated due to irregularities, a supplementary election takes place later. In these cases, we consider the results of the last election, which determines the identity of the mayor in office during the COVID-19 crisis. We end up using the results of the supplementary election for 25 municipalities in our sample and we show that the results are robust to excluding them. We further exclude one municipality for which the supplementary election took place in March 2020, implying that two different mayors were in office during our period of analysis.

B5.2 Sanity checks and corrections

Vote data. We ran sanity checks on all 2016 election results and corrected the erroneous data using alternative online sources, such as press coverage of local elections. We corrected vote results for the following elections:

- Eleven elections for which the number of votes was missing for some or all candidates. We imputed it from alternative sources.
- Twelve elections in which the candidate who got the most votes was not labelled "elected" in the TSE data. We manually checked each case: for seven, the winner did take office and the variable "elected" was wrongly coded. In the remaining five, the winner ended up not taking office due to irregularities. We removed them (only one would have ended up in our final analysis sample).
- Seven elections in which the total number of votes reported did not match the sum of the votes received by all candidates. In all cases, the total number of votes was incorrectly reported, so we corrected it.

Candidates' gender. The TSE data report the gender of each candidate. To validate it, we generated an alternative gender measure based on the candidate's first name, using the R package genderBR (Meireles, 2021). We then checked manually all cases in which a discrepancy was found between the TSE classification and our own, using online sources. In all cases, the TSE measure was correct. We are thus confident that the gender of each candidate is correctly assigned.

Candidates' incumbency status. The TSE data report the self-declared incumbency status of candidates. This variable is key to assessing whether the candidate is able to run again if elected. Indeed, the winner of the election is term-limited if they already served as mayor at some point during the last term.[37] To verify the accuracy of the TSE variable, we built our own incumbency indicator, using the results of the 2012 election and identifying a candidate as incumbent if they won in 2012. The two variables differ for 278 candidates, whom we investigated manually. This enabled us to correct 69 cases in which candidates erroneously reported their incumbency status in 2016. For the remaining cases, either the candidate with the most votes in 2012 was removed from office before the start of the term and thus did not serve as mayor (cases for which our incumbency indicator was equal to 1 whereas the TSE variable was correctly equal to 0) or the candidate in 2016 served as mayor during the previous term without having been directly elected in 2012 – for example, as vice-mayor stepping in after a mayor's death (cases for which our incumbency indicator was equal to 0 whereas TSE variable was correctly equal to 1). Out of the 69 corrections we made, 13 cases ended up in our analysis sample.

B5.3 Invalidated top-two candidates

Finally, after restricting our focus on elections in which the top two contenders were one woman and one man, we identified 40 elections in which one of the two candidates with the most votes had their votes invalidated by the electoral justice due to irregularities, such as having registered their candidacy after the official deadline. We removed those elections, as the candidates who were eventually assigned first and second place were not the ones who received the most votes (or, in the case of elections with only two candidates, the reported vote shares of the two front-runners do not reflect the actual number of votes they originally received). We used the following tests to identify those cases and checked them all manually:

- Sixteen elections with only two candidate, in which the second-place candidate had zero votes. For such cases, the second-place candidate originally received votes, but their candidacy was then invalidated.

- Six elections for which TSE registered some invalidated votes. For four cases, the invalidated votes were for one of the original top-two candidates.

- 19 elections in which the number of null votes (in which invalidated votes are often counted) was larger than the number of votes received by the second-place candidate. All were indeed cases in which one of the top-two candidates was invalidated.

- One election in which one of the top-two candidate was considered ineligible to run (labelled "*inapto*" in TSE data) and was invalidated.

[37] The only exceptions are when the mayor was elected during the last election but removed from office before the start of the term or when a politician served only as a short-term interim mayor during the previous term, as long as this does not take place within 6 months of the next election, as defined by the Article 14, Paragraph 5, of the Federal Constitution of Brazil (Brasil, 1988).

C Balance tests

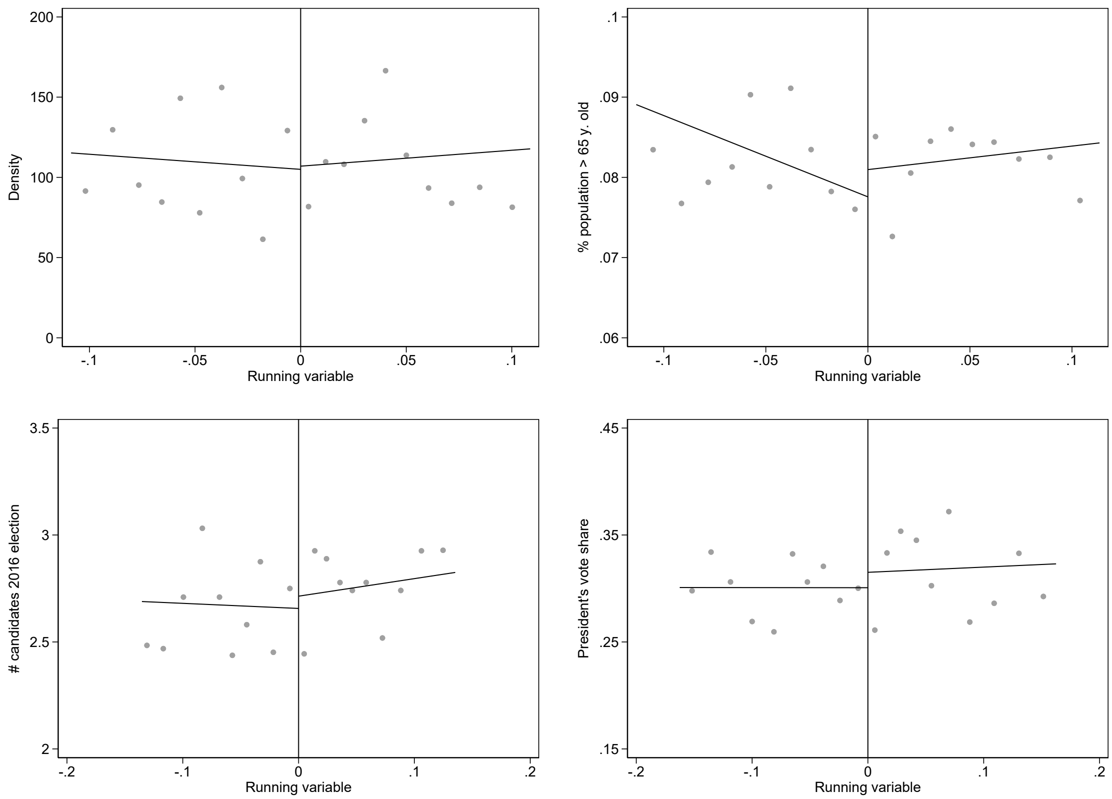
Table C1: Balance test: Municipality characteristics

Outc.	(1) Pop	(2) Density	(3) Persons /room	(4) Commuting	(5) % above 65 y. old	(6) Nursing h. residents	(7) Area	(8) Distance to São Paulo	(9) Km to airport
Treat.	-2,851	2.0	-0.032	0.276	0.003	-1.096	-1,794*	-109	-65.0
	(1,993)	(23.7)	(0.037)	(0.858)	(0.004)	(1.458)	(838)	(123)	(36.6)
P-value	0.201	0.780	0.456	0.736	0.359	0.610	0.062	0.487	0.117
Obs	648	489	606	515	499	580	538	604	587
Polyn.	1	1	1	1	1	1	1	1	1
Bdw	0.152	0.109	0.142	0.119	0.114	0.134	0.125	0.140	0.136
Mean	15,263	105.0	0.731	21.300	0.078	4.007	2,923	1,552	344.72

Outc.	(10) Median income	(11) Inform. rate	(12) Unemp. rate	(13) % college employed	(14) % black & mixed	(15) % employed agriculture	(16) % evangelical	(17) Turnout	(18) Number cand	(19) President vote share
Treat.	34.4	0.003	-0.004	-0.005	-0.046	-0.003	-0.010	0.019	0.057	0.014
	(20.6)	(0.010)	(0.004)	(0.005)	(0.036)	(0.025)	(0.016)	(0.010)	(0.181)	(0.030)
P-value	0.136	0.779	0.497	0.439	0.288	0.878	0.454	0.138	0.898	0.849
Obs	719	565	606	584	549	622	577	579	586	677
Polyn.	1	1	1	1	1	1	1	1	1	1
Bdw	0.184	0.130	0.141	0.135	0.127	0.145	0.132	0.133	0.135	0.163
Mean	293.1	0.168	0.046	0.069	0.626	0.446	0.156	0.846	2.657	0.301

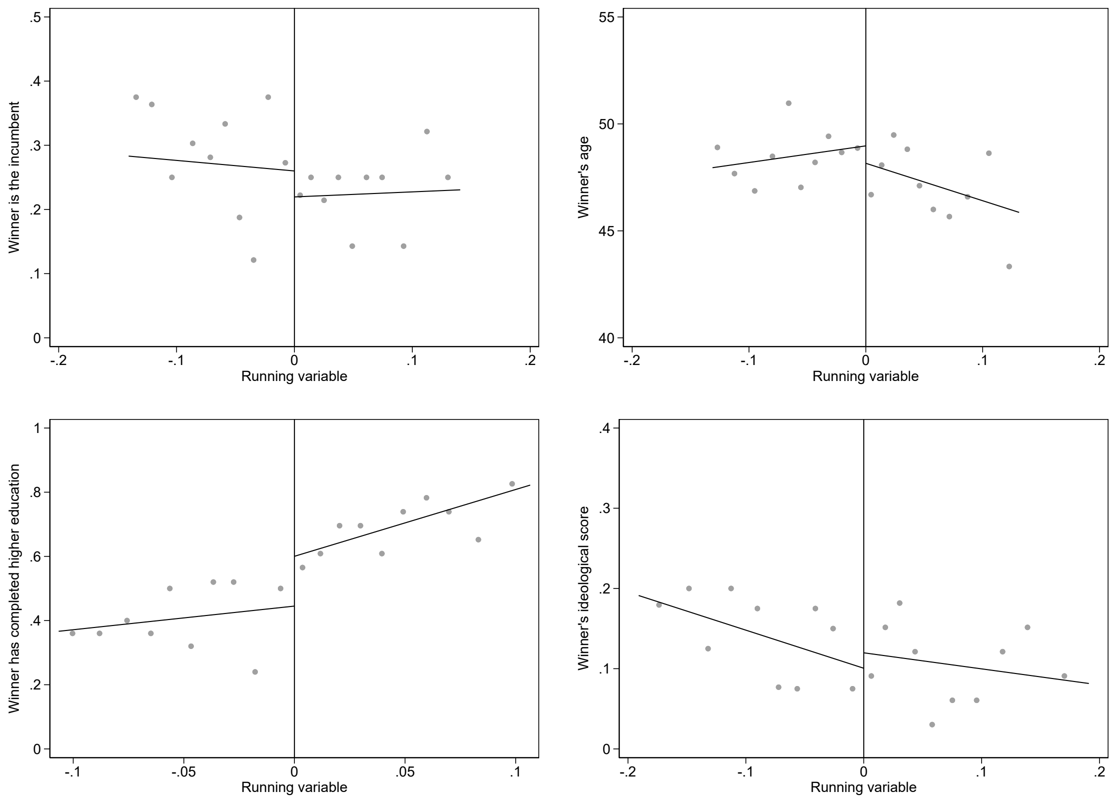
Notes: Each column considers a specific baseline characteristic, as defined in Table B1. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths (referred to as "Bdw" in the table). We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Figure C1: Balance test: Municipality characteristics



Notes: This figure shows the balance test results visually for four baseline variables discussed in the text (density, share of the population above 65 years old, number of candidates in the 2016 election, and vote share of the president in office during the COVID-19 outbreak). Each graph is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the baseline characteristic. Averages are calculated within quantile spaced bins of the running variable. The running variable is the margin of victory of the female candidate in the 2016 election (percentage-point difference between the vote share of the female and the male candidates). Positive (negative) values denote that the female (male) candidate won.

Figure C2: Balance test: Characteristics of the winner of the election



Notes: This figure shows the balance test results visually for four winner's characteristics (incumbency, age, education, and ideological score). Each graph is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the outcome variable. Averages are calculated within quantile spaced bins of the running variable. The running variable is the margin of victory of the female candidate in the 2016 election (percentage-point difference between the vote share of the female and the male candidates). Positive (negative) values denote that the female (male) candidate won.

D Robustness tests

Alternative death measure. To make sure that our results are not affected by misreporting, we use as an alternative outcome the number of deaths attributed to severe acute respiratory infections (SARI) from the SIVEP-Gripe dataset described in Section 2.3. Figure D1 shows the strong correlation in the cumulative number of deaths as of January 31, 2021 between the two data sources. Tables D1 and D2 replicate our main results using the number of deaths by period and month, respectively. As in our main tables (Table 3 and Appendix Table A6) the point estimate is large and positive in Period 1, an effect driven by the month of May 2020, but large and negative in Period 4, an effect driven by the months of November and December 2020. Finally, Figure D2 plots the daily estimates for both SARI deaths and our main measure of COVID-19 deaths. The patterns are very similar, with positive coefficients at the beginning of the period of analysis and negative coefficients at the end of the year.

Controls. We test the robustness of our results to adding a wide range of controls. In Panel A of Appendix Table D3, we include the 19 municipality characteristics presented in Table 1, while in Panel B we include the 12 winner characteristics presented in Table 2. All estimates are very close in magnitude when including either set of controls and all remain significant at the five-percent level.

State fixed effects. Policies implemented at the state level might influence mayors' decisions and COVID-19 outcomes. However, variations in state policies are unlikely to explain our results. First, Figure A2 and the balance tests in Appendix C show that female- and male-led municipalities are evenly geographically distributed. Second, Appendix Table D4 shows that our results remain virtually unchanged when we exploit within-state variation only, through the inclusion of state fixed effects. Note that in order to include state fixed effects, we had to remove nine states that contain less than 20 municipalities, accounting for eight percent of our sample.

Sample selection. We test the robustness of the results to excluding some unusual observations from the sample: municipalities in the state of Mato Grosso, for which we observed some irregularities in the data (3.3 percent of the sample), and municipalities that held supplementary elections (2.6 percent). As shown in Table D5, the results are not affected by this restriction.

Polynomial order and bandwidth choice. Table D6 shows that our results are robust to using a second-order polynomial, while Figure D3 shows that the point estimates remain stable over a wide range of bandwidths.

Table D1: Impact of having a female mayor on SARI deaths, by period

	(1)	(2)	(3)	(4)
Outcome	# SARI deaths per 10,000 inhabitants			
	Period 1	Period 2	Period 3	Period 4
Treatment	0.739**	-0.050	-0.219	-0.854
	(0.303)	(0.538)	(0.313)	(0.476)
Robust p-value	0.014	0.972	0.397	0.107
Observations	487	487	621	579
Polyn. order	1	1	1	1
Bandwidth	0.108	0.108	0.145	0.133
Mean, left of threshold	0.640	3.123	1.707	2.728

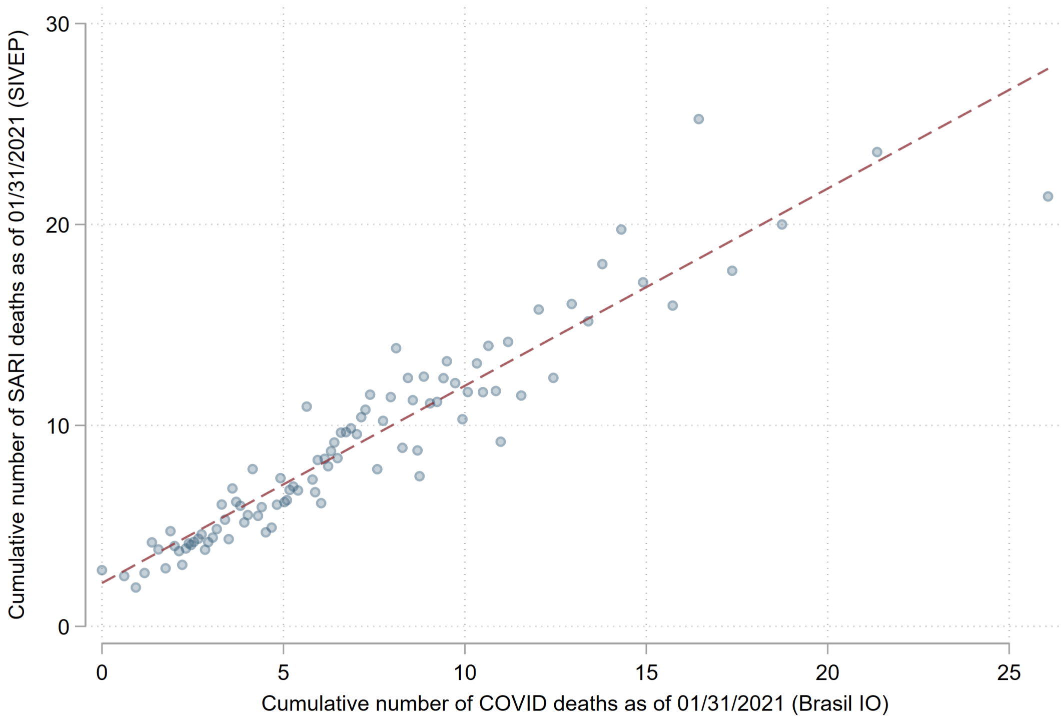
Notes: Each column takes as outcome the number of SARI deaths per 10,000 inhabitants (using the 2010 population) during the period of interest. Period 1 (resp. 2, 3, and 4) is April-May 2020 (resp. June-August 2020, September-October 2020, and November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table D2: Impact of having a female mayor on SARI deaths, by month

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome	Number of SARI deaths per 10,000 inhabitants									
	04/20	05/20	06/20	07/20	08/20	09/20	10/20	11/20	12/20	01/21
Treatment	0.050	0.619**	0.127	-0.119	-0.025	-0.002	-0.320	-0.344*	-0.611**	0.118
	(0.110)	(0.261)	(0.293)	(0.255)	(0.283)	(0.223)	(0.228)	(0.163)	(0.279)	(0.250)
R. p-value	0.614	0.018	0.538	0.678	0.870	0.997	0.133	0.084	0.036	0.666
Obs.	576	524	489	491	518	584	495	580	574	543
Polyn.	1	1	1	1	1	1	1	1	1	1
Bandwidth	0.132	0.122	0.109	0.109	0.121	0.135	0.111	0.134	0.131	0.126
Mean	0.242	0.425	0.915	1.142	1.062	0.956	0.820	0.697	1.185	0.836

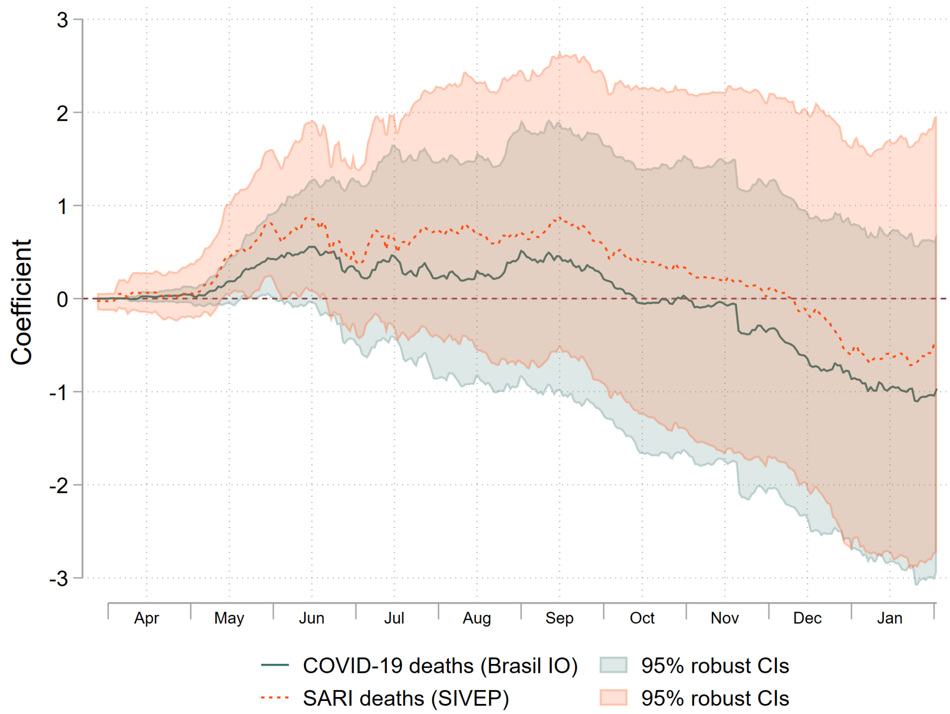
Notes: Each column takes as outcome the number of SARI deaths per 10,000 inhabitants (using the 2010 population) during the month of interest. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robust p-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Figure D1: Correlation of municipal COVID-19 deaths (Brasil.io) and deaths attributed to severe acute respiratory infections (SIVEP-Gripe)



Notes: This scatterplot reports the cumulative number of COVID-19 deaths per 10,000 inhabitants as of January 31, 2021, in each municipality in our sample, using the Brasil.io dataset (x-axis), and the cumulative number of deaths per 10,000 inhabitants attributed to severe acute respiratory infections (SARI), using the SIVEP-Gripe dataset (y-axis).

Figure D2: Impact on the cumulative number of SARI and COVID-19 deaths



Notes: This figure plots the RD estimates obtained by taking as outcome the cumulative number of deaths per 10,000 inhabitants, for each day from April 1, 2020, to January 31, 2021. In orange, the point estimates and 95-percent robust confidence intervals correspond to deaths attributed to severe acute respiratory infections (SARI), using the SIVEP dataset. In blue, the point estimates and 95-percent robust confidence intervals correspond to COVID-19 deaths, using the Brasil.io dataset.

Table D3: Impact on COVID-19 deaths, including controls

Panel A: Controlling for municipality characteristics

	(1)	(2)	(3)	(4)
Outcome	Number of Covid-19 deaths			
	No control		With controls	
	Period 1	Period 4	Period 1	Period 4
Treatment	0.391**	-0.999**	0.497***	-0.996**
	(0.176)	(0.405)	(0.164)	(0.392)
Robust p-value	0.035	0.016	0.003	0.021
Observations	578	513	466	495
Polyn. order	1	1	1	1
Bandwidth	0.133	0.118	0.103	0.111
Mean	0.203	2.432	0.169	2.397

Panel B: Controlling for winner characteristics

	(1)	(2)	(3)	(4)
Outcome	Number of Covid-19 deaths			
	No control		With controls	
	Period 1	Period 4	Period 1	Period 4
Treatment	0.391**	-0.999**	0.456**	-1.049**
	(0.176)	(0.405)	(0.179)	(0.413)
Robust p-value	0.035	0.016	0.015	0.015
Observations	578	513	513	479
Polyn. order	1	1	1	1
Bandwidth	0.133	0.118	0.117	0.105
Mean	0.203	2.432	0.177	2.367

Notes: In Panel A (resp. B), Columns 3 and 4, we include as controls all the municipal (resp. winner) characteristics presented in Table 1 (resp. Table 2). The outcome is the number of COVID-19 deaths per 10,000 inhabitants (using the 2010 population) during the period of interest. Period 1 (resp. 4) is April-May 2020 (resp. November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robustp-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table D4: Impact on COVID-19 deaths, including state fixed effects

	(1)	(2)	(3)	(4)
Outcome	Number of Covid-19 deaths			
	Full sample		Restricted + state FEs	
	Period 1	Period 4	Period 1	Period 4
Treatment	0.391**	-0.999**	0.414**	-1.045**
	(0.176)	(0.405)	(0.159)	(0.410)
Robust p-value	0.035	0.016	0.015	0.013
Observations	578	513	560	471
Polyn. order	1	1	1	1
Bandwidth	0.133	0.118	0.142	0.117
Mean	0.203	2.432	0.206	2.423

Notes: In Columns 3 and 4, we include state fixed effects and remove municipalities part of states with fewer than 20 municipalities in our sample (8 percent). The outcome is the number of COVID-19 deaths per 10,000 inhabitants (using the 2010 population) during the period of interest. Period 1 (resp. 4) is April-May 2020 (resp. November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robustp-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table D5: Impact on COVID-19 deaths, excluding unusual observations

	(1)	(2)	(3)	(4)
Outcome	Number of Covid-19 deaths			
	Full sample		Robustness sample	
	Period 1	Period 4	Period 1	Period 4
Treatment	0.391**	-0.999**	0.395**	-0.881**
	(0.176)	(0.405)	(0.178)	(0.414)
Robust p-value	0.035	0.016	0.034	0.040
Observations	578	513	560	486
Polyn. order	1	1	1	1
Bandwidth	0.133	0.118	0.137	0.117
Mean	0.203	2.432	0.216	2.309

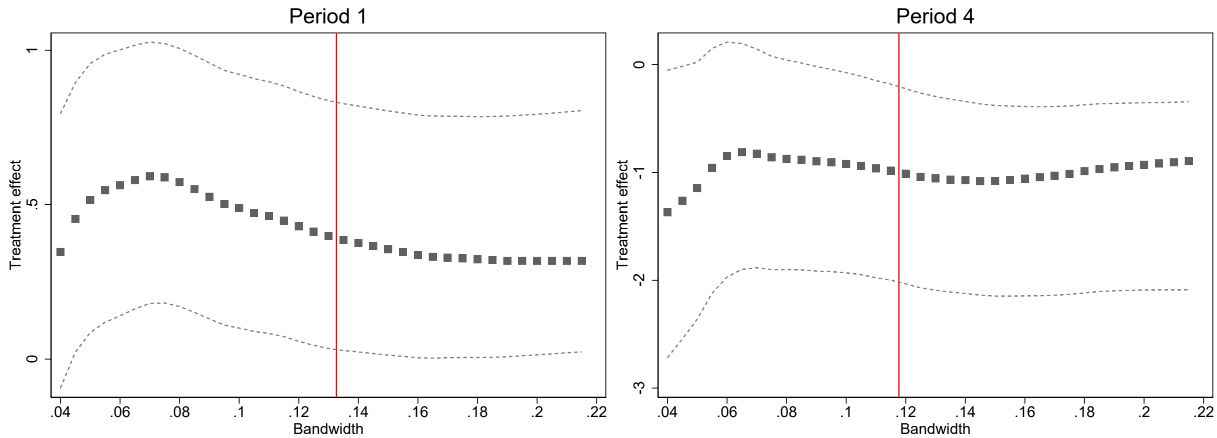
Notes: In Columns 3 and 4, we exclude municipalities in Mato Grosso state and municipalities that held a supplementary election – 3.3 and 2.6 percent of the sample, respectively. The outcome is the number of COVID-19 deaths per 10,000 inhabitants (using the 2010 population) during the period of interest. Period 1 (resp. 4) is April-May 2020 (resp. November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold) and use MSERD data-driven bandwidths. We assess statistical significance based on the robustp-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

Table D6: Impact on COVID-19 deaths, using a second-order polynomial

	(1)	(2)	(3)	(4)
Outcome	Number of Covid-19 deaths			
	Period 1	Period 4	Period 1	Period 4
Treatment	0.391**	-0.999**	0.483**	-1.173**
	(0.176)	(0.405)	(0.207)	(0.464)
Robust p-value	0.035	0.016	0.023	0.027
Observations	578	513	727	736
Polyn. order	1	1	2	2
Bandwidth	0.133	0.118	0.190	0.195
Mean	0.203	2.432	0.139	2.451

Notes: In Columns 3 and 4, we use a second-order polynomial instead of fitting linear regressions. The outcome is the number of COVID-19 deaths per 10,000 inhabitants (using the 2010 population) during the period of interest. Period 1 (resp. 4) is April-May 2020 (resp. November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use MSERD data-driven bandwidths and assess statistical significance based on the robustp-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

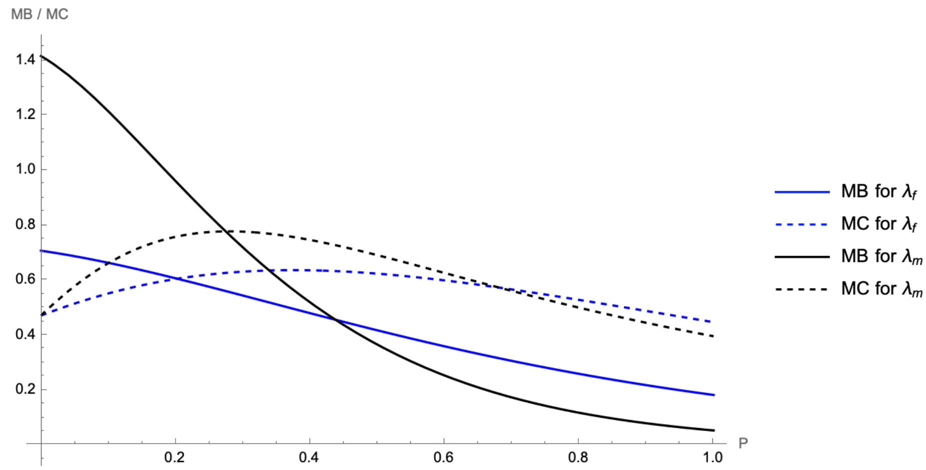
Figure D3: Impact on COVID-19 deaths: Robustness to bandwidth choice



Notes: These figures show the sensitivity of the point estimate to bandwidth choice. Dots represent the estimated treatment effect using different bandwidths (horizontal axis). Dotted lines represent the 95-percent robust confidence interval. The estimates are reported for values of the bandwidth from 4 to 22 percentage points, in steps of 0.2 percentage points. The vertical red line gives the value of the MSERD optimal bandwidth used in the main estimation. The outcome is the number of COVID-19 deaths per 10,000 inhabitants in Period 1 (left graph) or in Period 4 (right graph). The independent variable is an indicator equal to 1 if the female candidate won in 2016. Each estimation uses a nonparametric estimation procedure (fitting two linear regressions separately on each side of the threshold).

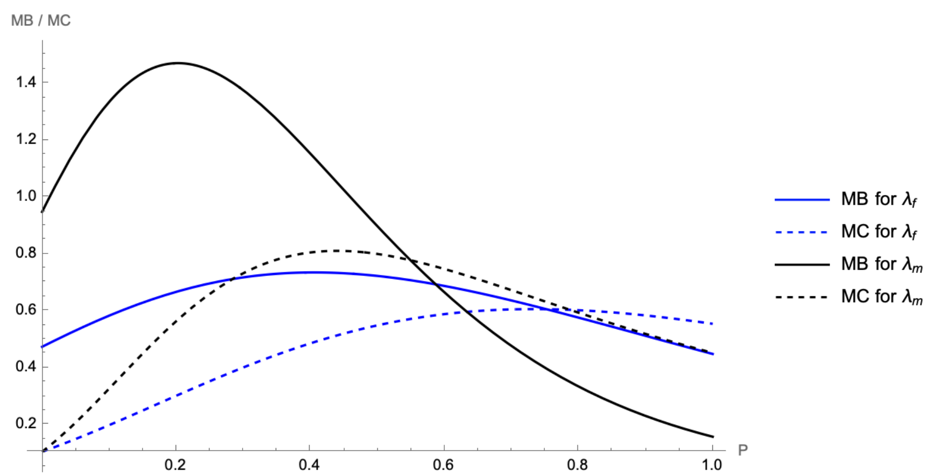
E Model appendix

Figure E1: Marginal benefits and marginal costs of the policy at low levels of belief in the shock ($p = 0.25$)



Notes: The figure plots the marginal benefits (MB) and marginal costs (MC) to voters of the policy P for the case in which voters believe that the policy is relatively ineffective ($\lambda_s = \lambda_f = 2$) and for the case in which they believe it is relatively effective ($\lambda_s = \lambda_m = 4$). It assumes low levels of belief in the shock ($p = 0.25$), a pre-crisis amount of the public good of $\tilde{g} = 1$, and a shock of magnitude $\psi = 3$.

Figure E2: Marginal benefits and marginal costs of the policy at high levels of belief in the shock ($p = 0.75$)



Notes: The figure plots the marginal benefits (MB) and marginal costs (MC) to voters of the policy P for the case in which voters believe that the policy is relatively ineffective ($\lambda_s = \lambda_f = 2$) and for the case in which they believe it is relatively effective ($\lambda_s = \lambda_m = 4$). It assumes high levels of belief in the shock ($p = 0.75$), a pre-crisis amount of the public good of $\tilde{g} = 1$, and a shock of magnitude $\psi = 3$.