NBER WORKING PAPER SERIES

GENDER AND ELECTORAL INCENTIVES: EVIDENCE FROM COVID-19 RESPONSE

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Working Paper 32410
http://www.nber.org/papers/w32410

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2024, Revised April 2026

We thank NBER discussant Filipe Campante, as well as Samuel Berlinski, Julia Cagé, Andy Eggers, Claudio Ferraz, Johannes Fleck, Jeff Frieden, Ed Glaeser, Nathan Nunn, Nadine Riedel, Marco Tabellini, Clementine Van Effenterre, Razvan Vlaicu, Nico Voigtländer, Romain Wacziarg, Melanie Wasserman, and seminar participants at the NBER Political Economy Fall Meeting, IADB-RES, the Urban LACEA Network, the Urban Economics Association Annual Meetings, UCLA Anderson, CalTech, UWE Bristol, Early Career Urban Economics Group, CesIfo, the University of Utah, UBC's Vancouver School of Economics, the University of Southern California, the University of California Irvine, the University of Chicago, the University of California San Diego, Stanford University, Johns Hopkins University, PERICLES and Axa research lab on Gender Equality Workshop, California Center for Population Research, CEPR WE_ARE Seminar Series, Rochester University, EAYE Conference, CREST, AMSE, Benelux PECO, HEC Montreal, and the ASSA 2025 annual meeting for their helpful comments and suggestions. We are also grateful to Stephanie Kestelman for her help and guidance in the production of the policy data, to Rafael M. Rubião and Nicolás Herrera L. for their outstanding research assistance, and to Livia Almeida, Luisa Curcio, Juliana Pinillos, Vinícius Princiotti, Haydée Svab, Julio Trecenti, and Bruna Wundervald for additional research support. The opinions expressed in this publication are those of the authors and do not necessarily reflect the views of the Inter-American Development Bank, its Board of Directors, the countries they represent, or the National Bureau of Economic Research.

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NBER Working Paper No. 32410
May 2024, Revised April 2026
JEL No. D72, H11, J16

ABSTRACT

This paper provides new evidence on why men and women leaders make different choices. Exploiting Brazilian close elections, we show that female mayors responded differently to the COVID-19 crisis over the year 2020. Female mayors were less likely to close non-essential businesses early in the pandemic and female-led municipalities experienced higher mortality, while the reverse was true later on. We show that these findings can be rationalized by a simple political agency model in which politicians seek re-election and voters are gender biased. Consistent with this interpretation, the gender differences we find are driven exclusively by mayors facing re-election.

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1 Introduction

A large literature documents that men and women make different choices, ranging from the educational paths pursued by students and the choices of workers in the labor market to politicians' career and policy decisions. These differences are persistent and explain a large part of the gender inequalities we observe (Goldin, 2014; Bertrand, 2020; Hessami and da Fonseca, 2020; Wasserman, 2023). A common interpretation is that men and women have different personality traits and preferences (Bertrand, 2011; Croson and Gneezy, 2009). Another explanation is that the existence of gender discrimination and stereotypes creates incentives for women to act differently. If women expect to be judged more harshly and face more backlash for a given action (Bertrand and Duflo, 2017; Sarsons, 2022), it is rationally less beneficial for them to adopt it. Identifying why women make different choices is key to understanding the persistence of gender inequalities and for the design of policies that seek to reduce them.

This paper investigates this question in politics, where prior research documents differences in the choices made by female politicians, as well as the presence of gender bias among voters (e.g., Brollo and Troiano, 2016; Le Barbanchon and Sauvagnat, 2022). We study the response of politicians to the COVID-19 crisis, an exogenous shock that made policies particularly salient to voters. Our analysis focuses on Brazilian mayors, who could independently decide over containment policies and who faced new municipal elections at the end of 2020. Using local daily data on COVID-19 deaths and policies, we analyze female and male mayors' responses to the crisis throughout the last year of their term. We then study the role of electoral incentives in shaping these responses, both theoretically, through a model of political agency with gender-biased voter beliefs, and empirically, by exploiting variation in electoral incentives across mayors.

In order to isolate the 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 2016 – the last election 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 the individual characteristics of the winner. Closely-elected female and male mayors are similar in a wide range of observable attributes including incumbency status, age, race, occupation, and political orientation. One exception is educational attainment, which is, on average, higher for female mayors, consistent with the presence of gender discrimination and positive selection. We are, however, confident that our results are not driven by the independent effect of education or ability: our results are robust to controlling for politicians' observable characteristics (including education and experience), and, using a separate RDD, we show that education has no independent impact on our outcomes of interest.

Our main outcome is the number of COVID-19 deaths in the municipality. We find large but opposite effects at the beginning and at the end of the year. At the beginning of the first wave (April-May 2020),[1] having a female mayor led to 0.44 more deaths per 10,000 inhabitants, representing a threefold 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 40 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. Using novel 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 later on. Commerce restrictions were in place 2.8 and 10.4 fewer days in female-led municipalities in March and April 2020, as female mayors started closing non-essential businesses 53 days later on average. In contrast, commerce restrictions were in place 8.1 and 7.6 more days in female-led municipalities in September and October 2020.[2]

Overall, our results suggest that female and male mayors handled the COVID-19 crisis differently: female mayors were less likely to undertake containment efforts early in the

[1] The COVID-19 pandemic hit Brazil relatively late compared to other large countries, and the vast majority of municipalities experienced their first death in April 2020.

[2] Assessing the causal impact of containment 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.

pandemic, but imposed stricter policies later on. These time-varying differences are unlikely to be solely driven by differences in policy preferences, as female mayors do not consistently prioritize health over the economy, or the reverse.

The second part of the paper uncovers the role of electoral incentives. We start by using a simple political agency model to illustrate how electoral incentives and voter bias can rationalize the reversal we observe. The key insight is that the direction of the gender gap depends on the interaction between voters' gender bias and their perception of the crisis severity. In the model, a re-election-seeking politician can enact policies that mitigate an incoming shock to a public good but that also generate a direct cost to voters.[3] We assume that voters evaluate female leaders less favorably than male leaders. More specifically, we assume that female politicians receive less credit for the same policy decisions, consistent with evidence of gender attribution gaps in political and organizational settings (e.g., Egan et al., 2022; Sarsons, 2022). When voters believe that there is a low likelihood that the shock will materialize – such as early in the pandemic – they are less willing to accept containment policies. In this context, since their actions are undervalued, female politicians have an incentive to do even less than male politicians. Conversely, when voters believe that there is a significant threat to the public good – once the health consequences have become more apparent – they are more willing to accept the disutility associated with containment policies in order to preserve the public good. In this case, female leaders have an incentive to enact more policies than male leaders to compensate for the relative lack of recognition for their actions.

We then investigate the role of electoral incentives empirically. To do so, we exploit the two-term limit rule and 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. In line with the results being driven by mayors with electoral incentives, we find that the gender differences in the evolution of COVID-19 deaths are only driven by mayors eligible to run for re-election. In other words, we do not find significant gender differences among mayors who could not run in the 2020 election. We also show that these heterogeneity results are unlikely to be driven by differences in experience, as, among first-time mayors, the results are not stronger for younger mayors or for mayors who did not serve as municipal councilors previously.

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

Finally, we provide additional evidence that our results are unlikely to be driven by alternative mechanisms, including gender differences in policy priorities, risk or competition aversion, or differential support by the government or municipal council.

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 biases. Our results have important implications for the way we interpret gender differences in decision-making. These differences might stem not from differences in women's intrinsic preferences or personality traits but rather from differences in the incentives they face. This is also key for the design of policies: if gender differences are driven by the presence of stereotypes, policies designed to "de-bias" institutions and public opinion can help address gender inequalities (Bohnet, 2016).

Contribution to the Literature

By exploring why female politicians make different choices, we bridge the gap between three important, but so far largely disconnected streams 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.

A large literature documents 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) and spend more on education and health (Clots-Figueras, 2011, 2012; Bhalotra and Clots-Figueras, 2014; Funk and Philips, 2019). The results are less conclusive in high-income countries.[5] However, null effects can mask more subtle differences: Accettura and Profeta (2021) find that, although having a female mayor does not impact overall spending in Italy, it affects the timing of public expenditures, stressing the importance of investigating gender differences over time.[6] Only a few recent papers focus on gender differences in leadership in crisis

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

[5]While female legislators are more likely to support bills related to family and children's issues (Besley and Case, 2003; Lippmann, 2022), 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).

[6]While the paper does not explore the mechanisms explaining those results, Accettura and Profeta (2021) hypothesize that it could come from voters disliking women engaging into political business cycles. This explanation is in line with the evidence we provide in this paper.

contexts. Dube and Harish (2020) find that European queens were historically more likely to be at war than kings, and Eslava (2024) shows that having a female mayor reduced the number of guerrilla attacks in Colombia.

Particularly relevant to the empirical setting used in this study, two papers provide evidence of gender differences in leaders' behavior using close election designs in Brazil: Brollo and Troiano (2016) show that female Brazilian leaders are less corrupt and Bruce et al. (2022) find that female-led municipalities in Brazil had a lower number of total COVID-19 deaths by the end of 2020. While we use a similar research design as Bruce et al. (2022), we address different questions. Our paper moves beyond static comparisons to analyze how gender differences in crisis response evolve over time and why they arise. We show that conclusions about the role of female leadership can vary sharply depending on the timing of analysis. Moreover, we leverage this result and variation in re-election eligibility to shed light on the mechanisms. Our key contribution is to show that gender differences in policymaking can stem from differences in electoral incentives, rather than from intrinsic differences in preferences or leadership styles. Our findings offer a new plausible interpretation for existing results. For instance, the findings of Brollo and Troiano (2016) could be explained by the fact that female leaders expect more backlash from voters for engaging in corruption and patronage than male leaders do.

A large body of work finds evidence of voter bias against female candidates (e.g., Fréchette et al., 2008; De Paola et al., 2010; Le Barbanchon and Sauvagnat, 2022; Eyméoud and Vertier, 2023; de Albuquerque et al., 2025).[7] Beyond gender discrimination at the electoral stage, voters also appear to be 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

[7] Exceptions in the quasi-experimental literature include Baltrunaite et al. (2019) and Broockman and Soltas (2020). 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; Gonzalez-Eiras and Sanz, 2021; Fujiwara et al., 2023).

"feminine" issues such as child care and education (Herrnson et al., 2003; Lawless, 2004; Beaman et al., 2009; Eggers et al., 2018).

Importantly, gender biases against female leaders materialize even when they are elected with the same support as male politicians, and despite the fact that they tend to be positively selected (Baltrunaite et al., 2014; Besley et al., 2017). Indeed, using the same close-election design as in our analysis – and thus comparing female and male mayors with the same electoral support – Gagliarducci and Paserman (2012) show that Italian female mayors are more likely to experience an early termination of their mandate and Daniele et al. (2025) find that they are more likely to be the targets of violent attacks during their time in power. Gender biases in performance evaluation have also 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).

If women expect to be judged more harshly, they have incentives to make different choices.[8] We show that this can rationalize why female leaders 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 re-election exert more effort than term-limited ones (Besley and Case, 1995; List and Sturm, 2006; Sieg and Yoon, 2017; Fouirnaies and Hall, 2022).[9] In Brazil, Ferraz and Finan (2011) show that non–term-limited mayors engage less in corruption. Looking at two separate conditional cash transfer programs, de Janvry et al. (2012) find that election-seeking mayors implemented the program more effectively, while Frey (2021) shows that electoral incentives led to poorer program targeting.

The effects of electoral incentives on the behavior of politicians are more pronounced when voters are more aware of leaders' policy decisions and performance (Snyder and

[8]As Bertrand (2020) argues, preferences and personality traits themselves are likely endogenous to the gender stereotypes. For instance, Bowles et al. (2007) show that women who initiate negotiation receive systematically worse evaluation, which can help explain why they are found to have a lower "preference" for negotiating. Similarly, several studies find that gender differences in risk aversion arise from women expecting negative consequences from not conforming to gender stereotypes (e.g., Carr and Steele, 2010).

[9]Similarly, 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.

Strömberg, 2010; Ashworth, 2012). Crises tend to create such higher-accountability environments, and there is ample evidence that leaders' responses in a crisis matter for electoral outcomes; for example, during the Ebola pandemic (e.g., Maffioli, 2021; Campante et al., 2024), after a natural disaster (e.g., Healy and Malhotra, 2009) 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; 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).[10]

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 both female and male leaders respond to electoral incentives, they do so differently due to the presence of voters' bias.

The remainder of the paper is organized as follows. Section 2 presents the setting and data, and Section 3 describes the sample and empirical strategy. We present the results showing gender differences in the evolution of COVID-19 deaths and policies in Section 4. Section 5 investigates the mechanisms, and Section 6 concludes.

2 Setting and Data

2.1 The COVID-19 Pandemic in Brazil

The authorities announced Brazil's first COVID-19 case on February 26, 2020, followed by the first death three weeks later, on March 17. Over the course of 2020, both the severity of the pandemic and public perceptions of its gravity evolved substantially.

Although Brazil ultimately became one of the countries most severely affected by the crisis, COVID-19 mortality emerged relatively late compared to other countries, such as the

[10]More broadly, our study also relates to a recent body of work studying the economic consequences of government responses to the COVID-19 crisis. Using real-time private-sector data, Chetty et al. (2024) show that stimulus payments increased spending, particularly among low-income households, supporting the view that targeted transfers can both stabilize the economy and mitigate inequality (Bayer et al., 2023). Turning to the effects of containment policies, Baek et al. (2021) find that stay-at-home orders accounted for roughly one quarter of unemployment insurance claims filed during the first months of the pandemic.

United States and the United Kingdom (Appendix Figure A1). By the end of April, most Brazilian municipalities had not recorded any COVID-19 deaths (Appendix Figure A2). Moreover, Brazil's president publicly downplayed the severity of the epidemic, fueling public skepticism (Ajzenman et al., 2023). An IPSOS survey conducted in early April found that 85 percent of Brazilian respondents expected life to return to normal by June, well above the global average of 57 percent and the respective 46 and 27 percent in the US and the UK, respectively (Ipsos, 2020a).[11] The first months of the pandemic were therefore marked by a low death toll and limited perceived severity.

The consequences of the pandemic became far more salient following the first wave. Beginning in May, the virus spread exponentially across the country, accompanied by a sharp rise in mortality (Roser et al., 2020). By the end of that month, seven out of ten Brazilians opposed the reopening of non-essential businesses (Ipsos, 2020b). From June through August, Brazil recorded the highest number of weekly deaths worldwide. By the end of August, cumulative deaths in Brazil had surpassed those in the United Kingdom and were second only to those in the United States.

The daily number of deaths declined between August and November before rising again toward the end of the year. The final two months of 2020 marked the onset of a second wave, which ultimately proved even deadlier than the first. By the end of our period of analysis (January 2021), daily deaths had returned to levels comparable to the peak of the first wave, and Brazil had recorded more than 224,000 cumulative COVID-19 deaths.

2.2 Brazilian Local Governments

Brazil is divided into 5,570 municipalities, the lowest subnational government tier in the country.[12] 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).

[11]While we would ideally want to measure voters' perception of the crisis using more granular survey data at the municipal level, to the best of our knowledge, such data are not available over this period.

[12]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.

The constitution recognizes municipalities as "federal entities," granting them the status of autonomous governments with the authority to determine local policies independently. Municipal governments have an executive branch presided by the mayor (*prefeitura*) and a legislative branch (*câmara municipal*). 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 enactment of laws and decrees.

Local governments were key actors in the response to the pandemic in Brazil. At the onset of COVID-19, the national congress reaffirmed municipalities' power to implement containment measures (Law Nº 13.979), and the Brazilian Supreme Court further clarified that the federal government could not overrule subnational health policies (Decision ADPF 672), leaving mayors with broad scope to enact containment policies in their municipalities.

Nationwide containment policies were limited: the federal government did not impose restrictions on internal mobility or gatherings and instead focused on border restrictions and economic and fiscal support. The federal government introduced a large emergency cash transfer program ("Auxílio Emergencial") targeted to low-income and informal workers (De Leon et al., 2023). It also provided additional fiscal assistance to states and municipalities – through direct transfers and debt relief later formalized under Complementary Law No. 173/2020. These allocations were largely based on population size and mostly non-earmarked (World Bank, 2020).

Due to the absence of national containment measures, 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 B2 provide more details about containment policies across municipalities and over time.

2.3 Local Elections

In Brazil, voting is mandatory for adults between the ages of 18 and 70. Mayors are elected by popular vote every four years. 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.[13] Since a constitutional amendment introduced in 1997, mayors are subject to a

[13] Local legislators are elected at the same time as mayors, using an open-list proportional system.

two-term limit, meaning that mayors serving a second term cannot run for re-election.

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,[14] and the new mayors took office on January 1, 2021. Our period of analysis spans the year 2020 – the last year of the mayor's term – through the end of January 2021.[15]

Female representation 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).[16]

2.4 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

[14]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.

[15]We start our analysis of containment policy in March 2020, when municipalities in our sample started implementing them, and we begin our analysis of COVID-19 deaths in April 2020, when municipalities in our sample started experiencing fatalities. We include the first month of the new municipal administration at the end of our period of analysis, as COVID-19 deaths tend to materialize a few weeks after infection, implying that people who died from the disease in January 2021 likely became infected while the prior mayor was still in office.

[16]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).

as well as of the spread of other major pandemics (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. 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.[17]

Containment policies. We built a novel policy dataset based on publicly available municipal legislation documents. 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.[18] 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 enacted over our period of interest. In particular, while it was more common to find a dedicated

[17]As discussed in more detail in Chauvin (2024), 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.

[18]In contrast, prior studies have relied on data from a survey conducted by the Brazilian Confederation of Municipalities between May and July, 2020 that asked mayors what policies they had implemented so far (de Souza Santos et al., 2021).

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 over 10,000 inhabitants, accounting for 50 percent of our sample.[19]

We consider 10 containment policies, which we defined in line with the international policies featured in the Oxford COVID-19 Government Response Tracker (Hale et al., 2021): commerce restrictions (closing non-essential businesses), curfews, event cancellations, face mask mandates, restrictions on gathering, lockdowns, school closures, workplace restrictions, transport restrictions, and travel restrictions. Four of these policies (school closures, event cancellations, face masks mandates, and restrictions on gathering) were enacted by the vast majority of municipalities (Appendix Table B2) and consistently throughout our period of analysis (Appendix Figures B1 and B2), providing little variation with which to identify the effects of interest. We therefore focus our analysis on the remaining six policies. See Appendix B2 for a more extensive discussion of the policy data.

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 B3 for further details). For each candidate in each municipality, we know their 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 to increase the precision of our estimates by including them as controls.[20] Most of these baseline variables are constructed directly from the microdata of the 2010 demographic census (the last one before the 2016 elections).[21] We made sure to include variables that have been shown to

[19]We follow the procedure introduced by Chauvin et al. (2020), who conclude that policy data could be credibly recovered only for municipalities above this conservative threshold. Note that we could not find *any* policy document for 24 municipalities, among which only four have over 10,000 inhabitants. We consider them as missing.

[20]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.

[21]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.

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, 2024). We further include the number of health facilities and hospital beds available in the municipality as of December 2016, just before the new mayor enters into office, using data from the Brazilian Ministry of Health's Cadastro Nacional de Estabelecimentos de Saúde (CNES, 2021).

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.[22] 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.[23]

We further exclude municipalities where the impact of local government actions on COVID-19 outcomes is confounded by neighboring municipalities. Specifically, we exclude 15.9 percent of municipalities with mix-gender elections that are part of a commuting zone (*arranjos populacionais*), as defined by the Brazilian Institute of Geography and Statistics (IBGE, 2016). Commuting zones consist of groups of municipalities linked by commuting flows and that often coordinate on urban services such as transportation and health facilities. Although containment policies are formally decided at the municipal level, we expect substantial spillovers within these zones, including coordinated policy decisions and virus transmissions across municipalities due to commuting patterns. These spillovers make it challenging to attribute COVID-19 outcomes to a municipality's own mayor.

Our final sample consists of 1,014 municipalities. As shown in Appendix Figure A3, 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.

[22]For elections that took place in two rounds, we consider the results of the second round. This concerns only two municipalities in our final sample.

[23]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 (Appendix D). See Appendix B3 for more details on the electoral data cleaning.

Summary statistics. 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 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 18,029 inhabitants on average in 2010; the average monthly median household income per capita was 324 reais (184 US dollars at the contemporary exchange rate); and 2.7 candidates ran in the 2016 elections on average.

Table 1: Descriptive statistics

	Mean	Sd	Min	Max	N
Panel A	*Socio-demographic characteristics*				
Population	**18,029**	35,441	1,037	784,610	1,014
Density	**157.8**	377.6	0.0	8031.1	1,014
Average persons per room	**0.704**	0.242	0.435	4.282	1,014
Commuting time	**21.61**	4.55	9.03	44.59	1,014
Share of population $\geq$ 65 years old	**0.082**	0.023	0.022	0.179	1,014
Nursing home residents per 10k wa pop	**4.091**	12.658	0.000	252.493	1,014
Health facilities per 10k pop	**12.67**	6.61	2.14	48.84	1,014
Hospital beds per 10k pop	**15.30**	17.09	0.00	110.52	1,014
Area	**1,956**	7,353	27	160,372	1,014
Distance to São Paulo	**1,447**	745	49	3,441	1,014
Km to airport connecting to COVID hot spots	**303.2**	220.3	0.0	1556.9	1,014
Median household income p/c	**324.0**	146.4	80.0	836.5	1,014
Informality rate	**0.168**	0.054	0.036	0.418	1,014
Unemployment rate	**0.044**	0.021	0.000	0.173	1,014
College graduate employment share	**0.069**	0.031	0.005	0.205	1,014
Black and mixed-race population share	**0.599**	0.215	0.019	0.952	1,014
Agriculture employment share	**0.413**	0.155	0.024	0.814	1,014
Evangelical share of population	**0.159**	0.092	0.009	0.838	1,014
Panel B	*Political characteristics*				
Turnout	**0.854**	0.060	0.673	0.980	1,014
Number of candidates	**2.688**	0.967	2.000	9.000	1,014
President's vote share	**0.324**	0.188	0.025	0.808	1,014

Notes: The sample includes municipalities outside of commuting zones and where one man and one woman were the two front-runners in the 2016 election. Socio-demographic variables come from the 2010 census. Density – defined as the total population living within 1 km of the average inhabitant of the municipality – is computed using the 2015 data from the Global Human Settlement Layer. The number of health facilities and hospital beds are measured as of December 2016 and come from the Ministry of Health's CNES dataset. 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.

Representativeness. Appendix Table A1 compares our sample to the rest of the country. Because we exclude municipalities part of a commuting zone, municipalities in our sample are on average smaller and less dense than the average Brazilian municipality. Still, 59 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.

Our sample is also representative of the evolution of COVID-19 in Brazil. Appendix Figure A4 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.

Finally, Appendix Table B2 presents the share of municipalities that enacted 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. As in the random sample of municipalities (first two columns), around 90 percent of municipalities in our sample enacted 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.

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:

$$Y_i = \alpha + \tau T_i + \beta_1 X_i + \beta_2 X_i T_i + \gamma W_i + \mu_i \, , \tag{1}$$

where i indexes municipalities and W_i is a vector of municipal controls. We include all municipalities' characteristics listed in Table 1 as controls, in order to increase the precision of our estimates. Appendix D shows the robustness of our main results to not including any control, to controlling for winners' characteristics, and to adding state fixed effects.

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 (Appendix D). Finally, we follow Calonico et al. (2017) when presenting the RDD 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 RDD point estimator.

The parameter of interest τ captures the local average treatment effect (LATE) in close elections. As shown in Appendix Table A1, 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. Moreover, Appendix Table A2 shows that race closeness – and thus inclusion in our analysis sample – is not predicted by baseline municipal characteristics. Finally, municipalities are equally distributed around the threshold, with 51 percent of municipalities close to the threshold electing a female mayor (right of the discontinuity), and 49 percent electing a male mayor (left of the discontinuity).[24]

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.

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 C1 and C2, 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 (Anagol and Fujiwara, 2016). We proceed as follows: we first regress the treatment variable T on all 21 baseline variables presented in Table 1, we then predict the treatment status of each municipality using the regression coefficients, and we finally test for a jump in the predicted value at the discontinuity. As shown in Appendix Table C1 and Appendix Figure C3, the point estimate is small and not significant.

[24] 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.

We also test for a jump in each of the characteristics taken individually (Appendix Table C2 and Figure C4). Only one coefficient out of 21 turns out significant (at the 5 percent level). Consistent with Appendix Figure A3, municipalities close to the threshold are balanced in their distance to São Paulo or to the nearest airport, confirming the absence of geographic sorting. They are also balanced in 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 were equally likely to have voted for Jair Bolsonaro, the president in office during the COVID-19 outbreak, in the 2018 presidential election, and 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.

Finally, Appendix Tables C3 and C4 show balance tests on detailed age brackets (splitting further the above-65-years-old category) and on additional labor market characteristics. All coefficients are close to 0, with only one coefficient out of 14 significant at the 10-percent level. Notably, municipalities at the threshold are balanced in the service sector employment share and in the gender gap in labor force participation.

3.3.2 Characteristics of the Winner

Next, we assess whether female candidates closely defeating male candidates differ from male candidates closely defeating female candidates in attributes other than gender. Table 2 tests for a jump at the threshold in the following characteristics of the winner: incumbency status, age, race, education, occupation, and political orientation.[25]

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 and with the presence of voters' bias, the coefficient on education suggests that closely-elected female mayors are more likely to have completed higher education (Column 4 of Table

[25] 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).

2 and Appendix Figure C4), even though the effect is not significant. 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. We observe a similar pattern when looking at *all* 2016 candidates: female candidates are more likely to have completed higher education, while they are very similar to the average male candidate in terms of age, race, incumbency status, and political orientation (Appendix Table A3).

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	Occupation				Ideology	PMDB	PSDB	PT
				education	Politics	Public	Health	Business	score			
Female	-0.060	-0.635	0.127	0.111	-0.035	0.036	-0.027	0.004	0.073	0.034	-0.012	0.009
	(0.071)	(1.648)	(0.073)	(0.097)	(0.073)	(0.057)	(0.048)	(0.050)	(0.058)	(0.061)	(0.049)	(0.033)
R. p-value	0.404	0.817	0.113	0.515	0.643	0.667	0.728	0.902	0.337	0.734	0.919	0.730
Observations	677	628	587	417	591	534	601	631	678	534	586	550
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	0.155	0.142	0.131	0.086	0.132	0.120	0.135	0.143	0.155	0.120	0.130	0.124
Mean	0.267	49.269	0.646	0.472	0.228	0.107	0.113	0.099	0.213	0.145	0.090	0.023

Notes: The outcome in column 1 (resp. 3, 4, 5, 6, 7, 8, 10, 11, 12), is a dummy equal to 1 if the winner is the incumbent (resp. white; has completed higher education; works in politics, the public sector, the health sector or is a business owner; runs under the PMDB, PSDB, or PT party label). In Column 2, the outcome is the winner's age at the time of the election. In Column 9, the outcome is the ideological score of the candidate's party, 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. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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.

One concern for the interpretation of our results is that the "compensating attributes" of positively-selected female politicians may affect our outcomes of interest independently from gender. Three pieces of evidence suggest, however, that this is unlikely to explain our results. First, as shown in Appendix D, the results remain unchanged when controlling for all winners' characteristics displayed in Table 2, including education and other characteristics likely correlated with ability, such as political experience. Second, we run a separate regression discontinuity analysis in which we focus on male candidates and compare municipalities led by mayors with or without higher education; we find no significant differences in the evolution of COVID-19 deaths (Appendix Table A4). Finally, the heterogeneity analyses in Appendix Tables E2 to E4 show that the effects do not systematically vary based on the mayor's education level, age, past experience, or ideology.[26]

[26]For each analysis, we focus on non-term-limited mayors – those who drive the effect (see Section 5) –

3.3.3 Robustness tests

To further support our identification strategy, we conduct a series of robustness checks on our main outcomes, which we summarize here. The corresponding tables and figures are reported in Appendix D.

First, we assess the sensitivity of our results to alternative sets of controls and to the inclusion of state fixed effects (Appendix Table D4). All estimates remain large in magnitude and statistically significant. Second, we show that our findings do not hinge on RD specification choices: point estimates remain stable when using a quadratic specification (Appendix Table D5) and across a wide range of bandwidths (Appendix Figure D3). Third, our results are virtually unchanged when we impose a common bandwidth across outcomes, rather than using data-driven bandwidths that vary by outcome (Appendix Table D6). Finally, we conduct several tests to ensure that our estimates are not driven by a subset of observations near the cutoff. Appendix Figure D4 presents jackknife estimates excluding one municipality at a time, while Appendix Figure D5 reports donut estimates that exclude observations within 0 to 2 percent of the estimation bandwidth. Across all exercises, the point estimates remain very similar.

4 Gender differences in crisis response over time

4.1 COVID-19 Deaths

In this section, we use the empirical strategy described above to assess the impact of having a female mayor on our first and main outcome: the number of COVID-19 deaths.

To examine gender differences throughout 2020, we partition the data in different ways: daily cumulative number of deaths, monthly total deaths, and total deaths in four distinct periods. We consider the latter as our main specification, as it enables us to summarize the patterns with four estimates, which we then use to conduct heterogeneity analyses. We define our four periods based on the evolution of COVID-19 deaths in Brazil (see Appendix Figure A4): 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).[27] We normalize the number of deaths by

and we split the sample in two: at the median for age and ideology, and based on whether the mayor has higher education or not, or has served as municipal councilor before or not.

[27]Note that having a female mayor did not affect the timing at which municipalities started to experience

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: 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
Female	0.439***	0.055	-0.245	-0.964**
	(0.163)	(0.429)	(0.257)	(0.368)
Robust p-value	0.008	0.883	0.344	0.023
Observations	482	544	711	498
Polyn. order	1	1	1	1
Bandwidth	0.104	0.124	0.169	0.107
Mean, left of threshold	0.220	2.544	1.398	2.410

Notes: The outcome is the number of deaths per 10,000 inhabitants 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. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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.

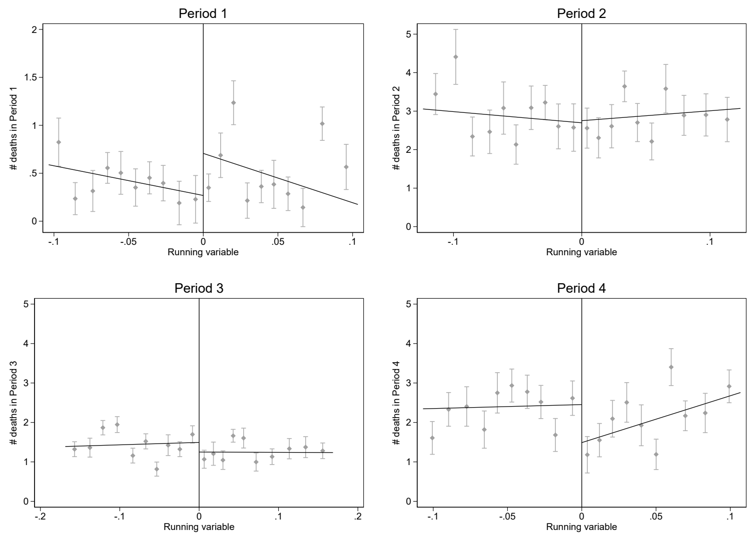
Table 3 shows that, on average, having a female mayor led to a 0.44 increase in the number of deaths per 10,000 inhabitants in the first period, a coefficient significant at the one-percent level. This corresponds to a threefold increase compared to the average number of deaths in male-led municipalities at the threshold. Conversely, we find that female-led municipalities experienced, on average, 0.96 fewer deaths per 10,000 inhabitants in the last period. This effect is significant at the five-percent level and corresponds to a 40.0-percent decrease compared to male-led municipalities. We find no significant effect during the second and third periods – the middle and end of the first wave. The point estimates are nevertheless consistent with a reversal of the effect as the crisis unfolds, as the coefficient drops to virtually 0 in period 2 before turning negative in period 3.

Figure 1 plots the number of deaths against the running variable for each period separately. Each dot represents the average value of the outcome within a given bin of the running variable, and each bin contains approximately 25 municipalities. Consistent with the formal estimation, we see an upward jump at the threshold at the beginning of the first

fatalities (Appendix Table A5 and Figure A5), so that we can use the same time frame to study the evolution of COVID-19 deaths in female- and male-led municipalities. We start in April, as no death occurred in municipalities in our sample in March (only 201 COVID-19 deaths occurred across the country.)

wave that turns into a downward jump later in the year.

Figure 1: Impact on COVID-19 deaths, by period

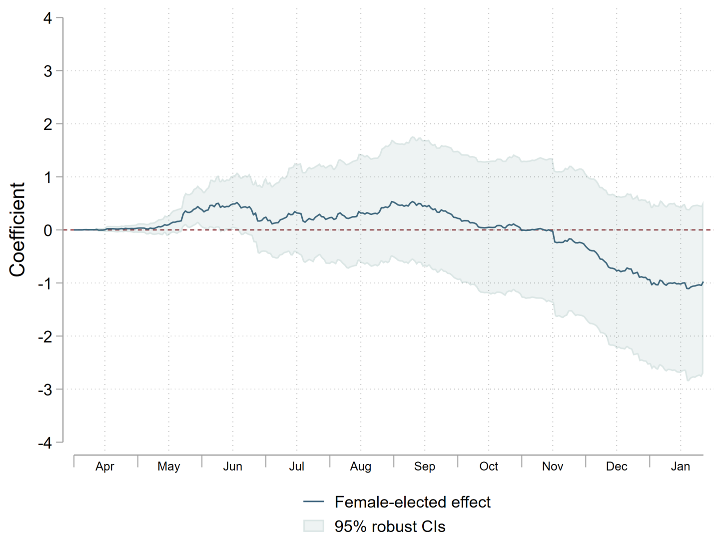


Notes: Each graph is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the number of COVID-19 deaths per 10,000 inhabitants in the municipality during the period of interest. Averages are calculated within quantile spaced bins of the running variable. Vertical lines represent 95-percent robust confidence intervals. The running variable is the percentage-point difference between the vote shares of the female and male candidates in the 2016 election. Positive (negative) values denote that the female (male) candidate won. All municipal characteristics presented in Table 1 are included as controls. 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).

Zooming in month by month, Appendix Table A6 and Figure A6 show 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.

Finally, we look at how these effects translate into the evolution of the number of cumulative deaths. Figure 2 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. We see that the magnitude and eventually the sign of the coefficients change along the period. 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 remain 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, the point estimates become negative starting in November.[28]

Figure 2: Impact on cumulative number of COVID-19 deaths: Daily estimates



Notes: This figure plots the RDD 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.

A key concern is whether the late-period effect reflects an independent impact or is driven by earlier differences in mortality. In principle, higher initial mortality could reduce subsequent deaths through mean reversion, either because immunity accumulates or because the most vulnerable individuals have already died. Early mortality differences could also affect perceptions and policy demand, indirectly shaping outcomes in subsequent periods. We view these mechanisms as unlikely. The early effect emerges when infection levels were still very low. It is therefore implausible that the early differences generated meaningful immunity or persistent changes in perceptions and policy demand. Moreover, the brunt of the first wave unfolded between the early and late periods, when municipalities experienced record-high mortality, with no differences between female- and male-led municipalities (periods 2 and 3, Table 3). Hence, both types of municipalities entered period 4 with the same number of cumulative deaths, and thus likely the same perceptions of the crisis severity.

We provide two additional pieces of evidence supporting the independence of the early and late effects. First, mortality at the beginning of the year is uncorrelated with

[28]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 (14.3 percent), on average, but the coefficient is not statistically significant (Appendix Table A7 and Appendix Figure A7).

both mortality and policy responses at the end of 2020 (Appendix Figures A8 and A9). Second, our results are virtually unchanged when controlling for cumulative past deaths (Appendix Table A8). Taken together, these findings indicate that the gender of the mayor independently affected mortality outcomes at both the beginning and the end of the year.

As municipalities on either side of the threshold differ only in the mayor's gender, these results suggest that female mayors handled the crisis differently than male mayors, and in opposite directions over time. To further support this interpretation, we now investigate the impact of female leadership on containment policies.

4.2 Containment Policies

As discussed in Section 2.4, our policy analysis focuses on municipalities with over 10,000 inhabitants from March through October 2020, and on 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.

We begin by examining gender differences in the initial response to COVID-19. We consider two measures: the first day at which any of the six containment policies was implemented, and the probability that at least one policy was implemented by the end of April. As shown in Table 4, female mayors started implementing policies 23 days later than male mayors on average, and female-led municipalities were 35.6 percentage point less likely to have a policy in place early on, compared to a baseline of 79.9 percent for male-led municipalities at the threshold.

We next investigate the implementation of each of the six policy over time. Figure 3 shows the estimated impact of having a female mayor on the probability that the policy was in place in the municipality day by day. The daily effects on commerce restrictions stand out: we see a stark reversal, with large and significant negative estimates at the beginning of the year, and large and significant positive estimates at the end of the period, showing that female mayors were significantly less likely to close non-essential businesses early in the pandemic but became significantly more likely to do so later on. The daily estimates on the other policies are generally small and relatively stable over the period of analysis.

Our policy results are thus mainly driven by gender differences in the use of commerce restrictions. This is not surprising, as Brazilian mayors used this policy extensively and with a lot of flexibility over time. Closing non-essential businesses was one the first policies enacted and more than two-thirds of the municipalities in our sample implemented it at some point in 2020. Mayors oftentimes reverted and reinstated the policy, making their decisions particularly likely to be influenced by time-varying electoral incentives. Indeed, among municipalities that closed non-essential businesses, more than 20 percent did so multiple times over the period of analysis. This was commonly referred to in the media as the "open-close policy".[29]

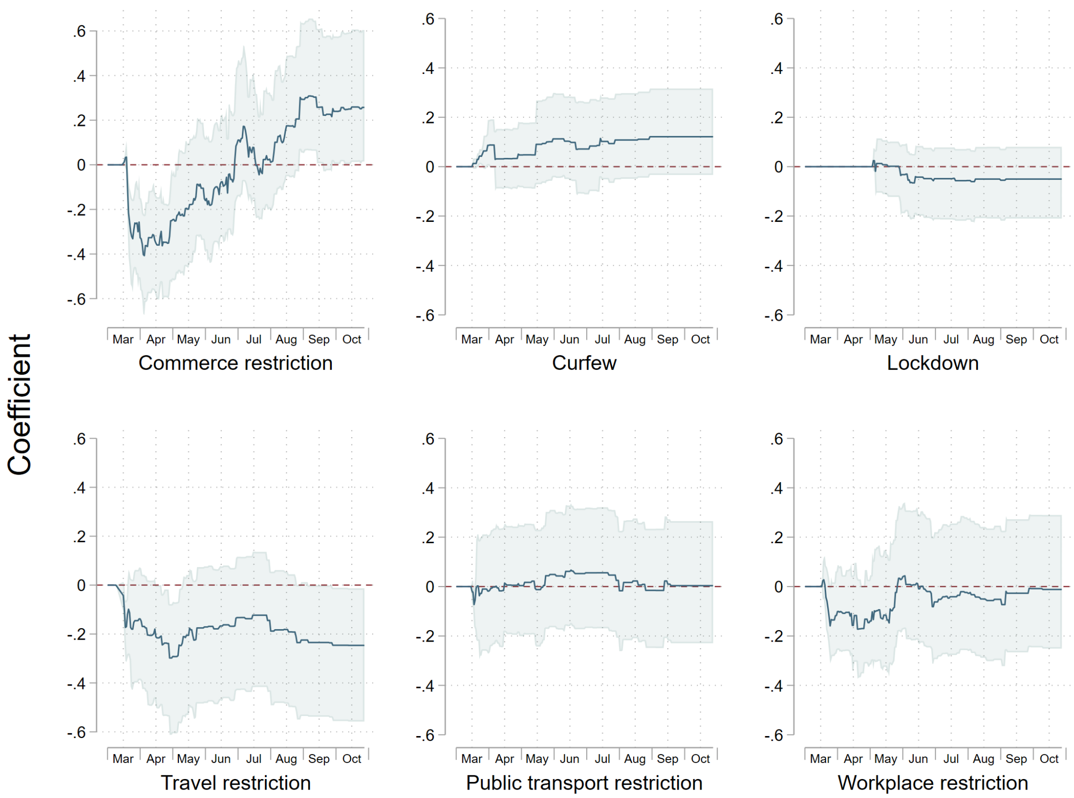
Table 4: Impact on mayors' initial policy response

	(1)	(2)
Outcome	Any policy	
	First implementation day	At least one by the end of April
Female	22.900**	-0.356***
	(9.564)	(0.116)
Robust p-value	0.020	0.004
Observations	239	274
Polyn. order	1	1
Bandwidth	0.114	0.112
Mean, left of threshold	96.016	0.799

Notes: The sample is restricted to municipalities with over 10,000 inhabitants. The outcome is the number of days between December 31, 2019, and the first day on which the municipality enacted any of the six policies (Column 1), or the probability that at least one policy was enacted at some point by the end of April 2020 (Column 2). All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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.

[29]See, for instance, discussions of the repeated closure of non-essential businesses in interviews with epidemiologists and private sector leaders. Other policies, in contrast, were used less flexibly. Curfews or lockdowns, the most extreme restrictions, were implemented by very few municipalities over the year 2020 (Appendix B2). Public transport and workplace restrictions were implemented by less than a third of municipalities, and almost all municipalities that implemented these policies at some point did so only once. Finally, while travel restrictions were implemented more frequently, they mainly restricted the mobility of non-residents, making them less likely to be affected by voters' preferences and thus electoral incentives.

Figure 3: Impact of having a female mayor on policies: Daily estimates



Notes: The sample is restricted to municipalities with over 10,000 inhabitants. This figure plots the daily estimated effect of having a female mayor on an indicator equal to 1 if the policy was enacted on that day. Blue shaded areas represent robust confidence intervals.

To further characterize the gender differences in the use of commerce restrictions, we next look at the impact on the number of days in which non-essential businesses were closed in the municipality, month by month. As shown in Table 5, on average, non-essential businesses were closed 2.8 and 10.4 fewer days in female-led municipalities in March and April, respectively, compared to an average of 4.2 and 12.8 days in male-led municipalities at the threshold. These effects are significant at the one-percent level and are driven by the fact that female mayors started closing non-essential businesses 53 days later on average (Appendix Table A9, Column 1). Instead, non-essential businesses were closed on average 8.1 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-percent level. These effects appear to be driven by female mayors being less likely to lift commerce restrictions at the end of the first wave, as they were 13.3 percentage points less likely to reopen non-essential businesses between August and

October 2020 (Appendix Table A9, Column 2).

Appendix Figure A10 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.

The timing of the policy results aligns well with the evolution of the number of COVID-19 deaths: female mayors were less likely 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, 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.

Table 5: 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
Female	-2.844***	-10.371***	-5.674	-3.168	0.122	3.777	8.070**	7.619**
	(0.885)	(2.613)	(3.389)	(3.365)	(3.083)	(3.248)	(4.048)	(4.069)
Robust p-value	0.003	0.000	0.189	0.528	0.758	0.182	0.032	0.042
Observations	315	257	271	266	286	279	208	213
Polyn. order	1	1	1	1	1	1	1	1
Bandwidth	0.137	0.104	0.110	0.106	0.124	0.116	0.079	0.082
Mean, left of threshold	4.156	12.839	12.758	12.596	11.884	8.975	8.621	8.142

Notes: The sample is restricted to municipalities with over 10,000 inhabitants. The outcome is, for each month, the number of days during which the policy was in place. The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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.

5 Mechanisms

In this section, we study the role of electoral incentives in shaping the gender differences documented above. We first show in Section 5.1 that the reversal can be rationalized by a model where politicians seek re-election, voters attribute less credit to female politicians, and voters' perception of the crisis varies over time. Section 5.2 then provides empirical support for this interpretation by exploiting variations in electoral incentives. Finally, in Section 5.3, we discuss alternative interpretations and provide further evidence supporting the electoral incentives interpretation.

5.1 A Simple Model of Political Agency With Voter Bias

Public Good

Society is a representative democracy made up of a unit mass 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 $\psi > 0$ that threatens to reduce the public good. The politician has access to a policy $0 \leq P \leq 1$, 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.[30]

The amount of the public good that will be available after the shock is given by:

$$g = \overline{g} - \overline{g}\, f_1(\psi_m) \; , \tag{2}$$

where $\psi_m > 0$ represents the size of the "mitigated" shock (i.e. after the policy intervention), and where the damage function $f_1 : \mathbb{R}_+ \to [0, 1]$ is defined as:

$$f_1(\psi_m) = 1 - exp(-\psi_m) \; . \tag{3}$$

The concavity of the damage function implies a decreasing marginal effect of the shock on the public good. This assumes that increases in the severity of the crisis generate relatively less additional damage at high levels of the shock than at low levels. This assumption is aligned with the Susceptible-Infected-Recovered (SIR) model of disease

[30] In our context, P could be interpreted as the number of days with business closures. Indeed, as stressed in Section 4.2, Brazilian mayors used this policy extensively and with a lot of flexibility over time.

spread (Kermack and McKendrick, 1927): as a larger share of the population becomes infected, a smaller proportion remains susceptible to new infections.

The size of the shock after the policy intervention depends, in turn, on the original severity of the shock ψ and on the policy level P, according to:

$$\psi_m = \psi\,(1 - f_2(P))\,, \tag{4}$$

where the abatement function $f_2 : \mathbb{R}_+ \to [0, 1]$ is defined as:[31]

$$f_2(P) = 1 - \exp(-P)\,. \tag{5}$$

The concavity of the abatement function implies that the policy intervention has decreasing marginal returns. This assumes that increasing containment efforts has larger benefits when little has been done in response to the crisis than when there are already sizeable containment measures in place. This can be rationalized, for instance, by the fact that additional restrictions create compliance fatigue, reducing their marginal impact.[32]

Combining equations 2 to 5, the level of public good available can be written as a function of the original size of the shock ψ and the policy level P, namely:

$$g = \overline{g}\ \exp(-\,\psi\ \exp(-P)\,)\,. \tag{6}$$

Voters' Evaluation of the Politician's Actions

Voters observe the policy level chosen by the politician and make their electoral decisions based on the utility they attribute to these actions. This utility reflects both the perceived benefits and costs of the policy.

The benefits voters derive from P come from the amount of public good that they expect to enjoy due to the politician's actions, $\tilde{g}$. This is based on their subjective beliefs about (a) the likelihood of the shock and (b) how much of the shock mitigation can be attributed to the politician's policy decisions.[33]

[31]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). While we adopt an exponential functional form for f_1 and f_2 to obtain a tractable closed-form solution, we show in Appendix G that the main predictions of the model hold for any strictly concave functions satisfying standard regularity conditions.

[32]The same assumption is made in other models of crisis response. For instance Cohen and Werker (2008) model the government's response to natural disasters and assume decreasing marginal returns in their mitigation function.

[33]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

With respect to (a), voters assign a weight $0 \leq p \leq 1$ to the likelihood that the shock will materialize, such that $p\psi$ captures the perceived severity of the shock. Only when $p = 1$ do voters believe that the full potential of the shock (ψ) will materialize.[34]

With respect to (b), the mitigation effect of the policy P is augmented by how much credit voters give to the politician's action. Credit attribution is captured by the parameter $\lambda_s \geq 1$, which multiplies P. The larger λ_s, the higher the level of public good voters attribute to the politicians' actions. We assume that voters' credit attribution is gender biased, so that $\lambda_f < \lambda_m$. Hence, for the same policy, voters attribute less credit to a female politician than to a male politician and derive less utility from their actions. This formulation is consistent with recent evidence of gender attribution gaps in political and organizational settings (e.g., Egan et al., 2022; Sarsons, 2022). It is also in line with the "role incongruity" theory, according to which the traits required for effective leadership are perceived as inconsistent with the traits traditionally attributed to women. This makes voters unlikely to credit women as strong leaders, especially in crisis contexts, where "male" attributes are particularly valued (Eagly and Karau, 2002; Bertrand and Duflo, 2017).

Incorporating these two assumptions into equation 6, we obtain:

$$\tilde{g} = \overline{g} \ \exp(-\, p\, \psi \ \exp(-\lambda_s\, P)\,)\,. \tag{7}$$

Finally, we assume that policies impose a direct cost on voters by closing the economy and limiting freedom. Combining the costs and benefits, the utility that voters attribute to the politician's policy decisions is given by:

$$U \ = \ \tilde{g}\, \exp(-P) \ = \ \overline{g} \ \exp(-p\, \psi \ \exp(-\lambda_s P)\,) \ \exp(-P)\,. \tag{8}$$

Note that the disutility caused by the policy, $\exp(-P)$, enters equation 8 multiplying $\tilde{g}$ such that the larger $\tilde{g}$, the more disutility the policy generates. This is to capture voters' higher willingness to accept containment policies if the shock is perceived as more severe (higher p), in line with recent survey evidence showing that the willingness to sacrifice civil liberties increases with the perception of health insecurity (Alsan et al., 2023).

Equation 8 reflects the tradeoff that the politician faces when deciding on 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. Note that, while our model is motivated by the COVID-19 pandemic context, it can also apply to other crisis contexts involving policy

competing political narratives (Eliaz and Spiegler, 2020) and the recurrence of populism (Levy et al., 2022).

[34]The parameter 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.

interventions that can be costly to voters, such as fiscal austerity or environmental policies.

Optimal Policy and Comparative Statics

We assume that politicians seek to maximize their likelihood of re-election. Since that is an unobserved positive function of voters' utility, politicians optimize it by choosing the policy level that maximizes voters' utility (Equation 8).[35] This yields the following optimal policy equation:[36]

$$P_s^* = \frac{1}{\lambda_s} \log(\lambda_s p \psi) \ . \tag{9}$$

We are interested in how the optimal policy P_s^* varies with the politician's gender. Gender differences in the model come exclusively from the fact that voters attribute less credit to female politicians (i.e., $\lambda_f < \lambda_m$). We are thus interested in analyzing how P_s^* varies with λ_s. The optimal policy level is a non-monotonic function of voters' credit attribution: P_s^* is increasing in λ_s at low values of λ_s, while P_s^* is decreasing in λ_s at high values of λ_s. In turn, the support of λ_s over which P_s^* is decreasing in λ_s depends on voters' beliefs about the severity of the threat (p). Specifically, P_s^* is increasing in λ_s as long as $\lambda_s < \frac{e}{p\psi}$, and decreasing in λ_s when $\lambda_s > \frac{e}{p\psi}$. The threshold $(\frac{e}{p\psi})$ is a negative function of p, which implies that the higher the voters' perception of the threat, the larger the support of λ_s over which voters demand more policies from mayors to whom they attribute less credit.

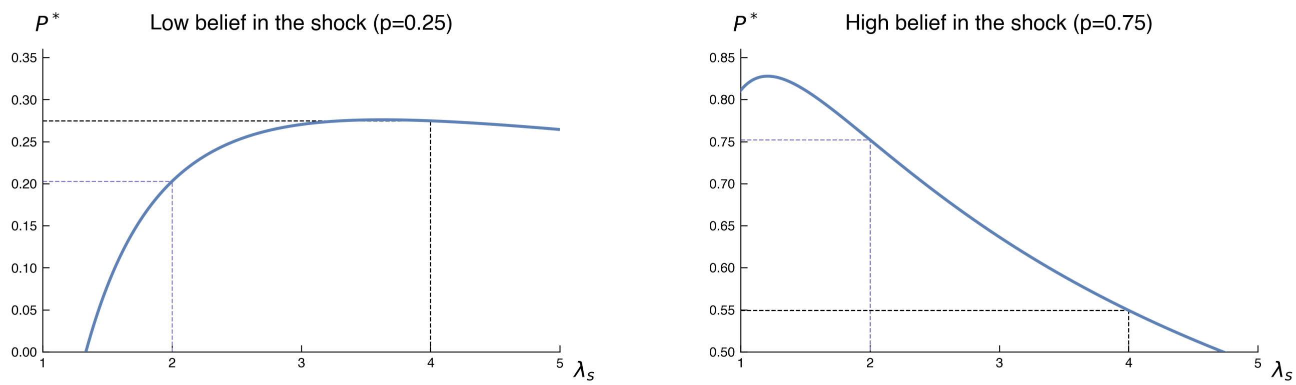
Figure 4 illustrates this relationship by plotting the optimal policy level as a function of λ_s in two scenarios: one in which voters believe that the shock is unlikely (low p), and one in which voters perceive the shock as likely (high p). We see that the relationship between P_s^* and λ_s flips across the two scenarios. When voters believe that the likelihood p of the 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 female politicians choose a lower level of policy than male

[35]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 their reservation utility. Their best strategy is therefore to choose the policy that delivers the maximum utility possible given voters' beliefs and the severity of the shock.

[36]The model also yields corner solutions, where $P_s^* = 0$ for low shock likelihood ($p \leq \frac{1}{\lambda_s \psi}$) and $P_s^* = 1$ for high shock likelihood ($p \geq \frac{\exp(\lambda_s)}{\lambda_s \psi}$). These boundary conditions represent the thresholds outside of which either no intervention or maximum intervention is warranted. We focus here on the interior solution, where $0 \leq P \leq 1$, which occurs whenever $\frac{1}{\lambda_s \psi} < p < \frac{\exp(\lambda_s)}{\lambda_s \psi}$.

politicians (as $\lambda_f < \lambda_m$). In contrast, when voters believe that the likelihood p of the 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 female politicians choose a higher level of policy.

Figure 4: Optimal policy (P^*) as a function of voters' credit attribution (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' credit attribution under two scenarios – one in which voters believe that the shock has a low likelihood of occurring ($p = 0.25$) and one in which voters believe it has a high likelihood of occurring ($p = 0.75$). We normalize the pre-crisis amount of the public good to $\bar{g} = 1$ and assume a shock of magnitude $\psi = 3$. The dashed lines illustrate the optimal policy chosen by a politician with low ($\lambda_s = 2$) versus high ($\lambda_s = 4$) credit.

Intuition

The reversal illustrated in Figure 4 comes from the joint effect of credit attribution and severity perception on the marginal utility of the policy. Consider first the role of credit attribution. When voters are generous in attributing credit (high λ_s), the first units of policy have a high marginal utility, but the benefit of further policy effort quickly diminishes due to decreasing marginal returns. This means that, while at low levels of policy the marginal utility is higher for male politicians, this eventually reverts, such that it becomes higher for female politicians at high levels of P. In other words, because voters give more credit to policies enacted by male politicians, there comes a point beyond which additional policies are less necessary for them than for female politicians.

Consider now the role of voters' belief about the crisis severity, which is captured by the perceived likelihood p of the shock ψ. When voters believe that the likelihood of the shock is low, the demand for policy is low. Politicians choose a level of policy up to the point where the marginal utility equals zero. This happens faster for female politicians because, as discussed above, at low levels of P, the marginal utility of policies is higher for male politicians. Instead, when voters believe that the threat is high, the demand for policy is high. At high levels of P, the marginal utility of additional policy effort remains positive over a wider range for female politicians. This is illustrated in Appendix Figure A11, which depicts the marginal utility of the policy as a function of the policy level P and the perceived severity p, for female and male politicians separately.

Intuitively, when voters believe that the threat to the public good is low, female politicians find it optimal to do even less than male politicians, since their actions are undervalued and voters are unwilling to bear high policy costs. In contrast, when voters believe that there is a significant threat, they are more willing to bear the disutility associated with the policies to preserve the public good. In this case, female politicians need to enact more containment policies than their male counterparts to compensate for the relative lack of recognition for their actions. In other words, female politicians are more likely to be penalized for both "doing too much" – when the crisis is not perceived as serious – and "doing too little" – when the crisis is thought to be severe.

Discussion

The model shows that we can reconcile the gender differences we find as a function of voter gender bias and voter perceptions of the crisis. This requires three key assumptions.

First, the link between the model and the empirical results assumes that the initial phase of the pandemic – during which female mayors implemented fewer policies (March–April 2020) – was characterized by low perceived severity (low p), and that, by contrast, perceived severity was high later in the year, when female mayors implemented more policies (September–October 2020). This characterization is consistent with the Brazilian context. As detailed in Section 2.1, by the end of April most municipalities had not yet experienced any COVID-19 deaths, and the federal government's stance toward the pandemic fostered widespread skepticism. Instead, following the deadly first wave that unfolded between May and August 2020, the crisis became markedly more salient.

Second, the model treats the two periods – the low- and high-p scenarios – as independent. Given the unprecedented nature of the crisis, it is unlikely that mayors made early policy decisions under low p anticipating how the pandemic would evolve. Accordingly, early policy choices are unlikely to have been influenced by conditions in the later period. Conversely, as argued in Section 4.1, the low number of deaths in the first months makes it unlikely that early mortality differences generated persistent changes in voters' perceptions that would have influenced policy decisions later in the year under high p. We therefore interpret the early and late periods – the low- and high-p scenarios – as two distinct states of the world in which politicians made independent policy decisions.

Finally, the model assumes that voter gender bias remains constant across the two periods. Indeed, we interpret voters' attribution bias as reflecting entrenched gender stereotypes that are unlikely to change over short time horizons. The presence of such deeply ingrained biases is consistent with recent evidence of taste-based discrimination against female candidates in Brazilian local elections (de Albuquerque et al., 2025).

5.2 Gender Differences and Electoral Incentives

We next test whether, as implied by the model, the gender differences in crisis response we observe are driven by mayors with electoral incentives. Indeed, the only parameter in the model that varies by gender is voters' assessment of policy choices, which matters only if the politician cares about re-election. We exploit the two-term limit and compare mayors who could run for re-election to those who could not.[37]

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 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 re-election 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 re-election, Table 6 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). Importantly, conditioning on the incumbency status of the ultimate winner is unlikely to create selection issues in our setting, given the null impact

[37]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 we have data on policies for half of the municipalities, making subsample analysis difficult. Moreover, given that municipalities at the threshold 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 indicators.

of having a female mayor 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.[38]

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. In period 1, having a female mayor increases the number of deaths by 0.58 in municipalities where the mayor can run for re-election, an effect significant at the one-percent level (Column 3). Instead, the point estimate is small, negative, and not significant in municipalities where the mayor is term limited (Column 5). The difference between the two estimates is statistically significant at the 5 percent level. In period 4, the result is similar in magnitude and significance as in the full sample for municipalities where the mayor can run again (-1.1, Column 4), whereas the effect is not significant and close to 0 when the mayor is term limited (Column 6), although the difference between the two coefficients is not statistically significant (p-value of 0.188).

Table 6: 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
Female	0.439***	-0.964**	0.581***	-1.063**	-0.084	-0.047
	(0.163)	(0.368)	(0.198)	(0.463)	(0.219)	(0.613)
Robust p-value	0.008	0.023	0.005	0.038	0.680	0.784
P-value (3)=(5)					0.026	
P-value (4)=(6)						0.188
Observations	482	498	347	376	113	116
Polyn. order	1	1	1	1	1	1
Bandwidth	0.104	0.107	0.096	0.105	0.099	0.100
Mean, left of threshold	0.220	2.410	0.163	2.780	0.327	1.500

Notes: In Columns 3 and 4 (resp. 5 and 6), the sample is restricted to municipalities where the mayor is not term-limited and thus allowed to run again in 2020 (resp. is term-limited and cannot run again). In columns 1, 3, and 5 (resp. 2, 4, and 6), the outcome is the total number of deaths per 10,000 inhabitants in period 1 (resp. in period 4). The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 of the outcome for male-led municipalities at the threshold.

[38]Appendix F presents the same balance tests as the ones described in Section 3.3, but performed in each subsample separately.

One concern could be that term-limited and non–term-limited mayors do not differ only in the electoral incentives they face. 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 previously, suggesting that less-able or less-experienced mayors are not driving the results (Appendix Tables E1 to E3). Finally, if differences in ability drive the heterogeneity we find between term limited and non-term-limited mayors, we should expect mayors with higher ability to handle the crisis differently, independently from electoral incentives. Instead, as mentioned in Section 3.3 and shown in Appendix Table A4, we find no differences in the evolution of COVID-19 deaths in municipalities led by mayors with or without higher education, whether we look at all mayors (Columns 1-4) or only at non-term-limited mayors (Columns 5-8).

5.3 Discussion of 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.

Policy preferences. Gender differences in COVID-19 responses could come from gender differences in policy preferences and priorities. 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 (Galasso 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. The same reasoning applies if, instead, we expect female mayors to be more reluctant to close the economy – for example, because they may place greater weight on the adverse effects of isolation on domestic violence (e.g., Leslie and Wilson 2020). This interpretation could account for the earlier effect but not for the later one, nor would it explain why these gender differences emerge only when mayors face electoral incentives.[39]

To provide further evidence that gender differences in policy preferences are unlikely to drive our effects, we examine gender differences in several municipal budget items in 2019 – the year before COVID-19 and corresponding to the third year of the municipal term.[40] As shown in Appendix Table A10, total spending per capita is balanced at the threshold, and female mayors spent the same amount per capita as male mayors in health, education, social assistance, urbanism, and the administration.[41] Moreover, we do not see gender differences in the budget balance, nor in the share of total revenue coming from federal transfers – two possible proxies of the mayor's effectiveness at managing the budget. This is true whether we consider all mayors, or only the non-term-limited ones who are driving our effects (Appendix Table A11). These results support the fact that female mayors did not have different policy priorities or abilities, and that their differential response to the COVID-19 were primarily driven by electoral incentives.

Risk aversion and competitive behaviors. Alternatively, the effect 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 apparent. This interpretation is also consistent with evidence suggesting that women adopt less aggressive strategic behavior, including a lower propensity to negotiate and to self-select into competition (Niederle and Vesterlund, 2007; Exley et al., 2020). However, if this was the main mechanism behind our results, the same time-varying patterns should hold for both term-limited and non-term-limited mayors.

Still, one could argue that female mayors act differently precisely when they are in a competitive environment (Gneezy et al., 2003). This could lead them to behave more cautiously only when their actions have electoral consequences – an interpretation consistent with electoral incentives but not requiring voters' bias. Several pieces of information speak

[39]Our results are also unlikely to be driven by female candidates being elected by different groups of voters with different preferences, and thus by female mayors catering to different electorates. 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 in Brazil, meaning that closely-elected female and male mayors faced the same electorate.

[40]We use the 2019 FINBRA data on municipal budgets, maintained by the Brazilian National Treasury's Public Sector Accounting and Fiscal Information System (SICONFI, 2025)

[41]These five categories constitute the largest components of municipal spending in Brazil. In 2019, they accounted, on average, for 83% of total municipal expenditures in our analysis sample.

against this interpretation. First, female mayors are balanced in their prior occupation (Table 2), which speaks against selection on risk or competitiveness aversion, to the extent that it is correlated with prior professional experience. Second, some evidence suggests that gender differences in risk aversion dissipate with education and among people in risk-taking careers (Croson and Gneezy, 2009). The fact that our findings are not driven by less-educated mayors (Appendix Table E1) and that we study individuals who self-selected into a highly competitive environment makes risk or competitiveness aversion unlikely to be the main driver of our results.

Instead, we interpret the results as female mayors acting in response to voters' bias. This interpretation is consistent with evidence suggesting that observed gender differences in risk aversion do not reflect higher intrinsic aversion among women, but rather women's expectations of negative consequences when deviating from gender stereotypes (e.g., Kawakami et al., 2007; Carr and Steele, 2010). To further probe this interpretation, we present suggestive evidence that the gender differences we document are more likely to emerge in contexts where voter gender bias is plausibly stronger. Although we cannot directly measure voters' differential evaluations of female mayors, we use two indirect proxies for gender discrimination at the municipal level: the gender wage gap, following Le Barbanchon and Sauvagnat (2022), and the share of past female councilors.[42] As shown in Appendix Table E5, the effects are primarily driven by municipalities with above-median gender wage gaps (Panel A, Columns 5–6) and with below-median shares of past female councilors (Panel B, Columns 3–4).

Institutional pushback. Beyond voters bias, female mayors might have faced more pushback from the national government, or, internally, from the municipal council. We believe this to be unlikely to explain our results. First, one would need to assume institutional constraints to be time-varying – preventing female mayors to implement policies early on but then pushing them to keep containment measures in place later on – and to be directed only toward non-term-limited mayors. Second, as stressed in Section 2.2, the federal government could not overrule the policies of local governments, following the decision of the Brazilian Supreme Court. Moreover, the analysis of the 2019 municipal budget suggests no differential treatment between female-led and male-led municipalities, as they received the same amount of federal transfers (Appendix Tables A10 and A11). Third, after an extensive online search, we only found a few examples of conflicts between the

[42]The gender wage gap is computed using all workers residing in the municipality, measuring the difference in wages between female and male workers after accounting for age, education, and occupation.

mayor and the municipal council. In all cases, the council was opposed to the containment policies implemented by the mayor but in none of those cases did they succeed to overrule them.[43] To further test whether pushback from the municipal council could contribute to our results, we exploit variations in the political alignment between the mayor and the council. If municipal councilors' actions are driving our results, we should expect larger effects in municipalities where the mayor has less control over the council. To test this, we focus on municipalities with non-term-limited mayors (those driving our effects) and compare municipalities where the share of seats going to the mayor's party or coalition is above vs. below the median. As shown in Appendix Table E6, the effects remain strong in municipalities above the median where the mayor has more control (columns 5 and 6).

6 Conclusion

This paper provides new evidence that electoral incentives and voters' gender bias can explain why female and male politicians make different decisions.

Our empirical analysis uncovers time-varying differences in how female mayors responded to the COVID-19 crisis in Brazil: female mayors exerted lower containment efforts at the beginning of the pandemic, but shifted to stricter measures later in the year as the crisis unfolded. Specifically, female mayors were less likely to impose commerce restrictions early on, and having a female mayor tripled the number of deaths in the first months of the pandemic. Later in the year, however, female mayors became more likely to close non-essential businesses, and having a female mayor led to 40 percent fewer deaths.

We show that this reversal can be rationalized by a simple political agency model in which politicians seek re-election, voters attribute less credit to female politicians, and perceptions of the crisis evolve over time. In line with the electoral incentives channel, our results are driven exclusively by non-term-limited mayors who can run for re-election.

Overall, our results suggest that observed gender differences in political behavior need not reflect intrinsic policy preferences, but can instead emerge from strategic responses to biased electoral incentives. Such differences are therefore most likely to arise in competitive elections and in policy domains that are highly salient to voters.

[43] See as an illustration the failed attempt by a councilor to stop the municipal lockdown in Rio de Janeiro (link).

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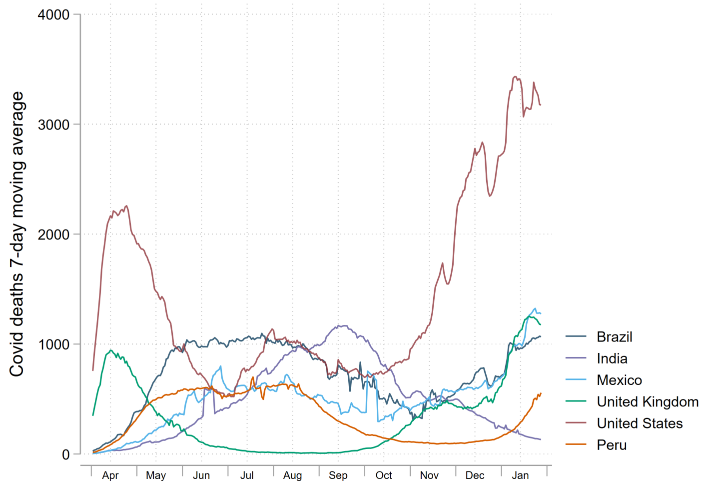
Online Appendix

Table of Contents

A Additional figures and tables

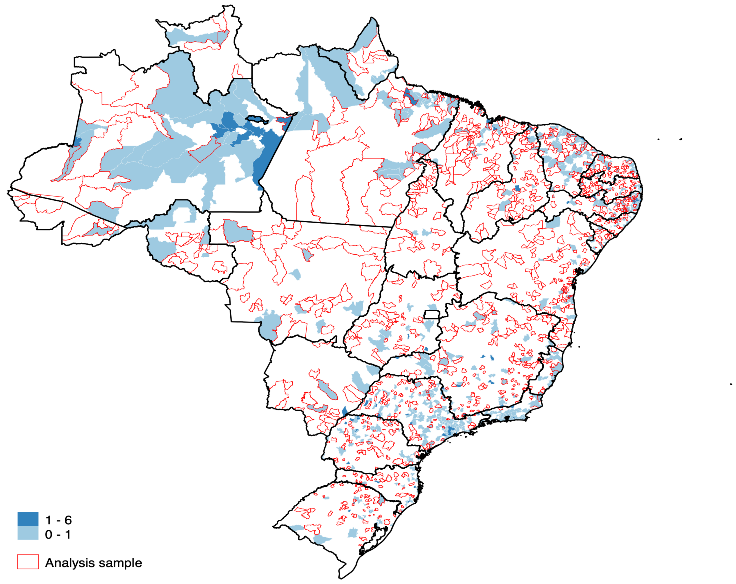
A1 Additional figures

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



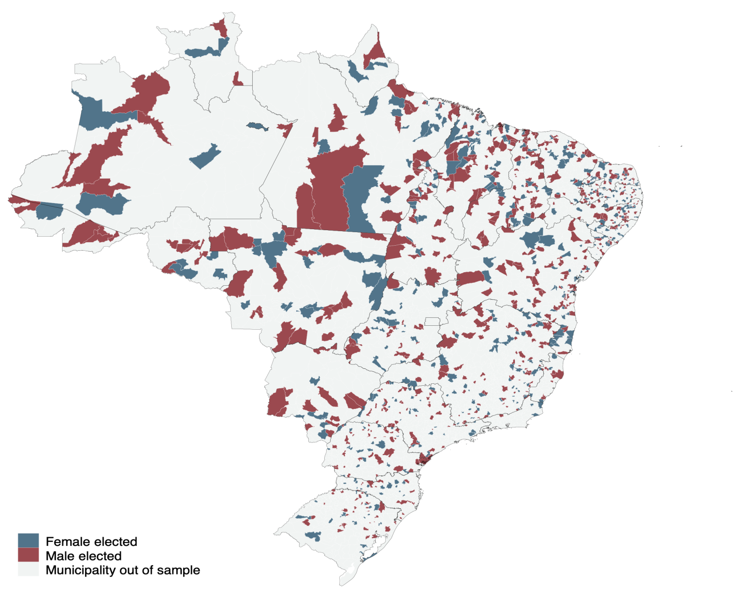
Notes: This figure includes the six countries with the highest number of COVID-19 deaths in the world as of January 31, 2021. It shows the number of COVID-19 deaths, smoothed using a 7-day moving average centered in the current day. Data from Our World in Data, accessed on June 23, 2021.

Figure A2: Total number of COVID-19 deaths in Brazilian municipalities by the end of April



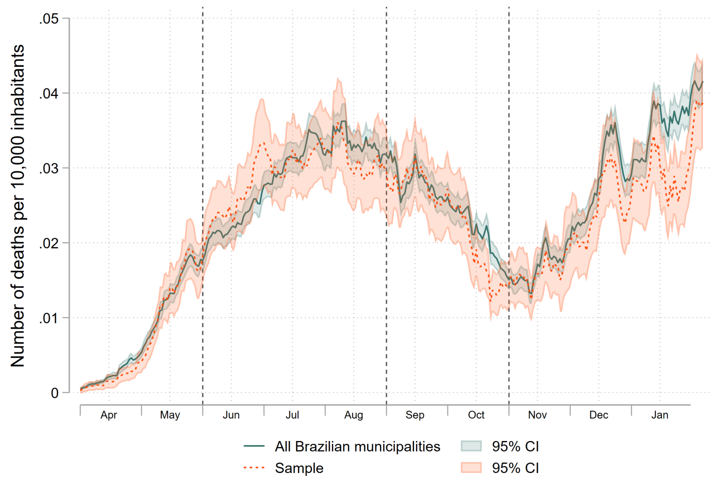
Notes: This figure shows the number of COVID-19 death per 10,000 inhabitants in each Brazilian municipality by the end of April 2020. White indicates zero deaths; light blue indicates between 0 and 1 deaths per 10,000 inhabitants; dark blue indicates between 1 and 6 deaths per 10,000 inhabitants. Municipalities circled in red are the ones part of our sample of analysis.

Figure A3: Municipalities in the analysis sample by gender of the election winner



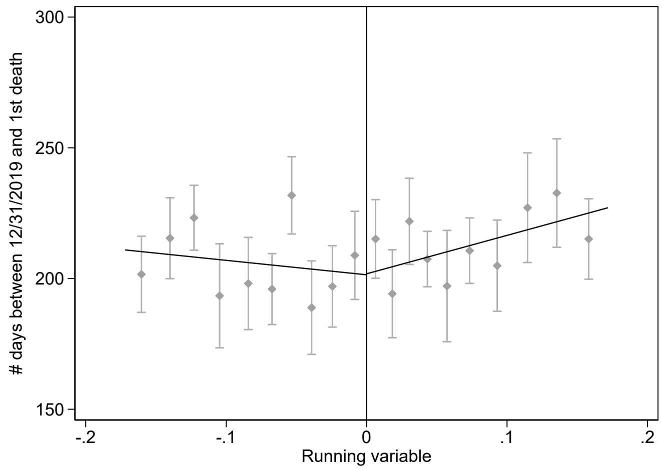
Notes: This figure plots the geographical distribution of municipalities in our sample of analysis. Municipalities in blue (red) are where a female (male) candidate was elected in 2016.

Figure A4: Evolution of COVID-19 deaths across Brazilian municipalities



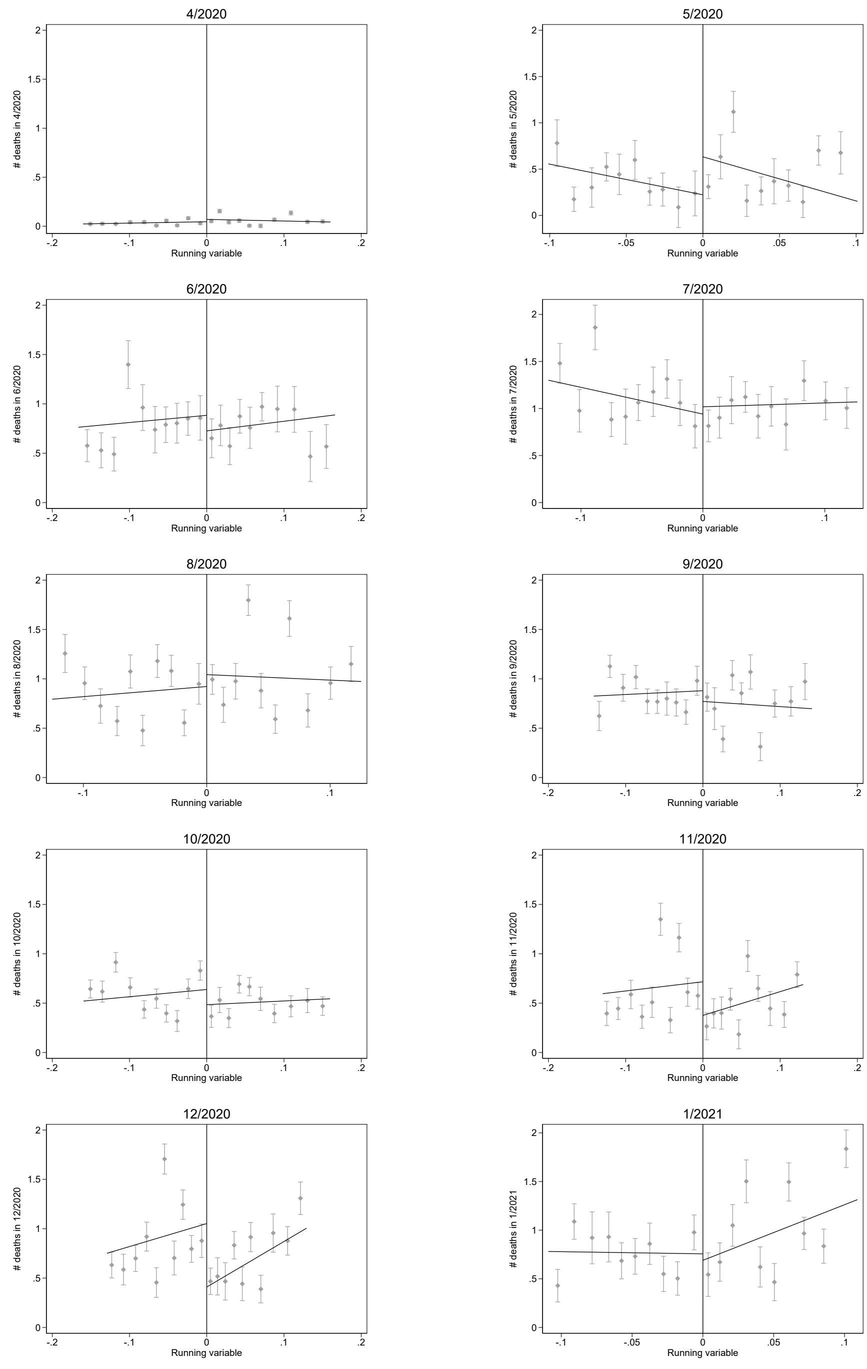
Notes: This graph plots the 7-day moving average of the number of deaths per 10,000 inhabitants across Brazilian municipalities for each day from April 1, 2020, to January 31, 2021. In green, we consider all Brazilian municipalities, while in orange we consider only municipalities in our sample of analysis. 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).

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



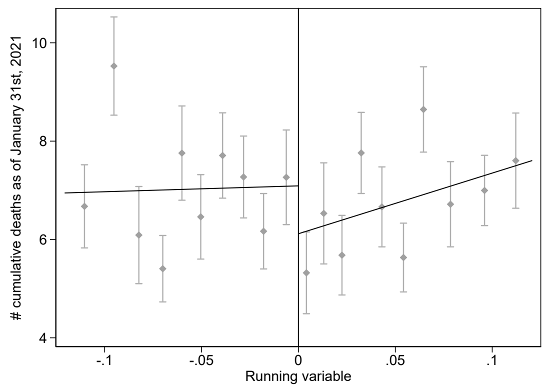
Notes: This figure is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the number of days between December 31, 2019, and the first reported COVID-19 death. Averages are calculated within quantile spaced bins of the running variable. Vertical lines represent 95-percent robust confidence intervals. The running variable is the percentage-point difference between the vote shares of the female and male candidates in the 2016 election. Positive (negative) values denote that the female (male) candidate won. All municipal characteristics presented in Table 1 are included as controls.

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



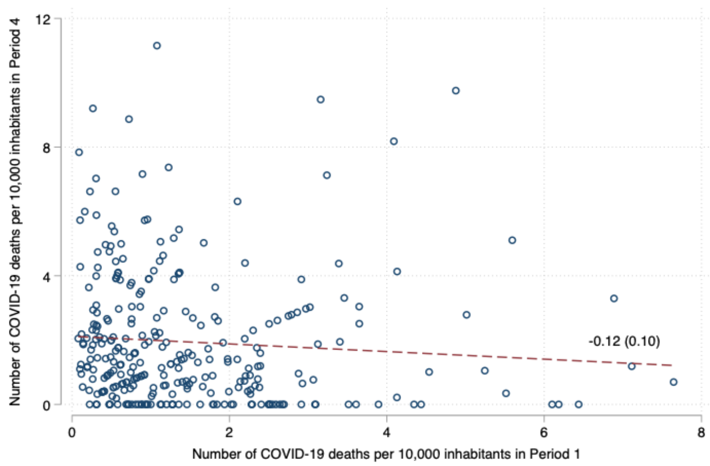
Notes: Each graph is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the total number COVID-19 deaths per 10,000 inhabitants in the municipality during the month of interest. Averages are calculated within quantile spaced bins of the running variable. Vertical lines represent 95-percent robust confidence intervals. The running variable is the percentage-point difference between the vote shares of the female and male candidates in the 2016 election. Positive (negative) values denote that the female (male) candidate won. All municipal characteristics presented in Table 1 are included as controls.

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



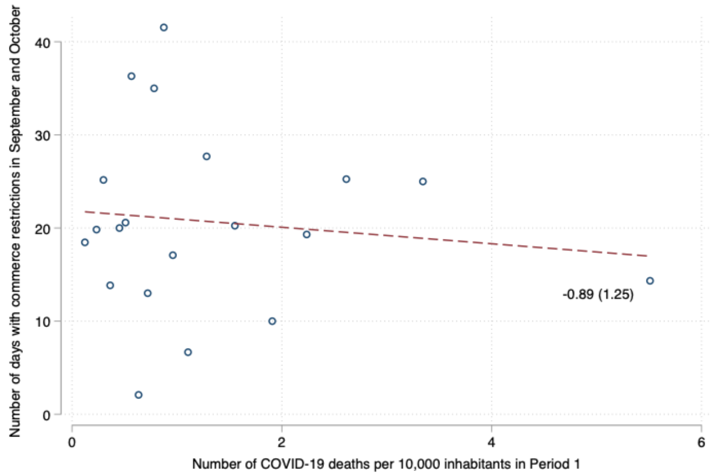
Notes: This figure is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the cumulative number COVID-19 deaths per 10,000 inhabitants in the municipality as of January 31, 2021. Averages are calculated within quantile spaced bins of the running variable. Vertical lines represent 95-percent robust confidence intervals. The running variable is the percentage-point difference between the vote shares of the female and male candidates in the 2016 election. Positive (negative) values denote that the female (male) candidate won. All municipal characteristics presented in Table 1 are included as controls.

Figure A8: Correlation between COVID-19 deaths in period 1 and period 4



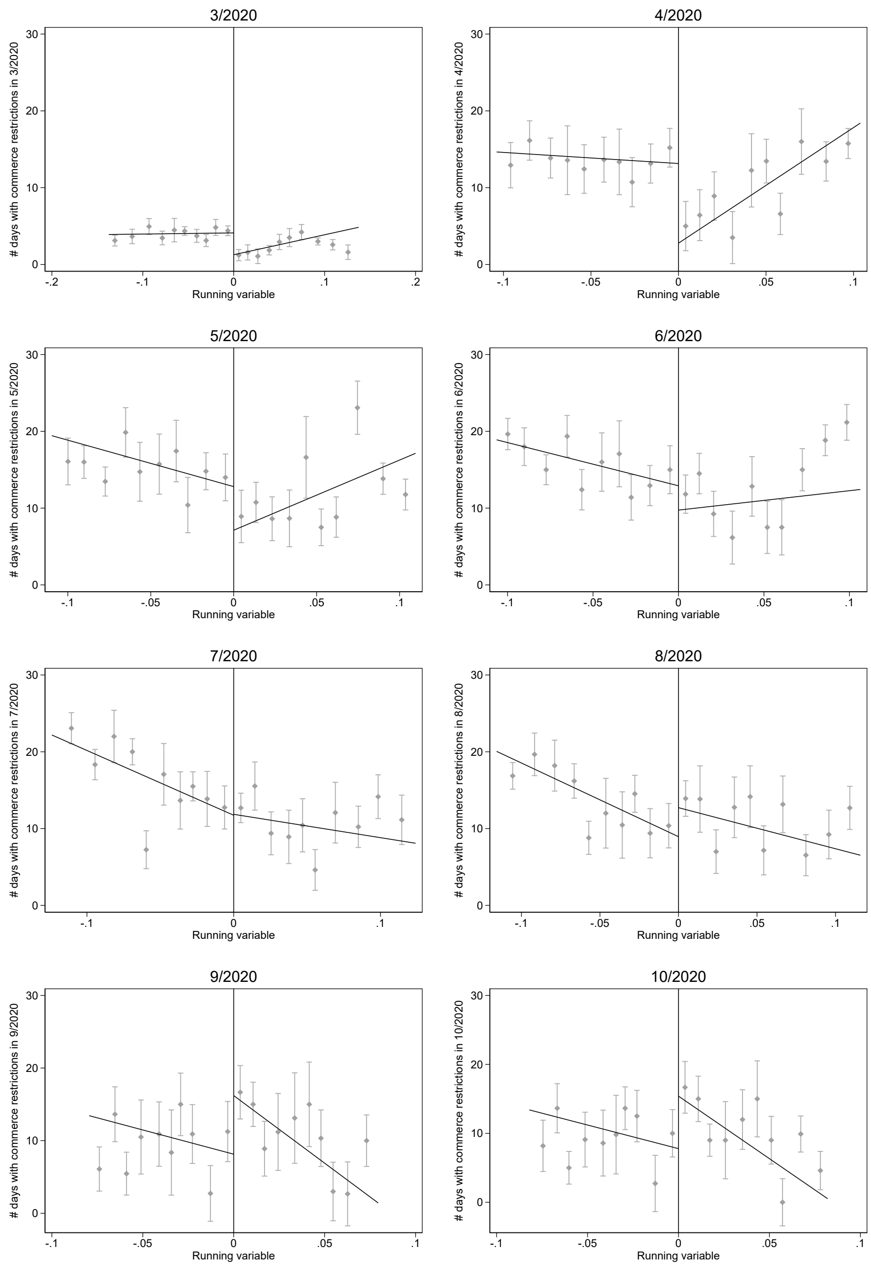
Notes: This scatterplot reports the total number of COVID-19 deaths per 10,000 inhabitants in the first period (April-May 2020) of analysis (x-axis) and in the last period (November 2020-January 2021) of analysis (y-axis), restricting the sample to municipalities that had at least one death in the first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

Figure A9: Correlation between COVID-19 deaths in period 1 and commerce restrictions in September and October



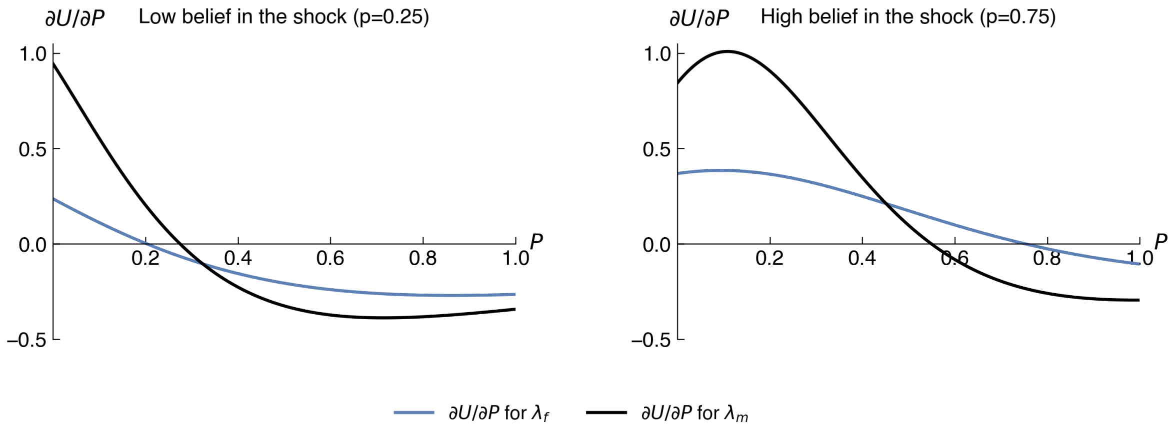
Notes: This scatterplot reports the total number of COVID-19 deaths per 10,000 inhabitants in the first period (April-May 2020) of analysis (x-axis) and the total number of days during which non-essential businesses were closed in September and October 2020 (y-axis), restricting the sample to municipalities that had at least one death in the first period.

Figure A10: Impact of having a female mayor on commerce restrictions, by month



Notes: The sample is restricted to municipalities with over 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 enacted in the municipality during the month of interest. Averages are calculated within quantile spaced bins of the running variable. Vertical lines represent 95-percent robust confidence intervals. The running variable is the percentage-point difference between the vote shares of the female and male candidates in the 2016 election. Positive (negative) values denote that the female (male) candidate won. All municipal characteristics presented in Table 1 are included as controls.

Figure A11: Marginal utility of the policy ($\frac{\partial U}{\partial P}$) as a function of P at different levels of belief in the likelihood of the shock (p)



Notes: The figure plots the marginal utility of the policy ($\frac{\partial U}{\partial P}$) as a function of the policy level P for female mayors ($\lambda_f = 2$) and male mayors ($\lambda_m = 4$) under two scenarios – one in which voters believe that the shock has a low likelihood of occurring ($p = 0.25$) and one in which voters believe it has a high likelihood of occurring ($p = 0.75$) – normalizing the pre-crisis amount of the public good to $\bar{g} = 1$ and assuming a shock of magnitude $\psi = 3$.

A2 Additional tables

Table A1: Descriptive statistics: sample comparisons

	All (N=5,556)		Sample (N=1,014)		Close (N=207)	
	Mean	Sd	**Mean**	Sd	**Mean**	Sd
Panel A	*Socio-demographic characteristics*					
Population	**33,706**	199,763	**18,029**	35,441	**16,096**	19,520
Density	**501.2**	1667.8	**157.8**	377.6	**1,34.6**	238.3
Average persons per room	**0.664**	0.213	**0.704**	0.242	**0.705**	0.208
Commuting time	**22.23**	5.98	**21.61**	4.55	**21.66**	4.77
Share of population $\geq$ 65 years old	**0.084**	0.025	**0.082**	0.023	**0.081**	0.023
Nursing home residents per 10k wa pop	**6.243**	13.818	**4.091**	12.658	**3.612**	8.201
Health facilities per 10k pop	**13.86**	7.66	**12.67**	6.61	**12.54**	6.38
Hospital beds per 10k pop	**17.26**	20.98	**15.30**	17.09	**16.26**	17.93
Area	**1,525**	5,645	**1,956**	7,353	**1,683**	4,582
Distance to São Paulo	**1,168**	754	**1,447**	745	**1,479**	732
Km to airport connecting to COVID hot spots	**272.7**	205.6	**303.2**	220.3	**294.6**	202.8
Median household income p/c	**388.3**	165.6	**324.0**	146.4	**320.4**	153.7
Informality rate	**0.158**	0.055	**0.168**	0.054	**0.166**	0.057
Unemployment rate	**0.043**	0.022	**0.044**	0.021	**0.044**	0.023
College graduate employment share	**0.076**	0.036	**0.069**	0.031	**0.068**	0.033
Black and mixed-race population share	**0.524**	0.238	**0.599**	0.215	**0.593**	0.227
Agriculture employment share	**0.364**	0.184	**0.413**	0.155	**0.432**	0.163
Evangelical share of population	**0.171**	0.095	**0.159**	0.092	**0.150**	0.090
Panel B	*Political characteristics*					
Turnout	**0.855**	0.060	**0.854**	0.060	**0.857**	0.058
Number of candidates	**2.748**	1.170	**2.688**	0.967	**2.768**	1.108
President's vote share	**0.387**	0.190	**0.324**	0.188	**0.313**	0.195

Notes: The sample includes either all Brazilian municipalities (first two columns), only municipalities in our sample of analysis (middle two columns), or only municipalities in our analysis sample close to the discontinuity, defined as those where the victory margin is lower than 4 percentage points (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 – defined as the population living within 1 km of the average inhabitant of the municipality and computed using the 2015 data from the Global Human Settlement Layer – and health variables – obtained from the Ministry of Health's Cadastro Nacional de Estabelecimentos de Saúde and measured in December 2016. 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. Note that municipalities are not weighted by their population, so that cross-municipality averages may differ from national averages. All variables are defined in Appendix Table B1.

Table A2: Race closeness and municipality baseline characteristics

Outcome	(1) (Continous) Vote Margin	(2) (Binary) Vote Margin $\leq$ 10pp
Population	-0.000	0.000
	(0.000)	(0.000)
Density	0.000	-0.000
	(0.000)	(0.000)
Average persons per room	-0.001	-0.044
	(0.023)	(0.093)
Commuting time	-0.001	0.002
	(0.001)	(0.004)
Share of population $\geq$ 65 years old	-0.223	-0.188
	(0.250)	(0.927)
Nursing home residents per 10k wa pop	-0.000	0.001
	(0.000)	(0.001)
Health facilities per 10k pop	0.001	-0.001
	(0.001)	(0.003)
Hospital beds per 10k pop	-0.000	-0.000
	(0.000)	(0.001)
Area	-0.000	0.000
	(0.000)	(0.000)
Distance to São Paulo	-0.000	0.000
	(0.000)	(0.000)
Km to airport connecting to COVID hot spots	0.000	0.000
	(0.000)	(0.000)
Median household income p/c	0.000	0.000
	(0.000)	(0.000)
Informality rate	0.025	-0.241
	(0.078)	(0.318)
Unemployment rate	-0.197	0.530
	(0.213)	(0.888)
College graduate employment share	-0.028	-0.129
	(0.160)	(0.632)
Black and mixed-race population share	0.039	0.057
	(0.037)	(0.142)
Agriculture employment share	-0.033	0.128
	(0.037)	(0.141)
Evangelical share of population	0.018	-0.292
	(0.047)	(0.192)
Observations	1,014	1,014
R-squared	0.017	0.014
Joint F-Stat	0.488	0.470

Notes: In Column 1 we regress the 2016 vote margin on all the baseline municipal characteristics presented in Panel A of Table 1. In Column 2, the dependent variable is a dummy equal to 1 if the race is closer than 10 percentage points. All variables are defined in Appendix Table B1.

Table A3: 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 A4: Impact of the mayor's level of education on COVID-19 deaths

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	# COVID-19 deaths per 10,000 inhabitants							
Sample	All mayors				Non-term-limited mayors			
	Period 1	Period 2	Period 3	Period 4	Period 1	Period 2	Period 3	Period 4
Female	-0.149	0.280	-0.112	0.008	0.044	0.343	0.322	0.101
	(0.144)	(0.328)	(0.217)	(0.410)	(0.153)	(0.377)	(0.273)	(0.475)
Robust p-value	0.321	0.373	0.843	0.946	0.821	0.375	0.124	0.727
Observations	925	790	895	836	626	635	489	657
Polyn. order	1	1	1	1	1	1	1	1
Bandwidth	0.155	0.123	0.147	0.134	0.117	0.120	0.087	0.126
Mean	0.444	2.105	1.388	2.988	0.384	2.169	1.107	3.102

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. In Columns 5 to 8, the sample is further restricted to first-term mayors who are not term limited. 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 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 and 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 A5: Impact of having a female mayor on the timing of the first COVID-19 death

	(1)
Outcome	Date of the first death
Female	0.367
	(10.843)
Robust p-value	0.999
Observations	691
Polyn. order	1
Bandwidth	0.172
Mean, left of threshold	201.400

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. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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
Female	0.024	0.410**	-0.158	0.078	0.121	-0.109	-0.154	-0.341*	-0.643***	-0.067
	(0.034)	(0.159)	(0.199)	(0.215)	(0.243)	(0.182)	(0.171)	(0.172)	(0.219)	(0.252)
R. p-value	0.614	0.011	0.466	0.627	0.744	0.639	0.344	0.053	0.008	0.685
Obs.	695	473	707	561	553	624	695	582	577	505
Polyn.	1	1	1	1	1	1	1	1	1	1
Bandwidth	0.160	0.101	0.166	0.127	0.125	0.141	0.160	0.130	0.129	0.109
Mean	0.041	0.180	0.873	0.918	0.844	0.815	0.602	0.743	1.007	0.752

Notes: Each column takes as outcome the number of deaths per 10,000 inhabitants during the month of interest. The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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

	(1)
Outcome	Cumulative number of COVID-19 deaths as of 01/31/2021
Female	-0.974
	(0.704)
Robust p-value	0.193
Observations	534
Polyn. order	1
Bandwidth	0.121
Mean, left of threshold	6.787

Notes: The outcome is the cumulative number of deaths per 10,000 inhabitants as of January 31, 2021. The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 on COVID-19 deaths, controlling for cumulative deaths

	(1)	(2)	(3)	(4)
Outcome	# COVID-19 deaths per 10,000 inhabitants			
	Period 1	Period 2	Period 3	Period 4
Female	0.439***	-0.120	-0.298	-0.966**
	(0.163)	(0.422)	(0.266)	(0.368)
Robust p-value	0.008	0.807	0.295	0.022
Observations	482	543	676	498
Polyn. order	1	1	1	1
Bandwidth	0.104	0.123	0.154	0.107
Mean, left of threshold	0.220	2.546	1.417	2.413

Notes: The outcome is the number of deaths per 10,000 inhabitants 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. All municipal characteristics presented in Table 1 are included as controls. Column 2 further includes as control the number of deaths in period 1, column 3 the total number of deaths in periods 1 and 2, and column 4 the total number of deaths in periods 1 to 3. We use a nonparametric estimation procedure and 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 first closure and on commerce reopening

	(1)	(2)
Outcome	Commerce restrictions	
	First implementation day	Lifted August-October
Female	52.913***	-0.133
	(11.712)	(0.083)
Robust p-value	0.000	0.101
Observations	159	231
Polyn. order	1	1
Bandwidth	0.093	0.091
Mean, left of threshold	94.321	0.147

Notes: The sample is restricted to municipalities with over 10,000 inhabitants. The outcome is the number of days between December 31, 2019, and the first day on which the municipality enacted commerce restrictions (Column 1) or an indicator equal to 1 if the mayor reopened non-essential businesses at some point between August and October 2020 (Column 2). The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 on the 2019 budget

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	2019 Budget items, per capita							
	Spending						Balance	%Transfers
	Total	Admin.	Education	Health	Social	Urbanism		
Female	63.30	66.17	-23.44	39.06	5.675	-8.827	149.6	0.001
	(182.3)	(70.47)	(58.27)	(42.68)	(11.84)	(25.60)	(115.2)	(0.016)
Robust p-value	0.894	0.402	0.598	0.550	0.660	0.753	0.123	0.838
Observations	478	746	480	464	646	505	410	500
Polyn. order	1	1	1	1	1	1	1	1
Bandwidth	0.102	0.187	0.104	0.0979	0.147	0.114	0.0829	0.129
Mean	3187	468.8	1049	728	123.7	238.7	-658.9	0.540

Each outcome represents the 2019 annual amount, expressed in Brazilian reais per capita, except for the last outcome that is expressed in percentage. In Column 1, the outcome is total spending, while in columns 2 to 6 the outcome is the amount spent in a given spending category. In column 7, the outcome is the budget balance, defined as the difference between total spending and total revenue. In column 8, the outcome is the percentage of the total municipal revenue coming from transfers by the federal government. The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 on the 2019 budget, focusing on non-term-limited mayors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	2019 Budget items, per capita - Non-term-limited mayors							
	Spending						Balance	%Transfers
	Total	Admin.	Education	Health	Social	Urbanism		
Female	51.10	99.49	1.063	33.23	5.287	-8.279	84.23	0.005
	(199.7)	(105.6)	(59.41)	(47.84)	(12.76)	(32.35)	(116.0)	(0.021)
Robust p-value	0.901	0.323	0.969	0.624	0.771	0.789	0.340	0.615
Observations	393	451	397	433	392	368	353	275
Polyn. order	1	1	1	1	1	1	1	1
Bandwidth	0.116	0.138	0.119	0.130	0.116	0.108	0.0996	0.0875
Mean	3347	494.2	1050	768.6	131.5	255.3	-622.5	0.531

The sample includes only elections in which the mayor is not term-limited. Each outcome represents the 2019 annual amount, expressed in Brazilian reais per capita, except for the last outcome that is expressed in percentage. In Column 1, the outcome is total spending, while in columns 2 to 6 the outcome is the amount spent in a given spending category. In column 7, the outcome is the budget balance, defined as the difference between total spending and total revenue. In column 8, the outcome is the percentage of the total municipal revenue coming from transfers by the federal government. The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 1 km of the average inhabitant of the municipality. For each municipality, we count the population living in a 1km radius (encompassing areas inside and outside the municipality's perimeter) around each 0.2-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 ≥65 years old	Census	2010	Share of the municipality´s population aged 65 or above.
Nursing home residents per 10k wa pop	Census	2010	Number of individuals aged 65 or above living in nursings homes or asylums, per 10,000 working-aged individuals.
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 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 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 that is employed or unemployed.
College graduate employment share	Census	2010	The share of employed individuals (who reported their educational status) that had completed college or a higher educational level.
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.
Health facilities per 10k pop	CNES	Dec 2016	Number of public and private health establishments registered in CNES (2021) and active in December 2016 within the municipality, per 10,000 inhabitants.
Hospital beds per 10k pop	CNES	Dec 2016	Total number of hospital beds registered in CNES (2021) and active in December 2016 in establishments located in the municipality, per 10,000 inhabitants.

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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.
Share of past female councilors	TSE	2008-12	Share of women in the total number of councilors elected in the 2008 and 2012 municipal elections.
Panel C: Candidate-level electoral variables			
Vote share	TSE	2016	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.
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	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	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 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.
Served as councilor previously	TSE	2008-12	Dummy variable that equals 1 if the candidate was elected in the last two terms to serve in the city council.

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Variable	Dataset	Date	Description / comments
			Panel D: Municipal budget variables
Total spending per capita	SICONFI	2019	Total annual municipal spending, expressed in Brazilian reais per capita.
Spending by category per capita	SICONFI	2019	Annual municipal spending in a given category—administrative, education, health, social, and urbanism—expressed in Brazilian reais per capita.
Budget balance	SICONFI	2019	Difference between total municipal spending and total municipal revenue, expressed in Brazilian reais per capita.
Share of transfers	SICONFI	2019	Percentage of total municipal revenue coming from transfers by the federal government.
			Panel E: 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 states' health secretaries.
Deaths per 10k	SIVEP-Gripe	2020–21	Number of SARI 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.
Containment policies	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. (2020) (see Appendix B2).

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

B2 Policies data

We constructed our policy data directly from publicly available municipal legislation documents, following Chauvin et al. (2020).

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, corresponding to

519 observations (51.2 percent of our sample).[44]

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 (Hale et al., 2021), 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 dawn). 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.

- *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).

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

- *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 over 10,000 inhabitants. 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) at least once: event cancellations, face mask mandates, restrictions on gathering, and school closures. Moreover, looking at the share of municipalities in our analysis sample that had a given policy in place day by day, Figure B1 shows that these four policies were in place in most municipalities for most of the period of analysis.

To explore the variation in the policy data more formally, figure B2 considers the variation in the use of policies across municipalities and across time. 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.

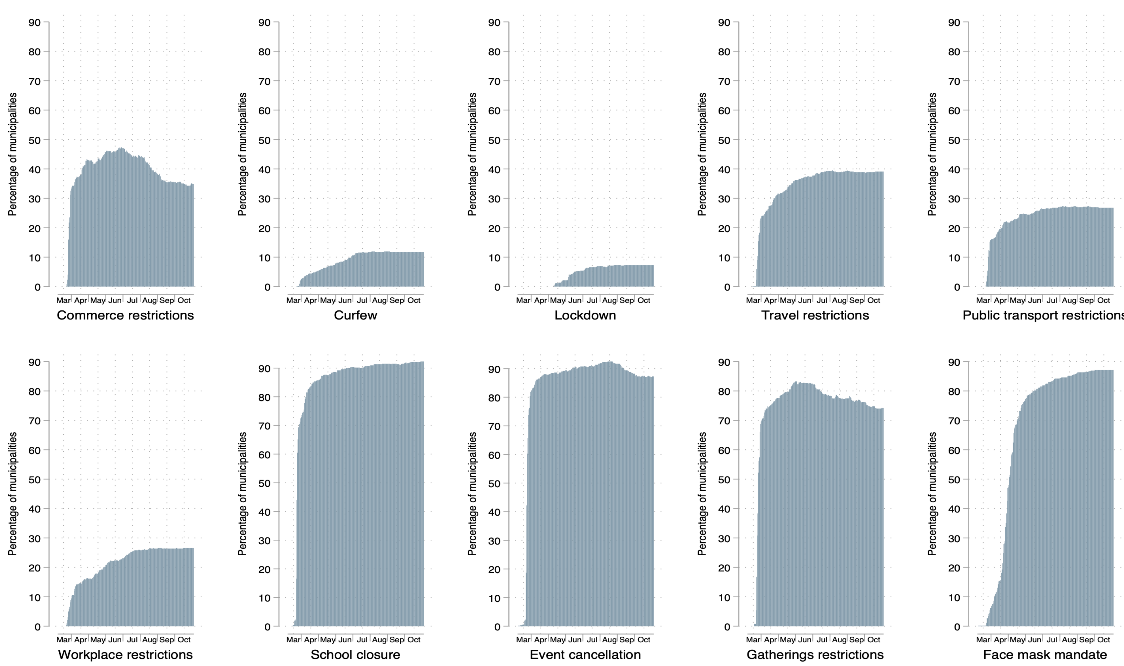
We thus focus our analysis on the remaining six policies, for which we have enough variation to identify the effects of interest.

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

Policy	Representative municipalities		Municipalities in sample	
	Count	Share of total (%)	Count	Share of total (%)
Commerce close	410	69.02	355	68.40
Curfew	68	11.45	64	12.33
Events cancellation	555	93.43	490	94.41
Facemask mandatory	534	89.90	452	87.09
Gatherings restriction	533	89.73	467	89.98
Lockdown	46	7.74	40	7.71
School close	544	91.58	485	93.45
Transport	237	39.90	161	31.02
Travel canceled	246	41.41	217	41.81
Workplace close	169	28.45	161	31.02
Total	594	100.00	519	100.00

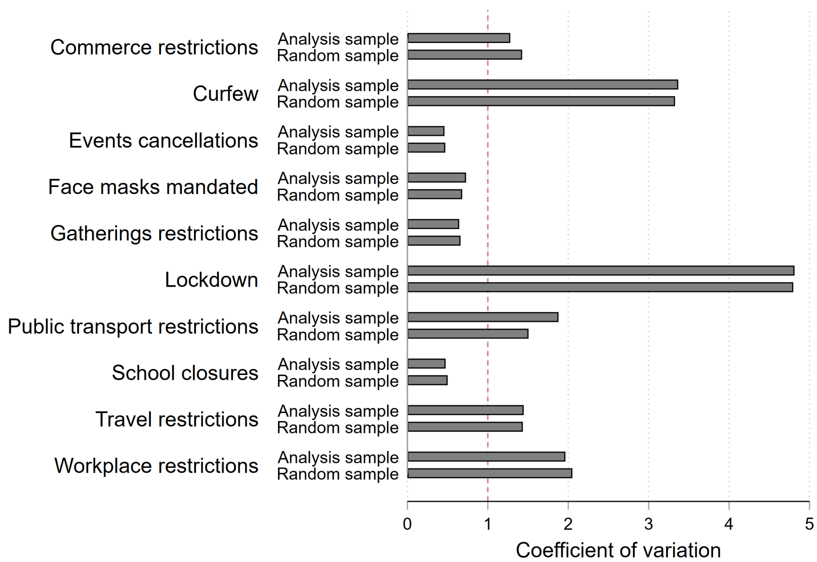
Notes: This table gives the number and share of municipalities that enacted the policy at least once from March to October 2020. The first two columns consider a 20-percent random sample representative of Brazilian municipalities with a population of 10,000 or larger. The last two columns consider municipalities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

Figure B1: Share of municipalities where a given policy was in place on a given date



Notes: This figure plots, for each policy, the share of municipalities in our analysis sample that had the policy in place, for each day over the period March-October 2020.

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 and for municipalities in our analysis sample with a population of 10,000 or larger.

B3 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.

B3.1 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.

B3.2 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 assess 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.[45] 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.

B3.3 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.

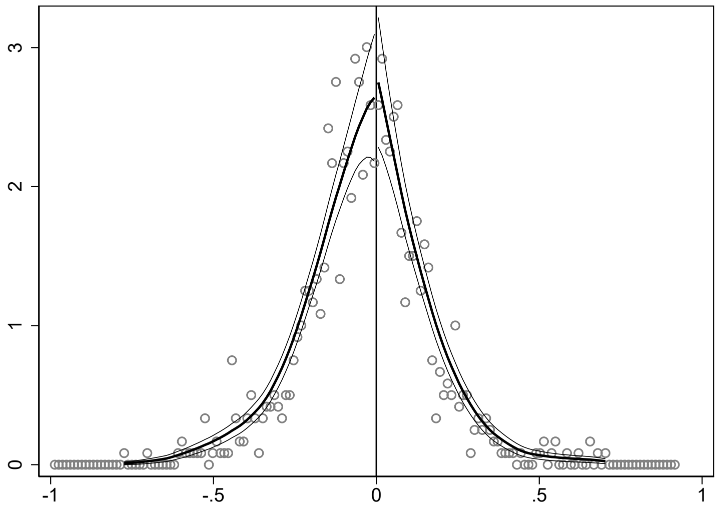
- Nineteen elections in which the number of null votes (in which invalidated votes are often counted) was larger than the number of votes received by the second-place candidate. All were indeed cases in which one of the top-two candidates was invalidated.

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

[45]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).

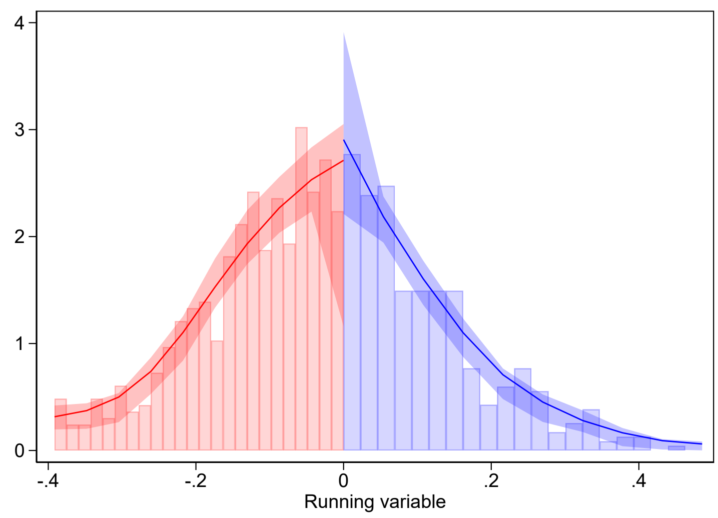
C Validity tests

Figure C1: 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 C2: Cattaneo et al. (2018)'s density test



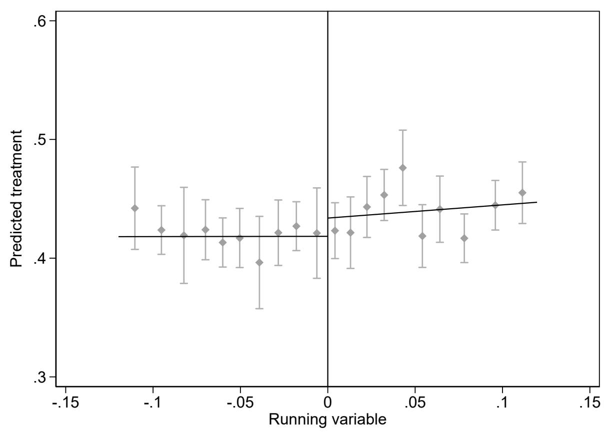
Notes: This Figure tests for a jump in the density of the running variable (the victory margin of the female candidate) at the threshold using the method developed by Cattaneo et al. (2018). The solid line represents the density of the running variable. Thin lines represent the confidence intervals. The p-value associated with the density test is 0.15.

Table C1: General balance test

	(1)
Outcome	Predicted Female
Female	0.015
	(0.013)
Robust p-value	0.376
Observations	533
Polyn. order	1
Bandwidth	0.120
Mean, left of threshold	0.418

Notes: The outcome is the treatment variable predicted by municipal characteristics. We compute the outcome as follows: we first regress the treatment variable T on all 21 baseline variables presented in Table 1 and we then predict the treatment status of each municipality using the regression coefficients. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure and 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 C3: 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). The outcome is the treatment variable predicted by municipal characteristics. We compute the outcome as follows: we first regress the treatment variable T on all 21 baseline variables presented in Table 1 and we then predict the treatment status of each municipality using the regression coefficients. The independent variable is an indicator equal to 1 if the female candidate won in 2016. Dots represent the local averages of the outcome variable calculated within quantile-spaced bins of the running variable. The running variable is the percentage-point difference between the vote shares of the female and male candidates in the 2016 election. Positive (negative) values denote that the female (male) candidate won.

Table C2: Balance test: Municipality characteristics

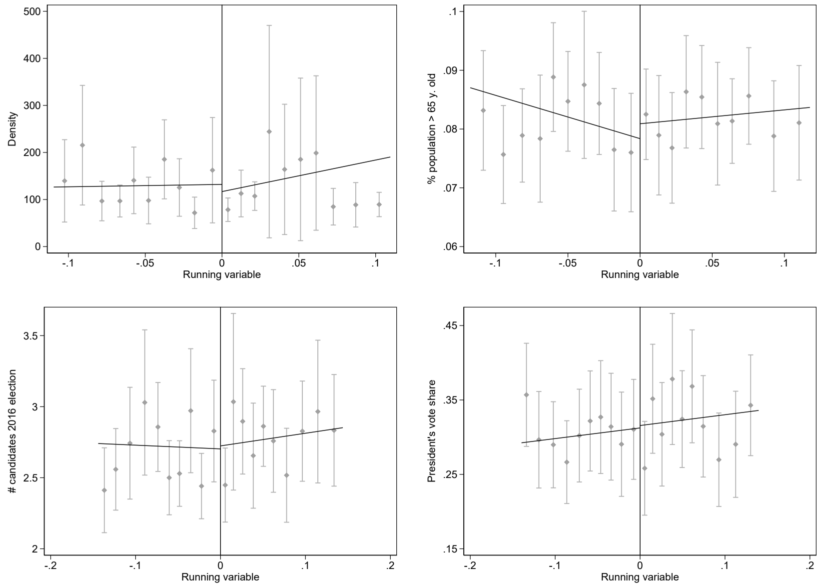
Outc.	(1) Pop	(2) Density	(3) Persons /room	(4) Commuting	(5) % above 65 y. old	(6) Nursing h. residents	(7) Health facilities	(8) Hospital beds	(9) Area	(10) Distance to São Paulo	(11) Km to airport
Treat.	-4,373	-15.2	-0.027	0.111	0.003	-1.815	0.775	-2.784	-1,922**	-80	-55.9
	(3,275)	(40.5)	(0.035)	(0.874)	(0.004)	(1.716)	(1.210)	(2.616)	(801)	(124)	(35.7)
P-value	0.148	0.615	0.536	0.915	0.441	0.341	0.469	0.231	0.031	0.662	0.151
Obs	602	508	634	550	529	708	572	596	640	598	599
Polyn.	1	1	1	1	1	1	1	1	1	1	1
Bdw	0.135	0.110	0.144	0.124	0.118	0.167	0.128	0.134	0.145	0.135	0.135
Mean	17,314	132.1	0.725	21.496	0.078	4.985	12.378	17.322	3,093	1,521	339.0

Outc.	(12) Median income	(13) Inform. rate	(14) Unemp. rate	(15) % college employed	(16) % black & mixed	(17) % employed agriculture	(18) % evangelical	(19) Turnout	(20) Number cand	(21) President vote share
Treat.	24.6	0.007	-0.004	-0.005	-0.032	0.007	-0.009	0.017	0.021	0.003
	(22.5)	(0.010)	(0.004)	(0.005)	(0.036)	(0.023)	(0.015)	(0.009)	(0.176)	(0.032)
P-value	0.382	0.479	0.482	0.379	0.532	0.758	0.424	0.117	0.982	0.894
Obs	695	604	612	631	544	729	630	690	635	620
Polyn.	1	1	1	1	1	1	1	1	1	1
Bdw	0.160	0.136	0.138	0.143	0.124	0.180	0.142	0.158	0.144	0.140
Mean	304.5	0.163	0.046	0.071	0.612	0.432	0.155	0.846	2.703	0.312

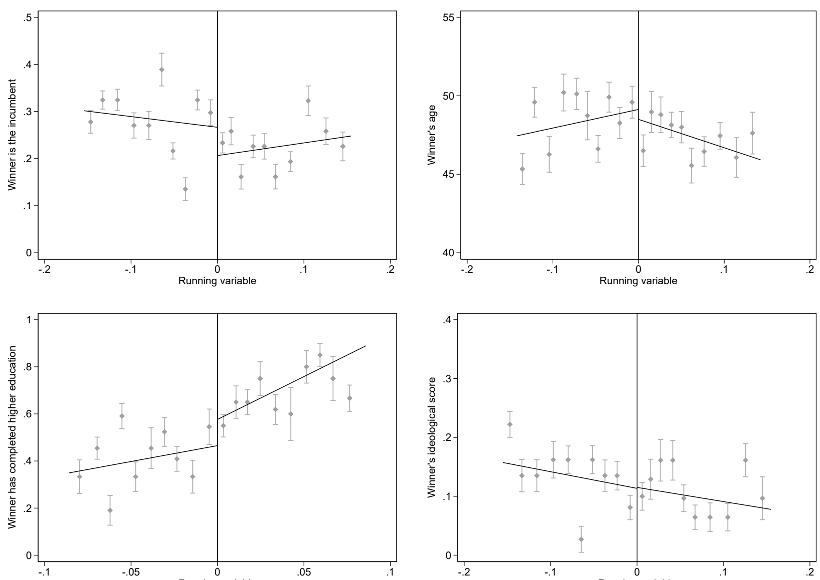
Notes: Each column considers a specific baseline characteristic, as defined in Table B1. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure and MSERD data-driven bandwidths (referred to as "Bdw" in the table). We assess 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 C4: Balance test

Panel A. Municipality characteristics



Panel B. Characteristics of the winner of the election



Notes: Panel A focuses on four municipality characteristics (density, share of the population above 65 years old, number of candidates in the 2016 election, and vote share of the president), while Panel B focuses on 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. Vertical lines represent 95-percent robust confidence intervals. The running variable is the percentage-point difference between the vote shares of the female and male candidates in the 2016 election. Positive (negative) values denote that the female (male) candidate won. In Panel B, all municipal characteristics presented in Table 1 are included as controls.

Table C3: Balance test: Age brackets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome	Age bracket								
	≤14	15-24	25-34	35-44	45-54	55-64	65-74	75-84	≥85
Female	-0.007	-0.004	-0.002	0.003	0.003	0.002	0.000	0.001	0.001*
	(0.008)	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)	(0.002)	(0.001)	(0.001)
Robust p-value	0.423	0.199	0.392	0.613	0.340	0.424	0.729	0.363	0.070
Observations	722	613	505	532	603	684	543	575	453
Polyn. order	1	1	1	1	1	1	1	1	1
Bandwidth	0.174	0.139	0.109	0.119	0.135	0.156	0.123	0.129	0.095
Mean, left of threshold	0.277	0.186	0.156	0.127	0.104	0.073	0.048	0.023	0.007

Notes: Each column considers the share of the population falling in a given age bracket. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure and 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 C4: Balance test: Industry characteristics

	(1)	(2)	(3)	(4)	(5)
Outcome	Industry share				Labor force participation
	Manufacturing	Services	Agriculture	Government	Gender gap
Female	0.005	-0.014	0.007	0.003	0.004
	(0.011)	(0.019)	(0.023)	(0.006)	(0.013)
Robust p-value	0.621	0.427	0.758	0.612	0.928
Observations	732	720	729	634	538
Polyn. order	1	1	1	1	1
Bandwidth	0.180	0.173	0.180	0.143	0.121
Mean, left of threshold	0.069	0.433	0.432	0.065	0.233

Notes: In columns 1 to 4, each variable considers the share of the workforce working in a given industry sector. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure and 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.

D Robustness tests

Alternative death measure. As described in Section 2.4, 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. 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.

Alternative sample. As explained in Section 3.1, 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 D3, the results are not affected by this restriction.

Specification choices. As previewed in Section 3.3.3, we conduct a series of robustness checks to provide additional support for our identification strategy.

First, Appendix Table D4 tests the robustness of our results to using different combinations of controls and state fixed-effects: no control, state-fixed effects, only municipal characteristics controls (corresponding to the main specification), both municipal and winner characteristics controls. All estimates are very close in magnitude and all remain significant.[46]

Second, Table D5 shows that our results are robust to using a second-order polynomial. Moreover, Figure D3 shows that the point estimates remain stable over a wide range of bandwidths, and Table D6 that our results are virtually unchanged when we impose a common bandwidth across outcomes.

Finally, we conduct several tests to ensure that our estimates are not driven by a subset of observations near the cutoff. Appendix Figure D4 presents jackknife estimates excluding one municipality at a time, while Appendix Figure D5 reports donut estimates that exclude observations within 0 to 2 percent of the estimation bandwidth. Across all exercises, the point estimates remain very similar.

[46] In order to include state fixed effects, in column 2, we had to remove nine states that contain less than 20 municipalities, accounting for eight percent of our sample.

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
Female	0.698**	-0.027	-0.252	-0.751
	(0.293)	(0.470)	(0.278)	(0.431)
Robust p-value	0.014	0.941	0.265	0.175
Observations	425	498	634	479
Polyn. order	1	1	1	1
Bandwidth	0.088	0.107	0.143	0.102
Mean, left of threshold	0.766	3.134	1.710	2.695

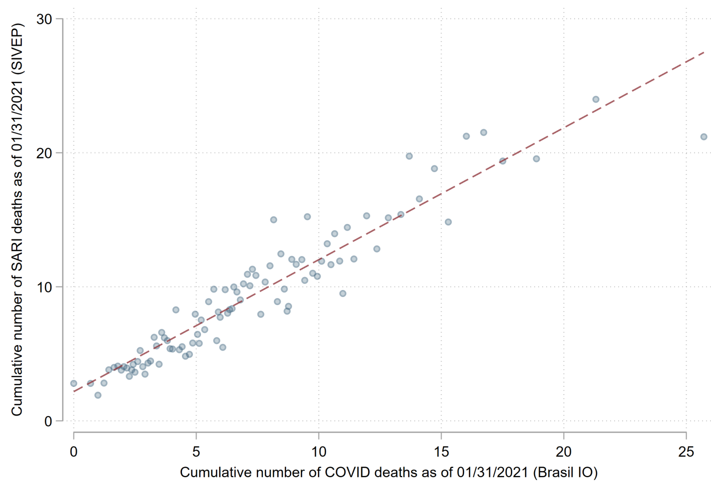
Notes: Each column takes as outcome the number of SARI deaths per 10,000 inhabitants during the period of interest. Period 1 (resp. 2, 3, and 4) is April-May 2020 (resp. June-August 2020, September-October 2020, and November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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
Female	0.014	0.732***	0.024	-0.096	-0.031	-0.045	-0.335*	-0.422***	-0.748***	0.252
	(0.104)	(0.268)	(0.211)	(0.240)	(0.253)	(0.211)	(0.203)	(0.141)	(0.256)	(0.252)
R. p-value	0.888	0.005	0.795	0.786	0.893	0.856	0.072	0.006	0.009	0.342
Obs.	533	412	699	481	551	624	469	640	510	503
Polyn.	1	1	1	1	1	1	1	1	1	1
Bandwidth	0.120	0.083	0.162	0.103	0.125	0.141	0.099	0.145	0.110	0.109
Mean	0.253	0.503	1.015	1.143	1.045	0.950	0.823	0.717	1.249	0.798

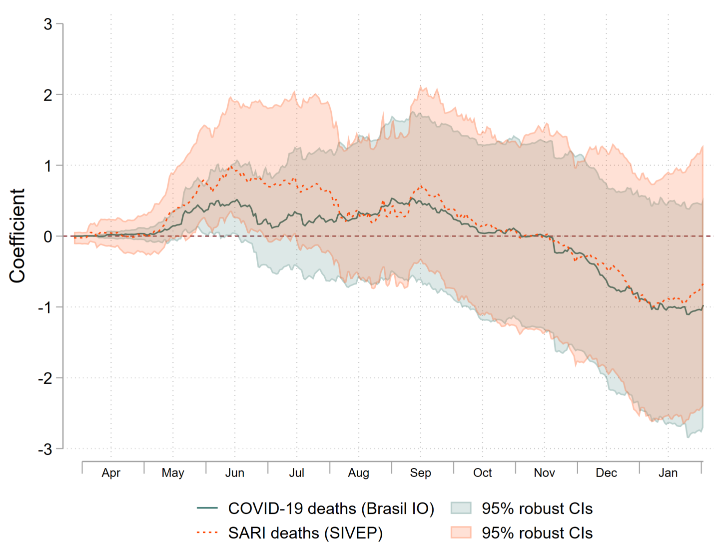
Notes: Each column takes as outcome the number of SARI deaths per 10,000 inhabitants during the month of interest. The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 RDD 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 green, 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, 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
Female	0.439***	-0.964**	0.429**	-0.918**
	(0.163)	(0.368)	(0.165)	(0.364)
Robust p-value	0.008	0.023	0.011	0.026
Observations	482	498	472	471
Polyn. order	1	1	1	1
Bandwidth	0.104	0.107	0.107	0.107
Mean	0.220	2.410	0.227	2.312

Notes: In Columns 3 and 4, we exclude municipalities in Mato Grosso state and municipalities that held a supplementary election – 3.3 and 2.7 percent of the sample, respectively. The outcome is the number of COVID-19 deaths per 10,000 inhabitants 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. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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, varying the set of controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Number of Covid-19 deaths							
Controls	None		State FEs		Municipality		Municipality+Winner	
	Period 1	Period 4	Period 1	Period 4	Period 1	Period 4	Period 1	Period 4
Female	0.333*	-1.002**	0.336**	-0.996**	0.439***	-0.964**	0.345**	-0.904**
	(0.174)	(0.399)	(0.147)	(0.399)	(0.163)	(0.368)	(0.149)	(0.364)
R. p-value	0.075	0.013	0.036	0.017	0.008	0.023	0.028	0.032
Obs.	624	513	654	470	482	498	572	462
Polyn.	1	1	1	1	1	1	1	1
Bandwidth	0.141	0.113	0.167	0.112	0.104	0.107	0.128	0.097
Mean	0.250	2.443	0.264	2.434	0.220	2.410	0.232	2.369

Notes: Columns 1 and 2 do not include any control. Columns 3 and 4 include state fixed effects and remove municipalities part of states with fewer than 20 municipalities in our sample (8.6 percent). Columns 5 and 6 include as controls the municipal characteristics presented in Table 1. Columns 7 and 8 further add the winner's characteristics presented in Table 2. The outcome is the number of COVID-19 deaths per 10,000 inhabitants during the period of interest. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure and 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, using a second-order polynomial

	(1)	(2)	(3)	(4)
Outcome	Number of Covid-19 deaths			
	Period 1	Period 4	Period 1	Period 4
Female	0.439***	-0.964**	0.528***	-1.188**
	(0.163)	(0.368)	(0.185)	(0.425)
Robust p-value	0.008	0.023	0.004	0.012
Observations	482	498	669	744
Polyn. order	1	1	2	2
Bandwidth	0.104	0.107	0.152	0.184
Mean	0.220	2.410	0.165	2.466

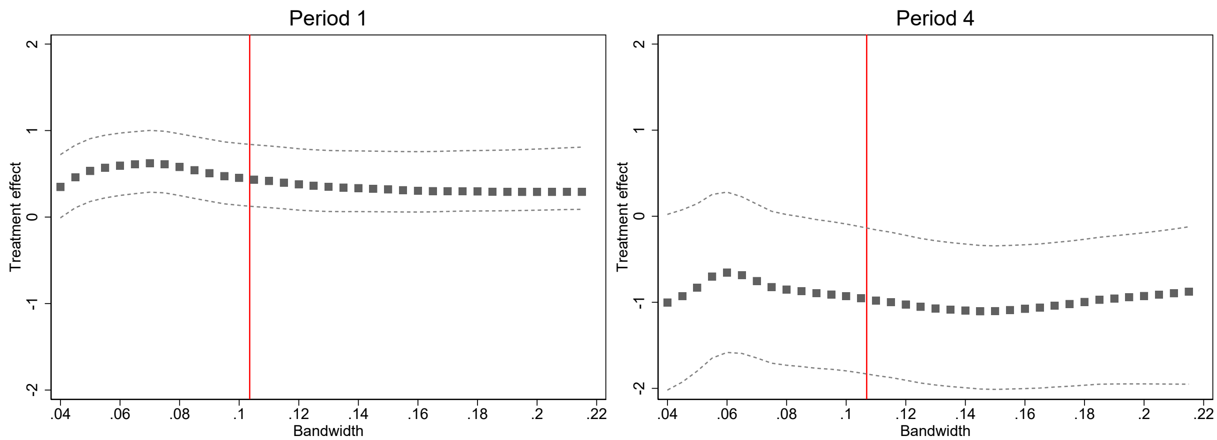
Notes: In Columns 3 and 4, we use a second-order polynomial instead of fitting linear regressions. The outcome is the number of COVID-19 deaths per 10,000 inhabitants during the period of interest. Period 1 (resp. 4) is April-May 2020 (resp. November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. 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.

Table D6: Impact on COVID-19 deaths, by period, using a unique bandwidth

	(1)	(2)	(3)	(4)
Outcome	Number of Covid-19 deaths			
	Period 1	Period 2	Period 3	Period 4
RD_Estimate	0.439***	-0.001	-0.440	-0.946**
	(0.163)	(0.456)	(0.318)	(0.372)
Robust p-value	0.008	0.993	0.171	0.019
Observations	482	482	482	482
Polyn. order	1	1	1	1
Bandwidth	0.104	0.104	0.104	0.104
Mean, left of threshold	0.220	2.602	1.612	2.394

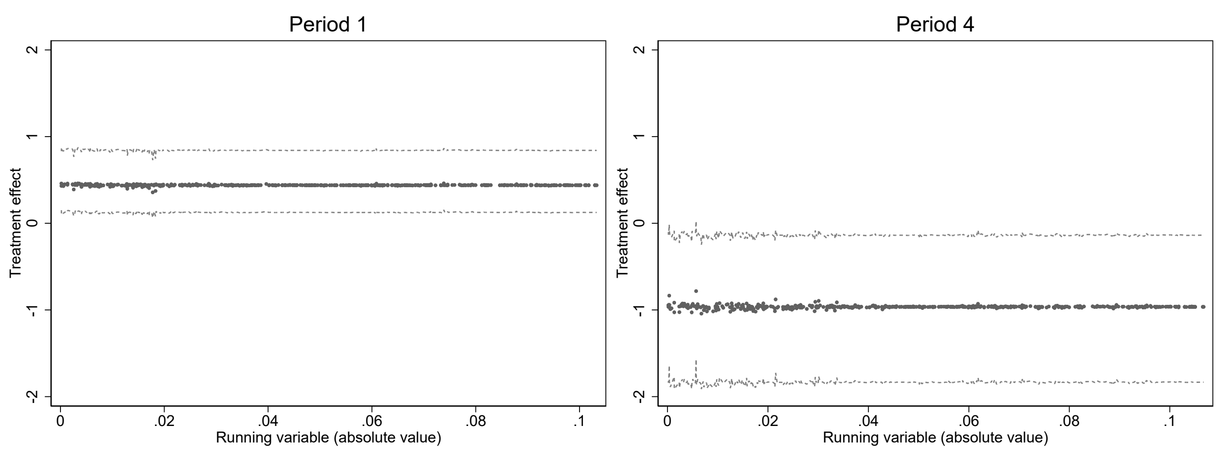
Notes: The outcome is the number of deaths per 10,000 inhabitants during the period of interest. Period 1 (resp. 2, 3, and 4) is April-May 2020 (resp. June-August 2020, September-October 2020, and November 2020-January 2021). The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure. In Column 1, we use the MSERD data-driven bandwidths. Columns 2 to 4 use the same bandwidth as in column 1. 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 D3: Impact on COVID-19 deaths: Robustness to bandwidth choice



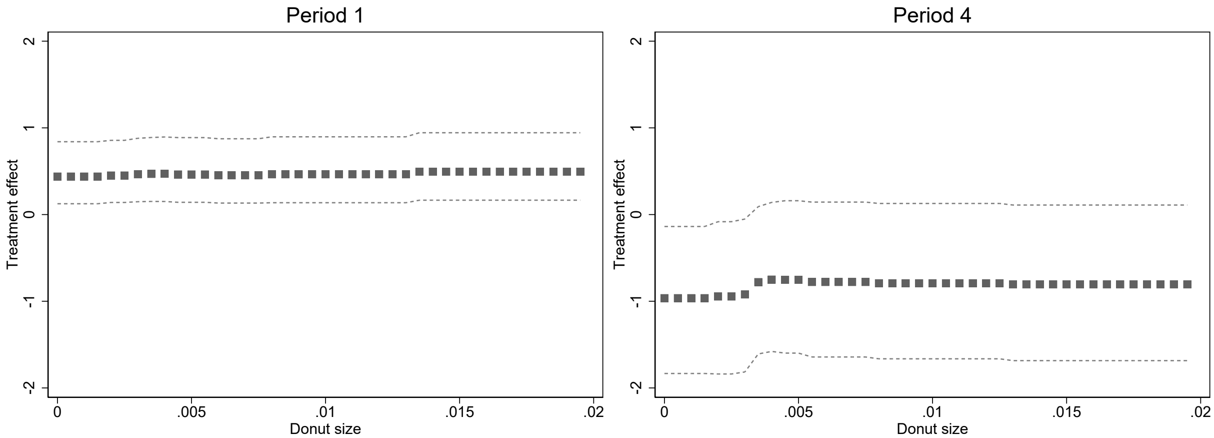
Notes: These figures show the sensitivity of the point estimate to bandwidth choice. Dots represent the estimated treatment effect using different bandwidths (horizontal axis). Dotted lines represent the 95-percent robust confidence interval. The estimates are reported for values of the bandwidth from 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. All municipal characteristics presented in Table 1 are included as controls. Each estimation uses a nonparametric estimation procedure.

Figure D4: Impact on COVID-19 deaths: Leave one out



Notes: These figures show the sensitivity of the point estimate when removing one municipality at a time. The very first dot on each graph provides the baseline estimate. Then, each dot provides the point estimate associated with removing a given municipality. Municipalities are ordered based on the absolute value of their running variable, so that the second dot provides the point estimate associated with removing the municipality the closest to the cutoff. Dotted lines represent the 95-percent robust confidence interval. 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. All municipal characteristics presented in Table 1 are included as controls. Each estimation uses a nonparametric estimation procedure.

Figure D5: Impact on COVID-19 deaths: Donut estimations



Notes: These figures show the sensitivity of the point estimate when removing observations very close to the threshold. We define the donut size as a percentage of the estimation bandwidths. Dots represent the estimated treatment effect using different donut sizes (horizontal axis). Dotted lines represent the 95-percent robust confidence interval. The estimates are reported for values of the donut hole from 0 to 2 percent, in steps of 0.05 percent. 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. All municipal characteristics presented in Table 1 are included as controls. Each estimation uses a nonparametric estimation procedure.

E Heterogeneity analysis

Table E1: 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
Female	0.581***	-1.063**	0.796**	-1.255	0.281	-1.084**
	(0.198)	(0.463)	(0.337)	(0.833)	(0.196)	(0.431)
Robust p-value	0.005	0.038	0.029	0.266	0.129	0.013
Observations	347	376	186	135	175	186
Polyn. order	1	1	1	1	1	1
Bandwidth	0.096	0.105	0.119	0.076	0.091	0.097
Mean	0.163	2.780	0.253	2.941	0.080	2.453

The sample includes only elections in which the mayor is not term-limited. 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 outcome is the number of deaths per 10,000 inhabitants during period 1 (resp. 4). The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 E2: 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
Female	0.581***	-1.063**	0.417**	-1.597**	0.741**	-0.952
	(0.198)	(0.463)	(0.163)	(0.602)	(0.310)	(0.632)
Robust p-value	0.005	0.038	0.019	0.014	0.028	0.217
Observations	347	376	213	201	181	162
Polyn. order	1	1	1	1	1	1
Bandwidth	0.096	0.105	0.120	0.106	0.111	0.096
Mean	0.163	2.780	0.138	2.811	0.224	2.684

The sample includes only elections in which the mayor is not term-limited. 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 outcome is the number of deaths per 10,000 inhabitants during period 1 (resp. 4). The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 E3: 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
Female	0.581***	-1.063**	0.553**	-0.603	0.644***	-1.755
	(0.198)	(0.463)	(0.218)	(0.436)	(0.173)	(1.158)
Robust p-value	0.005	0.038	0.013	0.223	0.001	0.227
Observations	347	376	297	293	60	69
Polyn. order	1	1	1	1	1	1
Bandwidth	0.096	0.105	0.102	0.099	0.097	0.122
Mean	0.163	2.780	0.223	2.086	-0.076	6.159

The sample includes only elections in which the mayor is not term-limited. 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 previous two terms (resp. has served as a legislator). In Columns 1, 3, and 5 (resp. 2, 4, and 6), the outcome is the number of deaths per 10,000 inhabitants during period 1 (resp. 4). The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 E4: Impact on COVID-19 deaths, by mayor's ideology

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths					
	Full sample		Below median		Above median	
Periods	1	4	1	4	1	4
Female	0.581***	-1.063**	0.355	-1.769**	0.711**	-0.415
	(0.198)	(0.463)	(0.258)	(0.697)	(0.277)	(0.546)
Robust p-value	0.005	0.038	0.191	0.019	0.012	0.561
Observations	347	376	225	203	163	174
Polyn. order	1	1	1	1	1	1
Bandwidth	0.096	0.105	0.126	0.106	0.098	0.108
Mean	0.163	2.780	0.290	3.017	0.083	2.485

The sample includes only elections in which the mayor is not term-limited. In Columns 3 and 4 (resp. 5 and 6), the sample is restricted to municipalities where the mayor is below (resp. above) the ideological score median. In Columns 1, 3, and 5 (resp. 2, 4, and 6), the outcome is the number of deaths per 10,000 inhabitants during period 1 (resp. 4). The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 E5: Impact on COVID-19 deaths, by municipality gender discrimination

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths					
	Full sample		Below median		Above median	
Periods	1	4	1	4	1	4
Panel A	*Gender wage gap*					
Female	0.581***	-1.063**	0.267	0.265	0.738***	-1.763**
	(0.198)	(0.463)	(0.251)	(0.488)	(0.240)	(0.755)
Robust p-value	0.005	0.038	0.425	0.369	0.002	0.025
P-value (3)=(5)					0.178	
P-value (4)=(6)						0.026
Observations	347	376	209	159	188	176
Polyn. order	1	1	1	1	1	1
Bandwidth	0.096	0.105	0.129	0.094	0.099	0.089
Mean	0.163	2.780	0.312	1.747	0.036	3.556
Panel B	*Share of past female councilors*					
Female	0.581***	-1.063**	1.052***	-1.352*	0.269	-0.453
	(0.198)	(0.463)	(0.302)	(0.815)	(0.169)	(0.490)
Robust p-value	0.005	0.038	0.001	0.088	0.104	0.594
P-value (3)=(5)					0.025	
P-value (4)=(6)						0.345
Observations	347	376	161	187	187	188
Polyn. order	1	1	1	1	1	1
Bandwidth	0.096	0.105	0.091	0.111	0.100	0.100
Mean	0.163	2.780	0.141	3.708	0.163	2.038

Notes: This table focuses on elections in which the mayor is not term-limited. In Panel A, Columns 3 and 4 (resp. 5 and 6) restrict the sample to municipalities in which the gender wage gap is below (resp. above) the median. In Panel B, Columns 3 and 4 (resp. 5 and 6) restrict the sample to municipalities where the share of female councilors elected in the last two elections is below (resp. above) 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 in period 1 (resp. period 4). The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 E6: Impact on COVID-19 deaths, by mayor's control over the council

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths					
	Full sample		Below median		Above median	
Periods	1	4	1	4	1	4
Female	0.581***	-1.063**	0.858***	-0.591	0.207	-1.170*
	(0.198)	(0.463)	(0.293)	(0.551)	(0.193)	(0.623)
Robust p-value	0.005	0.038	0.003	0.335	0.246	0.093
Observations	347	376	163	177	190	252
Polyn. order	1	1	1	1	1	1
Bandwidth	0.096	0.105	0.089	0.098	0.113	0.154
Mean	0.163	2.780	0.269	2.846	0.020	2.749

The sample includes only elections in which the mayor is not term-limited. In Columns 3 and 4 (resp. 5 and 6), the sample is restricted to municipalities where the mayor's party or coalition has a number of seats in the council that is below (resp. above) the median. In Columns 1, 3, and 5 (resp. 2, 4, and 6), the outcome is the number of deaths per 10,000 inhabitants during period 1 (resp. 4). The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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.

F Additional analyses by term-limit status

In this appendix, we consider the two subsamples defined in Section 5.2, based on the mayor's term-limit status, and assess the validity of the RD design separately within each subsample.

First, we implement our general balance test within each term-limit group, using as outcome the treatment predicted by baseline municipal characteristics. As shown in Appendix Table F1, the point estimates are small and statistically insignificant both for non-term-limited mayors (column 1) and for term-limited mayors (column 2). These findings indicate no systematic imbalance in observable municipal characteristics around the cutoff in either subsample.

Second, we conduct balance checks on the full set of winners' characteristics within each term-limit group. Appendix Table F2 reports the results for non-term-limited mayors and shows that none of the coefficients is statistically significant at conventional levels. This is reassuring, as this subsample drives our main results on COVID-19 deaths.

For term-limited mayors, we detect imbalances in two out of eleven characteristics: age at election and an indicator for having higher education (Appendix Table F3). A potential concern is that these imbalances may drive our heterogeneity results. For this to be the case, education (or age) would need to have an independent effect on COVID outcomes, and this effect would need to vary over time and operate in the opposite direction of our main gender effect, thereby producing a null effect in the term-limited sample.

We view this explanation as unlikely for three reasons. First, Appendix Table F4 shows that our subsample results are robust to controlling for the mayor's age and education. Second, as discussed in Section 3.3 and documented in Appendix Table A4, we find no effect of the mayor's education on COVID-19 outcomes when estimating a separate RDD comparing mayors with and without higher education. Third, when restricting the analysis to first-term mayors, the estimated effects do not differ systematically by education level or age (Appendix Tables E1 and E2). Taken together, the robustness of our results to additional controls and the absence of heterogeneous effects along other dimensions support the conclusion that the gender differences we document are driven by electoral incentives.

Table F1: General balance test - By term limit status

	(1)	(2)
Outcome	Predicted Female	
	Mayor can run	Mayor cannot run
Female	0.010	0.057
	(0.014)	(0.047)
Robust p-value	0.476	0.382
Observations	425	152
Polyn. order	1	1
Bandwidth	0.128	0.133
Mean, left of threshold	0.437	0.315

Notes: Column 1 (resp. 2) restricts the sample to municipalities where the mayor is not term-limited and thus allowed to run again in 2020 (resp. is term-limited and cannot run again). The outcome is the treatment variable predicted by municipal characteristics. We compute the outcome as follows: we first regress the treatment variable T on all 21 baseline variables presented in Table 1 and we then predict the treatment status of each municipality using the regression coefficients. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure and 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 F2: Balance test: Characteristics of the election winner - Mayor can run

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Outcome	Age	White	Higher	Occupation				Ideology	PMDB	PSDB	PT
			education	Politics	Public	Health	Business	score			
Female	0.262	0.099	0.075	0.068	0.013	-0.021	-0.006	0.077	0.055	-0.037	0.012
	(2.310)	(0.083)	(0.101)	(0.056)	(0.072)	(0.059)	(0.060)	(0.065)	(0.066)	(0.058)	(0.042)
R. p-value	0.781	0.276	0.780	0.162	0.955	0.764	0.964	0.358	0.579	0.512	0.681
Observations	396	408	360	385	388	410	528	543	472	398	398
Polyn. order	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	0.117	0.124	0.101	0.110	0.114	0.124	0.169	0.180	0.145	0.119	0.118
Mean	49.448	0.671	0.490	0.047	0.152	0.105	0.121	0.217	0.146	0.101	0.033

Notes: The sample is restricted to municipalities where the mayor is not term-limited. 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 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 winner's age at the time of the election. In Column 9, the outcome is the ideological score of the candidate's party, 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. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 F3: Balance test: Characteristics of the election winner - Mayor cannot run

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Outcome	Age	White	Higher	Occupation				Ideology	PMDB	PSDB	PT
			education	Politics	Public	Health	Business	score			
Female	-6.764***	0.039	0.416**	-0.111	0.060	-0.090	0.056	-0.013	-0.114	0.048	0.018
	(2.537)	(0.122)	(0.156)	(0.165)	(0.080)	(0.084)	(0.088)	(0.088)	(0.109)	(0.091)	(0.042)
R. p-value	0.010	0.829	0.043	0.499	0.420	0.343	0.413	0.778	0.164	0.785	0.740
Observations	105	183	96	112	120	183	113	102	91	106	114
Polyn. order	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	0.090	0.170	0.076	0.097	0.105	0.169	0.098	0.086	0.071	0.092	0.099
Mean	48.483	0.592	0.436	0.731	-0.008	0.143	0.031	0.267	0.145	0.090	-0.006

Notes: The sample is restricted to municipalities where the mayor is term-limited. 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 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 winner's age at the time of the election. In Column 9, the outcome is the ideological score of the candidate's party, 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. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric estimation procedure and 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 F4: Impact on COVID-19 deaths by mayor term-limit status, controlling for age and education

	(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
Female	0.414**	-0.866**	0.552***	-1.015**	-0.015	0.202
	(0.157)	(0.366)	(0.194)	(0.467)	(0.210)	(0.612)
Robust p-value	0.012	0.035	0.007	0.045	0.922	0.508
P-value (3)=(5)					0.049	
P-value (4)=(6)						0.116
Observations	523	482	358	361	126	117
Polyn. order	1	1	1	1	1	1
Bandwidth	0.115	0.104	0.101	0.102	0.112	0.101
Mean, left of threshold	0.219	2.395	0.171	2.775	0.291	1.509

Notes: In Columns 3 and 4 (resp. 5 and 6), the sample is restricted to municipalities where the mayor is not term-limited and thus allowed to run again in 2020 (resp. is term-limited and cannot run again). In columns 1, 3, and 5 (resp. 2, 4, and 6), the outcome is the total number of deaths per 10,000 inhabitants in period 1 (resp. in period 4). The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We further include the winner's age and education level as controls. We use a nonparametric estimation procedure and 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 of the outcome for male-led municipalities at the threshold.

G Model Appendix

In this section, we demonstrate that the key qualitative result of the model does not depend on the exponential baseline functional forms. It suffices that the damage and abatement functions f_1, f_2 are strictly increasing and strictly concave (with standard boundary conditions), and that the policy-cost function f_3 is strictly decreasing and positive.

Generalized Expressions and Regularity Conditions

Define the perceived public good

$$\tilde{g}(P, \lambda_s) \; = \; \overline{g} \; - \; \overline{g}\, f_1\Big(p\, \psi \left[1 - f_2(\lambda_s\, P)\right]\Big),$$

which generalizes equation (7) in the main text (i.e., it embeds voters' beliefs about the severity of the shock p and the gender-biased attribution λ_s).

Similarly, denote voters' utility as

$$U\big(P, \lambda_s; p\big) \; = \; \tilde{g}(P, \lambda_s) \; \cdot \; f_3(P), \tag{10}$$

which generalizes equation (8) in the main text.

Furthermore, assume the following regularity conditions:

(i) *Damage and abatement functions (f_1 and f_2).* The functions $f_1 : \mathbb{R}_+ \to [0, 1]$ and $f_2 : \mathbb{R}_+ \to [0, 1]$ are twice continuously differentiable, strictly increasing, and strictly concave, with boundary conditions $f_1(0) = 0$, $\lim_{x\to\infty} f_1(x) = 1$, and $f_2(0) = 0$, $\lim_{z\to\infty} f_2(z) = 1$.

(ii) *Policy-cost function (f_3).* The function $f_3 : [0, 1] \to \mathbb{R}_+$ is continuously differentiable, strictly decreasing, and positive on $[0, 1]$.

(iii) *Interior optimum.* For each fixed p, there exists a unique interior solution $0 < P_s^*(\lambda_s; p) < 1$ that maximizes $U(P, \lambda_s; p)$.

(iv) *Single-peakedness in P.* For each fixed (λ_s, p), the function $U(P, \lambda_s; p)$ is single-peaked in P; i.e., the second-order condition $\frac{\partial^2 U}{\partial P^2}(P, \lambda_s; p) < 0$ holds at the interior optimum.

(v) *Single-crossing in λ_s.* The function $\Phi(\lambda_s) \equiv \frac{\partial F}{\partial \lambda_s}\big(P_s^*(\lambda_s; p),\ \lambda_s;\ p\big)$, which is positive for small λ_s and negative for large λ_s (by boundary behavior and continuity), crosses zero exactly once.

We show next that, under these conditions, there exists a unique threshold $\widehat{\lambda}(p)$ such that the optimal policy P^* *increases* in λ_s below the threshold, and *decreases* in λ_s above the threshold.

First-Order Condition and Optimal Policy

Define the residual damage share as:

$$G(P,\lambda_s;p) \;\equiv\; f_1\Big(p\,\psi\,\big[1-f_2(\lambda_s\,P)\big]\Big). \tag{11}$$

which captures the share of the potential damage that voters perceive as materializing, after considering the mitigating effect of the (credit-weighted) abatement policy.

Equation (11) allows us to write $\tilde{g}(P,\lambda_s)=\overline{g}\,\big[1-G(P,\lambda_s;p)\big]$, and consequently rewrite the utility in equation (10) as:

$$U(P,\lambda_s;p) \;=\; \overline{g}\,\big[1-G(P,\lambda_s;p)\big]\;f_3(P).$$

Denote the partial derivative of U w.r.t. P as $F(P,\lambda_s;p)$, such that:

$$F(P,\lambda_s;p) \;\equiv\; \frac{\partial U}{\partial P}\big(P,\lambda_s;p\big) \;=\; \overline{g}\,\Big\{-\frac{\partial G(P,\lambda_s;p)}{\partial P}\,f_3(P) \;+\; \big[1-G(P,\lambda_s;p)\big]\,\frac{\partial f_3(P)}{\partial P}\Big\}, \tag{12}$$

Since f_2 is strictly increasing, it follows that $\dfrac{\partial G(P,\lambda_s;p)}{\partial P}<0$. Likewise, because f_3 is strictly decreasing in P, $\dfrac{\partial f_3(P)}{\partial P}<0$. Setting this derivative equal to zero for an interior solution we obtain:

$$-\frac{\partial G(P,\lambda_s;p)}{\partial P}\,f_3(P) \;=\; \big[1-G(P,\lambda_s;p)\big]\,\Big(-\frac{\partial f_3(P)}{\partial P}\Big),$$

where the corresponding interior solution $P_s^*(\lambda_s;p)$ is, per assumption (iv), a local (and global) maximum. This can be interpreted as a "marginal benefit = marginal cost" condition: the left-hand side is the marginal benefit of abating the shock (via reducing the residual damage G), while the right-hand side is the marginal cost coming from the policy disutility $(-\frac{\partial f_3}{\partial P}(P))$.

Non-Monotonicity and the Unique Threshold

We now examine how the optimal policy $P_s^*\big(\lambda_s;p\big)$ varies with λ_s.

Proposition 1 (Non-Monotonicity of P_s^* in λ_s)**.** *Let Assumptions (i)–(v) hold, and assume additionally that U is twice continuously differentiable in (P,λ_s) and continuously differentiable in p. Then there exists a unique threshold $\widehat{\lambda}(p)>0$ such that*

$$\lambda_s<\widehat{\lambda} \quad\Longrightarrow\quad \frac{\partial P_s^*}{\partial\lambda_s}\;>\;0, \qquad \lambda_s>\widehat{\lambda} \quad\Longrightarrow\quad \frac{\partial P_s^*}{\partial\lambda_s}\;<\;0.$$

Hence, P_s^* increases *with λ_s when λ_s is small but* decreases *once λ_s exceeds $\widehat{\lambda}$.*

Proof.

Note first that $F\big(P_s^*(\lambda_s;p),\ \lambda_s;p\big) = 0$, meaning that $P_s^*(\lambda_s)$ is chosen so that the marginal utility of P is zero. Since $\frac{\partial F}{\partial \lambda_s}$ captures how λ_s affects the marginal benefit from abatement, its sign depends on whether the abatement function $f_2\big(\lambda_s\, P\big)$ is in its *increasing* or *saturating* part. Strict concavity of f_2 ensures that as λ_s grows large, the marginal returns to further increases in P will taper off more quickly.

By standard continuity arguments, together with the strict concavity of f_1 and f_2, $\frac{\partial F\big(P_s^*(\lambda_s),\lambda_s\big)}{\partial \lambda_s}$ must start positive at low λ_s and eventually become negative at high λ_s. By Assumption (v), this transition occurs exactly once. Since U is single-peaked in P, this transition can only occur once. Denoting $\widehat{\lambda}$ the unique point where $\frac{\partial F\big(P_s^*(\widehat{\lambda}),\widehat{\lambda}\big)}{\partial \lambda_s} = 0$, we have that $\frac{\partial F}{\partial \lambda_s} > 0$ if $\lambda_s < \widehat{\lambda}$ and $\frac{\partial F}{\partial \lambda_s} < 0$ if $\lambda_s > \widehat{\lambda}$.

Lastly, to see how P_s^* changes with λ_s, we apply the implicit-function theorem, which gives

$$\frac{dP_s^*}{d\lambda_s} = -\frac{\dfrac{\partial F\big(P_s^*(\lambda_s;p),\ \lambda_s;p\big)}{\partial \lambda_s}}{\dfrac{\partial F\big(P_s^*(\lambda_s;p),\ \lambda_s;p\big)}{\partial P}} = -\frac{\dfrac{\partial F\big(P_s^*,\ \lambda_s;p\big)}{\partial \lambda_s}}{\dfrac{\partial^2 U\big(P_s^*,\lambda_s;p\big)}{\partial P^2}}.$$

Because $\frac{\partial F(P_s^*,\lambda_s)}{\partial P} = \frac{\partial^2 U(P_s^*,\lambda_s)}{\partial P^2} < 0$ (by Assumption (iv)) the sign of $\frac{dP_s^*}{d\lambda_s}$ is the *same* as the sign of $\frac{\partial F(P_s^*,\lambda_s;p)}{\partial \lambda_s}$. It follows that, for $\lambda_s < \widehat{\lambda}$ (where $\frac{\partial F}{\partial \lambda_s} > 0$), we have $\frac{dP_s^*}{\partial \lambda_s} > 0$. Conversely, for $\lambda_s > \widehat{\lambda}$ (where $\frac{\partial F}{\partial \lambda_s} < 0$), we have $\frac{dP_s^*}{\partial \lambda_s} < 0$. Hence the policy P_s^* increases with λ_s up to $\widehat{\lambda}$ and decreases thereafter.

□

Monotonicity of the Unique Threshold in p

Next, we consider the relationship between the threshold $\widehat{\lambda}$ and voters' beliefs on the likelihood of the crisis (p) under these general concavity assumptions. We first prove that the optimal policy is increasing in p, formally:

Proposition 2 (Monotonicity of the Optimal Policy in p)**.** *Let Assumptions* (i)–(iv) *hold, and assume additionally that, for each fixed λ_s, the marginal-utility function shifts up with p at the interior optimum; that is,*

$$\frac{\partial F}{\partial p}\Big(P_s^*(\lambda_s;p),\ \lambda_s;\ p\Big) > 0.$$

Then the interior solution $P_s^*(\lambda_s;\ p)$ that maximizes $U\big(P,\lambda_s;\ p\big)$ is strictly increasing in p.

Proof.

By Assumption (iv), there is a unique interior solution $P_s^*(\lambda_s; p)$ satisfying the first-order condition $F\big(P_s^*(\lambda_s; p),\, \lambda_s;\, p\big) \;=\; 0$, with $\frac{\partial^2 U}{\partial P^2} < 0$ at P_s^*, ensuring single-peakedness. We differentiate this condition with respect to p and apply the chain rule:

$$\frac{dF(\cdot)}{dp} \;=\; \underbrace{\frac{\partial F}{\partial P}\Big(P_s^*(\lambda_s; p),\, \lambda_s;\, p\Big)}_{<0 \text{ by concavity}} \cdot \frac{dP_s^*(\lambda_s; p)}{dp} \;+\; \underbrace{\frac{\partial F}{\partial p}\Big(P_s^*(\lambda_s; p),\, \lambda_s;\, p\Big)}_{>0 \text{ at } P=P_s^* \text{ by assumption}}.$$

Equalizing to zero and rearranging, we obtain

$$\frac{dP_s^*(\lambda_s; p)}{dp} \;=\; -\,\frac{\frac{\partial F}{\partial p}\big(P_s^*, \lambda_s; p\big)}{\frac{\partial F}{\partial P}\big(P_s^*, \lambda_s; p\big)}.$$

Because the second-order condition implies $\frac{\partial F}{\partial P}\big(P_s^*, \lambda_s; p\big) < 0$, while by assumption $\frac{\partial F}{\partial p}\big(P_s^*, \lambda_s; p\big) > 0$, their ratio is *negative*, and the extra minus sign makes $\frac{dP_s^*}{dp} > 0$. Thus, an increase in p raises the marginal benefit of policy at any fixed P, so the politician must optimally choose a higher policy level to restore the zero-marginal-utility condition.

□

Finally we show that under a mild condition on the marginal return to credit attribution, the threshold $\widehat{\lambda}(p)$ is strictly decreasing in p. Intuitively, when voters both (i) take the crisis more seriously (higher p) and (ii) attribute more policy credit (higher λ_s), the marginal return to further increases in λ_s tapers off faster around the optimum. This pushes the crossing point $\widehat{\lambda}(p)$ leftward as p rises.

Formally:

Proposition 3 (Monotonicity of the Threshold in p)**.** *Let Assumptions* (i)–(v) *hold, and suppose the interior optimum $P_s^*(\lambda_s; p)$ is strictly increasing in p, as in Proposition 2. Define the composite function $\Phi(\lambda_s, p) := \frac{\partial F}{\partial \lambda_s}\big(P_s^*(\lambda_s; p),\, \lambda_s;\, p\big)$, which captures the marginal return to credit attribution evaluated along the optimal policy path. Assume that Φ is strictly decreasing in p near the threshold; that is,*

$$\frac{\partial \Phi}{\partial p}\big(\lambda_s,\, p\big) < 0 \quad \textit{in a neighborhood of} \quad \big(\widehat{\lambda}(p),\, p\big).$$

This condition states that an increase in perceived crisis severity reduces the marginal return to credit attribution at the optimum, accounting for the politician's policy adjustment.[47] *Since* Φ

[47]A sufficient condition is that F is submodular in (p, λ_s) – i.e., $\frac{\partial^2 F}{\partial p\, \partial \lambda_s} < 0$ – and that the indirect effect

crosses zero from positive to negative as λ_s rises (Proposition 1), we also have $\partial\Phi/\partial\lambda_s < 0$ at the crossing. Then the unique threshold $\widehat{\lambda}(p)$ is strictly decreasing *in p.*

Proof.

By Proposition 1, there is a unique $\widehat{\lambda}(p)$ such that

$$\Phi\big(\widehat{\lambda}(p),\, p\big) \;=\; 0,$$

and this point separates the region where $P_s^*(\lambda_s; p)$ is increasing in λ_s from where it is decreasing in λ_s. To see how $\widehat{\lambda}(p)$ varies with p, we differentiate the above equation w.r.t. p. By the chain rule,

$$0 \;=\; \frac{d}{dp}\,\Phi\Big(\widehat{\lambda}(p),\, p\Big) \;=\; \underbrace{\frac{\partial\Phi}{\partial\lambda_s}\Big(\widehat{\lambda}(p),\, p\Big)}_{\text{A}}\cdot\widehat{\lambda}'(p) \;+\; \underbrace{\frac{\partial\Phi}{\partial p}\Big(\widehat{\lambda}(p),\, p\Big)}_{\text{B}}.$$

Thus,

$$\widehat{\lambda}'(p) \;=\; -\,\frac{\text{B}}{\text{A}} \;=\; -\,\frac{\frac{\partial\Phi}{\partial p}(\widehat{\lambda}(p), p)}{\frac{\partial\Phi}{\partial\lambda_s}(\widehat{\lambda}(p), p)}.$$

Since Φ crosses zero from positive to negative as λ_s rises (Proposition 1), we have $A = \frac{\partial\Phi}{\partial\lambda_s}(\widehat{\lambda}(p), p) < 0$ at the crossing. Moreover, the assumption that Φ is strictly decreasing in p near the threshold gives $B = \frac{\partial\Phi}{\partial p}(\widehat{\lambda}(p), p) < 0$. Since both A and B are negative, $\widehat{\lambda}'(p) = -B/A < 0$.

□

through the optimal policy response, $\frac{\partial^2 F}{\partial P\,\partial\lambda_s} \cdot \frac{\partial P_s^*}{\partial p}$, does not reverse this. The exponential specification used in the main text satisfies this condition.