Gender and Electoral Incentives: Evidence from Crisis Response*

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March, 2025

Abstract

This paper provides new evidence on why men and women leaders make different choices. We first show theoretically how voters' gender bias can lead female politicians to undertake different policies. We then test the model's predictions by exploring leaders' responses to COVID-19. Exploiting Brazilian close elections, we find that, consistent with the model, female mayors were less likely to close non-essential businesses at first and that female-led municipalities experienced higher mortality, while the reverse was true later on. These results are exclusively driven by mayors facing reelection and are stronger in municipalities where voters' bias is more likely to materialize.

JEL Codes: D72, J16, H11.

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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 there is evidence that female politicians make different choices, and evidence of the presence of voters' gender bias (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. We first outline a simple political agency model to illustrate how voters' gender bias and electoral incentives can push female and male leaders to undertake different policies. Assuming voters' bias in a crisis context, the model generates specific testable predictions in terms of policy choices by gender. We test them empirically using local daily data on COVID-19 deaths and policies in Brazil, and exploiting variation in electoral incentives across mayors and heterogeneity in municipalities' characteristics.

In the model, voters care about a public good. The politician, who cares about reelection, can enact policies that mitigate the incoming shock to the public good but that also generate a direct cost to voters.[1] Our model assumes that voters evaluate female leaders less favorably than male leaders. More specifically, we assume that voters expect policies to be less effective if decided by a female politician, such that female politicians receive less credit for the same policy decisions.[2] Crucially, the model delivers opposite predictions depending on voters'

[1]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.

[2]This bias can arise as long as there is uncertainty about the mapping between policies and outcomes. In our setting, voters are uncertain about the effect of containment policies on COVID-19 and, if their leader is a

beliefs about the likelihood of the shock. First, when voters believe that there is a low probability that the shock will materialize – such as early in the pandemic – they are less willing to accept containment policies and even less so if decided by female politicians who are perceived as less effective. 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, and female mayors need to do more to achieve the same level of perceived safety. The intuition of the model is also in line with the idea that women are more likely to be blamed for mistakes – as found in other contexts (e.g., Egan et al., 2022; Sarsons, 2022) –, which would ultimately lead female politicians to adopt the decisions most aligned with voters' preferences.

Turning to the empirical analysis, we first test the main prediction of the model: that female politicians undertake lower containment efforts at the beginning of the pandemic, and that the reverse is true as the crisis unfolds. We focus on Brazilian mayors, who could independently decide over containment policies and who faced new municipal elections at the end of 2020. Using daily panel data at the municipal level, we explore female and male mayors' responses to the crisis throughout the last year of their term.

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.

woman, they tend to be more skeptic about policy effectiveness.

Our main outcome is the number of COVID-19 deaths in the municipality. Consistent with the model prediction, 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),[3] having a female mayor led to 0.50 more deaths per 10,000 inhabitants, representing a fourfold increase compared to the average number of deaths in male-led municipalities at the threshold. Instead, at the end of the year (November-December 2020), female-led municipalities experienced one fewer death per 10,000 inhabitants, corresponding to a 41.6 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 2.9 and 9.8 fewer days in female-led municipalities in March and April 2020, as female mayors started closing non-essential businesses 56 days later on average. In contrast, commerce restrictions were in place 8.1 more days in female-led municipalities in both September and October 2020.[4]

These results are consistent with the main prediction of the model and show that the gender differences in policymaking we observe 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. We next provide further evidence that the observed differences are instead driven by reelection concerns and voters' bias.

We start by showing that the gender differences in crisis response we observe are driven by mayors with electoral incentives. We consider two measures of mayors' electoral incentives. First, we exploit the two-term limit and compare mayors who were elected for

[3]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.

[4]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.

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 reelection. 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. Second, we explore the heterogeneity of the effect with respect to the competitiveness of the race, a measure of electoral incentives that does not rely on past experience. Departing from the RD framework and using an OLS estimation, we show that the effects are stronger in municipalities where the mayor won with a small victory margin in 2016 and where the next election is thus likely to be more competitive. These results are consistent with electoral incentives explaining gender differences in leaders' behavior. They also suggest that, when gender differences in policymaking are due to female and male leaders facing different electoral incentives, the effects captured by close election designs are likely to dissipate in less contested races.

Finally, we provide two additional pieces of evidence supporting the interpretation that female mayors with electoral incentives acted in response to voters' bias. First, we show that our results are stronger in municipalities where voters' gender bias is the most likely to materialize: in municipalities where the gender wage gap is higher, and in municipalities that elected a lower share of female councilors in the past. Second, we find no gender differences in the probability of running or being reelected, or in vote share in the 2020 election. Absent voter discrimination, taking the actions most aligned with voters' preferences should help female incumbents secure a higher vote share. Instead, this null result is consistent with male and female mayors optimizing their policy choices by factoring in gender biases in voters' assessments.

Taken together, these results support the fact 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 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.[5] 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.[6]. 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.[7] Only a few recent papers focus on gender differences in leadership in crisis contexts. Dube and Harish (2020) find that European queens were historically more likely to be at war than kings, and Eslava (2021) 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.

Our paper expands this literature in two significant ways. First, we investigate gender differences across time. We do so by using daily data not only on COVID-19 deaths but

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

[6]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)

[7]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.

also on policy, leveraging a novel dataset constructed directly from laws and decrees.[8] We show that gender differences in crisis response can vary and even reverse over time. This is important, as we can reach opposite conclusions depending on when we assess the impact of female leadership.[9] Second, we go beyond documenting gender differences in outcomes and policymaking, and focus on investigating where they stem from. We do so by providing a theoretical framework that highlights the role of voters' gender biases, and empirical evidence aligned with the framework's predictions. 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).[10] 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 "feminine" issues such as child care and education (Herrnson et al., 2003; Lawless, 2004; Beaman et al., 2009; Eggers et al., 2018). Using quasi-experimental evidence from Italy, Gagliarducci and Paserman (2012) show that female mayors are more likely to experience an early termination of their mandate in regions where people display less-favorable attitudes towards working women, and

[8] 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).

[9] For instance, as shown in Figure 3, looking at total deaths in June would lead to the conclusion that having a female mayor increases mortality, whereas looking at total deaths in December would lead to the reverse conclusion.

[10] 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).

Daniele et al. (2023) find that they are more likely to be the targets of violent attacks.[11] 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.[12] 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 reelection exert more effort than term-limited ones (Besley and Case, 1995; List and Sturm, 2006; Sieg and Yoon, 2017; Fouirnaies and Hall, 2022).[13] 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 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., 2023), 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

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

[12]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).

[13]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.

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).

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 illustrates with a simple model how voters' bias can lead to gender differences in policymaking. Section 3 presents the setting and data, and Section 4 describes the sample and empirical strategy used to test the model predictions. We present the results showing gender differences in the evolution of COVID-19 deaths and policies in Section 5 and additional analyses supporting the main mechanism in Section 6. Section 7 concludes.

2 A Simple Model of Political Agency With Voter Bias

Public Good

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

Society faces an emerging shock $\psi > 0$ 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.

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

$$g = \overline{g} - \overline{g}\, f_1(\psi_m) \; , \tag{1}$$

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

$$f_1(\psi_m) = 1 - exp(-\psi_m) \; . \tag{2}$$

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

$$\psi_m = \psi\,(1 - f_2(P))\,, \tag{3}$$

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

$$f_2(P) = 1 - \exp(-\lambda P)\,, \tag{4}$$

and where $\lambda \geq 1$ is a parameter that captures the effectiveness of the politician's actions at mitigating the effects of the shock.[14]

Combining equations (1) to (4) yields the following production function for the public good as a function of the original size of the shock ψ, the policy level P, and the policy effectiveness λ:

$$g = \overline{g}\ \exp(-\psi\ \exp(-\lambda P)\,)\,. \tag{5}$$

The production function's functional form embeds two main assumptions. First, the concavity of the damage function (equation 2) 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 spread (Kermack and McKendrick, 1927): as a larger share of the population becomes infected, a smaller proportion remains susceptible to new infections. Second, the concavity of the abatement function (equation 4) implies that the policy intervention has decreasing marginal returns.[15] 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 additional restrictions create compliance fatigue, reducing their marginal impact. [16]

[14]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).

[15]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.

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

Voters' Utility

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

$$U = \tilde{g} \exp(-P) \,, \tag{6}$$

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

Equation 6 reflects the tradeoff of containment policies: on the one hand, they increase voters' utility by preserving the public good, but on the other, they impose a direct cost on voters by closing the economy and limiting freedom. Importantly, the disutility caused by the policy enters Equation 6 multiplying $\tilde{g}$, such that the larger the anticipated level of public good, the more disutility the policy generates. This captures voters' higher willingness to accept containment policies if the shock is perceived as more severe, in line with recent survey evidence across 15 countries showing that the willingness to sacrifice civil liberties increases with the perception of health insecurity (Alsan et al., 2023). While our model is motivated by the COVID-19 pandemic context, it can apply to other crisis contexts involving policy interventions that can be costly to voters, such as fiscal austerity policies or environmental policies.

Voters' Beliefs

The level of public good that voters anticipate is based on their subjective beliefs about (a) the likelihood of the shock and (b) the expected effectiveness of the policy, such that $\tilde{g}$ can differ from g.[17]

With respect to (a), voters believe that the shock will happen with probability $0 \leq p \leq 1$, such that $p\psi$ captures how severe voters believe the shock will be. Only when $p = 1$ do voters believe that the full potential of the shock (ψ) will materialize.[18]

With respect to (b), we assume that voters have gender-biased expectations about the effectiveness of policies at mitigating the shock: they expect that crisis containment policies will be less effective if decided by a female politician than if decided by a male

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

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

politician, such that $\lambda_f < \lambda_m$. This captures the idea that there is uncertainty about the effect of containment policies and that voters tend to be more skeptic about policy effectiveness if their leader is a woman. This assumption is based on 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 perceive women as strong leaders, especially in crisis contexts, where "male" attributes are particularly valued (Eagly and Karau, 2002; Bertrand and Duflo, 2017). As a consequence, female leaders get less credit for the same policy decisions.

Incorporating these two assumptions into the production function (equation 5), we can re-write voters' utility (equation 6) as:

$$U = \bar{g}\ \exp(-p\ \psi\ \exp(-\lambda_s P)\ )\ \exp(-P)\ , \tag{7}$$

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

Optimal Policy and Comparative Statics

We assume that politicians seek to maximize their likelihood of reelection. Since that is an unobserved positive function of voters' utility, politicians optimize it by choosing the policy level that maximizes voters' utility (Equation 7).[19] This yields the following optimal policy equation:[20]

$$P_s^* = \frac{1}{\lambda_s}\log(\lambda_s p \psi)\ . \tag{8}$$

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 a lower expected effectiveness (λ_s) to policies decided by women. We are thus interested in analyzing how the optimal policy level varies with λ_s.

The optimal policy level is a non-monotonic function of the perceived policy effectiveness: P_s^* is increasing in λ_s at low values of λ_s, while P_s^* is decreasing in λ_s at high values

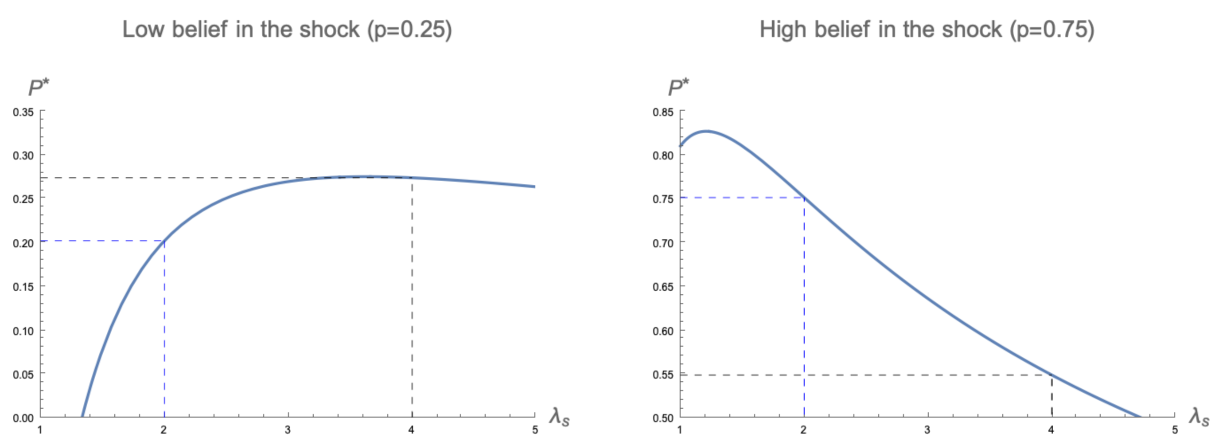
[19]Specifically, we assume that voters will reelect a politician if their utility – a function of the politician's policy choice – is higher than their reservation utility. We assume that politicians know voters' preferences and beliefs, but do not observe their reservation utility. Their best strategy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

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

of λ_s. 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 perceived as less effective.

Figure 1 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 to happen (low p), and one in which voters take the threat seriously and believe that the shock is likely to materialize (high p). We see an inverse relationship between P_s^* and λ_s across the two graphs. When voters believe that the probability p of a shock is small (left graph), the level of optimal policy P_s^* is increasing in λ_s over much of the support of λ_s, meaning that female politicians choose a lower level of policy than male politicians (as $\lambda_f < \lambda_m$). In contrast, when voters believe that the probability p of a shock is large (right graph), the level of optimal policy P_s^* is decreasing with λ_s over much of the support of λ_s, meaning that female politicians choose a higher level of policy.

Figure 1: Optimal policy (P^*) as a function of voters' gender-biased expectations about policy effectiveness (λ_s) at different levels of belief in the likelihood of the shock (p)



Notes: The figure plots the optimal level of policy chosen by the politician as a function of voters' beliefs about the expected effectiveness of containment policies under two scenarios – one in which voters believe that the shock has a low probability of occurring ($p = 0.25$) and one in which voters believe it has a high probability of occurring ($p = 0.75$). 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 a low ($\lambda_s = 2$) versus high ($\lambda_s = 4$) perceived policy effectiveness.

Intuition

The reversal illustrated in Figure 1 comes from the joint effect of the expected policy effectiveness and the belief about the severity of the shock on the marginal utility of the policy. Consider first the role of expected policy effectiveness. A higher effectiveness means a higher marginal utility of the first units of policy, but it also implies that the marginal utility decreases more rapidly with P due to diminishing 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 expect policies enacted by male politicians to be more effective at preserving the public good, there comes a point beyond which additional policies are seen as less necessary than they would be if enacted by a female politician.

Consider now the role of voters' belief about the crisis severity, which is captured by the probability p of shock ψ. When voters believe that the probability 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 probability of the shock is high, the demand for policy is high. At high levels of P, the marginal utility is larger for female politicians and it reaches zero faster for male politicians. This is illustrated in Appendix Figure A1, 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, voters are less willing to accept containment policies, and even less so if the politician is a woman, as her policies are expected to be less effective. 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 to compensate for voters' higher skepticism about the effectiveness of their policy interventions. More broadly, the model is consistent with women being judged more harshly and having more to lose: they are more likely to be punished for "doing too much" when the crisis is perceived as unlikely, and for "doing too little" when the crisis is taken seriously, in line with recent evidence showing that women are more likely to be punished for mistakes (e.g., Egan et al., 2022; Sarsons, 2022).

Model Predictions

We now link the model's main comparative statics to our empirical setting. The model predicts that the direction of the gender differences in containment efforts will depend on how severe voters perceive the shock to be.

In Brazil, the perception of the COVID-19 pandemic significantly evolved over the year 2020. The authorities announced the first COVID-19 case on February 26, 2020, and the first death three weeks later, on March 17 (Appendix Figure A2). Given its unprecedented nature, the start of the pandemic was characterized by a high degree of skepticism. Moreover, Brazil's president publicly downplayed the gravity of the epidemic, fueling the public's doubts and reducing individuals' efforts to contain the spread of the disease (Ajzenman et al., 2023). In a survey conducted by IPSOS at the beginning of April, 85 percent of Brazilian respondents expected things 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 (Ipsos, 2020a). The first months of the pandemic in Brazil can thus be characterized as a period of relatively low perceived severity of the crisis (i.e. low p).

The consequences of the pandemic became much more salient to Brazilians during the peak of the first wave. Starting in May, the disease expanded exponentially across the country and so did the death toll (Roser et al., 2020). By the end of that month, seven out of ten Brazilians did not agree with reopening non-essential businesses (Ipsos, 2020b). The number of deaths remained at the highest level worldwide from June through August. By the end of the first wave, Brazil's cumulative deaths exceeded those reported by the UK and were then second only to those reported by the US. The second half of 2020 can thus be characterized as a period of high p, when the crisis was perceived as serious.[21]

In this context, the model predicts that female politicians should have undertaken less containment effort at the beginning of the pandemic than male politicians, while the reverse should have been true as the crisis unfolded. Note that this prediction is at odds with what a story purely based on policy preferences would predict. If for instance, female politicians had a stronger preference for health policies, we would expect them to consistently enact more containment policies throughout the year.

[21]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.

3 Setting and Data

3.1 Brazilian Local Governments

Brazil is divided into 5,570 municipalities, the lowest subnational government tier in the country.[22] Municipal governments are in charge of providing public services of local interest, including water and sanitation, transportation, basic education, and – importantly for this paper – public health. Municipalities' expenditures were 18.9 percent of total public spending in 2019. Their revenues come mainly from constitutionally mandated intergovernment transfers (56.7 percent of total municipal revenues in 2019), followed by local taxes and user fees (IBGE, 2020).

The constitution recognizes municipalities as "federal entities," which gives them the status of autonomous governments, with the ability to independently decide local policies. 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 the COVID-19 pandemic, the national congress reaffirmed municipalities' power to implement containment policies (Law Nº 13.979). The Brazilian Supreme Court further ruled that the federal government could not overrule the policies of local governments (Decision ADPF 672). The federal government primarily implemented social assistance programs and border restrictions, while largely refraining from imposing restrictions on mobility and gatherings within the country. Meanwhile, multiple states and municipal governments declared states of emergency and implemented containment policies such as commerce closures. Only a few local governments decided to go further and implement curfews and lockdowns. Section 5.2 and Appendix B3 provide more details about containment policies across municipalities and over time.

[22]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.

3.2 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.[23] Since a constitutional amendment introduced in 1997, mayors are subject to a two-term limit, meaning that mayors serving a second term cannot run for reelection.

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,[24] 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.[25]

Participation by female candidates in the last two municipal elections was higher than in prior ones, but remained small. The share of female mayoral candidates in the 2016 (2020) elections was 12.9 (13.5) percent and only 11.5 (12.1) percent of the elected mayors were female. This was a small improvement relative to 2000, when women made up 7.6 percent of mayoral candidates and 5.7 percent of elected mayors (TSE, 2021). This political participation gap is also observed in congressional elections: in 2020, the share of congresswomen in Brazil's parliament was 14.6 percent, less than half of the averages for Latin America (32.8 percent) and the OECD (31.5 percent) (The World Bank, 2021).[26]

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

[24] 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.

[25] 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.

[26] 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).

3.3 Data

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

COVID-19 deaths. Our main outcome, the number of COVID-19 deaths, comes from Brasil.io. This open-data platform collects, cleans, and assembles the COVID-19 information provided by the state health secretaries and makes it publicly available as a daily municipal-level panel (Justen, 2021). We focus on deaths rather than cases. Deaths are less likely to go unrecorded and are thus considered a more reliable measure of the spread of COVID-19 as well as of the spread of other major pandemics (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 4.1.

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.[27]

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

[27]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.

articles, and used them to construct a daily panel of indicator variables that denote whether a given policy was in place in the municipality on a given day. Finally, we validated the quality of the text algorithm by using a testing dataset built manually for a random subset of municipalities. One challenge is that some municipalities might not have systematically released their laws online, which would limit our ability to capture all policies enacted over our period of interest. In particular, while it was more common to find a dedicated online repository for COVID-19 legislation in larger municipalities, collection turned out to be harder for smaller municipalities, likely due to scarcer resources and lower institutional capacity. We thus focus our policy analysis on municipalities with over 10,000 inhabitants, accounting for 50 percent of our sample.[28]

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, and restrictions on transport and travel. 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 B2 and B3), providing little variation with which to identify the effects of interest. We therefore focus our analysis on the remaining six policies. See Appendix B3 for a more extensive discussion of the policy data.

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 B4 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. Most of these baseline variables are con-

[28]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.

structed directly from the microdata of the 2010 demographic census (the last one before the 2016 elections).[29] We made sure to include variables that have been shown to predict the geographic variation in COVID-19 deaths, such as population density, the share of residents above 65 years old, proximity to internationally connected airports, the number of nursing home residents, and household income (Chauvin, 2024).[30]

4 Empirical Strategy

4.1 Sample and Descriptive Statistics

To estimate the causal impact of female leadership, we use a regression discontinuity design (RDD) and compare municipalities where a female candidate narrowly defeated a male candidate to those where a male candidate narrowly defeated a female candidate. We thus restrict our sample to the 22.4 percent of Brazilian municipalities where the top two contenders in the 2016 election were one female and one male candidate.[31]

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

Our final sample consists of 981 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.

[29] 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.

[30] 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.

[31] 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 B4 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[32] and based on the first round of the 2018 presidential election for the vote share of the president at the municipal level. Municipalities in our sample had 13,928 inhabitants on average in 2010; the average monthly median household income per capita was 319 reais (56.2 US dollars at the contemporary exchange rate); and 2.6 candidates ran in the 2016 elections on average.

Table 1: Descriptive statistics

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

Notes: The sample includes municipalities outside of 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, except density, which is defined as the total population living within 10 km of the average inhabitant of the municipality and which is computed using the 2015 data from the Global Human Settlement Layer. The political variables are computed using the results of the first round of the 2016 municipal election, except for *President's vote share*, which uses data from the first round of the 2018 presidential election.

[32] All municipalities in our sample had fewer than 200,000 eligible voters and thus had only one round.

Representativeness. Appendix Table A1 compares our sample to the rest of the country. Although municipalities in our sample are on average smaller and less dense than the average Brazilian municipality, 60 percent of the residents in our sample live in urban areas. Moreover, the average municipality in our sample is very similar in all the other socio-demographic and political characteristics to the average Brazilian municipality.

Our sample is also representative of the evolution of COVID-19 in Brazil. Appendix Figure B1 plots the number of COVID-19 deaths over time separately for our sample and for all Brazilian municipalities and shows that the two samples experienced a similar number of deaths per capita throughout the period of analysis.

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 to 95 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.

4.2 Specification

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

$$Y_i = \alpha_i + \tau T_i + \beta_1 X_i + \beta_2 X_i T_i + \gamma W_i + \mu_i \, , \tag{9}$$

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, municipalities are equally distributed around the threshold, with 52 percent of municipalities close to the threshold electing a female mayor (right of the discontinuity), and 48 percent electing a male mayor (left of the discontinuity).[33] In Section 6, we perform subsample analyses to shed light on the mechanisms, and we investigate how the effects vary when we move away from the threshold, thus considering less competitive elections.

4.3 Validity of the Design

4.3.1 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 19 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.

[33]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 19 is significant at the 10-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.

4.3.2 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.[34]

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 2 and Appendix Figure C4), even though the effect is not significant. In contrast, female

[34]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).

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 A2).

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. Two pieces of evidence suggest, however, that this is unlikely to explain our results. First, as shown in Appendix D, while female candidates are indeed more educated on average, the results remain unchanged when controlling for education and for other characteristics likely correlated with ability, such as political experience. Second, we run 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 A14).

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	-0.052	-0.856	0.107	0.143	-0.037	0.035	-0.032	-0.001	0.079	0.020	0.017	0.010
	(0.078)	(1.545)	(0.074)	(0.097)	(0.074)	(0.059)	(0.048)	(0.052)	(0.059)	(0.062)	(0.050)	(0.034)
R. p-value	0.508	0.620	0.214	0.329	0.597	0.670	0.604	0.947	0.313	0.898	0.677	0.729
Observations	577	703	579	410	518	514	534	591	664	511	542	511
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	0.132	0.175	0.133	0.088	0.121	0.118	0.124	0.137	0.157	0.117	0.126	0.117
Mean	0.264	49.017	0.645	0.442	0.216	0.112	0.125	0.102	0.210	0.153	0.060	0.022

Notes: In Column 1 (resp. 3, 4, 5, 6, 7, 8, 10, 11, 12), the outcome is an indicator variable equal to 1 if the winner 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.

5 Gender differences in crisis response over time

5.1 COVID-19 Deaths

In this section, we use the empirical strategy described above to test the model main prediction, namely that female mayors undertook less containment effort at the beginning of the pandemic, but more containment effort as the crisis unfolded. Our first and main outcome is the number of COVID-19 deaths.

To measure gender differences over time, we first split the year 2020 into four periods characterizing the evolution of COVID-19 in Brazil (see Appendix Figure B1). We look at the impact on the total number of deaths in each period separately: 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). We next look at the impact on COVID-19 deaths month by month, before estimating the effect on cumulative deaths day by day.[35] We normalize the number of deaths by the 2010 population and multiply by 10,000 so that the outcome measures the number of deaths in the municipality per 10,000 inhabitants.

Table 3 shows that, on average, having a female mayor led to a 0.50 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 fourfold increase compared to the average number of deaths in male-led municipalities at the threshold. Conversely, we find that female-led municipalities experienced, on average, one fewer death per 10,000 inhabitants in the last period. This effect is significant at the five-percent level and corresponds to a 41.6-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.

[35]Note that having a female mayor did not affect the timing at which municipalities started to experience fatalities (Appendix Table A3 and Figure A4), 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.)

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	0.497***	-0.064	-0.381	-0.996**
	(0.164)	(0.440)	(0.303)	(0.392)
Robust p-value	0.003	0.890	0.214	0.021
Observations	466	524	538	495
Polyn. order	1	1	1	1
Bandwidth	0.103	0.122	0.125	0.111
Mean, left of threshold	0.169	2.560	1.500	2.397

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.

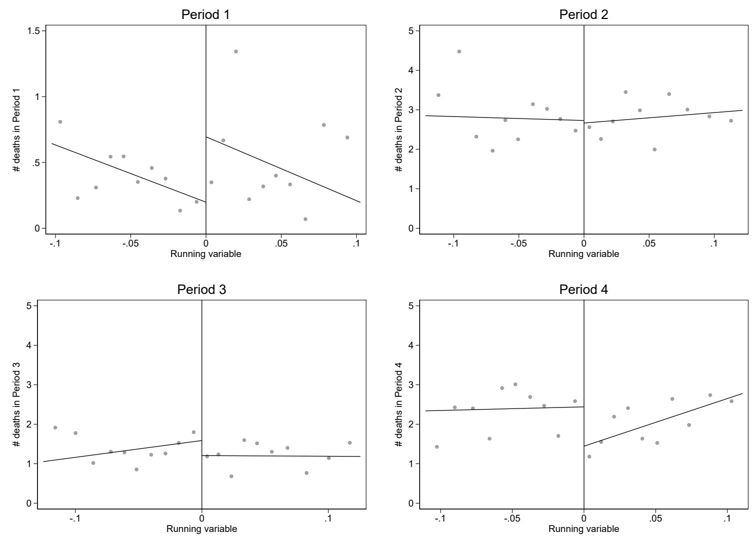
Figure 2 plots the number of deaths against the running variable for each period separately. Each dot represents the average value of the outcome within a given bin of the running variable, and each bin contains approximately 25 municipalities. Consistent with the formal estimation, we see an upward jump at the threshold at the beginning of the first wave that turns into a downward jump later in the year.

Zooming in month by month, Appendix Table A4 and Figure A5 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.

While a higher initial mortality rate could mechanically lead to fewer deaths later due to reversion to the mean (as the population develops immunity and the most vulnerable individuals have already died), such a mechanical effect is unlikely to explain the impact in period 4. As shown in Appendix Figure A6, there is no correlation between the number of deaths at the beginning and at the end of the year 2020. This is due to the fact that infections were still at very low levels in period 1, making it implausible that female-led municipalities reached herd immunity earlier in the pandemic.

Finally, we look at how these effects translate into the evolution of the number of cumulative deaths. Figure 3 shows, for each day from April 1 to January 31, the estimated impact of having a female mayor on the number of deaths up to that date. 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.[36]

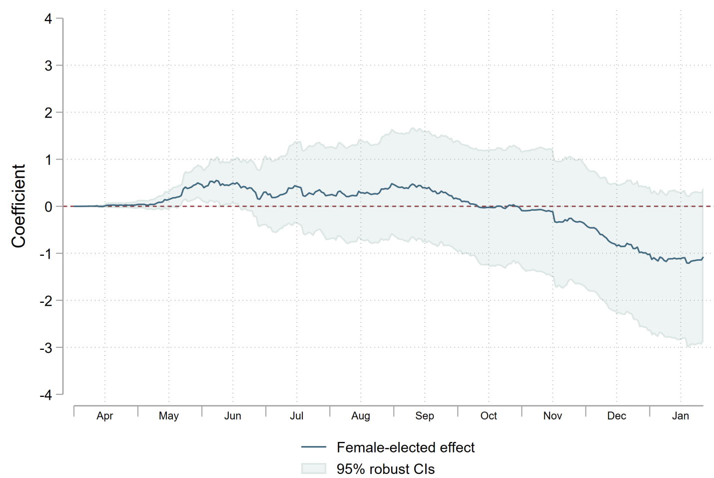
Figure 2: Impact on COVID-19 deaths, by period



Notes: Each graph is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the number COVID-19 deaths per 10,000 inhabitants in the municipality during the period of interest. Averages are calculated within quantile spaced bins of the running variable. The running variable is the 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).

[36]Looking at the number of deaths over the whole period, we find that having a female mayor reduced cumulative deaths by 1.08 per 10,000 inhabitants as of January 31, 2021 (16.0 percent), on average, but the coefficient is not statistically significant (Appendix Table A5 and Appendix Figure A7).

Figure 3: 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.

The impact of female leadership on the evolution of COVID-19 deaths is robust in both magnitude and significance to the inclusion of mayors' characteristics other than gender; to the inclusion of state fixed effects; to not controlling for municipality characteristics; to the exclusion of unusual observations (Mato Grosso state and supplementary elections); and to specification choices (use of a second polynomial order and different bandwidths). In addition, the same patterns are found if we use as outcome the overall number of SARI deaths (as defined in Section 3.3). Appendix D describes the robustness tests in more detail and presents the corresponding tables and figures.

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, consistent with the model main prediction. To further support this interpretation, we now investigate the impact of female leadership on containment policies.

5.2 Containment Policies

As discussed in Section 3.3, 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 18 days later than male mayors on average, and female-led municipalities were 31.5 percentage point less likely to have a policy in place early on, compared to a baseline of 77.7 percent for male-led municipalities at the threshold.

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	18.323**	-0.315***
	(8.849)	(0.114)
Robust p-value	0.042	0.009
Observations	284	264
Polyn. order	1	1
Bandwidth	0.157	0.117
Mean, left of threshold	98.270	0.777

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.

We next investigate the implementation of each of the six policy over time. Figure 4 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".[37]

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 2.9 and 9.8 fewer days in female-led municipalities in March and April, respectively, compared to an average of 3.8 and 13.6 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 56 days later on average (Appendix Table A6, Column 1). Instead, non-essential businesses were closed on average 8.1 more days in female-led municipalities in both September and October. 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 16.5 percentage points less likely to reopen non-essential businesses between August and October 2020 (Appendix Table A6, Column 2).

Appendix Figure A8 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.

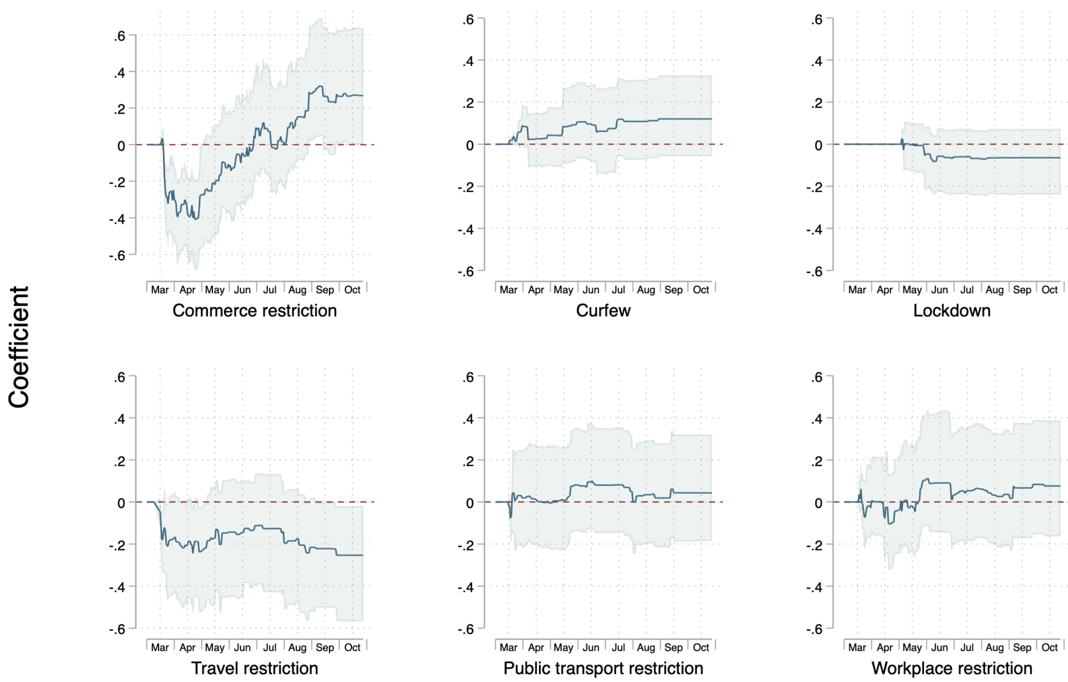
The timing of the policy results aligns well with the evolution of the number of COVID-19 deaths: female mayors were less likely to close commerce in March and April and female-led municipalities experienced more deaths in May; they became more likely than male mayors to close commerce in September and October and their municipalities experienced fewer deaths in November and December. Moreover, the higher likelihood of closing

[37]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 B3). 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.

non-essential businesses later in the year is unlikely to come mechanically from a higher number of deaths in the earlier period. Indeed, as shown in Appendix Figure A9, the correlation between the two variables is negative and not statistically significant.

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, in line with the model prediction.

Figure 4: 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.

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	-2.941***	-9.755***	-6.100	-1.751	1.577	3.708	8.108**	8.112**
	(0.949)	(2.454)	(3.581)	(3.699)	(3.776)	(3.747)	(4.356)	(4.345)
Robust p-value	0.003	0.000	0.176	0.820	0.483	0.227	0.049	0.047
Observations	251	285	237	226	207	212	191	198
Polyn. order	1	1	1	1	1	1	1	1
Bandwidth	0.106	0.130	0.098	0.093	0.085	0.087	0.077	0.079
Mean, left of threshold	3.831	13.566	13.469	12.386	12.167	9.212	7.723	7.178

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.

6 Mechanisms

6.1 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 reelection. We consider two measures of electoral incentives. First, we exploit the two-term limit and compare mayors who could run for reelection to those who could not. We then run a second test that does not rely on mayors' incumbency status: departing from the RDD framework, we test whether our results vary with the margin of victory and thus with the competitiveness of the election.

In the rest of the paper, 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.

Term Limits

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

In order to test whether the results are driven by mayors who *can* run for reelection, Table 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 of having a female mayor on the probability that the 2016 winner is the incumbent (Section 4). We replicate our main analysis on COVID-19 deaths in each subsample separately.

Table 6: Impact on COVID-19 deaths, by mayor term-limit status

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths per 10,000 inhabitants					
	Full sample		Mayor can run		Mayor cannot run	
Periods	1	4	1	4	1	4
Female	0.497***	-0.996**	0.600***	-1.178**	0.095	0.004
	(0.164)	(0.392)	(0.201)	(0.505)	(0.215)	(0.625)
Robust p-value	0.003	0.021	0.004	0.030	0.752	0.980
P-value (3)=(5)					0.089	
P-value (4)=(6)						0.144
Observations	466	495	332	366	108	110
Polyn. order	1	1	1	1	1	1
Bandwidth	0.103	0.111	0.094	0.107	0.097	0.099
Mean, left of threshold	0.169	2.397	0.159	2.707	0.158	1.543

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.

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 0.60 in municipalities where the mayor can run for reelection, an effect significant at the one-percent level (Column 3). Instead, the point estimate is small and not significant in municipalities where the mayor is term limited (0.095, Column 5). The difference between the two estimates is statistically significant. In period 4, the result is similar in magnitude and significance as in the full sample for municipalities where the mayor can run again (-1.2, Column 4), whereas the effect is not significant, positive 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 0.144).

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 during the previous term, suggesting that less-able or less-experienced mayors are not driving the results (Appendix Tables A7 to A9).[38] Third, as shown in the next section, we also find that our results are driven by more-competitive elections, a proxy for electoral incentives that does not rely on term limits.

Election Competitiveness

Next, we run heterogeneity analyses based on an alternative measure of electoral incentives: the competitiveness of the race. By construction, the RDD focuses on mayors who won by a small margin in 2016 and who are thus likely to face more competition in the next election than mayors who secured a large victory margin. If electoral incentives are driving our results, we would expect the effects to be larger for the former.

[38] First-time mayors could have served as municipal legislators during the previous term. Elections for municipal legislators and for mayors happen at the same time and involve the same voters. While the subsample becomes very small, Appendix Table A9 shows that the coefficients are not smaller in magnitude for mayors who served as legislators during the 2012-2016 term.

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

As shown in Table A10, the impact of the treatment goes in the same direction as in the main RDD analysis for both periods. In the full sample, the point estimates associated with the treatment are small and nonsignificant (Columns 1 and 4). The effects become large and significant when we restrict the sample to more competitive elections – those with a victory margin smaller than 10 or 5 percentage points (Columns 2 and 5 and Columns 3 and 6, respectively). More interestingly, in all regressions, the coefficient of the interaction term is negative in period 1 and positive in period 4. Focusing on elections won by a vote margin smaller than 10 percentage points, we see that the estimates associated with the treatment are very close to those obtained with the RDD (0.37 for period 1 and -0.95 for period 4; Columns 2 and 5). This is reassuring, as these effects can be interpreted as the impact of having a female mayor when the vote margin is zero, which corresponds to the impact at the discontinuity estimated with the RDD. The coefficient of the interaction term further shows that the magnitude of the effect decreases as the victory margin increases, disappearing if we go from a 0- to a 10-percentage-point victory margin.[39]

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

6.2 Gender Differences and Voters' Gender Bias

The results presented so far suggest that the gender differences we document in Section 5 are unlikely to come from gender differences in policy preferences. Specifically, we could have expected female leaders to prioritize public health, 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

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

seriously than men (Vincenzo et al., 2020). While this interpretation could rationalize the later effect, it does not explain why female mayors delayed their crisis response at the beginning of the pandemic. Moreover, it would not account for the fact that gender differences materialize only when mayors face electoral incentives.[40] Similarly, gender differences in risk aversion (Eckel and Grossman, 2008; Croson and Gneezy, 2009), which could have led female mayors to be more likely to wait and learn, are unlikely to drive the effects. If this was the main mechanism behind our results, the same time-varying patterns should hold with or without electoral incentives, contrary to what we find.[41]

Still, alternative interpretations remain plausible: female mayors might act differently than male mayors in competitive environment (e.g., Gneezy et al., 2003), or they could be more averse to the risk of losing an election, leading them to take the actions most aligned with voters' perceptions of the crisis.

In this section, we provide two additional pieces of evidence supporting the mechanism highlighted by our model, namely that female mayors acted in response to voters' bias rather than being driven by their intrinsic preferences.[42]

Heterogeneity analyses

First, we provide suggestive evidence that the gender differences in crisis response we observe are more likely to materialize in places where we expect greater voters' gender bias. To do so, we run heterogeneity analyses based on the extent of gender discrimination in the municipality, using two different proxies. First, we follow Le Barbanchon and Sauvagnat (2022) and rely on the magnitude of gender inequalities on the labor market. We consider

[40]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 4.3) and voting is mandatory in Brazil, meaning that closely-elected female and male mayors faced the same electorate.

[41]Several additional pieces of information contradict the risk-aversion interpretation. First, female mayors are balanced in their prior occupation (Table 2), which speaks against selection on risk aversion, to the extent that it is correlated with prior professional experience. Moreover, 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 results are not driven by less-educated mayors (Appendix Table A7) and that we study individuals who self-selected into a highly competitive environment makes risk aversion unlikely to be the main driver of our results.

[42]This interpretation relates to several studies that find that gender differences in risk aversion arise from women expecting negative consequences from not conforming to gender stereotypes (e.g., Kawakami et al., 2007; Carr and Steele, 2010).

all workers living in a given municipality and compute the gap in the wages received by female and male workers, after accounting for age, education, and occupation. Second, we consider the share of female councilors elected in the municipality in the last two elections (2008 and 2012), as a measure of voters' predisposition to support female politicians. Table 7 focuses on municipalities where the mayor can run for reelection, which drive our effects (Section 6.1), and present the results separately for municipalities above and below the median value of the heterogeneity variables.

As shown in Columns 3 to 6, the positive impact on COVID-19 deaths in period 1 and the negative effect in period 4 are mainly driven by municipalities above the median gender wage gap (Panel A, Columns 3 and 4)[43] and by municipalities below the median share of past female councilors (Panel B, Column 5 and 6). In these subsamples (as in the full sample), the coefficients are large and significant, whereas the coefficients are small and not significant for municipalities in the other subsamples. The difference between municipalities above and below the median gender wage gap is statistically significant for period 4 but not for period 1, while the reverse is true when considering the share of past female councilors.

These results should be interpreted with caution in the absence of municipal-level survey data to directly measure voters' gender biases, and municipalities above or below the median of our bias proxies might differ in other characteristics.[44] Still, the fact that the results go in the same direction for both measures (one reflecting labor market characteristics and the other past electoral outcomes) reinforces our confidence in the proposed mechanism.

[43]We obtain a similar pattern if we consider the gender gap in labor force participation as an alternative proxy for gender discrimination on the labor market (Appendix Table A11).

[44]Appendix Tables A12 and A13 provide summary statistics for the four subsamples. In each table, we begin with the subsample where voters' bias is most likely to manifest: municipalities above the gender wage gap median (Appendix Table A12) and municipalities below the median share of past female councilors (Appendix Table A13). Importantly, splitting the sample based on the gender wage gap or based on the share of past female councilors do not result in identical samples. First, municipalities are more comparable when the split is based on the share of past female councilors. Second, the differences across the two subsamples sometimes go in opposite direction. For instance, municipalities with a larger gender wage gap tend to be smaller, less dense and located further away from the capital, whereas the opposite is true when considering municipalities with a smaller share of past female councilors.

Table 7: Impact on COVID-19 deaths, by municipality gender discrimination

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths per 10,000 inhabitants					
	Full sample		Above median		Below median	
Periods	1	4	1	4	1	4
Panel A	*Gender wage gap*					
Female	0.600***	-1.178**	0.748***	-1.784**	0.314	0.052
	(0.201)	(0.505)	(0.240)	(0.798)	(0.267)	(0.537)
Robust p-value	0.004	0.030	0.002	0.030	0.316	0.675
P-value (3)=(5)					0.229	
P-value (4)=(6)						0.058
Observations	332	366	192	179	188	152
Polyn. order	1	1	1	1	1	1
Bandwidth	0.094	0.107	0.106	0.097	0.119	0.091
Mean	0.159	2.707	0.059	3.538	0.323	1.497
Panel B	*Share of past female councilors*					
Female	0.600***	-1.178**	0.116	-0.615	0.830**	-1.429*
	(0.201)	(0.505)	(0.153)	(0.513)	(0.329)	(0.821)
Robust p-value	0.004	0.030	0.437	0.368	0.016	0.067
P-value (3)=(5)					0.051	
P-value (4)=(6)						0.401
Observations	332	366	163	183	168	190
Polyn. order	1	1	1	1	1	1
Bandwidth	0.094	0.107	0.093	0.107	0.097	0.113
Mean	0.159	2.707	0.183	2.073	0.153	3.528

Notes: This table focuses on elections in which the mayor is not term-limited and can thus run for reelection. In Panel A, Columns 3 and 4 (resp. 5 and 6) restrict the sample to municipalities in which the gender wage gap is above (resp. below) 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 above (resp. below) the median. In Columns 1, 3, and 5 (resp. 2, 4, and 6), the outcome is the number of COVID-19 deaths per 10,000 inhabitants 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.

Results of the 2020 election

Finally, we assess whether female mayors closely elected in 2016 performed differently than closely-elected male mayors in the next election. Absent voter discrimination, taking the actions most aligned with voters' preferences should help female incumbents secure a higher vote share.

Instead, as shown in Table 8, we find no gender differences in the probability of running or being reelected or in vote share in the 2020 election. This null result is consistent with male and female mayors optimizing their policy choices by factoring in the gender biases in voters' assessments.

Table 8: Impact on the 2020 election

	(1)	(2)	(3)	(4)
Outcome	Run	Win	Conditional on running	
			Vote share	Win
Female	0.047	0.057	0.002	0.059
	(0.090)	(0.081)	(0.035)	(0.109)
Robust p-value	0.652	0.430	0.803	0.490
Observations	558	568	287	383
Polyn. order	1	1	1	1
Bandwidth	0.129	0.130	0.116	0.159
Mean, left of threshold	0.565	0.245	0.445	0.447

Notes: In Column 1 (resp. 2 and 4), the outcome is an indicator variable equal to 1 if the 2016 mayor runs in the 2020 election (resp. wins the election). In Column 3, the outcome is the vote share obtained in the first round. Columns 3 and 4 focus on mayors who ran again in 2020. Note that this restriction is unlikely to create selection issues due to the null impact on running in Column 1. The independent variable is an indicator equal to 1 if the female candidate won in 2016. 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.

7 Conclusion

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

Consistent with a model where voters assess female politicians more harshly and where politicians seek reelection, we show that female mayors in Brazil undertook lower containment efforts at the beginning of the pandemic, but more so later in the year once the crisis unfolded. Specifically, female mayors were less likely to impose commerce restrictions early on, and having a female mayor quadrupled the number of deaths in the first months of the pandemic. Instead, at the end of the year, female mayors became more likely to close non-essential businesses, and having a female mayor led to 41.6 percent fewer deaths.

In line with electoral incentives explaining these gender differences, our results are driven exclusively by non-term limited mayors who can run for reelection and the effects are stronger in more competitive races. Moreover, consistent with voters' gender bias creating different incentives for male and female politicians, we find that gender differences in crisis response are more pronounced in municipalities where this bias is more likely to materialize.

All in all, our paper shows that gender differences in leaders' behavior can be explained by leaders' incentives to adapt their policy choices to voters' gender biases. This makes gender differences in leaders' behavior particularly likely in competitive elections, for policies salient to voters, and in contexts where policy effectiveness is uncertain.

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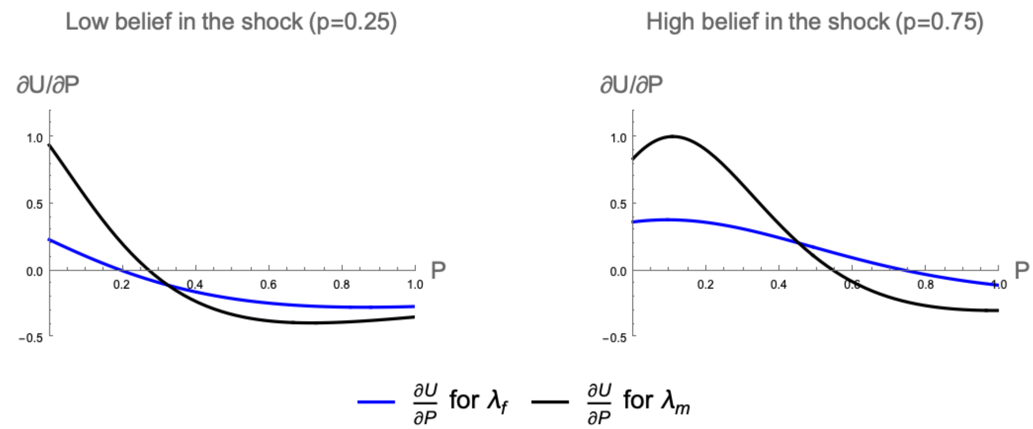
Online Appendix

Table of Contents

A Additional figures and tables

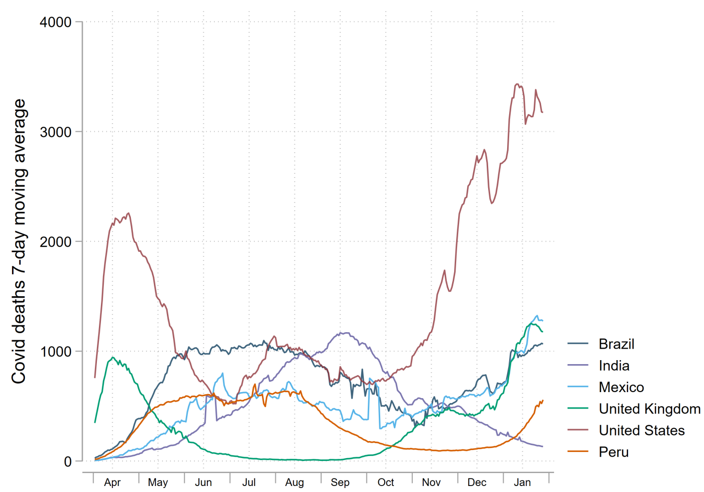
A1 Additional figures

Figure A1: 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)



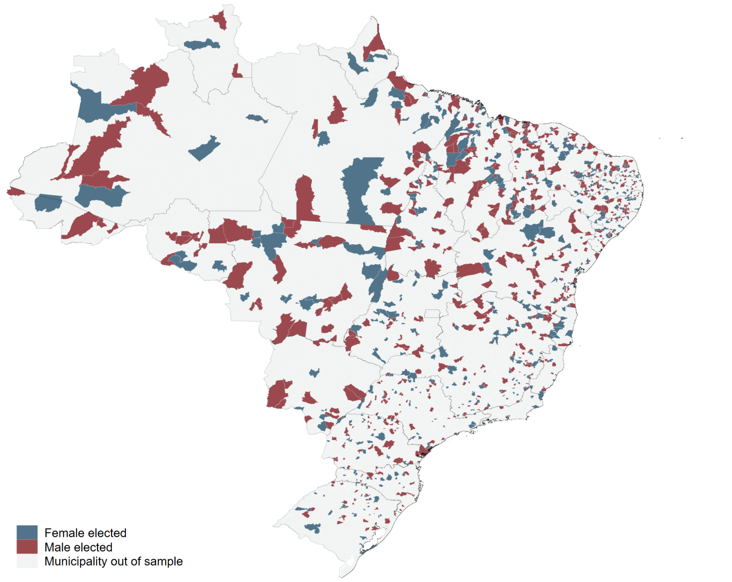
Notes: The figure plots the marginal utility of the policy ($\frac{\partial U}{\partial P}$) as a function of the policy level P for female mayors (expected policy effectiveness $\lambda_f = 2$) and male mayors (expected policy effectiveness $\lambda_m = 4$) under two scenarios – one in which voters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

Figure A2: Daily number of COVID-19 deaths in Brazil and in the other five countries with the highest mortality (7-day moving average)



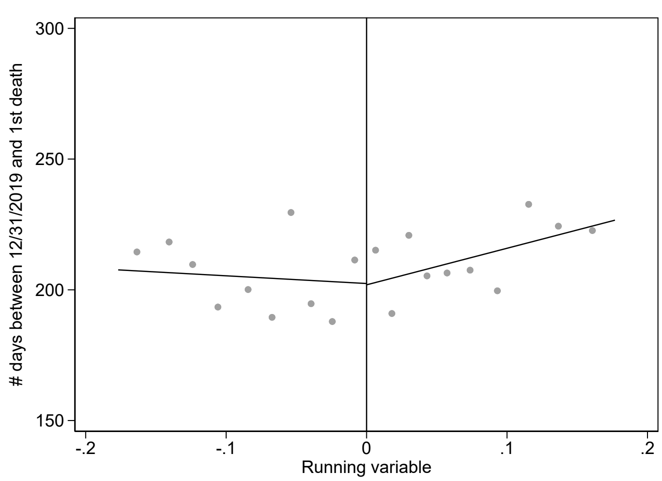
Notes: This figure includes the six countries with the highest number of COVID-19 deaths in the world as of January 31, 2021. It shows the number of COVID-19 deaths, smoothed using a 7-day moving average centered in the current day. Data from Our World in Data, accessed on June 23, 2021.

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



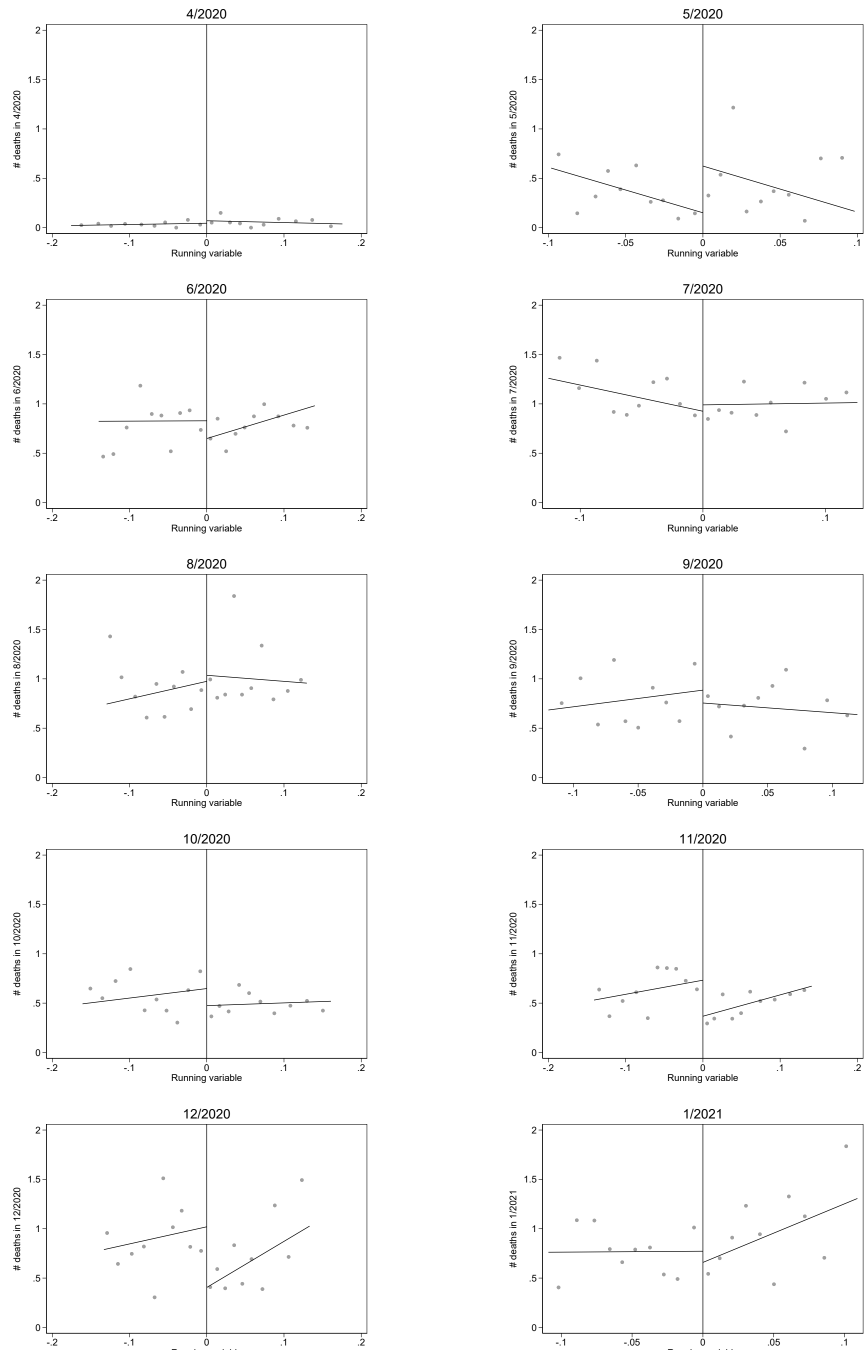
Notes: This figure plots the geographical distribution of municipalities in our sample of analysis. Municipalities in blue (red) are where a female (male) candidate was elected in 2016.

Figure A4: Impact of having a female mayor on the timing of the first COVID-19 death



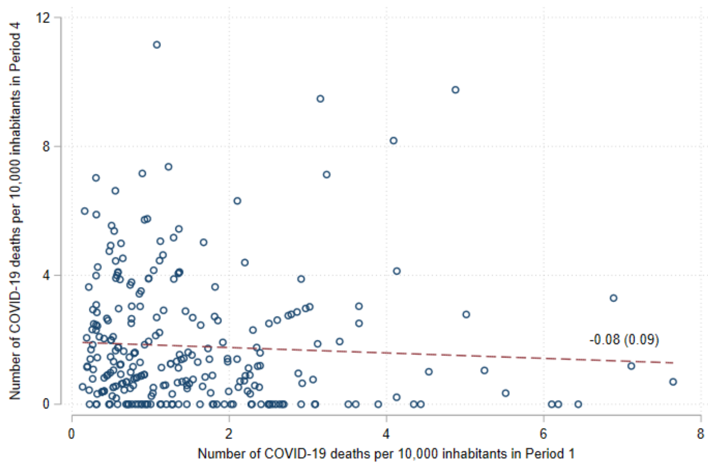
Notes: This figure is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the number of days between December 31, 2019, and the first reported COVID-19 death. Averages are calculated within quantile spaced bins of the running variable. The running variable is the 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 A5: Impact of having a female mayor on COVID-19 deaths, by month



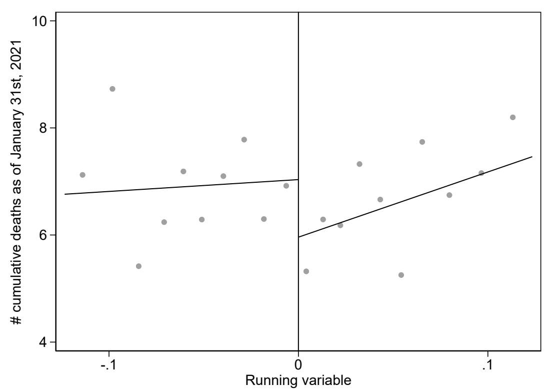
Notes: Each graph is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the total number COVID-19 deaths per 10,000 inhabitants in the municipality during the month of interest. Averages are calculated within quantile spaced bins of the running variable. The running variable is the 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: Correlation between COVID-19 deaths in period 1 and period 4



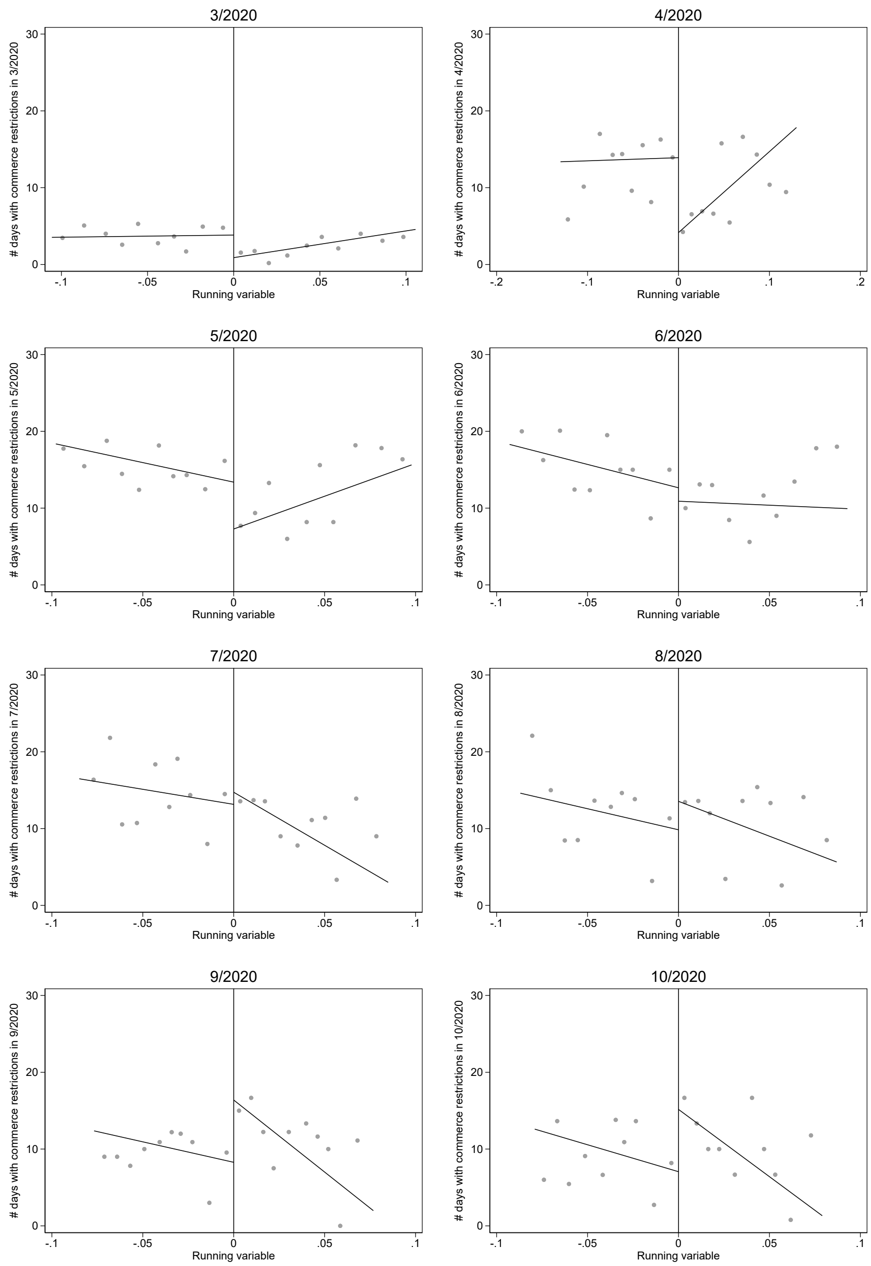
Notes: This scatterplot reports the total number of COVID-19 deaths per 10,000 inhabitants in the first period (April-May 2020) of analysis (x-axis) and in the last period (November 2020-January 2021) of analysis (y-axis), restricting the sample to municipalities that had at least one death in the first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

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



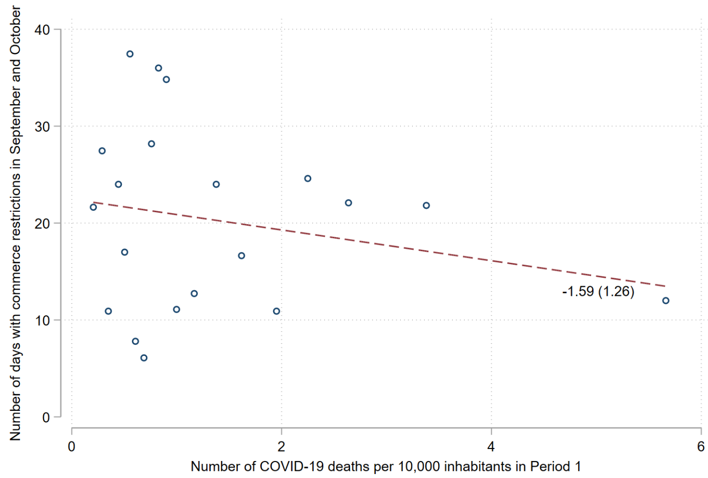
Notes: This figure is constructed by restricting the support to observations in the estimation bandwidths and by setting the fit to match the local polynomial point estimator (polynomial order 1 and triangular kernel). Dots represent the local averages of the cumulative number COVID-19 deaths per 10,000 inhabitants in the municipality as of January 31, 2021. Averages are calculated within quantile spaced bins of the running variable. 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: 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. 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 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.

A2 Additional tables

Table A1: Descriptive statistics: sample comparisons

	All (N=5,556)		Sample (N=981)		Close (N=202)	
	Mean	Sd	**Mean**	Sd	**Mean**	Sd
Panel A	*Socio-demographic characteristics*					
Population	**33,706**	199,763	**13,928**	12,724	**13,880**	11,254
Density	**501.2**	1667.8	**119.5**	186.3	**109.7**	117.9
Average persons per room	**0.664**	0.213	**0.704**	0.243	**0.708**	0.209
Commuting time	**22.23**	5.98	**21.57**	4.57	**21.59**	4.70
Share of population $\geq$ 65 years old	**0.084**	0.025	**0.083**	0.023	**0.081**	0.023
Nursing home residents per 10k pop	**5.876**	12.832	**3.742**	11.488	**3.215**	7.650
Area	**1,525**	5,645	**1,765**	5477	**1,682**	4,634
Distance to São Paulo	**1,168**	754	**1,448**	739	**1,492**	730
Km to airport connecting to COVID hot spots	**272.7**	205.6	**301.3**	214.6	**294.3**	202.7
Median household income p/c	**388.3**	165.6	**319.3**	143.9	**314.4**	148.4
Informality rate	**0.158**	0.055	**0.169**	0.055	**0.167**	0.057
Unemployment rate	**0.043**	0.022	**0.044**	0.021	**0.044**	0.023
College graduate employment share	**0.076**	0.036	**0.067**	0.030	**0.066**	0.031
Black and mixed-race population share	**0.524**	0.238	**0.600**	0.215	**0.598**	0.225
Agriculture employment share	**0.364**	0.184	**0.422**	0.149	**0.439**	0.156
Evangelical share of population	**0.171**	0.095	**0.156**	0.091	**0.149**	0.090
Panel B	*Political characteristics*					
Turnout	**0.855**	0.060	**0.855**	0.059	**0.858**	0.057
Number of candidates	**2.748**	1.170	**2.642**	0.920	**2.733**	1.092
President's vote share	**0.387**	0.190	**0.318**	0.186	**0.307**	0.193

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, which is defined as the population living within 10 km of the average inhabitant of the municipality and is computed using the 2015 data from the Global Human Settlement Layer. The political variables are computed using the results of the first round of the 2016 municipal election, except for the last, which uses data from the first round of the 2018 presidential election. The area, distance to São Paulo, and number of kilometers to the closest airport are missing for 5 municipalities in the full sample. 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: 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 A3: Impact of having a female mayor on the timing of the first COVID-19 death

Outcome	(1) Date of the first death
Female	-0.502
	(10.977)
Robust p-value	0.958
Observations	702
Polyn. order	1
Bandwidth	0.177
Mean, left of threshold	200.748

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 A4: 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	0.025	0.473***	-0.180	0.065	0.061	-0.130	-0.173	-0.365**	-0.615**	-0.114
	(0.034)	(0.161)	(0.222)	(0.224)	(0.241)	(0.202)	(0.177)	(0.181)	(0.219)	(0.247)
R. p-value	0.554	0.004	0.467	0.682	0.913	0.549	0.302	0.046	0.012	0.552
Obs.	704	455	603	541	565	516	674	606	579	490
Polyn.	1	1	1	1	1	1	1	1	1	1
Bandwidth	0.176	0.098	0.140	0.126	0.130	0.119	0.161	0.141	0.133	0.109
Mean	0.040	0.129	0.802	0.921	0.853	0.843	0.598	0.763	0.973	0.756

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 A5: Impact on the cumulative number of COVID-19 deaths as of January 31, 2021

Outcome	(1) Cumulative number of COVID-19 deaths per 10,000 inhabitants as of 01/31/2021
Female	-1.075
	(0.714)
Robust p-value	0.139
Observations	528
Polyn. order	1
Bandwidth	0.123
Mean, left of threshold	6.710

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 A6: 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	56.098***	-0.165*
	(11.531)	(0.093)
Robust p-value	0.000	0.076
Observations	156	208
Polyn. order	1	1
Bandwidth	0.097	0.085
Mean, left of threshold	92.203	0.165

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 A7: Impact on COVID-19 deaths, by mayor's education level

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths per 10,000 inhabitants					
	Full sample		No higher educaton		Higher education	
Periods	1	4	1	4	1	4
Female	0.600***	-1.178**	0.781**	-1.650	0.339*	-1.172***
	(0.201)	(0.505)	(0.338)	(0.887)	(0.213)	(0.443)
Robust p-value	0.004	0.030	0.030	0.134	0.096	0.008
Observations	332	366	179	138	166	179
Polyn. order	1	1	1	1	1	1
Bandwidth	0.094	0.107	0.116	0.081	0.090	0.097
Mean	0.159	2.707	0.243	2.939	0.076	2.295

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 A8: Impact on COVID-19 deaths, by mayor's age

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths per 10,000 inhabitants					
	Full sample		Below age median		Above age median	
Periods	1	4	1	4	1	4
Female	0.600***	-1.178**	0.426**	-2.248***	0.651**	-0.603
	(0.201)	(0.505)	(0.159)	(0.623)	(0.288)	(0.699)
Robust p-value	0.004	0.030	0.010	0.001	0.037	0.474
Observations	332	366	224	185	210	172
Polyn. order	1	1	1	1	1	1
Bandwidth	0.094	0.107	0.139	0.108	0.130	0.098
Mean	0.159	2.707	0.185	2.804	0.259	2.520

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 A9: Impact on COVID-19 deaths, by mayor's previous legislative office

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths per 10,000 inhabitants					
	Full sample		Has not served		Has served	
Periods	1	4	1	4	1	4
Female	0.600***	-1.178**	0.736***	-0.767	0.382	-4.068***
	(0.201)	(0.505)	(0.218)	(0.494)	(0.206)	(0.985)
Robust p-value	0.004	0.030	0.001	0.134	0.185	0.001
Observations	332	366	275	320	34	42
Polyn. order	1	1	1	1	1	1
Bandwidth	0.094	0.107	0.088	0.105	0.081	0.099
Mean	0.159	2.707	0.167	2.179	-0.078	6.009

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 2012-2016 term (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 A10: OLS estimates of the impact of having a female mayor on COVID-19 deaths

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	# COVID-19 deaths in Period 1 per 10,000 inhabitants			# COVID-19 deaths in Period 4 per 10,000 inhabitants		
Vote margin	All	<=10pp	<=5pp	All	<=10pp	<=5pp
Female	0.064	0.368**	0.697***	-0.102	-0.954**	-0.982*
	(0.088)	(0.172)	(0.225)	(0.236)	(0.431)	0.567
Female*Vote margin	-0.621	-4.906	-18.040***	0.606	15.408**	14.383
	(0.398)	(2.990)	(6.844)	(1.277)	(7.463)	(18.190)
Observations	981	458	252	981	458	252
Mean	0.402	0.458	0.434	2.343	2.200	1.993

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

Table A11: Impact on COVID-19 deaths, by the gender gap in labor force participation

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of Covid-19 deaths per 10,000 inhabitants					
	Full sample		Above median		Below median	
Periods	1	4	1	4	1	4
Female	0.600***	-1.178**	0.889***	-1.902**	0.327	-0.585
	(0.201)	(0.505)	(0.297)	(0.667)	(0.226)	(0.660)
Robust p-value	0.004	0.030	0.002	0.010	0.196	0.470
P-value (3)=(5)					0.134	
P-value (4)=(6)						0.162
Observations	332	366	155	175	193	191
Polyn. order	1	1	1	1	1	1
Bandwidth	0.094	0.107	0.085	0.097	0.120	0.116
Mean	0.159	2.707	0.042	3.180	0.164	2.166

Notes: 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 further restricted to municipalities where the gap in the labor force participation of female and male residents is above the median (resp. below the median). In Columns 1, 3, and 5 (resp. 2, 4, and 6), the 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 A12: Descriptive statistics: above vs. below the gender wage gap median

	Gender wage gap			
	Above median (N=363)		Above median (N=364)	
	Mean	Sd	**Mean**	Sd
Panel A	*Socio-demographic characteristics*			
Population	12,799	11,487	15,651	14,047
Density	110.6	130.0	118.2	166.1
Average persons per room	0.655	0.135	0.772	0.329
Commuting time	21.25	4.31	21.98	4.92
Share of population $\geq$ 65 years old	0.083	0.023	0.081	0.023
Nursing home residents per 10k pop	4.876	14.272	3.068	9.813
Area	1,505	2927	2,368	8063
Distance to São Paulo	1,261	690	1,680	746
Km to airport connecting to COVID hot spots	320.9	224.1	281.1	220.7
Median household income p/c	352.0	135.9	279.0	137.7
Informality rate	0.181	0.054	0.156	0.050
Unemployment rate	0.044	0.020	0.044	0.022
College graduate employment share	0.071	0.029	0.064	0.030
Black and mixed-race population share	0.585	0.201	0.632	0.220
Agriculture employment share	0.405	0.135	0.434	0.158
Evangelical share of population	0.170	0.088	0.144	0.094
Panel B	*Political characteristics*			
Turnout	0.854	0.060	0.854	0.060
Number of candidates	2.612	0.861	2.764	1.017
President's vote share	0.364	0.182	0.269	0.175

Notes: The sample includes only municipalities in which the mayor is not term-limited and can thus run for reelection. The first two columns (resp. last two columns) consider municipalities above (resp. below) the gender wage gap median. Socio-demographic variables come from the 2010 census, except for density, which is defined as the population living within 10 km of the average inhabitant of the municipality and is computed using the 2015 data from the Global Human Settlement Layer. The political variables are computed using the results of the first round of the 2016 municipal election, except for the last, which uses data from the first round of the 2018 presidential election. All variables are defined in Appendix Table B1.

Table A13: Descriptive statistics: above vs. below the median share of past female councilors

	Share of past female councilors			
	Below median (N=378)		Above median (N=349)	
	Mean	Sd	**Mean**	Sd
Panel A	*Socio-demographic characteristics*			
Population	15,571	13,855	12,771	11631
Density	123.7	145.7	104.3	152.4
Average persons per room	0.710	0.238	0.717	0.279
Commuting time	21.84	4.61	21.38	4.66
Share of population $\geq$ 65 years old	0.082	0.023	0.082	0.023
Nursing home residents per 10k pop	4.213	10.144	3.709	14.228
Area	2,246	7332	1,602	4319
Distance to São Paulo	1,396	761	1,551	726
Km to airport connecting to COVID hot spots	300.1	231.6	301.9	214.0
Median household income p/c	322.9	142.1	307.4	140.6
Informality rate	0.170	0.053	0.168	0.054
Unemployment rate	0.045	0.021	0.043	0.020
College graduate employment share	0.066	0.029	0.069	0.030
Black and mixed-race population share	0.603	0.211	0.615	0.213
Agriculture employment share	0.421	0.153	0.418	0.141
Evangelical share of population	0.164	0.091	0.150	0.091
Panel B	*Political characteristics*			
Turnout	0.849	0.061	0.860	0.059
Number of candidates	2.741	1.018	2.630	0.857
President's vote share	0.333	0.189	0.299	0.178

Notes: The sample includes only municipalities in which the mayor is not term-limited and can thus run for reelection. The first two columns (resp. last two columns) consider municipalities above (resp. below) the median share of past female councilors. Socio-demographic variables come from the 2010 census, except for density, which is defined as the population living within 10 km of the average inhabitant of the municipality and is computed using the 2015 data from the Global Human Settlement Layer. The political variables are computed using the results of the first round of the 2016 municipal election, except for the last, which uses data from the first round of the 2018 presidential election. All variables are defined in Appendix Table B1.

Table A14: Impact of the mayor's level of education on COVID-19 deaths

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

Notes: The sample is restricted to municipalities where the two front-runners in 2016 were male candidates and where one had completed higher education while the other had not. The same sample restrictions as for the main analysis also apply (see Section 4) and we end up with a sample of 1,408 municipalities. The independent variable is an indicator equal to 1 if the higher-educated candidate won the election. Each column takes as outcome the number of deaths per 10,000 inhabitants 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.

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 pop	Census	2010	Number of individuals aged 65 or above living in nursings homes or asylums, per 10,000 individuals (considering residents aged 18 or above) living in the municipality.
Area	IBGE	2010	Area of the municipality in square kilometers.
Distance to São Paulo	IBGE	2010	Geographical distance (straight line along earth's surface), in kilometers, between each municipality and the city of São Paulo.
Km to airport connecting to COVID hot spots	ANAC	2010	Geographical distance, in kilometers (straight line along earth's surface), to nearest airport having at least one flight connecting Brazil with the US, UK, France, Spain, Italy, Germany, or China.
Median household income p/c	Census	2010	Municipality's median household income per capita. Total household income includes all sources of income, both labor and non-labor income, and is divided by the number of household members.
Informality rate	Census	2010	Share of the municipality's working age population (18 y.o. or above) that work as employees without a signed work card. Self-employed individuals are not considered informal.
Unemployment rate	Census	2010	Share of the municipality's working age population (18 y.o. or above) that did not work for at least one hour in the week of reference, but actively looked for a job in that month.
Gender wage gap	Census	2010	Gender difference in the municipality's mean residual labor income. Residual income is computed from a linear regression of the individual's total labor income on age, education, and occupation.
Labor force participation gap	Census	2010	Gender difference in the municipality's labor force participation rate. The participation rate is the share of the municipality's working age population (18 y.o. or above) that is employed or unemployed.
College graduate employment share	Census	2010	The share of the municipality's population that had completed college or higher educational level among those employed who reported their educational status in the census.
Black and mixed-race population share	Census	2010	Share of the municipality's population that is black or mixed-race.
Agriculture employment share	Census	2010	Share of employed individuals working in agriculture, based on CNAE - Domiciliar sector definition.
Evangelical share of population	Census	2010	Share of the municipality's population belonging to an evangelical religion.

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Variable	Dataset	Date	Description / comments
			Panel B: Municipality-level electoral variables
Turnout	TSE	2016	Share of registered voters who cast a vote in the first round of the 2016 election.
Number of candidates	TSE	2016	Number of candidates running for mayor in the first round of the 2016 election.
Elected president's vote share	TSE	2018	Share of votes in the first round of the 2018 presidential elections that went to the elected president.
Share of past female councilors	TSE	2008-12	Share of women in the total number of councilors elected in both 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's date and the candidate's date of birth as registered by the electoral justice. In the case of supplementary elections, we follow the same logic and compute the candidate's age as of the supplementary election date.
Education	TSE	2016	Dummy variable that equals 1 if the candidate has completed tertiary-level education.
Race	TSE	2016	Dummy variable that equals 1 if the candidate is white.
Occupation	TSE	2016	Professional occupation of the candidate. There are 167 occupations declared by the candidates in the 2016 election data. We manually classified these occupations into four relevant areas: politics, public servants, health-related, and business owners.
Political party	TSE	2016	Political party under which the mayoral candidate ran in the 2016 election.
Ideological score	BLS	2019	To each candidate, we assign their party's ideology score from the 2018 wave of the Brazilian Legislative Survey (BLS) (Zucco and Power, 2019). We use data and replications files from Power and Rodrigues-Silveira (2019), who further impute the score for smaller parties. The score is centered around zero and goes from -1 (extreme left) to +1 (extreme right) and is adjusted to take into account party movements across years.
Served as councilor in the previous term	TSE	2012	Dummy variable that equals 1 if the candidate was elected in 2012 to serve in the city council.
2020 election outcomes	TSE	2020	Run: Dummy variable that equals 1 if the candidate ran for mayor in the 2020 election. Vote share: Share of valid votes as registered by the electoral justice in the first round. Win: 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.

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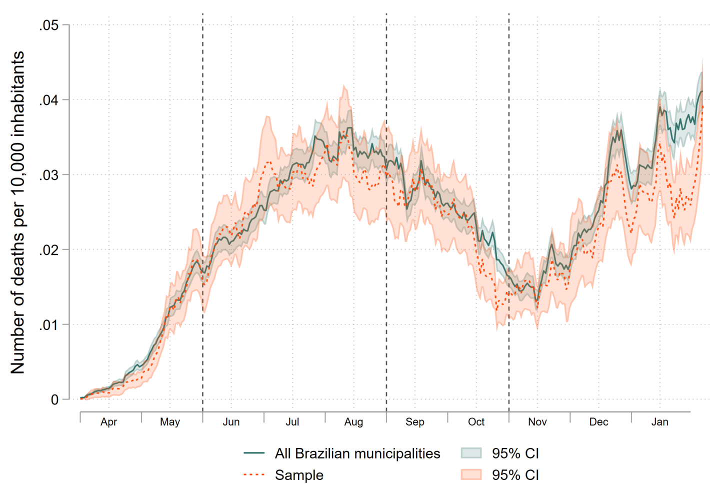
Variable	Dataset	Date	Description / comments
			Panel D: Main outcomes
Deaths per 10k	Brazil IO	2020-21	Number of COVID-19 deaths registered in the municipality for each day, normalized using the 2010 population so that it gives the number of daily deaths per 10,000 inhabitants. We then either use the data day by day or aggregate it by months and periods. Brazil IO collected the data directly from state's health secretaries.
Deaths per 10k	SIVEP-Gripe	2020-21	Number of 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 **?** (see Appendix B3).

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

B2 COVID-19 data

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



Notes: This graph plots the 7-day moving average of the number of deaths per 10,000 inhabitants 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 5.1: the beginning of the first wave (April-May 2020), peak of the first wave (June-August 2020), end of the first wave (September-October 2020), and beginning of the second wave (November 2020-January 2021).

B3 Policies data

We constructed our policy data directly from publicly available municipal legislation documents.

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 486 observations (49.5 percent of our sample).[45]

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 dusk). This does not include lockdowns (see below).

[45] 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.

- *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).

- *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 B2 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 B3 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 B3, 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.

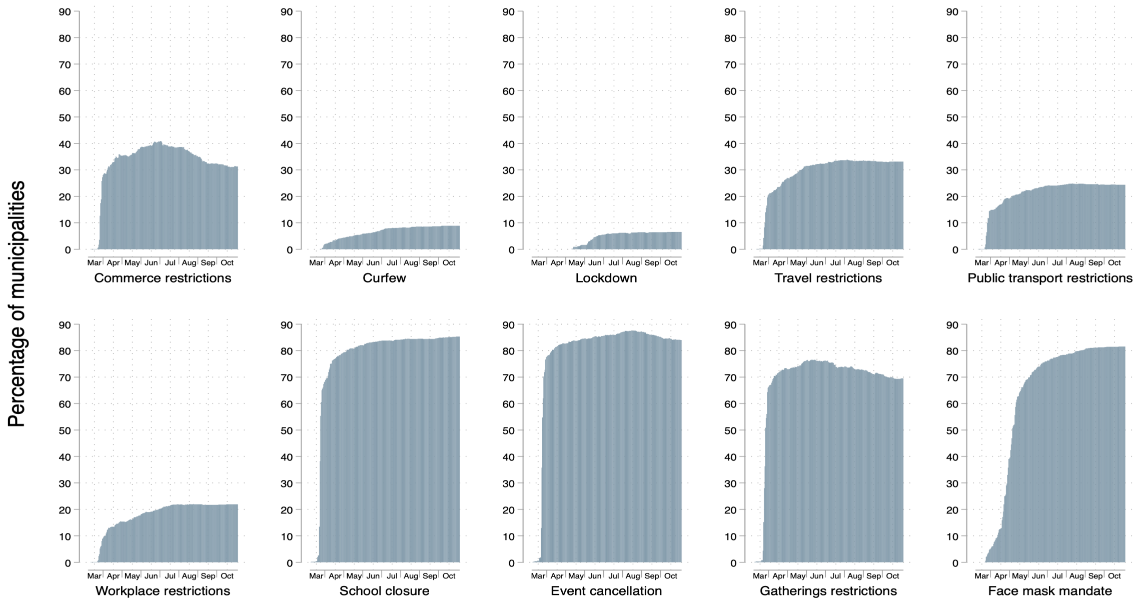
We thus focus our analysis on the remaining six policies, for which we have enough variation to identify the effects of interest.

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

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

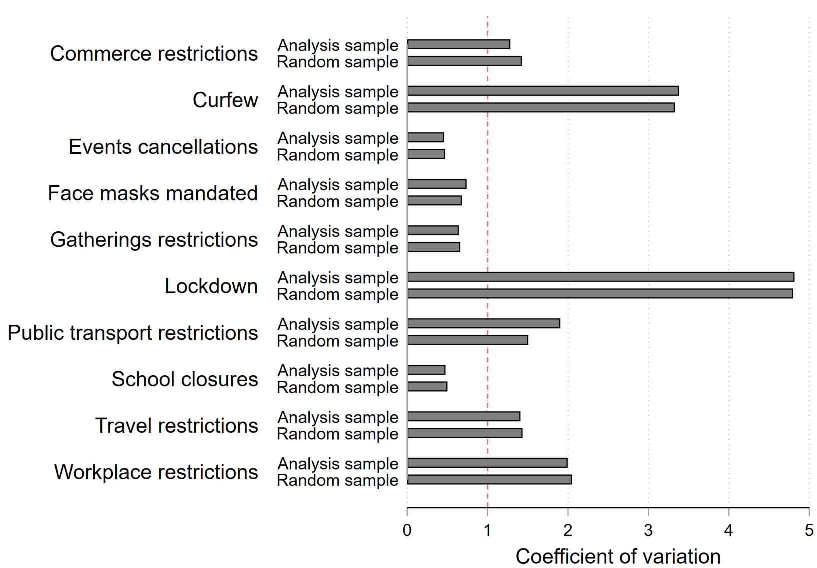
Notes: This table gives the number and share of municipalities that enacted the policy at least once from March to October 2020. The first two columns consider a 20-percent random sample representative of Brazilian municipalities with a population of 10,000 or larger. The last two columns consider municipalities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

Figure B2: 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 B3: 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.

B4 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.

B4.1 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.

B4.2 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.[46] 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.

B4.3 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.

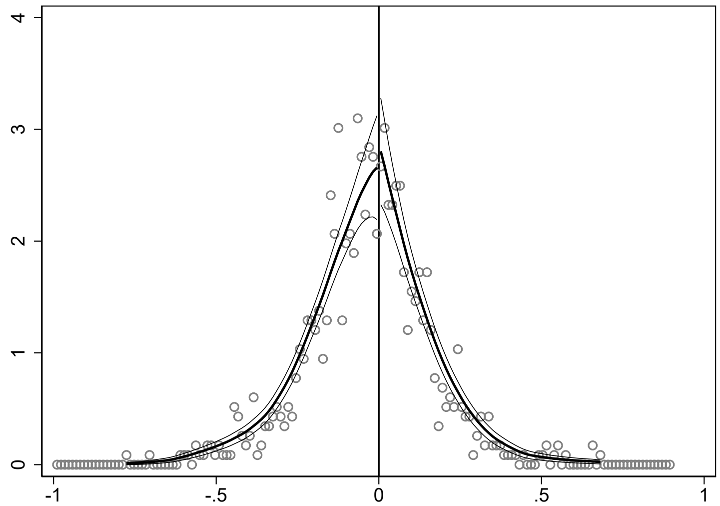
- Nineteen elections in which the number of null votes (in which invalidated votes are often counted) was larger than the number of votes received by the second-place candidate. All were indeed cases in which one of the top-two candidates was invalidated.

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

[46]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).

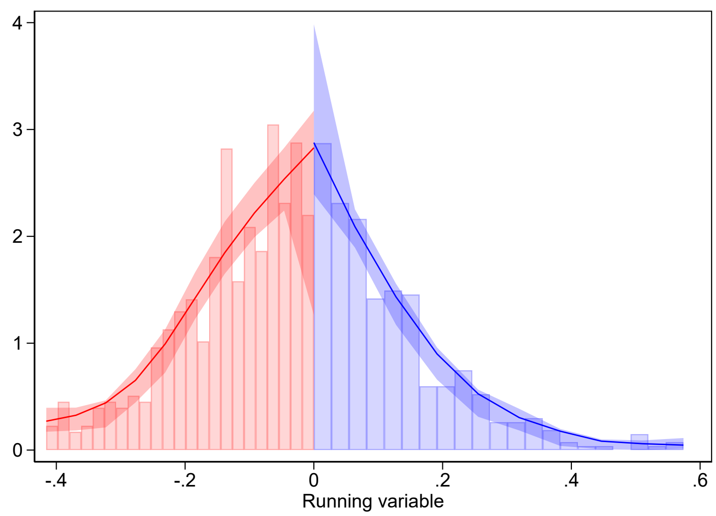
C Validity tests

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



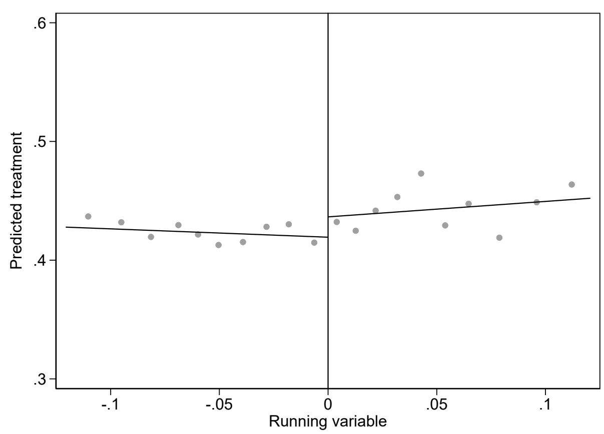
Notes: This Figure tests for a jump in the density of the running variable (the victory margin of the female candidate) at the threshold using the method developed by Cattaneo et al. (2018). The solid line represents the density of the running variable. Thin lines represent the confidence intervals.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

Table C1: General balance test

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

Notes: The outcome is the treatment variable predicted by municipal characteristics. We compute the outcome as follows: we first regress the treatment variable T on all 19 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 19 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) Area	(8) Distance to São Paulo	(9) Km to airport
Female	-2,851	2.0	-0.032	0.276	0.003	-1.096	-1,794*	-109	-65.0
	(1,993)	(23.7)	(0.037)	(0.858)	(0.004)	(1.458)	(838)	(123)	(36.6)
P-value	0.201	0.780	0.456	0.736	0.359	0.610	0.062	0.487	0.117
Obs	648	489	606	515	499	580	538	604	587
Polyn.	1	1	1	1	1	1	1	1	1
Bdw	0.152	0.109	0.142	0.119	0.114	0.134	0.125	0.140	0.136
Mean	15,263	105.0	0.731	21.300	0.078	4.007	2,923	1,552	344.72

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

Notes: Each column considers a specific baseline characteristic, as defined in Table B1. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure 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.

Table C3: Balance test: Age brackets

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Age bracket								
	≤14	15-24	25-34	35-44	45-54	55-64	65-74	75-84	≥85
Female	-0.009	-0.004	-0.001	0.003	0.004	0.002	0.001	0.001	0.001*
	(0.008)	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)	(0.001)
Robust p-value	0.286	0.127	0.535	0.458	0.227	0.390	0.587	0.327	0.076
Observations	705	664	495	524	570	591	514	527	441
Polyn. order	1	1	1	1	1	1	1	1	1
Bandwidth	0.178	0.158	0.110	0.122	0.130	0.137	0.119	0.122	0.095
Mean, left of threshold	0.280	0.187	0.155	0.127	0.102	0.073	0.047	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.

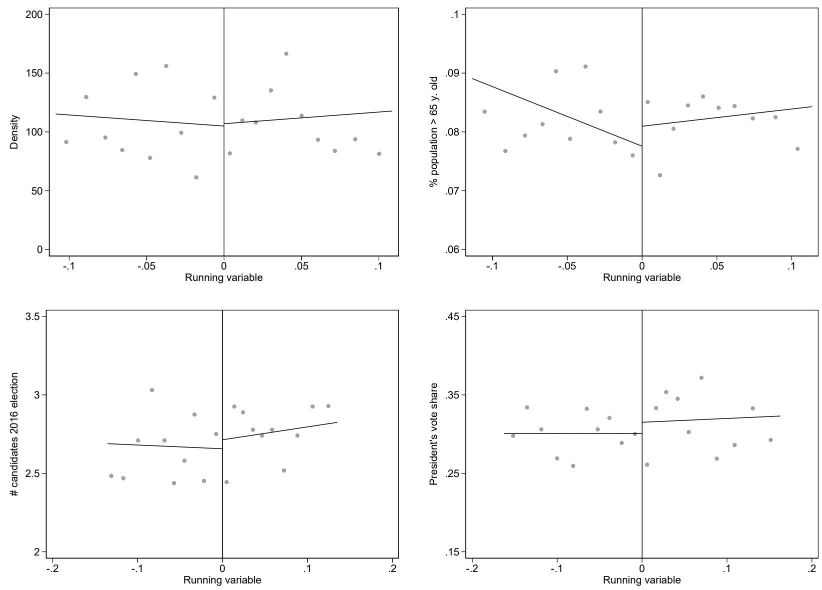
Table C4: Balance test: Industry characteristics

Outcome	(1) Manufacturing	(2) Industry share Services	(3) Agriculture	(4) Government	(5) Labor force participation Gender gap
Female	0.016	0.013	0.001	-0.026	0.003
	(0.020)	(0.011)	(0.008)	(0.019)	(0.014)
Robust p-value	0.457	0.235	0.873	0.162	0.981
Observations	700	612	668	697	513
Polyn. order	1	1	1	1	1
Bandwidth	0.174	0.142	0.158	0.172	0.117
Mean, left of threshold	0.337	0.068	0.082	0.509	0.234

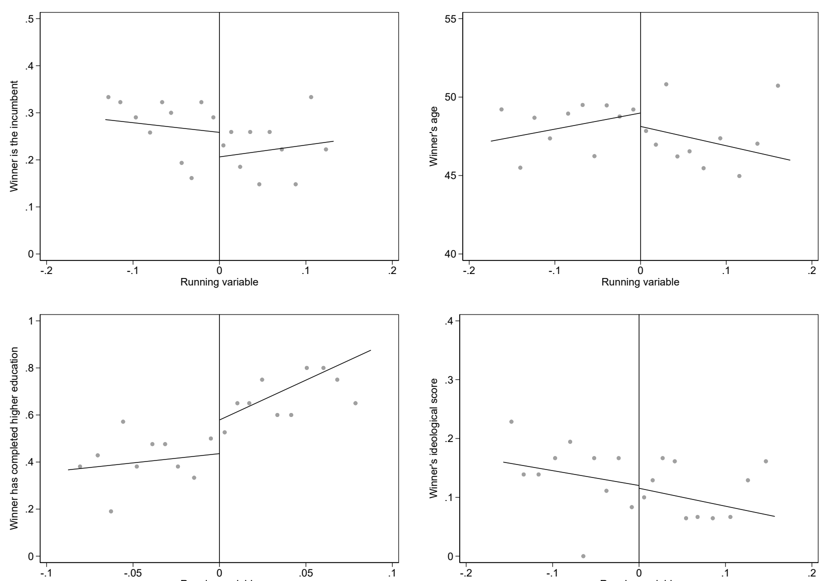
Notes: In columns 1 to 4, each variable considers the share of the workforce working in a given industry sector. The independent variable is an indicator equal to 1 if the female candidate won in 2016. We use a nonparametric estimation procedure and MSERD data-driven bandwidths. We assess statistical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

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. 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.

D Robustness tests

Alternative death measure. To make sure that our results are not affected by misreporting, we use as an alternative outcome the number of deaths attributed to severe acute respiratory infections (SARI) from the SIVEP-Gripe dataset described in Section 3.3. Figure D1 shows the strong correlation in the cumulative number of deaths as of January 31, 2021 between the two data sources. Tables D1 and D2 replicate our main results using the number of deaths by period and month, respectively. As in our main tables (Table 3 and Appendix Table A4) the point estimate is large and positive in period 1, an effect driven by the month of May 2020, but large and negative in period 4, an effect driven by the months of November and December 2020. Finally, Figure D2 plots the daily estimates for both SARI deaths and our main measure of COVID-19 deaths. The patterns are very similar, with positive coefficients at the beginning of the period of analysis and negative coefficients at the end of the year.

Controls. Appendix Table D3 tests the robustness of our results to using four different combinations of controls: no control (Column 1), only municipal characteristics controls (Column 2, corresponding to the main specification), only winner characteristics controls (Column 3), both municipal and winner characteristics controls (Column 4). All estimates are very close in magnitude when including either set of controls and all remain significant at the five-percent level.

State fixed effects. Policies implemented at the state level might influence mayors' decisions and COVID-19 outcomes. However, variations in state policies are unlikely to explain our results. First, Figure A3 and the balance tests in Appendix **??** show that female- and male-led municipalities are evenly geographically distributed. Second, Appendix Table D4 shows that our results remain virtually unchanged when we exploit within-state variation only, through the inclusion of state fixed effects. Note that in order to include state fixed effects, we had to remove nine states that contain less than 20 municipalities, accounting for eight percent of our sample.

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

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

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

	(1)	(2)	(3)	(4)
Outcome	# SARI deaths per 10,000 inhabitants			
	Period 1	Period 2	Period 3	Period 4
Female	0.814***	-0.197	-0.395	-0.796
	(0.283)	(0.473)	(0.318)	(0.450)
Robust p-value	0.003	0.783	0.179	0.155
Observations	403	497	498	491
Polyn. order	1	1	1	1
Bandwidth	0.083	0.111	0.112	0.109
Mean, left of threshold	0.600	3.124	1.817	2.647

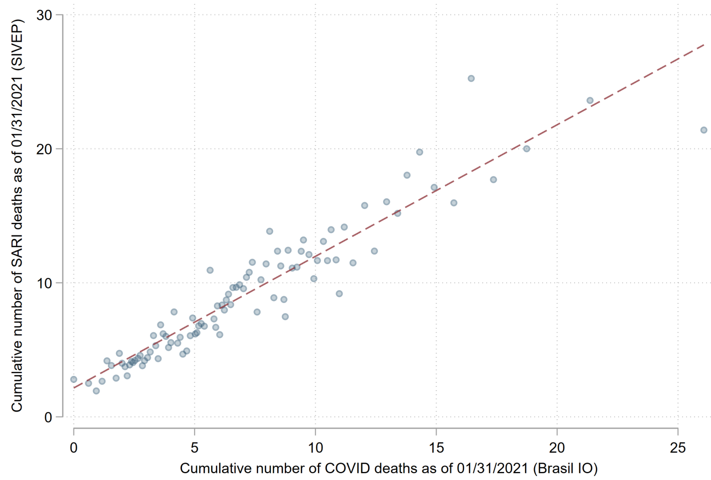
Notes: Each column takes as outcome the number of SARI deaths per 10,000 inhabitants during the period of interest. Period 1 (resp. 2, 3, and 4) is April-May 2020 (resp. June-August 2020, September-October 2020, and November 2020-January 2021). The independent variable is an indicator equal to 1 if the 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	0.023	0.839***	0.038	-0.120	-0.099	-0.075	-0.344*	-0.411**	-0.725**	0.262
	(0.104)	(0.260)	(0.245)	(0.245)	(0.262)	(0.228)	(0.210)	(0.147)	(0.267)	(0.252)
R. p-value	0.812	0.001	0.732	0.687	0.695	0.746	0.080	0.013	0.015	0.310
Obs.	527	388	565	483	534	529	472	616	499	481
Polyn.	1	1	1	1	1	1	1	1	1	1
Bandwidth	0.122	0.079	0.130	0.107	0.125	0.124	0.104	0.143	0.113	0.106
Mean	0.243	0.346	0.912	1.141	1.050	0.985	0.829	0.706	1.201	0.767

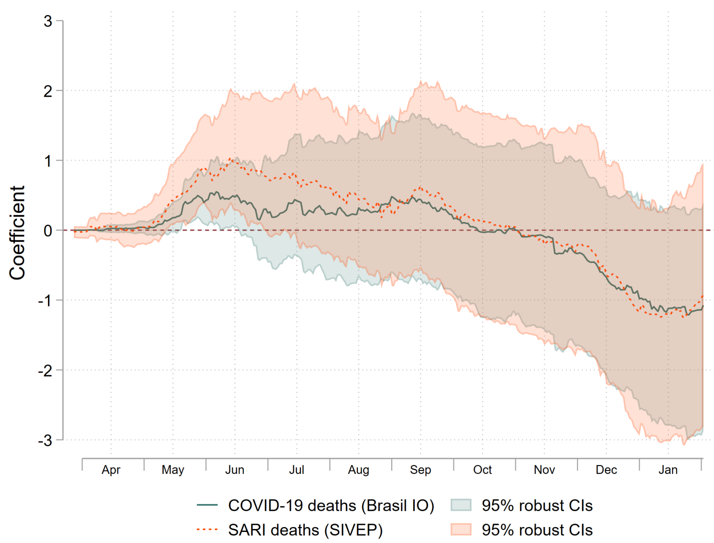
Notes: Each column takes as outcome the number of SARI deaths per 10,000 inhabitants during the month of interest. The independent variable is an indicator equal to 1 if the female candidate won in 2016. All municipal characteristics presented in Table 1 are included as controls. We use a nonparametric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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Number of Covid-19 deaths per 10,000 inhabitants							
Controls	None		Municipality		Winner		M+W	
	Period 1	Period 4	Period 1	Period 4	Period 1	Period 4	Period 1	Period 4
Female	0.391**	-0.999**	0.497***	-0.996**	0.456**	-1.049**	0.400***	-0.935**
	(0.176)	(0.405)	(0.164)	(0.392)	(0.179)	(0.413)	(0.149)	(0.389)
R. p-value	0.035	0.016	0.003	0.021	0.015	0.015	0.009	0.033
Obs.	578	513	466	495	513	479	555	464
Polyn.	1	1	1	1	1	1	1	1
Bandwidth	0.133	0.118	0.103	0.111	0.117	0.105	0.128	0.102
Mean	0.203	2.432	0.169	2.397	0.177	2.367	0.194	2.354

Notes: Columns 1 and 2 do not include any control. Columns 3 and 4 (resp. 5 and 6) include as controls the municipal (resp. winner) characteristics presented in Table 1 (resp. Table 2). Columns 7 and 8 include both set of controls. 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 D4: Impact on COVID-19 deaths, including state fixed effects

	(1)	(2)	(3)	(4)
Outcome	Number of Covid-19 deaths per 10,000 inhabitants			
	Full sample		Restricted + state FEs	
	Period 1	Period 4	Period 1	Period 4
Female	0.497***	-0.996**	0.466***	-0.748*
	(0.164)	(0.392)	(0.159)	(0.392)
Robust p-value	0.003	0.021	0.005	0.070
Observations	466	495	420	456
Polyn. order	1	1	1	1
Bandwidth	0.103	0.111	0.100	0.111
Mean	0.169	2.397	0.179	2.394

Notes: In Columns 3 and 4, we include state fixed effects and remove municipalities part of states with fewer than 20 municipalities in our sample (8 percent). The outcome is the number of COVID-19 deaths per 10,000 inhabitants during the period 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 robustp-value. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The mean gives the average value of the outcome for male-led municipalities at the threshold.

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

	(1)	(2)	(3)	(4)
Outcome	Number of Covid-19 deaths per 10,000 inhabitants			
	Full sample		Robustness sample	
	Period 1	Period 4	Period 1	Period 4
Female	0.497***	-0.996**	0.478***	-0.940**
	(0.164)	(0.392)	(0.167)	(0.392)
Robust p-value	0.003	0.021	0.006	0.031
Observations	466	495	458	465
Polyn. order	1	1	1	1
Bandwidth	0.103	0.111	0.107	0.109
Mean	0.169	2.397	0.175	2.282

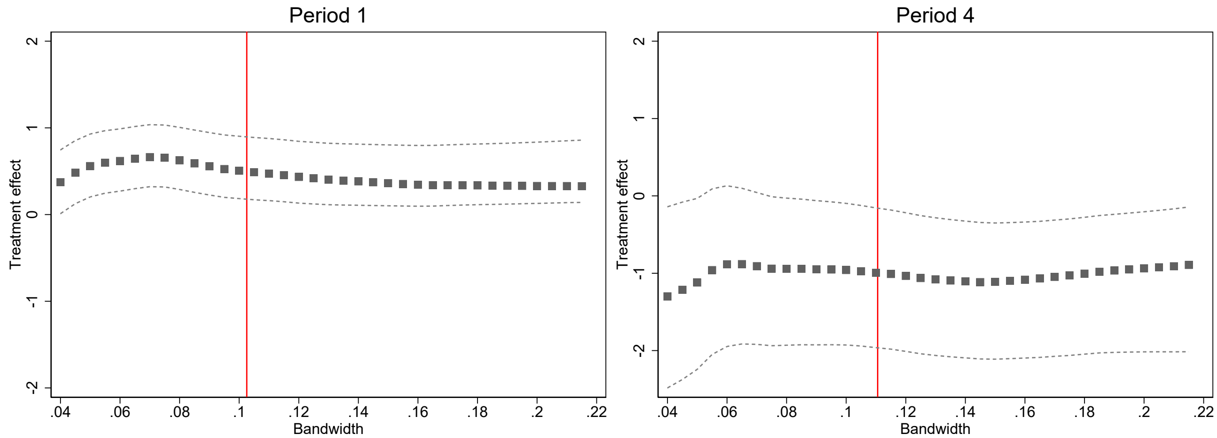
Notes: In Columns 3 and 4, we exclude municipalities in Mato Grosso state and municipalities that held a supplementary election – 3.3 and 2.6 percent of the sample, respectively. The outcome is the number of COVID-19 deaths per 10,000 inhabitants during the period of interest. Period 1 (resp. 4) is April-May 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 D6: Impact on COVID-19 deaths, using a second-order polynomial

	(1)	(2)	(3)	(4)
Outcome	Number of Covid-19 deaths per 10,000 inhabitants			
	Period 1	Period 4	Period 1	Period 4
Female	0.497***	-0.996**	0.591***	-1.170**
	(0.164)	(0.392)	(0.185)	(0.447)
Robust p-value	0.003	0.021	0.002	0.020
Observations	466	495	654	727
Polyn. order	1	1	2	2
Bandwidth	0.103	0.111	0.154	0.190
Mean	0.169	2.397	0.102	2.440

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.

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.

E Model Appendix

In this appendix, we demonstrate that the key qualitative result of the model does not depend on the specific functional forms assumed for the damage function f_1, the abatement function f_2, and the policy-cost function entering the utility function (all specified as exponentials). Rather, it only requires these functions to be concave.

Generalized Expressions and Regularity Conditions

Denote the public good production function

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

which generalizes equation (5) in the main text.

Similarly, denote voters' utility function as

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

which generalizes equation (6).

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 weakly concave in P, with $f_3(P) > 0$ for all $P \in [0, 1]$.

(iii) *Interior optimum.* There exists a unique interior solution $0 < P_s^*(\lambda_s) < 1$ that maximizes $U(P, \lambda_s)$.

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

We show next that, under these conditions, there exists a unique threshold $\widehat{\lambda}$ 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 following residual damage function

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

which captures the share of the potential damage that actually materializes, after considering the mitigating effect of the abatement policy.

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

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

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

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

Since f_2 is strictly increasing, it follows that $\dfrac{\partial G(P, \lambda_s)}{\partial P} \;<\; 0$. 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:

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

where the corresponding interior solution $P_s^*(\lambda_s)$ 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 $P_s^*\big(\lambda_s\big)$ varies with λ_s.

Proposition 1 (Non-Monotonicity of P_s^* in λ_s)**.** *Let Assumptions* (*i*)–(*iv*) *hold, and assume additionally that* $U(P, \lambda_s)$ *is twice continuously differentiable in* (P, λ_s)*. Then there exists a unique threshold* $\widehat{\lambda} > 0$ *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 $F\big(P_s^*(\lambda_s),\, \lambda_s\big) \;=\; 0$, 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 alters the marginal benefit from abatement, its sign depends on whether the abatement function $f_2(\lambda_s P)$ 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, $\frac{\partial F\left(P_s^*(\lambda_s),\lambda_s\right)}{\partial \lambda_s}$ must start positive at low λ_s (where more effective abatement *raises* the marginal return) and eventually become negative at high λ_s (where abatement saturates more quickly). Since U is single-peaked in P, this transition can only occur once. Denoting $\widehat{\lambda}$ the unique point where $\frac{\partial F\left(P_s^*(\widehat{\lambda}),\widehat{\lambda}\right)}{\partial \lambda_s} = 0$, 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

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

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 *opposite* the sign of $\frac{\partial F(P_s^*,\lambda_s)}{\partial \lambda_s}$: whenever $\frac{\partial F}{\partial \lambda_s}$ is positive (negative), the slope $\frac{dP_s^*}{d\lambda_s}$ is negative (positive). 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 is strictly increasing in p; that is,*

$$\frac{\partial F\left(P, \lambda_s; p\right)}{\partial p} > 0 \quad \textit{for all } 0 < P < 1.$$

Then the interior solution $P_s^*(\lambda_s;\, p)$ that maximizes $U\left(P, \lambda_s;\, p\right)$ 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:

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

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 if we add a mild "submodularity" condition, the threshold $\widehat{\lambda}(p)$ is strictly decreasing in p. This condition, which requires a negative cross derivative, implies that simultaneous increases in λ_s and p yield a smaller marginal utility than when they increase independently. In other words,when both the crisis is taken more seriously and politicians are more effective, the point at which additional increases in λ_s cease to be worthwhile is reached more quickly than when only voter's beliefs or policy effectiveness change). Formally:

Proposition 3 (Monotonicity of the Threshold in p)**.** *Let Assumptions* (i)–(iv) *hold, and suppose the interior optimum $P_s^*(\lambda_s; p)$ is strictly increasing in p, as in Proposition 2. In addition, assume that the residual-damage function $G(P, \lambda_s, p) = f_1(p\,\psi\,[\,1 - f_2(\lambda_s\,P)])$ satisfies the following "submodularity" property: as p and λ_s both increase* (*holding P at its interior optimum*)*, the cross-partial derivative $\frac{\partial^2}{\partial p\,\partial \lambda_s}\Big(-\frac{\partial G}{\partial P}\Big)$ is sufficiently large and positive to ensure that, at the threshold, $\frac{\partial \Phi}{\partial p}$ and $\frac{\partial \Phi}{\partial \lambda_s}$ share the same sign, where*

$$\Phi(\lambda_s, p) \;:=\; \frac{\partial}{\partial \lambda_s}\, F\Big(P_s^*(\lambda_s; p),\ \lambda_s,\ p\Big).$$

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,

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

Differentiating w.r.t. p shows that $\widehat{\lambda}'(p)$ is negative precisely when $\frac{\partial \Phi}{\partial p}$ and $\frac{\partial \Phi}{\partial \lambda_s}$ have the same sign at $\lambda_s = \widehat{\lambda}(p)$. Hence it suffices to show that this is the case under the submodularity condition stated in the proposition.

Recall that F is

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

where $G = f_1\big(p\,\psi\,[1 - f_2(\lambda_s P)]\big)$. By Proposition 2, we already know P_s^* rises with p. The submodularity assumption states that, at higher p and P, the marginal returns to enlarging λ_s "saturate faster," so that $\frac{\partial}{\partial p}\left(\frac{\partial F}{\partial \lambda_s}\right)$ remains in the same sign regime as $\frac{\partial}{\partial \lambda_s}\left(\frac{\partial F}{\partial \lambda_s}\right)$ at the threshold. Technically, this means that the signs of $\left(\frac{\partial}{\partial p}\,\frac{\partial F}{\partial \lambda_s}\right)$ and $\left(\frac{\partial}{\partial \lambda_s}\,\frac{\partial F}{\partial \lambda_s}\right)$ evaluated at $\big(P_s^*, \widehat{\lambda}(p), p\big)$ are the same. Hence $\frac{\partial \Phi}{\partial p}$ and $\frac{\partial \Phi}{\partial \lambda_s}$ necessarily share the same sign.

□