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IZA DP No. 17081

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ABSTRACT

Gender Bias in the Reelection of Politicians (When a Crisis Strikes)*

This paper sheds light on a neglected reason for women's underrepresentation in politics: crisis-induced gender gaps in incumbents' reelection with lasting negative effects on female representation. We use hand-collected data on 173,339 candidates in open-list local council elections (1997-2021) in the German state of Hesse. We exploit the March 2021 election one year into the Covid-19 pandemic and exclusive local Covid-19 mortality data in a continuous DiD framework. In a setting where (individual) councilors had no role in fighting the pandemic, we provide robust evidence for a *gender blame attribution gap*: at an average of one death/1,000 inhabitants, an additional death (≈ one SD treatment) leads to a 4.3 and 7.8 ppt lower reelection probability for male and female incumbents, respectively. Further results exclude various alternative mechanisms. Simulations predict persistent negative effects on future female councilor shares of 3-4 ppts.

JEL Classification: D72, H12, H70, I18, J16

Keywords: gender, retrospective voting, incumbency, crisis, local elections, political selection

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1 Introduction

Although there has been improvement over the past few decades, women continue to be underrepresented in politics even in Western democracies. According to the Global Gender Gap Index by the World Economic Forum, gender disparities in politics remain much larger than in any other domain.[1] This is a cause for concern since female political representation does not only carry symbolic value. Previous studies show that women's underrepresentation likely has adverse substantive consequences for women's and children's welfare (Chattopadhyay and Duflo, 2004; Baskaran and Hessami, 2024; Hessami and Lopes da Fonseca, 2020).

Why are gender disparities in politics so hard to surmount? Existing literature studies various potential reasons: anti-female party or voter bias, a limited supply of women willing to compete in elections, and incumbency advantages predominantly enjoyed by men (Fox and Lawless, 2014; Casas-Arce and Saiz, 2015; Baskaran and Hessami, 2018; Gonzalez-Eiras and Sanz, 2021; Brown et al., 2019; Lippmann, 2023) as well as women being less likely to recontest even after being elected into office (Baskaran and Hessami, 2022; Bernhard and de Benedictis-Kessner, 2021; Shair-Rosenfield and Hinojosa, 2014; Wasserman, 2023; Bhalotra, Clots-Figueras and Iyer, 2018; Brown et al., 2019).

Researchers have so far overlooked the impact of exceptional crises on gender bias in elections and the subsequent persistence of women's underrepresentation in politics. When a crisis strikes, women in office may be unduly punished by voters, more so than men. Previous literature in behavioral/labor economics indicates that women are evaluated more negatively than men for poor performance, especially in male-dominated domains. Egan, Matvos and Seru (2022) show that female financial advisors are 20% more likely to get fired and 30% less likely to find new jobs compared to male advisors after having engaged in misconduct.

[1] In 2022, the educational attainment gap and the health and survival gender gaps have shrunk to 5.6% and 4.2%. The economic participation and opportunity gap remains at 39.7%; it is only about half as large as the gender disparity in political empowerment with a 78% gap.

Sarsons (2017) provides evidence that female surgeons are less likely to receive a referral from a physician after one of her patients died compared to male surgeons who lose a patient.[2]

Our paper focuses on incumbent politicians and gender bias in their reelection in a crisis. We do not measure performance evaluation but blame attribution by voters towards politicians who are in office. Thereby, we depart from existing literature on gender gaps in (negative) performance evaluation (see Egan, Matvos and Seru (2022); Sarsons (2017)) and study a setting where the crisis performance is not in the politicians' hands. Taking into account the chronic underrepresentation of women in politics, it will be even more difficult to achieve a lasting increase in female representation if women who have been elected into office face elections in the midst of a crisis where a truthful evaluation of their candidacy is particularly overshadowed by anti-female voter bias. Our goal is to implement a strong test for gender bias by voters, i.e. we investigate the existence of a deep-rooted variant of bias blind to circumstantial facts.

We use the example of the Covid-19 pandemic as an exogenous shock.[3] This specific crisis has several advantages which make it suitable for our analysis. First, at the subnational (state, county, municipal) level, there has been large variation in pandemic severity. This allows us to study how different levels of crisis severity affect gender bias in elections. Second, this crisis was entirely unexpected and essentially started from one day to the next ruling out any anticipation effects. Most importantly, local politicians such as individual members of the local council essentially had no role in the containment of the pandemic.[4]

[2]A number of studies also uncover biases against women as university instructors independent of teaching performance (Boring, 2017; Mengel, Sauermann and Zölitz, 2019).

[3]We consider the local severity of Covid-19 (captured by mortality) as mostly random from the viewpoint of individual municipalities. Balance tests across mortality levels show indeed that municipalities' characteristics do not vary significantly with Covid-19 mortality (see Section 3.2.2 for supporting evidence).

[4]Federal and state governments had at their disposal and made use of various measures to contain the pandemic (school/daycare closures, work-from-home mandates, mask mandates, curfews, mandatory testing, etc.). During the first twelve months when a vaccine was unavailable for large parts of the population, policymakers at higher tiers of government carried a large responsibility in selecting and implementing appropriate measures.

The paper is set in the context of the German state of Hesse where local council elections took place one year into the pandemic. In March 2021, Germany entered the third Covid-19 wave, having just witnessed a devastating second wave from October 2020 to January 2021. In this environment, voters in the 422 Hessian municipalities elected their local councils on March 14th, 2021. We use hand-collected data on 173,339 candidates for six local council elections in Hesse over the 1997-2021 period. This allows us to track candidates across several elections and to use their reelection as a dummy outcome variable in our estimations.

Our difference-in-differences (DiD) estimations with a continuous treatment (municipal variation in Covid-19 mortality) show that incumbents were less likely to get reelected in municipalities where the pandemic's consequences were more severe. A quadratic specification for the continuous mortality rate reveals that the significantly negative and marginally diminishing effect is observable for almost the entire range of mortality. Separate estimations for female and male councilors expose a gender gap: female incumbents are punished almost twice as harshly as male incumbents throughout the entire distribution of Covid-19 mortality rates. At the mean mortality rate of one death per 1000 inhabitants, one more death leads to a 4.3 and 7.8 ppt lower reelection probability for male and female incumbents, respectively.[5]

We interpret this as a *gender blame attribution gap*, i.e. voters blame female and male politicians in varying degrees for a crisis. This in itself is a significant contribution to the existing literature that underlines how women face biases even when they have already been elected into office while most existing literature documents evidence for voter bias against female candidates attempting to enter office for the first time (Baskaran and Hessami, 2018; Le Barbanchon and Sauvagnat, 2022). Essentially, this indicates that a crisis can reverse previously achieved progress in female representation.

[5]The fact that this gap has nothing to do with the actual performance of councilors becomes even more plausible when taking into account that women were generally considered to have shown more effective leadership during the Covid-19 pandemic (Bruce et al., 2022). If anything, this should have led to an electoral advantage of female over male incumbents.

A number of robustness tests confirm our baseline result. First, we provide results for two types of placebo tests (randomly reassigning treatment across municipalities and applying treatment in 2016 instead of 2021). Second, we show that our baseline results for the gender gap in the reelection of incumbents due to Covid-19 mortality also materializes when using incumbent vote shares instead of a binary reelection dummy as dependent variable. Third, we show that the gender gap can also be observed when using a measure of excess mortality instead of Covid-19 mortality data. Finally, we illustrate the robustness of our results to the econometric specification (probit, treatment scaling, spatial correction of standard errors) and sample composition (excluding large cities, outliers, balanced municipality-election sample).

Additional estimations address five potential alternative mechanisms. First, it could be that the observed gender gap is due to parties with many female councilors being less successful in the 2021 election or women being placed on worse list ranks in the 2021 election. Estimations that control for candidates' party affiliation and initial list ranks provide similar estimates as the baseline and refute party-level channels. Second, we show that gender recontest gaps do not vary with Covid-19 mortality rates. In addition, those female incumbent councilors that do not run in 2021 do not have characteristics (education, performance in 2016, tenure in office) that differ significantly from those that do recontest in 2021. Third, we show that the gender gap is not a result of gender being confounded by other councilor characteristics: when we control for councilors' age and education and interact it with the treatment, the gender gap persists. Fourth, we test whether women's increased absence in the council during the pandemic explains the reelection gap. However, when we control for councilor's absence based on information from meeting minutes, the results are unchanged. Finally, we investigate the role of electoral turnout: Covid-19 has indeed slightly depressed turnout rates. However, when we control for municipal turnout rates in the baseline estimations we obtain the same results as in the baseline.

Overall, the effect of one additional death/1,000 inhabitants on the gender gap in incumbents' reelection can be considered as large. A decline in female incumbents' reelection probability by 7.8 ppts corresponds to a 10% decline in female incumbents' reelection given that female incumbents were reelected in 2016 with a 78% probability. In additional simulations, where we construct a counterfactual scenario without Covid-19, we illustrate that this one-time shock has persistent effects on the female councilor share. With rising female representation in the past in Hessian local councils (2006: 21.6%, 2011: 24.5%, 2016: 27.4%), we simulate a 29% share in 2021 in the absence of Covid-19 in contrast to the actual share of 27.3% in 2021. Extrapolating female-specific recontest rates, reelection rates, etc., our simulations indicate a lasting 3-4 ppt gap even for ten elections in the future. In an average council with 31 seats, this means that 1.3 councilors are male instead of female. While this may not seem like a large effect and only maintains a pre-existing minority status, recent literature indicates that especially in small councils an additional woman can make a difference with regard to policy choices for public goods, e.g. the provision of child care (Baskaran and Hessami, 2024).

This paper primarily contributes to the literature on the reasons for women's underrepresentation in politics which has three subbranches. One strand explores statistical or taste-based discrimination by voters. Baskaran and Hessami (2018) find that exposure to female mayors in Germany makes voters more likely to elect women to local councils. Thus, statistical discrimination among voters can be reduced via exposure to successful female leaders. Le Barbanchon and Sauvagnat (2022) report similar evidence for anti-female biases among voters in France. Gonzalez-Eiras and Sanz (2021) show that female candidates in Spain are more successful when running on closed lists (party determines list ranks), i.e. with open lists where voters have more influence on individual candidates' success, women are less likely to get elected.

A second subbranch emphasizes gender biases in candidate (re)nominations. Casas-Arce and Saiz (2015) show that parties in Spain would have been more successful with a higher share of female candidates, suggesting that party leaders discriminate against women. Brown et al. (2019) suspect that a political glass ceiling exists given their finding that female politicians are less likely to run for higher US offices. Shair-Rosenfield and Hinojosa (2014) show that political parties in Chile renominate incumbent women similar to incumbent men, but discriminate against non-incumbent females. In contrast, Smrek (2020) finds for the Czech national parliament that conditional on performance, parties display no renomination bias against women.

A third subbranch focuses on gender gaps in political ambition and grit. Fox and Lawless (2014) suggest that differences in parental encouragement, peer experiences, etc. depress young women's interest in running for office. Wasserman (2023) explores differences in persistence among men and women after having entered politics: women are less likely to rerun for office after electoral setbacks than men. In contrast, Bernhard and de Benedictis-Kessner (2021) find no gender gaps in recontest likelihoods in US local and state races after close defeats. Bhalotra, Clots-Figueras and Iyer (2018) show that women are more likely than men to remain in politics after electoral victories. Baskaran and Hessami (2022) expose a gender recontest gap of 4-5 ppts in German local elections not due to prior defeats/victories but rather incompatibilities between family and political duties as well as a culture of male dominance in local politics.

This paper also links up with previous literature on political consequences of natural disasters (Malhotra and Kuo (2008), Healy and Malhotra (2009), Bechtel and Hainmueller (2011), Bodet, Thomas and Tessier (2016)), increasingly addressing pandemic retrospective voting. Campante, Depetris-Chauvin and Durante (2024) find that a panic caused by Ebola spread just before the US mid-term elections in 2014 reduced turnout and hurt the Democrats' chances of winning as the Republican campaign linked Ebola to immigration. Baccini, Brodeur and Weymouth (2021) show that Donald Trump could have been reelected as president in 2020 if it had not been for Covid-19. Gutiérrez, Meriläinen and Rubli (2022) document a negative impact of a local H1N1 outbreak on the incumbent party's performance in elections to the lower house of the Mexican Congress in 2009: a thousand more infection cases reduced the incumbent vote share by 0.17 ppts, persisting into 2012. Abad and Maurer (2021) show that incumbents lost votes due to the Spanish flu in 1918: in gubernatorial and congressional elections, voters in areas that experienced a disproportionate number of deaths voted against the incumbent party.

Note that the above literature analyzes settings in which the politicians up for reelection were actually in charge of fighting the pandemic, whereas in our context local politicians had essentially no role in doing so. Punishing politicians for negative events beyond their control is known as *blind retrospective voting* (Achen and Bartels (2017); Arceneaux and Stein (2006)).[6] One major contribution of our paper is that we combine the blind retrospective voting perspective with insights from the literature on women in politics and gender biases in elections. A unique asset of our dataset is that we are able to conduct our analysis at the smallest local level possible using fine-grained data at the candidate level while also allowing us to study several potential alternative mechanisms.

Our results provide important insights for our understanding of voter behavior and voters' attribution of responsibility in a crisis. It appears that voters associate a dire situation with those politicians who happen to stand for reelection during a pandemic.[7] Given this evidence, it stands to reason that holding elections during "the eye of the storm" unduly affects political selection, in particular with regard to female politicians.

2 Background

2.1 Covid-19 in Hesse

Hesse is a medium-sized state with more than 6 million inhabitants corresponding to 7.5% of Germany's population. The first case of Covid-19 in Hesse was confirmed on February 27, 2020, while the first death was reported on March 18, 2020. Figure 1 illustrates that with regard to Covid-19 infection and mortality rates, Hesse has fared similarly to the rest of Germany.

[6] This contrasts with the more traditional "performance-based" retrospective voting literature. Do voters engage in retrospective voting and evaluate in-office politicians based on past performance (Ashworth, Bueno de Mesquita and Friedenberg, 2018)? If yes, voters would hold incumbents accountable (Ferejohn, 1986) and remove ineffective incumbents (Rogoff, 1990). McDermott and Jones (2003) find indeed that voters hold the congressional majority party, rather than just their particular congressman, accountable for poor performance of the US Congress. de Benedictis-Kessner and Warshaw (2020) show that congressional and gubernatorial incumbents were punished by voters for the state of their respective local economies based on US county-level economic data.

[7] Given the daily media coverage and the permanent appearance of federal- and state-level politicians in the news and talkshows, it is unlikely that voters did not know who was in charge of and equipped with legal measures to contain the pandemic.

[Figure 1 goes here]

Hessian municipal council elections in 2021 took place as scheduled on March 14, which coincides almost with the beginning of the third wave of the Covid-19 pandemic (dashed vertical line). On election day, almost 3.2% of the state population (198,742 people) had officially been tested positive and Covid-19 had been responsible for the untimely death of 6,035 people which amounts to about one death per 1,000 inhabitants (RKI, 2022). On December 27th, 2020 the vaccination campaign against the SARS-CoV-2 virus began in Hesse with the Pfizer-BionTech vaccine. Until election day, only 7.8% of the state population had received their first shot due to severe shortages in vaccine production (see Figure A.7 for more details). The pandemic had thus been a predominant issue for Hessian voters prior to and during the election.

2.2 Pandemic crisis management in Germany/Hesse

With regard to electoral accountability, it is important to determine who is responsible for dealing with pandemics. According to the Infection Protection Act (*Infektionsschutzgesetz*), the legal foundation for pandemic containment measures is established by the federal government. However, the sixteen state governments decide which concrete actions are taken: they are in charge of acting in accordance with the IfSG, in particular on measures of quarantine (§30), occupational bans (§31) and curfews (§28).

To illustrate the over-time variation in Covid-19 containment measures across German states as well as across Hessian counties, we use data on a strictness index of such measures ranging from 0 to 100 by the German Federal Statistical Office.[8] Figure 2 shows that Covid-19 containment measures reached the highest level of strictness all across Germany and Hesse during the election in March 2021.[9] As the institutional description above predicts, the strict-

[8]The index is calculated using strictness levels of several measures, such as surgical mask mandates, types and numbers of closed institutions, social distancing measures, etc.

[9]According to a representative survey in early March 2021, two-thirds of the German population evaluated the strictness of these measures as appropriate or asked for even stricter measures (tagesschau, 2021).

ness of measures varies remarkably across states (Figure 2a), while across Hessian counties it is nearly indistinguishable (Figure 2b).

[Figure 2 goes here]

The State Presidents and State Ministers of Health were mainly in charge of containing the pandemic. At the time of the 2021 elections, Volker Bouffier from the conservative CDU was the State President of Hesse, while the Minister for Social Affairs and Integration (also responsible for health issues) was Kai Klose from the Greens. Since late 2018, the state government had been a CDU-Green coalition (see Figure A.8 for parliamentary seat distribution).

The main political bodies in Hessian municipalities, i.e. local councils, had practically no role in addressing the pandemic. Municipalities had only limited measures available to them but which were hardly used (News4Teachers, 2022). Childcare facilities, primary and secondary schools in Germany are public and the institution in charge of them (i.e. the local governments) had the possibility to e.g. install air filters in these institutions. However, this was typically not before autumn 2021 when counties/municipalities prepared for the second Covid-19 winter period.[10] More importantly, individual councilors in a deliberative body, unlike executives at higher-tier government levels, could not take any direct measures to contain the pandemic.[11]

2.3 Hessian council elections

Council elections in Hesse take place in March every five years.[12] Councilors are elected via an open-list procedure (preferential voting). Prior to the election, parties compile a list of candidates and assign an initial rank to each candidate. The number of votes each voter has is

[10] Since June 2021, there are federal funds available that municipalities can apply for and which co-finance 50% of the cost of purchasing and installing air filters. The remaining 50% were financed by the municipal governments and potential additional grants from the respective state government (Deutsches Schulportal, 2021).

[11] Due to containment measures, campaigning in early 2021 was basically impossible. Thus, the policy positions of individual councilors on Covid-19 (if they even existed) are unlikely to have reached and swayed voters.

[12] Before 2001, elections were held every four years.

equal to the number of council seats. Voters may freely allocate their votes to candidates across different lists (*Panaschieren*) and may cast up to three votes for a single candidate (*Kumulieren*).

Lists receive seats according to the votes received. For example, a list with 20% of the votes in a council election with 30 seats receives 5 seats. Candidates are ranked according to their preferential votes and enter the council if their post-election (final) rank is lower than or equal to the number of seats to which their list is entitled (Baskaran and Hessami, 2024).[13]

3 Data and empirical strategy

3.1 Data

3.1.1 Council election data

We build on the hand-collected council election data from Baskaran and Hessami (2018) and extend it with newly collected data for the 2021 election. In Section A.1.1 of the online appendix, we explain how we collected and cleaned the data. The combined dataset includes information on 173,339 candidates who competed for municipal council seats in Bavaria in 1997, 2001, 2006, 2011, 2016 and 2021. Figure 3a shows the evolution of our data coverage which is incomplete due to hand-collection of the data. For the last two elections, our data covers the universe of Hessian municipalities.[14]

Our data comprises candidates' names, gender, party, initial list ranks, final ranks, number of preferential votes received, and incumbency. We also have details on candidates' educational background, occupation, and birth year.[15] Since many candidates competed for office in multiple elections, we match individuals within municipalities across elections.[16] This matching helps us to fill in missing information on candidates' occupation, age and education.

[13]Thus, a candidate's preelection rank does not directly determine the outcome of the election. On the other hand, candidates at the top of a list are more noticeable, and the initial list rank may indicate a candidate's quality.

[14]The total number of municipalities in the state decreased from 426 to 422 due to mergers in 2020.

[15]Age is calculated as election year minus birth year.

[16]See Section A.1.2 of the online appendix for details on the matching process.

[Figure 3 goes here]

Matching candidates across elections also enables us to identify incumbents. We code candidates who were elected into the council in period $t-1$ and rerun in period t as incumbents in period t. Incumbency is coded as missing when data for the $t-1$ election is not available for a municipality. Figure 3b shows how many candidates we could classify as (non-)incumbents, which was the case on average for 70% of the candidates in the entire 2001-2021 period. 2021 represents the ideal case where we could classify almost all candidates.[17]

Figure A.10 provides descriptive statistics on reelection probabilities for Hessian local councils. Incumbent candidates are almost three times more likely to enter the council than non-incumbents. Comparing the elections from 2001 to 2021, the largest incumbency advantages materialized in 2006 with a declining trend thereafter. In 2001 and 2006, female incumbents were about 3.5% less likely to get reelected than male incumbents. For the 2011 to 2021 elections, the reelection probabilities for male and female incumbents are almost indistinguishable at about 75-78%. Thus, at a descriptive level there seems to be no general bias against female incumbents in getting reelected in recent elections.[18]

3.1.2 Covid-19 mortality data

For our DiD estimations, we rely on differences in pandemic severity across municipalities in Hesse as treatment, i.e. the number of lives lost due to Covid-19 per 1,000 inhabitants (mortality rate) from the beginning of the pandemic until the 2021 election. Since Covid-19 mortality data is publicly only available at the county level and for the five county-free cities, we hand-collected this data for the municipal level.

[17]We could not classify six 2021 candidates with the same first and last names within the same list. We cannot tell the incumbency status of candidates in 1997 since we do not have data for the previous election.

[18]In Online Appendix A.2, we estimate incumbency advantages in a regression framework. We find that both female and male incumbents are 56-57% more likely to get reelected than non-incumbents and these numbers were slightly smaller in 2021 than in previous elections, essentially confirming the descriptive observations above.

We contacted all 21 counties in Hesse and asked them to provide the cumulative number of infections and deaths since the beginning of the pandemic and for the last two weeks until election day, March 14, 2021.[19] 11 of the 21 counties followed our request (see Section A.1.3 for details). Our resulting estimation sample consists of 220 municipalities: 215 regular municipalities and 5 county-free cities. Table A.8 in the online appendix illustrates that counties that provided the data versus those that did not have similar characteristics (population size and age shares, fiscal variables, party seat shares, etc.). Most importantly, they do not differ in the severity of the Covid-19 shock (infection numbers and mortality). One could have suspected that only those counties responded to our request that have the "best" Covid-19 performance. This appears not to be the case.

Figure A.9 in the online appendix plots the distribution of municipal Covid-19 mortality rates from 0 to 5 deaths per 1,000 inhabitants. Figure 4 visualizes how this variation is distributed across Hesse. We classify Covid-19 mortality into four equally-sized municipality groups which are indicated in different shades in Figure 4. The fact that the distribution is geographically quite dispersed underlines that Covid-19 mortality variation is at least to some extent random and not clustered in a specific region.

[Figure 4 goes here]

3.1.3 Further municipal data

We additionally collect municipality-level data on demographics (population, gender and age composition statistics), fiscal characteristics (revenues, transfers, and debt) and political characteristics (gender and party composition of municipal and county councils) from the Hessian State Statistical Office. This data is available for the period from 1991 to 2021, expect for the political variables where data is available as of 1997.

[19] As the data is updated every evening, the last statistics are from March 13.

3.2 Empirical strategy

3.2.1 Continuous DiD: Covid-19 mortality and reelection

In a first step, we test whether voters voted retrospectively. We want to know whether voters punished incumbents for comparatively high local mortality rates. Our estimations thus rely on within-state variation of cumulative Covid-19 mortality rates at the time of the election. Using only the subsample of incumbents, we apply a generalized difference-in-differences (DiD) design with a continuous treatment:[20]

$$\text{Reelected}_{imt} = \beta \text{Mortality Rate}_{m,t} + \alpha_m + \gamma_t + Z1_{mt}\xi_1 + Z2_{m,t-1}\xi_2 + \varepsilon_{imt} \tag{1}$$

where $Reelected_{imt}$ is a dummy equal to 1 if the incumbent is reelected and translates into a reelection probability of incumbent i in municipality m in election t. *Mortality rate* represents the number of Covid-related deaths per 1,000 inhabitants and takes non-zero values only for 2021. α_m are municipality fixed effects which control for time-invariant municipality characteristics. γ_t are year fixed effects which take care of time trends in incumbency advantages.

We add two types of controls to account for time-varying characteristics of municipalities: $Z1_{mt}$ (female share, elderly (65$<$) and children ($<$14) share, population density, per capita tax revenue, transfers, debt) and $Z2_{m,t-1}$ as the council-specific covariates (share of women, CDU, SPD , Gruene and FDP in the council). To rule out potential correlation in error terms within municipalities across election years, standard errors are clustered at the municipality level.

In a second step, we estimate Equation (1) with two subsamples: male and female incumbent councilors. This will allow us to investigate whether there is a gender gap in this type of blind retrospective voting, i.e. whether female local councilors are punished more harshly by voters than male councilors for a certain level of Covid-19 mortality in their municipality.

[20] See, e.g., Lindo et al. (2020) for a similar approach.

3.2.2 Identifying assumption

In a continuous DiD framework, the identifying assumption is that the treatment is orthogonal to the characteristics of the municipalities. We therefore have to test whether the "strong" parallel trends assumption holds: changes in the reelection probability of incumbents in low-mortality municipalities provide a "good counterfactual" for changes in the reelection probability of incumbents that would have been observed in high-mortality municipalities if they had experienced low mortality rates (Callaway, Goodman-Bacon and Sant'Anna, 2024).

We test whether the treatment intensity correlates with observable characteristics of municipalities.[21] In Figure 5, each row displays the coefficient estimate obtained from a separate regression of the standardized mortality rate on a standardized covariate defined along the horizontal axis. Overall, none of the variables is significantly correlated with Covid-19 mortality.

[Figure 5 goes here]

Alternatively, the "strong" parallel treatment assumption can be tested by subdividing the municipality sample according to treatment doses and to test whether municipal characteristics differ across dose groups.[22] Replacing the continuous treatment variable in Equation (1) with indicators for treatment dose groups allows us to compare the treatment effects in the higher treatment groups to the lowest group.[23] Figure A.1 presents balance tests across the four treatment dose groups with respect to demographic, fiscal and political variables. The fact that there

[21] This approach is inspired by Cook et al. (2022).

[22] We define dose groups in a way that all groups are equally large (quartiles) and define the low (treatment=1), medium-low (treatment=2), medium-high (treatment=3) and high (treatment=4) doses. A caveat of the TWFE approach is that it typically puts more weight on doses around the average of the treatment and less on the tails Callaway, Goodman-Bacon and Sant'Anna (2024). Therefore, the average causal response to treatment can be misleading. This may also apply in our case given the uneven distribution of the treatment as shown in Figure A.9. Choosing equally-sized dose groups circumvents this problem.

[23] Figure A.2 plots how the average reelection probability of incumbents across the treatment groups developed over time. Before the treatment, the four lines follow a similar general trend with a peak in 2006 and a subsequent decline until 2016 that evolves in a parallel fashion for all four treatment groups.

are no systematic differences between the four treatment groups assures us that the low dose groups would have the same outcome if they had received the high dose and vice versa.[24]

4 Results

4.1 Covid-19 mortality and incumbents' reelection: full sample

Table 1 collects the results for our DiD estimations based on Equation (1). Model (4) shows that compared to incumbents in municipalities in the first treatment dose group, incumbents in the second treatment dose group were almost 4.9 ppts less likely to get reelected, incumbents in the third dose group were 6.4 ppts less likely to get reelected, and incumbents in the fourth dose group were 4.6 ppts less likely to get reelected. Thus, medium to high mortality rates consistently led to lower reelection rates compared to low mortality rates.

[Table 1 goes here]

Another finding in Table 1 is that the treatment effect is largest for the medium-high dose group (6.4 ppts) while the treatment effects for the medium-low (4.9 ppts) and the high (4.6 ppts) dose group have a quite similar, slightly smaller magnitude. This may imply a quadratic relationship between mortality rates and incumbents' reelection probabilities, i.e. a negative effect that flattens out with higher mortality rates. One can rationalize this pattern by assuming that up to a certain point a higher Covid-19 mortality reduces reelection probabilities. However, when a certain level of severity is reached, there is no additional punishment.[25]

[24] In addition, note that the four groups are not clustered geographically (see Figure 4).

[25] There are alternative ways of defining dose groups. In Table A.9, we divide the treatment variable into two, three, five and six equally-sized dose groups. As before, all estimates are reported in comparison to the lowest treatment dose group. With two groups there is no significant treatment effect, even though the coefficient is negative (t-statistic > 1.5). With three treatment groups, the effect is significantly negative for the comparison between the lowest and the medium treatment group. With five and six treatment groups, there is also a pattern indicating that as of a certain threshold of mortality, incumbents are punished by voters. Overall, the results

To formally investigate nonlinear effects, we replace the treatment dose groups with a continuous mortality rate variable and its square. Table 2 collects the results for the quadratic specification. Model (4) shows that each additional Covid-related death per 1,000 inhabitants indeed is associated with a reduction in the reelection probability of incumbents at a decreasing rate. Thus, up to a point, additional mortality leads to harsher punishment of incumbents by voters. Thereafter, the marginal effect declines.[26] [27]

[Table 2 goes here]

4.2 Covid-19 mortality and incumbents' reelection: gender gap

To investigate whether the effect differs between male and female incumbents, we run additional estimations for subsamples of candidates according to their gender (see Table 3).

[Table 3 goes here]

Comparing the most complete models for the two subgroups (Models (2) and (4)), we find that the quadratic mortality rate coefficient is almost twice as large for female than for male incumbents. This indicates that women in office are punished more harshly for a given Covid-19 mortality rate in their municipality than men.

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confirm a quadratic relationship between the mortality rate and incumbents' reelection probability. Further results from non-parametric and semi-parametric regressions confirm this finding (available upon request).

[26]Having established the curvilinear relationship between outcome and treatment variable, we can confirm the absence of pre-trends (argued in Figure A.2) using an event-study graph as suggested by Roth (2022). Figure A.3 plots the linear coefficient estimates obtained from regressing the reelection probability of incumbents on interactions of Covid mortality plus controls and fixed effects. The estimated effects of Covid mortality in treatment year is larger than the pre-trend coefficients and is the only one that lies within the 90% confidence interval. This additionally underlines the validity of our empirical approach already discussed in Section 3.2.

[27]In Section A.4 of the online appendix, we run estimations where we additionally control for infections and Covid-19 deaths that occurred in the two weeks right before the election. The results are qualitatively the same as in the baseline, i.e. the baseline results are not driven by short-term emotional voting.

Figure 6 visualizes the quadratic relationship while indicating the relevant percentiles of the mortality rate within our sample with dotted vertical lines. Note that the slope of the effect changes its sign only after the 95th percentile of the mortality rate, i.e. it is hardly relevant for our sample. The straight line refers to female incumbents, while the dashed line refers to male incumbents. The figure indicates that at all levels of mortality female incumbents are punished almost twice as harshly as their male counterparts. Evaluating the marginal effect at the mean mortality rate of about one death per 1000 inhabitants, male incumbents are 4.3 ppts less likely to be reelected while women are 7.8 ppts less likely to be reelected.[28]

[Figure 6 goes here]

5 Robustness

5.1 Alternative samples and specifications

We use two alternative samples to test the robustness of our results. First, since our election data does not have full coverage (see Figure 3), we test the robustness of our results using a balanced municipality-election panel for 2006 to 2021. For 132 municipalities in our dataset, data is available for all four elections. Model (2) of Table A.4 shows that with this balanced sample the results are qualitatively the same as in the baseline. As a second test, we exclude the five largest cities from the sample as outliers given their size and special status. Model (3) shows that our estimates are robust to their exclusion from the sample.

[28]This decline in incumbency advantages naturally raises the question of who was instead elected into the local councils. Additional estimations in Table A.12 and the associated margins plots in Figure A.11 indicate that both male and female non-incumbents appear to have benefited from the smaller incumbency advantages, with the gains being larger and closer to reaching conventional levels of significance (t-statistic of 1.3 for the squared treatment) for male non-incumbents. In these estimations at the municipality level, only the coefficients for the female non-incumbents are significantly negative. The effects for the four groups (male/female incumbents/non-incumbents) do not add up to zero due to the unbalancedness of the dataset.

We additionally run three estimations to test the robustness of our baseline estimates to changes in the model specification. First, since our dependent variable is a dummy variable, we estimate a binary probit model. Model (4) of Table A.4 shows that the probit estimation results are qualitatively in line with our previous results. The magnitude of the coefficients differs as the interpretation is now different. However, the probit results also indicate a statistically significant curvilinear relationship and are comparable in size. Second, in Model (5), we scale the treatment with the inverse hyperbolic sine to reduce the influence of potential outliers and to take account of the right-skewed nature of our dependent variable. The results are qualitatively the same as in the baseline.[29] Third, to address concerns about spillover effects between neighboring municipalities, we use the spatial correction proposed by Conley (1999) with a threshold of 20km. This means that the error of each municipality is assumed to be correlated with those of all municipalities located within a 20km radius. The results are not affected by this change in the specification (Model (6)).[30] Overall, the robustness tests in this Subsection using alternative samples and specification confirm a gender gap of about 2.4 (IHS) to 5.2 (balanced sample) which is in line with the baseline gap of 3.5 ppts (see Figure A.6).

5.2 Placebo tests

To further substantiate our estimates, we conduct two types of placebo tests.[31] First, we randomly reassign treatment (Covid-19 mortality rate) among all municipalities in our sample.[32] We then reestimate our baseline model for each reassignment using Monte Carlo-type simulations (Cameron and Trivedi, 2010). Figure A.4 in the online appendix plots the density curves

[29]Due to the scaling of the treatment variable, the coefficient estimates are larger in magnitude. However, the gender gap persists. When these estimates are transformed back to the original scale, a one-unit increase in the IHS mortality rate is equivalent to a rise of 0.36 units in the original mortality rate.

[30]The results remain unchanged when using 10, 30 and 40 kilometers of radius and also when additionally accounting for temporal correlations with a one year lag (available upon request).

[31]In doing so, we follow the traditional and more recent literature on randomization inference (see for instance Fisher (1935), Imbens and Rubin (2015), Young (2019)).

[32]We use mortality rates within the range of the actual mortality (from 0 to 4.7).

of the reestimated coefficients based on 1,000 draws and compares them to the baseline separately for female and male incumbents. The linear and quadratic treatment coefficients from the baseline end up in the left and right tail of the density curves. Thus, hardly any random assignments of the mortality rate produce a larger effect on incumbents' reelection probabilities.

In a second placebo test, we shift the treatment from the 2021 election to the 2016 election. The corresponding results in Table A.6 report almost precise zeros for the effect of Covid-19 mortality on the reelection of male and female incumbents. This additionally confirms that our results are not artefacts of a correlation between municipalities' characteristics and the spatial distribution of Covid-19 mortality.

5.3 Incumbents' vote share

We use incumbents' individual vote shares instead of a reelection dummy as the outcome variable in our estimations as an alternative test for robustness. Vote shares are calculated as the share of votes an individual candidate receives relative to the total votes cast in a given municipality. The advantage of this outcome variable is that it is a more sensitive measure than the reelection dummy and that it has more variation. On the other hand, we prefer the reelection dummy for the main specification because getting reelected bears direct consequences and is the most important electoral outcome.

Table A.5 collects the results using this alternative outcome variable. The estimated coefficients have the same sign and statistical significance as in the baseline both for male and female incumbents. The results indicate a curvilinear relationship between Covid-19 mortality and incumbents' vote shares of both genders. For male incumbents, the quadratic coefficient is about 50% larger indicating that women are punished more strongly in terms of votes shares and that this gap grows with the level of the mortality rate. In terms of marginal effects at the mean mortality, we find that with one additional death/1,000 inhabitants female and male incumbents lose 9.9 and 3.2 ppts of their vote shares, respectively (see Figure A.6).

5.4 Excess mortality

In a final robustness test, we use a measure of excess mortality instead of the officially reported Covid-19 mortality data. Whether individuals are included in the Covid-19 mortality data depends on whether they were tested positive before or after their death. This may indicate measurement error. Since official excess mortality data is only available at the county level but not at the municipality level, we construct our own measure of excess mortality for the year 2020 following existing methodology. This, of course, requires discretionary choices as to how this measure is calculated. In addition, voters could read about official Covid-19 mortality rates in the news. For these two reasons, we believe that the official statistics are better suited for the baseline estimations than a measure of self-constructed excess mortality.

We derive excess mortality as follows. First, we employ a two-way fixed effects regression model, using annual mortality rates across municipalities from 2016 to 2020. This model uses various controls to account for time-varying fiscal and demographic characteristics, as well as the lagged mortality rate to control for temporal persistence.[33] Using this model, we then calculate predicted mortality for 2020 and calculate the difference between actual and predicted mortality in 2020 for each municipality.[34]

The results, presented in Table A.7, are broadly in line with our baseline results. Specifically, the marginal effect of excess mortality at the mean corresponds to a 8.8 ppt decrease in the reelection probability of female incumbents. In contrast, this effect is statistically insignificant for the male subsample, confirming a differential impact of the crisis by gender.

[33]Figure A.5 compares the distribution of the calculated excess mortality with the actual mortality rate in 2021. The overlap of these distributions supports the validity of our approach to calculating excess mortality.

[34]To ensure a meaningful comparison with our baseline results - which only includes non-negative values of Covid-19 mortality – we include only municipalities with positive excess mortality in 2020. Repeating the same exercise for our baseline sample of 224 municipalities yields similar results.

6 Mechanisms

We investigate five alternative mechanisms that could explain our main baseline finding, i.e. that female incumbents were significantly less likely to be reelected than male incumbents when the impact of the pandemic crisis was more severe (higher Covid-19 mortality).

6.1 Party-level effects

In open-list elections, parties wield significant influence in shaping electoral outcomes through strategic placements of candidates on list ranks. Candidates ranked higher on a list are more popular and tend to secure a disproportionately larger share of votes. If female incumbents tend to be disadvantaged in their initial list placement and if this was especially the case in 2021, this may provide an alternative explanation for our main results.[35] A second party-level effect that may explain our baseline result is that a shift in the popularity of parties due to Covid-19 may have hurt individual parties, maybe especially those with many female incumbents. This may explain why female incumbents had a harder time getting reelected than male incumbents.[36]

In Table 4, we report the results for additional estimations that include both party dummies as well as initial ranks as controls to address both potential party-level explanations for the main results.[37] The inclusion of party dummies (Models (1)-(2) and (4)-(5)) hardly changes the main results (compared to Models (1)-(4) in Table 3). As expected, incumbents' initial rank adversely impacts their reelection probability. Being placed one rank further below implies

[35]Table A.10 in the online appendix collects results for estimations on determinants of initial ranks: women are on average placed 0.4 ranks higher (i.e. further at the top) than male candidates and incumbents are placed on average 7 ranks higher than non-incumbents, while female incumbents are moved up an extra 0.75 ranks. Thus, female candidates are generally placed favorably. Most importantly, the results show that Covid-19 mortality in 2021 has on average not induced parties to place female candidates in a different way on initial ranks.

[36]Shifts in party success in Hessian local elections in 2021 vs. 2016 are illustrated in Figure A.12. The SPD lost about 5 ppts and the Greens lost about 7 ppts. These center-left parties indeed tend to be more female-friendly.

[37]In Table A.11 in the online appendix, we run the same estimations using party vote shares as controls instead of party fixed effects. The results are qualitatively similar.

that the reelection probability of both male and female incumbents declines by 2.6 or 2.7% (see Models (3) and (6)). Even though the mortality rate squared coefficient slightly shrinks in these most complete models, the gender gap persists: at the mean mortality rate an additional death/1,000 inhabitants causes that male and female incumbents are 3.4 and 6.7 ppts less likely to be reelected (see Figure A.16 in the online appendix).

[Table 4 goes here]

Overall, we conclude that the gender gap is not due to the gender composition of parties' incumbent councilors and party-level shifts in voting and that there is no strategic response by parties in the placement of female councilors on candidate list ranks due to Covid-19.

6.2 Gender recontest gap

Another explanation for our main result could be that in 2021 women and men made different choices in terms of running again as an incumbent. The Covid-19 pandemic was particularly challenging for young families and often especially women had to take a step back. If especially the most experienced and most popular female incumbents decided not to run in 2021, this may explain why male incumbents were more likely to get reelected than female incumbents in municipalities where Covid-19 mortality was particularly high. To address this issue, we first analyze descriptively whether the gender recontest gap was particularly pronounced in 2021.

In Figure A.13, we illustrate the percentages of female and male incumbents who, after being elected in election t-1, opted to run again in the succeeding election t. An over-time comparison reveals that the gender disparity in the decision to rerun witnessed a steady decline as of 2006, narrowing to a mere 2 ppts in 2016. This trajectory was disrupted in 2021, when the gap between the share of female and male incumbents choosing to rerun expanded to 5 ppts.

To further investigate the potential impact of elevated mortality rates on gender gaps in recontest decisions, we construct a model with a recontest dummy as dependent variable and the mortality rate, a female candidate dummy, and their interactions as explanatory variables (see Table 5). Although the gender recontest gap highlighted in Figure A.13 is confirmed (about 4 ppts), Covid-19 mortality rates have not affected the size of the gender recontest gap.

[Table 5 goes here]

Nevertheless, it could be that female incumbents that opted out in 2021 have specific observable characteristics that differentiate them from female incumbents that did recontest. In Figure A.14, we compare female incumbents who stood for the 2021 elections against those who abstained. Subfigure (a) uncovers a significant age disparity: female non-recontesters were on average ten years younger than female recontesters. This indeed suggests that larger family duties due to childcare/school closures, might have played a role. In contrast, Subfigures (b), (c) and (d) reveal no differences in education or prior election outcomes (final rank in 2016 and tenure in office), refuting the hypothesis that the 2021 female incumbents on average performed worse due to a concentration of lower-performing or less qualified female candidates.

6.3 Confoundedness of gender

Gender may coincide with other characteristics. Therefore, the gender bias that we identify in blind retrospective voting may be due to other differences between candidates. To address this issue, we run additional estimations that include additional individual controls (see Table 6).

[Table 6 goes here]

The results in Table 6 indicate that older incumbents are slightly less likely to get reelected both in the male and in the female incumbent subsample. Being one year older reduces this probability by about 0.4 to 0.6 ppts. Education is captured with a categorical dummy variable that ranges from (1) for high school degree to (4) for PhD. It appears that for women educational degrees do not influence reelection probabilities while for male incumbents having one more degree raises the reelection probability by 2.2 ppts. Models (3) and (6) are the most complete models and additionally include dummies for occupations.

Most importantly, the nonlinear effect of Covid-19 mortality on female and male incumbents' reelection is essentially not affected. Evaluating the marginal effect at the mean mortality rate of one death/1000 inhabitants, with one additional death male and female incumbents are 5.5 and 8.1 ppts less likely to be reelected (see margins plot in Figure A.16 in the online appendix). We conclude that what we observe is indeed a gender gap and not a result of candidates' differences in other characteristics.

6.4 Gender gap in meeting attendance

One alternative mechanism is that female incumbents reelected at a lower rate because they were less able to fulfill their duties as councilors. If female councilors were on average absent more often than male councilors due to larger family duties (closed schools/daycare, isolating elderly relatives) and if this was especially the case in municipalities with high Covid-19 mortality, this may explain our main findings.

To investigate this channel, we hand-collect minutes for council meetings that were held over the 2016-2021 legislative period and code councilor attendance (see Section A.1.4 in the online appendix for details). We calculate the absence rate for each councilor as the share of missed meetings. Figure A.15 illustrates average quarterly absence rates by gender. Prior to the pandemic, men and women exhibited similar absence rates of about 15%. In the early Covid-19 period, the absence rate for female councilors diverges, peaking at 23% in Q2-2020. Towards the end of the term (Q4-2020 and Q1-2021), female councilors were on average 4% more absent than male councilors, coinciding with the first and second Covid-19 waves.

To test whether this gender gap in attendance explains the harsher electoral penalties faced by female incumbents in high-mortality municipalities, we run regressions similar to our baseline model. We regress the 2021 reelection dummy on the mortality rate and its square and additionally control for meeting attendance. Note that these are not continuous DiD estimations but OLS estimations (only using 2021 election data) that investigate correlations, notably whether the link between Covid-19 mortality and gender-specific reelection likelihoods is affected by controlling for councilors' meeting absence. The results are collected in Table 7.

[Table 7 goes here]

Despite the inclusion of councilors' absence rate in the twelve months before the election as a control, the gender gap from the baseline estimations persists. Margins plots for Models (2) and (4) in the online appendix (see Subfigure A.16d) reveal that male and female incumbents are 8.7 and 12.3 ppts less likely to get reelected, i.e. the gap aligns with the baseline results.[38] We conclude that voters appear to disproportionately attribute the blame for the crisis to female incumbents, irrespective of their observable activity in the local council.

6.5 Voter turnout

One alternative explanation for our finding that higher mortality led to a lower likelihood for (male and female) incumbents to get reelected is that high Covid-19 mortality kept citizens from casting their votes, i.e. from turning out. Thus, our baseline results may be driven by a change in the share/composition of the electorate that turned out.[39]

To investigate the relevance of this channel, we have obtained official data from the Hessian Statistical Office on municipal turnout rates per election. We first test whether local Covid-19 mortality rates affected voters' turnout decision. In Models (1) and (2) of Table 8, we reestimate our baseline model using the turnout rate as outcome variable. The results indicate that

[38]Note that the estimations in Table 7 are based not only on 2021 election results but also only 52 municipalities for which minutes were available for the entire legislative term. Therefore, the deviation in the level of reelection probabilities from the baseline is not surprising. To isolate the effect of including absence rates as controls, we report an additional margins plot in Subfigure A.16c) with an effect size of 9.7 and 12.5 ppts. Thus, the inclusion of absence rates even widens (rather than shrinks) the gender gap (from 2.8 to 3.6 ppts).

[39]Existing literature is inconclusive about the effect of turnout on the electoral performance of incumbents. Theory suggests that increased turnout may be related to incumbency advantage losses (Grofman, Owen and Collet, 1999). While Godbout (2013); Hansford and Gomez (2010); Martins and Veiga (2014); Trounstine (2013) report a negative effect of a higher voter turnout on incumbents' electoral success, Frank, Stadelmann and Torgler (2023) discover that in Bavarian mayoral elections, a rise in turnout brought benefits to incumbents. On the other hand, Baskaran, Hessami and Khasanboev (2023) find that slightly increased turnout due to Covid-19 outbreaks had hardly any effect on incumbents' reelection probabilities in Bavarian council elections.

one additional Covid-19 related death per 1,000 inhabitants linearly reduces the turnout rate approximately by 1.5 ppts (statistically significant at the 10% level).

[Table 8 goes here]

In a second step, we test if this decrease in turnout drives our main results by reestimating our main specification including the turnout rate as an additional control (see Models (3)-(6)). In the most complete models for the male and female subsample (Models(4) and (6)), we find a small negative effect of turnout on reelection probabilities (which is only significant for the male subsample, however). More importantly, we find that the effect of mortality on male and female incumbents' reelection likelihood are unaffected, indicating that a change in turnout, which has a negligible effect on reelection probabilities, does not drive our main finding.

7 Long-term effects on female representation

Does a one-time crisis that depresses female incumbents' reelection probabilities affect female representation in the future? In additional simulations, we construct a counterfactual scenario without Covid-19 and illustrate that this one-time shock has persistent negative effects on the female councilor share. With rising female representation in the past in Hessian local councils (2006: 21.6%, 2011: 24.5%, 2016: 27.4%), we simulate a 29% share in 2021 in the absence of Covid-19 in contrast to the actual share of 27.3% in 2021.

In Figure 7, we illustrate future female councilor shares by extrapolating female-specific recontest rates and reelection rates. In particular, we contrast a scenario with the Covid-19 shock (one SD treatment) and without the shock. These simulations are based on observed information that is summarized in Figure A.17 in the online appendix.

[Figure 7 goes here]

Our simulations indicate a lasting 3-4 ppt gap even for ten elections in the future. In an average council with 31 seats, this means that 1.3 councilors are male instead of female. While this may not seem like a large effect and only maintains a preexisting minority status, existing literature indicates that especially in small councils an additional woman can make a difference with regard to policy choices for public goods, e.g. the provision of child care (Baskaran and Hessami, 2024).

8 Conclusion

This paper sheds light on a so far neglected potential reason for women's underrepresentation in politics: crisis-induced gender gaps in the reelection of incumbents. Using hand-collected data on 173,339 candidates in open-list local council elections (1997-2021) in a German state, we apply continuous difference-in-differences estimations using exclusive Covid-19 mortality data to measure treatment intensity.

We analyze whether voters change their voting behavior in a crisis, exploiting local elections in Hesse that took place one year into the Covid-19 pandemic in mid-March 2021. At this point in time, local mortality rate increases due to Covid-19 had been observed by voters for a full year, less than 10% of voters had received their first vaccination shot, infection rates continued to be high, and it was clear that the pandemic would cause more damage in the coming months or even years. Interestingly, in our setting (individual) local councilors had no role in fighting the pandemic, placing our paper within the *blind retrospective voting* literature.

Descriptively, incumbent councilors typically enjoy a large incumbency advantage in Hesse. We find that while the incumbency advantage remained high in 2021, it dropped in those municipalities where local Covid-19-related mortality rates were particularly high. Our main result is, however, that we uncover a *gender blame attribution gap* for incumbent councilors in times of crisis. At a mean Covid-19 mortality of one death per 1,000 inhabitants, an increase by one death leads to a 4.3 vs. 7.8 ppt lower reelection probability for male and female incumbents, respectively. We exclude five alternative potential mechanisms (party-level effects, gender recontest gaps, confoundedness of gender, gender gaps in meeting absence, turnout effects) and interpret our results as voter bias against female candidates in blind retrospective voting.

Additional simulations indicate a lasting 3-4 ppt gap even for ten elections in the future. In an average council with 31 seats, this means that 1.3 councilors are male instead of female. While this may not seem like a large effect and only maintains a preexisting minority status, existing literature indicates that especially in small councils an additional woman can make a difference with regard to policy choices for public goods, e.g. the provision of child care (Baskaran and Hessami, 2024).

Our results provide important insights for our understanding of voter behavior and voters' attribution of blame for an exogenous crisis. It appears that voters associate a dire situation with those politicians who happen to stand for reelection during a pandemic, in particular female incumbents. Given this evidence, it stands to reason that holding elections during "the eye of the storm" unduly affects political selection, in particular regarding politicians with specific personal characteristics such as female politicians.

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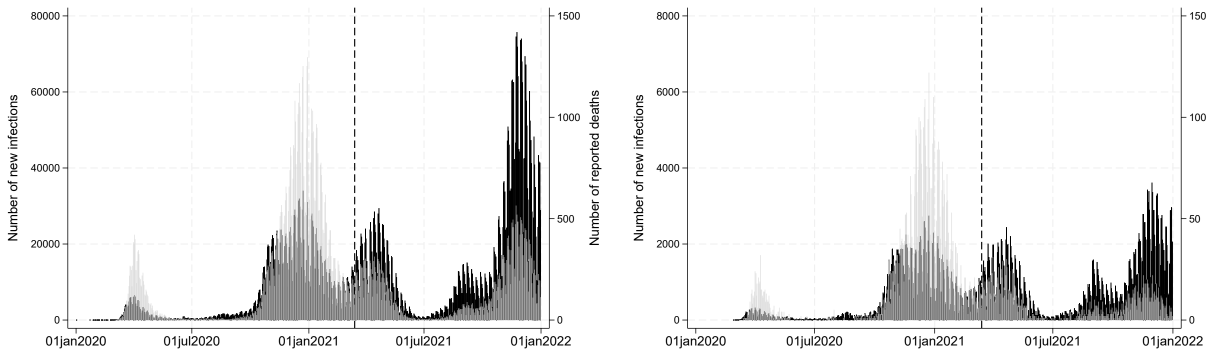
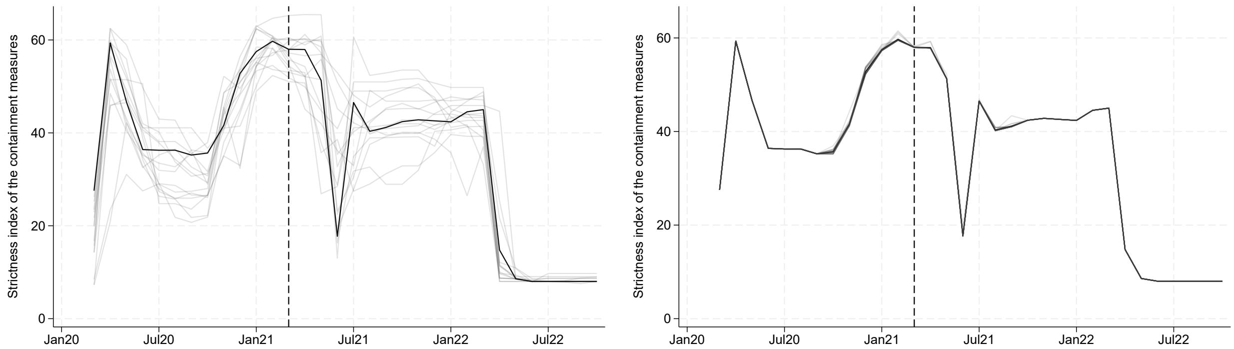


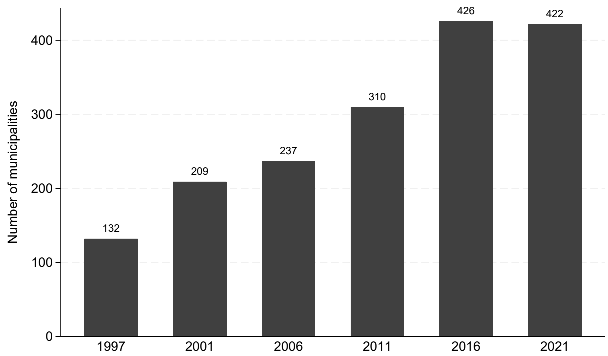
(a) Germany **(b)** Hesse

Figure 1: Covid-19 infections and mortality: Germany vs. Hesse. This figure plots the development of Covid-19 infections (left y-axis) and mortality (right y-axis) from January 2020 to December 2021. Subfigure (a) illustrates the daily number of infections and deaths in Germany, while in subfigure (b) the corresponding numbers for Hesse are presented. In both graphs, black-shaded bars represent the number of infections, while grey-shaded bars represent the number of deaths. The vertical dashed lines indicate the election day (March 14, 2021).

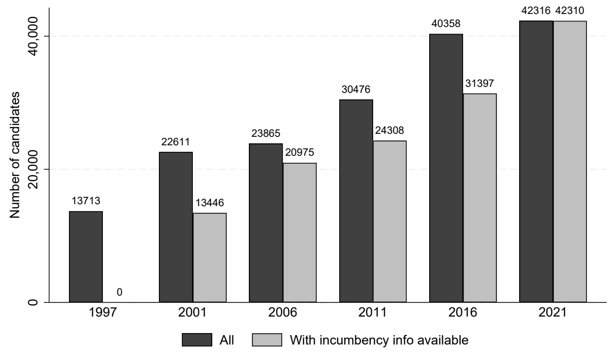


(a) Across German states **(b)** Across Hessian counties

Figure 2: Strictness of Covid-19 containment measures across Germany and Hesse. This figure illustrates the strictness of monthly Covid-19 containment measures using an index scaled from 0 to 100. The data is provided by the Federal Statistical Office. Subfigure (a) illustrates the variation of the index across sixteen German states, while in subfigure (b) the corresponding numbers for twenty-one Hessian counties are presented. In both graphs, the solid line represents the strictness index for Hesse. The vertical dashed line in each figure indicates the election month (March 2021).



(a) Elections/municipalities



(b) (Non-)incumbents identification

Figure 3: Data coverage: elections and council candidates. This figure illustrates our electoral data coverage across municipalities/elections and candidates. Subfigure (a) shows the number of municipalities included in our sample in each legislative period (corresponding with the number of elections for which we have data). For the last two elections we have full coverage. The official number of municipalities in Hesse has shrunk from 426 to 422 due to mergers that took place after 2016. Subfigure (b) illustrates how many candidates we could identify as (non-)incumbents.

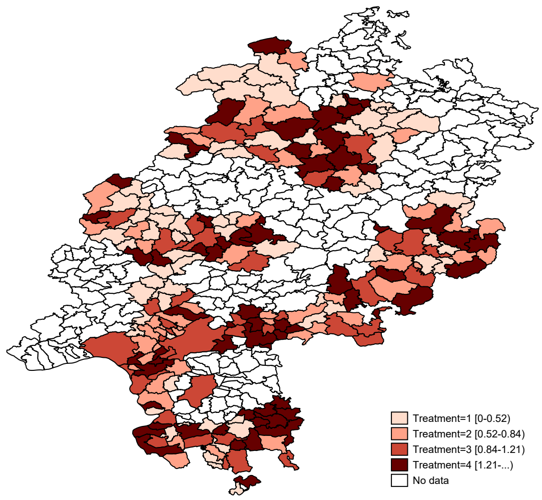


Figure 4: Spatial distribution of Covid-19 mortality. This figure illustrates the geographical dispersion of Covid-19 mortality rates across Hessian municipalities (within the eleven counties that provided us with data). Municipalities are grouped via the mortality rate quartiles in which they fall (cutoffs are at the 25th, 50th and 75th pctiles). The darker the shade, the higher was the treatment dose, i.e. the higher are the cumulative Covid-19 mortality rates (in the year before the election).

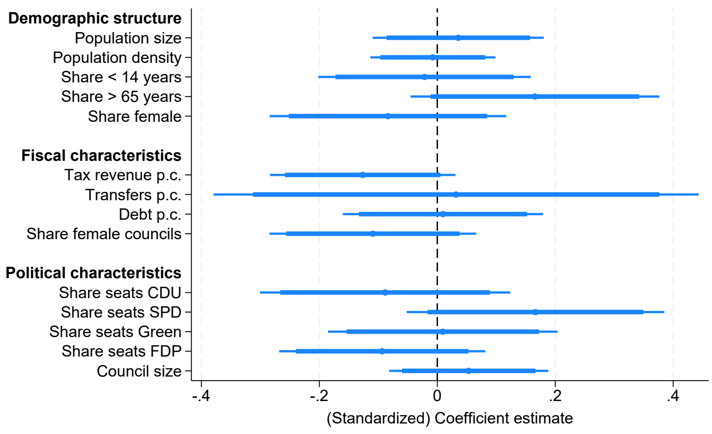


Figure 5: Balance tests. This graph reports coefficients from a regression of Covid-19 mortality rates on various observable municipality characteristics. Demographic and fiscal variables refer to 2020, whereas council characteristics of municipalities refer to the latest pretreatment elections in 2016. Both the dependent and independent variables are standardized. Standard errors are clustered at the municipality level. For each variable indicated on the y-axis, the figure shows a point estimate (dot) and a 90% (thick line) and 95% (thin line) confidence interval.

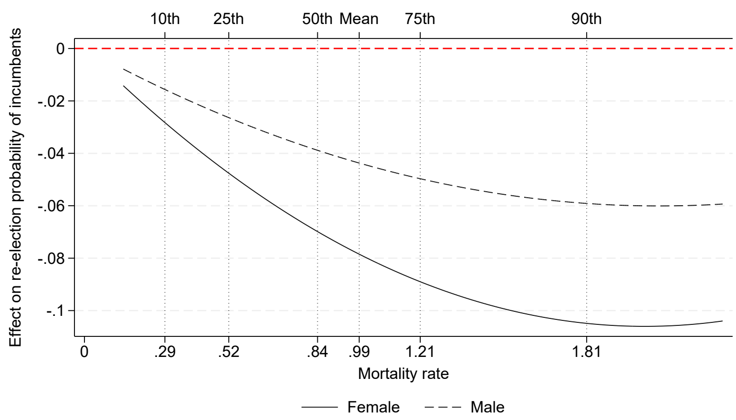


Figure 6: Treatment effect across mortality rates, male vs. female incumbents. This figure plots quadratic curves using the treatment effect coefficients from Table 3. The dotted vertical lines refer to the respective percentiles of the mortality rate labeled on the upper x-axis. The solid curve refers to female incumbents and the dashed line refers to male incumbents. The y-axis reports effect sizes in terms of ppts of the reelection probability of incumbents.

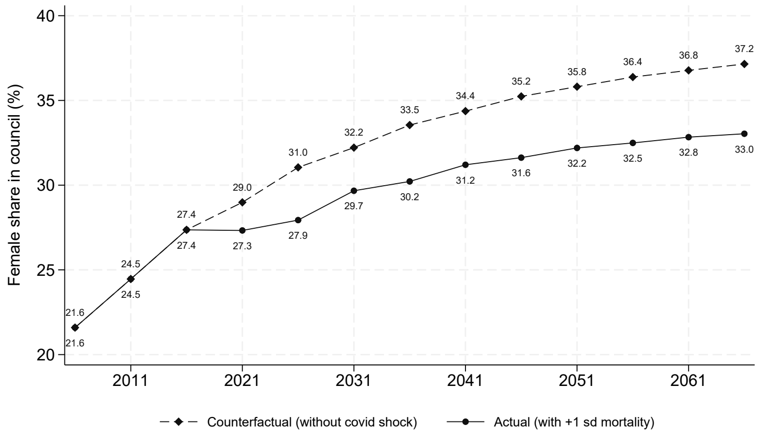


Figure 7: Long-term effect of Covid-19 on female councilor share. This graph models the projected outcomes of future elections concerning the proportion of female representation in an average Hessian local council with 31 seats. The solid line represents the scenario where the crisis persists in future elections, indicating the potential long-term impact of the pandemic. The dashed line depicts the counterfactual scenario where no pandemic took place. The simulation steps that we followed and the observed information on recontest and reelection probabilities that we used to generate these projections are summarized in Figure A.17 in the online appendix.

Table 1: Covid-19 mortality and incumbents' reelection, treatment doses

Dep. var.: Reelected	(1)	(2)	(3)	(4)	Linear effect of treatment (5)
Treatment=2	-0.039***	-0.034**	-0.031	-0.049**	
	(0.014)	(0.017)	(0.023)	(0.025)	
Treatment=3	-0.068***	-0.062***	-0.058**	-0.064**	
	(0.018)	(0.022)	(0.026)	(0.028)	
Treatment=4	-0.025*	-0.019	-0.033	-0.046*	
	(0.014)	(0.018)	(0.024)	(0.026)	
Treatment					-0.015*
					(0.008)
Year FE	No	Yes	Yes	Yes	Yes
Municipality FE	No	No	Yes	Yes	Yes
Municipal level controls	No	No	No	Yes	Yes
Observations	15733	15733	15733	15098	15098
R-squared	0.00	0.00	0.02	0.02	0.02

Notes: In this table, we estimate the effect of Covid-19 mortality on the reelection probability of incumbents. Treatment coefficients reported in Model (1)-(4) report the effect on the reelection probability in these groups compared to the lowest treatment group (treatment=1). Model (5) reports the linear effect treating the treatment variable (with values of 1, 2, 3 and 4) as a continuous variable. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Table 2: Covid-19 mortality and incumbents' reelection, quadratic model

Dep. var.: Reelected	(1)	(2)	(3)	(4)
Mortality rate	-0.057***	-0.054***	-0.057**	-0.069**
	(0.011)	(0.019)	(0.026)	(0.028)
Mortality rate^2	0.015***	0.015***	0.014***	0.016***
	(0.003)	(0.004)	(0.005)	(0.005)
Year FE	No	Yes	Yes	Yes
Municipality FE	No	No	Yes	Yes
Municipal level controls	No	No	No	Yes
Observations	15733	15733	15733	15098
R-squared	0.00	0.00	0.02	0.02

Notes: In this table, we estimate the effect of Covid-19 mortality on the reelection probability of incumbents, modeling a quadratic relationship, i.e. we include the mortality rate and its square as explanatory variables. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Table 3: Covid-19 mortality and incumbents' reelection, by gender

Dep. var.: Reelected	Female		Male	
	(1)	(2)	(3)	(4)
Mortality rate	-0.083	-0.105*	-0.050*	-0.058*
	(0.053)	(0.056)	(0.029)	(0.030)
Mortality rate^2	0.020*	0.026**	0.013**	0.014**
	(0.011)	(0.011)	(0.005)	(0.006)
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Municipal level controls	No	Yes	No	Yes
Observations	3611	3430	12115	11661
R-squared	0.07	0.08	0.03	0.03

Notes: In this table, we run the same regression model as in Table 2 but instead of the full sample we use a female incumbent (Models (1) and (2)) and a male incumbent subsample (Models (3) and (4)). Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Table 4: Mechanism I: Party-level effects, party dummies and initial ranks

Dep. var.: Reelected	Female			Male		
	(1)	(2)	(3)	(4)	(5)	(6)
Mortality rate	-0.085	-0.106*	-0.090*	-0.048*	-0.058*	-0.044*
	(0.053)	(0.056)	(0.046)	(0.029)	(0.030)	(0.026)
Mortality rate^2	0.022**	0.027**	0.022**	0.012**	0.013**	0.010**
	(0.011)	(0.012)	(0.010)	(0.005)	(0.006)	(0.005)
Initial list rank			-0.026***			-0.027***
			(0.002)			(0.001)
Party dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal level controls	No	Yes	Yes	No	Yes	Yes
Observations	3611	3430	3420	12115	11661	11613
R-squared	0.08	0.09	0.41	0.03	0.03	0.32

Notes: In this table, we reestimate the baseline model with gender subsamples in Table 3 additionally controlling for the party affiliation and initial list rank of incumbents. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Table 5: Mechanism II: Gender gaps in recontest decision

Dep. var.: Recontest	(1)	(2)
Female candidate	-0.039***	-0.036***
	(0.008)	(0.009)
Female candidate × Mortality rate		-0.005
		(0.024)
Female candidate × Mortality rate^2		-0.003
		(0.009)
Year FE	Yes	Yes
Municipality FE	Yes	Yes
Municipal level controls	Yes	Yes
Observations	21794	21794
R-squared	0.04	0.04

Notes: In this table, we estimate how Covid-19 mortality has comparatively affected male and female incumbents' decision to run in the next election. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Table 6: Mechanism III: Confoundedness of gender by other characteristics

Dep. var.: Reelected	Female			Male		
	(1)	(2)	(3)	(4)	(5)	(6)
Mortality rate	-0.123**	-0.107*	-0.107*	-0.083**	-0.075**	-0.073**
	(0.055)	(0.058)	(0.059)	(0.034)	(0.033)	(0.034)
Mortality rate^2	0.026**	0.025**	0.025**	0.020***	0.017**	0.017**
	(0.011)	(0.012)	(0.012)	(0.007)	(0.007)	(0.007)
Incumbent's age	-0.006***	-0.004***	-0.004***	-0.005***	-0.004***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Education degree		0.005	0.012		0.022***	0.025***
		(0.009)	(0.010)		(0.005)	(0.006)
Occupation dummies	No	No	Yes	No	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal level controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2828	2472	2471	9669	8891	8880
R-squared	0.11	0.10	0.11	0.05	0.05	0.05

Notes: In this table, we reestimate the baseline model in Table 3 additionally controlling for incumbents' age and education. Age is a continuous variable. Education is coded as a discrete, ordinal variable that ranges from 1 to 4 with 1 for high school graduates and 4 indicating incumbents with a PhD. In Models (3), (4) and (7), (8) we additionally control for incumbents' occupation via dummy variables. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Table 7: Mechanism IV: Gender gaps in council meeting attendance

Dep. var.: Reelected	Female		Male	
	(1)	(2)	(3)	(4)
Mortality rate	-0.158**	-0.155**	-0.128***	-0.118***
	(0.074)	(0.073)	(0.043)	(0.043)
Mortality rate^2	0.032**	0.031**	0.030***	0.030***
	(0.015)	(0.015)	(0.007)	(0.007)
Absence rate		-0.003**		-0.003***
		(0.001)		(0.001)
Municipal level controls	Yes	Yes	Yes	Yes
Number of municipalities	52	52	52	52
Observations	304	304	848	848
R-squared	0.06	0.08	0.03	0.04

Notes: In this table, we estimate whether the baseline results can be explained by additionally controlling for (gender gaps in) the absence of councilors during the last year of their term. These estimations rely only attendance rates for councilors in the 2016-2021 term and the electoral outcomes in 2021. The absence rate is calculated as the proportion of council meetings that a councilor did not attend between April 2020 and March 2021. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Table 8: Mechanism V: Voter turnout effects

			Female		Male	
Dep. var.:	Turnout	Turnout	Reelected	Reelected	Reelected	Reelected
	(1)	(2)	(3)	(4)	(5)	(6)
Mortality rate	-1.621**	-1.462*	-0.084	-0.105*	-0.053*	-0.060**
	(0.809)	(0.772)	(0.054)	(0.057)	(0.028)	(0.029)
Mortality rate^2	0.289*	0.253	0.020*	0.026**	0.013**	0.014**
	(0.168)	(0.164)	(0.011)	(0.012)	(0.005)	(0.006)
Turnout			-0.004	-0.004	-0.005***	-0.006***
			(0.003)	(0.003)	(0.002)	(0.002)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal level controls	No	Yes	No	Yes	No	Yes
Observations	853	853	3611	3430	12115	11661
R-squared	0.88	0.88	0.07	0.08	0.03	0.03

Notes: In this table, we test whether changes in turnout rates due to Covid-19 mortality had an effect on incumbents' reelection probability. Estimates in model (1) and (2) are based on a quadratic regression of the turnout rate on the continuous Covid-19 mortality rate and show that turnout was indeed lower where mortality was higher. In Models (3)-(6), we reestimate the baseline model additionally controlling for municipal turnout rates for local council elections. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Online appendix

A.1 Details on data collection

A.1.1 Council election data

We use the municipal election data from Baskaran and Hessami (2018) and extend it with the latest election results in 2021. Our student assistants retrieved the election results, which were often in pdf format, from municipal websites. The data was manually converted into Excel spreadsheets. For information on previous elections that was not accessible online, the mayor's office was contacted. The Excel spreadsheets were then combined into one dataset using the municipal code and year. As manual data collection is prone to inaccuracies, numerous plausibility tests were carried out to ensure data quality. Errors that were detected were either corrected or the relevant observations were coded as missing.

A.1.2 Fuzzy matching of council candidates

The availability of our hand-collected election data improves over time with the last two elections being complete. In order to identify incumbents and to gather additional information on the birth year and occupation of candidates in previous elections, we match candidates using a fuzzy match strategy.[40] Our goal is to identify candidates who contested in multiple elections. We match candidates according to their first and last names as well as their list, which are merged into a single string with all special characters removed.

Candidates are matched only within municipalities. The tolerance of the fuzzy match ensures that spelling mistakes and minor deviations are not an obstacle. However, there are certain drawbacks to this strategy. We cannot identify individuals who have migrated to other towns or who have changed lists between years. Changes in names as a result of marriages are not detectable. Also, it is impossible to rule out the possibility of two people with the same

[40]We use the Stata command *strgroup* by Julian Reif (University of Chicago).

name living in the same municipality and being on the same list. However, we believe that these inaccuracies are unrelated to outcomes, and hence are not a cause for concern.

A.1.3 Municipal Covid-19 mortality data

Our data collection took about four months (June-September 2022). We sent out our requests to all 21 counties in Hesse in four waves, i.e we sent them three emails and placed a phone call with one month-intervals. For the municipalities who did not respond to our first two emails, we tried to contact the responsible county office directly. Overall, we heard back from fourteen counties, eleven of which delivered the data. The remaining counties either refused to share the data or had not collected the Covid-19-related death statistics at the municipality level.[41] Overall, there are 215 municipalities and five county-free cities in our main sample.

Note that some municipalities merged in 2020, which means that these merger municipalities only exist in 2021. In order to overcome this inconsistency, we identify such municipalities and to which municipalities they were merged to and assign to them the same mortality rate calculated for their mergers where necessary.[42]

A.1.4 Meeting attendance rates based on council minutes

Our research assistants manually searched and downloaded the minutes of the council meetings in a PDF format from the websites of the municipalities in our sample for the legislative period of 2016 to 2021. We were able to find at least one minutes document for 156 out of the 224 municipalities in our sample. Further, given that municipalities do not adhere to a standardized format for their minutes, some of the available documents lacked attendance information.

To ensure consistency in the absence statistics in our analysis, we focused on a subset of 52 municipalities for which we had a complete set of minutes for all meetings held during the

[41] One county had delivered the death statistics with missing values for municipalities where the number of Covid-19-related deaths was smaller than 5. For our main sample, we replaced these missings with their potential median value of 3 (our results are consistent with missing values as well).

[42] There was only one such merger in our sample, which affected four municipalities.

term. The individual format of these minutes unfortunately precluded the use of automated digitization methods. As a result, our research assistants manually coded individual councilor attendance for each meeting, after which we conducted several validation tests to identify potential errors due to manual data entry.

A.2 OLS: Incumbency advantages (over time)

As a first step, we estimate general incumbency advantages of local councilors in Hesse:

$$\text{Elected}_{itm} = \beta * Incumbent_{itm} + \theta_t + \gamma_m + W'_{tm} + \varepsilon_{itm}, \tag{A.1}$$

where the dependent variable is a dummy variable that indicates whether a candidate i is elected into the council in municipality m in election t.[43] $Incumbent_{itm}$ is a dummy variable which is 1 for incumbent candidates. Our coefficient of interest β quantifies the effect of incumbency on the likelihood of being elected (compared to non-incumbents). θ_t and γ_m are time and municipality fixed effects. W'_{tm} is a vector of municipal control variables including population size, the share of women, the share of population over 65 and under 14, and council size.

Next, we estimate an interaction model to examine how the incumbency advantage changes over time using interactions between the incumbent dummy and a dummy for each election:[44]

$$\begin{aligned}\text{Elected}_{itm} = {} & \beta_1 * Incumbent_{itm} \times 2001_{itm} + \beta_2 * Incumbent_{itm} \times 2006 + \beta_3 * Incumbent_{itm} \times 2011 \\ & + \beta_4 * Incumbent_{itm} \times 2016 + \beta_5 * Incumbent_{itm} \times 2021 + \gamma_m + W'_{tm} + \varepsilon_{itm}. \end{aligned} \tag{A.2}$$

Table A.1 estimates incumbency advantages of local councilors in Hesse based on Equation (A.1). While Model (1) is a simple bivariate regression, we add fixed effects and time-

[43] As stated in Baskaran and Hessami (2018), the amount of votes or vote shares received by candidates is a subpar proxy for voter preferences in open-list elections since there is a positive association between initial rankings and final ranks of candidates. Therefore, in order to properly represent voter preferences, we include the candidates' ultimate election status as dependent variable in our regressions.

[44] Due to lack of data on candidates' incumbency status the 1997 election is excluded from the regressions.

varying controls in subsequent models. The estimates hardly change in Models (2)-(4). Model (4), which is the most complete model, shows that incumbents are 57% more likely to get elected into the council than their non-incumbents (in line with Figure A.10).

Table A.1: Incumbency advantages

Dep. var.: Election probability	Female			Male		
	(1)	(2)	(3)	(4)	(5)	(6)
Incumbent	0.575***	0.560***	0.560***	0.581***	0.568***	0.568***
	(0.009)	(0.008)	(0.008)	(0.006)	(0.004)	(0.004)
Time FE	No	Yes	Yes	No	Yes	Yes
Municipality FE	No	Yes	Yes	No	Yes	Yes
Municipal level controls	No	No	Yes	No	No	Yes
Observations	36360	36360	36360	96040	96040	96040
Municipalities	428	428	428	428	428	428
R-squared	0.23	0.26	0.26	0.28	0.29	0.29

Notes: This table collects regression results for Equation A.1. The estimations include municipality and time fixed effects. Municipal controls consists of demographic and fiscal characteristics of municipalities and council size. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

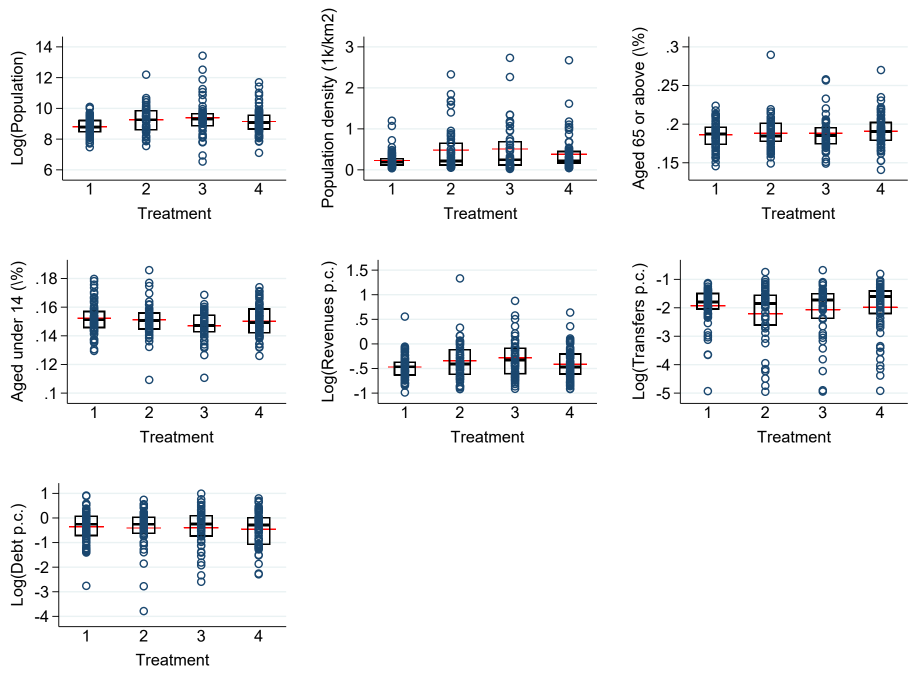
Table A.2 collects the results based on Equation (A.2). The only difference is that the incumbent dummy is interacted with election year dummies. The estimates should be interpreted as the difference in the election probability of incumbents and challengers in a given election year. In Model (4), the incumbency premium reaches its peak at 61% in 2006.

Table A.2: Incumbency advantages over time, 2001-2021

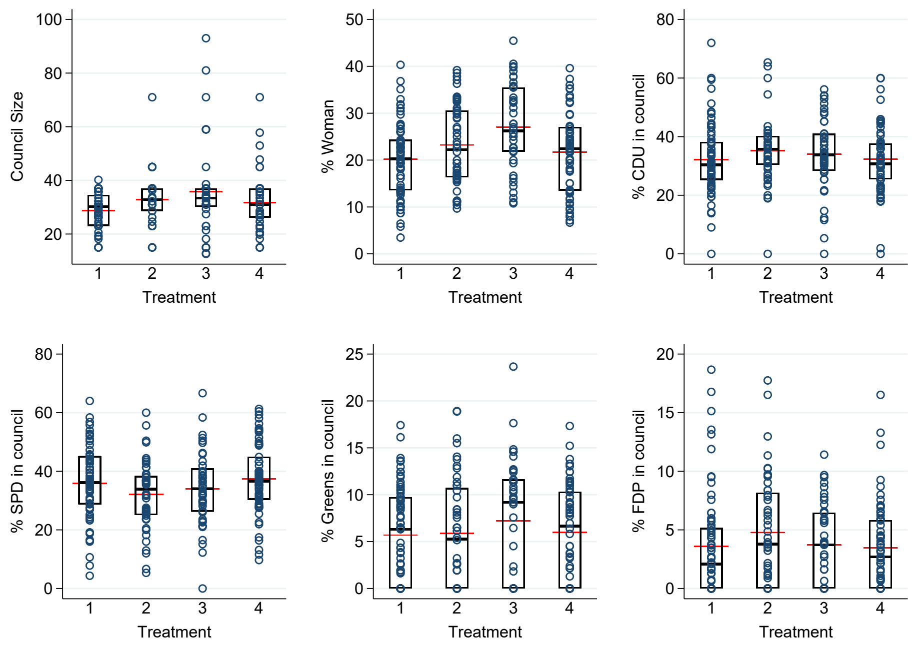
Dep. var.: Election probability	Female			Male		
	(1)	(2)	(3)	(4)	(5)	(6)
Incumbent × Election2001	0.546***	0.525***	0.526***	0.564***	0.531***	0.532***
	(0.023)	(0.024)	(0.024)	(0.011)	(0.014)	(0.014)
Incumbent × Election2006	0.612***	0.617***	0.617***	0.618***	0.609***	0.610***
	(0.016)	(0.016)	(0.016)	(0.008)	(0.009)	(0.009)
Incumbent × Election2011	0.590***	0.590***	0.590***	0.605***	0.594***	0.594***
	(0.013)	(0.013)	(0.013)	(0.008)	(0.008)	(0.008)
Incumbent × Election2016	0.563***	0.561***	0.560***	0.579***	0.563***	0.563***
	(0.013)	(0.013)	(0.013)	(0.008)	(0.007)	(0.007)
Incumbent × Election2021	0.565***	0.525***	0.516***	0.553***	0.543***	0.541***
	(0.012)	(0.012)	(0.012)	(0.007)	(0.007)	(0.007)
Time FE	No	Yes	Yes	No	Yes	Yes
Municipality FE	No	Yes	Yes	No	Yes	Yes
Municipal level controls	No	No	Yes	No	No	Yes
Observations	36360	36360	35394	96040	96040	94046
Municipalities	428	428	422	428	428	422
R-squared	0.23	0.26	0.26	0.28	0.29	0.29

Notes: This table reports regression results for Equation A.2. The estimations include municipality and time fixed effects. Municipal controls consists of demographic and fiscal characteristics of municipalities and council size. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

A.3 Continuous DiD validity



(a) Demographic and fiscal characteristics



(b) Political characteristics

Figure A.1: Municipality characteristics: four treatment dose groups. This figure presents results for balance tests across treatment groups. Subfigure (a) compares demographic and fiscal characteristics among the four treatment groups (averaged for 1991-2020). Subfigure (b) compares political characteristics of treatment groups (averaged for 1997-2016). The values on the x-axis in each plot indicate the respective treatment group.

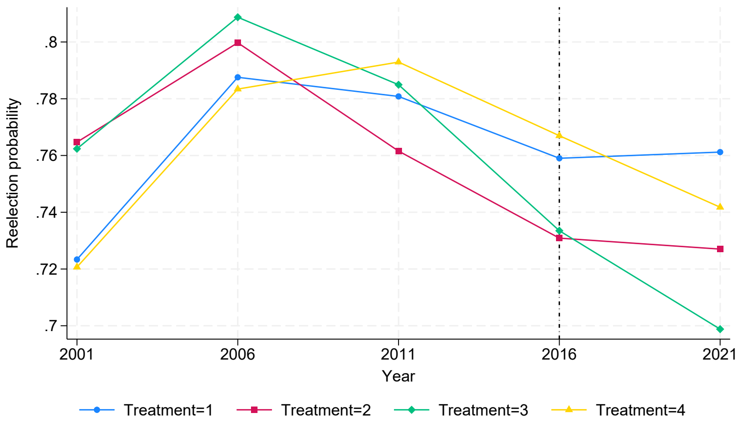


Figure A.2: Incumbents' reelection over time and across treatment dose groups. This graph illustrates the rate at which incumbent local councilors were reelected in Hesse over time and across the four treatment dose groups. Municipalities are grouped via quartiles of the mortality rate distribution. The dotted vertical line highlights the last election prior to the treatment.

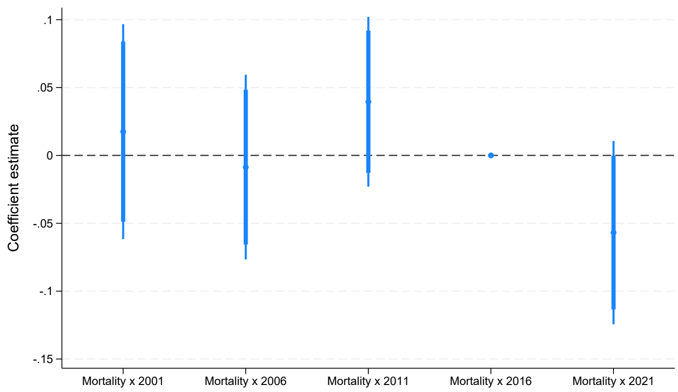


Figure A.3: Absence of pretrends. This event-study plot illustrates the coefficient estimates obtained from regressing the reelection probability of incumbents on the interactions of the Covid-19 mortality rate and its square plus controls and fixed effects. Only linear coefficients are considered. For each election year (see x-axis), the graph reports a point estimate (dot) and a 90% (thick line) and 95% (thin line) confidence interval.

A.4 Emotional voting

Are the losses in incumbency advantage due to Covid-19 mortality due to emotional voting, i.e. "blind" emotional reactions to recent events? Achen and Bartels (2012) illustrate that voters base their decisions on feelings of (dis)satisfaction which can be affected by circumstances, even if those circumstances are outside the government's control. They find that shark attacks on the east coast of the US were the reason why incumbent president Woodrow Wilson lost the presidential election in 1916. Healy, Malhotra and Mo (2010) find that the results of football or basketball matches by local teams which took place right before elections were mirrored in the election outcome; i.e. victories led to a 1-2 ppt increase in incumbents' vote share.

One could hypothesize that in places where the consequences of the disease were particularly severe immediately preceding the election in 2021, incumbents were less likely to get reelected. As described in Section 3.1.2, we collected Covid-19 data also for the fourteen days preceding the election. We add both short-term mortality and infection statistics as controls to our main specification. The results in Table A.3 show that neither absolute nor relative measures of mortality and infections in the immediate past have a significant effect on the outcome and do not affect the baseline treatment coefficients.

Table A.3: Emotional voting

Dep. var.: Reelected	(1)	(2)	(3)	(4)
Mortality rate	-0.068**	-0.066**	-0.069**	-0.070**
	(0.029)	(0.029)	(0.028)	(0.028)
Mortality rate^2	0.016***	0.016***	0.016***	0.017***
	(0.006)	(0.006)	(0.006)	(0.006)
Covid deaths (last two weeks)	-0.001			
	(0.003)			
Covid mortality (last two weeks)		-0.050		
		(0.078)		
Covid infections (last two weeks)			-0.000	
			(0.000)	
Infected population (% last two weeks)				0.051
				(0.076)
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Municipal level controls	Yes	Yes	Yes	Yes
Observations	15098	15098	15098	15098
R-squared	0.02	0.02	0.02	0.02

Notes: This table estimates the model controlling for Covid-19 deaths and mortality within the last 14 days before the election and tests if the voters punishing behavior was driven by their emotions. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

A.5 Robustness tests

Table A.4: Robustness I: Alternative samples and model specifications

Dep. var.: Reelected	Baseline (1)	Balanced sample (2)	Without large cities (3)	Baseline Probit (4)	Baseline IHS (5)	Baseline Conley (6)
Panel A: Female incumbents						
Mortality rate	-0.105*	-0.123**	-0.104*	-0.426*		-0.105**
	(0.056)	(0.059)	(0.056)	(0.219)		(0.050)
Mortality rate^2	0.026**	0.027**	0.025**	0.111**		0.026**
	(0.011)	(0.012)	(0.012)	(0.052)		(0.011)
IHS Mortality rate					-0.253**	
					(0.113)	
IHS Mortality rate^2					0.115**	
					(0.051)	
Observations	3430	2545	3039	3430	3430	3430
(Pseudo) R-squared	0.08	0.08	0.09	0.08	0.08	0.01
Panel B: Male incumbents						
Mortality rate	-0.058*	-0.067**	-0.059*	-0.192*		-0.058**
	(0.030)	(0.032)	(0.030)	(0.098)		(0.027)
Mortality rate^2	0.014**	0.014**	0.013**	0.047**		0.014***
	(0.006)	(0.006)	(0.006)	(0.020)		(0.005)
IHS Mortality rate					-0.116**	
					(0.058)	
IHS Mortality rate^2					0.050*	
					(0.028)	
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal level controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11661	8659	11019	11661	11661	11661
(Pseudo) R-squared	0.03	0.03	0.03	0.03	0.03	0.00

Notes: This table reestimates the treatment effect on female incumbents with different samples and model specifications. Model (1) reprints the baseline result. Model (2) reestimates the model using a balanced sample with 132 municipalities where we have data in all years; in Model (3), county-free municipalities are omitted. Model (4) is estimated using a binary probit model (with pseudo R-squared). In Model (5), the treatment is transformed using the inverse hyperbolic sine. Model (6) is estimated using Conley standard errors to allow for spatial spillovers. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Table A.5: Robustness II: Alternative election outcome

Dep. var.: Vote share	Female		Male	
	(1)	(2)	(3)	(4)
Mortality rate	-0.156	-0.140	-0.084	-0.059
	(0.102)	(0.087)	(0.111)	(0.087)
Mortality rate^2	0.031	0.040**	0.018	0.027*
	(0.020)	(0.018)	(0.022)	(0.015)
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Municipal level controls	No	Yes	No	Yes
Observations	3345	3168	11131	10706
R-squared	0.60	0.61	0.38	0.38

Notes: This table reestimates the treatment effect using vote share of female incumbents as an alternative outcome variable. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Table A.6: Robustness III: Placebo treatment in 2016

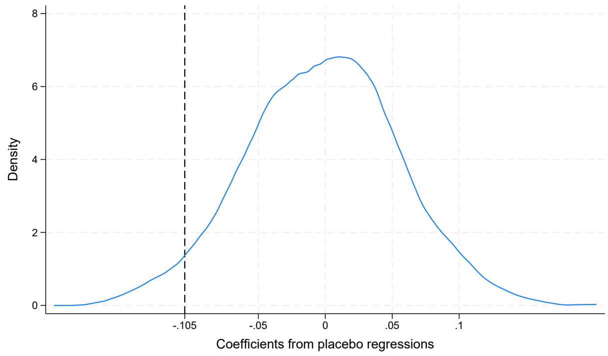
Dep. var.: Reelected	Female		Male	
	(1)	(2)	(3)	(4)
Mortality rate	0.043	0.066	-0.007	-0.004
	(0.048)	(0.048)	(0.030)	(0.031)
Mortality rate^2	-0.005	-0.009	0.005	0.004
	(0.009)	(0.009)	(0.005)	(0.005)
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Municipal level controls	No	Yes	No	Yes
Observations	3611	3430	12115	11661
R-squared	0.07	0.08	0.03	0.03

Notes: This table reestimates the baseline model assuming that 2016 (instead of 2021) is the treatment year. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

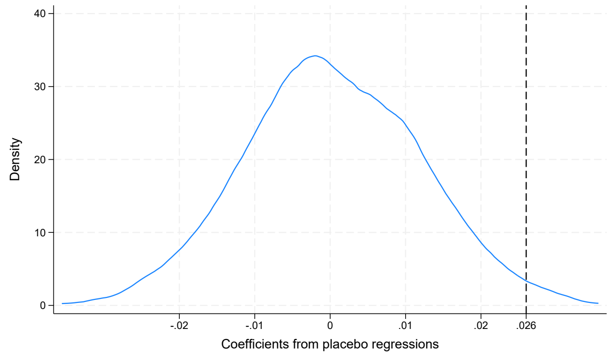
Table A.7: Robustness IV: Excess mortality as treatment

Dep. var.: Reelected	Female		Male	
	(1)	(2)	(3)	(4)
Excess mortality	-0.137*	-0.155**	-0.027	-0.015
	(0.079)	(0.077)	(0.044)	(0.046)
Excess mortality^2	0.055*	0.066**	0.007	0.004
	(0.032)	(0.031)	(0.014)	(0.015)
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Municipal level controls	No	Yes	No	Yes
Observations	3281	3270	11743	11704
R-squared	0.08	0.09	0.03	0.03

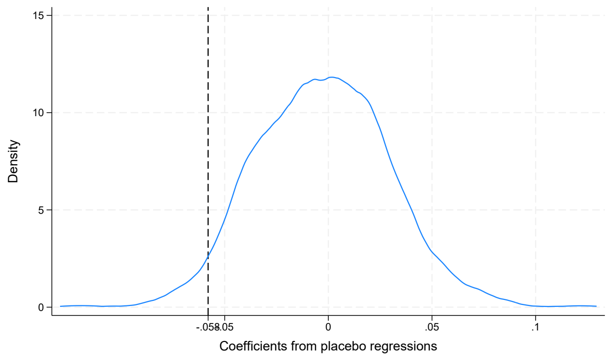
Notes: This table reestimates baseline model using excess mortality in 2020 as an alternative treatment variable. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.



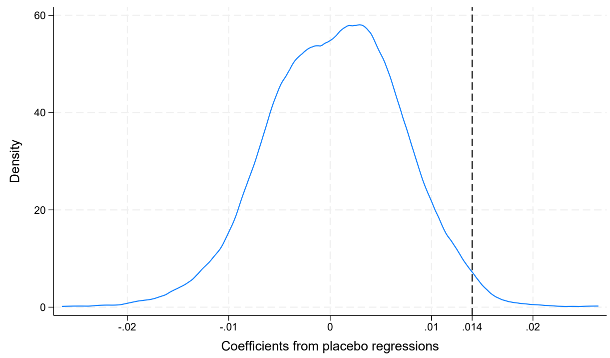
(a) Linear effect (female)



(b) Quadratic effect (female)



(c) Linear effect (male)



(d) Quadratic effect (male)

Figure A.4: Robustness III: Placebo test. This figure plots the density curves of (a, c) linear and (b, d) quadratic treatment coefficients that are estimated using random simulations of treatment assignment on subsamples with female and male incumbents. The vertical dashed lines mark the locations of the baseline estimates with actual mortality rate. The more to the left/right this lines are located, the more likely we are to reject the null hypothesis that the estimated treatment effects are produced rather by chance.

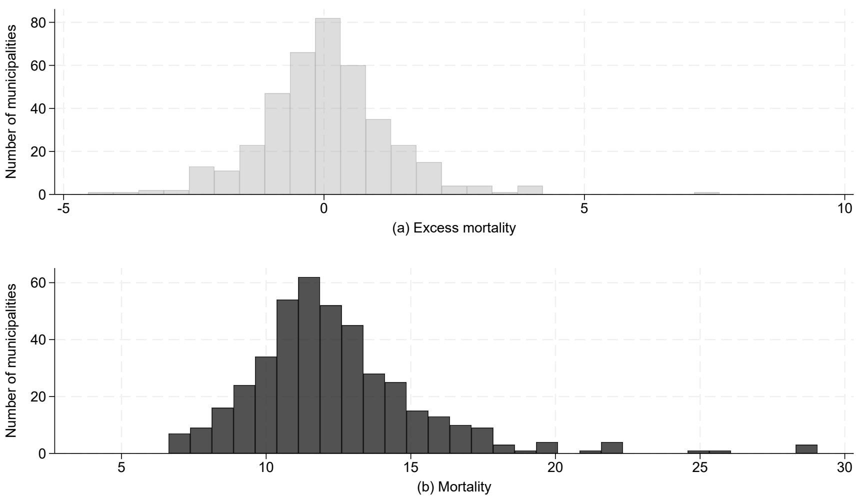
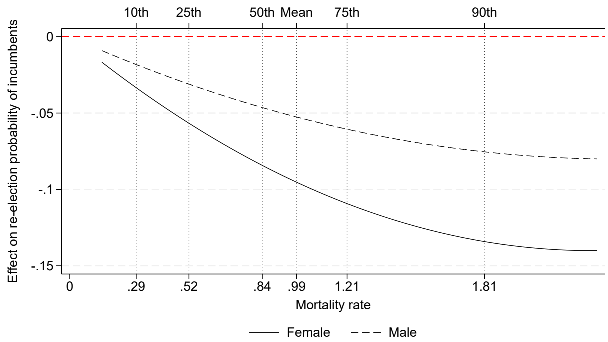
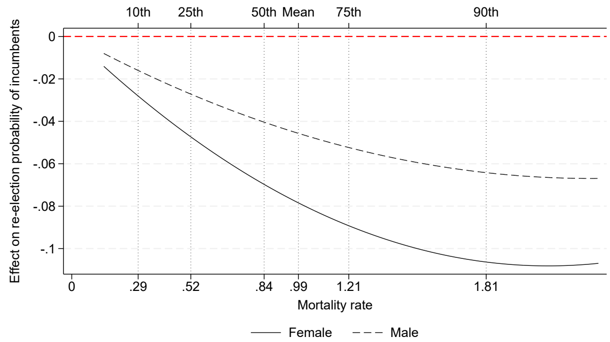


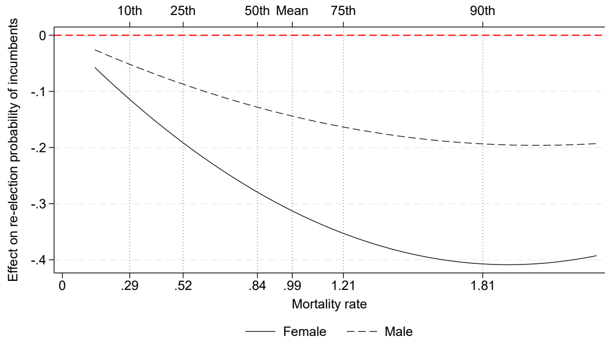
Figure A.5: Robustness IV: Mortality vs. Excess mortality. This figure plots the distributions of (a) excess mortality and (b) mortality in 2021. Excess mortality is calculated as described in Section 5.4, while mortality is obtained from the Hessian Statistical Office. The figure shows that the calculated excess mortality rate overlaps with the actual mortality rate in 2021.



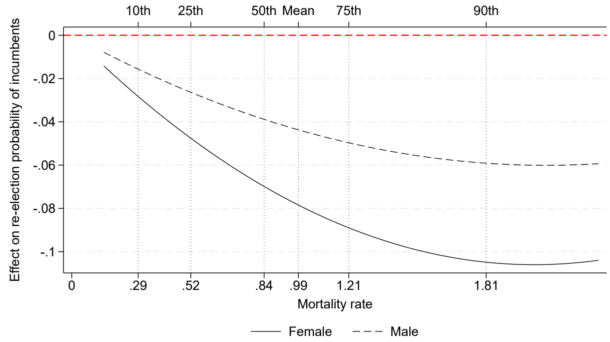
(a) Robustness I: Balanced sample



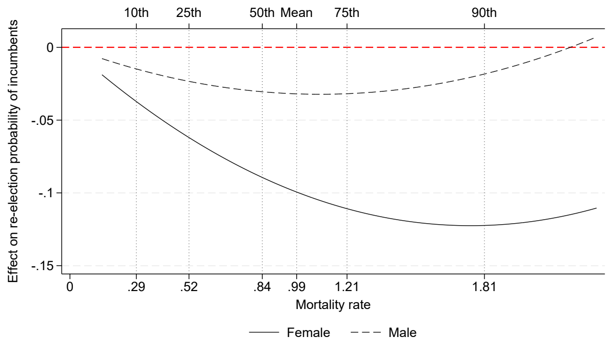
(b) Robustness I: Large cities



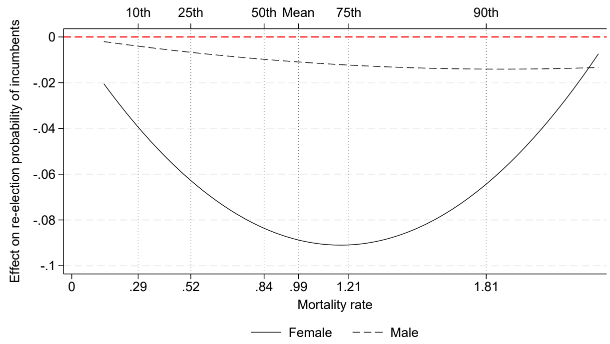
(c) Robustness I: Probit



(d) Robustness I: IHS



(e) Robustness II: Vote share



(f) Robustness IV: Excess mortality

Figure A.6: Margins plots: Robustness tests. This set of margins plots is based on the regression results from the respective robustness tests in Section 5. The dotted vertical lines refer to the respective percentiles of the mortality rate labeled on the upper x-axis. The solid curve refers to female incumbents and the dashed line refers to male incumbents. The y-axis reports effect sizes in terms of ppts of the reelection probability of incumbents.

A.6 Further figures

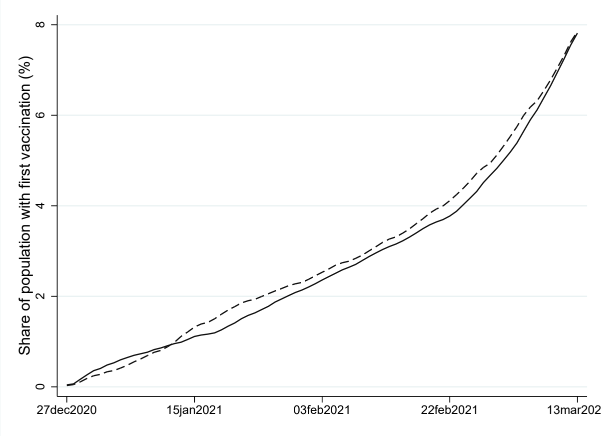


Figure A.7: Roll-out of Covid-19 vaccines in Hesse and Germany. This figure plots the share of population that had received their first vaccination until the election day. The black solid line represents the state Hesse and the grey dashed line is for Germany. Vaccine and population statistics are obtained from German Federal Statistical Office.

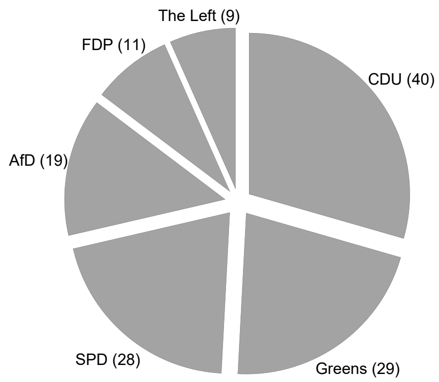


Figure A.8: Hessian State Parliament since 2018. This figure plots the distribution of seats across parties in the Hessian Parliament since 2018 based on official data from the Hessian Statistical Office.

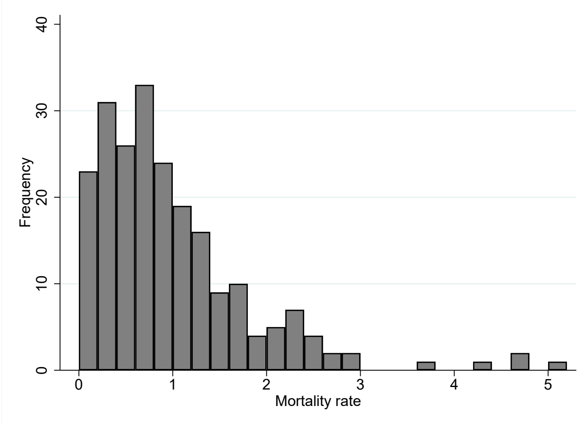
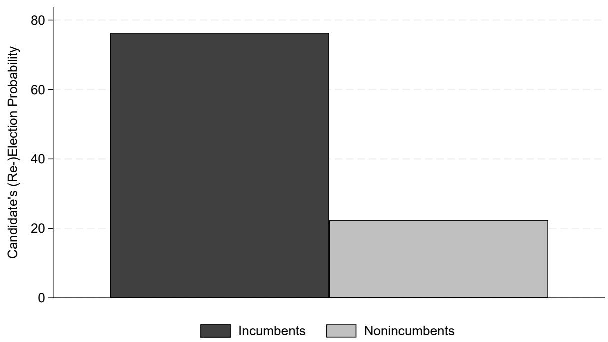
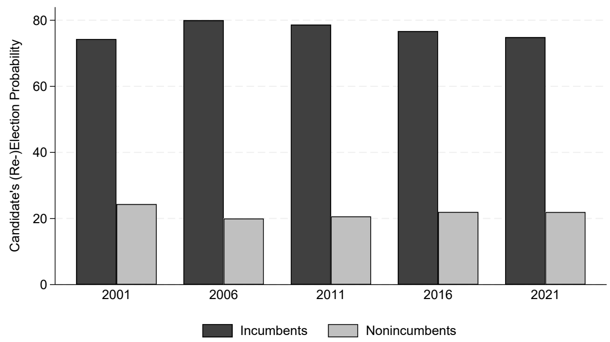


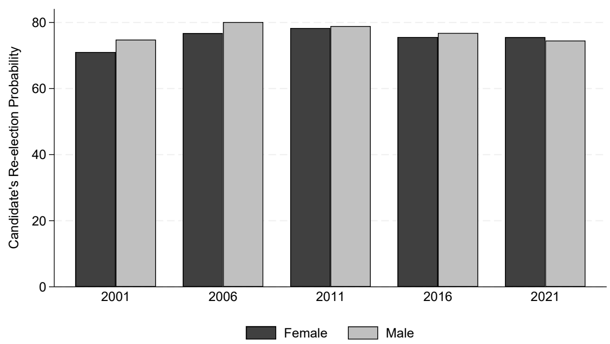
Figure A.9: Distribution of municipal Covid-19 mortality rates. This histogram shows how the Covid-19 mortality rate is distributed across the municipalities in our sample. The x-axis indicates the cumulative number of deaths per 1000 inhabitants in a municipality from the beginning of the pandemic until election day.



(a) Incumbents vs. non-incumbents



(b) Variation over time



(c) Gender gap in reelection

Figure A.10: Incumbency and gender advantages (over time). Subfigure (a) contrasts the election probability of incumbents and non-incumbents which is as a mean percentage of elected (non-)incumbents. Subfigure (b) plots this difference across different election years. Subfigure (c) provides averages for reelection rates for female and male incumbents across elections.

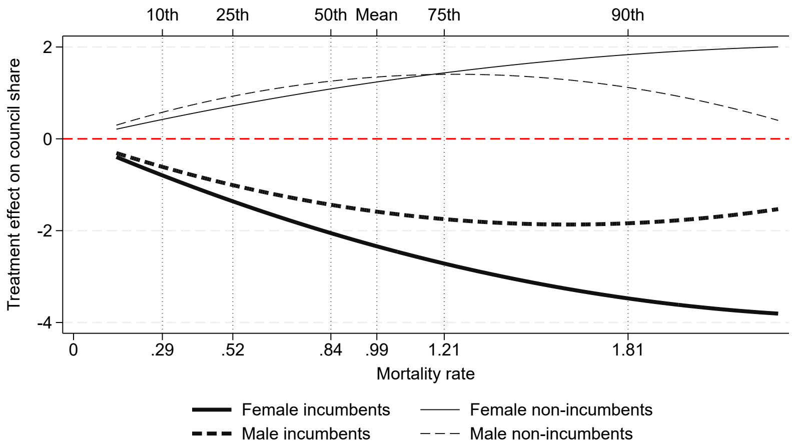


Figure A.11: Margins plot: Covid-19 mortality and council composition. This figure plots quadratic curves for the treatment effects for council shares of male/female incumbents and male/female non-incumbents in Table A.12. The dotted vertical lines refer to the respective percentiles of the mortality rate labeled on the upper x-axis.

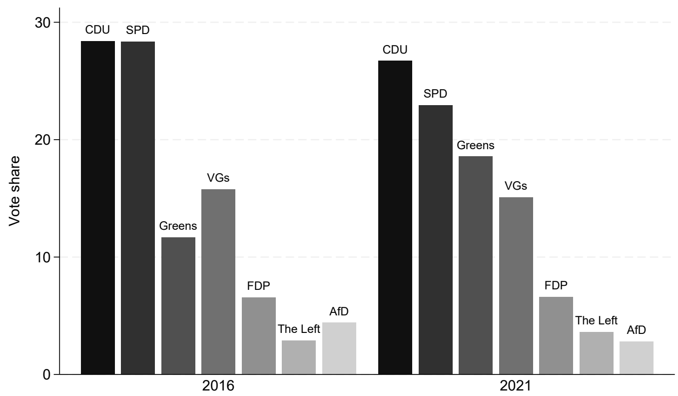


Figure A.12: Mechanism I: Party vote shares in Hessian local council elections, 2016 vs. 2021. This chart presents a side-by-side comparison of party vote percentages from the two most recent elections, with specific emphasis on the performance of the AfD. VGs denote local voters' groups (*Whlergruppen*).

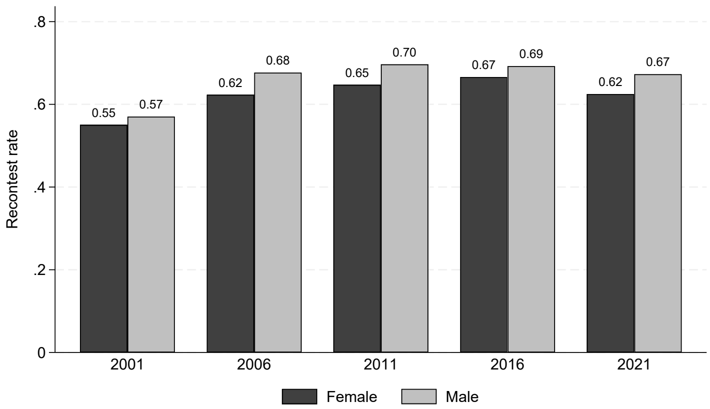
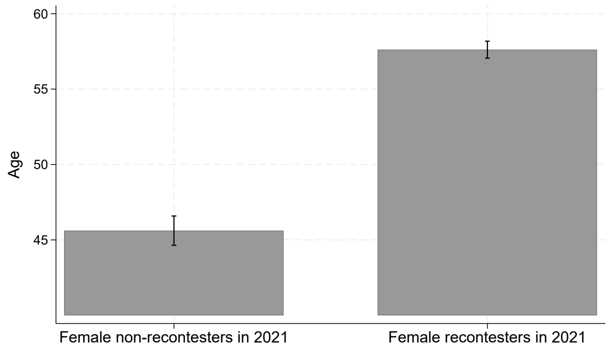
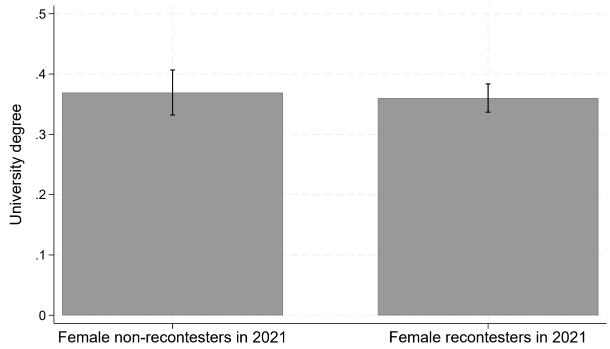


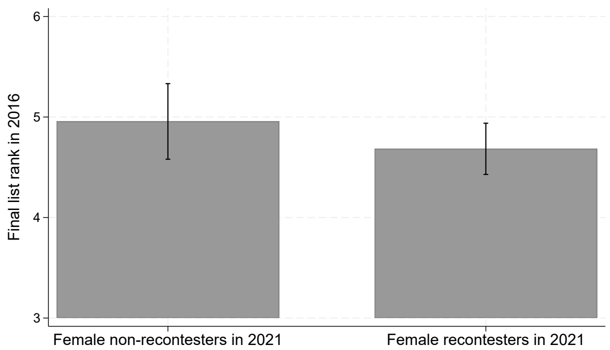
Figure A.13: Mechanism II: Gender recontest gap over time. This figure depicts the proportion of female and male incumbents elected in election t-1 who chose to seek reelection in subsequent election t over time.



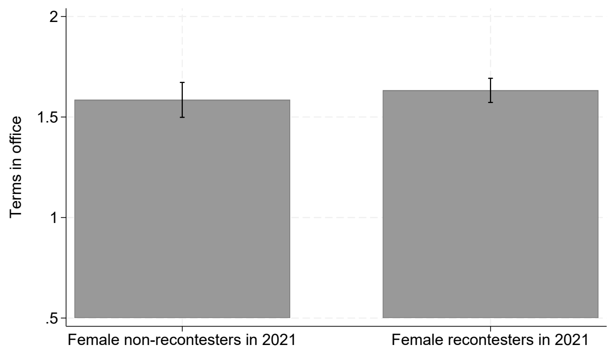
(a) Age



(b) Education



(c) Final rank in 2016



(d) Terms in office

Figure A.14: Mechanism III: Female recontester vs. non-recontester characteristics. These balance tests contrast (a) age, (b) education, and previous election performance between female incumbents who opted to rerun in 2021 and those who did not. We measure education with a dummy variable indicating tertiary education. Performance in past elections is gauged by the (c) candidate's final list rank in the 2016 election and (d) for how many terms she had been in office before 2021 (i.e. her tenure/experience). The bars report average values for these characteristics, while the vertical lines provide a 95% confidence interval.

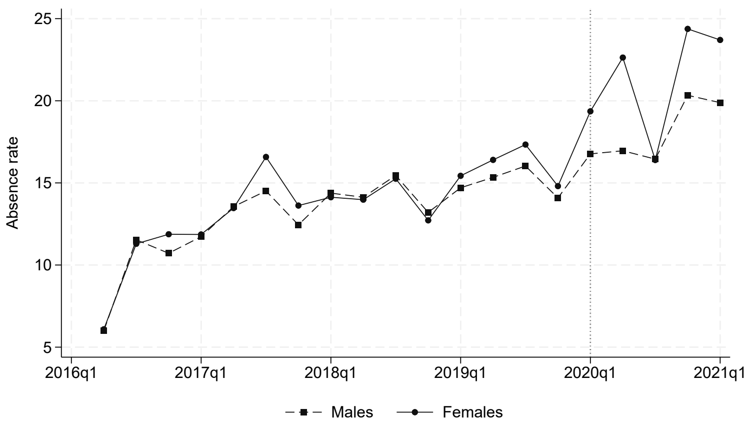
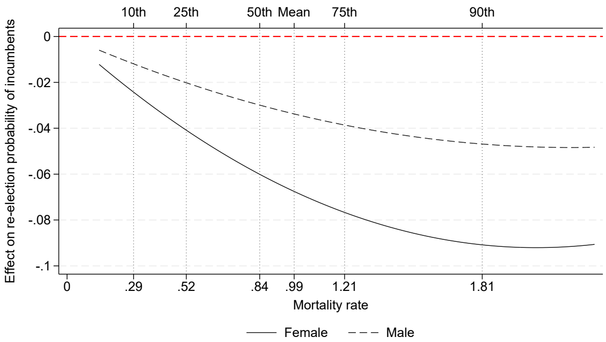
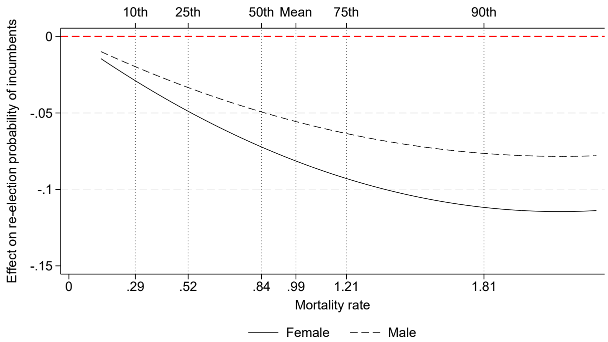


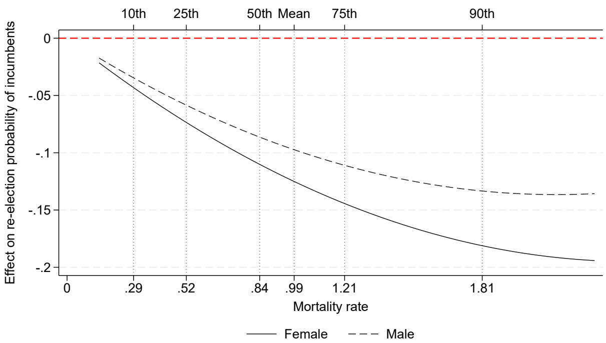
Figure A.15: Mechanism IV: Gender gaps in meeting absence rates, 2016-2020. This figure displays the quarterly average absence rates of male and female councilors throughout their term in office. The dotted vertical line represents the onset of the Covid-19 pandemic.



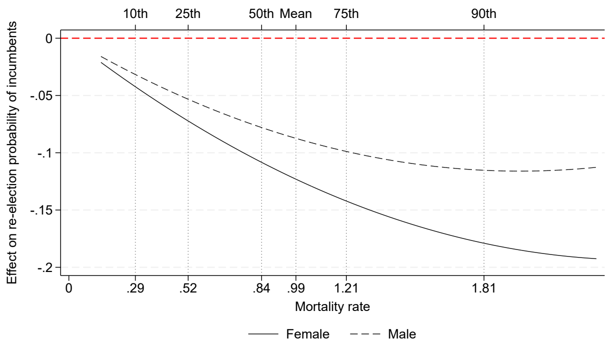
(a) Mechanism I: Party-level effects



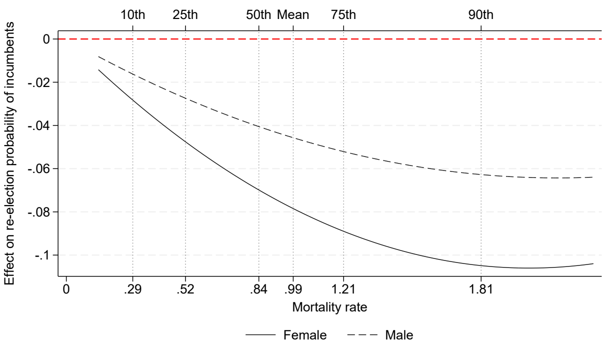
(b) Mechanism III: Gender confoundedness



(c) Mechanism IV: Without absence rate



(d) Mechanism IV: With absence rate



(e) Mechanism V: Turnout

Figure A.16: Margins plots: Mechanisms. This set of margins plots are plotted using the regression results from the mechanism tests in Section 6. The dotted vertical lines refer to the respective percentiles of the mortality rate labeled on the upper x-axis. The solid curve refers to female incumbents and the dashed line refers to male incumbents. The y-axis reports effect sizes in terms of ppts of the reelection probability of incumbents.

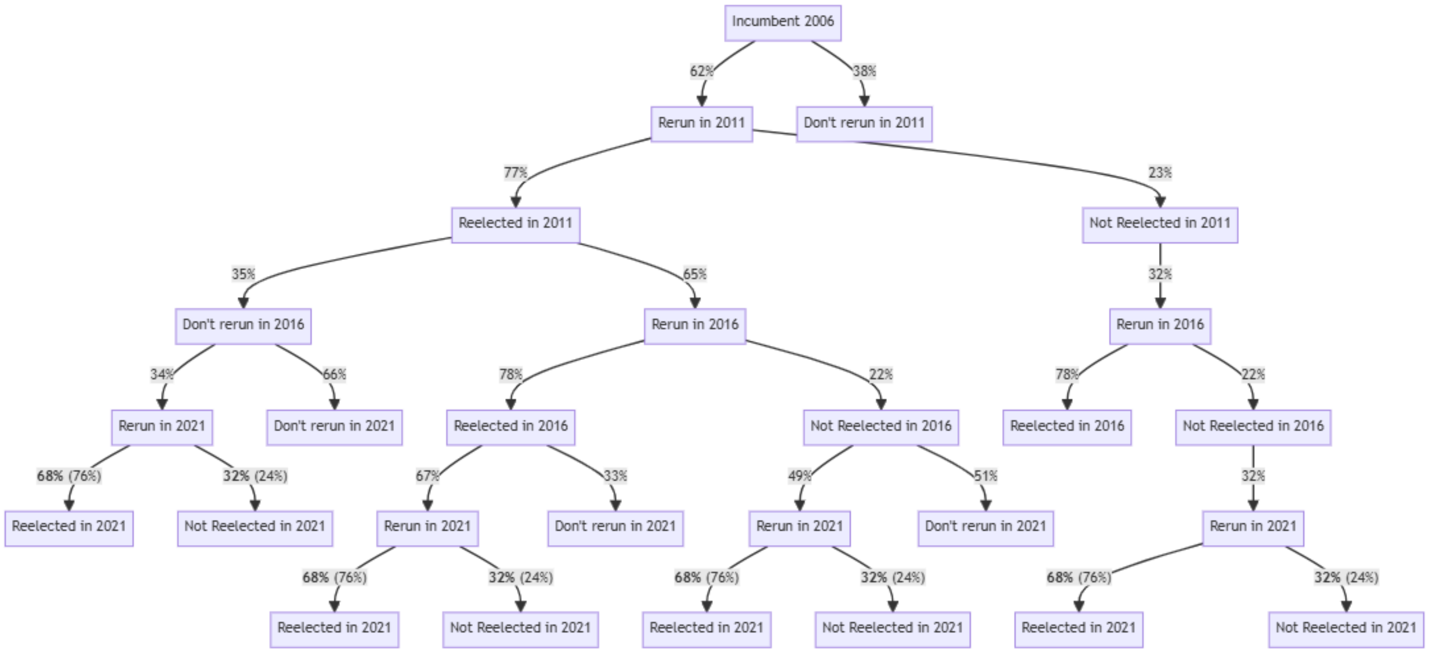


Figure A.17: Data used for simulation of long-term effects on female representation. This figure details the information used to generate projections of female representation in Hessian local councils. Presented as a decision-tree graph, it shows reelection probabilities based on whether incumbents recontest in subsequent elections. For elections before 2021, observed information on recontesters and their reelection chances is reported. The numbers without parentheses indicate actual reelection probabilities adjusted for the crisis impact. Starting from 2021, the model assumes constant probabilities of recontesting and reelection based on 2016 values. For future elections, the numbers in parentheses represent counterfactual projections, extrapolating the 2016 values. The graph concludes with the 2021 election, detailing the probabilities of incumbents from the 2006, 2011, and 2016 terms participating in the 2021 election and their chances of reelection. This decision tree is extended in the same way to generate projections for subsequent years, as presented in Figure 7.

A.7 Further tables

Table A.8: Summary statistics: in-sample vs. not in-sample counties

Variable	In sample	Not in sample	Difference	Std. Errors	Obs.
Covid mortality rate (‰)	1.05	1.15	0.099	0.119	21
Covid infection rate (‰)	31.17	28.83	-2.331	2.081	21
Number of municipalities	19.55	20.20	0.655	2.347	21
Log(Population)	12.30	12.19	-0.117	0.168	21
Population density (1k/km2)	0.34	0.28	-0.061	0.114	21
Aged 65 or above (%)	0.19	0.19	0.005	0.007	21
Aged under 14 (%)	0.15	0.15	-0.004*	0.002	21
Log(Revenues p.c.)	2.63	2.57	-0.069	0.106	21
Log(Transfers p.c.)	-1.93	-1.76	0.169	0.217	21
Log(Debt p.c.)	2.75	2.80	0.046	0.178	21
Council Size	75.09	72.10	-2.991	4.352	21
% CDU in council	35.44	35.60	0.156	2.912	21
% SPD in council	32.90	37.43	4.533	3.307	21
% Greens in council	10.56	10.02	-0.546	1.185	21
% FDP in council	5.40	4.58	-0.815	0.657	21

Notes: This table compares the characteristics of the eleven counties in our sample with the other ten counties. The data on demographic and fiscal variables as well as council size are averaged for the period 1991-2020 while the political variables use 2001-2016 averages.

Table A.9: Alternative number of treatment groups

Dep. var.: Reelected	2 Groups Median (1)	3 Groups p25-75 (2)	4 Groups Quartiles (3)	5 Groups Quintiles (4)	6 Groups (5)
Treatment=2	-0.031	-0.054**	-0.049**	-0.028	-0.036
	(0.020)	(0.023)	(0.025)	(0.026)	(0.031)
Treatment=3		-0.034	-0.064**	-0.079***	-0.059**
		(0.024)	(0.028)	(0.029)	(0.026)
Treatment=4			-0.046*	-0.050*	-0.088**
			(0.026)	(0.030)	(0.036)
Treatment=5				-0.039	-0.063**
				(0.030)	(0.031)
Treatment=6					-0.042
					(0.032)
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Municipal level controls	Yes	Yes	Yes	Yes	Yes
Observations	15098	15098	15098	15098	15098
R-squared	0.02	0.02	0.02	0.02	0.02

Notes: This table reestimates the treatment effect using different treatment groups. Model (3) reprints the baseline result (Model (4) from Table 1) for comparison. All models include municipality and year fixed effects, as well as the time-varying municipality control variables. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Coefficients are compared to the lowest treatment group (treatment=1). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Table A.10: Mechanism I: Party-level effects, determinants of initial ranks

Dep. var.: Initial list rank	(1)	(2)	(3)
Female candidate	-0.368***	-0.534***	-0.412**
	(0.136)	(0.149)	(0.169)
Incumbent candidate		-7.175***	-7.419***
		(0.275)	(0.302)
Female × Incumbent		-0.750***	-0.794***
		(0.254)	(0.302)
Female × Incumbent × Mortality rate			0.020
			(0.749)
Female × Incumbent × Mortality rate^2			0.035
			(0.188)
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Municipal level controls	Yes	Yes	Yes
Observations	74242	74208	74208
R-squared	0.20	0.25	0.25

Notes: This table estimates how candidate gender, incumbency status, and Covid-19 mortality have affected the initial list rank placements of candidates. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Table A.11: Mechanism I: Party-level effects, vote shares (instead of party FE) and initial ranks

Dep. var.: Reelected	Female			Male		
	(1)	(2)	(3)	(4)	(5)	(6)
Mortality rate	-0.077	-0.114**	-0.104**	-0.051*	-0.058*	-0.052*
	(0.054)	(0.054)	(0.048)	(0.030)	(0.032)	(0.029)
Mortality rate^2	0.018	0.028**	0.024**	0.012**	0.013**	0.012**
	(0.011)	(0.011)	(0.010)	(0.006)	(0.006)	(0.005)
CDU vote share	-0.001	-0.002	-0.001	0.002***	0.002***	0.002**
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
SPD vote share	0.005**	0.005**	0.003	0.004***	0.003***	0.003**
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
FDP vote share	0.007*	0.006	0.003	-0.003	-0.003	-0.003
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Greens vote share	0.004	0.003	0.001	0.001	0.001	-0.000
	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)
Initial list rank			-0.025***			-0.026***
			(0.002)			(0.001)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal level controls	No	Yes	Yes	No	Yes	Yes
Observations	3339	3162	3152	11128	10703	10655
R-squared	0.08	0.09	0.39	0.03	0.03	0.30

Notes: In this table, we reestimate the baseline model in Table 3 by additionally controlling for the initial list rank of candidates and party vote shares. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Cluster-robust standard errors are in parentheses. The unit of clustering is the municipality of the candidate.

Table A.12: Covid-19 mortality and council composition

	Female		Male	
Dep. var.: Council share	Incumbents (1)	Non-incumbents (2)	Incumbents (3)	Non-incumbents (4)
Mortality rate	-2.899**	1.538	-2.310	2.256
	(1.327)	(1.790)	(2.492)	(3.308)
Mortality rate^2	0.541*	-0.290	0.715	-0.905
	(0.275)	(0.401)	(0.560)	(0.725)
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Municipal level controls	Yes	Yes	Yes	Yes
Observations	582	623	605	624
R-squared	0.66	0.51	0.58	0.41

Notes: In this table, we use the council shares of incumbent (Models 1 and 3) and non-incumbent (Models 2 and 4) councilors from both genders as outcome variable and reestimate our baseline model at the municipality level. We exclude the council size and the female council share in year t-1 from the list of control variables as they had already been used in calculation the outcome variables. Stars indicate significance levels at 10%(*), 5%(**) and 1%(***). Standard errors clustered at municipality level are in parentheses.