"Critical Events as Drivers of Infectious Diseases: Synthetic Control Analyses of Elections and Natural Disasters"

Dissertation

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Halle (Saale)

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Contents

1 General Introduction

The COVID-19 pandemic, which began in 2020, led to significant disruptions worldwide, affecting public health, economies, and societies in general. Early on in the pandemic, estimates underscored not only its immediate health risks but also its far-reaching economic and social repercussions. For example, Bethune and Korinek (2020) estimated as early as April 2020 that the cost of an additional COVID-19 infection in the U.S. at that time was approximately $80,000, with total social costs (including externalities) exceeding $286,000 per case. Similarly, in July 2020, Dobson et al. (2020) projected that the global cost of the pandemic would likely fall between $8.1 trillion and $15.8 trillion. These estimates continued to rise in the following months. By October 2020, after the first global wave, Cutler and Summers (2020) estimated the total cost of COVID-19 for the United States alone at $16 trillion, with roughly half attributed to income losses from the recession and the other half to reduced life expectancy and declining health. Fueled by the scale of these estimates, and building on prior research — such as the work of economists[1], the pandemic expanded interest beyond epidemiology, prompting investigations into potential factors facilitating viral spread across the (social) sciences.

From the outset of the pandemic, researchers have particularly examined how events and social gatherings contribute to the spread of infectious airborne diseases. For the purpose of this thesis, the research in this context is categorized into two groups based on the necessity of the events: non-critical events and critical events. The first category includes events and social gatherings that are not essential for the fundamental functioning of a (democratic) society. For example, Ahammer et al. (2023) investigate the impact of NBA and NHL games on COVID-19 spread in the U.S., finding that each additional game led to a 10.3% increase in cumulative COVID-19 deaths in affected urban areas, with a total increase of 36% in counties hosting multiple games. Their results indicate that banning indoor sport events is an effective non-pharmaceutical intervention to curb virus transmission. Similarly, Mangrum and Niekamp (2022) explore how university spring break timing influenced COVID-19 transmission, showing that counties with more early spring break students experienced higher case growth rates, peaking two weeks after spring break.

[1] For instance, Adda (2016) examines the unintended consequences of economic activity on the spread of viral infections, the effectiveness of measures limiting interpersonal contact, and the optimal allocation of resources to control disease transmission using high-frequency data from France over 25 years.

Their findings indicate that (mass) gatherings of students, particularly in locations that, at the time exhibited high COVID-19 spread dynamics played a significant role in both primary and secondary transmission. Additionally, those gatherings increased mortality rates in the communities surrounding university campuses. Furthermore, Whaley et al. (2021) examine the role of informal social gatherings in COVID-19 transmission by analyzing whether household COVID-19 infections increased after a birthday celebration. Their study of 2.9 million U.S. households finds that in counties with high COVID-19 prevalence, households with a birthday had a 31% higher infection rate, with child birthdays associated with a greater increase in cases than adult birthdays, highlighting the role of small gatherings in virus spread.

Although this strain of the literature explores a variety of specific questions[2], its overarching aim is to examine how non-critical events contribute to public health risks through the spread of the respiratory viruses in order to evaluate whether canceling or postponing such events during periods of heightened viral transmission is justified.[3]

The second category comprises critical events that are difficult to cancel or postpone due to their significant public interest. One such set of events that hold fundamental importance in democratic societies are elections. Canceling of elections violates each individuals human right to participate in her country's government according to Article 21(3) of the *Universal Declaration of Human Rights* (see United Nations, 1948). Additionally, while elections were postponed during the COVID-19 pandemic (Lee, 2024), democratic societies have strong incentives to maintain the originally scheduled election dates or, if postponed, to minimize delays. Beyond practical concerns, such as country-specific legal requirements for election timing (Ellena, 2020) or the increasing challenges associated with implementing postponed elections[4], there is also the risk of alienating voters. Postponing an election can be perceived by both the political opposition and the

[2]For example, Mangrum and Niekamp (2022) additionally highlight the role of universities in making decisions that affect the health of their surrounding communities.

[3]For instance, research in this field led the German *Robert Koch-Institute* to recommend that policymakers closely assess the necessity of non-critical events in the context of the COVID-19 pandemic (RKI, 2020b). Moreover, the *World Health Organization* (WHO) advocated for a risk-based approach based on that research, emphasizing precautionary measures such as physical distancing, mask-wearing, and proper ventilation while urging decision-makers to continuously reassess and adapt mitigation strategies based on evolving epidemiological conditions (WHO, 2021).

[4]For instance, the postponement of the English local elections in 2020 led to their concurrent scheduling with a broad array of other elections in 2021, including those for the Scottish and Welsh parliaments, English councils, police and crime commissioners, the London mayor and assembly, as well as regional and local mayors. The diversity in legal frameworks and electoral systems governing these elections raised concerns about potential voter confusion and administrative complexities (James & Alihodzic, 2020).

public as an attempt by the incumbent government to extend its tenure (Egmont Institute, 2020; Zamfir & Fardel, 2020). From a different perspective, Bol et al. (2021) show that holding elections during a pandemic may benefit the reigning government. They analyze data from a representative web-based survey conducted in Western Europe during March and April 2020, comparing political support for incumbents among respondents surveyed just before and after the start of lockdowns. Their findings indicate that lockdowns increased voter intentions to support the party of the Prime Minister or President and boosted trust in their government. Similarly, Frank et al. (2020) examine the German municipal elections in the state of Bavaria in March 2020 and show that declaring a state of emergency between the first and second round resulted in a 10-percentage-point increase in voter turnout compared to previous elections. Additionally, Leininger and Schaub (2023) investigate the causal effect of COVID-19 cases on electoral outcomes for the same election across Bavarian districts, demonstrating that the pandemic consistently favors the dominant regional party, the center-right Christian Social Union (CSU), and its candidates.

In addition to elections, natural disasters represent another type of critical event, as they lead to essential gatherings that typically cannot be prohibited or postponed, because these gatherings are directly tied to providing assistance to those in urgent need of help. They involve sheltering displaced residents and aid workers assisting in affected regions, along with collaborative efforts within and between both groups to reinstate livable conditions.

Another key reason for closely examining these events is that, unlike non-critical events, which are primarily assessed based on whether they should be canceled or postponed, pivotal events carry distinct policy implications. Given the reasons outlined above, justified countermeasures are unlikely to include postponing or canceling these events and their associated gatherings. Instead, they may prioritize alternative strategies, such as distributing face masks or implementing targeted monitoring of disease transmission.

Furthermore, a significant link between such events and the spread of respiratory diseases could lead to unintended and undesirable behavioral changes. In the context of elections, these increases risks might discourage vulnerable populations from exercising their fundamental right to vote due to health concerns. Similarly, in the aftermath of natural disasters, the increased risk of infection could reduce the willingness of aid workers to help, thereby hampering critical relief efforts. Anticipating these potential risks enables societies to develop effective strategies in advance, ensuring both a better public health protection and the continuity of essential social functions.

While non-critical events have been studied to some extend, much less is known about how critical events impact respiratory health and as for the literature of non-critical events, a majority of the literature for elections so far is conducted in the context of the COVID-19 pandemic with mixed results. Bernheim et al. (2020) find that Trump campaign rallies held between June and September 2020 led to over 250 additional COVID-19 cases per 100,000 residents. Mello and Moscelli (2022) show that in-person voting during the Italian regional elections in September 2020 contributed to the spread of COVID-19, with each additional percentage point of voter turnout leading to a 1.1% increase in new infections. Similarly, Cipullo and Le Moglie (2022) find that electoral campaigns preceding this election in Italy significantly worsened the public health situation, causing a 7% rise in new infections, a 15% increase in the percentage of positive tests, a 24% rise in hospitalizations, a 5.3% increase in ICU admissions, and a 0.6% increase in deaths. Furthermore, the evidence from the March 2020 elections in France is also mixed: Bach et al. (2021) and Zeitoun et al. (2020) find no impact on excess mortality or case numbers, while Bertoli et al. (2020) and Cassan and Sangnier (2022) link higher voter turnout to increased mortality among the elderly and hospitalizations in high-case areas. Conflicting results also emerge for the Wisconsin primary in April 2020. Cotti et al. (2021) associate in-person voting with a 17.7% rise in the positive test rate, while Berry et al. (2020) find no effect. For the Czech Republic, Palguta et al. (2022) find that Senate elections in October 2020 accelerated infection rates and hospitalizations, particularly among younger individuals.

Aside from the literature on elections as one type of critical event, most studies on natural disasters and the spread of respiratory diseases primarily employ descriptive analyses, yielding mixed results. Murray et al. (2009) and Rath et al. (2011) observe increased respiratory symptoms after Hurricane Katrina in 2005. Mavroulis et al. (2021) report a rise in COVID-19 cases only after Cyclone Ianos in Greece, while other disasters they examine show no effect. Similarly, Frausto-Martínez et al. (2020) find no clear surge in COVID-19 cases during tropical storms. Likewise, Čivljak et al. (2020) and Ćurković et al. (2021) observe no immediate increase in cases following earthquakes in Croatia in

March 2020.

Thus, this thesis contributes to the literature on the impact of critical events on the spread of respiratory diseases in four distinct ways. First, it examines the municipal election in the German state of Bavaria, which took place early in the pandemic in Germany when knowledge of virus transmission and prevention was still limited. Unlike later elections and campaigns, where precautionary measures were widely adopted, this case offers valuable insights into how elections may influence the spread of respiratory infections in the absence of sufficient countermeasures. Second, it broadens the scope beyond COVID-19 by analyzing other respiratory tract infections, thereby exploring the potential link between elections and the spread of airborne diseases. Third, it investigates the relationship between elections and infectious diseases in periods less severe than a pandemic, providing further insights into the potential health risks associated with elections under milder baseline conditions. Finally, the thesis aims to deepen the understanding of how natural disasters influence the spread of airborne diseases. Using a causal approach, it monitors the weekly progression of the COVID-19 pandemic around the onset of Storm Bernd on July 14 and the subsequent flooding in Western Germany. This quasi-experimental setting may enable a causal analysis of the post-disaster phases that contribute to increased respiratory disease transmission.

All analyses presented in the thesis are based on data on either the district or the federal-level from Germany, which was selected as the primary data focus for three main reasons: First, infectious disease surveillance, particularly during the COVID-19 pandemic, is highly active, largely due to the strong efforts of the Robert Koch-Institute (RKI) in close cooperation with the regional health authorities. Second, Germany's mandatory health insurance system covers a significant portion of the population, allowing for an analysis of both infection rates and sick leaves (see second study), which also enables rough cost estimations related to increased absences from work. Third, beyond infectious disease data, several providers offer extensive datasets covering economic and sociodemographic characteristics for Germany that serve as valuable sources for control variables.

With this data established, the thesis incorporates a methodological framework designed to isolate the causal effects of critical events on respiratory health outcomes. It primarily employs the Synthetic Control Method (SCM). Since its first application by

Abadie and Gardeazabal (2003), SCM has become a widely recognized method in empirical economics and beyond. In their study on the state of the econometric policy evaluation, Susan Athey and Guido Imbens describe synthetic controls as "[...] the most important innovation in the policy evaluation literature in the last 15 years" (Athey & Imbens, 2017, p. 9). SCM builds on a difference-in-differences framework but, instead of relying on a single control unit or a simple average of multiple units, it constructs a weighted combination of control units to create a synthetic counterpart that more closely resembles the treated unit's outcome trajectory. Its straight forward to interpret output, combined with its transparency in revealing both unit and predictor weights, has contributed to its widespread application across diverse empirical research fields.[5] While individual-level data is crucial in epidemiology for studying the spread of infectious diseases — such as measuring incubation periods (see Lauer et al. (2020) for COVID-19 and Jhung et al. (2013) for Influenza) — it is often inaccessible due to, for instance, data protection regulations. In contrast, aggregated data is often more readily available, for which SCM is a well-established method (Abadie, 2021). As a result, SCM has gained traction in epidemiological research. For example, Bruhn et al. (2017) apply it to assess the impact of pneumococcal conjugate vaccines on pneumonia-related hospitalizations, while Sun et al. (2024) use it to evaluate the effect of gastric cancer screening programs on age-standardized mortality and other upper gastrointestinal diseases.[6] Overall, SCM serves as a valuable tool when more granular data is unavailable and pure randomization is not applicable.

This thesis investigates the relationship between critical events and respiratory diseases through three essays and is structured as follows: Chapter 2 presents the first study, which examines whether the regional elections in the German federal state of Bavaria on March 15, 2020, contributed to the spread of COVID-19. The results indicate that over a third of the increase in positive cases cannot be explained by demographic, economic, health, or tourism-related factors, and that districts with higher voter participation experienced a steeper rise in cases and deaths. The results suggest that the timing of the elections played a significant role in spreading the SARS-CoV-2 virus. While these findings are intriguing in their own right for reassessing early responses to the COVID-19

[5] For an overview of the various research areas utilizing SCM, see Abadie (2021, Chapter 2).

[6] Other applications of SCM by epidemiologists can be found in Casey et al. (2023), Nianogo et al. (2024), and Prunas et al. (2022).

pandemic, they also raise questions for research with relevance beyond crisis periods. Bavaria, along with other German states, has long held elections in March or even earlier in the year, coinciding with Influenza seasons. Thus, the second essay presented in Chapter 3 investigates whether regional elections held outside of pandemic periods contribute to the spread of respiratory infections. Using sick leave data from the German health insurance provider Barmer, it analyzes elections in Bavaria, Hesse, and Thuringia. While no transmission effects are found for the 2018 Bavarian election, the 2018 Hessian and 2019 Thuringian elections show a significant impact on respiratory sick leaves. The third essay in Chapter 4 examines whether the severe flood in Germany in July 2021 contributed to the spread of COVID-19. It compares COVID-19 case numbers and ICU admissions in flood-affected districts with unaffected counterparts, finding a positive divergence in cases but less conclusive effects on intensive care admissions. Finally, Chapter 5 discusses overarching conclusions, offers directions for future research and provides policy implications.

The toll of voting in a pandemic? Regional elections and the spread of COVID-19 in Bavaria

Jochen Güntner Gerrit Stahn Amelie Wuppermann Felix Zwies

Abstract

This study investigates whether the regional elections held in Bavaria on March 15 of 2020 — shortly after the WHO declared COVID-19 a global pandemic — contributed to the spread of COVID-19 cases and COVID-related deaths in this German state. Constructing synthetic controls for each of Bavaria's 96 districts based on non-Bavarian German districts, we find that over a third of the increase in positive test results cannot be explained by district-level demographic, economic, health or child care characteristics, nor by the distance to Ischgl — a proxy for skiing tourism associated with the first COVID-19 wave in Germany. Within Bavaria, districts with higher voter participation witnessed a steeper increase in COVID-19 cases and deaths after the election, while controlling for alternative drivers, such as the distance to Ischgl and the number of strong-beer festivals. Our results are highly robust and suggest that an unfortunate timing of elections contributed to the spreading of an infectious disease.

Keywords: COVID-19, municipal elections, pandemic, synthetic control method

JEL classification: H11, H12, I12, I18

2.1 Introduction

Elections are the backbone of democracy. When they take place in precarious environments, however, voting may come along with nontrivial risks, which can lead to a trade-off between the right to vote and the right to maintain a healthy life. Some of those health risks became salient during the COVID-19 pandemic. As a consequence, many countries deferred national and subnational elections or switched to postal ballots.[1] The German state of Bavaria instead called close to 10 million voters to cast their vote in the regional elections on March 15 of 2020, when only a few precautionary measures were already in place. In calendar week 13 — two weeks after the election indicated by the vertical dashed line in Figure 1 — Bavaria left behind all other German states in terms of COVID-19 cases per 100,000 residents reported and was one of the federal states with the highest number of deaths per 100,000 residents.[2]

In this study, we investigate the effects of the regional election on the spread of COVID-19 cases and deaths using two econometric approaches. First, we use synthetic controls matched on a host of district-level demographic, economic, health care and child care characteristics as well as the distance to Ischgl to proxy for skiing tourism, which was identified as an early driver of COVID-19 in Germany (Felbermayr et al., 2021), and find that Bavarian districts suffered an unexpectedly large increase in COVID-19 cases and deaths after March 15. To further isolate the effects of the elections, we then regress the post-election differences in cases and deaths on voter participation as a measure of the "treatment intensity" of the elections across Bavaria's 96 districts, while again controlling for demographic, economic, health care and child care characteristics as well as the distance to Ischgl and the number of confirmed strong-beer festivals held in March in several districts. In the most conservative specification, which includes administrative and structural district-type dummy variables, a 1 p.p. increase in voter participation across Bavarian districts is associated with an additional 12.8 positive test results and 1.8 deaths per 100,000 inhabitants. Sensitivity analyses show that the results of both approaches are highly robust.

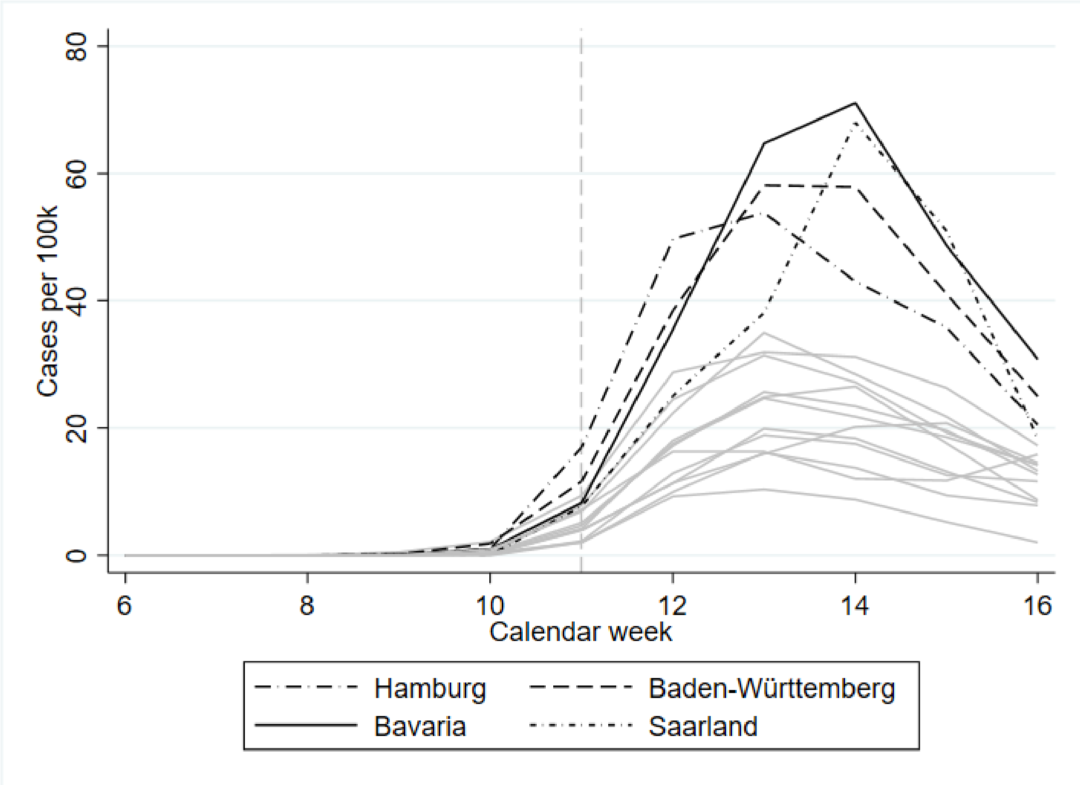
Since the start of the pandemic, a host of contributions has investigated the relation-

[1] The International Institute for Democratic and Electoral Assistance (IDEA) provides a global overview of the impact of COVID-19 on national and subnational elections.

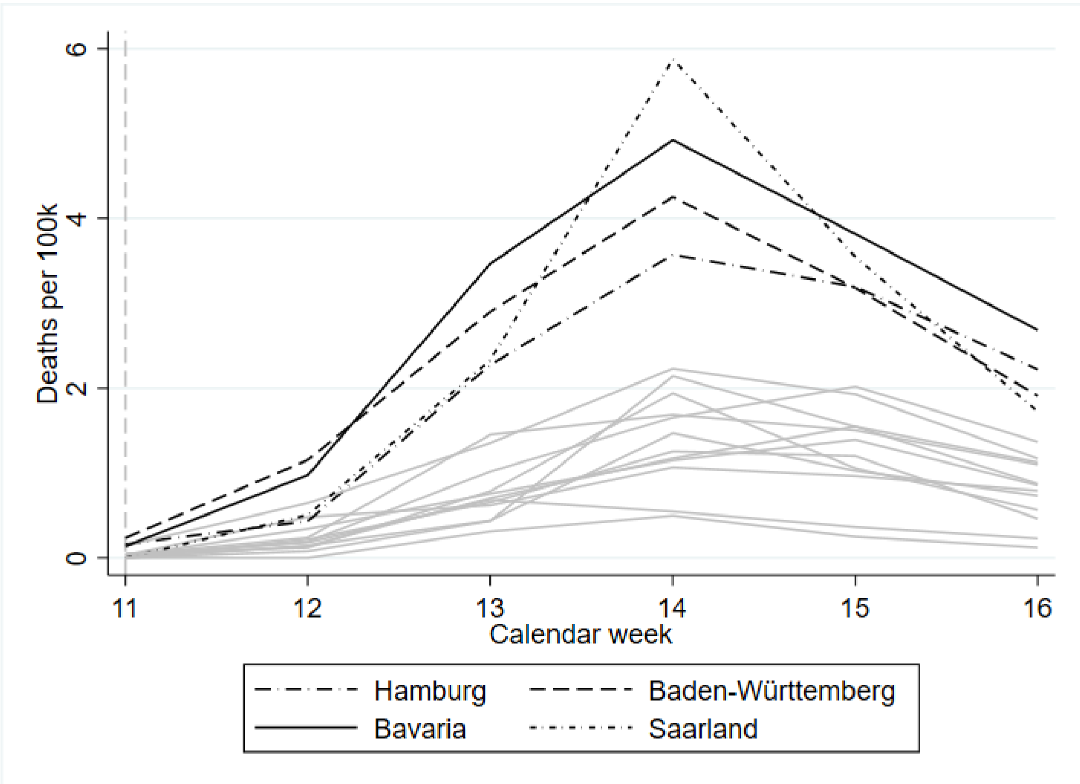
[2] Cases are defined according to the Robert Koch-Institute. In this paper we use the terms *infections*, *cases* and *tested positive* as synonyms.

Figure 1: Weekly COVID-19 cases and deaths by German state

(a) Reported cases per week



(b) Reported deaths per week



Notes: Figure 1 displays weekly COVID-19 cases (panel a) and deaths (panel b) per 100,000 inhabitants across the 16 German federal states. The development in Hamburg, Baden-Württemberg, Bavaria, and Saarland are highlighted as they had the highest reported numbers at that time. The dashed vertical line marks calendar week 11, when the Bavarian election occurred.

ship between social factors, political measures, and the spread of COVID-19 cases and deaths (see, e.g., Ahammer et al., 2023; Allcott et al., 2020; Andersen, 2020; Mangrum & Niekamp, 2022; Whaley et al., 2021). Focusing on German districts, Alipour et al. (2023, 2021) and Felbermayr et al. (2021) analyze the effect of working from home and the proximity to the Austrian ski resort Ischgl, respectively, while Mitze et al. (2020) study the containment effect of wearing face masks in public.

There is also growing but mixed empirical evidence on the health risks of holding elections in a pandemic. Bernheim et al. (2020) investigate the effect of eighteen Trump campaign rallies (held between June 20 and September 22 of 2020) on the spread of COVID-19. By applying the synthetic control method, they report an average treatment effect across the events suggesting an increase of more than 250 confirmed COVID-19 cases per 100,000 residents. Similarly, Cipullo and Le Moglie (2022) show that electoral campaigns preceding the regional elections in Italy in September 2020 significantly worsened the spread of COVID-19, finding a 7% increase in new infections, a 15% increase in the percentage of positive tests, a 24% increase in hospitalizations, a 5.3% increase in intensive care unit (ICU) admissions, and a 0.6% increase in deaths. These findings align with evidence on in-person voting for the same election, as Mello and Moscelli (2022) demonstrate that each additional percentage point of voter turnout led to a 1.1% rise in new infections.

On the same day as the elections in Bavaria, on March 15 of 2020, regional elections also took place in France. Bach et al. (2021) find no effect of this election on the excess mortality of 163,000 male candidates aged above 60 relative to the general population, regardless of the intensity of the election race and how candidates fared in the 2014 elections. Similarly, Zeitoun et al. (2020) find no effect on COVID cases. However, Bertoli et al. (2020) find that higher voter turnout did increase the mortality risks of the generation 80+ and Cassan and Sangnier (2022) report that higher voter turnout is related to increases in hospitalizations in departments, in which there were already relatively many cases at the time of the election.

Similarly, conflicting results exist for the Wisconsin primary that took place on April 7 of 2020. Cotti et al. (2021) find a statistically significant relationship between in-person voting and the spread of COVID-19 two to three weeks after the Wisconsin primary, where a 10% difference in average in-person voters per polling location is associated with a 17.7% increase in the positive test rate across counties, suggesting that the primary was related to about 700 additional cases in Wisconsin. On the contrary, Berry et al. (2020) find no effect of the Wisconsin primary on COVID-19 cases. An analysis in the Economist links higher in-person voting in the U.S. federal election on November 3 of 2020 to an increase in COVID-19 cases (The Economist, 2021).

Palguta et al. (2022) leverage a natural experiment for the Czech Republic, where one-third of Senate constituencies hold elections every two years, to estimate how the Senate elections in October 2020 influenced COVID-19 infection rates. They find that voting constituencies experienced significantly faster growth in COVID-19 infections compared to non-voting constituencies in the weeks following the elections as well as an increase in hospital admissions, indicating that the acceleration of infections was not only due to stricter testing regimens. The effects are most pronounced among individuals below 65, possibly due to the strategic absenteeism of senior voters.

We add to the literature by examining an election that took place at the start of the COVID-19 pandemic, aligning with studies on the election in France. Unlike the elections held later in the pandemic, where precautionary measures were already in place, the Bavarian (as well as the French) election took place, when knowledge of the virus transmission and prevention methods was still limited. At the time of the Bavarian election, mask-wearing was even discouraged in Germany to avoid depleting supplies needed for hospitals and medical staff (Bayerische Staatszeitung, 2020; Tagesschau, 2020). Analyzing an election held at the onset of the pandemic yields insights into how the virus spreads in the absence of suitable precautionary measures.

While most related studies primarily examine in-person voting, we analyze the effect of overall voter turnout for two reasons. First, obtaining ballot papers before the election is a necessary condition for voting by mail. However, the number of voters who receive ballot papers does not perfectly indicate the proportion of those who vote in-person versus by mail, as some voters may still choose to vote in person despite having obtained their ballot papers in advance.[3] Second, while in-person voting may contribute to the spread of COVID-19 through direct interactions among voters at polling stations, overall turnout could also play a role in transmission. A higher turnout necessitates more election workers to process and count the votes. Additionally, increased voter participation may reflect

[3]Nonetheless, we attempt to disentangle the effects of different types of voter turnout in some of our specifications.

heightened interest in the election, which could be linked to more intensive campaigning in the lead-up to the vote.

Our results strongly and robustly indicate that the Bavarian regional elections contributed to the spread of COVID-19 in Bavaria. These results suggest more generally that the timing of elections within the year, which often take place in spring or fall, may foster the spread of other seasonal infectious diseases, such as Influenza. We leave this question for future research.

The rest of the paper is structured as follows. Section 2.2 gives information on the Bavarian district elections and the timing of events. Sections 2.3 and 2.4 describe the data and empirical approaches, respectively. Section 2.5 presents the results and section 2.6 concludes.

2.2 Background

Starting in 1946, after the end of the Second World War, Bavaria held municipal elections in intervals of two to four years. In 1960, the election period was extended to six years and has not been modified since. The last seven municipal elections took place in March of an election year, indicating that the polls on March 15 did not deviate from the regular schedule. In contrast to prior polls, however, the district elections in 2020 took place "at the dawn of a global pandemic" (Leininger & Schaub, 2023). The first known German case of COVID-19 occurred in the Bavarian district of Starnberg, where a 33-year-old male employee of the automotive supplier Webasto was infected by a mildly symptomatic Chinese colleague, who was tested positive after returning to China. Subsequently, 13 colleagues at Webasto or their relatives were tested positive. Unrelated to the Webasto outbreak, a German women was infected, while staying at the Dortmunder Hütte, an alpine cottage in Tirol, Austria, during January 24–26. In both cases, the infected individuals were isolated and the outbreaks seemed to be under control. By March 1, the cumulated number of proven COVID-19 cases in Bavaria had increased to a mere 25, and the potential health risks were widely considered as minor.[4]

In predominantly Catholic Bavaria, the period of Lent (between Ash Wednesday and Holy Saturday) is also a high season for the state's famous strong-beer festivals. Several

[4]Bavaria's public broadcasting service BR interviewed "patient zero" after quarantine on February 28, who said that "Although it is a new virus, it is not as bad as the flu.".

such events took place in early March, mainly in the administrative regions of Oberbayern, Niederbayern, and Oberpfalz, while others were canceled due to increasing COVID-19 concerns. Two large and now infamous festivals took place in the districts Tirschenreuth and Rosenheim, with 1,400 and 1,500 visitors, respectively.[5] Another 1,500 visitors attended a festival in Straubing on March 7. It did not go unnoticed by the media that these and their neighboring districts were also most strongly affected by COVID-19 cases and deaths afterwards (Lill, 2020). Given that an earlier outbreak in the North Rhine-Westphalian district of Heinsberg had been traced back unambiguously to an indoor event with a mere 300 visitors on February 15, the health risks of mass gatherings were already known by the beginning of March.[6]

First signs of rising COVID-19 cases induced the Bavarian government to send an email to district and community offices on March 4, urging election workers to “adhere to standard practices for protection against infectious diseases such as hand hygiene, keeping physical distance as well as cough and sneeze hygiene” (Bayerisches Staatsministerium des Innern, für Sport und Integration, 2020b, p. 2). A second email on March 11 pointed out the procedures for recruiting election workers and the possibility of consolidating polling locations in the event of excess absenteeism on short notice (Bayerisches Staatsministerium des Innern, für Sport und Integration, 2020a, pp. 2–3). While the first email leaves the provision of disinfectants at the discretion of the local health authorities, facial masks or other protective gear are not mentioned in either email.

On March 11, the World Health Organization (WHO) publicly assessed that “COVID-19 can be characterized as a pandemic” (WHO, 2020). At the same time, an exceptionally close race for mayor’s offices in many Bavarian city and town halls spurred voters’ interest ahead of the elections.[7] Rather than shying away from the polls on March 15, voter participation in the elections increased for the first time since 1990, from 54.7% in 2014

[5]The festival in Mitterteich (Tirschenreuth) took place on Saturday, March 7. The festival in Rosenheim was discontinued after three days on March 9. Table A.1 in the appendix lists Bavarian districts, where at least one strong-beer festival took place in early March, as well as the estimated number of visitors.

[6]Through April 2020, eight of the ten districts with the highest number of COVID-10 cases per 100,000 inhabitants are located in Bavaria. The two remaining districts are Hohenlohekreis (Baden-Württemberg) and Heinsberg (North Rhine-Westphalia). All eight of those districts had either hosted a strong-beer festival or are directly adjacent to one that did. (Source: Own calculations)

[7]According to a survey published by BR on March 14, 2020, 79% of survey participants displayed “strong or very strong interest” in the elections, an increase of 9 percentage points relative to the elections in 2014 (Bayerischer Rundfunk, 2020). Indeed, 16 out of 24 races for city halls, among them the five most populous Bavarian cities, and 46% of the races for town halls were only decided in a run-off ballot on March 29, 2020.

to 58.8% in 2020.

One might argue that neither the strong-beer festivals nor the district elections seemed particularly risky at the time. While the first COVID-19 victim in Bavaria, an 80-year-old resident of a nursing home in Würzburg, Unterfranken, died on March 12 (Bayerisches Staatsministerium für Gesundheit und Pflege, 2020a), deaths in Rosenheim, Straubing, and Tirschenreuth did not start to cluster until after the elections.[8] The day after the voting, however, public life in Bavaria was restricted immediately. On March 16, the Bavarian government declared a state-wide emergency, which eventually lasted for three months until June 16, prohibiting public gatherings and events and closing all non-essential shops and amenities (Bayerische Staatsregierung, 2020). Two days later, Mitterteich was subjected to the first German curfew under the Infection Prevention Law, anticipating a state-wide lockdown, which came into effect on March 21 and was initially foreseen to last for only two weeks until April 3 (Bayerisches Staatsministerium für Gesundheit und Pflege, 2020b).[9]

2.3 Data

The analyses presented in this paper are based on district-level (Kreis) data from Germany. In 2020 the Federal Republic of Germany comprised 16 states and 401 districts, of which 294 were rural and 107 were urban districts. The Free State of Bavaria comprised 96 districts, of which 71 were rural and 25 were urban districts, including the state capital Munich.

We use official data on registered COVID-19 cases and deaths at the district level for January 28, 2020 through August 20, 2024 from the SARS-CoV2-Github page of the Robert Koch-Institute (RKI).[10] For each positive test result reported to the RKI, the data contain information on the state, district, age group, sex, reporting date, date of the first symptoms and whether the person tested has recovered or died in the meantime.

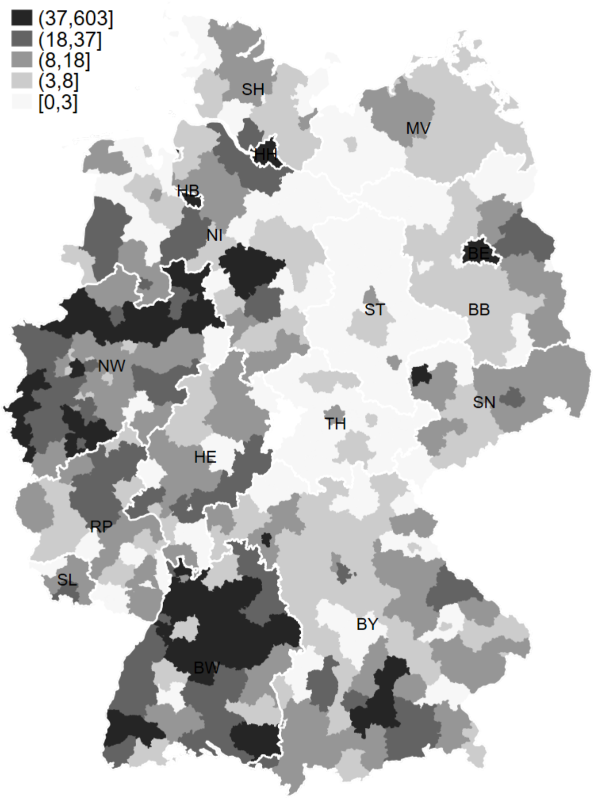
[8] By March 15, the Robert Koch-Institute had documented zero COVID-19-related deaths in Rosenheim (urban and rural district) and Straubing, 2 in Tirschenreuth, and a maximum of 7 in Würzburg.

[9] The state-wide lockdown restricted leaving home to absolute necessities such as going to work, shopping groceries, visiting pharmacies, doctors and partners as well as elderly, sick or people in need outside of hospitals and nursing homes. The general decree also granted the right to spend time outside for the sake of physical exercise, albeit only with pets or people of the same household. Violations could be sanctioned according to the Infection Prevention Law up to a maximum fine of €25,000 (Stroh, 2020).

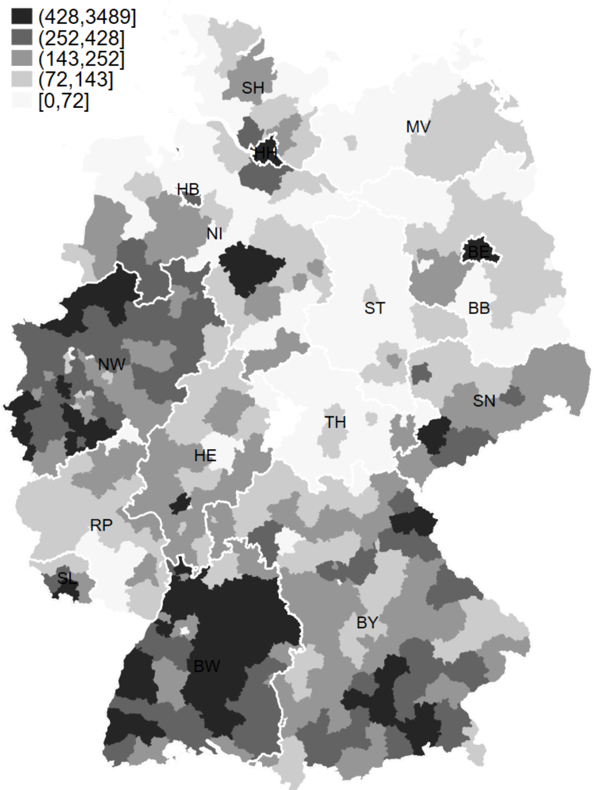
[10] We retrieved the data on August 21, 2024. Due to different sources, these data deviate from those reported by Center for Systems Science and Engineering (CSSE) at Johns Hopkins University.

Figure 2: Occurrence of COVID-19 cases by German district

(a) Cumulated cases on March 15, 2020



(b) Cumulated cases on April 4, 2020



Notes: Maps visualize the cumulated number of reported COVID-19 cases per 100,000 residents until March 10 and April 4, 2020 for each German district. The class breaks are based on the respective 25%, 50%, 75% and 90% percentiles. We do not report data for the two excluded districts Eisenach and Wartburgkreis.

We excluded observations with significant discrepancies between the date of a positive test result and the onset of symptoms. This leads to the deletion of around 1.78% of the COVID-19 cases and around 1.45% of the COVID-19 deaths.[11] Panels (a) and (b) in Figure 2 illustrate the occurrence of COVID-19 cases by German district on March 15, the day of the district elections, and on April 4, two weeks after the state-wide lockdown came into effect in Bavaria. Additionally, Table 1 highlights the development of COVID-19 cases and deaths in Bavarian districts compared to non-Bavarian districts. Up to March 15, the number of cases and deaths remained relatively low in all of Germany. However, by April 4, there was a significant increase in the average number of COVID-19 cases reported for Bavarian districts, reaching 195.52 cases per 100,000 inhabitants, compared to 96.17 in non-Bavarian districts. This stark increase is a clear indicator of the virus's rapid spread after mid-March. Furthermore, the number of COVID-19-related deaths increased rapidly at that time in comparison to all other non-Bavarian districts, with Bavarian districts reporting 10.50 deaths per 100,000 inhabitants by April 4, compared to 3.86 in other regions.

We refrain from using daily reported cases and deaths due to fluctuations caused by weekly variations in testing and reporting habits, such as on weekends. Instead, we aggregate the data. Specifically, for our analysis, we sum the daily reported cases and deaths for each calendar week for two reasons: First, compared to cumulative data, weekly values allow for better control of short-term shifts, such as those caused by lockdowns or mask mandates. Second, cumulative data continually increases, making it difficult to assess whether the rate of spread is accelerating or decelerating. Weekly figures provide a clearer perspective on how the spread is evolving, particularly in relation to public health measures and therefore offer a more accurate reflection of the current state of the pandemic in a specific region, as it is not dependent on the past severity of the outbreak in that region.

Data on German demographic, economic, health care and child care characteristics at the district level come from the Federal Statistical Office and from the Federal Institute for Building and Regional Planning. Summary statistics for the full list of control variables for Bavarian and non-Bavarian districts are reported in Table 1.

The Bavarian State Office for Statistics provided data on voter participation in 2020

[11] We believe these unusual delays are likely due to reporting errors to the RKI. We applied a threshold of 14 days for cases and 60 days for deaths when excluding data.

and 2014 as well as the number of voters with ballot papers (Wahlschein) in 2020. Getting ballot papers before the election is a prerequisite for casting the vote by mail. Nevertheless, the number of voters with ballot papers is not perfect for constructing the share of voters who voted in-person or by mail, as voters can also cast their vote in person after having received the ballot papers beforehand. We therefore mainly focus on overall voter participation, and then separate the share of voters with ballot papers and the remaining voters to get an idea of whether voting by mail or in-person may be driving our results.

Table 1: Summary statistics for dependent and control variables at the district level

		Districts in	
	Variable	Bavaria	all but Bavaria
COVID-19	Cases until March 15 per 100,000 inhabitants	8.08 (7.85)	7.38 (14.65)
	Cases until April 5 per 100,000 inhabitants	175.27 (108.25)	88.40 (62.80)
	Deaths until March 15 per 100,000 inhabitants	0.17 (0.71)	0.09 (0.45)
	Deaths until April 5 per 100,000 inhabitants	10.50 (13.75)	3.86 (5.03)
Demographic	Inhabitants per km^2 of settlement and traffic area	1745.45 (1028.40)	1849.25(1088.98)
	Age structure of population		
	% aged under 6	5.66 (0.44)	5.51 (0.51)
	% aged 6–17	10.66 (0.85)	10.72 (0.82)
	% aged 18–24	7.84 (1.09)	7.25 (1.65)
	% aged 25–29	6.24 (1.26)	5.59 (1.57)
	% aged 30–49	25.04 (1.59)	24.23 (1.67)
	% aged 50–64	23.32 (1.87)	23.73 (2.16)
	% aged ≥65	21.24 (2.00)	22.96 (3.09)
	Population development per 1,000 inhabitants	4.41 (3.43)	3.99 (3.16)
	Female share of population	50.37 (0.72)	50.65 (0.59)
	Foreign share of population	11.59 (4.86)	10.61 (5.54)
	Religion		
	Share of catholics	0.57 (0.19)	0.26 (0.22)
	Share of protestants	0.23 (0.17)	0.34 (0.17)

(continues)

Table 1: Summary statistics *continued*

	Variable	Districts in Bavaria	Districts in all but Bavaria
Health and Social issues	Child care participation rates		
	% aged 0–2 years	27.23 (7.30)	36.96 (13.05)
	Hospital beds per 10,000 inhabitants	65.38 (54.62)	62.11 (32.34)
	Geriatric demand and supply per 10,000 inhabitants		
	Elderly in need of care	411.91 (102.32)	547.55 (110.77)
	Nursing home places	109.27 (31.87)	114.61 (29.19)
	General physicians per 100,000 inhabitants	65.65 (6.82)	62.14 (5.69)
Economic	Unemployment		
	Unemployment rate in %	2.87 (1.00)	5.27 (2.03)
	Share of unemployed aged 55–64	27.10 (4.19)	23.70 (4.57)
	Unemployment rate women in %	2.67 (0.95)	4.99 (1.94)
	Employment rate	64.45 (2.92)	61.75 (4.37)
	Household income per capita per month	2080.75 (194.20)	1905.85 (187.45)
	GDP per capita in EUR 1,000	44.85 (20.71)	37.05 (15.65)
	Commuters		
	% out	45.91 (13.19)	41.39 (13.20)
	% in	43.22 (15.36)	37.35 (13.60)
	Share of workers with academic degree	14.02 (6.70)	14.00 (6.57)
	Tourism		
	Stays in hotels per capita	7.16 (7.84)	5.66 (7.11)
	Share of stays in hotels by foreigners	16.15 (9.49)	13.69 (9.20)
	Share of workers working from home	23.51 (3.03)	23.54 (3.04)
	Driving time to Ischgl (in hours)	4.11 (0.91)	6.85 (1.68)
Bavaria	Voter participation in 2020	47.22 (6.85)	–
	Share of in-person voters in 2020	18.07 (3.05)	–
	Share of ballot voters in 2020	29.14 (7.31)	–
	# of strong-beer festivals	0.14 (0.49)	–

Notes: Unweighted sample means with standard deviation in parentheses. In the SCM analysis and some regression specifications, we include administrative and structural district-type dummies.

Alipour et al. (2023, 2021) and Felbermayr et al. (2021) shared their data on the share of employees working from home and different measures of the distance to Ischgl at the district level, respectively.[12] For the analysis in this paper, we generated our own measure of the driving time in hours between Ischgl and the administrative center of each

[12]We are grateful to the respective authors for sharing their data.

German district based on the "preferred route" in openrouteservice.org. Its correlation with the driving time measure in Felbermayr et al. (2021) is 0.99 for all German and 0.96 for Bavarian districts.

2.4 Methods

We use two different approaches to investigate whether the Bavarian local elections in March 2020 played a role in the disproportionate increase in COVID-19 cases in Bavaria until mid-April.

2.4.1 Synthetic Control Method

We consider district-level data on COVID-19 cases per 100,000 residents as our outcome and employ the Synthetic Control Method (SCM) for causal inference in comparative case studies as developed in Abadie and Gardeazabal (2003), Abadie et al. (2010) and Abadie et al. (2015). We define the potential effect of the election $\delta_{i,t}$ as

$$\delta_{i,t} = Y_{i,t}^{Treat} - Y_{i,t}^{C} \text{ for all } t > T_0, \tag{1}$$

where subscript $i = 1, ..., K$ denotes the K Bavarian districts exposed to the intervention at time t. We differentiate the time period $t = 1, \dots, T$ in a pre-treatment period $t = 1, \dots, T_0$ and a post-treatment period $t = T_0 + 1, \dots, T$.[13] $Y_{i,t}^{Treat}$ denotes the outcome for a district i at time t that was exposed to the intervention, while $Y_{i,t}^{C}$ denotes the counterfactual outcome without the intervention for district i at time t. Given that we do not observe the counterfactual, we impute $Y_{i,t}^{C}$ by a weighted average of the outcomes from all non-Bavarian districts $j = 1, ..., J$, which are also known as "donor units":

$$Y_{i,t=1}^{C} = \sum_{j=1}^{J} w_j \cdot Y_{j,t=1}. \tag{2}$$

The vector of weights $W = \{w_1, \dots, w_J\}$ is derived numerically by solving the following minimization problem:

$$\min \left[\sum_{m=1}^{N} v_m (X_{i,m} - X_{j,m} W)^2 \right], \tag{3}$$

[13]The pre-treatment period starts with calendar week 6, which is the first full week after the first reported COVID-19 case in Germany on January 28. The post-treatment period ends with calendar week 27 in 2020.

where $w_j \geqq 0$ for $j = 1, \ldots, J$ and $\sum_{j=1}^{J} w_j = 1$. The factor v_m is a weight that reflects the importance of the mth variable from our set of predictor variables (X_m, with $m = 1, \ldots, N$) used to measure the distance between the treated and the control districts. The predictor weights $V_m = (v_1, ..., v_N)$ are determined by minimizing the mean squared prediction error for the pre-treatment period, i.e.

$$\min \left[\sum_{t=1}^{T_0} (Y_{i,t} - (\sum_{j=1}^{J} w_i(V) Y_{j,t}))^2 \right]. \tag{4}$$

In addition to the variables shown in Table 1, we include the values of weekly cases and deaths per 100,000 residents from the weeks leading up to the election day as additional predictors in our SCM approaches.

Following Cavallo et al. (2013), we extend the one unit synthetical treatment analysis by repeating the calculation for each Bavarian district $i = 1, \ldots, K$ while excluding all other treated districts from the pool of donor units. We then compute the average treatment effect on the treated as follows:

$$ATT_t = \frac{1}{K} \sum_{i=1}^{K} \delta_{i,t}. \tag{5}$$

To assess the statistical significance of our results, we conduct placebo tests and estimate the same model for each untreated district, treating it as if it had received the intervention. For better comparability, we standardize both the individual treatment effects and the placebo effects by dividing all estimates by their corresponding pre-treatment match quality, yielding standardized (studentized) measures. The resulting adjusted p-values represent the proportion of placebo estimates that produce an effect at least as large as the ATT.[14]

Another way to assess the validity of the measured effect is to analyze the ratio of the *root mean squared prediction error* (RMSPE) from the pre-treatment period to the RMSPE from the post-election period. This ratio provides a meaningful metric because, as Abadie et al. (2015) states, "[a] large post-intervention RMSPE is not indicative of a large effect of the intervention if the synthetic control does not closely reproduce the outcome of interest prior to the intervention. That is, a large post-intervention RMSPE

[14] For our SCM analysis, we use the STATA packages `synth` by Abadie et al. (2015) and `synth_runner` by Galiani and Quistorff (2017).

is not indicative of a large effect of the intervention if the pre-intervention RMSPE is also large." (p. 505). To illustrate this, we present boxplots of these ratios for both the placebo (non-Bavarian) districts and the Bavarian districts.

For our baseline analysis of COVID-19 cases and deaths, we limit our donor pool to 299 districts. We exclude Ludwigshafen am Rhein, Pirmasens, Germersheim, and Rhein-Pfalz-Kreis due to missing data for some predictor variables. Additionally, we exclude Stadtkreis Eisenach and Wartburgkreis because the former was merged into the latter in July 2021, and some of our sources report data separately for both, while others report only for Wartburgkreis. We also remove Stadtkreis Jena because this district was one of the first to mandate face masks in public on April 2nd. Furthermore, we exclude all Bavarian districts that hosted a strong-beer festival before the election (see Table A.1), as we expect that the spread of COVID in these districts was significantly impacted by the festivals. This leaves us with 87 treated units in our baseline specification.

2.4.2 Within-State Regression Analysis

In our second approach we restrict our analysis to data from within Bavaria and investigate whether COVID-19 cases and deaths after the election on March 15, 2020 developed differently depending on voter participation. Voter participation is meant to capture the "intensity of the election" and the population's exposure to physical interaction *due to the election*. Importantly, our main measure comprises all voters (in-person and mail) and it thus may not only capture the effect of casting the vote at the ballot box but could also capture effects of other events surrounding the election that are related to higher overall voter participation. In order to investigate whether the effect can be attributed to casting the vote in person, we later include a proxy for the share of in-person voters.

The dependent variables are the number of COVID-19 cases and deaths, respectively, between Sunday, March 15 and Sunday, April 5, two weeks after the state-wide lockdown came into effect on March 21. To address the fact that different districts were in different phases of the pandemic at the time of the election, we control for the number of of COVID-19 cases and deaths per 100,000 inhabitants between March 1 and March 15. Like in the SCM approach, we further control for differences in demographic, economic, health and child care characteristics across districts and acknowledge findings in the literature on drivers of COVID-19 cases and deaths across Germany by controlling for route distances to the Austrian ski resort Ischgl (Felbermayr et al., 2021), measured by the *driving time in hours*, and the *share of employees ever working from home* (Alipour et al., 2023, 2021) for each district.

In some specifications, we also include administrative (i.e. *Kreisfreie Stadt* or *Landkreis*) and structural dummy variables identifying district types (i.e. city, urban, rural, and sparse) according to the classification of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (*Bundesinstitut für Bau-, Stadt- und Raumforschung*, BBSR). The final specifications control for the number of confirmed strong-beer festivals held in Bavarian municipalities in early March 2020.[15]

This yields the following regression model for the differences in cases between March 15 and April 5 per 100,000 inhabitants for every Bavarian district i:

$$\Delta Cases_{i,\text{April 5--March 15}} = \alpha + \beta \cdot Voter_i + \gamma \cdot \Delta Cases_{i,\text{March 15--March 1}} + \mathbf{X}_i \cdot \delta + \eta \cdot Beer_i + \varepsilon_i, \quad (6)$$

where α denotes a common intercept, β the coefficient of interest on voter participation in the Bavarian municipal elections, γ the coefficient on the difference of known cases in district i between March 1 and March 15, and δ a vector of coefficients pertaining to the district-level control variables in the matrix $\mathbf{X}_i$. Equation 6 is estimated by ordinary least squares (OLS) with White (1980) robust standard errors. In the model for COVID-19-related deaths, we merely substitute $\Delta Cases_{i,\text{April 5--March 15}}$ for $\Delta Deaths_{i,\text{April 5--March 15}}$.

2.5 Results

This section presents the empirical evidence based on the econometric approaches discussed above.

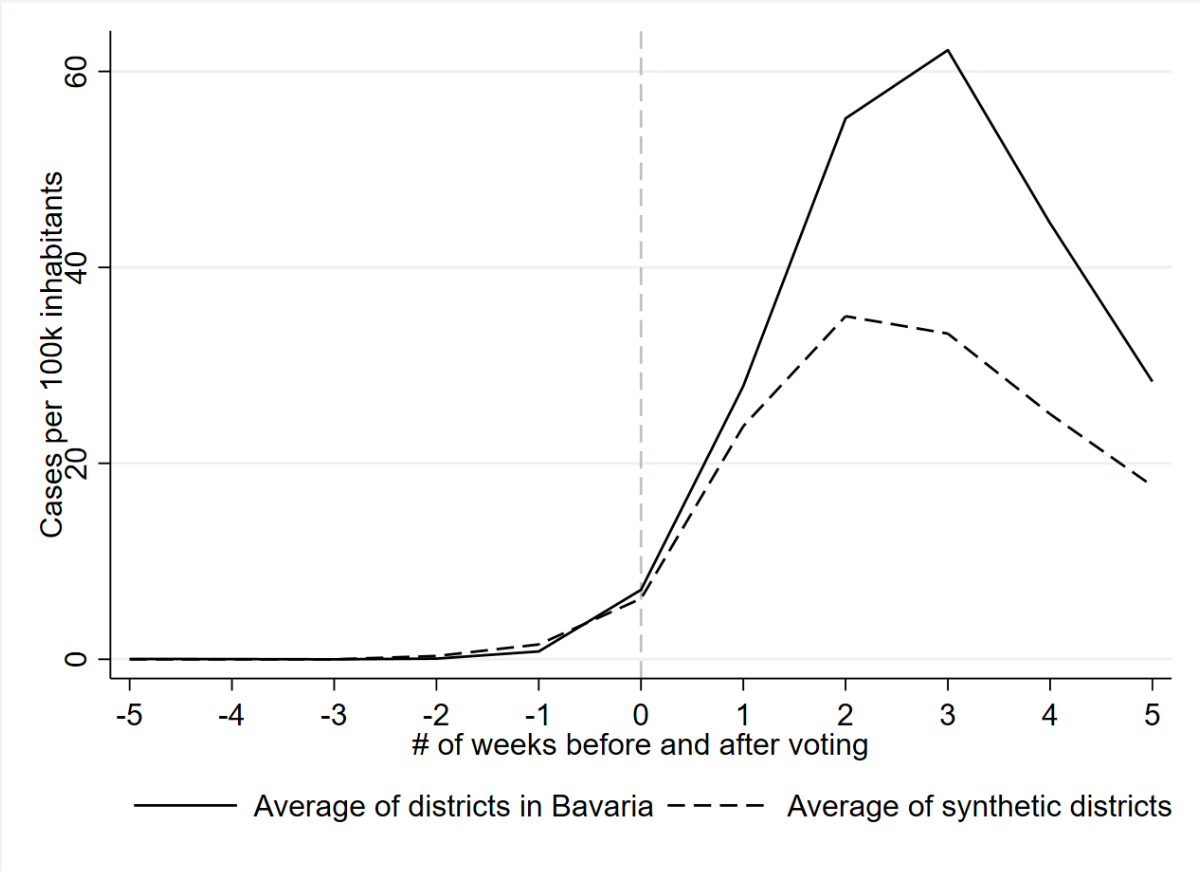
2.5.1 Synthetic Control Results

Figure 3 displays the SCM results for the district elections in Bavaria on March 15 for the number of weekly COVID-19 cases per 100,000. In panel a), the comparison of the evolution of cases for an average Bavarian district with the evolution for an average synthetic control district suggests a good fit (RMSPE=0.007) during the pre-treatment period from calendar week 6 until 11 2020. At the same time, we observe a widening

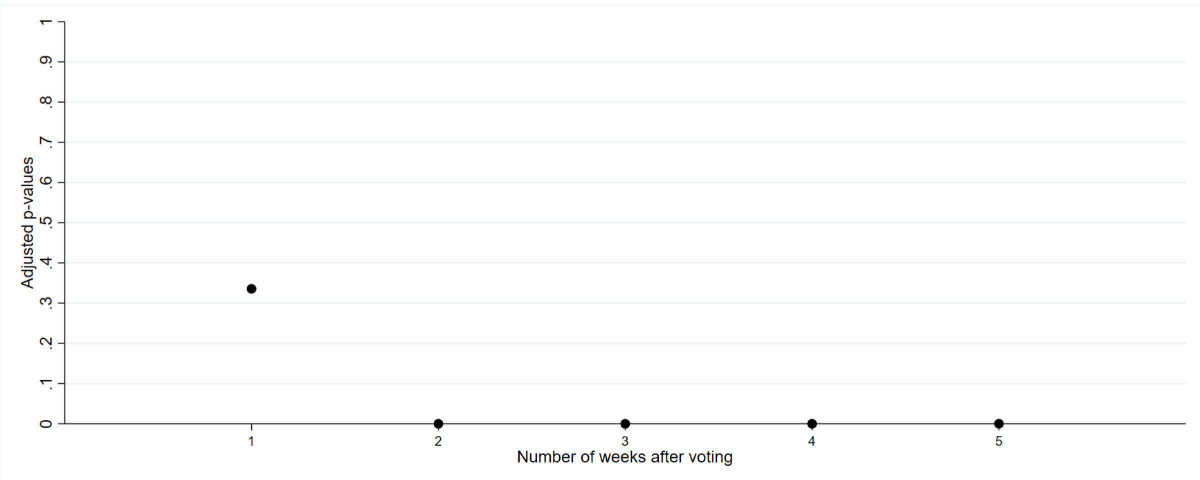
[15]Table 1 and A.1 reports summary statistics for the control variables used in the regression analysis and the Bavarian municipalities hosting strong-beer festivals in March 2020, respectively.

Figure 3: SCM - Cases per 100,000 inhabitants

a) Development COVID-19 Cases Bavaria and Synthetic Control



b) Adjusted p-values



Notes: Graph in Panel a shows the development of COVID-19 cases per week per 100,000 residents (black line) for each of the 87 Bavarian districts considered together with its synthetic counterpart (dashed line). Panel b plots the adjusted p-values for every week after the election.

gap after calendar week 11. As expected, COVID-19 cases in Bavaria start to increase relative to the respective synthetic control districts, albeit with some delay. This delay is expected, as the estimated median incubation period for the then-dominant Alpha variant of the virus is around five days. Furthermore, 97.5% of those who develop symptoms will do so within around 12 days after an infection (see, e.g. Lauer et al., 2020; Linton et al., 2020). Starting at one week after and clearly visible at two weeks after the election, we observe an increasing difference, which may be interpreted as the treatment effect on the treated. Regarding the estimated difference in calendar week 13, we find that about 20 per 100,000 or more than one-third of the total increase in COVID-19 cases are not explained by district-level demographic, economic, health and child care characteristics, nor the distance to Ischgl.

Table A.2 in the Appendix lists, for each Bavarian district, the three districts that contribute the largest weights in the synthetic control. While most districts have more than three contributing donors, the top three collectively account for at least two-thirds of the total weight. Additionally, Table A.3 shows that districts in neighboring federal states, such as Baden-Württemberg (BW) and Hesse (HE), frequently rank among the top three donors. Districts from more distant states, including Lower Saxony (NS) and Rhineland-Palatinate (RP), are also among the highest contributors.

Figure A.1 shows the estimated effect for each of the 87 Bavarian districts considered. Although some districts (like Unterallgäu or Kempten) have no difference and one district (Memmingen) reports a slightly negative difference around two weeks after the election, most of the districts have a pattern comparable to the average treatment effect on the treated observed in Figure 3.

To investigate whether the observed difference may result of chance, we ran placebo-in-space tests. For these, the districts of the other 15 German states are treated as treated units. These pseudo-treatment effects are then used to construct adjusted p-values that capture the probability of a larger treatment effect than the one observed for Bavaria. The adjusted p-values plotted in Figure 3, panel b) indicate that the increase in COVID-19 cases in Bavarian districts did not happen by pure chance, but is likely related to an event in mid-March of 2020.

Figure 4 presents two boxplots - one for the ratio from the placebo SCMs, where we define the actually not affected districts as treated, and one from the SCMs for affected districts. Although some of the non-affected districts report a relatively high ratio, we see that the quartiles of the Bavarian districts are always higher than the quartiles of the non-affected districts. A one-sided t-test clearly rejects the null hypothesis that the difference in the average of the ratios is equal to zero in favor of the alternative that the ratio for Bavarian districts is on average higher (p-value=0.002).

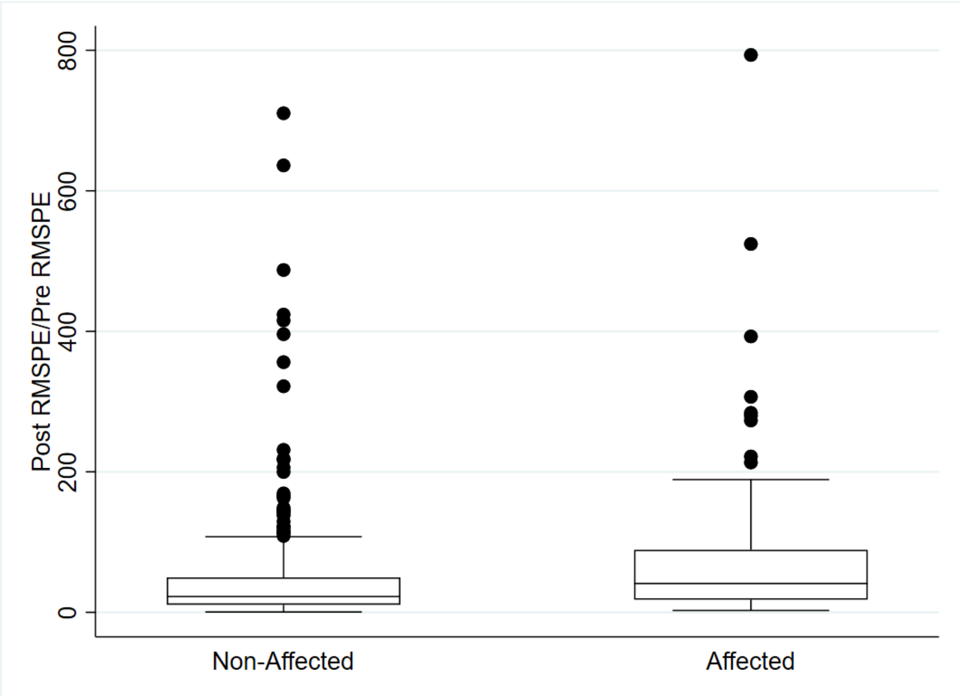
As an additional sensitivity analysis, we assigned a hypothetical treatment date to March 1, two weeks before the actual elections. As Figure A.2 indicates, there are no differences between the average Bavarian district and the average synthetic control before March 20 and a clear spread two weeks after the election, suggesting that the difference occurred due to the election or other events close in time, and not due to earlier events.

In an additional placebo approach, we applied the same SCM procedure to a hypothetical election set for mid-March 2022 in Bavaria.[16] During this period, both the Delta and Omicron variants of COVID-19 were spreading widely in Germany (Tolksdorf et al., 2022). To account for these developments, we extended the pre-treatment period compared to our main analysis to begin already in calendar week 40 of 2021. Figure A.3 in the appendix shows no significant change in the number of registered COVID-19 cases following the placebo election in mid-March 2022, though the overall pre-treatment fit appears worse than in the baseline analysis. However, the ratio of post- to pre-RMSPE is equal to 0.58. A potential limitation of this approach could be variations in testing behavior. However, we do not anticipate significant differences in testing behavior in our baseline setting, given the early stage of the pandemic in Germany in March 2020. Yet, two years later, variations in testing practices across districts or federal states may have emerged. Nevertheless, it seems unlikely that potential differences in testing behavior in the spring of 2022 obscure systematic disparities in the spread of COVID-19 in Bavaria compared to the rest of Germany after March 15, 2022.

A potential confounder for the effects of the election on March 15, 2020 could be other super spreader events such as carnival parties or strong-beer festivals, that occurred close in time to the election. Unfortunately, we lack reliable data to control for such other super spreader events generally. However, we collected information on strong-beer festivals and excluded districts with strong-beer festivals to avoid confounding the election effect with

[16]We selected mid-March 2022 (calendar week 11) instead of mid-March 2021 for this placebo election because three federal states with the highest number of districts among the top three donors from the baseline SCM analysis (Baden-Württemberg, Rhineland-Palatinate, and Hesse) held regional elections in March 2021.

Figure 4: Boxplots for the RMSPE-Ratio – cases



Notes: Graph includes two boxplots of the pre-treatment to post-treatment ratio of the RMSPE for all donor districts and all Bavarian districts.

the effect of strong-beer festivals. When we instead include these districts, the average treatment effects are larger, see Figure A.4 with an estimated effect of about 28 cases per 100,000 residents two weeks after the election for districts with strong-beer festivals, compared to an effect of 20 cases from the baseline analysis. This possibly indicates that strong-beer festivals and elections had re-enforcing effects.[17]

Another robustness test aims to address the relatively short pre-treatment period in our baseline setting (5 weeks) compared to the large number of potential donors. When the pre-treatment period is limited and many donors are included, random events in the data can create a misleading appearance of a good fit (Abadie, 2021). To reduce this risk of over-fitting, we applied the SCM approach at the federal level rather than the district level. Here, Bavaria serves as the treatment unit, while the donor pool consists of the other 15 federal states. Figure A.5 shows a development comparable to the baseline and to the specification including the strong-beer districts. The estimated effect two weeks post-election is approximately 28 cases per 100,000 residents.

As a final robustness check, we applied the Synthetic Difference-in-Differences (SDiD)

[17] An analysis that includes the city of Jena in the donor pool yields results comparable to the baseline. Results are available upon request.

approach developed by Arkhangelsky et al. (2021). Like the SCM, SDiD reweights and aligns pre-exposure trends, minimizing reliance on parallel trend assumptions. Similar to difference-in-differences, it is unaffected by additive unit-level shifts and enables valid inference in large-panel contexts.[18] By reapplying our baseline setting (once again excluding districts with strong-beer festivals), Figure A.6 illustrates a widening gap following the election held in calendar week 11. Note that the SDiD method does not require the synthetic counterpart to be identical to the treated unit in the pre-treatment period. The estimated difference two weeks post-election is approximately 33 cases per 100,000 people, and the treatment effect estimate for the entire post-treatment period suggests that Bavarian districts faced about 24 additional cases in each post-treatment week.

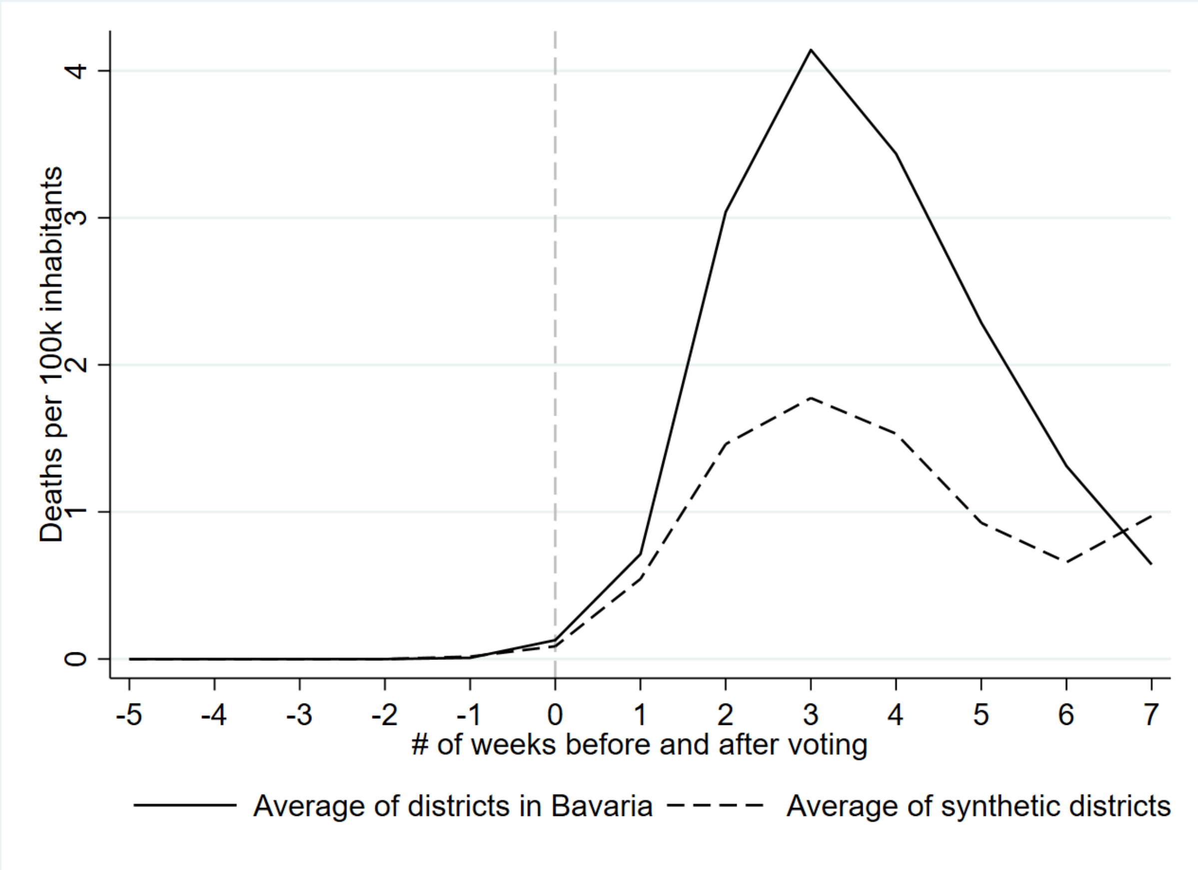
Figure 5 illustrates the progression of weekly COVID-19-related deaths in the average Bavarian district compared to the average synthetic control (panel a) and shows the adjusted p-values for testing the null hypothesis of no systematic difference in trends (panel b). Tables A.4 and A.5 in the appendix display the top three donors along with their respective federal states. Contrary to expectations based on an estimated time-lag of around 16 days (95% CI: 13–19 days) between infection and death due to COVID-19 (Khalili et al., 2020), Figure 5 suggests that COVID-19-related deaths begin to rise more in Bavaria than in the average control district even before the election. Figure 5, panel b), however, indicates that the observed differences for the COVID-19 related deaths could just be appearing by chance and should not be interpreted as significant. This result may be due to limitations of the synthetic control method in identifying appropriate control districts for COVID-19 deaths, as there were very few deaths before March 15 — many Bavarian districts had none — and the evolution of other predictors was insufficient to create reliable synthetic control units. This assumption is supported by examining the results for each Bavarian district individually, as shown in Figure A.7 in the appendix. While reported COVID-19 deaths in Germany increase noticeably after mid-March (see Figure 1), nearly all Bavarian districts have synthetic counterparts with an almost flat line of COVID-19 deaths.

Given the estimated lag between COVID-19 symptom onset and death, we conducted an additional SCM analysis for COVID-19 deaths, designating the treatment period to

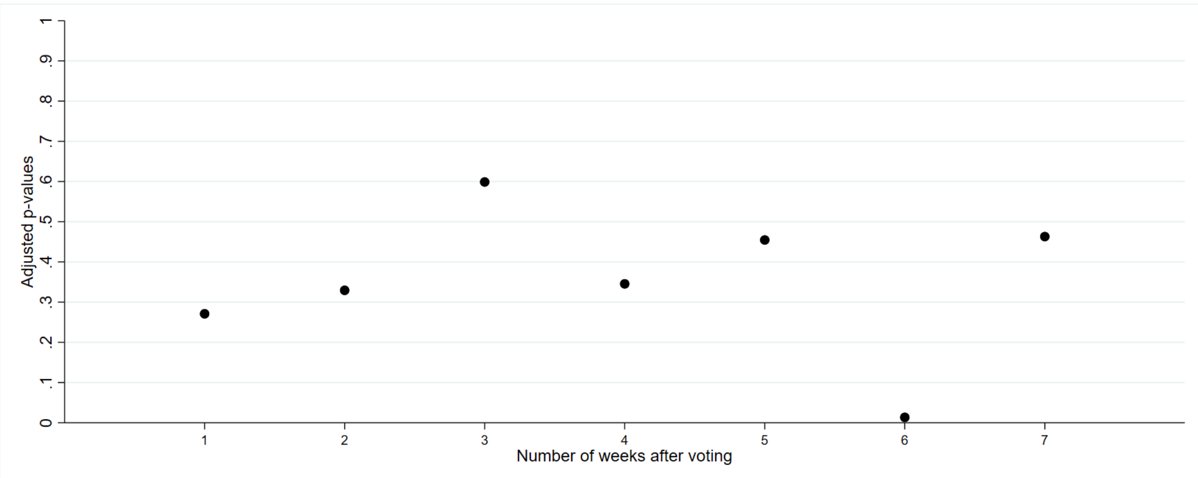
[18] Unlike the baseline procedure using the *synth_runner* package, the Stata package *sdid* by Clarke et al. (2023), employed for our SDiD analyses, first constructs an average Bavarian district before creating a synthetic counterpart from other German districts for comparison. This feature serves as an additional sensitivity aspect of the SDiD approach compared to the SCM.

Figure 5: SCM – Deaths per 100,000 inhabitants

a) Development of COVID-19 Deaths Bavaria and Synthetic Control



b) Adjusted p-values



Notes: Graph in Panel a shows the development of COVID-19 deaths per week per 100,000 residents (black line) for each of the 87 Bavarian districts considered together with its synthetic counterpart (dashed line). Panel b plots the adjusted p-values for every week after the election.

start two weeks after the election. We chose a two-week lag based on the lower boundary of the time-lag estimated by Khalili et al. (2020) of 13 days. This approach further enables us to incorporate the number of COVID-19-related deaths before calendar week 13 as an additional predictor. The results shown in Figure 6 are inconclusive. Although panel a displays a noticeable divergence in deaths per 100,000 residents after calendar week 13, the pre-treatment fit appears poor. Additionally, the adjusted p-values in panel b suggest that the observed differences may be due to random variation. We interpret these findings as being impacted by the low variation in the number of deaths per 100,000 residents at the beginning of the pandemic in Germany, where regional events likely have a significant effect on the overall results.[19]

2.5.2 Within-State Regression Results

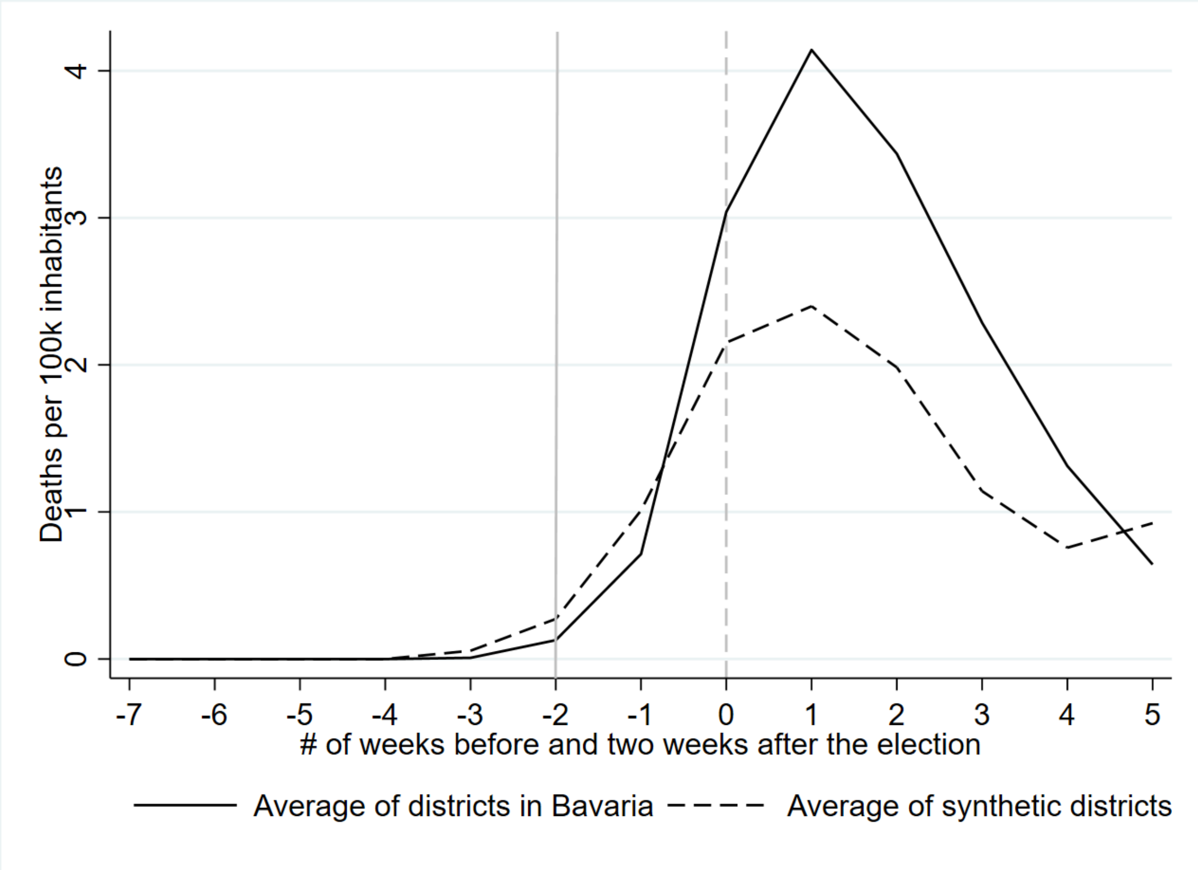
While the results based on the SCM in Section 2.5.1 strongly suggest an unexpectedly large increase of COVID-19 cases in Bavaria relative to the synthetic control districts, these differences may not necessarily be driven by the election but could reflect other unobserved differences between the Bavarian districts and their synthetic controls. Furthermore, the SCM approach was not able to deliver reliable results for effects of the election on COVID-19-related deaths. In this section, we therefore try to isolate the effect of the municipal elections by regressing the increase in COVID-19 cases and deaths on the number of voters per population as a measure of the elections' "treatment intensity", while again controlling for demographic, economic, health and child care characteristics as well as other candidate variables that may help explain the spread of COVID-19. Table 2 and Table 3 report the results for four different specifications of Equation (6) for increase in cases and deaths between March 15 and April 5 (three weeks after the election and two weeks after the lockdown was imposed), respectively. All regressions are estimated by OLS at the district level. Furthermore, Figure 7 explores the development of the election effect over time for cases/100,000 (panel a) and deaths/100,000 inhabitants (panel b).

All specifications in Tables 2 and 3 include the number of COVID-19 cases between March 1 and March 15 to control for the epidemiological state of a given district, the share of employees ever working from home, and the driving time to Ischgl in hours. The latter two have been shown to explain the spread of the pandemic across German

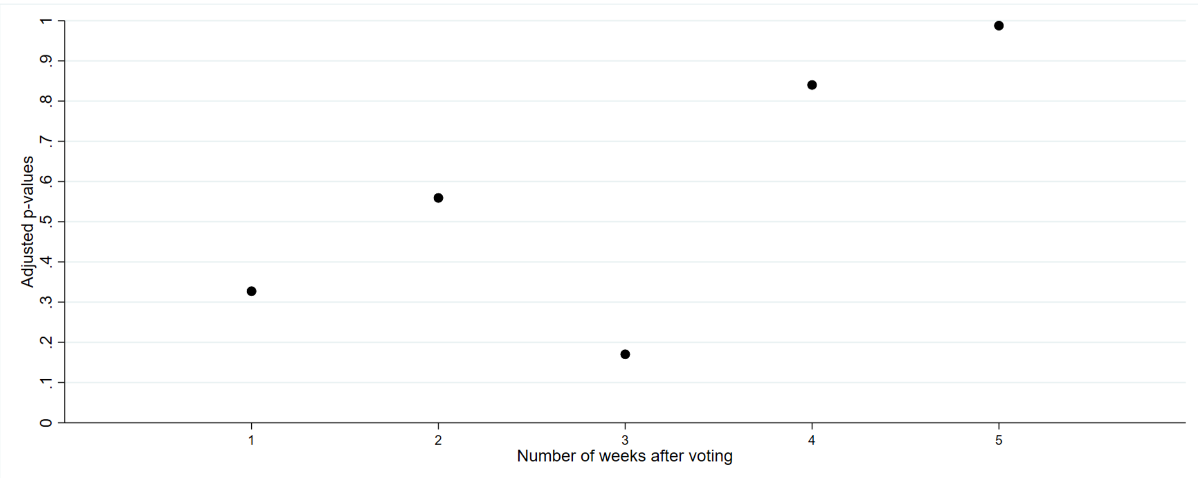
[19]Additional analyses in line with the sensitivity checks for COVID-19 cases were performed. They align with the inconclusive results of the baseline specification. These results are available upon request.

Figure 6: SCM for Deaths with two weeks time-lag

a) Development of COVID-19 Deaths Bavaria and Synthetic Control



b) Adjusted p-values



Notes: Graph in Panel a shows the development of COVID-19 deaths per week per 100,000 residents (black line) for each Bavarian district considered together with its synthetic counterpart (dashed line). Panel b plots the adjusted p-values for every week after the calendar week 13 2020.

districts in Alipour et al. (2023, 2021) and Felbermayr et al. (2021), respectively. Note that the coefficient of main interest is on *voter participation* in the Bavarian municipal elections on March 15, measured as the number of actual voters per population in percent. Accordingly, the point estimate of this coefficient can be interpreted as the increase in the number of COVID-19 cases and deaths, respectively, per 100,000 inhabitants associated with a 1 percentage point-increase in overall voter participation.

In specification (1) in Table 2, this point estimate equals 15.0 cases per 100,000 inhabitants and is statistically significant at the 5% level. While the coefficient on the *working from home* index is positive and not statistically different from zero, the estimate for *distance to Ischgl* has the expected negative sign and is statistically significant at the 10% level, indicating that a 1-hour-increase in the driving time to Ischgl reduces the increase in COVID-19 cases by 49.1 cases per 100,000 inhabitants.

In specification (2), we furthermore account for unobserved characteristics that may be common to *Kreisfreie Städte* as opposed to *Landkreise* or common to city, urban, rural, and sparsely populated districts, respectively. When including administrative and structural district-type dummies, the coefficient estimate on *voter participation* stays almost the same, while its standard error increases slightly. In Table 3 the point estimate decreases from 2.19 to 1.98 COVID-19-related deaths per 100,000 inhabitants, when adding administrative and structural district-type dummies, without any effect on the level of statistical significance.

In specification (3), we add the number of confirmed strong-beer festivals held in several Bavarian municipalities in early March (see Table A.1). While this reduces the point estimate of the coefficients on *voter participation* in Tables 2 and 3, it also accounts for some of the unexplained variance in the regressions and reduces thus the standard error of the coefficients, which remain statistically significant at the 5%, respectively. It is important to note the large positive and statistically significant coefficient on *beerfestival*, which suggests that one such event raised the number of COVID-19 cases and deaths between March 15 and April 5 in the hosting district by 97.1 and 9.04, respectively, per 100,000 inhabitants. Note also that the coefficient estimate associated with the *distance to Ischgl* in both tables reduces drastically and becomes statistically insignificant, suggesting that within Bavaria the distance to Ischgl (as a proxy for skiing tourism) is not as important as in the Germany-wide analysis conducted by Felbermayr et al. (2021).

Table 2: Increase in COVID-19 cases/100,000 between March 15 and April 5

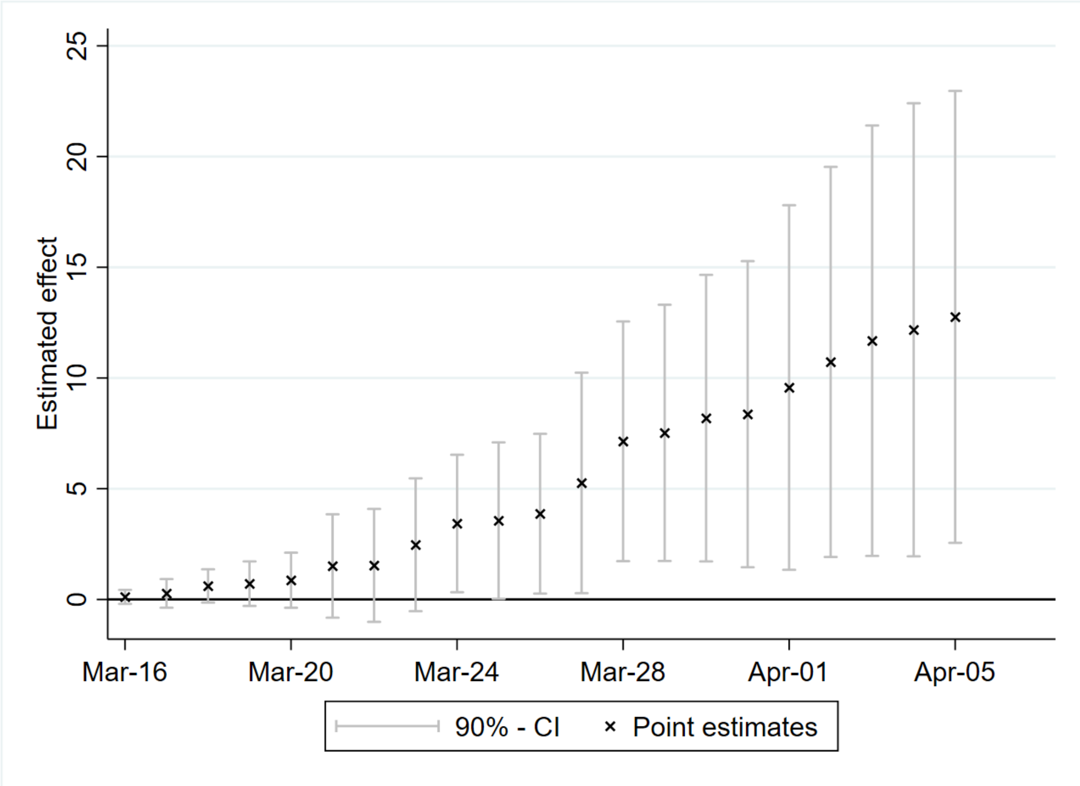
	(1)	(2)	(3)	(4)
$\Delta Cases_{March15-March1}$	4.35* (2.44)	4.54* (2.44)	4.30* (2.26)	4.19* (2.44)
$Voters$ / *pop.*	15.0** (6.35)	15.0** (6.44)	12.8** (5.10)	
$In-person\ voters$ / *pop.*				10.7 (8.59)
Ballot papers / pop.				12.8** (5.10)
$Working\ from\ home$	2.39 (10.91)	3.98 (11.55)	2.99 (8.31)	2.84 (8.44)
$Distance\ from\ Ischgl$	-49.1* (25.45)	-45.8* (26.82)	-20.5 (22.07)	-23.9 (24.53)
$Beer\ festival$			97.1*** (24.92)	97.7*** (25.07)
N	96	96	96	96
adj. R^2	0.236	0.252	0.459	0.451
Demographic	Y	Y	Y	Y
Economic	Y	Y	Y	Y
H&C care	Y	Y	Y	Y
District type dummies	N	Y	Y	Y

Notes: Coefficient estimates with robust standard errors in parentheses. ***/**/* indicates statistical significance at the 1/5/10% level. $\Delta Cases_{\text{March 15- March 1}}$ denotes the number of COVID-19 cases per 100,000 residents up to date x. $Beer\ festival$ measures the number of confirmed strong-beer festivals held in a given district in March 2020 (see Table A.1). $Voter$/*pop.* denotes the number of voters in the Bavarian municipal elections on March 15, 2020 relative to the population. *Demographic*, *Economic*, and *H&C care* denote demographic, economic, health care and social care controls at the district level. *District type dummies* denotes administrative and structural classifications by the *Bundesinstitut fuer Bau-, Stadt- und Raumforschung* (BBSR). The complete set of control variables is listed in Table 1. We do not report the intercept in the table.

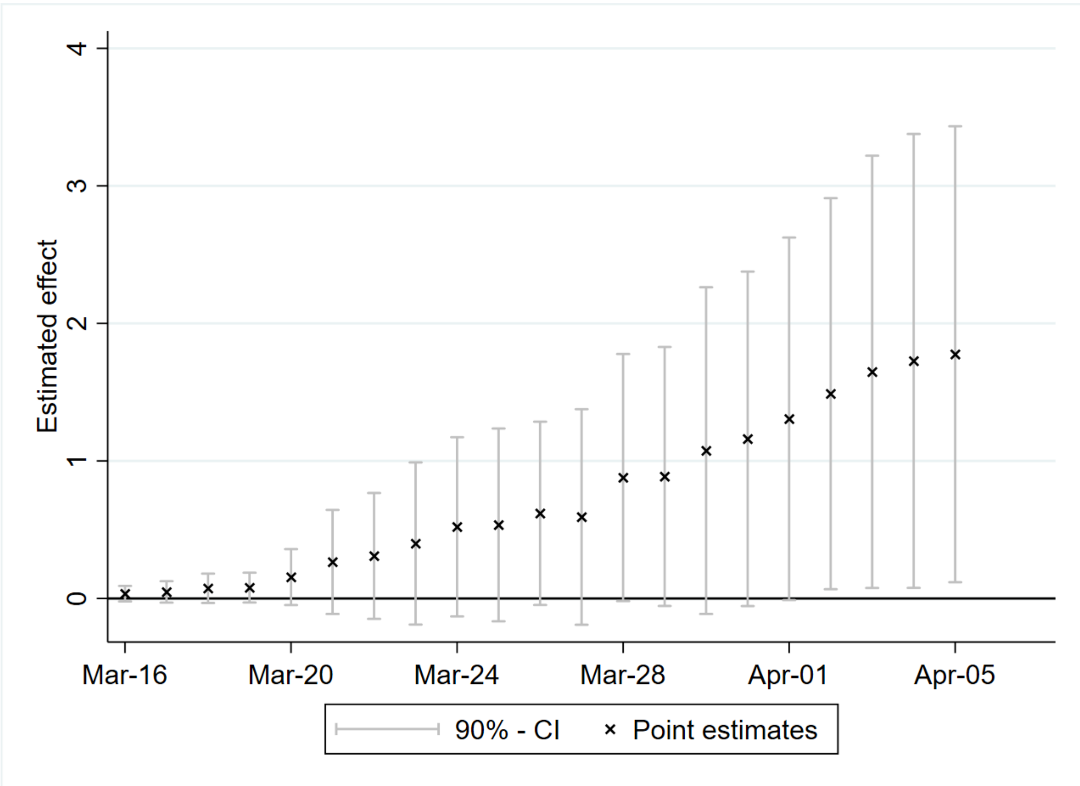
Based on specification (3) Figure 7 explores the development of the association of voter participation and COVID-19 cases (panel a) and deaths (panel b) over time. The figure displays coefficient estimates and 90% confidence intervals of the voter participation variable for the difference in cases (deaths) of any day between March 10 and April 05 and March 15, i.e., election day. The results align nicely with the ones reported in Section 2.5.1. For cases, the association is significantly different from zero at the 10%-level on March 24 and for all days after March 26. For deaths, the association is significant at the 10%-level after April 01. That the associations are only continuously significant starting roughly two weeks after the election for cases and three weeks later for deaths is as expected given the incubation time of the virus and the time lag between infection and death.

Figure 7: Effect of voter participation over time

a) COVID-19 Cases/100,000 inhabitants



b) COVID-19-related Deaths/100,000 inhabitants



Notes: The figure displays coefficient estimates and 90% confidence intervals of the voter participation variable for the difference in cumulated cases (deaths) of any day after March 15 and April 05.

Table 3: Increase in COVID-19 deaths/100,000 between March 15 and April 5

	(1)	(2)	(3)	(4)
$\Delta Cases_{March15-March1}$	0.42 (0.37)	0.43 (0.37)	0.40 (0.35)	0.40 (0.37)
$Voters\ /\ pop.$	2.19** (0.87)	1.98** (0.92)	1.78** (0.83)	
$In-person\ voters\ /\ pop.$				1.75 (1.24)
$Ballot\ papers\ /\ pop.$				1.78** (0.83)
$Working\ from\ home$	-0.15 (1.29)	-0.41 (1.53)	-0.51 (1.32)	-0.51 (1.33)
$Distance\ from\ Ischgl$	-2.51 (3.06)	-3.35 (3.15)	-0.99 (2.77)	-1.03 (2.97)
$Beer\ festival$			9.04*** (2.94)	9.05*** (2.94)
N	96	96	96	96
adj. R^2	0.219	0.245	0.350	0.339
Demographic	Y	Y	Y	Y
Economic	Y	Y	Y	Y
H&C care	Y	Y	Y	Y
District type dummies	N	Y	Y	Y

Notes: Coefficient estimates with robust standard errors in parentheses. ***/**/* indicates statistical significance at the 1/5/10% level. $\Delta Cases_{\text{March 15- March 1}}$ denotes the number of COVID-19 cases per 100,000 residents up to date x. $Beer\ festival$ measures the number of confirmed strong-beer festivals held in a given district in March 2020 (see Table A.1). $Voter$/*pop.* denotes the number of voters in the Bavarian municipal elections on March 15, 2020 relative to the population. *Demographic*, *Economic*, and *H&C care* denote demographic, economic, health care and social care controls at the district level. *District type dummies* denotes administrative and structural classifications by the *Bundesinstitut fuer Bau-, Stadt- und Raumforschung* (BBSR). The complete set of control variables is listed in Table 1. We do not report the intercept in the table.

In order to quantify the potential effects of the regional elections, consider specification (3), which includes all district-level control and dummy variables as well as the number of beer festivals. The coefficient estimates in Tables 2 and 3 imply that a 1 percentage point increase in voter participation is associated with an additional 12.8 COVID-19 cases and 1.8 COVID-19-related deaths per 100,000 inhabitants, translating to roughly 1,680 cases and 236 deaths at the state level between March 15 and April 5.

In order to interpret our results causally, we have to assume that the variation in voter participation across districts was exogenous and not related to unobservable factors that also influenced the number of COVID-19 cases and deaths. For example, a potential worry is that in districts in which people were more concerned about or aware of the virus, voter participation could have been lower and at the same time, the virus spread less quickly because people were more prudent. If this was the case, our coefficient would be biased upwards. However, as we focus on overall voter participation and voters had the option of casting their vote by mail, this worry seems less important. Quite to the contrary, one might argue that voter participation is generally higher in areas in which people are more law-abiding and thus also more likely to abide to lockdown measures, which would bias our estimate downward. This argument is further supported by results presented in Appendix Table A.6. This table displays the conditional association of voter participation in the run-off ballots that took place exclusively by mail-in voting in 84 of the 96 districts on March 29 and the development of cases and deaths in the 20 days following the run-offs. Controlling for the development of cases up to the date of the run-off ballot in addition to the controls of our main specification, we see a negative association between voter participation and the development of cases and deaths in the 20 days following the run-off. This may be driven by higher participation in districts with more law-abiding inhabitants.

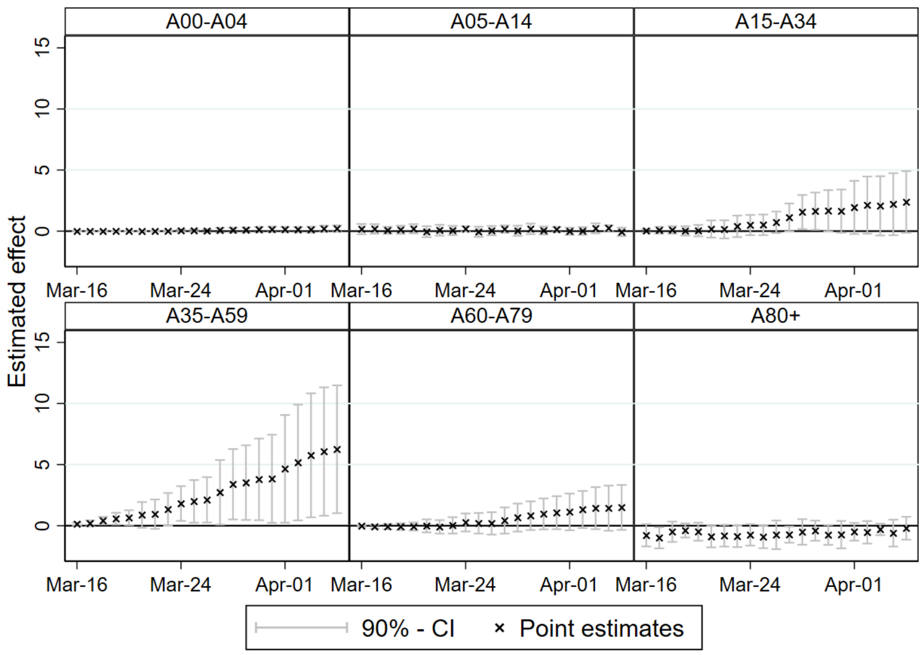
As a placebo analysis, we further tested our analysis by examining the relationship between voter participation in the main election on March 15 and the progression of COVID-19 cases and deaths between June 7 and 21, 2020, using the same control variables as in our main estimation. We find no statistically significant association between voter participation and COVID-19 cases (column (3b) in Table 4) or deaths (column (3b) in Table 5).[20]

Another interesting aspect is how different age groups were affected by the election. Figure 8 reports the estimated coefficients and their 90%-confidence intervals of the voter participation variable on the development of COVID-19 cases and deaths for different age groups. Tables A.7 and A.8 in the Appendix display the full regression results. Panel a) shows that the effect on cases is significantly different from 0 at the 10%-level only for the age group from 35 to 59 years. The effects on deaths are only significant for the oldest (aged 80+) which is not surprising given the higher death rate in this group. As Table A.8 in the Appendix indicates, for other age groups there are no significant effects on death at the 10% -level.

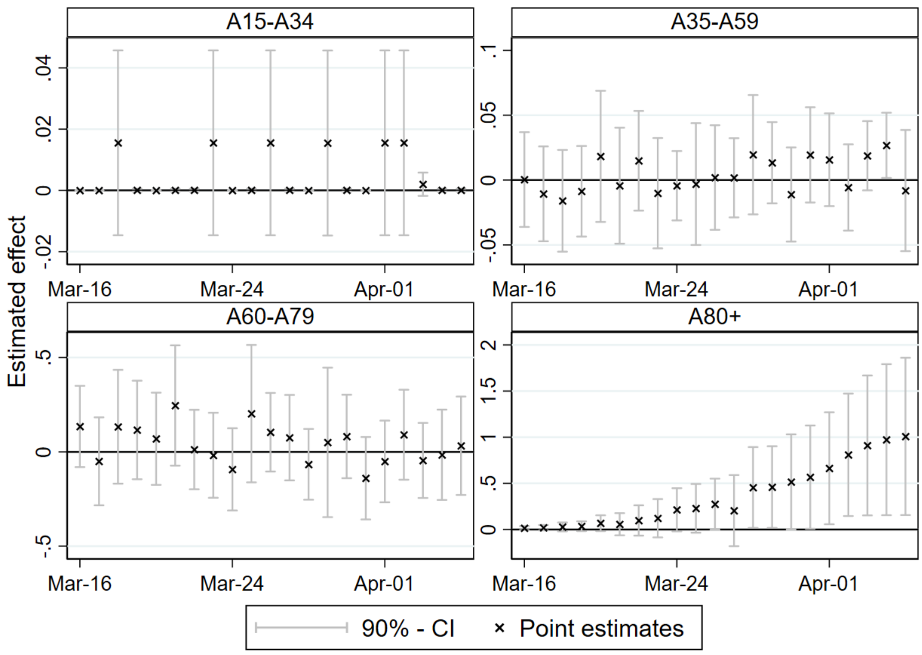
[20] We have thought about and tried various instruments in order to account for the potential endogeneity of our main variable of interest. Unfortunately, all of our potential instruments, which were voter participation in the last Bavarian municipal elections in 2014, weather on election day, and expected tightness of the election outcome, turned out to be weak instruments. Given that we only have 96 observations, we thus refrained from further pursuing this route.

Figure 8: Heterogeneity by age of effect of voter participation over time

a) COVID-19 Cases/100,000 inhabitants



b) COVID-19-related Deaths/100,000 inhabitants



Notes: The figure presents coefficient estimates and 90% confidence intervals for the voter participation variable, showing the difference in cases (deaths) for each day after March 15 and April 5 across age groups. As no COVID-19-related deaths were observed among children under 15, the two youngest age groups are not relevant for COVID-19 mortality outcomes.

Table 4: Robustness checks of specification (3) for COVID-19 cases

	(3)	(3a)	(3b)	(3c)
$\Delta Cases_{\text{March 15- March 1}}$	4.30*	3.26***		
	(2.26)	(0.35)		
$\Delta Cases_{\text{June 07- May 24}}$			0.36***	
			(0.12)	
$ln(\Delta Cases_{\text{March 15- March 1}})$				0.014
				(0.08)
$Voters\ /\ pop.$	12.8**	8.23**	0.24	0.075***
	(5.10)	(3.76)	(0.25)	(0.02)
$Working\ from\ home$	2.99	13.0**	0.52	-0.0081
	(8.31)	(6.40)	(0.47)	(0.06)
$Distance\ from\ Ischgl$	-20.5	-19.9	-0.35	-0.19
	(22.07)	(16.68)	(1.81)	(0.16)
$Beer\ festival$	97.1***	59.2***	1.08	0.40***
	(24.92)	(17.31)	(0.94)	(0.12)
N	96	96	96	90
adj. R^2	0.459	0.736	0.108	0.448
Demographic	Y	Y	Y	Y
Economic	Y	Y	Y	Y
H&C care	Y	Y	Y	Y
District type dummies	Y	Y	Y	Y

Notes: Coefficient estimates with robust standard errors in parentheses. ***/**/* indicates statistical significance at the 1/5/10% level. (3) repeats the main results from Table 2. (3a) replicates (3) for the *day of first symptoms* reported by the RKI. (3b) represents a *Placebo test* of (3), where we consider a counterfactual election day on June 7, 2020. (3c) replicates (3) in *logarithms rather than levels* of the cumulated increase of COVID-19 cases between March 15 and April 5 of 2020, i.e. the dependent variable is $\log\left(\Delta\text{Cases}_{\text{April 5–March 15}}\right)$. We do not report the intercept in the table.

Table 5: Robustness checks of specification (3) for COVID-19 deaths

	(3)	(3a)	(3b)	(3c)
$\Delta Cases_{\text{March 15- March 1}}$	0.40	0.41***		
	(0.35)	(0.10)		
$\Delta Cases_{\text{June 07- May 24}}$			0.0040	
			(0.01)	
$ln(\Delta Cases_{\text{March 15- March 1}})$				-0.093
				(0.15)
$Voters$ / *pop.*	1.78**	0.94	-0.0063	0.15**
	(0.83)	(0.59)	(0.01)	(0.06)
$Working\ from\ home$	-0.51	0.55	0.027	-0.015
	(1.32)	(1.08)	(0.03)	(0.14)
$Distance\ from\ Ischgl$	-0.99	-0.95	0.067	-0.19
	(2.77)	(2.77)	(0.06)	(0.34)
$Beer\ festival$	9.04***	5.53***	0.023	0.61***
	(2.94)	(1.84)	(0.03)	(0.19)
N	96	96	96	87
adj. R^2	0.350	0.652	0.018	0.303
Demographic	Y	Y	Y	Y
Economic	Y	Y	Y	Y
H&C care	Y	Y	Y	Y
District type dummies	Y	Y	Y	Y

Notes: Coefficient estimates with robust standard errors in parentheses. ***/**/* indicates statistical significance at the 1/5/10% level. (3) repeats the main results from Table 2. (3a) replicates (3) for the *day of first symptoms* reported by the RKI. (3b) represents a *Placebo test* of (3), where we consider a counterfactual election day on June 7, 2020. (3c) replicates (3) in *logarithms rather than levels* of the cumulated increase of COVID-19 deaths between March 15 and April 5 of 2020, i.e. the dependent variable is $\log(\Delta \text{Deaths}_{\text{April 5–March 15}})$. We do not report the intercept in the table.

So far, we have only looked at the effect of overall voter participation, which includes voters voting in person and voting by mail. In specification (4), we separately include the share of voters with ballot papers (i.e., those who may have voted by mail) and the the share of voters who voted in person (all voter minus those with ballot papers). As column (4) of Tables 2 and 3 shows both voter participation measures are positively associated with the increase in cases and deaths after the election. We interpret these results to suggest that not only the act of casting the vote in person but also likely other events surrounding the election, such as more intense election campaigning (in line with the results of Cipullo & Le Moglie, 2022) in districts with higher voter participation or more intense interactions while counting votes in districts with higher voter participation may be captured in the estimated effect.

As a robustness check, we consider the date of first symptoms reported to the RKI rather than the reporting date as the relevant time for computing the increase in COVID-19-related cases and deaths. Although the date of first symptoms might seem preferable from a medical perspective, as it is arguably closer to the date of the infection, it is also subject to larger measurement error for at least three reasons. First, the dating hinges on the patients' ability to recollect and correctly judge the beginning of COVID-19 symptoms. Second, the date of first symptoms reported to the RKI is likely based on information by third parties, such as family members or nursing home staff, in case of a medical emergency. Third, when persons without symptoms are tested positively, the date of first symptoms is not meaningful and coincides with the reporting date by definition. These caveats are illustrated in Figure A.8 in the Appendix, which plots the absolute number of cases and deaths by the delay in days between the date of first symptoms and the reporting date for cases in Germany. When the delay is positive, this indicates that symptoms started before the person was tested positive. For both cases and deaths, there is a pronounced peak at 0. This arises as the reporting date is used as date of first symptoms if the latter is unknown. In the vast majority of cases, the date of first symptoms occurred fewer than ten days before a positive test result. Column (3a) in Table 4 and Table 5 presents the regression results for COVID-19 cases and deaths, respectively, using the same control variables as in our baseline specification. The findings for cases reinforce the previous results, showing a positive effect significant on the 5%-level. However, the estimator for deaths decreases in magnitude and loses significance at any conventional level. This is likely because, at that time, the day of infection is probably not closely aligned with the day of death but rather with the onset of symptoms in the deceased individual. This could also explain why the pre-election difference in deaths now becomes significant.

The remaining columns of Tables 4 and 5 report results of a placebo analysis in time, already discussed above (columns, 3b in each Table), and use the logarithm of the outcome instead of the absolute change in cases and deaths. It's reassuring to see no association of voter participation in March and development of cases/deaths in June of 2020 as this suggests that voter participation is not taking up some other unobserved factor at the district level that determines the spread of COVID. At the same time, the results are robust to the log-transformation of the dependent variable.

In further analyses not reported here, we explore whether there is spatial dependence between the error terms across districts. As the lockdown became effective not long after election day, it's not surprising that Moran's I test suggests no spatial correlation.[21]

2.6 Conclusion

This paper quantifies the toll of voting in a pandemic by considering the case of the Bavarian municipal elections on March 15, 2020. In contrast to the subsequent run-off ballots, which were held on March 29 using postal ballots only, a substantial share of the ten million eligible voters voted at local polling stations, while public life was severely restricted on the very next day.

Using synthetic controls matched on a host of district-level demographic, economic, health care and child care characteristics as well as the distance to Ischgl, we show that Bavaria suffered an unexpectedly large increase in COVID-19 cases after March 15. To closer link the effect to the election, we further regress the increase in cases and deaths on voter participation as a measure for the "treatment intensity" of the elections across Bavaria's 96 districts, while again controlling for demographic, economic, health care and child care characteristics as well as the distance to Ischgl and the number of confirmed strong-beer festivals held in March in several municipalities. In the most conservative specification, which includes administrative and structural district-type dummy variables, our OLS analysis reveals a 1 percentage point increase in voter participation

[21] Results available upon request.

across Bavarian districts is associated with an additional 12.8 positive test results and 1.8 deaths per 100,000 inhabitants or 1,680 positive test results and 236 deaths at the state level between March 15 and April 5. Importantly, different from other papers, our measure of voting intensity does not only capture in-person voting but relies on overall voter participation, including in-person and voting by mail. When we include the two separately, both seem to be positively related to the spread of COVID-19. This suggests that not only the act of casting the vote but also other events related to the election on election day or closely before or after – possibly interaction between election workers counting the votes or campaigning efforts shortly before election day – have contributed to the spread of COVID 19 in Bavaria.

We conclude that the unfortunate timing of the municipal elections “at the dawn of a global pandemic” (Leininger & Schaub, 2023) has likely contributed to the spread of the at that time novel corona virus in Bavaria.

Appendix

Table A.1: Bavarian municipalities hosting strong-beer festivals in March 2020

Municipality	Type	Venue	Date	Visitors
Neustadt a. d. Waldnaab	Landkreis	Flossenbürg	March 7	—
Neustadt a. d. Waldnaab	Landkreis	Pressath	March 7	—
Mühldorf a. Inn	Landkreis	Neumarkt-Sankt Veit	March 7	—
Rosenheim	Kreisfreie Stadt	Rosenheim	March 6–8	1,500/day
Rosenheim	Landkreis	Rosenheim	March 6–8	1,500/day
Rottal-Inn	Landkreis	Pfarrkirch	March 7	—
Schwandorf	Landkreis	Wackersdorf	March 7	—
Straubing	Kreisfreie Stadt	Straubing	March 7	1,500
Tirschenreuth	Landkreis	Mitterteich	March 7	1,400
Wunsiedel i. Fichtelgebirge	Landkreis	Niederlamitz	March 7	—

Notes: — indicates no information on the estimated number of visitors, which is likely small. The strong-beer festival in Rosenheim was discontinued by the organizers after three days. A team of German-French broadcaster ARTE attended the festival in Niederlamitz (Theodor, 2020).

Table A.2: Weights for cases

Bavarian district	First	Second	Third	Donors
Ingolstadt	Frankfurt a.M., SK (.427)	Wolfsburg, SK (.311)	Potsdam, SK (.124)	6
München, SK	Stuttgart, SK (.57)	Frankfurt a.M., SK (.32)	Leipzig, SK (.11)	3
Altötting	Birk. (.298)	Biberach (.247)	Rottweil (.185)	10
Bercht. Land	Suhl, SK (.253)	Bernkastel-W. (.153)	Waldshut (.134)	11
Bad Tölz-W.	Biberach (.352)	Konstanz (.163)	Osterholz (.124)	11
Dachau	Teltow-Fläming (.355)	Vechta (.341)	Frankfurt a.M., SK (.24)	6
Ebersberg	Vechta (.441)	Potsdam-M. (.239)	Potsdam, SK (.167)	5
Eichstätt	Vechta (.55)	Teltow-Fläming (.177)	Frankfurt a.M., SK (.157)	5
Erding	Vechta (.415)	Alzey-W. (.227)	Frankfurt a.M., SK (.221)	5
Freising	Vechta (.452)	Frankfurt a.M., SK (.385)	Heinsberg (.133)	4
Fürstenfeldbruck	Alb-Donau-K (.302)	Main-Taunus-K (.271)	Sächsische Schweiz (.165)	8
Garmisch	Schwarzwald-Baar-K (.365)	Suhl, SK (.257)	Ahrweiler (.174)	13
Landsberg am Lech	Potsdam-M. (.241)	Vechta (.228)	Olpe (.168)	8
Miesbach	Bodenseek. (.253)	Hohenlohek. (.2)	Baden-B., SK (.181)	9
München	Main-Taunus-K (.255)	Vechta (.214)	Dresden, SK (.212)	6
Neuburg-S.	Biberach (.221)	Olpe (.217)	Vechta (.207)	7
Pfaffenhofen	Alzey-W. (.415)	Frankfurt a.M., SK (.291)	Vechta (.185)	5
Starnberg	Hochtaunusk. (.422)	Lippe (.411)	Heinsberg (.097)	5
Traunstein	Schwarzwald-Baar-K (.209)	Rottweil (.172)	Biberach (.11)	13
Weilheim-Schongau	Coesfeld (.281)	Rottweil (.2)	Biberach (.156)	11
Landshut, SK	Heilbronn, SK (.23)	Frankfurt a.M., SK (.163)	Braunschweig, SK (.158)	10
Passau, SK	Heidelbg., SK (.534)	Wilhelmsh., SK (.462)	Suhl, SK (.004)	3
Deggendorf	Trier, SK (.284)	Merzig-Wadern (.257)	Olpe (.224)	8
Freyung-Grafenau	Alzey-W. (.405)	St. Wendel (.26)	Südwestp. (.175)	5
Kelheim	Vechta (.312)	Alzey-W. (.2)	Potsdam-M. (.187)	9
Landshut	Alzey-W. (.461)	Vechta (.271)	Frankfurt a.M., SK (.144)	4
Passau	Hildburghausen (.223)	Sigmaringen (.203)	Merzig-Wadern (.157)	12
Regen	Südwestp. (.283)	Eifelk. Bitburg-Prüm (.163)	Hildburghausen (.161)	10
Straubing-Bogen	Alzey-W. (.5)	Potsdam-M. (.205)	Vechta (.116)	6
Dingolfing-Landau	Alzey-W. (.684)	Frankfurt a.M., SK (.181)	Trier, SK (.1)	4
Amberg	Braunschweig, SK (.356)	Vulk. (.106)	Dessau-Roßlau, SK (.103)	10
Regensburg, SK	Mainz, SK (.6)	Stuttgart, SK (.206)	Frankfurt a.M., SK (.102)	4
Weiden	Koblenz, SK (.3)	Main-Tauber-K (.16)	Neunkirchen (.118)	12
Amberg-Sulzbach	Alzey-W. (.637)	Südwestp. (.186)	Landau, SK (.111)	5
Cham	Coesfeld (.201)	Ludwig.-P. (.178)	Trier, SK (.175)	11
Neumarkt	Alzey-W. (.72)	Vechta (.117)	Trier, SK (.112)	5
Regensburg	Alzey-W. (.577)	Potsdam-M. (.15)	Frankfurt a.M., SK (.111)	6
Bamberg, SK	Braunschweig, SK (.429)	Kiel, SK (.319)	Mainz, SK (.143)	5
Bayreuth, SK	Flensburg, SK (.607)	Wilhelmsh., SK (.207)	Heidelbg., SK (.187)	3
Coburg, SK	Braunschweig, SK (.378)	Landau, SK (.206)	Suhl, SK (.192)	6
Hof, SK	Salzgitter, SK (.35)	Suhl, SK (.227)	Heidelbg., SK (.174)	8
Bamberg	Alzey-W. (.858)	Frankfurt a.M., SK (.11)	Vechta (.032)	3
Bayreuth	Oldenburg (.233)	Potsdam-M. (.202)	St. Wendel (.11)	9
Coburg	Südwestp. (.333)	Dahme-Spreewald (.212)	Alzey-W. (.151)	10
Forchheim	Alzey-W. (.739)	Olpe (.098)	Trier, SK (.044)	7

(continues)

Table A.2, continued

Bavarian district	First	Second	Third	Donors
Hof	Vogelsbergk. (.305)	Südwestp. (.276)	Uelzen (.137)	9
Kronach	Südwestp. (.459)	St. Wendel (.243)	Börde (.139)	7
Kulmbach	Merzig-Wadern (.41)	Südwestp. (.288)	Landau, SK (.118)	6
Lichtenfels	Südwestp. (.483)	Ludwig.-P. (.157)	Trier, SK (.11)	9
Ansbach, SK	Kaiserslautern (.281)	Wolfsburg, SK (.216)	Birk. (.159)	9
Erlangen	Darmstadt, SK (.661)	Stuttgart, SK (.188)	Kiel, SK (.096)	5
Fürth, SK	Frankfurt a.M., SK (.408)	Alzey-W. (.32)	Ludwig.-P. (.138)	6
Nürnberg	Frankfurt a.M., SK (.26)	Düsseldorf, SK (.144)	Pforzheim, SK (.143)	12
Schwabach	Kaiserslautern (.328)	Offenbach (.232)	Enzk. (.116)	11
Ansbach	Olpe (.232)	Biberach (.23)	Alzey-W. (.211)	9
Erlangen-H.	Gifhorn (.372)	Teltow-Fläming (.194)	Main-Taunus-K (.179)	10
Fürth	Kaiserslautern (.4)	Dahme-Spreewald (.366)	Landau, SK (.072)	8
Nürnberger Land	Enzk. (.358)	Dahme-Spreewald (.287)	Biberach (.081)	13
Neustadt a.d.Aisch	Alzey-W. (.289)	Biberach (.194)	Hildburghausen (.148)	10
Roth	Alzey-W. (.443)	Teltow-Fläming (.172)	Kaiserslautern (.15)	7
Weißenburg-G.	Biberach (.294)	Osnabrück (.204)	Birk. (.172)	8
Aschaffenburg, SK	Frankfurt a.M., SK (.248)	Südwestp. (.178)	Neunkirchen (.134)	10
Schweinfurt, SK	Salzgitter, SK (.251)	Chemnitz, SK (.235)	Flensburg, SK (.2)	7
Würzburg, SK	Heidelbg., SK (.685)	Wilhelmsh., SK (.306)	Heinsberg (.01)	3
Aschaffenburg	Alzey-W. (.435)	Südwestp. (.179)	Rhein-Neckar-K (.113)	10
Bad Kissingen	Waldeck-Frankenberg (.249)	Südwestp. (.173)	Sonneberg (.114)	12
Rhön-Grabfeld	Südwestp. (.247)	Rottweil (.244)	Olpe (.19)	9
Haßberge	Alzey-W. (.697)	Südwestp. (.165)	Viersen (.065)	5
Kitzingen	Alzey-W. (.289)	Eifelk. Bitburg-Prüm (.195)	Südwestp. (.183)	10
Miltenberg	Enzk. (.172)	Olpe (.169)	Neckar-Odenwald-K (.142)	13
Main-Spessart	Südwestp. (.443)	Eifelk. Bitburg-Prüm (.136)	Dahme-Spreewald (.093)	10
Schweinfurt	Alzey-W. (.233)	Eifelk. Bitburg-Prüm (.194)	Südwestp. (.173)	8
Würzburg	Coesfeld (.287)	Kaiserslautern (.196)	Südwestp. (.138)	11
Augsburg, SK	Mainz, SK (.345)	Frankfurt a.M., SK (.187)	Suhl, SK (.112)	10
Kaufbeuren	Pforzheim, SK (.396)	Birk. (.135)	Suhl, SK (.114)	11
Kempten (Allgäu)	Koblenz, SK (.582)	Pforzheim, SK (.178)	Chemnitz, SK (.076)	9
Memmingen	Rottweil (.291)	Heilbronn, SK (.151)	Braunschweig, SK (.146)	11
Aichach-Friedberg	Gifhorn (.242)	Vechta (.239)	Teltow-Fläming (.218)	8
Augsburg	Dahme-Spreewald (.247)	Biberach (.188)	Kaiserslautern (.185)	11
Dillingen a.d.Donau	Olpe (.345)	Biberach (.258)	Vechta (.125)	8
Günzburg	Olpe (.578)	Frankfurt a.M., SK (.121)	Vechta (.088)	7
Neu-Ulm	Alb-Donau-K (.302)	G.-Gerau (.191)	Sonneberg (.139)	8
Lindau (Bodensee)	Bodenseek. (.339)	Konstanz (.126)	Bergstraße (.107)	12
Ostallgäu	Biberach (.473)	Breisgau-H. (.234)	Kaiserslautern (.115)	9
Unterallgäu	Biberach (.457)	Ahrweiler (.156)	Konstanz (.123)	9
Donau-Ries	Biberach (.272)	Marburg-Biedenkopf (.27)	Potsdam-M. (.214)	9
Oberallgäu	Ahrweiler (.377)	Kaiserslautern (.136)	Biberach (.129)	8

Note: The table reports up to three of the biggest non-Bavarian districts (column two until four), receiving the biggest weights for each treated Bavarian district (column one) for the SCM of the COVID-19 cases per 100,000 residents. Column five includes the overall number districts receiving a weight bigger than 0.

Table A.3: Federal states with the highest number of districts among the top three donors

SH	NS	NW	HE	RP	BW	SL	BB	MV	SN	SA	TH
4	35	18	29	74	53	7	19	3	5	2	12

Note: Table includes the number of districts for specific German federal states (BW for Baden-Württemberg, BY for Bavaria, BE for Berlin, BB for Brandenburg, HB for Bremen, HH for Hamburg, HE for Hesse, NS for Lower Saxony, MV for Mecklenburg-Vorpommern, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt, SH for Schleswig-Holstein, and TH for Thuringia) contributing to the three largest donors in the baseline specification for cases per 100,000 residents. Federal states without districts ranked among the top three donors are omitted.

Table A.4: Weights for deaths

Bavarian district	First	Second	Thrid	Donors
Ingolstadt	Frankfurt a.M., SK (.42)	Wolfsburg, SK (.303)	Biberach (.113)	7
München, SK	Stuttgart, SK (.542)	Frankfurt a.M., SK (.334)	Leipzig, SK (.125)	3
Altötting	Biberach (.575)	Birk. (.182)	Suhl, SK (.144)	7
Bercht. Land	Schwarzwald-Baar-K (.501)	Suhl, SK (.165)	Leipzig, SK (.151)	7
Bad Tölz-W.	Biberach (.274)	Konstanz (.19)	Böblingen (.188)	9
Dachau	Teltow-Fläming (.374)	Vechta (.347)	Frankfurt a.M., SK (.237)	5
Ebersberg	Vechta (.452)	Potsdam-M. (.251)	Potsdam, SK (.173)	5
Eichstätt	Vechta (.56)	Teltow-Fläming (.193)	Frankfurt a.M., SK (.152)	5
Erding	Vechta (.415)	Alzey-W. (.264)	Frankfurt a.M., SK (.218)	4
Freising	Vechta (.453)	Frankfurt a.M., SK (.382)	Heinsberg (.111)	5
Fürstenfeldbruck	Main-Taunus-K (.387)	Alb-Donau-K (.184)	Dresden, SK (.145)	9
Garmisch	Schwarzwald-Baar-K (.324)	Ahrweiler (.293)	Suhl, SK (.223)	8
Landsberg am Lech	Vechta (.223)	Potsdam-M. (.217)	Oldenburg (.172)	8
Miesbach	Bodenseek. (.508)	Hohenlohek. (.177)	Dahme-Spreewald (.161)	8
München	Hochtaunusk. (.358)	Vechta (.235)	Potsdam, SK (.199)	5
Neuburg-S.	Biberach (.354)	Olpe (.153)	Teltow-Fläming (.105)	8
Pfaffenhofen	Alzey-W. (.423)	Frankfurt a.M., SK (.29)	Vechta (.185)	4
Starnberg	Hochtaunusk. (.437)	Lippe (.422)	Heinsberg (.076)	5
Traunstein	Schwarzwald-Baar-K (.446)	Rottweil (.24)	Sonneberg (.062)	10
Weilheim-Schongau	Rottweil (.227)	Osterholz (.184)	Bodenseek. (.166)	11
Landshut, SK	Braunschweig, SK (.165)	Heilbronn, SK (.165)	Frankfurt a.M., SK (.164)	11
Passau, SK	Heidelbg., SK (.535)	Wilhelmsh., SK (.462)	Suhl, SK (.003)	3
Deggendorf	Trier, SK (.277)	Merzig-Wadern (.266)	Olpe (.206)	7
Freyung-Grafenau	Alzey-W. (.397)	St. Wendel (.396)	Landau, SK (.162)	5
Kelheim	Vechta (.277)	Alzey-W. (.22)	Potsdam-M. (.178)	9
Landshut	Alzey-W. (.46)	Vechta (.271)	Frankfurt a.M., SK (.144)	4
Passau	Merzig-Wadern (.258)	Alb-Donau-K (.209)	Sonneberg (.111)	10
Regen	Südwestp. (.237)	Cochem-Zell (.214)	Hohenlohek. (.141)	10
Straubing-Bogen	Alzey-W. (.539)	Potsdam-M. (.192)	Vechta (.11)	6

(continues)

Table A.4, continued

Bavarian district	First	Second	Thrid	Donors
Dingolfing-Landau	Alzey-W. (.687)	Frankfurt a.M., SK (.18)	Trier, SK (.101)	5
Amberg	Braunschweig, SK (.446)	Ostholstein (.222)	Neustadt a.d.W., SK (.121)	8
Regensburg, SK	Mainz, SK (.586)	Stuttgart, SK (.222)	Heidelbg., SK (.101)	4
Weiden	Koblenz, SK (.27)	Baden-B., SK (.263)	Main-Tauber-K (.119)	10
Amberg-Sulzbach	Alzey-W. (.642)	Südwestp. (.191)	Landau, SK (.112)	5
Cham	Alzey-W. (.198)	Südwestp. (.193)	Hohenlohek. (.171)	9
Neumarkt	Alzey-W. (.734)	Münster, SK (.116)	Vechta (.091)	4
Regensburg	Alzey-W. (.581)	Potsdam-M. (.146)	Vechta (.111)	7
Bamberg, SK	Braunschweig, SK (.397)	Kiel, SK (.363)	Stuttgart, SK (.137)	4
Bayreuth, SK	Flensburg, SK (.572)	Wilhelmsh., SK (.222)	Heidelbg., SK (.206)	3
Coburg, SK	Braunschweig, SK (.282)	Suhl, SK (.2)	Südwestp. (.197)	7
Hof, SK	Suhl, SK (.252)	Salzgitter, SK (.222)	Flensburg, SK (.215)	9
Bamberg	Alzey-W. (.859)	Frankfurt a.M., SK (.11)	Vechta (.031)	3
Bayreuth	Merzig-Wadern (.323)	Oldenburg (.277)	Hildburghausen (.166)	8
Coburg	Südwestp. (.332)	Dahme-Spreewald (.234)	Alzey-W. (.143)	10
Forchheim	Alzey-W. (.81)	Merzig-Wadern (.073)	Südwestp. (.039)	7
Hof	Südwestp. (.289)	Vogelsbergk. (.283)	Ostholstein (.166)	8
Kronach	St. Wendel (.651)	Ludwig.-P. (.122)	Landau, SK (.071)	7
Kulmbach	Merzig-Wadern (.383)	Südwestp. (.19)	Landau, SK (.129)	6
Lichtenfels	Südwestp. (.512)	Alzey-W. (.115)	Trier, SK (.084)	10
Ansbach, SK	Kaiserslautern (.357)	Wolfsburg, SK (.285)	Darmstadt, SK (.108)	8
Erlangen	Darmstadt, SK (.655)	Stuttgart, SK (.187)	Kiel, SK (.104)	5
Fürth, SK	Frankfurt a.M., SK (.408)	Alzey-W. (.32)	Ludwig.-P. (.14)	5
Nürnberg	Pforzheim, SK (.221)	Düsseldorf, SK (.184)	Braunschweig, SK (.171)	10
Schwabach	Kaiserslautern (.346)	Offenbach (.165)	Böblingen (.155)	9
Ansbach	Alzey-W. (.375)	Biberach (.295)	Landau, SK (.107)	10
Erlangen-H.	Biberach (.233)	Dahme-Spreewald (.22)	Gifhorn (.169)	9
Fürth	Kaiserslautern (.442)	Dahme-Spreewald (.372)	Suhl, SK (.051)	7
Nürnberger Land	Enzk. (.539)	Dahme-Spreewald (.267)	Schmalkalden-M. (.071)	9
Neustadt a.d.Aisch	Alzey-W. (.346)	Enzk. (.244)	Hildburghausen (.128)	8
Roth	Alzey-W. (.476)	Dahme-Spreewald (.133)	Landau, SK (.101)	8
Weißenburg-G.	Enzk. (.261)	Biberach (.236)	Birk. (.167)	9
Aschaffenburg, SK	Frankfurt a.M., SK (.257)	Neunkirchen (.203)	Eifelk. Bitburg-Prüm (.121)	9
Schweinfurt, SK	Chemnitz, SK (.26)	Salzgitter, SK (.224)	Flensburg, SK (.2)	7
Würzburg, SK	Heidelbg., SK (.7)	Wilhelmsh., SK (.3)	-	2
Aschaffenburg	Alzey-W. (.383)	Eifelk. Bitburg-Prüm (.168)	St. Wendel (.131)	12
Bad Kissingen	Rottweil (.196)	Südwestp. (.193)	Höxter (.134)	8
Rhön-Grabfeld	Südwestp. (.359)	Rottweil (.268)	Grafs.-Bentheim (.109)	9
Haßberge	Alzey-W. (.709)	Südwestp. (.173)	Landau, SK (.064)	5
Kitzingen	Alzey-W. (.322)	Eifelk. Bitburg-Prüm (.241)	Südwestp. (.153)	8
Miltenberg	Olpe (.21)	Enzk. (.172)	Neckar-Odenwald-K (.167)	11
Main-Spessart	Südwestp. (.448)	Eifelk. Bitburg-Prüm (.135)	Coesfeld (.084)	9
Schweinfurt	Eifelk. Bitburg-Prüm (.363)	Kaiserslautern (.216)	Südwestp. (.194)	7
Würzburg	Dahme-Spreewald (.228)	Coesfeld (.221)	Eifelk. Bitburg-Prüm (.166)	8
Augsburg, SK	Mainz, SK (.39)	Frankfurt a.M., SK (.185)	Leipzig, SK (.09)	11

(continues)

Table A.4, continued

Bavarian district	First	Second	Thrid	Donors
Kaufbeuren	Pforzheim, SK (.424)	Biberach (.126)	Ahrweiler (.126)	8
Kempten (Allgäu)	Koblenz, SK (.515)	Frankenthal (Pfalz), SK (.158)	Leipzig, SK (.109)	8
Memmingen	Rottweil (.275)	Braunschweig, SK (.201)	Waldshut (.141)	9
Aichach-Friedberg	Gifhorn (.413)	Vechta (.195)	Teltow-Fläming (.18)	8
Augsburg	Biberach (.298)	Teltow-Fläming (.224)	Kaiserslautern (.213)	9
Dillingen a.d.Donau	Olpe (.378)	Biberach (.299)	Enzk. (.148)	8
Günzburg	Olpe (.367)	Alzey-W. (.244)	Biberach (.163)	8
Neu-Ulm	Biberach (.363)	Alb-Donau-K (.18)	Heidelbg., SK (.151)	10
Lindau (Bodensee)	Ravensburg (.36)	Bodenseek. (.119)	Suhl, SK (.103)	12
Ostallgäu	Biberach (.449)	Ahrweiler (.16)	Breisgau-H. (.128)	9
Unterallgäu	Biberach (.418)	Ahrweiler (.226)	Konstanz (.191)	9
Donau-Ries	Emsland (.289)	Potsdam-M. (.224)	Biberach (.195)	8
Oberallgäu	Ahrweiler (.432)	Kaiserslautern (.182)	Böblingen (.104)	10

Note: The table reports up to three of the biggest non-Bavarian districts (column two until four), receiving the biggest weights for each treated Bavarian district (column one) for the SCM of the COVID-19 deaths per 100,000 residents. Column five includes the overall number districts receiving a weight bigger than 0.

Table A.5: Federal states with the highest number of districts among the top three donors

SH	NS	NW	HE	RP	BW	SL	BE	BB	MV	SN	SA	TH
6	33	17	25	83	45	9	2	20	6	5	1	7

Note: Table includes the number of districts for specific German federal states (BW for Baden-Württemberg, BY for Bavaria, BE for Berlin, BB for Brandenburg, HB for Bremen, HH for Hamburg, HE for Hesse, NS for Lower Saxony, MV for Mecklenburg-Vorpommern, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt, SH for Schleswig-Holstein, and TH for Thuringia) contributing to the three largest donors in the baseline specification for deaths per 100,000 residents. Federal states without districts ranked among the top three donors are omitted.

Figure A.1: Development COVID-19 Cases All Bavarian districts

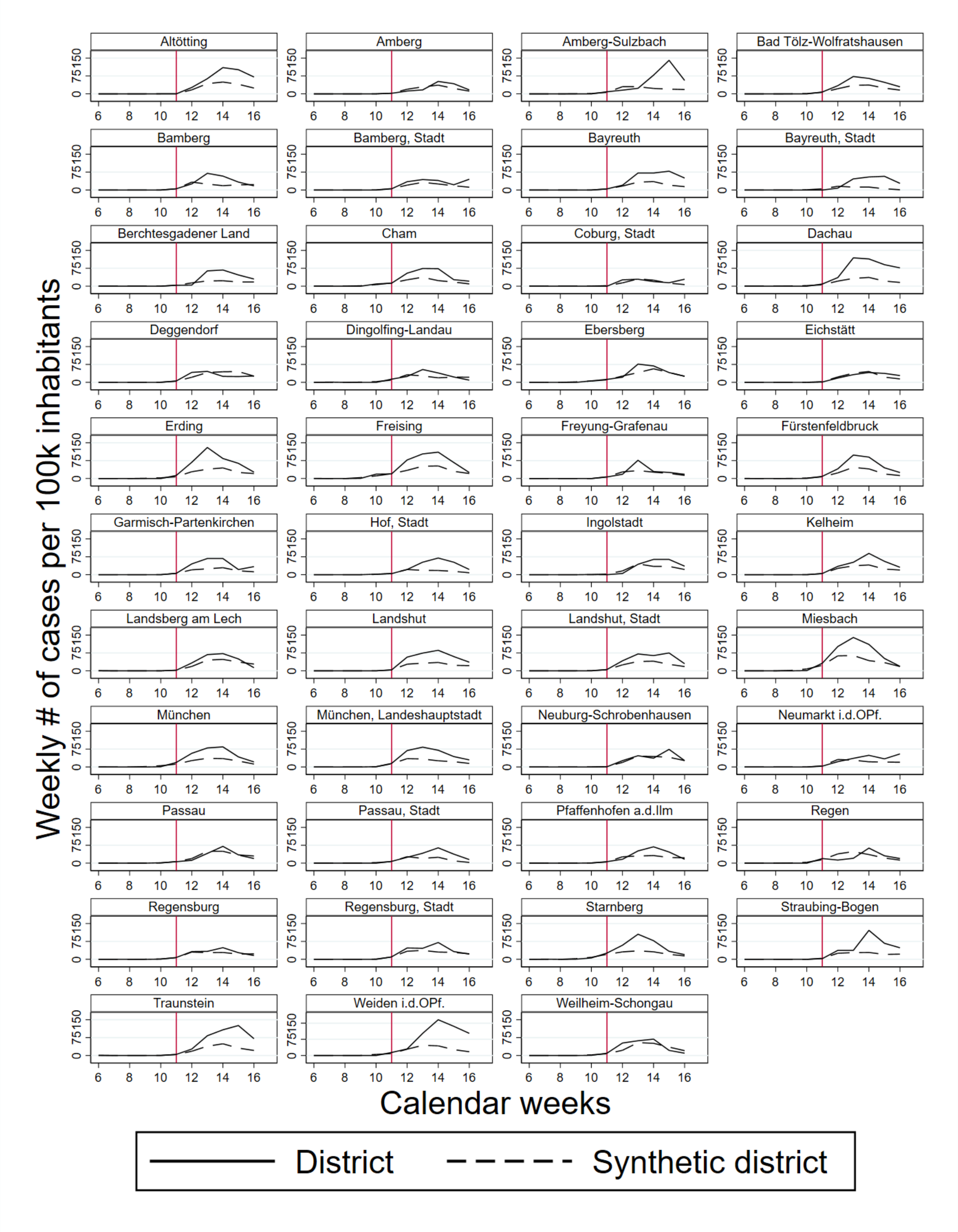
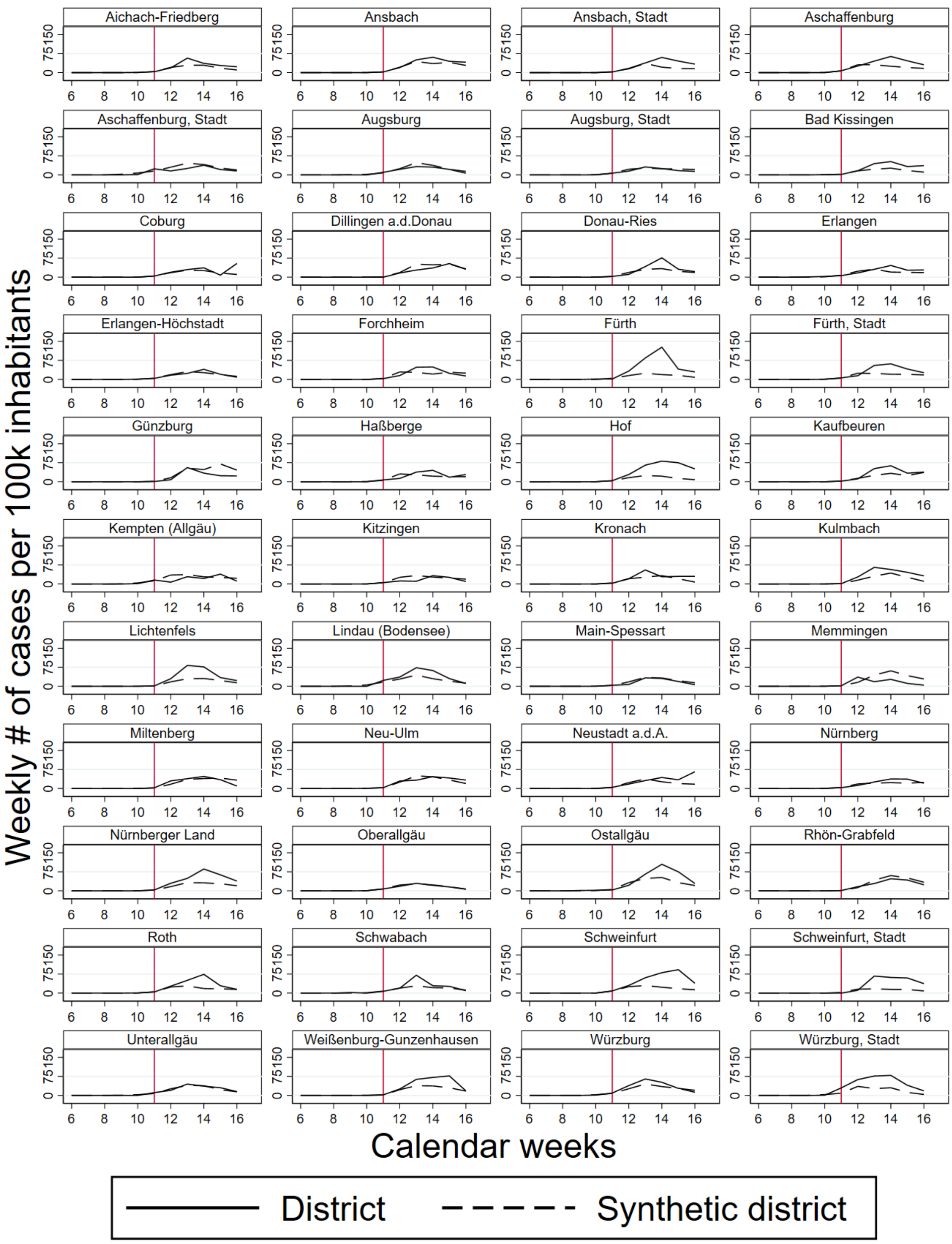
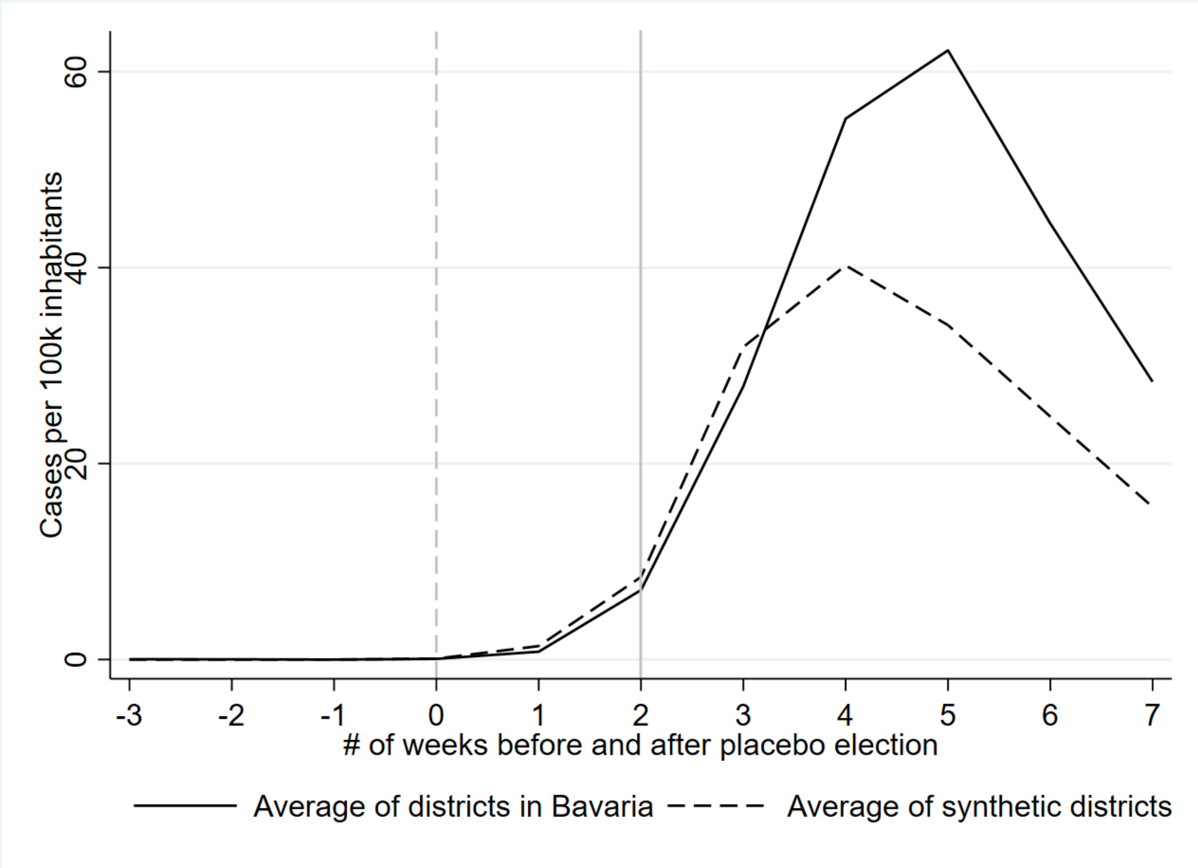


Figure A.1 continued



Notes: Graphs show the development of COVID-19 cases per week per 100,000 residents (black line) for each of the 87 Bavarian districts considered together with its synthetic counterpart (dashed line).

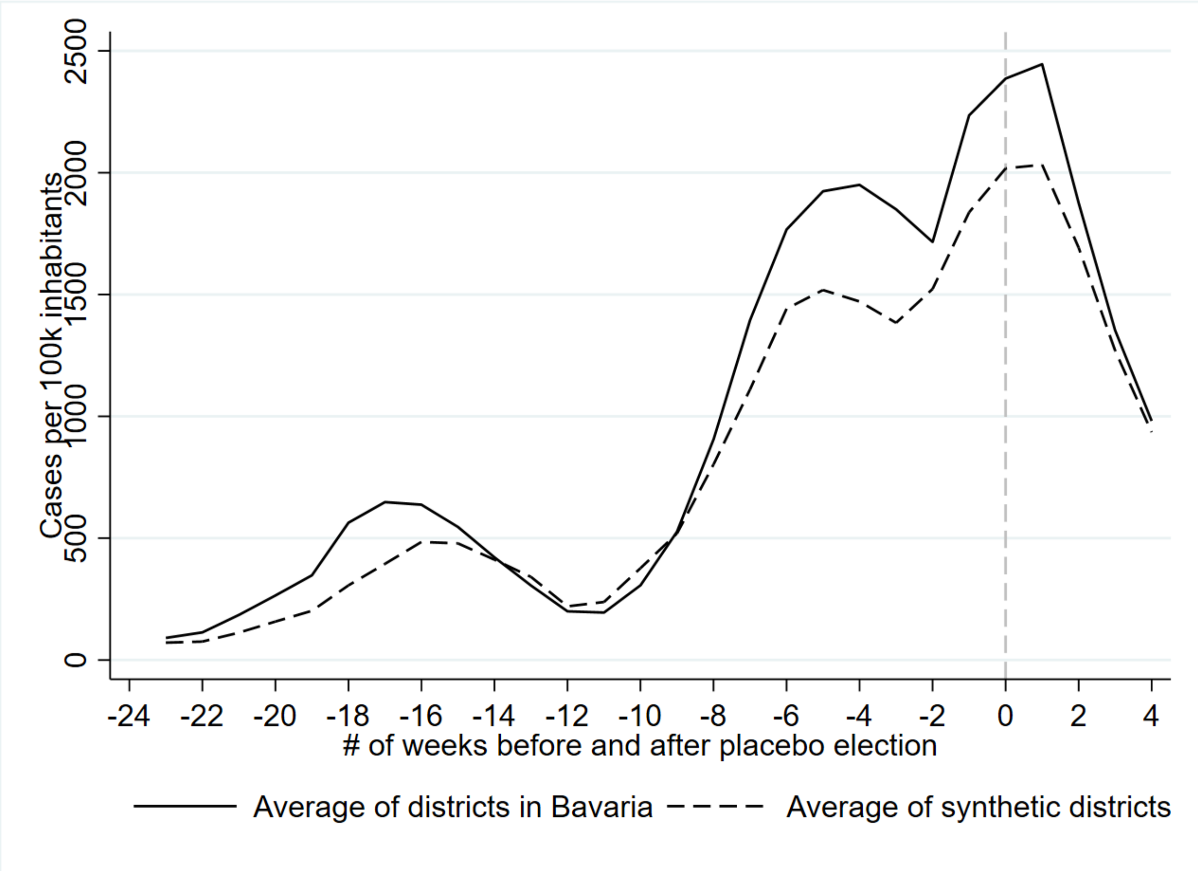
Figure A.2: Placebo-in-time for a placebo election two weeks prior - cases



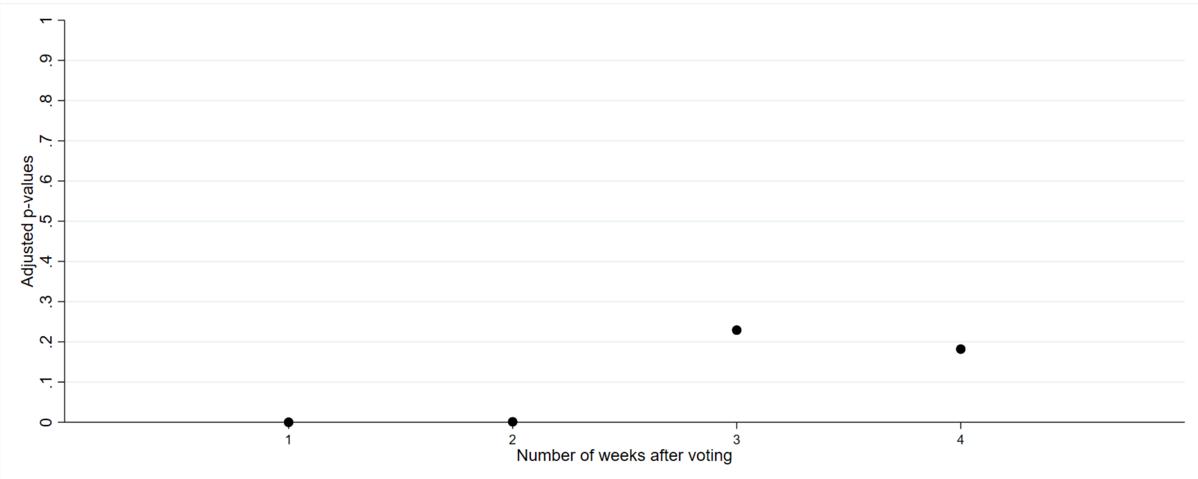
Notes: Graph shows the development of COVID-19 cases per week per 100,000 residents (black line) for each of the 87 Bavarian districts considered together with its synthetic counterpart (dashed line) - assuming a election was held two weeks earlier.

Figure A.3: Placebo election in March 2022

a) Development COVID-19 cases



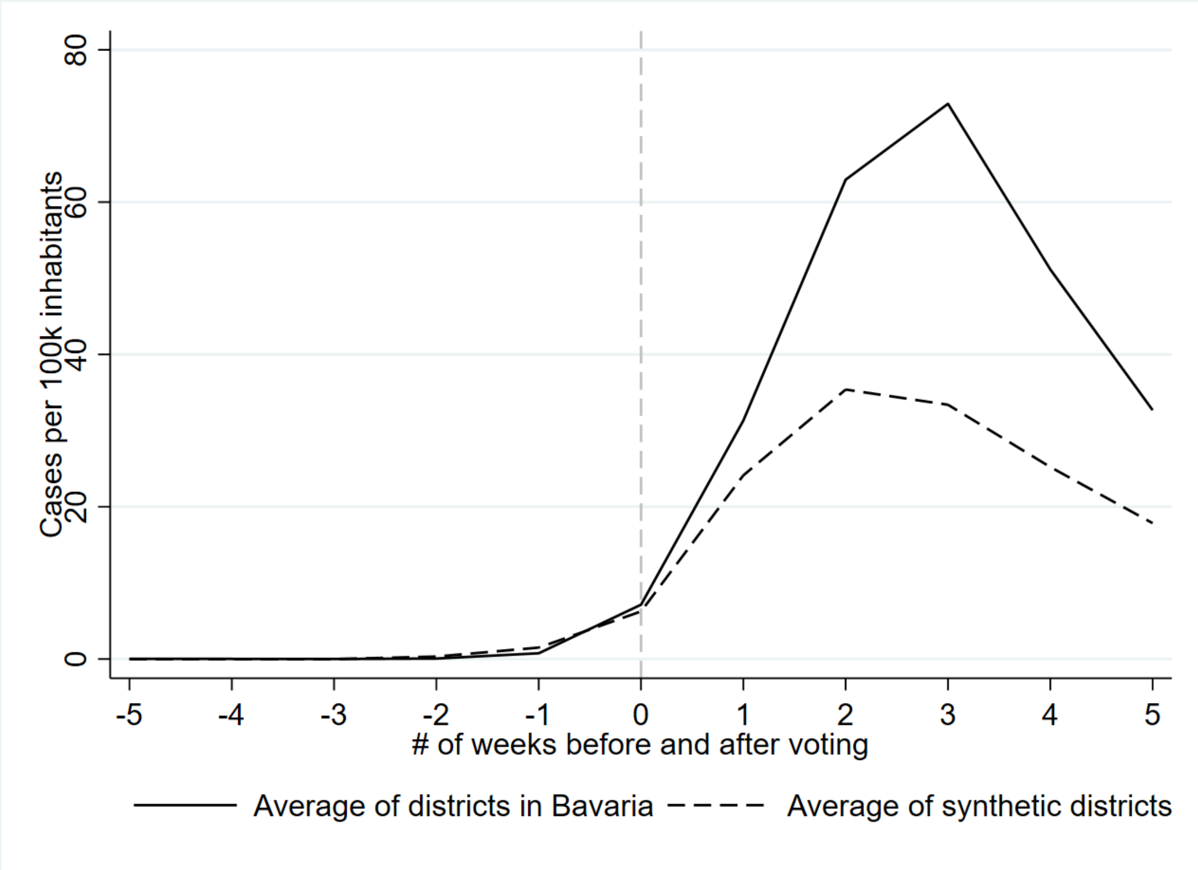
b) Adjusted p-values



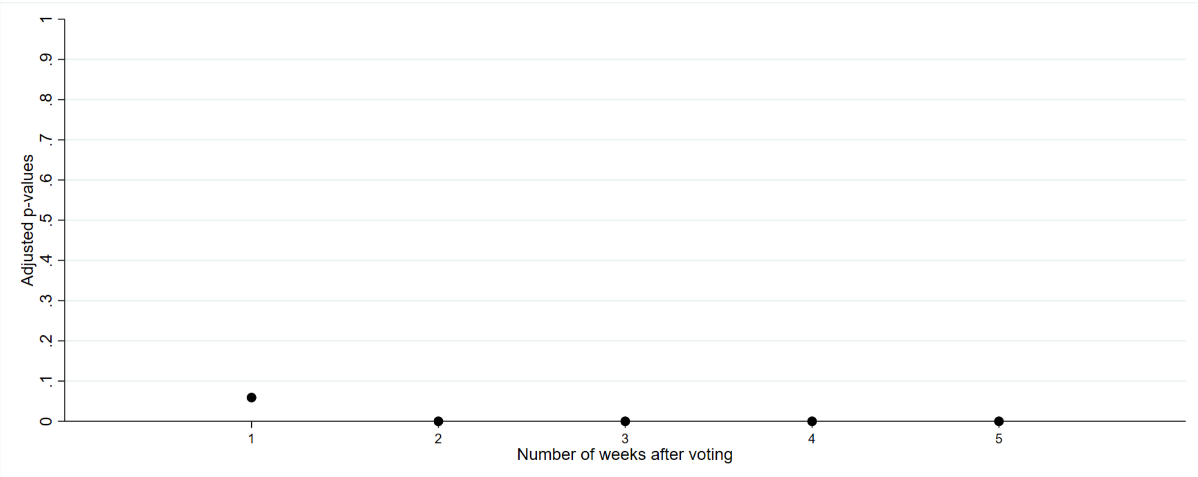
Notes: Panel (a) displays the average weekly development of COVID-19 cases per 100,000 residents (solid black line) across 87 Bavarian districts, along with its synthetic counterpart (dashed line) for a placebo election conducted in mid-March 2022. Panel (b) shows the corresponding adjusted p-values for each week following the placebo election.

Figure A.4: Sensitivity analysis including districts with strong-beer festivals – cases

a) Development COVID-19 cases



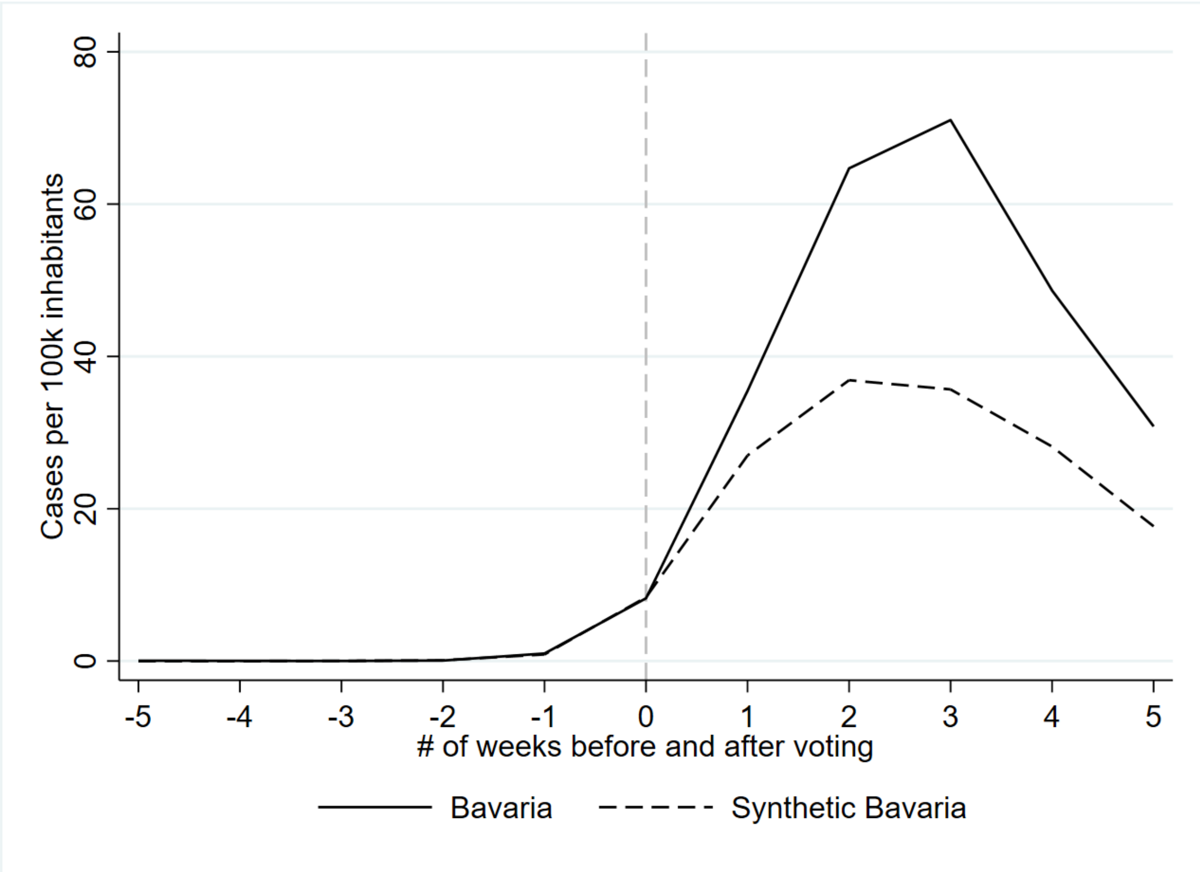
b) Adjusted p-values



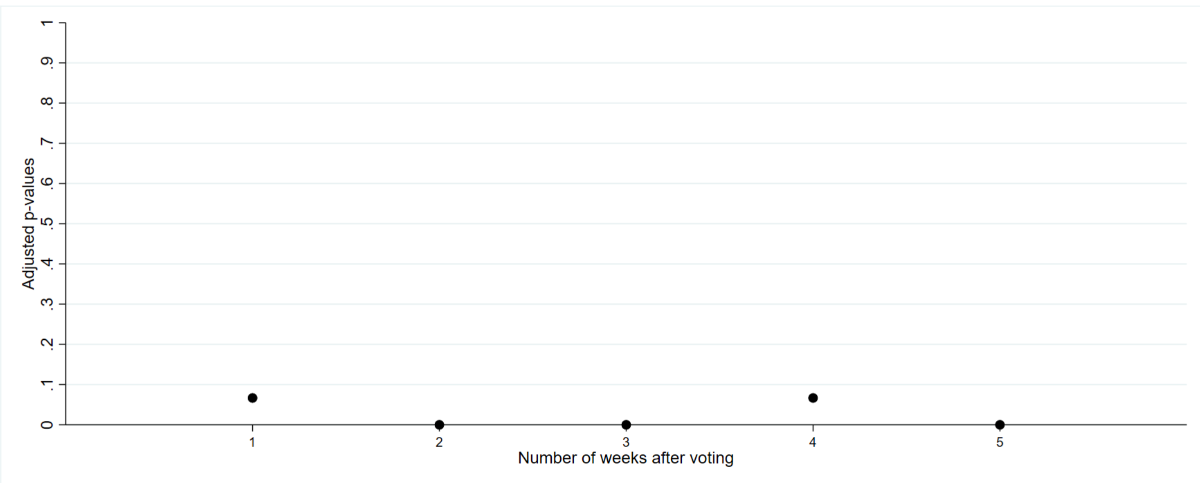
Notes: Graph in Panel a shows the average development of COVID-19 cases per week per 100,000 residents (black line) for all 96 Bavarian districts (including those who held strong-beer festivals close to the election date) together with its synthetic counterpart (dashed line). Panel b plots the adjusted p-values for every week after the election.

Figure A.5: SCM for federal states - cases

a) Development COVID-19 cases - Bavaria and Synthetic Bavaria

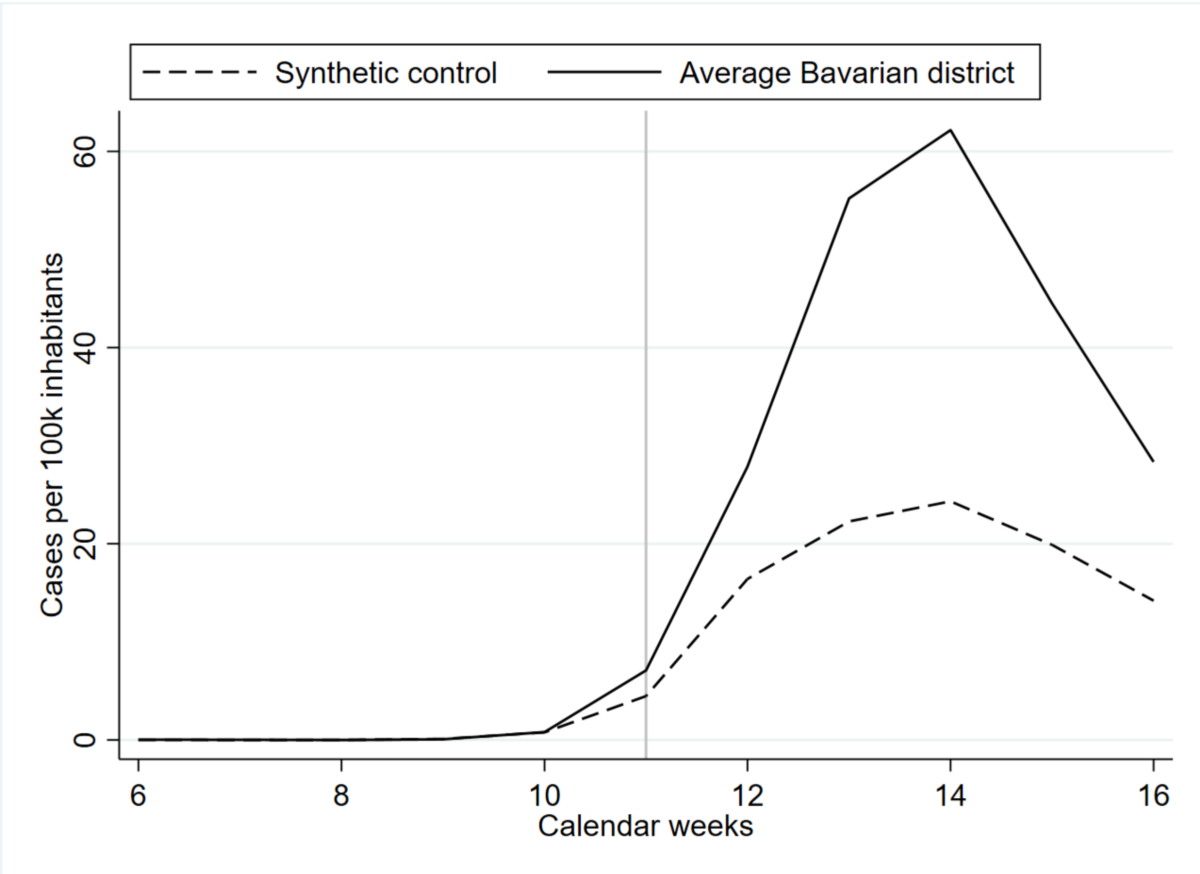


b) Adjusted p-values



Notes: Graph in Panel a shows the development of COVID-19 cases per week per 100,000 residents (black line) for Bavaria together with its synthetic counterpart (dashed line). Panel b plots the adjusted p-values for every week after the election.

Figure A.6: SDiD - cases



Notes: Graph shows the average development of COVID-19 cases per week per 100,000 residents (black line) across 87 Bavarian districts together with its synthetic counterpart (dashed line).

Figure A.7: Development COVID-19 Deaths All Bavarian districts

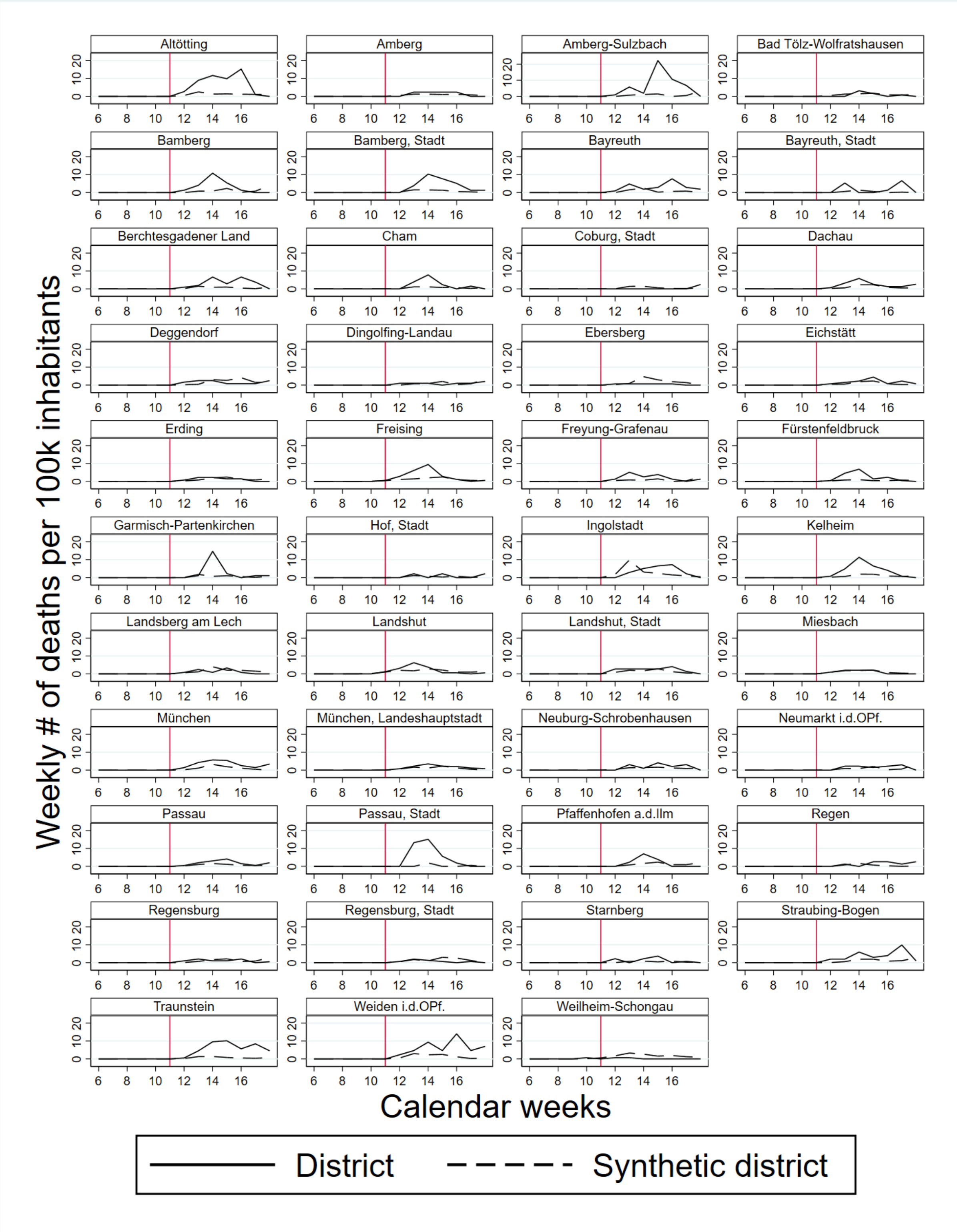
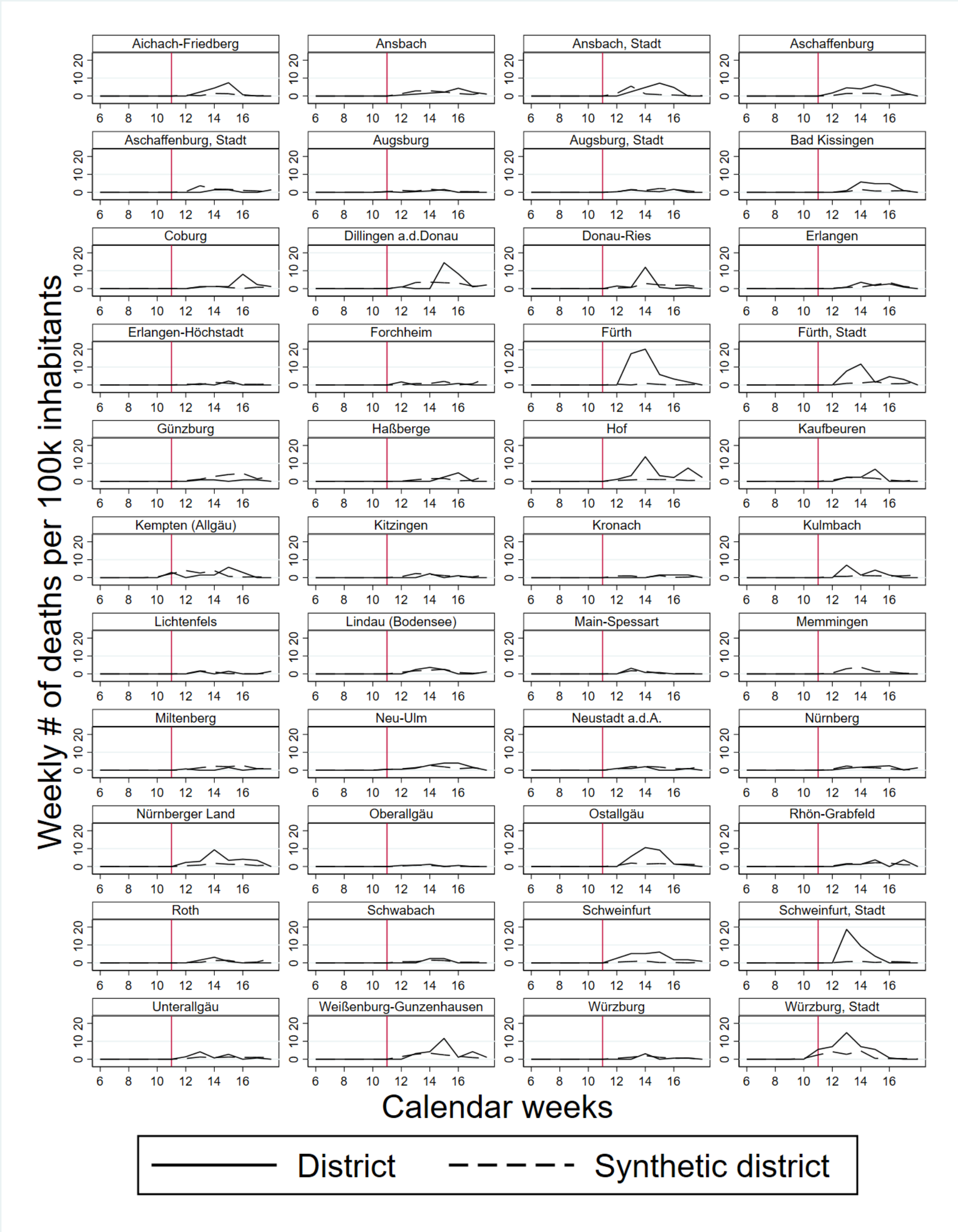


Figure A.7 continued



Notes: Graphs show the development of COVID-19 deaths per week per 100,000 residents (black line) for each of the 87 Bavarian districts considered together with its synthetic counterpart (dashed line).

Table A.6: Regression results for voter participation in run-off elections on March 29

	Cases per 100k	Deaths per 100k
$\Delta Cases_{\text{March 29- March 1}}$	1.21***	0.14***
	(0.13)	(0.04)
Voters / pop.	-0.76*	-0.083*
	(0.38)	(0.05)
Working from home	18.2*	-0.77
	(9.12)	(1.32)
Distance from Ischgl	5.25	2.70
	(17.53)	(3.34)
Beer festival	21.7	1.74
	(14.14)	(2.41)
N	84	84
adj. R^2	0.712	0.603
Demographic	Y	Y
Economic	Y	Y
H&C care	Y	Y
District type dummies	Y	Y

Notes: Coefficient estimates with robust standard errors in parentheses. ***/**/* indicates statistical significance at the 1/5/10% level. $Cases per 100k$ denotes the number of COVID-19 infections. $Deaths per 100k$ denotes the number of COVID-19 deaths. *Beer festival* measures the number of confirmed strong-beer festivals held in a given district in March 2020 (see Table A.1). *Voter participation* denotes the number of voters in the Bavarian run-off elections on March 29, 2020 relative to the population. *Demographic*, *Economic*, and *H&C care* denote demographic, economic, health and child care controls at the district level. *District type dummies* denotes administrative and structural classifications by the *Bundesinstitut fuer Bau-, Stadt- und Raumforschung* (BBSR). The complete set of control variables is listed in Table 1.

Table A.7: Regression results for increase in COVID-19 infections by age group

	A00-A04	A05-A14	A15-A34	A35-A59	A60-A79	A80+
$\Delta Cases_{\text{March 15- March 1}}$	-1.72*	-0.47***	3.17***	1.86	7.56**	-0.33**
	(0.99)	(0.14)	(0.94)	(1.54)	(3.03)	(0.14)
$Voters\ /\ pop.$	0.21	-0.058	2.38*	6.25**	1.50	-0.20
	(0.13)	(0.17)	(1.26)	(2.61)	(0.92)	(0.47)
$Working\ from\ home$	0.22	0.38	-0.65	3.16	1.42	-0.063
	(0.19)	(0.38)	(1.95)	(3.95)	(1.88)	(0.72)
$Distance\ from\ Ischgl$	-1.30	1.30	-8.41*	-9.28	4.18	2.03
	(0.91)	(1.08)	(5.00)	(10.84)	(5.98)	(1.67)
$Beer\ festival$	-0.49	0.13	20.9***	47.8***	21.2***	1.81
	(0.36)	(0.69)	(5.31)	(11.91)	(5.46)	(1.69)
N	96	96	96	96	96	96
adj. R^2	-0.025	0.184	0.481	0.412	0.470	0.156
Demographic	Y	Y	Y	Y	Y	Y
Economic	Y	Y	Y	Y	Y	Y
H&C care	Y	Y	Y	Y	Y	Y
District type dummies	Y	Y	Y	Y	Y	Y

Notes: Coefficient estimates with robust standard errors in parentheses. ***/**/* indicates statistical significance at the 1/5/10% level. $Cases_x$ denotes the number of COVID-19 infections up to date x. $Beer\ festival$ measures the number of confirmed strong-beer festivals held in a given district in March 2020 (see Table A.1). $Voter\ participation$ denotes the number of voters in the Bavarian municipal elections on March 15, 2020 relative to the population. *Demographic*, *Economic*, and *H&C care* denote demographic, economic, health and child care controls at the district level. *District type dummies* denotes administrative and structural classifications by the *Bundesinstitut fuer Bau-, Stadt- und Raumforschung* (BBSR). The complete set of control variables is listed in Table 1.

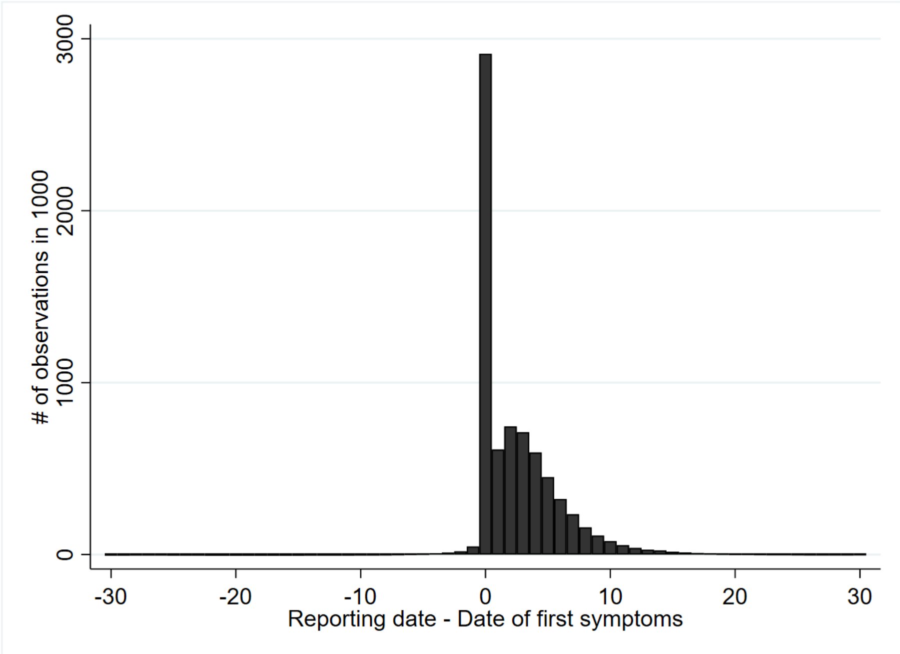
Table A.8: Regression results for increase in COVID-19 deaths by age group

	A15-A34	A35-A59	A60-A79	A80+
$\Delta cases_{\text{March 15- March 1}}$	0.00038	0.013	-0.46	0.32
	(0.00)	(0.02)	(0.43)	(0.27)
Voters / pop.	0.000073	-0.0081	0.032	1.01**
	(0.00)	(0.02)	(0.13)	(0.43)
Working from home	-0.00020	-0.012	-0.22	-0.089
	(0.00)	(0.04)	(0.28)	(0.82)
Distance from Ischgl	0.0012	0.031	-0.083	-2.28
	(0.00)	(0.12)	(0.77)	(1.81)
Beer festival	-0.00086	-0.27**	0.97*	6.84***
	(0.00)	(0.10)	(0.50)	(2.08)
N	96	96	96	96
adj. R^2	0.572	0.051	-0.102	0.407
Demographic	Y	Y	Y	Y
Economic	Y	Y	Y	Y
H&C care	Y	Y	Y	Y
District type dummies	Y	Y	Y	Y

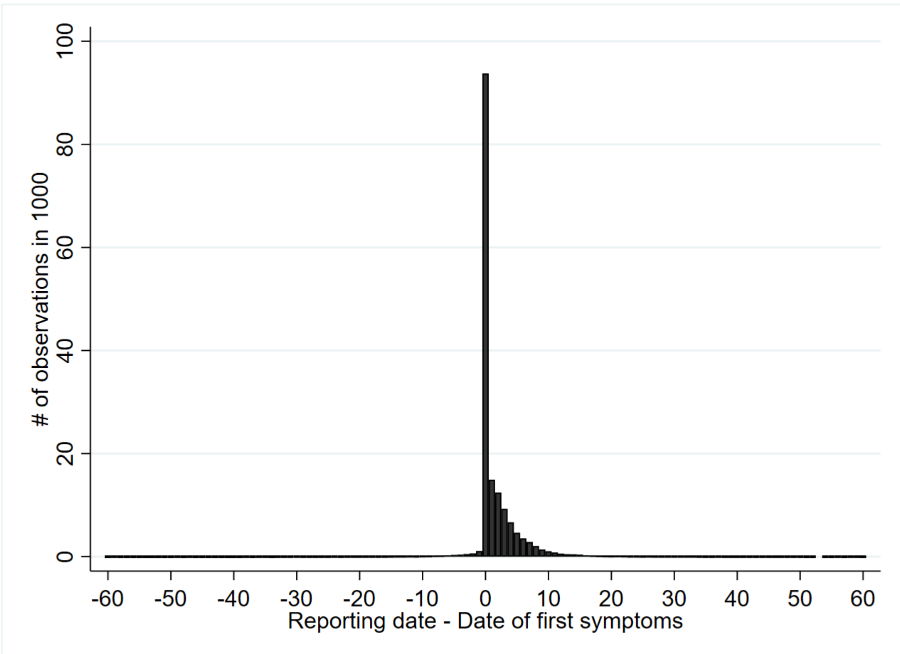
Notes: Coefficient estimates with robust standard errors in parentheses. ***/**/* indicates statistical significance at the 1/5/10% level. $cases_x$ denotes the number of COVID-19 infections up to date x. *Beer festival* measures the number of confirmed strong-beer festivals held in a given district in March 2020 (see Table A.1). *Voter participation* denotes the number of voters in the Bavarian municipal elections on March 15, 2020 relative to the population. *Demographic*, *Economic*, and *H&C care* denote demographic, economic, health and child care controls at the district level. *District type dummies* denotes administrative and structural classifications by the *Bundesinstitut fuer Bau-, Stadt- und Raumforschung* (BBSR). The complete set of control variables is listed in Table 1.

Figure A.8: Delay between date of first symptoms and reporting date

(a) Cases



(b) Deaths



Epidemics at the Polls? The Role of Election Dates in the Spread of Respiratory Tract Infections

Gerrit Stahn

Abstract

This study examines whether regional elections facilitate the spread of respiratory infections. Mainly building on previous research conducted in the context of the COVID-19 pandemic, we explore whether similar effects can be observed for other acute respiratory infections in non-pandemic periods. The analysis uses the number of sick leaves per 1,000 insured individuals as the outcome variable, leveraging data from the German health insurance provider Barmer. By applying the Synthetic Control Method, we investigate potential effects of federal elections held in the German states of Bavaria, Hesse, and Thuringia. While we find no evidence of virus transmission for the 2018 Bavarian election, the 2018 Hessian election and the 2019 Thuringian election indicate a significant impact on respiratory infection rates.

Key words: Elections, respiratory tract infections, sick leave

JEL classification: H11, I12, I18

3.1 Introduction

Respiratory tract viruses and their associated diseases account for a significant share of global health care expenditures. Substantial evidence is available for Influenza, its most prominent representative, highlighting both direct costs (e.g., outpatient services, hospitalizations and medications (Federici et al., 2018)) and indirect costs (e.g., productivity losses resulting from sick leave (de Courville et al., 2022)). Consequently, a considerable body of literature seeks to identify social events that may drive the spread of such viruses. Elections, even at the regional level, can be seen as potential contributors to this spread, as they often involve large gatherings of people on a single day to cast their votes, leading to close contact with other voters or election helpers. During periods of heightened viral activity, voting may entail considerable risks, potentially creating a tension between exercising the right to vote and maintaining a healthy life.

This study investigates the effect of federal elections on the spread of respiratory tract infections using the Synthetic Control Method (SCM). The analysis focuses on the state elections held in October 2018 in Bavaria and Hesse, as well as the October 2019 state election in Thuringia. The outcome variable is the number of sick leaves due to respiratory infections among individuals insured by the German health insurance provider Barmer. By constructing synthetic controls based on a wide range of federal-level demographic, economic, healthcare, and childcare characteristics, we find mixed evidence of a significant rise in sick leaves in the weeks following these elections across the three states and two Influenza seasons.

The literature presents a broad range of estimates regarding the economic burden of Influenza.[1] A study by Ozawa et al. (2016), based on a sample of the U.S. population in 2015, estimate the total economic burden of Influenza at approximately \$15.35 billion.[2] Gil-de-Miguel et al. (2022) estimate the total cost of Influenza for the adult population (aged 18 and older) in Spain during the 2017/2018 flu season at approximately \$4.20 billion[3], highlighting the substantial financial impact of the disease. For Germany, Haas et al. (2016) analyze claims data from the Health Risk Institute, which includes records from around 80 German health insurance providers, and estimate the total cost of Influenza

[1] All costs mentioned in this section are adjusted to 2024 dollars.
[2] Equivalent to 0.3% of total U.S. health care spending in 2024 (Source: Own calculation).
[3] Equivalent to 0.2% of total health care spending in Spain in 2024 (Source: Own calculation).

during the 2012/13 season at \$501.42 million (≈ €464.28 million).[4] Further insights come from Ehlken et al. (2015), who use longitudinal patient-level data from electronic medical records of office-based physicians in Germany (covering the period from May 2010 to April 2012) to estimate an average cost per Influenza case of \$713.74 (≈ €660.87) based on 21,039 influenza-attributable cases.

The overall cost of Influenza comprises multiple contributing factors. For instance, Gasparini et al. (2012) emphasize the substantial societal and economic costs of Influenza, which include healthcare expenditures, lost productivity, and the strain on medical resources. The authors highlight that seasonal Influenza epidemics, though generally less severe than pandemics, still result in significant costs due to hospitalizations, outpatient visits, and missed workdays. In a systematic review of the economic burden of seasonal Influenza in high-income countries, Federici et al. (2018) analyze 27 studies published between January 2000 and December 2016. The review reveals a wide range of cost estimates per Influenza case, with inpatient services comprising a significant portion of these costs, followed by outpatient services and medications. More recent findings by de Courville et al. (2022) indicate that, among individuals aged 18 to 64, up to 88% of the economic burden of Influenza arises from indirect costs, while hospitalizations account for up to 75% of total direct costs. Furthermore, influenza-related expenses in this demographic tend to increase with age and the presence of underlying medical conditions. Complementing these findings, Villani et al. (2022) review the costs of Influenza in children, highlighting substantial variations in estimates due to differences in healthcare systems, study designs, and age groups. Their study underscores the significant financial strain that Influenza imposes on healthcare systems and families, particularly for children under five years of age, who represent the highest cost group.

These cost estimates underscore the importance of understanding the factors that drive the transmission of respiratory diseases. Therefore, a specific strain of the literature focuses on the role of social gatherings in that context. For example, the paper by Al-Tawfiq et al. (2016) concerns the Hajj pilgrimage, which is the largest recurring mass gathering in the world. The authors highlight that respiratory infections, including Influenza, are the most common illnesses spread during such gatherings. Crowded conditions, the diverse health status of attendees, and the difficulty of implementing infection

[4]Equivalent to 0.15% of total health care spending in Germany in 2013 (Source: Own calculation).

control measures contribute to the rapid transmission of viruses. Additionally the work by Rainey et al. (2016) analyzes the frequency and characteristics of mass gathering-related respiratory disease outbreaks in the U.S. from 2005 to 2014. Using a systematic literature review and data from the National Outbreak Reporting System, the authors identify 72 respiratory disease outbreaks, the majority associated with Influenza A transmitted at agricultural fairs and camps. The study concludes that while outbreaks at mass gatherings are in general relatively uncommon, large events such as agricultural fairs, where attendees have close contact with each other and with animals, pose a noticeable risk for zoonotic disease transmission.

The role of (mass) gatherings on the spread of diseases was further promoted by the recent COVID-19 pandemic. Since the start of the pandemic, a host of contributions has investigated the relationship between social factors, political measures, and the spread of COVID-19 cases and deaths. Ahammer et al. (2023) examine the impact of mass gatherings, specifically NBA and NHL games for the 2019-2020 season, on the early spread of COVID-19 in the United States, finding that each additional game increased cumulative COVID-19 deaths by 10.3% in affected counties. The study by Mangrum and Niekamp (2022) investigates how the timing of university spring breaks in the U.S. in March 2020 influenced the spread of COVID-19, revealing that counties with early breaks experienced significantly higher growth in cases and mortality due to increased student travel and subsequent secondary spread. Using smartphone mobility data, it highlights that destinations like New York City and Florida and modes of travel like air had a pronounced impact on infection rates. Furthermore, Whaley et al. (2021) observe that households in the top decile of county COVID-19 prevalence in 2020 with recent birthdays, particularly those involving children, experienced significantly higher COVID-19 diagnoses, indicating that even small gatherings posed an underestimated transmission risk.

A majority of the sparse literature on respiratory diseases and elections concerns the potential effect of pandemic events on voter turnout as well as election results. Urbatsch (2017) explores the relationship between Influenza outbreaks and voter turnout in Finland and the United States from 1995 to 2015. The results show that regions with higher local Influenza prevalence experienced lower voter turnout during elections. The article highlights that both the people feeling sick and those around them, such as caregivers or people concerned about exposure, are less likely to vote during Influenza outbreaks. The paper suggests that seasonal Influenza contributes to an underrepresentation of vulnerable populations, including the elderly and those with lower socioeconomic status, as they are more affected by illness. Additionally, Bauernschuster et al. (2023) examine the political impact of the 1918 Influenza pandemic on voting behavior in the Weimar Republic. Using constituency-level mortality data to measure the intensity of the pandemic, the authors analyze its influence on voting outcomes, particularly the shift towards left-wing parties. The study finds that constituencies more severely affected by the Influenza pandemic saw a significant increase in vote shares for left-leaning parties, particularly the Social Democratic Party (SPD). The authors argue that this shift was driven by the public's perception of the SPD's historical engagement with health issues, rather than dissatisfaction with incumbent parties. The paper underscores the long-term political consequences of pandemics and how health crises can reshape political landscapes by bringing public health into the political agenda. In the context of COVID-19, Picchio and Santolini (2022) investigate the impact of the pandemic on voter turnout in the 2020 local government elections in Italy. The study analyzes data from 702 municipalities, focusing on the elderly mortality rate as a proxy for the intensity of the pandemic's impact. The authors find that a 1% increase in elderly mortality leads to a 0.5% decrease in voter turnout. The effect is more pronounced in densely populated areas, leading to a 1.2% decrease. In contrast, Frank et al. (2020) demonstrate that for the 2020 municipal elections in Bavaria, Germany, the declaration of a state of emergency between the first and second ballots resulted in a 10 percentage point increase in voter participation compared to prior elections.

The studies concerned with the potential effect of elections on the spread of infectious diseases mainly look at voting events in the context of the COVID-19 pandemic. Cotti et al. (2021) explore the impact of in-person voting during the April 7, 2020, Wisconsin primary election on the spread of COVID-19. The study examines county-level voting data and COVID-19 test results, finding a significant association between in-person voting density and increased COVID-19 cases two to three weeks following the election. Specifically, a 10% increase in in-person voters per polling location led to an 18.4% rise in positive COVID-19 test rates. Additionally, Palguta et al. (2022) examine the causal impact of large-scale, in-person elections on the spread of COVID-19. The study utilizes a natural experiment in the Czech Republic, where one-third of Senate constituencies hold elections every two years, to estimate how the 2020 Senate elections influenced COVID-19 infection rates. The researchers find that voting constituencies experienced significantly faster growth in new COVID-19 infections compared to non-voting constituencies in the weeks following the elections. The study also observed a corresponding rise in hospital admissions, suggesting that the acceleration in infections resulted from genuine pandemic spread rather than increased testing. The effects are most pronounced among individuals under 65, likely due to strategic avoidance by elderly voters. Güntner et al. (Forthcoming) investigate the impact of the Bavarian municipal elections held on March 15, 2020, on the spread of COVID-19. Using a synthetic control method, the authors compare infection rates in Bavaria's 96 districts with control groups from other German districts outside of Bavaria. The estimates suggest that over a third of the increase in positive test results during the study period (March 15–April 4) cannot be explained by other demographic or economic factors. Additionally, the study reveals that districts with higher voter participation saw a larger increase in COVID-19 cases and deaths after the elections.

Our paper contributes to the existing literature in three distinct ways. First, by examining respiratory tract infections, we explore a potential link between elections and the spread of infectious diseases besides COVID-19. Second, we investigate the relationship between elections and infectious diseases during periods less severe than a global pandemic, providing further insights into the potential health risks associated with elections during less dynamic times. Therefore, we can evaluate whether the heightened risk of contracting a respiratory disease exists not only during pandemics but also during epidemic periods outside of a pandemic context. Finally, by utilizing unique data on sick leave occurrences, we are able to offer a estimation of the partial economic burden caused by infectious diseases potentially exacerbated by elections.

The remainder of the paper is organized as follows: Section 3.2 provides details on the data used in the analyses. Section 3.3 offers background information on the selected elections. Section 3.4 explains the empirical methodology, while Section 3.5 presents the results. Section 3.6 discusses the findings, and Section 3.7 concludes.

3.2 Data

For our analyses we retrieved data from the *Barmer Institut für Gesundheitsförderung* (bifg), which provides among other things detailed information on sickness absenteeism in Germany for people insured by the Barmer Ersatzkasse.[5] Barmer Ersatzkasse is the second largest health insurance provider in Germany, with around 7.35 million individuals insured across all federal states in 2019 (see Statista, 2023).[6] This database includes weekly reported sick leave rates across Germany's federal states, covering the period from calendar week 1 of 2018 to calendar week 52 of 2019. The data is collected from health insurance records and reflects individuals who are officially registered as unable to work due to respiratory illnesses. The denominator of the rate is defined as all persons "[...] who are in principle entitled to sickness benefit [...]. This includes, for example, employees, recipients of unemployment benefit, self-employed persons in their main occupation as well as those in temporary and short-term employment [...]." (bifg, 2024). For further convenience, we use the term *sick leaves per 1,000 insured* for the rate.

When examining German elections, other datasets might appear suitable as sources for potential outcome variables. One such dataset is SurvStat 2.0, an online database provided by the *Robert Koch-Institute* (RKI) (see RKI, 2024), which grants access to infectious disease surveillance data in Germany.[7] This database enables users to query data on diseases that are mandatorily notifiable under the *Infektionsschutzgesetz* (IfSG, or Infection Protection Act). However, there are at least two reasons why this dataset is unsuitable for our analyses. First, not all respiratory tract diseases, such as paranasal sinus inflammation, are subject to mandatory notification. Second, certain notifiable diseases, like Influenza, are only reported when the presence of the respective virus is confirmed through testing. Diagnoses made without conducting a test are not recorded. In that regard, the German guidelines for treating patients with conditions such as sore throat (DGHNO-KHC, 2024, p. 13), pneumonia (Ewig et al., 2021, p. 33), and cough (Kardos et al., 2019, p. 63) do not recommend microbiological diagnostics for non-severe cases. As a result, the dataset is likely to underrepresent the prevalence of respiratory diseases. This underreporting is evident in Tables A.1 to A.3 in the appendix. Despite

[5] The data is available via https://www.bifg.de.

[6] The average percentage of Barmer-insured individuals relative to the total population across all federal states was 11% in 2019 (Rädel et al., 2021).

[7] The data can be accessed via https://survstat.rki.de.

the fact that all three elections under consideration (see section 3.3) took place during the annual Influenza season, the number of reported cases being tested positive for Influenza per 100,000 inhabitants at that times was noticeably low.

Another potentially relevant dataset is the year-round syndromic surveillance of acute respiratory diseases (ARE-Konsultationsinzidenz) conducted by the RKI (Goerlitz et al., 2024). This dataset systematically collects information on symptoms and diagnoses from a voluntary sentinel network of primary care practices across Germany. These practices, comprising general and pediatric clinics, actively contribute to population-level disease surveillance, prevention, and control in addition to their routine patient care. However, data at the federal level is only available from the 2022/2023 Influenza season onward.

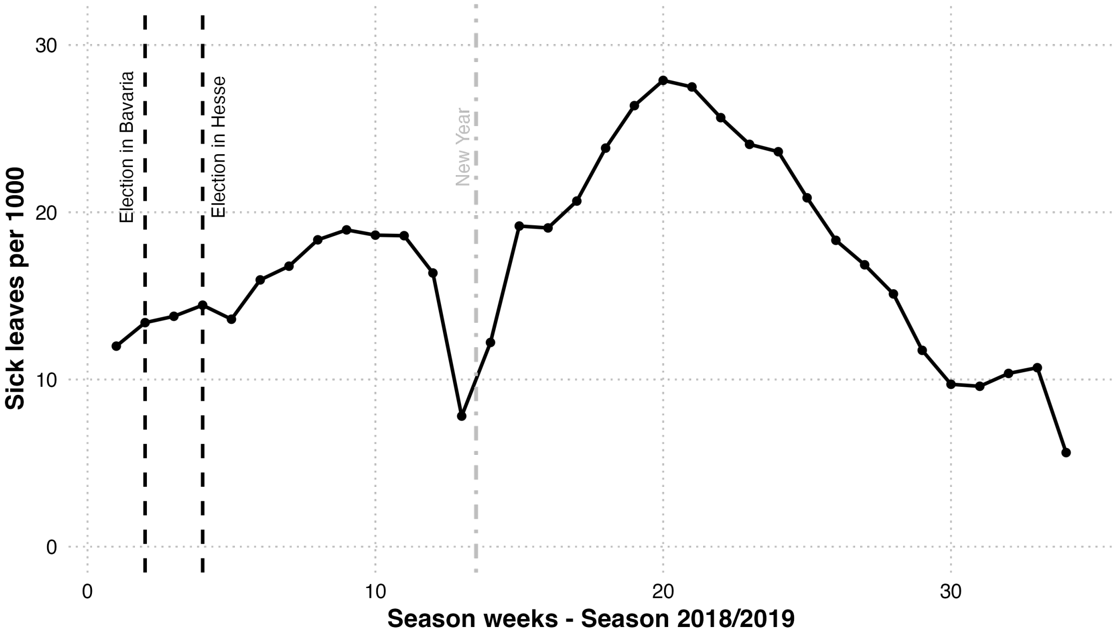
As additional predictors for our SCM analyses we use data on German demographic, economic, health care and child care characteristics at the federal level from the Federal Institute for Building and Regional Planning. Summary statistics for the full list of control variables separated by each election are reported in the Tables A.1, A.2 and A.3 in the appendix.

3.3 Elections

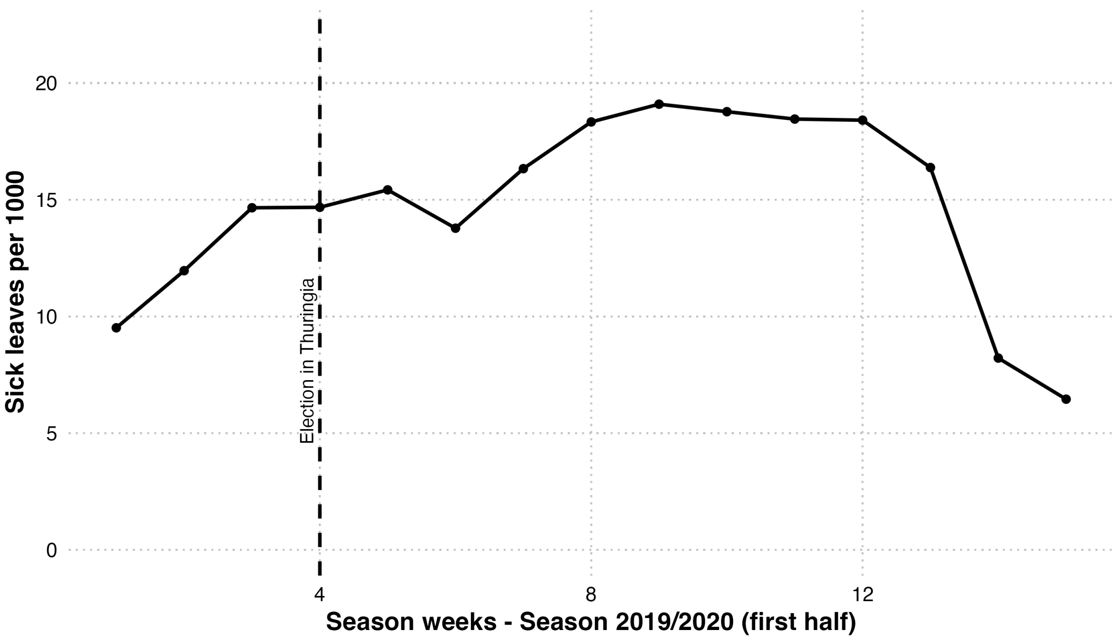
To identify the elections of relevance, we use an exploratory approach. Based on our assumption that the impact of elections on disease transmissions is influenced by the baseline spread of such viruses, we examine elections held outside pandemic periods but during the respiratory infection season. Since no season is defined for all respiratory infections, we stick with the yearly Influenza season. According to the RKI, the Influenza season runs from the 40th calendar week of one year to the 20th calendar week of the following year (Buda et al., 2019). The earliest available data point outlined in Section 3.2 begins in the first calendar week of 2018. Therefore, we focus on elections held during the 2018/2019 season and the first half of the 2019/2020 season, excluding the latter half due to the onset of the COVID-19 pandemic in Germany in January 2020 (Schilling et al., 2021).

Figure 1: Occurrence of sick leaves per 1,000 insured

(a) Season 2018/2019



(b) First half of season 2019



Note: Panel A plots the number of sick leaves reported per 1,000 people insured by the Barmer GEK for the Influenza season 2018/2019. The first season week refers to calendar week 40 in 2018 and the last season weeks to calendar week 20 in 2019. Panel B represents the same number for the first half of the season 2019/2020, including all calendar weeks of the season in 2019. The vertical dashed lines indicate the calendar weeks, in which the elections conducted.

Panel A of Figure 1 depicts the trajectory of sick leaves during the 2018/2019 season. In the first half of the season, sick leaves show a moderate week-to-week increase. The Barmer data for Germany indicate a peak around season week 20, corresponding to calendar week 8 in 2019. Although the prevalence of more severe Influenza subtypes, such as A(H1N1)pdm09 and A(H3N2), was rising at that time (Baldo et al., 2016; Buda et al., 2019; Jhung et al., 2013), the overall severity of the season remains comparatively lower than in preceding years (Buda et al., 2019). Two regional elections occurred during this season. The Bavarian state election was held on October 14, at the end of calendar week 41, drawing over 6.8 million voters (72.3% voter turnout) (Bayerisches Landesamt für Statistik, 2019). The Hessian state election followed on October 28, during calendar week 43, with approximately 3 million voters (67.3% voter turnout) (Hessisches Statistisches Landesamt, 2019). As shown in Panel A of Figure 1, these elections took place at the start of the 2018/2019 season, during a period of relatively low reported sick leave numbers. The first half of the 2019 season is depicted in Panel B of Figure 1. The season 2019/2020 is again considered less severe compared to previous years (RKI, 2020a). The only election held early in this period was the Thuringian state election on October 27, 2019 (calendar week 43). Of the approximately 1.7 million eligible voters, about 1.1 million (64.78%) participated (Thüringer Landesamt für Statistik, 2020). In terms of total number of voters, the Thuringian state election was significantly smaller compared to the elections held a year earlier in Bavaria and Hesse.

Given the generally low prevalence of respiratory infections during both seasons, this context in general provides a rather restrictive setting for testing whether elections held outside of pandemic periods influence the transmission of infectious diseases.

3.4 Method

We analyze federal-level data on sick leaves per 1,000 insured as the outcome variable, applying the SCM for causal inference in comparative case studies, as proposed by Abadie and Gardeazabal (2003), Abadie et al. (2010) and Abadie et al. (2015). The potential effect of the election, $\delta_{i,t}$, is defined as:

$$\delta_{i,t} = Y_{i,t}^{Treat} - Y_{i,t}^{C} \quad \text{for all } t > T_0, \tag{1}$$

where $i = 1, \dots, K$ represents the K federal states (here: Bavaria, Hesse or Thuringia) exposed to the intervention at calendar week t. The time frame $t = 1, \dots, T$ is divided into a pre-treatment period ($t = 1, \dots, T_0$) and a post-treatment period ($t = T_0 + 1, \dots, T$). For each election, the pre-treatment period begins six weeks before the election, while the post-treatment period concludes two weeks afterward. The post-treatment duration is selected based on the median incubation periods of the predominant subtypes A(H1N1)pdm09 and A(H3N2), which are approximately 2 days and 3 days (Cao et al., 2009; Jhung et al., 2013), together with a reasonable time required for potentially infected individuals to seek medical attention. The pre-treatment period is set to six weeks prior to the election (but including the week of the election) to ensure a sufficient number of observations for the analysis (Abadie, 2021). $Y_{i,t}^{Treat}$ represents the observed outcome for federal state i at time t after exposure to the intervention, while $Y_{i,t}^{C}$ denotes the counterfactual outcome for state i at time t in the absence of the intervention. For the elections held in 2018, the donor pool consists of 14 units, as either Bavaria or Hesse are excluded from respective donor pools. For the Thuringian election in 2019, the donor pool includes 15 federal states. Since the counterfactual outcome is unobservable, $Y_{i,t}^{C}$ is approximated using a weighted average of outcomes from the non-treated states ($j = 1, \dots, J$), referred to as "donor units":

$$Y_{i,t}^{C} = \sum_{j=1}^{J} w_j \cdot Y_{j,t}. \tag{2}$$

The weights $W = \{w_1, \dots, w_J\}$ are determined by solving the following minimization problem subject to $w_j \geq 0$ for all $j = 1, \dots, J$ and $\sum_{j=1}^{J} w_j = 1$:

$$\left[\sum_{m=1}^{N} v_m \left(X_{i,m} - \left(\sum_{j=1}^{J} w_j X_{j,m} \right) \right)^2 \right]^{\frac{1}{2}}. \tag{3}$$

Here, v_m reflects the relative importance of the m-th predictor variable X_m $(m = 1, \ldots, N)$ in assessing the similarity between treated and control districts. Predictor weights $V_m = (v_1, \ldots, v_N)$ are optimized to minimize the mean squared prediction error during the pre-treatment period:

$$\sum_{t=1}^{T_0} \left(Y_{i,t} - \left(\sum_{j=1}^{J} w_j(V_m) Y_{j,t} \right) \right)^2 . \tag{4}$$

In addition to the predictors listed in the Tables A.1 to A.3 in the appendix, our SCM approach incorporates weekly values of the sick leaves leading up to the election as additional predictors.

Inferential statistics are obtained by comparing the observed difference between the actual treated unit and its synthetic control with the differences between each unit from the donor pool (serving as a placebo) and its respective synthetic control. The rarity of the observed effect relative to placebo effects reflects the likelihood of observing such an effect by chance.

In this framework, inference relies on the ratio of the mean squared prediction error (MSPE) in the post-treatment period to that in the pre-treatment period:

$$\text{Ratio} = \frac{\text{MSPE}_{\text{Post}}}{\text{MSPE}_{\text{Pre}}}.$$

This ratio reflects the extent to which the pre-treatment fit diverges from the post-treatment trends (i.e., the causal effect). A strong pre-treatment fit indicates that the observed and synthetic control trends align closely before the intervention (Abadie et al., 2015). Divergence in the post-treatment period highlights the intervention's impact. A high ratio indicates substantial divergence between the two trends, suggesting a more pronounced causal effect. Conversely, a poor pre-treatment fit or minimal post-treatment divergence results in a lower ratio, indicating a weaker or less reliable effect.[8] [9]

Given that we utilize data on the number of sick leaves, we can calculate a rough estimate of the total costs associated with an increase in sick leaves following the election as follows:

$$\varnothing\Delta\text{Total costs}_{i,t} = \delta_{i,t} * \frac{Pop_i * EmployRate_i}{1000} * \text{Costs per leave} \tag{5}$$

[8] We do not report Fisher's exact p-values for the ranked ratios, as the maximum of 15 donor units is insufficient to achieve meaningful significance levels.

[9] For our SCM analysis, we use the R package `tidysynth` by Dunford (2023).

Here, *Total costs* represents the estimated additional costs caused by sick leaves in federal state i during week t. The term $\delta_{i,t}$ is the treatment effect for federal state i in week t, Pop_i denotes the population of state i, and $EmployRate_i$ is the employment rate in that state. Costs per leave refers to the estimated costs of an Influenza episode in Germany as provided by Ehlken et al. (2015), because this is the only estimate for a respiratory disease known to us. The estimated costs per case is $713.74 in 2024 dollars.

3.5 Results

This section provides the empirical findings derived from the econometric approach outlined earlier, applied to each of the three elections under consideration.

3.5.1 Bavarian election in October 2018

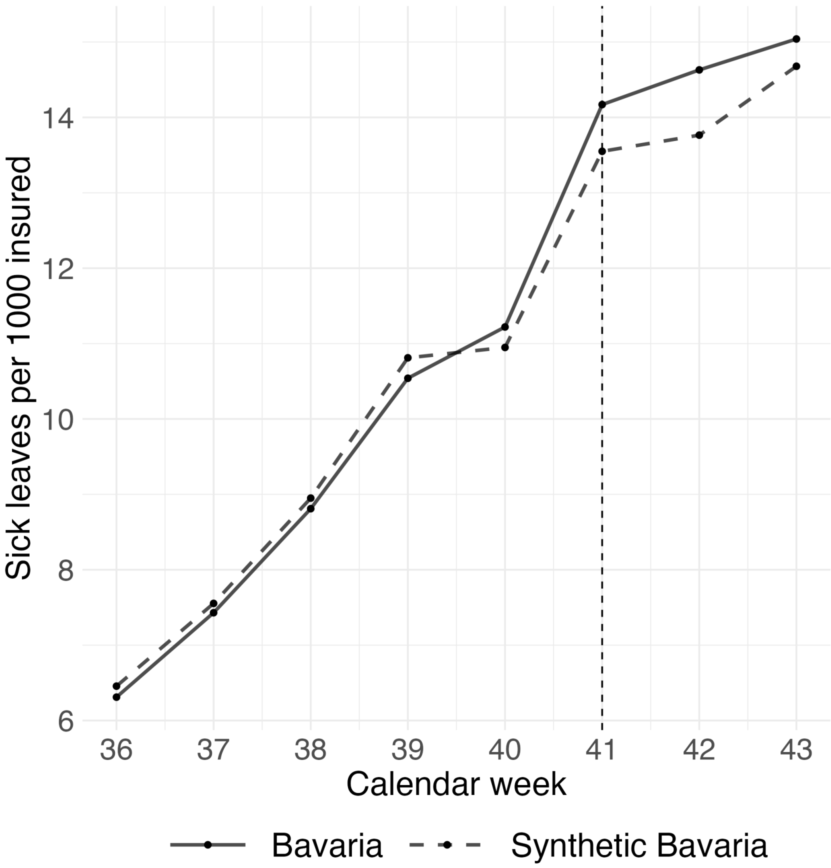
Figure 2 displays the results of the synthetic control estimation for the Bavarian state elections on October 14, focusing on weekly sick leaves per 1,000 insured individuals. The graph contrasts the trends in sick leaves for Bavaria with those of its synthetic counterpart, showing a moderate alignment (MSPE=0.10) during the pre-treatment period (calendar weeks 36 to 41 in 2018). The estimated effect, calculated as the difference between actual sick leaves in Bavaria and the synthetic control, is approximately 0.87 per 1,000 insured in calendar week 42 and around 0.36 in week 43. Interestingly, a slight divergence can already be observed roughly three weeks prior to the election.

Panel (a) of Figure 3 further contextualizes these effects by presenting the differences between observed sick leaves and their synthetic counterparts for each federal state (excluding Hesse). Bavaria is represented by the black line, while the grey lines correspond to the remaining 14 states, which serve as placebo comparisons. Relative to these placebo-in-space effects, Bavaria's differences, while positive, are not notably distinct. This interpretation is reinforced by Panel (b), which shows that Bavaria's ratio of post- to pre-treatment MSPE of 4.49 is not the largest, indicating no sizable divergence between the observed and synthetic trends compared to the other states.

A potential concern regarding the SCM results is the possibility of spillover effects between Bavaria and its neighboring states. If elections result in an increase in infections, spillover effects could dampen the estimated difference, as neighboring states might report a higher number of sick leaves in response. This concern is particularly relevant given that

Baden-Württemberg, a neighboring state, contributes a weight of 0.92 to the baseline results (see Table A.4 in the appendix). To address this, we conducted an additional SCM specification excluding not only Hesse but also the other neighboring states of Baden-Württemberg, Saxony, and Thuringia. However, excluding donors with previously substantial weights can compromise the pre-treatment fit, as seen in Figure A.1 in the appendix. While the estimated difference after the election increased compared to the baseline results, the pre-treatment fit deteriorated (MSPE=2.36). This is likely because Bavaria, as a federal state, lies outside the convex hull of the remaining non-neighboring states (Abadie, 2021).

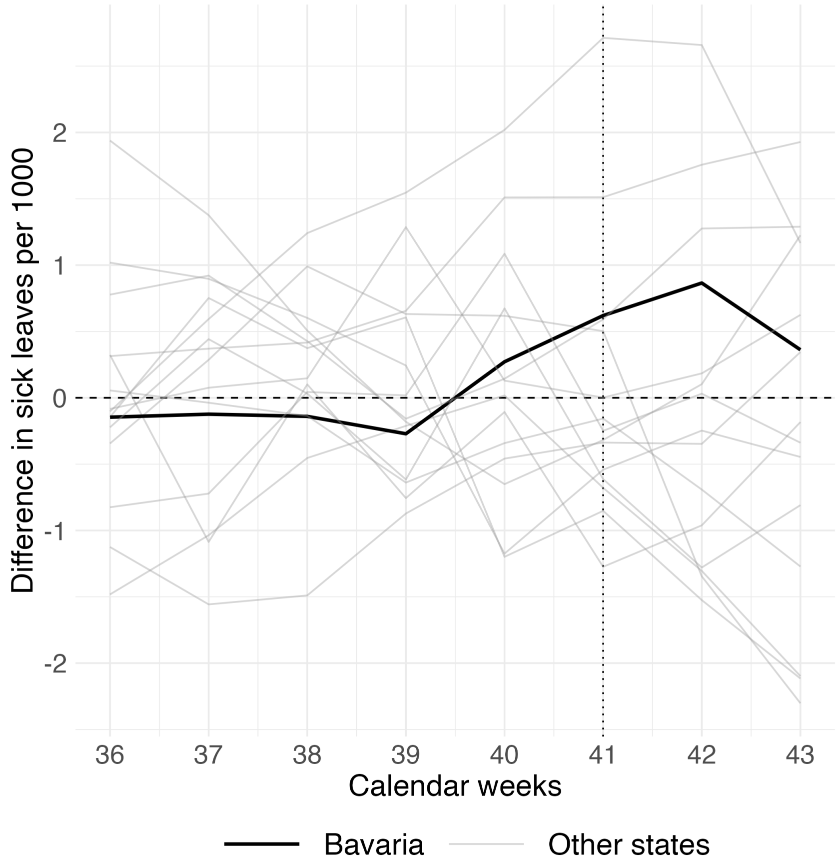
Figure 2: Bavarian election in October 2018: Development in sick leaves



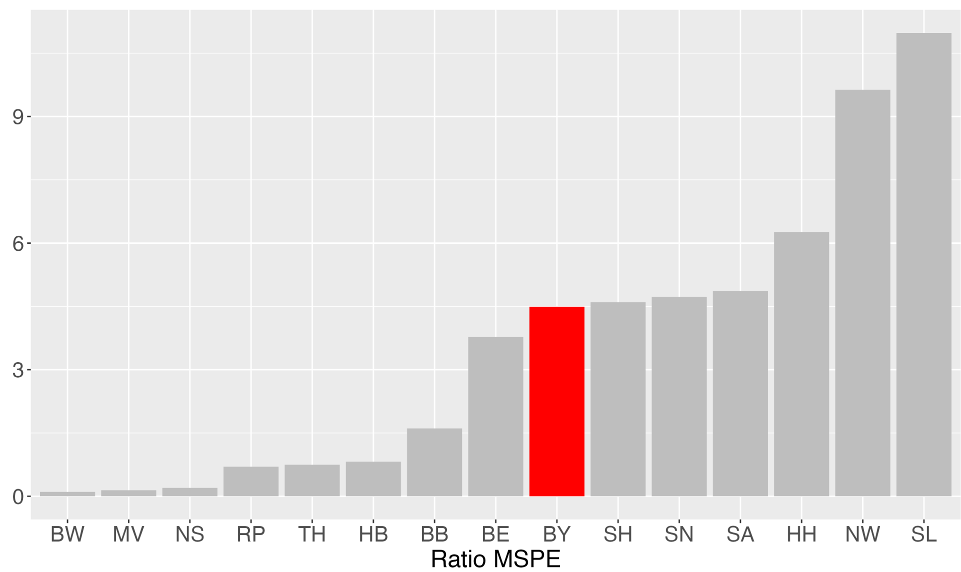
Note: The graph depicts the weekly trend of sick leave cases due to respiratory diseases per 1,000 individuals insured by Barmer in Bavaria (solid black line) alongside its synthetic counterpart (dashed line). The vertical dashed line indicates calendar week 41, the week of the election.

Figure 3: Bavarian election in October 2018 - Placebos in space

(a) Difference in sick leaves for Bavaria and other states



(b) Ratios of post- to pre-treatment MSPE



Note: Panel (a) shows the differences between each federal state and its synthetic counterpart, with the black line indicating the difference for Bavaria and the grey lines representing placebo comparisons for other federal states. The vertical dashed line indicates calendar week 41, the week of the election. Panel (b) includes the ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Bavaria (BY, red) and all other federal states except Hesse (BW for Baden-Württemberg, BE for Berlin, BB for Brandenburg, HB for Bremen, HH for Hamburg, NS for Lower Saxony, MV for Mecklenburg-Western Pomerania, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt, SH for Schleswig-Holstein, and TH for Thuringia).

Overall, the absence of a sizable effect for the Bavarian election in October 2018, which took place at the onset of the Influenza season in Germany, suggests that elections may have a negligible impact on the spread of respiratory diseases when the baseline virulence is low.

3.5.2 Hessian election in October 2018

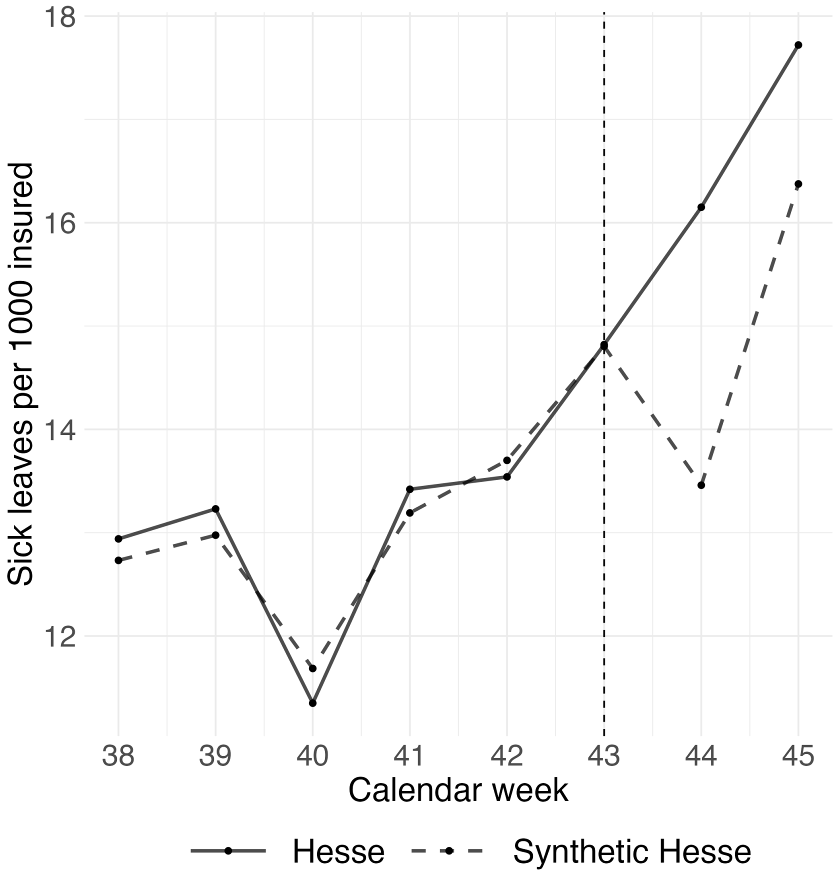
Figure 4 illustrates the trend in sick leaves for Hesse compared to its synthetic counterpart. The graph demonstrates a strong pre-treatment fit from calendar week 38 up to the election week 43 (MSPE=0.05). Starting in calendar week 42, the upward trend in reported sick leaves for Hesse remains even after the election. In contrast, the number of sick leaves for the *Synthetic Hesse* decreases sharply in the week following the election, leading to a significant estimated effect. The difference in sick leaves is estimated at approximately 2.69 per 1,000 insured for week 44 and about 1.34 for week 45. Based on these estimates, the corresponding back-of-the-envelope calculations suggest additional weekly costs of around \$9.8 million for week 44 and \$4.9 million for week 45. The corresponding weights for the donors can be seen in Table A.5 in the appendix.

Figure 5 shows that the observed difference for Hesse is highly unusual compared to the placebo estimates. Panel (a) reveals that the difference for Hesse is near zero during the pre-treatment period but rises sharply after calendar week 43. Moreover, Hesse exhibits by far the largest MSPE ratio of 90.85 among all states.

As shown in Figure A.2, the estimated effect is robust to the exclusion of the neighboring states Bavaria, Baden-Württemberg, North Rhine-Westphalia, Rhineland-Palatinate, and Thuringia. In this scenario, the estimated difference for week 44 is 2.38 additional sick leaves per 1,000 insured, and for week 45, it is 1.62. This suggests that spillover effects on the federal state level, if present, play only a minor role.

Another potential concern in this context is the impact of school holidays on the spread of infections. While empirical evidence suggests that summer holidays can facilitate the spread of infectious diseases (Plümper & Neumayer, 2021), we anticipate the opposite effect on reported sick leave numbers. During school holidays, many parents take time off to be with their children, and those who fall ill are less likely to visit a doctor and report being sick. Consequently, we hypothesize that fall holidays in at least one donor state could lead to an overestimation of the election effect.[10] To address this, we excluded five federal states with fall holidays in the post-treatment period (Baden-Württemberg, Bavaria, Berlin, Brandenburg, and Mecklenburg-Western Pomerania) from the donor pool. The adjusted results in Panel (a) and (b) of Figure A.3 show only minor changes compared to the baseline, with an estimated difference of 2.65 cases per 1,000 insured in calendar week 44 and 0.90 in week 45.

Figure 4: Hessian election in October 2018: Development in sick leaves

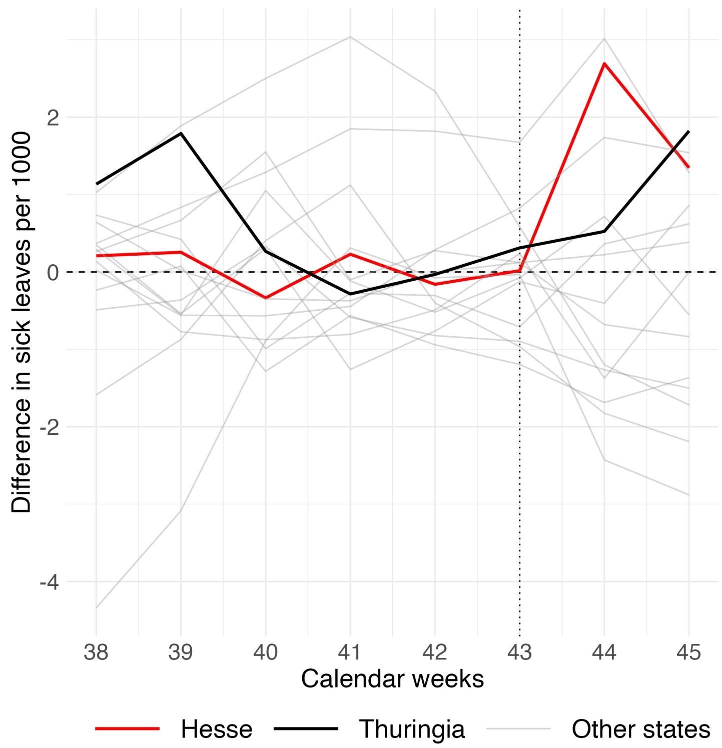


Note: The graph depicts the weekly trend of sick leave cases due to respiratory diseases per 1,000 individuals insured by Barmer in Hesse (solid black line) alongside its synthetic counterpart (dashed line). The vertical dashed line indicates calendar week 43, the week of the election.

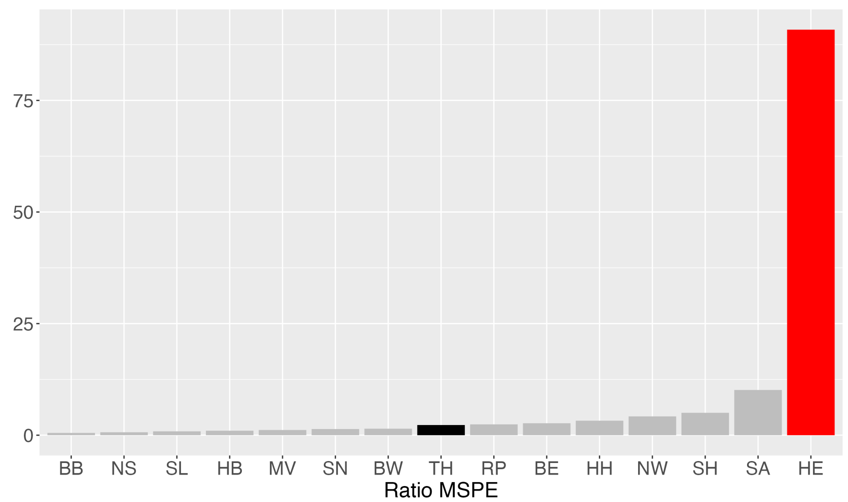
[10]The same reasoning applies to Bavaria, Hesse, and Thuringia, where holidays could have resulted in an underestimation of the actual election effect. However, none of these states had fall holidays during the respective election periods.

Figure 5: Hessian election in October 2018 - Placebos in space

(a) Difference in sick leaves Hesse and other federal states as placebos



(b) Ratios of post- to pre-treatment MSPE



Note: Panel (a) shows the differences in sick leaves per 1,000 insured between each federal state and its synthetic counterpart, with the red line indicating the difference for Hesse, the black line revealing the difference for Thuringia and the grey lines representing placebo comparisons for all other federal states (except Bavaria). The vertical dashed line indicates calendar week 43, the week of the election. Panel (b) includes the corresponding ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Hesse (HE, red), Thuringia (TH, black) and all other federal states except Bavaria (BW for Baden-Württemberg, BE for Berlin, BB for Brandenburg, HB for Bremen, HH for Hamburg, NS for Lower Saxony, MV for Mecklenburg-Western Pomerania, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt and SH for Schleswig-Holstein).

A placebo-in-time analysis, where a hypothetical election is assigned to a period with no actual election, can further investigate the link between elections and the spread of infections. If no effect is observed for this placebo election, it suggests that other events during that time of the year do not significantly influence the spread of respiratory diseases and, consequently, sick leaves. To explore this, we reference the placebo-in-space analysis conducted for the Thuringian election, which occurred in the same calendar week but one year after the Hessian election. The black line in Panel (a) of Figure 7 indicates a noticeable difference in reported sick leaves for Hesse following calendar week 43 in 2019. However, the pre-treatment fit decreases (MSPE=0.08), resulting in a lower yet still noticeable ratio of 34.74, as shown in Panel (b).

As an additional sensitivity analysis concerning the timing of the election, we assigned a hypothetical treatment date of calendar week 41, two weeks prior to the actual election, while keeping the number of pre-treatment weeks constant. This included two additional weeks (calendar weeks 36 and 37). Figure A.4 shows little to no differences between the hypothetical treatment in week 41 and the actual election week (43). This suggests that the observed differences are attributable to the election or events occurring close to it, rather than earlier events.

Additionally, using an extended pre-treatment period from the placebo-in-time analysis, the results remain consistent. The estimated differences for week 44 (2.77) and week 45 (1.30) show minimal change, reinforcing the robustness of the findings.

Any variation in vaccination uptake against respiratory diseases could account for differences observed in the baseline results. To address this, we finally incorporated the per capita vaccine consumption data for each federal state, as published by the Associations of Statutory Health Insurance Physicians (in German: *Kassenärztliche Vereinigungen*) through the Atlas of Medicinal Products (in German; *Arzneimittel-Atlas*) for 2018 (Häussler & Höer, 2019).[11] The results presented in Figure A.6 show only slight changes compared to the baseline analysis. Specifically, the estimated differences for week 44 are approximately 2.72 and 1.35 for week 45.

[11] The data is initially provided for 17 regional associations. However, 15 of these associations correspond directly to the areas of 15 federal states. For North Rhine-Westphalia, which has two associations, both reported the same per capita uptake in 2018 (0.45). Consequently, we used the value of 0.45 for North Rhine-Westphalia.

3.5.3 Thuringian election in October 2019

Figure 6 illustrates the trend in sick leaves for Hesse compared to its synthetic counterpart. The graph demonstrates a strong pre-treatment fit (MSPE=0.04) from calendar week 38 up to calendar week 41 week. Between week 41 and 43 the fit deteriorates slightly. After calendar week 43 both numbers drop significantly, with a bigger downward slope for *Synthetic Thuringia*. The resulting difference of 1.10 in calendar week 44 and 1.54 in calendar week 45 results in total costs of $1.44 million and $2.02 million in the respective weeks.

Figure 7 shows that the observed difference for Thuringia is highly unusual compared to the placebo estimates. Panel (a) reveals that the difference for Thuringia is near zero during the pre-treatment period but rises sharply after calendar week 43. Moreover, Thuringia exhibits by far the largest MSPE ratio of 44.94 among all states.

The estimated effect reduces in size when excluding neighboring states (Bavaria, Hesse, Lower Saxony, Saxony and Saxony-Anhalt) as shown in Figure A.7. In this scenario, the estimated difference for week 44 is 0.29 additional sick leaves per 1,000 insured, and for week 45, it is 1.13. However, the pre-treatment effect deteriorates again due to exclusion of the neighboring states, since the excluded states Saxony and Saxony-Anhalt contribute to a huge share of the weight in the baseline analysis (see Table A.6).

As part of the sensitivity analysis addressing the fall holidays in 2019, we removed Baden-Württemberg, Bavaria, and Mecklenburg-Western Pomerania from the donor pool. Panels (a) and (b) of Figure A.8 indicate a reduced estimated effect, with 0.71 additional sick leaves per 1,000 insured in calendar week 44 and 1.36 in calendar week 45.

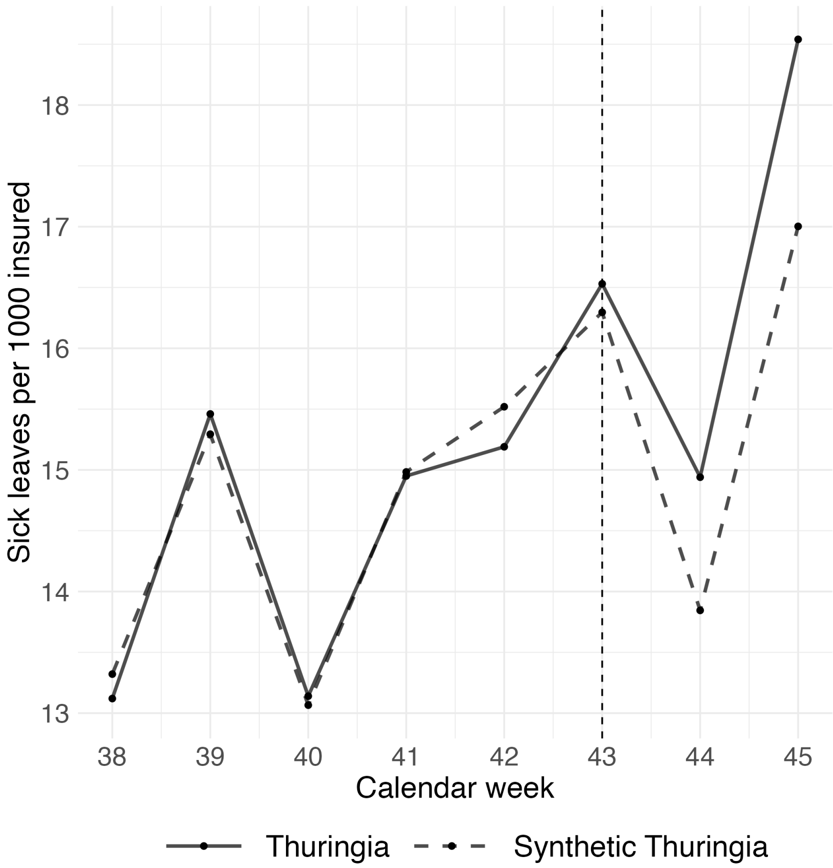
The placebo-in-time analysis presented in Figure 5 reveals an increase in the difference following the placebo treatment. However, it also highlights that the pre-treatment fit remains relatively weak, leading to a lower MSPE ratio, as depicted in Panel (b).

We again assigned a hypothetical treatment date of calendar week 41 in 2019, two weeks prior to the actual election, while keeping the number of pre-treatment weeks constant. Figure A.9 results again in a reduction of the estimated difference of 0.24 in calendar week 44 and 0.90 in calendar week 45.

Additionally, using an extended pre-treatment period from the placebo-in-time analysis, the estimated effects are again lower compared to the baseline. The estimated differences for week 44 is 0.24 and 0.90 for week 45.

Finally, the results shown in Figure A.11, incorporating vaccination consumption as an additional predictor variable, are largely consistent with the baseline results, with an estimated difference of approximately 0.95 in calendar week 44 and 1.62 in calendar week 45.

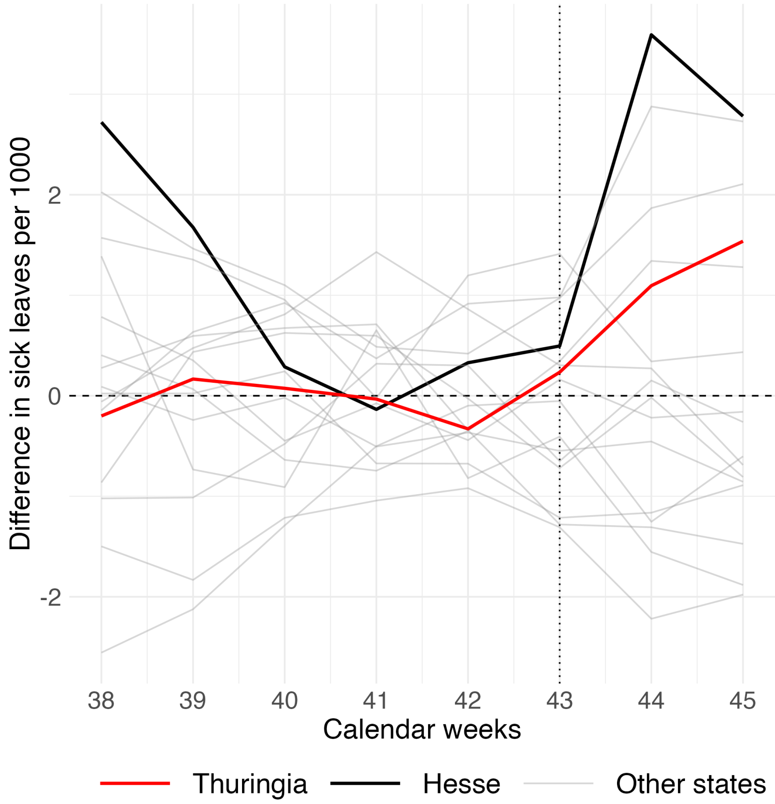
Figure 6: Thuringian election in October 2019 - Development in sick leaves



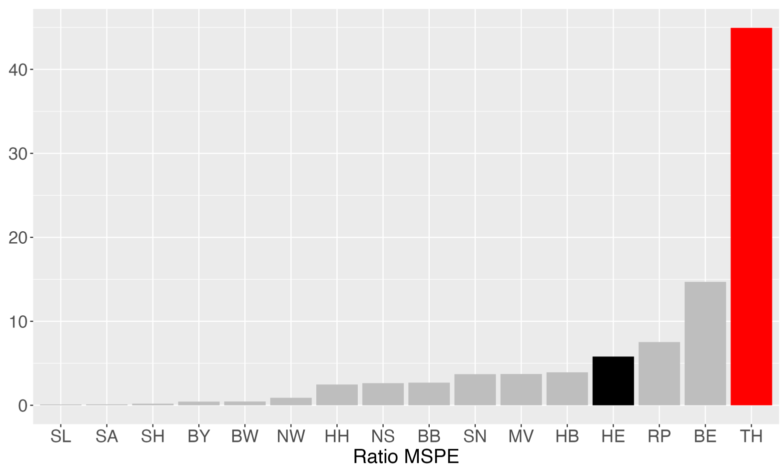
Note: The graph depicts the weekly trend of sick leave cases due to respiratory diseases per 1,000 individuals insured by Barmer in Thuringia (solid black line) alongside its synthetic counterpart (dashed line). The vertical dashed lines indicate calendar week 43, the week of the election.

Figure 7: Thuringian election in October 2019 - Placebos in space

(a) Difference in sick leaves Thuringia and other states



(b) Ratios of post- to pre-treatment MSPE



Note: Panel (a) shows the differences in sick leaves per 1,000 insured between each federal state and its synthetic counterpart, with the red line indicating the difference for Thuringia, the black line presents the difference for Hesse and the grey lines representing placebo comparisons for other federal states. The vertical dashed line indicates calendar week 43, the week of the election. Panel (b) includes the corresponding ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Thuringia (TH, red), Hesse (HE, black) and all other federal states (BW for Baden-Württemberg, BY for Bavaria, BE for Berlin, BB for Brandenburg, HB for Bremen, HH for Hamburg, NS for Lower Saxony, MV for Mecklenburg-Western Pomerania, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt and SH for Schleswig-Holstein).

3.6 Discussion

The findings indicate a moderate increase in sick leaves in Bavaria following the October 2018 election, closely aligning with trends observed in placebo states. Conversely, the Hessian election during held shortly after exhibited a noticeable increase in sick leaves. The results of the analyses highlight significant costs and robust findings across different sensitivity specifications. The Thuringian election in October 2019 demonstrated smaller yet distinct effects, diverging clearly from placebo trends. Robustness tests, including evaluations of spillover effects and neighboring state dynamics, confirmed the reliability of these results, despite occasional reductions in pre-treatment fit. Together, these findings underscore a connection between election events and post-election health-related absenteeism due to respiratory infections.

When combined with research on elections during the COVID-19 pandemic (Cotti et al., 2021; Güntner et al., Forthcoming; Palguta et al., 2022), these results suggest that electoral officers and administrators should exercise greater caution regarding public health risks. Although elections during pandemics or Influenza seasons may facilitate disease spread, we refrain from advocating for a blanket postponement of elections to periods of less virulent dynamics for three reasons: First, no existing studies have examined elections conducted during periods of typically low respiratory disease activity, such as summer, making such a recommendation premature. Second, election dates are often legally determined, and rescheduling could result in significant economic and social costs, likely exceeding the public health costs of holding elections during infectious seasons. Third, delaying an election could be perceived by both the political opposition and the public as an illegitimate effort by the incumbent government to extend its time in power (see e.g. Egmont Institute, 2020; Zamfir & Fardel, 2020).

Instead, we propose enhancing the provision of face masks at polling stations. Masks have proven effective and cost-efficient in preventing the spread of airborne diseases like Influenza and COVID-19 (Bartsch et al., 2022; Mitze et al., 2020; Sharma et al., 2024; Van Dyke, 2020; Wang et al., 2020). While the pandemic demonstrated the feasibility of rapidly distributing large quantities of masks, providing masks to all voters could still be expensive. An even better approach may involve distributing masks only to electoral staff, who act as potential transmission agents during in-person voting. By mitigating transmission at this critical juncture, masks for electoral staff could significantly reduce the health risks (and therefore the economic burden) associated with elections.

While the proposed measures provide practical solutions, it is equally crucial to discuss the potential limitations of this study. One potential issue not yet addressed is the possibility that other concurrent events may have contributed to the spread of infections, thereby influencing the observed increase in reported sick leaves. For example, the *Oktoberfest*, held annually in Munich, the capital of Bavaria, beginning in mid-September, draws millions of visitors as the world's largest folk festival. This event could potentially explain the slight upward trend in sick leaves observed in Figure 2 after calendar week 38. Similarly, recurring events leading to large-scale gatherings (for example the *Frankfurt Book Fair*) might also account for the increase in sick leaves noted in the placebo analysis of Hesse around calendar week 43 in 2019 (see Figure 7).

An additional potential concern arises from the literature indicating a negative impact of pandemic events on voter turnout (Bauernschuster et al., 2023; Picchio & Santolini, 2022; Urbatsch, 2017). Based on this reasoning, voters—particularly those with preexisting health conditions—might choose to abstain from voting due to the perceived heightened risk of infection, potentially leading to an underestimation of the actual effect. However, since both Influenza seasons in question were considered relatively mild (Buda et al., 2019; RKI, 2020a), we expect at most a minimal impact from such anticipatory concerns.

Another potential source of bias in the estimated effects could stem from differences uncontrolled by our SCM approach in the workforce insured by Barmer across federal states.[12] For example, the group of Barmer-insured individuals in Bavaria, Hesse, and Thuringia might include a relatively higher proportion of individuals at greater risk of contracting respiratory diseases during the season, such as healthcare workers. However, since our approach also matches donor states based on pre-trends in sick leave numbers, we anticipate this issue to have only a minimal impact on the results.

A related concern involves potential unobserved differences between Barmer-insured individuals and the general German population, which could affect the external validity of the findings. Nonetheless, given the large number of people insured by Barmer in Germany, we expect the results to be externally valid, at least for the statutorily insured

[12] An initial indication of this can be seen in the variation of the proportion of Barmer-insured individuals relative to the total population across German states, as presented in Table A.7 in the appendix.

population.[13]

3.7 Conclusion

This study examined the potential relationship between elections and the spread of acute respiratory diseases. Using an SCM approach, we find evidence suggesting an increase in reported sick leaves from work following regional elections. These findings highlight how elections may contribute to health risks and associated economic costs from respiratory tract infections, even outside pandemic contexts. However, it is important to emphasize that the results for the Bavarian election in October 2018 reveal no sizable effect, and alternative explanations for the findings related to the Hessian election in 2018 and the Thuringian election in 2019 cannot be entirely ruled out.

Future research could further investigate the connection between elections and disease transmission. One promising avenue would be to focus on elections held during periods with an even a lower baseline prevalence of respiratory viruses, such as the summer months. The absence of any sizable effect for the Bavarian election in October 2018, held at the very start of the Influenza season in Germany, may indicate that elections have a minimal impact on the spread of highly virulent diseases during such times. Additionally, expanding this analysis to include elections in other regions worldwide could provide valuable insights into the generalizability of these findings.

Another promising research direction would be to move beyond examining respiratory diseases as a whole and instead differentiate between specific illnesses based on distinct virus strains, such as those of the Influenza virus, Rhinovirus, or Adenovirus. This approach would offer a clearer understanding of which viruses in particular primarily contribute to heightened health risks associated with elections, facilitating the development of more targeted countermeasures. Moreover, it could enhance estimates of the overall economic burden. Given that much of the existing literature focuses on the economic impact of individual diseases or pathogens, such differentiation could provide a more nuanced assessment of election-related effects in this context.

[13] In 2019, approximately 88% of employees in Germany were covered by statutory health insurance, while the remaining individuals were primarily covered by private insurance (Statistisches Bundesamt, 2024).

Appendix

Table A.1: Bavarian election October 2018: Summary of Key Variables

	Variable	Bavaria	Other States
Morbidity	Influenza cases in calendar week 40*	0.030589	0.01293 (0.01907)
	Influenza cases in calendar week 42*	0.05353	0.03927 (0.06854)
	Sick leaves in calendar week 40†	11.22	12.10 (1.63)
	Sick leaves in calendar week 42†	14.63	13.73 (1.97)
Demographic	Inhabitants per km^2 of settlement and traffic area	1536.20	2037.73 (1407.59)
	Age structure of population		
	% aged under 6	5.70	5.49 (0.44)
	% aged 6–17	10.70	10.41 (0.50)
	% aged 18–24	7.98	7.01 (1.28)
	% aged 25–29	6.64	5.96 (1.23)
	% aged 30–49	26.15	25.24 (1.91)
	% aged 50–64	22.46	23.20 (2.03)
	% aged ≥65	20.36	22.68 (2.62)
	Population development††	6.60	4.62 (2.16)
	Female share of population	50.42	50.72 (0.21)
	Foreign share of population	13.21	10.39 (5.28)
Health and Social issues	Child care participation rates		
	% aged 0–2 years	26.88	40.74 (12.60)
	Hospital beds††	5.81	6.29 (0.77)
	Geriatric demand and supply		
	Elderly in need of care‡	3.41	4.92 (0.76)
	Nursing home places§	98.11	113.29 (17.67)
	General physicians§	6.55	6.37 (0.25)

Note: Table continues on the next page.

Table A.1: Bavarian election October 2018: Summary *continued*

	Variable	Bavaria	Other States
Economic	Unemployment rates		
	% unemployed	2.90	6.36 (1.67)
	% unemployed aged 55–64	3.93	6.86 (1.48)
	% unemployed women	2.78	5.95 (1.54)
	Employment rate	6.55	6.37 (0.25)
	Household income per capita per month	2137.62	1826.67 (143.74)
	GDP per capita♮	47.43	37.52 (10.22)
	Commuters		
	% out	45.08	35.10 (11.04)
	% In	46.05	36.15 (7.43)
	Share of workers with academic degree	16.87	16.07 (5.20)
	Tourism		
	Stays in hotels per capita	7.55	6.42 (4.28)
	Share of stays in hotels by foreigners	20.76	15.68 (11.30)

Note: Unweighted sample means with standard deviation in parentheses. The other states include all non-bavarian federal states in Germany except for Hesse.

* per 100,000 inhabitants, † per 1,000 insured, †† per 1,000 inhabitants, ‡ per 100 inhabitants, § per 10,000 inhabitants, ♮ in EUR 10,000

Table A.2: Hessian election October 2018: Summary of Key Variables

	Variable	Hesse	Other States
Morbidity	Influenza cases in calendar week 42*	0.00000	0.03927 (0.06854)
	Influenza cases in calendar week 44*	0.00000	0.06042 (0.11667)
	Sick leaves in calendar week 42†	13.54	13.73 (1.97)
	Sick leaves in calendar week 44†	16.15	13.56 (1.99)
Demographic	Inhabitants per km^2 of settlement and traffic area	1861.38	2037.73 (1407.59)
	Age structure of population		
	% aged under 6	5.73	5.49 (0.44)
	% aged 6–17	10.99	10.41 (0.50)
	% aged 18–24	7.93	7.01 (1.28)
	% aged 25–29	6.44	5.96 (1.23)
	% aged 30–49	25.83	25.24 (1.91)
	% aged 50–64	22.44	23.20 (2.03)
	% aged ≥65	20.63	22.68 (2.62)
	Population development††	4.76	4.62 (2.16)
	Female share of population	50.64	50.72 (0.21)
	Foreign share of population	16.17	10.39 (5.28)
Health and Social issues	Child care participation rates		
	% aged 0–2 years	30.15	40.74 (12.60)
	Hospital beds††	5.78	6.29 (0.77)
	Geriatric demand and supply		
	Elderly in need of care‡	4.57	4.92 (0.76)
	Nursing home places§	97.37	113.29 (17.67)
	General physicians§	6.10	6.37 (0.25)

Note: Table continues on the next page.

Table A.2: Hessian election October 2018: Summary *continued*

	Variable	Hesse	Other States
Economic	Unemployment rates		
	% unemployed	4.59	6.36 (1.67)
	% unemployed aged 55–64	4.79	6.86 (1.48)
	% unemployed women	4.45	5.95 (1.54)
	Employment rate	6.10	6.37 (0.25)
	Household income per capita per month	1985.83	1826.67 (143.74)
	GDP per capita[♮]	45.66	37.52 (10.22)
	Commuters		
	% out	45.90	35.10 (11.04)
	% In	48.65	36.15 (7.43)
	Share of workers with academic degree	18.36	16.07 (5.20)
	Tourism		
	Stays in hotels per capita	5.54	6.42 (4.28)
	Share of stays in hotels by foreigners	23.30	15.68 (11.30)

Note: Unweighted sample means with standard deviation in parentheses. The other states include all non-hessian federal states in Germany except for Bavaria.

[†] per 1,000 inhabitants or per 1,000 insured, [††] per 1,000 inhabitants, [‡] per 100 inhabitants, [§] per 10,000 inhabitants, [♮] in EUR 10,000

Table A.3: Thuringian election October 2019: Summary of Key Variables

	Variable	Thuringia	Other States
Morbidity	Influenza cases in calendar week 42*	0.5156	0.12710 (0.12634)
	Influenza cases in calendar week 44*	0.04687	0.11726 (0.13755)
	Sick leaves in calendar week 42†	15.19	14.64 (1.19)
	Sick leaves in calendar week 44†	14.94	13.70 (1.94)
Demographic	Inhabitants per km^2 of settlement and traffic area	1120.26	2053.25 (1351.60)
	Age structure of population		
	% aged under 6	5.13	5.60 (0.44)
	% aged 6–17	10.08	10.53 (0.43)
	% aged 18–24	5.71	7.21 (1.09)
	% aged 25–29	4.17	5.91 (1.29)
	% aged 30–49	24.42	25.26 (1.90)
	% aged 50–64	24.26	23.06 (1.85)
	% aged ≥65	26.23	22.43 (2.63)
	Population development††	1.58	3.97 (2.39)
	Female share of population	50.50	50.71 (0.21)
	Foreign share of population	5.21	11.65 (5.16)
Health and Social issues	Child care participation rates		
	% aged 0–2 years	56.64	39.73 (12.43)
	Hospital beds††	7.48	6.17 (0.70)
	Geriatric demand and supply		
	Elderly in need of care‡	6.36	5.15 (0.83)
	Nursing home places§	125.55	110.85 (18.18)
	General physicians§	6.70	6.32 (0.23)

Note: Table continues on the next page.

Table A.3: Thuringian election October 2019: Summary *continued*

	Variable	Thuringia	Other States
Economic	Unemployment rates		
	% unemployed	5.27	5.80 (1.82)
	% unemployed aged 55–64	6.22	6.17 (1.54)
	% unemployed women	4.89	5.39 (1.68)
	Employment rate	6.70	6.32 (0.23)
	Household income per capita per month	1735.63	1907.35 (147.67)
	GDP per capita♮	29.91	40.61 (10.24)
	Commuters		
	% out	38.76	36.23 (11.32)
	% In	34.33	37.69 (8.10)
	Share of workers with academic degree	13.33	17.06 (5.22)
	Tourism		
	Stays in hotels per capita	4.53	6.91 (4.55)
	Share of stays in hotels by foreigners	6.05	16.99 (10.63)

Note: Unweighted sample means with standard deviation in parentheses. The other states include all 15 non-thuringian federal states in Germany.

† per 1,000 inhabitants or per 1,000 insured, †† per 1,000 inhabitants, ‡ per 100 inhabitants, § per 10,000 inhabitants, ♮ in EUR 10,000

Table A.4: Donor weights: Bavaria 2018, Sick leaves

	States	Weight
1	Schleswig-Holstein	0.00
2	Hamburg	0.00
3	Lower Saxony	0.00
4	Bremen	0.00
5	North Rhine-Westphalia	0.00
6	Rhineland-Palatinate	0.00
7	Baden-Württemberg	0.92
8	Saarland	0.00
9	Berlin	0.00
10	Brandenburg	0.08
11	Mecklenburg-Western Pomerania	0.00
12	Saxony	0.00
13	Saxony-Anhalt	0.00
14	Thuringia	0.00

Table A.5: Donor weights: Hesse 2018, Sick leaves

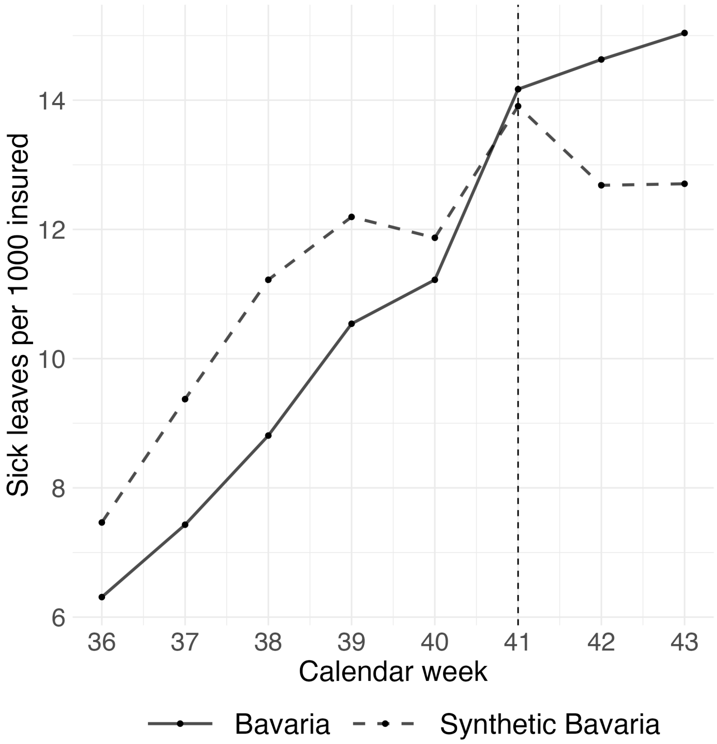
	States	Weight
1	Schleswig-Holstein	0.00
2	Hamburg	0.00
3	Lower Saxony	0.00
4	Bremen	0.12
5	North Rhine-Westphalia	0.04
6	Rhineland-Palatinate	0.60
7	Baden-Württemberg	0.00
8	Saarland	0.00
9	Berlin	0.15
10	Brandenburg	0.09
11	Mecklenburg-Western Pomerania	0.00
12	Saxony	0.00
13	Saxony-Anhalt	0.00
14	Thuringia	0.00

Table A.6: Donor weights: Thuringia 2019, Sick leaves

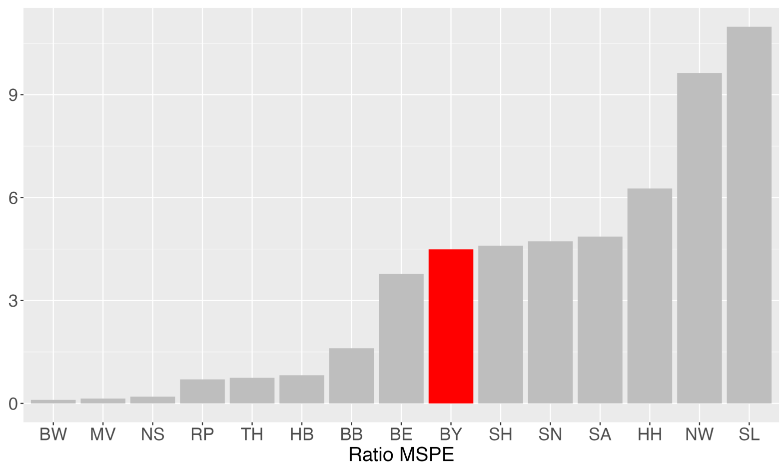
	States	Weight
1	Schleswig-Holstein	0.00
2	Hamburg	0.00
3	Lower Saxony	0.00
4	Bremen	0.00
5	North Rhine-Westphalia	0.00
6	Hesse	0.00
7	Rhineland-Palatinate	0.00
8	Baden-Württemberg	0.14
9	Bavaria	0.00
10	Saarland	0.00
11	Berlin	0.00
12	Brandenburg	0.00
13	Mecklenburg-Western Pomerania	0.00
14	Saxony	0.33
15	Saxony-Anhalt	0.53

Figure A.1: Bavarian election in October 2018 - No neighboring states

(a) Sick leaves for Bavaria and Synthetic Bavaria



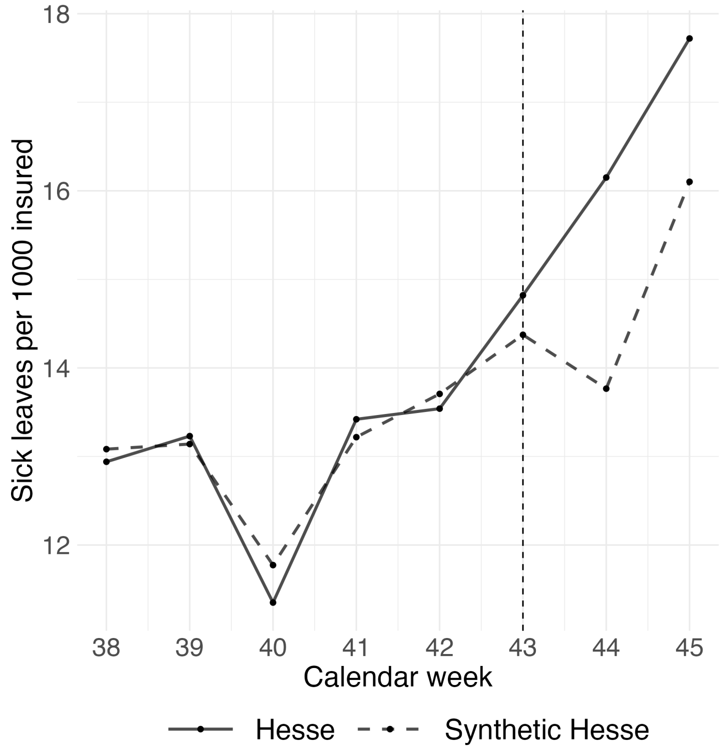
(b) Ratios of post- to pre-treatment MSPE



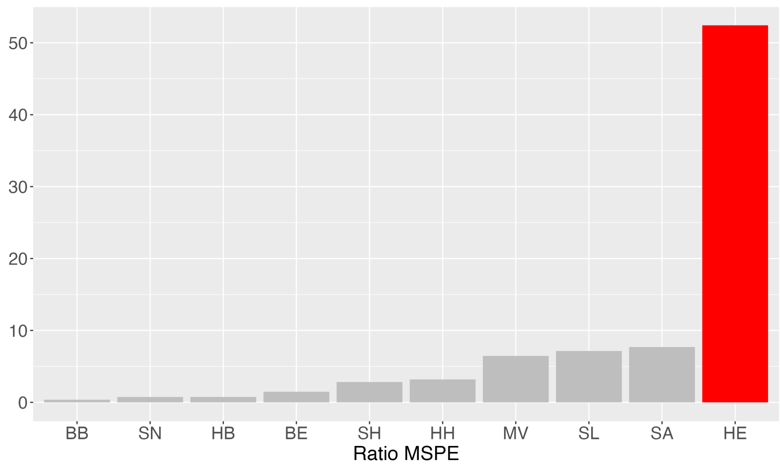
Note: Panel (a) illustrates the weekly trend in sick leave cases per 1,000 individuals insured by Barmer in Bavaria (solid black line) compared to its synthetic counterpart (dashed line), which is constructed as a weighted average of all federal states that do not border Bavaria. The vertical dashed line marks calendar week 41, the week of the election. Panel (b) includes the ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Bavaria (BY, red) and all other federal states that do not border Bavaria (BE for Berlin, BB for Brandenburg, HB for Bremen, HH for Hamburg, NS for Lower Saxony, MV for Mecklenburg-Western Pomerania, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SA for Saxony-Anhalt and SH for Schleswig-Holstein).

Figure A.2: Sick leaves for Hessian election in October 2018 - No neighboring states

(a) Sick leaves for Hesse and Synthetic Hesse



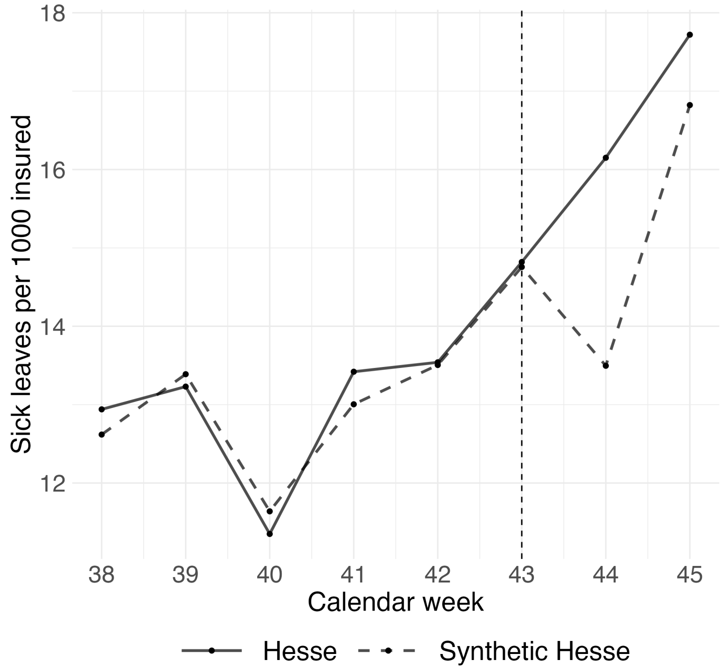
(b) Ratios of post- to pre-treatment MSPE



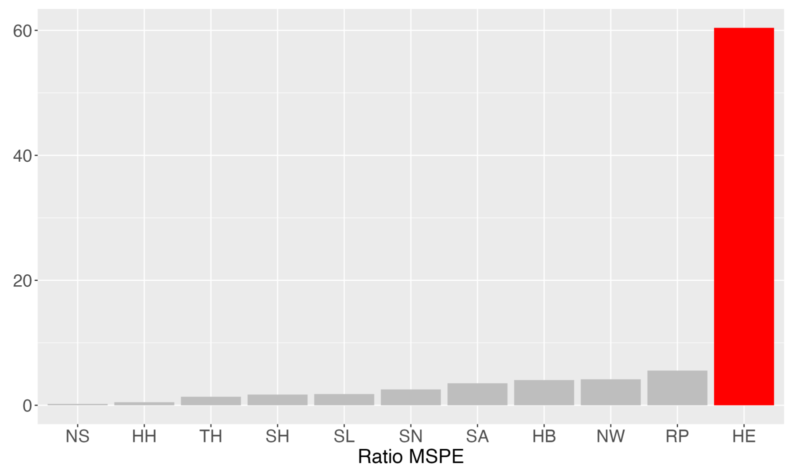
Note: Panel (a) illustrates the weekly trend in sick leave cases per 1,000 individuals insured by Barmer in Hesse (solid black line) compared to its synthetic counterpart (dashed line), which is constructed as a weighted average of all federal states that do not border Hesse. The vertical dashed line marks calendar week 43, the week of the election. Panel (b) includes the ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Hesse (HE, red) and all other federal states that do not border Hesse (BE for Berlin, BB for Brandenburg, HB for Bremen, HH for Hamburg, MV for Mecklenburg-Western Pomerania, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt and SH for Schleswig-Holstein).

Figure A.3: Sick leaves for Hessian election in October 2018 - No states with holidays

(a) Sick leaves for Hesse and Synthetic Hesse



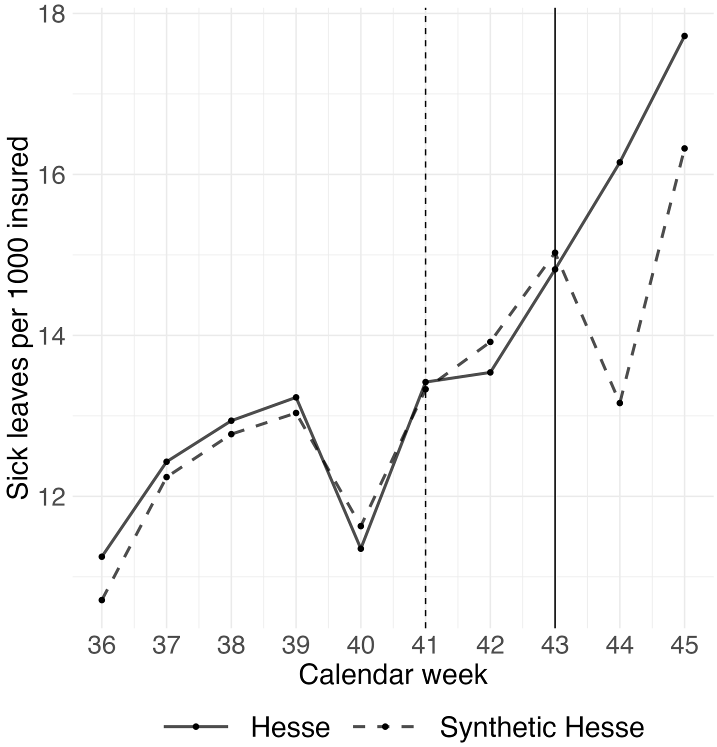
(b) Ratios of post- to pre-treatment MSPE



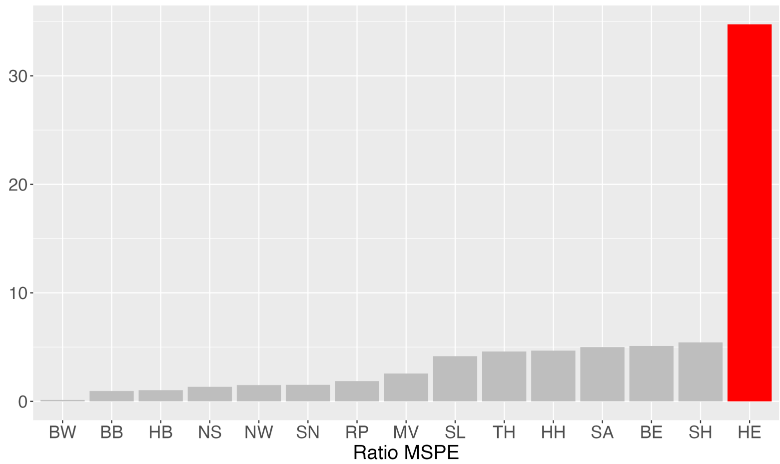
Note: Panel (a) illustrates the weekly trend in sick leave cases per 1,000 individuals insured by Barmer in Hesse (solid black line) compared to its synthetic counterpart (dashed line), which is constructed as a weighted average of all federal states that did not have fall holidays in the post-treatment period. The vertical dashed line marks calendar week 43, the week of the election. Panel (b) includes the ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Hesse (HE, red) and all other federal states that did not have fall holidays in the post-treatment period (HB for Bremen, HH for Hamburg, NS for Lower Saxony, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt, SH for Schleswig-Holstein and TH for Thuringia).

Figure A.4: Sick leaves for Hessian election in October 2018 - Placebo election two weeks earlier

(a) Sick leaves for Hesse and Synthetic Hesse



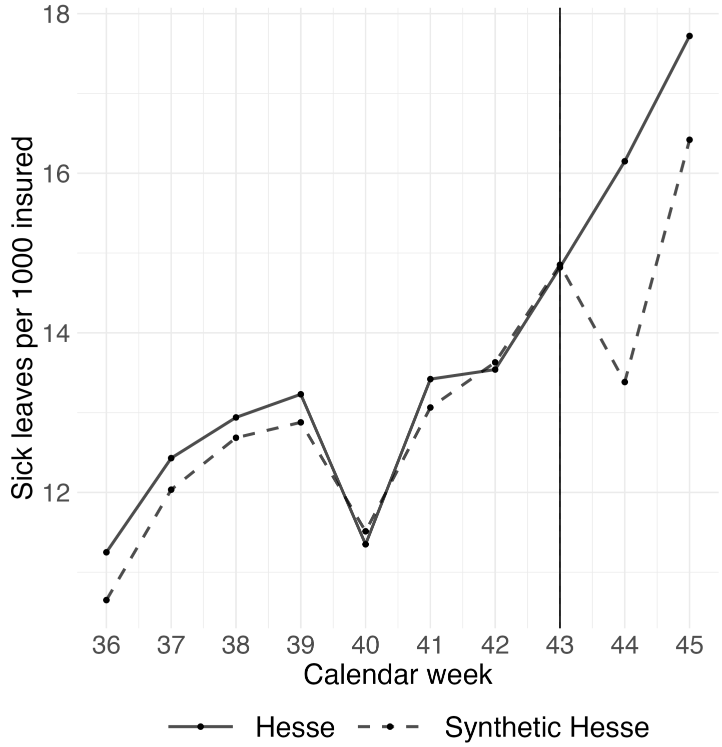
(b) Ratios of post- to pre-treatment MSPE



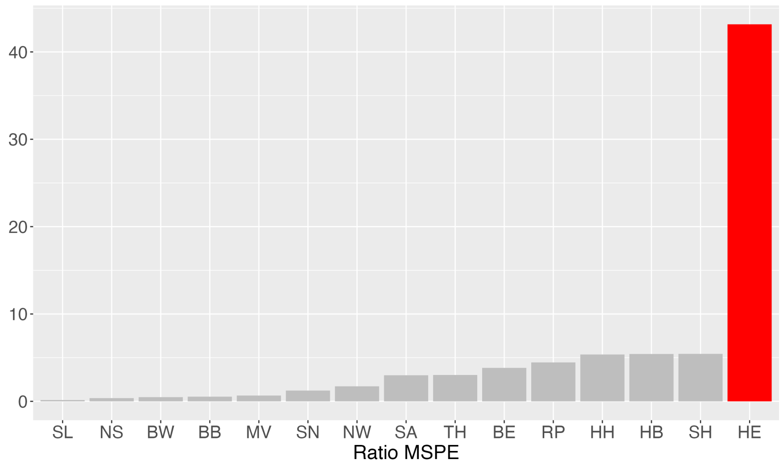
Note: Panel (a) illustrates the weekly trend in sick leave cases per 1,000 individuals insured by Barmer in Hesse (solid black line) compared to its synthetic counterpart (dashed line), which is constructed as a weighted average of all federal states. The vertical dashed line marks calendar week 41, the week of the placebo election. The vertical black line indicates calendar week 43, the week of the actual election. Panel (b) includes the corresponding ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Hesse (HE, red) and all other federal states except Bavaria (BW for Baden-Württemberg, BE for Berlin, BB for Brandenburg, HB for Bremen, HH for Hamburg, NS for Lower Saxony, MV for Mecklenburg-Western Pomerania, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt, SH for Schleswig-Holstein and TH for Thuringia).

Figure A.5: Sick leaves for Hessian election in October 2018 - Longer pre treatment period

(a) Sick leaves for Hesse and Synthetic Hesse



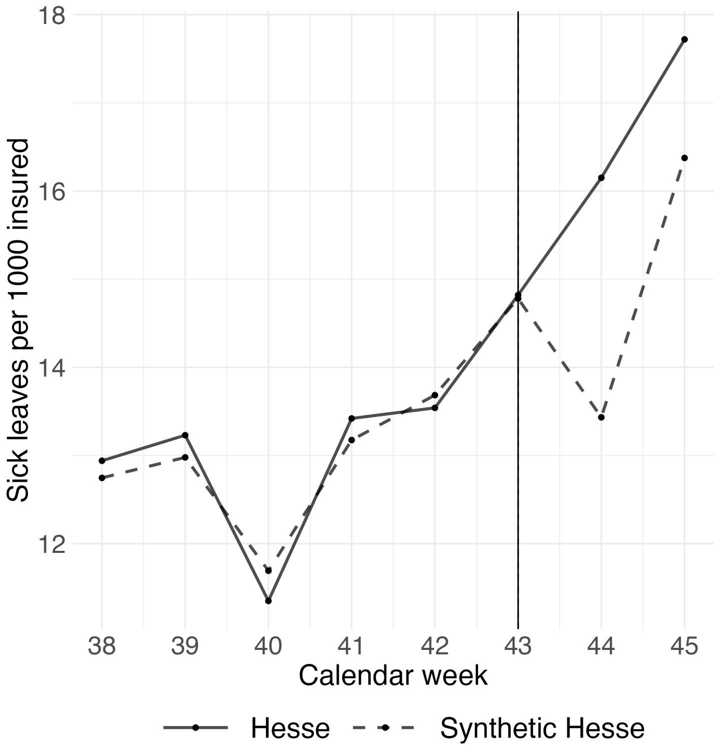
(b) Ratios of post- to pre-treatment MSPE



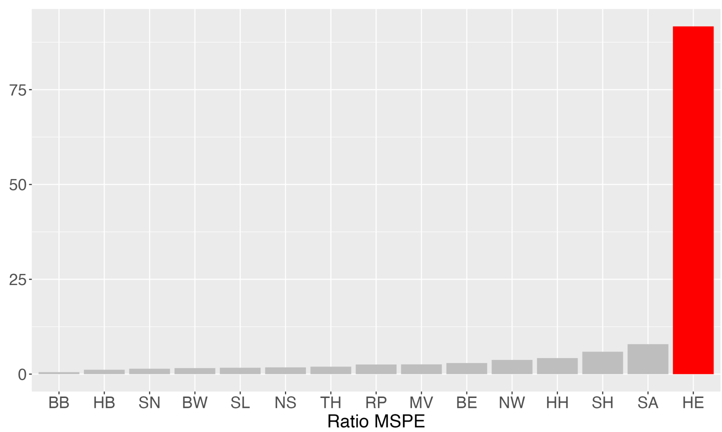
Note: Panel (a) illustrates the weekly trend in sick leave cases per 1,000 individuals insured by Barmer in Hesse (solid black line) compared to its synthetic counterpart (dashed line), which is constructed as a weighted average of all other federal states. The vertical dashed line indicates calendar week 43, the week of the election. Panel (b) includes the corresponding ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Hesse (HE, red) and all other federal states except Bavaria (BW for Baden-Württemberg, BE for Berlin, BB for Brandenburg, HB for Bremen, HH for Hamburg, NS for Lower Saxony, MV for Mecklenburg-Western Pomerania, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt, SH for Schleswig-Holstein and TH for Thuringia).

Figure A.6: Sick leaves for Hessian election in October 2018 - Vaccinations per capita as predictor

(a) Sick leaves for Thuringia and Synthetic Hesse



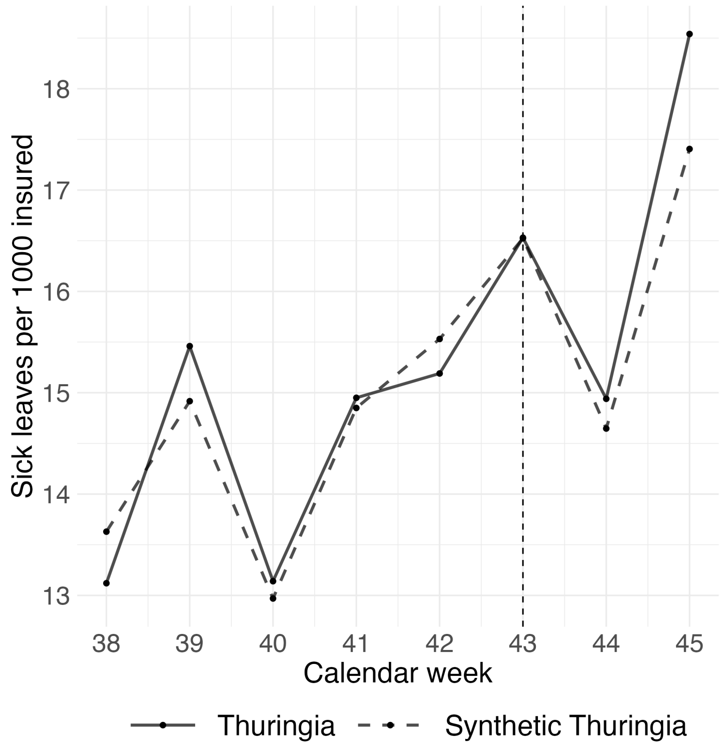
(b) Ratios of post- to pre-treatment MSPE



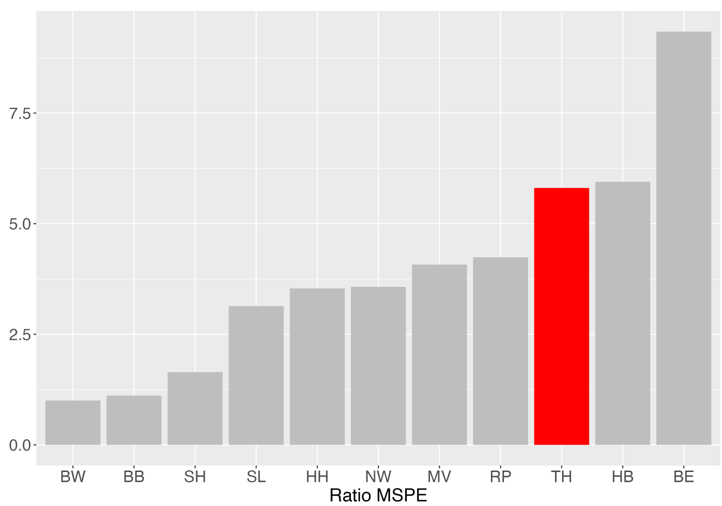
Note: The graph illustrates the weekly trend in sick leave cases per 1,000 individuals insured by Barmer in Hesse (solid black line) compared to its synthetic counterpart (dashed line), which is constructed as a weighted average of all other federal states. For this analysis the number of vaccinations per capita was added as an additional predictor. The vertical dashed line indicates calendar week 43, the week of the election. Panel (b) includes the corresponding ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Hesse (HE, red) and all other federal states (BW for Baden-Württemberg, BY for Bavaria, BE for Berlin, BB for Brandenburg, HB for Bremen, HH for Hamburg, NS for Lower Saxony, MV for Mecklenburg-Western Pomerania, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt, SH for Schleswig-Holstein and TH für Thuringia).

Figure A.7: Sick leaves for Thuringian election in October 2019 - No neighboring states

(a) Sick leaves for Thuringia and Synthetic Thuringia



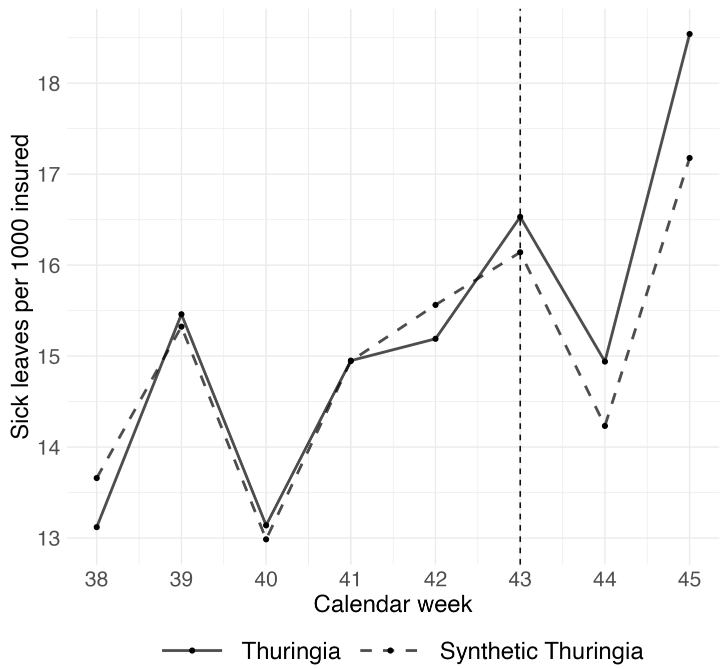
(b) Ratios of post- to pre-treatment MSPE



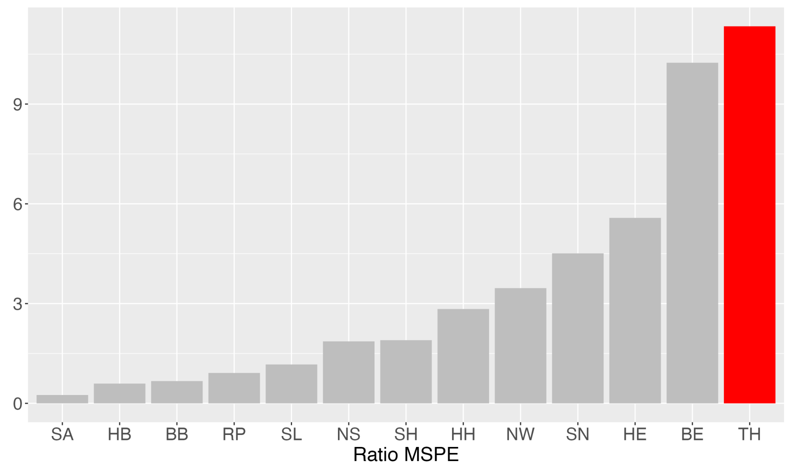
Note: Panel (a) illustrates the weekly trend in sick leave cases per 1,000 individuals insured by Barmer in Thuringia (solid black line) compared to its synthetic counterpart (dashed line), which is constructed as a weighted average of all federal states that do not border Thuringia. The vertical dashed line marks calendar week 43, the week of the election. Panel (b) includes the corresponding ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Thuringia (TH, red) and all other federal states that do not border Thuringia (BW for Baden-Württemberg, BE for Berlin, BB for Brandenburg, HB for Bremen, HH for Hamburg, MV for Mecklenburg-Western Pomerania, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland and SH for Schleswig-Holstein.

Figure A.8: Sick leaves for Thuringian election in October 2019 - No states with holidays

(a) Sick leaves for Hesse and Synthetic Hesse



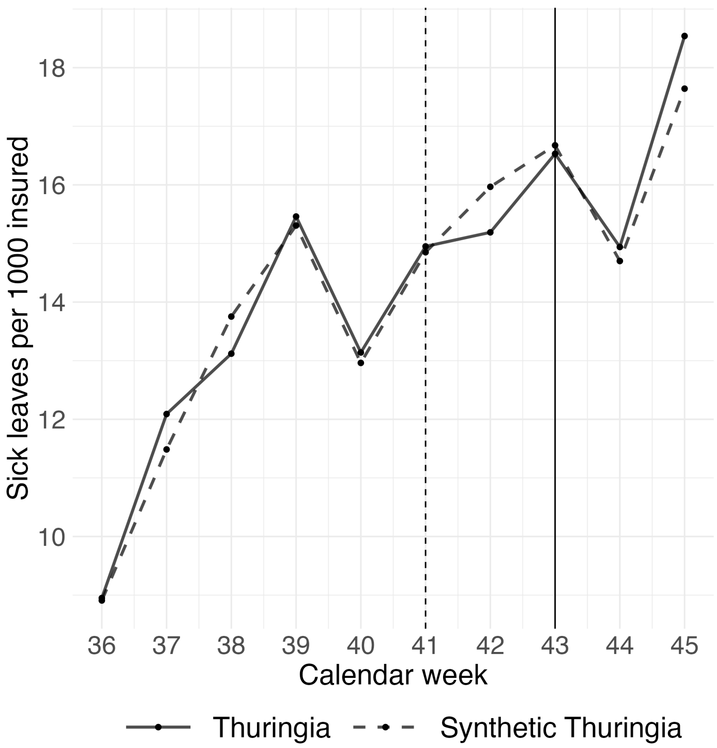
(b) Ratios of post- to pre-treatment MSPE



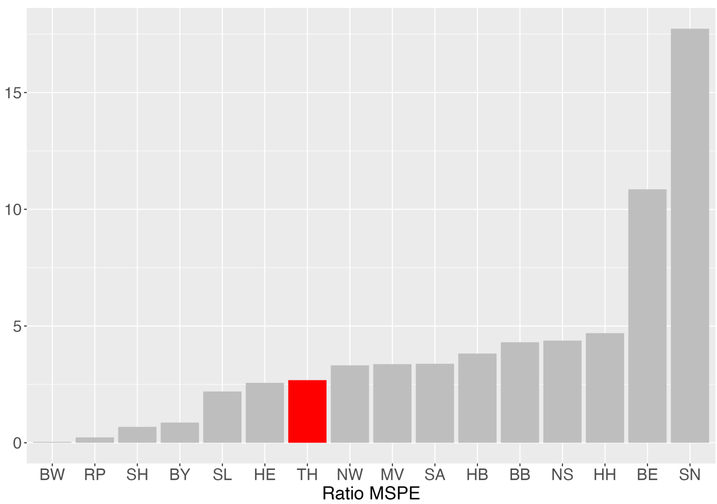
Note: Panel (a) illustrates the weekly trend in sick leave cases per 1,000 individuals insured by Barmer in Thuringia (solid black line) compared to its synthetic counterpart (dashed line), which is constructed as a weighted average of all federal states that did not have fall holidays in the post-treatment period. The vertical dashed line marks calendar week 43, the week of the election. Panel (b) includes the ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Thuringia (TH, red) and all other federal states that did not have fall holidays in the post-treatment period (BE for Berlin, BB for Brandenburg, HB for Bremen, HE for Hesse, HH for Hamburg, NS for Lower Saxony, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt and SH for Schleswig-Holstein).

Figure A.9: Sick leaves for Thuringian election in October 2019 - Placebo election two weeks earlier

(a) Sick leaves for Thuringia and Synthetic Thuringia



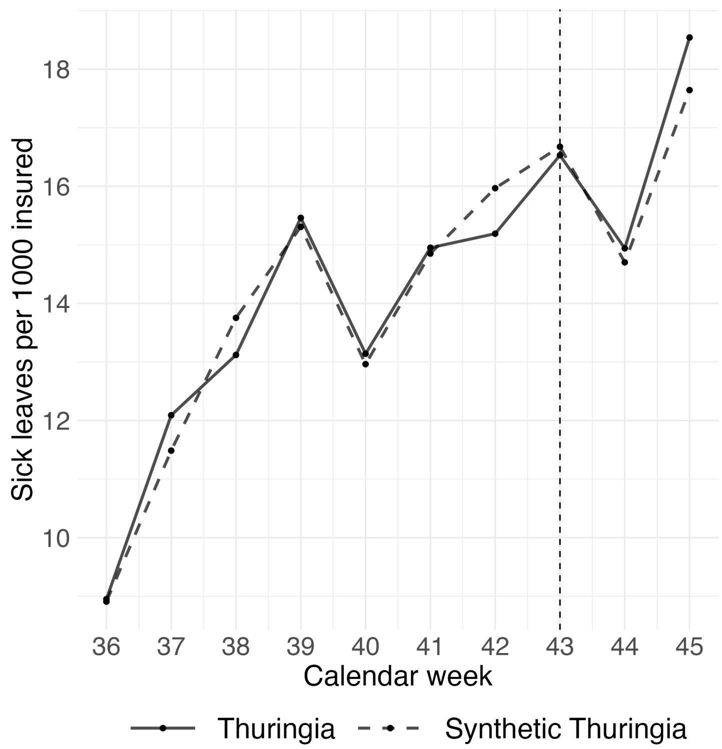
(b) Ratios of post- to pre-treatment MSPE



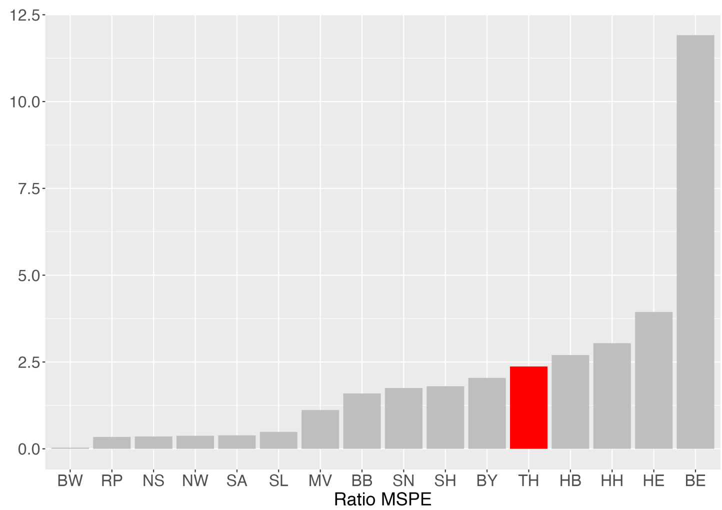
Note: Panel (a) illustrates the weekly trend in sick leave cases per 1,000 individuals insured by Barmer in Thuringia (solid black line) compared to its synthetic counterpart (dashed line), which is constructed as a weighted average of all federal states. The vertical dashed line marks calendar week 41, the week of the placebo election. The vertical black line indicates calendar week 43, the week of the actual election. Panel (b) includes the corresponding ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Thuringia (TH, red) and all other federal states (BW for Baden-Württemberg, BY for Bavaria, BE for Berlin, BB for Brandenburg, HB for Bremen, HE for Hesse, HH for Hamburg, NS for Lower Saxony, MV for Mecklenburg-Western Pomerania, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt and SH for Schleswig-Holstein).

Figure A.10: Sick leaves for Thuringian election in October 2019 - Longer pre treatment period

(a) Sick leaves for Thuringia and Synthetic Thuringia



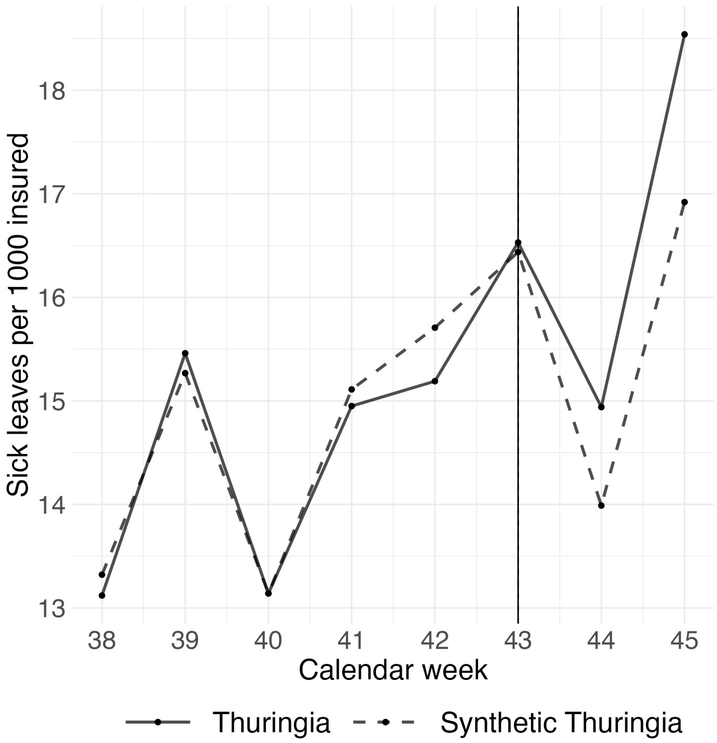
(b) Ratios of post- to pre-treatment MSPE



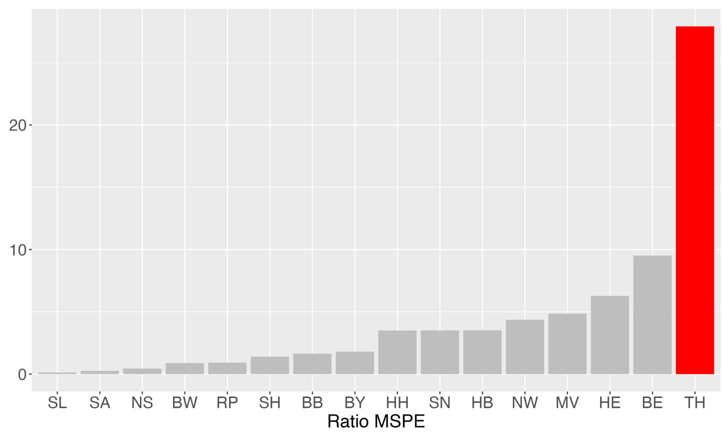
Note: The graph illustrates the weekly trend in sick leave cases per 1,000 individuals insured by Barmer in Thuringia (solid black line) compared to its synthetic counterpart (dashed line), which is constructed as a weighted average of all other federal states. The vertical dashed line indicates calendar week 43, the week of the election. Panel (b) includes the corresponding ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Thuringia (TH, red) and all other federal states (BW for Baden-Württemberg, BY for Bavaria, BE for Berlin, BB for Brandenburg, HB for Bremen, HE for Hesse, HH for Hamburg, NS for Lower Saxony, MV for Mecklenburg-Western Pomerania, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt and SH for Schleswig-Holstein).

Figure A.11: Sick leaves for Thuringian election in October 2019 - Vaccinations per capita as predictor

(a) Sick leaves for Thuringia and Synthetic Thuringia



(b) Ratios of post- to pre-treatment MSPE



Note: The graph illustrates the weekly trend in sick leave cases per 1,000 individuals insured by Barmer in Thuringia (solid black line) compared to its synthetic counterpart (dashed line), which is constructed as a weighted average of all other federal states. For this analysis the number of vaccinations per capita was added as an additional predictor. The vertical dashed line indicates calendar week 43, the week of the election. Panel (b) includes the corresponding ratios of the post- to pre-treatment mean squared prediction error (MSPE) for Thuringia (TH, red) and all other federal states (BW for Baden-Württemberg, BY for Bavaria, BE for Berlin, BB for Brandenburg, HB for Bremen, HE for Hesse, HH for Hamburg, NS for Lower Saxony, MV for Mecklenburg-Western Pomerania, NW for North Rhine-Westphalia, RP for Rhineland-Palatinate, SL for Saarland, SN for Saxony, SA for Saxony-Anhalt and SH for Schleswig-Holstein).

Table A.7: Percentage of Barmer-insured individuals relative to the total population in German states as of December 31, 2019.

State	**Percentage (%)**
Baden-Württemberg	7.2
Bavaria	9.3
Berlin	12.8
Brandenburg	17.9
Bremen	5.7
Hamburg	9.9
Hesse	12.2
Lower Saxony	10.7
Mecklenburg-Western Pomerania	16.9
North Rhine-Westphalia	12.5
Rhineland-Palatinate	11.2
Saarland	12.4
Saxony	8.6
Saxony-Anhalt	12.5
Schleswig-Holstein	13.3
Thuringia	10.2

Note: The table shows the proportion of BARMER-insured individuals as a percentage of the total population in each federal state (Bundesland) in Germany as of December 31, 2019. Source: BARMER Daten 2019 and Destatis 2020, (Rädel et al., 2021).

Impacts of Natural Disasters on Infectious Diseases: A Study of the 2021 Flood Disaster in Germany

Gerrit Stahn   Felix Zwies

Abstract

On July 14, 2021, central and Western Europe experienced heavy rainfall, leading to severe floodings in multiple regions. This study examines whether the resulting floodings contributed to the spread of COVID-19 in the affected areas of Germany. Our research expands the existing literature on the impact of natural disasters on disease transmission and the dynamics of COVID-19 spread during mass gatherings. Using a synthetic control approach, we compare the average weekly number of newly reported COVID-19 cases and the weekly averages of intensive care unit patients across flood-affected German districts with a synthetic control group composed of unaffected districts. Our findings show that COVID-19 case trends diverged between affected districts and their synthetic counterparts - indicating a positive effect of natural disasters on the spread of respiratory diseases. However, the impact on ICU admissions is less conclusive.

Keywords: COVID-19, Natural disasters, Floods, Pandemic, Compound hazards, Synthetic control method

JEL classification: H12, I12, I18, Q54

4.1 Introduction

As our world continues to evolve and faces challenges such as climate change and urbanization, the severity of natural disasters has increased (Paprotny et al., 2018). The concentrated clustering of population and infrastructure in vulnerable areas increases their susceptibility to the adverse consequences of these catastrophes (Tate et al., 2021). As a result, these disruptions create favorable conditions for the spread of infectious diseases (Suk et al., 2020). Understanding the dynamics of compound hazards, including how natural disasters amplify the spread of infectious diseases, such as COVID-19, and their implications, is crucial (Ford et al., 2022), because their understanding enables targeted interventions, resource allocation, and risk mitigation for affected populations while enhancing our comprehension of the complex interactions between natural and social systems. It guides strategies for resilient communities and can minimize future adverse impacts (Zscheischler et al., 2018). Given these considerations the consequences of natural disasters and infectious diseases have become an important research topic in social sciences, like economics.

A notable example of a compound hazard connecting the two areas of research occurred on July 14, 2021, when heavy rainfalls hit Central and Western Europe, leading to a devastating flood that caused destruction in several areas. Germany, in particular, experienced the major burden of storm Bernd, with over 180 lost lives, 766 injured, and severe damage to the infrastructure. The estimated damage to the insured property alone is projected to range between €4.5 and €5.5 billion (GDV, 2022). This flood not only caused immediate devastation but also raised concerns about the potential exacerbation of the at that time ongoing COVID-19 pandemic.

Our research seeks to understand the relationship between natural disasters and the transmission of viral diseases like COVID-19. Therefore, we utilize the Synthetic Control Method (SCM) to examine the transmission dynamics of COVID-19 in Germany following a severe flood, using official epidemiological data from the Robert Koch-Institute (RKI) and the German Interdisciplinary Association for Intensive Care and Emergency Medicine (German: Deutsche Interdisziplinäre Vereinigung für Intensiv- und Notfallmedizin, DIVI).

Despite the tragedy of this natural disaster, it offers valuable insights into the complex dynamics of disease transmission in its aftermath. We take advantage of the unanticipated nature of this natural disaster, which provides us with a quasi-experimental design and allows us to compare affected districts with unaffected districts. In addition, our study benefits from its timing, which occurred in the middle of the pandemic in Germany, in contrast to previous research that focused on the onset of the pandemic. Given the availability of data covering a substantial part of the pandemic period before the disaster, we expect it to enhance the reliability of our results once the SCM effectively aligns the outcome variables for treated and untreated districts prior to the flooding.

Our work contributes to two distinct strands of literature: First, research that has been conducted on the dynamics of COVID-19 transmission at social events characterized by mass gatherings. At these events, the virus can potentially spread more easily due to close interactions and shared environments. Several such instances have been described in the literature that contributed to the spread of COVID-19, or at least raised concerns about transmission of the SARS-CoV-2 virus. These include festivals (e.g., Domènech-Montoliu et al., 2021), sporting events (e.g., Ahammer et al., 2023), elections (e.g., Cassan and Sangnier, 2022; Güntner et al., Forthcoming; Mello and Moscelli, 2022; Palguta et al., 2022), weddings (e.g., Yusef et al., 2020), family gatherings (e.g., Whaley et al., 2021), cruise ship tours (e.g., Willebrand et al., 2022), as well as vacations and tourism (e.g., Felbermayr et al., 2021; Isphording et al., 2021; Mangrum and Niekamp, 2022). Despite not being considered "social events", natural disasters may increase transmission rates by prompting gatherings, such as those between helpers and victims, or due to the necessity of emergency shelters. Thus, analyzing natural disasters in this context can provide valuable insights.

Second, we contribute to the literature that directly examines the impact of natural disasters on the emergence and spread of infectious diseases. For instance, Berariu et al. (2015) highlight that the destruction of public health infrastructure and emergency response systems can leave affected populations more vulnerable by limiting resources and restricting access to healthcare services. Additionally, psychological stress induced by disasters may weaken individuals' immune systems, increasing their susceptibility to infections (Esterwood and Saeed, 2020). Moreover, displacement due to infrastructure damage often leads to overcrowding in emergency shelters, resulting in higher population density and an elevated risk of disease transmission (Charnley et al., 2021). In their review, Suk et al. (2020) analyze the effects of earthquakes and floods in Europe on infectious disease outbreaks. Reports link earthquakes to outbreaks of Salmonella enterica (Nigro et al., 2016), chickenpox (Pérez-Martín et al., 2017), and infectious diseases more broadly (Petrazzi et al., 2013). Similarly, floods are associated with outbreaks of gastrointestinal (Gertler et al., 2015; Harder-Lauridsen et al., 2013; Schmid et al., 2005) and zoonotic (Christova and Taseva, 2016; Desai et al., 2009; Radl et al., 2011; Socolovschi et al., 2011) infectious diseases. Furthermore, Shukla et al. (2018) find that endemic pathogens spread more frequently during hurricanes.

The extent to which natural disasters influence the spread of respiratory diseases like COVID-19 remains largely unclear due to the scarcity of empirical evidence. Murray et al. (2009) and Rath et al. (2011) detect a rise in respiratory symptoms following Hurricane Katrina's impact on the southeastern United States in August 2005. Most existing studies examining the relationship between natural disasters and COVID-19 rely primarily on descriptive analyses.[1] For example, Mavroulis et al. (2021) observe an increase in reported COVID-19 cases only after Cyclone Ianos struck Greece in September 2020, whereas no significant changes are detected following the earthquakes and floods they study. Similarly, Frausto-Martínez et al. (2020) find no direct evidence of a surge in COVID-19 cases during tropical storms. Likewise, Čivljak et al. (2020) and Ćurković et al. (2021) report no immediate rise in COVID-19 cases after earthquakes struck Croatia in March 2020.

Building on the existing literature, our study aims to provide a more comprehensive understanding of how natural disasters influence the spread of airborne diseases. This knowledge is crucial for developing effective risk reduction and resilience strategies for future societies facing both an epidemic and a natural disaster simultaneously.

Our empirical approach monitors the severity of the COVID-19 pandemic in Germany on a weekly basis around the onset of Storm Bernd on July 14, enabling us to examine which phase of the disaster may contribute to worsening respiratory disease transmission. In this context, we propose two potential mechanisms that could facilitate virus spread. First, during and in the immediate aftermath of the storm, individuals may be compelled to stay indoors longer or gather in emergency shelters, potentially creating conditions

[1] Some simulation-based studies have explored the interaction between natural disasters and COVID-19, focusing on forecasting potential scenarios rather than establishing causal links (see Pei et al., 2020; Quigley et al., 2020; Silva and Paul, 2021; Van Wyk de Vries and Rambabu, 2021), as they assume a connection between natural disasters and COVID-19.

that enhance viral spread. Second, the influx of volunteers from outside the affected areas in the subsequent weeks — despite their good intentions — may inadvertently pose an additional health risk to local communities. As demonstrated by Bier et al. (2025, 2022) in their large-scale survey of 2,500 volunteers, approximately 75% began working in the affected areas four days after the flood. Given the average incubation period of 4.41 days (Wu et al., 2022), if volunteers contribute to the severity of COVID-19, we would expect an increase in cases starting around two weeks after the flood.

Our analyses can serve as an initial step in evaluating the economic feasibility of potential countermeasures, such as distributing masks to evacuees and volunteers, to determine whether they can be implemented cost-effectively.

The baseline analysis, which examines COVID-19 cases per 100,000 residents as the outcome variable, indicates that the number of new weekly cases increased by approximately 36% to 93% following the flood in the affected districts. These results suggest that the spread of COVID-19 was more influenced by the influx of helpers rather than the prolonged indoor stay at home or with other evacuees in emergency shelters. However, an alternative specification, which focuses on the daily number of patients in intensive care—chosen to be less susceptible to unobserved influences—does not provide robust evidence of a significant worsening in the severity of the COVID-19 pandemic.

The remainder of the paper is organized as follows: Section 4.2 introduces the data, followed by Section 4.3, which details our methodological approaches. Section 4.4 presents the analysis results, while Chapter 4.5 discusses the findings. Finally, Chapter 4.6 provides the conclusion.

4.2 Data

The analyses presented in this paper are based on district (*Kreis*) level data from Germany. Until July 2021 the Federal Republic of Germany comprised 16 federal states and 401 districts, of which we consider 395[2] for our analyses. Of those 395 districts 289 are rural and 106 are urban districts. 29 of these districts from three different states were marked as affected by the flood by the Federal Office for Civil Protection (*Bundesamt für Bevölkerungsschutz und Katastrophenhilfe*, BBK): Bavaria (2)[3], North Rhine-Westphalia (22)[4] and Rhineland-Palatinate (5).[5] Of those affected, 10 are urban and 19 are rural districts.

Our data was retrieved from different sources. One dataset consists of COVID-19 cases[6] and deaths at the district level for January 2, 2020 through November 28, 2021 from the COVID-19-Dashboard of the RKI.[7] For each positive test result reported to the RKI, the data carry information on the state, district, age group, sex, reporting date, and whether the person tested has recovered or deceased in the meantime. In addition, we use the official data on registered COVID-19 vaccinations from the RKI.

The second dataset records the daily number of COVID-19 patients receiving treatment in intensive care units at the district level from December 27, 2020, to November 28, 2021[8] and was provided by the DIVI.

Rather than relying on cumulative data for daily reported cases, deaths, and ICU pa-

[2]In July 2021 the *Stadtkreis Eisenach* integrated in the *Wartburgkreis*. We excluded those two districts, since some of our resources reported data for both districts individually and some for the Wartburgkreis only. We expect no drawbacks, because analyses for an earlier draft of the paper based on datasets, which reported the two districts separately, show that both districts don't contribute to any synthetic control. Results for those analyses can be provided on request. We had to further exclude four districts for the ICU-specifications (*Rheinland-Pfalz-Kreis, Neustadt an der Waldnaab, Landkreis Coburg* and *Landkreis Fürth*), because they did not report any ICU cases over the whole duration of the available observed time periods.

[3]The affected districts in Bavaria are *Berchtesgadener Land* and *Hof*.

[4]The affected districts in North Rhine-Westphalia are *Städteregion Aachen, Bottrop, Ennepe-Ruhr-Kreis, Hochsauerlandkreis, Oberbergischer Kreis, Rhein-Sieg-Kreis, Mettmann, Heinsberg, Düren, Rheinisch-Bergischer Kreis, Wuppertal, Rhein-Erft-Kreis, Hagen, Mülheim an der Ruhr, Euskirchen, Essen, Köln, Leverkusen, Solingen, Märkischer Kreis, Oberhausen* and *Düsseldorf*.

[5]The affected districts in Rhineland-Palatinate are *Ahrweiler, Vulkaneifel, Bitburg-Prühm, Trier-Saarburg* and *Bernkastel-Wittlich*.

[6]Cases are defined according to the RKI. The terms *cases* and *positively tested* are therefore used synonymously in this paper.

[7]We retrieved the data on November 30, 2021. The most recent data on registered COVID-19 cases and deaths in Germany reported to the RKI are available on GitHub. Due to different sources, these data deviate from those reported by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University.

[8]The data is available through the DIVI ICU register.

tient numbers, we primarily aggregate these figures on a weekly basis for two key reasons. First, daily data is subject to weekly fluctuations in testing and hospital admissions, such as those influenced by weekends. Second, cumulative data continuously increase, making it challenging to assess whether virus transmission is accelerating or decelerating. Weekly figures, in contrast, provide a clearer view of the pandemic's trajectory, particularly in relation to public health measures. Since they are less depend on past infection levels, they offer a more accurate snapshot of the current state of the pandemic in a given region.

To operationalize this approach, we sum the daily reported cases and deaths over each seven-day period, aligning them with the seven-day period of the flood that began on July 14. To account for variations in district population sizes, we express these weekly figures per 100,000 residents.

Unlike the COVID-19 case and death data, the DIVI dataset does not include information on the number of newly admitted ICU patients per day. Consequently, for our analysis, we construct the weekly average of daily COVID-19 ICU patients per 100,000 residents per district.

We use data on German demographic, economic, health care and child care characteristics at the district level as our predictor variables. The data comes from the Federal Statistical Office and the Federal Institute for Building and Regional Planning. Alipour et al., 2023, 2021 shared their data on the share of employees working from home at the district level.[9] Summary statistics for the full list of variables separated by unaffected districts and all affected districts are reported in Table 1.

[9] We are grateful to the respective authors for sharing their data.

Table 1: Summary statistics for outcome and predictor variables at the district level

	Variable	Districts All Unaffected	Districts All Affected
COVID-19	Cases between July 13 and June 16†	21.23 (14.24)	27.62 (10.13)
	Cases between August 10 and July 14†	58.66 (34.04)	101.20 (45.74)
	Deaths between July 13 and June 16†	0.12 (0.39)	0.15 (0.25)
	Deaths between August 10 and July 14†	0.25 (0.65)	0.58 (0.62)
	ICU patients per day between July 13 and June 16†	0.68 (1.25)	0.64 (0.95)
	ICU patients per day between August 10 and July 14†	0.36 (0.72)	0.57 (0.77)
	Primary vaccinations between July 13 and June 16*	8.56 (3.50)	8.32 (1.82)
	Secondary vaccinations between July 13 and June 16*	14.88 (5.58)	15.64 (3.04)
Demographic	Inhabitants per km^2 of settlement and traffic area	1772.94 (1046.96)	2405.77(1240.18)
	Age structure of population		
	% aged under 6	5.57 (0.53)	5.69 (0.32)
	% aged 6–17	10.74 (0.82)	10.84 (0.56)
	% aged 18–24	7.29 (1.47)	7.30 (0.77)
	% aged 25–29	5.53 (1.61)	5.85 (0.94)
	% aged 30–49	24.41 (1.76)	24.08 (1.66)
	% aged 50–64	23.61 (2.07)	23.87 (1.48)
	% aged ≥65	22.85 (3.12)	22.37 (1.76)
	Population development§	3.35 (4.36)	3.47 (3.69)
	Female share of population	50.57 (0.61)	50.87 (0.63)
	Foreign share of population	10.94 (5.53)	13.42 (4.06)
	Religion		
	Share of catholics	0.32 (0.25)	0.48 (0.19)
	Share of protestants	0.32 (0.18)	0.25 (0.13)
Health and Social issues	Child care participation rates		
	% aged 0–2 years	35.94 (12.84)	28.66 (5.41)
	Hospital beds‡	63.01 (39.73)	55.66 (16.17)
	Geriatric demand and supply‡		
	Elderly in need of care	512.33 (125.95)	553.38 (83.39)
	Nursing home places	113.79 (30.70)	107.54 (18.61)
	General physicians†	63.04 (6.20)	60.65 (4.80)

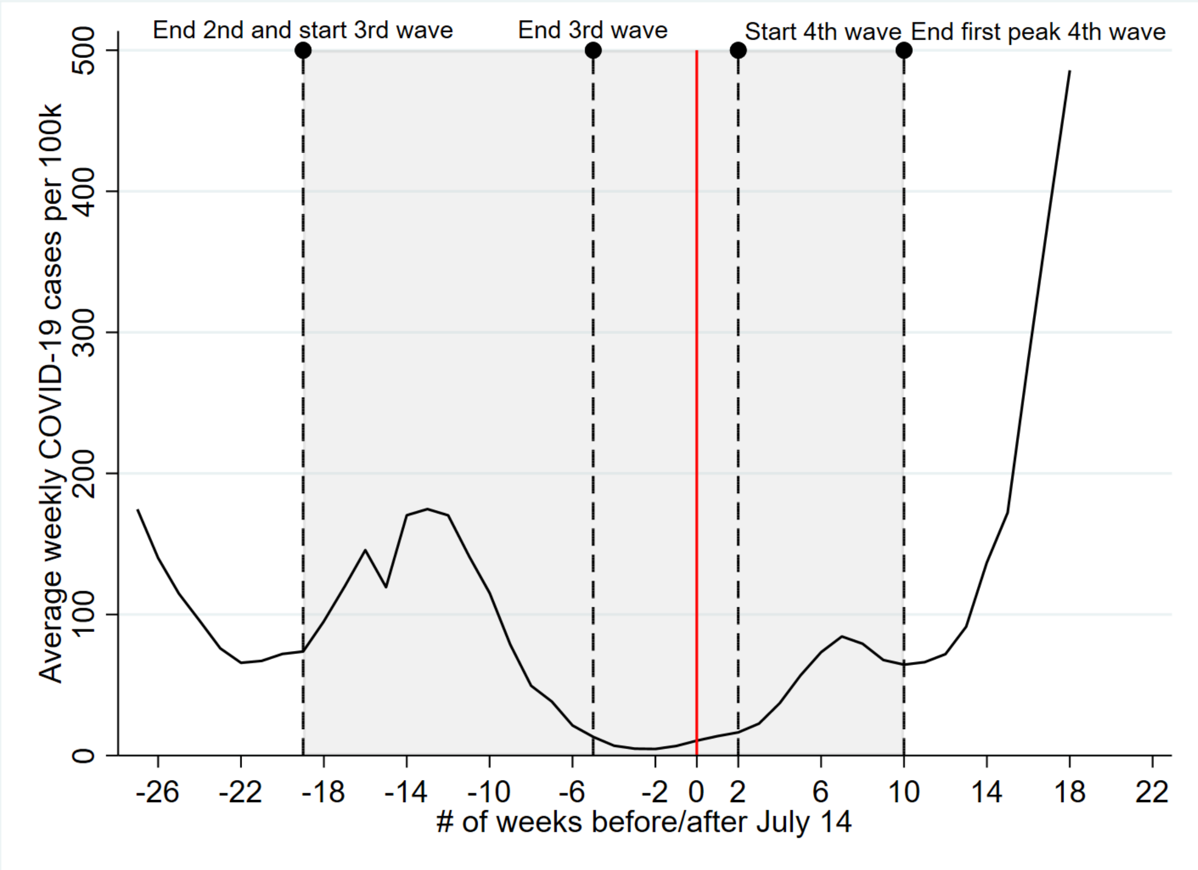
(continues)

Table 1: Summary statistics *continued*

	Variable	Districts: All Unaffected	Districts: All Affected
Economic	Unemployment		
	Unemployment rate in %	5.40 (2.16)	6.75 (2.40)
	Share of unemployed aged 55–64	24.02 (4.51)	22.25 (3.93)
	Unemployment rate women in %	5.01 (2.05)	6.31 (2.30)
	Employment rate	62.87 (4.05)	58.71 (4.28)
	Household income per capita per month	1974.83 (192.23)	1967.44 (149.69)
	GDP per capita♮	38.56 (16.25)	35.60 (11.91)
	Commuters		
	% out	42.23 (13.08)	43.43 (13.64)
	% in	38.64 (14.35)	39.11 (13.13)
	Share of workers with academic degree	14.55 (6.87)	14.26 (6.32)
	Tourism		
	Stays in hotels per capita	4.02 (5.64)	2.56 (4.01)
	Share of stays in hotels by foreigners	10.15 (7.25)	13.89 (8.39)
	Share of workers working from home	23.51 (3.04)	24.06 (3.16)

Note: Unweighted sample means with standard deviation in parentheses. June 16, 2021 (four full weeks prior to the start of the flood) and August 10, 2021 (four full weeks after) serve as the reference dates for the COVID-19 key figures. For instance, the average number of cases per 100,000 residents reported between July 13 and June 16 for all affected districts is equal to 21.23. *ICU patients per day* refers to the average number of ICU patients per day for the respective period. In the SCM analyses we include administrative and structural district-type dummies. † per 100,000 inhabitants, § per 1,000 inhabitants, * per 100 inhabitants, ‡ per 10,000 inhabitants, ♮ in EUR 1,000

Figure 1: Weekly average of COVID-19 cases per 100k



Notes: The y-axis represents the number of weekly registered COVID-19 cases per 100,000 residents averaged over all German districts. The x-axis covers all weeks (starting every Wednesday) from January 6th until November 24th 2021 relative to the 7 day period of the floodings starting on July 14th. The dashed vertical lines mark the end and the start of different COVID-19 periods in 2021 defined by Schilling et al. (2022). The gray area represents the considered period for the SCM analyses.

4.3 Methods

The key COVID-19 indicators presented in Table 1 suggest that, just before the onset of the natural disaster, the spread and severity of the pandemic were relatively low. The average number of COVID-19 cases, deaths, and ICU patients in the four weeks leading up to the flood remained at low baseline levels, with no notable differences between affected and unaffected districts. Following the flood, the average number of COVID-19 cases and deaths increased significantly, at least doubling compared to the pre-flood period. However, the average number of daily ICU patients declined in both groups, though the decrease was less pronounced in the affected districts. Similarly, Figure 1 depicts the different phases of the COVID-19 pandemic in Germany throughout 2021 and shows that the flooding occurred between the conclusion of the third major wave in Germany and the onset of the fourth wave, as defined by Schilling et al. (2022), during a period of relatively low reported case numbers.

This low baseline of COVID-19 spread around the time of the flooding is also evident in the four panels of Figure 2, which depicts the incidence of COVID-19 cases per 100,000 residents across German districts: one week before the floods began on July 14, during the week of the natural disaster, one week after, and four weeks after. A fraction of the affected districts (indicated by red dots) reported relatively high weekly case numbers per 100,000 residents already prior the catastrophe as can be seen in the first panel of Figure 2, which highlights a significant challenge for empirically analyzing the flood's impact on COVID-19 spread.

In total, those figures show that a causal analysis in that context has to account for pre-existing differences in case numbers, as an initially high number of cases is likely to result in higher case numbers in subsequent weeks. We propose that the SCM is a suitable empirical tool for this analysis. SCM allows the construction of a synthetic comparison district for each affected district, taking into account pre-flood values of the outcome variable and a range of other predictor variables.

For our baseline analysis, we apply the SCM for causal inference in comparative case studies, as developed by Abadie and Gardeazabal (2003), Abadie et al. (2010) and Abadie et al. (2015). The potential effect of the floodings, $\delta_{i,t}$, is defined as:

$$\delta_{i,t} = Y_{i,t}^{Treat} - Y_{i,t}^{C} \text{ for all } t > T_0, \tag{1}$$

where $i = 1, ..., 29$ represents the 29 German districts identified by the BBK as affected by the natural disaster at time t. The time period $t = 1, \ldots, T$ is divided into a pre-treatment period, $t = 1, \ldots, T_0$, and a post-treatment period, $t = T_0 + 1, \ldots, T$. In the framework of equation 1, $Y_{i,t}^{Treat}$ denotes the observed outcome for an affected district i at time t, while $Y_{i,t}^{C}$ represents the counterfactual outcome for district i had the intervention not occurred.

Figure 2: Weekly number of COVID-19 cases per 100,000 residents per district

One week before

Week of the flood

One week after

Four weeks after

Notes: Red dots mark districts labeled as treated by the BBK. The class breaks are defined as the respective 75%, 90%, 95% and 99% percentile. We do not report data for the excluded districts mentioned in section 4.2.

The pre-treatment period extends from Wednesday of calendar week 9 (March 3rd) to Tuesday of calendar week 28 (July 13th), the day before the onset of storm Bernd. This time frame is selected for two main reasons. First, the reliability of SCM hinges on its capacity to accurately mirror the trajectory of the outcome variable over a sufficiently long period prior to the intervention (Abadie, 2021; Abadie et al., 2010), which in this case refers to the natural disaster. Second, this period fully captures the third COVID-19 wave as well as four of the seven weeks of the summer plateau (Schilling et al., 2022). As illustrated in Figure 1, the dynamics of COVID-19 cases differ between these phases, with relatively high case numbers during the third wave and lower numbers during the summer plateau. By covering both phases with different underlying transmission dynamics, we create a donor pool where a strong pre-treatment fit would enhance the reliability of the results.

The post-treatment period ends on Tuesday of calendar week 39 (September 28th), marking the conclusion of the summer COVID-19 wave (Schilling et al., 2022, see Figure 1). Extending the post-treatment period would not enhance our analysis, as any observed differences in outcomes over a longer time frame are likely influenced by other idiosyncratic changes.

Since the $Y_{i,t}^C$ of Equation 1 is unobservable, we approximate it by using a weighted average of outcomes from unaffected districts, $j = 1, \ldots, 366$ (referred to as donor units):

$$Y_{i,t}^C = \sum_{j=1}^{366} w_j \cdot Y_{j,t}. \tag{2}$$

The weights $\{w_1, \ldots, w_{366}\}$ are determined for each affected district i by solving the following minimization problem subject to $w_j \geq 0$ for $j = 1, \ldots, 366$ and $\sum_{j=1}^{366} w_j = 1$:

$$\left[\sum_{m=1}^{N} v_m \left(X_{i,m} - \left(\sum_{j=1}^{366} w_j X_{j,m} \right) \right)^2 \right]^{\frac{1}{2}}. \tag{3}$$

Here, v_m represents the importance of the mth variable in the set of N predictors (X_m, with $m = 1, \ldots, N$) used to measure the similarity between treated and donor districts. In addition to the variables listed in Table 1, lagged values of each outcome variable are included as predictors. The matrix V, which reflects the relative importance of predictors, is estimated for each treated district by minimizing the mean squared prediction error during the pre-treatment period:

$$\min \left[\sum_{t=1}^{T_0} (Y_{i,t} - (\sum_{j=1}^{366} w_i(V) Y_{i,t}))^2 \right]. \tag{4}$$

Following Cavallo et al. (2013), we extend the one-unit synthetic treatment analysis by calculating results for each of the 29 affected districts, excluding them from the donor pool during their respective analyses. We then compute the average treatment effect on the treated (ATT) as:

$$ATT_t = \frac{1}{29} \sum_{i=1}^{29} \delta_{i,t}. \tag{5}$$

The statistical significance of the results is evaluated through placebo tests by estimating the model for each unaffected district as if it were treated. If the distribution of placebo effects shows many effects as large as the ATT estimate for affected districts, the observed effect is likely due to chance. The resulting p-values from the placebo test can be quite large if the treated and donor units are not well matched during the pre-treatment period. To mitigate this, we adjust (standardize) the estimated effects based on their respective pre-treatment match quality.[10]

As a robustness check, we further examine the development of our outcome variables using linear regression models. For these regressions, we classify the outcome variables Y into two groups. The first group includes the differences ($Y_{i;Tue}$) in the number of newly reported COVID-19 cases per 100,000 residents between July 14 (the onset of the flood) and each subsequent Tuesday until September 28.[11] The second group comprises the average daily number of ICU patients with COVID-19 per 100,000 residents for each period between July 14 and any subsequent Tuesday until September 28.

To account for the varying pandemic phases across districts at the time of the natural disaster, we control for the number of registered COVID-19 cases or ICU patients recorded between June 16 and July 13 (the four weeks preceding the flood). Consistent with the SCM approach, we also include controls for demographic, economic, health, and childcare characteristics at the district level, the share of employees ever working from home (Alipour et al., 2023, 2021), and the number of primary and secondary vaccinations administered in each district. This leads to the following general regression model for

[10] For our SCM analysis, we use the STATA packages `synth` (Abadie et al., 2015) and `synth_runner` (Galiani & Quistorff, 2017).

[11] For example, $Y_{i;2}$ includes all newly registered cases between July 14 and the second Tuesday (July 27) after.

each district d:

$$\begin{aligned} Y_{d,Tue} = \alpha + \beta_1 \cdot tr_{BBK} + \gamma_1 \cdot Y_{d,\text{June 16–July 13}} + \gamma_2 \cdot Primary_{d,\text{June 16–July 13}} \\ + \gamma_3 \cdot Secondary_{d,\text{June 16–July 13}} + \mathbf{X}_d \cdot \delta + \varepsilon_d \end{aligned} \tag{6}$$

where α represents a common intercept, γ denotes the coefficients for the number of COVID-19 cases or ICU patients between June 16 and July 13, as well as for the primary and secondary vaccinations in district d during the same period, and δ is a vector of coefficients associated with the district-level control variables contained in $\mathbf{X}_d$. The coefficient β_1 corresponds to the binary variable indicating whether a district d was affected by the flood, as identified by the BBK. The models described in equation (6) are estimated using ordinary least squares (OLS).

4.4 Results

This section presents the empirical evidence based on the econometric approaches discussed above.

Figure 3 displays the synthetic control estimation results for the districts affected by flooding beginning on July 14 for the weekly number of COVID-19 cases per 100,000.[12] Panel a) provides some insights: First, the development of the weekly COVID-19 cases for the average of the affected districts as well as their synthetic counterparts does follow the general average development of all German districts displayed in Figure 1. Second, comparing the evolution of cases for an average affected district with the development for an average synthetic control district shows small deviations between the average affected and the average synthetic district during the pre-treatment period. The overall pre-treatment *root mean squared prediction error* (RMSPE) is around 12.47. Third, we observe a widening gap beginning two weeks after the onset of the flood. A delay of some length between the start of the natural disaster and the rise in cases is expected, given that the estimated median incubation period for the Delta variant of the SARS-CoV-2 virus is 4.41 days (Wu et al., 2022).[13] Considering the incubation period, if events in

[12]The donors receiving the biggest weights for each affected district can be seen in Table A.1. While all districts have more than three contributing donors, the top three mostly account for at least two-thirds of the total weight.

[13]Around that time, the *Delta* variant was increasingly replacing the *Alpha* variant in Germany (see,

the immediate aftermath of the disaster (such as prolonged indoor stays and gatherings in emergency shelters) increased virus transmission, we would expect the gap to start widening as early as week 0 (the week of the catastrophe) or at least in the week after. However, the estimated effect, measured as the difference in the average number of weekly reported cases between the affected districts and their synthetic counterparts, begins at relatively low level of approximately 1.50 cases (compared to an average of 13.05 in the synthetic districts) in week 0. The difference starts to increase to 5.80 cases only after the second week after the onset of the flood (synthetic districts: 16.02), continues rising to 19.53 cases by the fifth week (synthetic districts: 42.47), and peaks at 39.53 cases in the sixth week (synthetic districts: 42.47) before declining sharply thereafter.

Similarly, the results from the permutation test presented in Panel b) of Figure 3 show an immediate decline in the p-value to almost zero after two weeks, where it stays around zero until 8 weeks after the floodings. The p-values together with the expected delay between getting infected and getting tested positive suggest that an event was happening in Mid-July 2021, which led to a significant increase of registered COVID-19 cases in the affected areas.

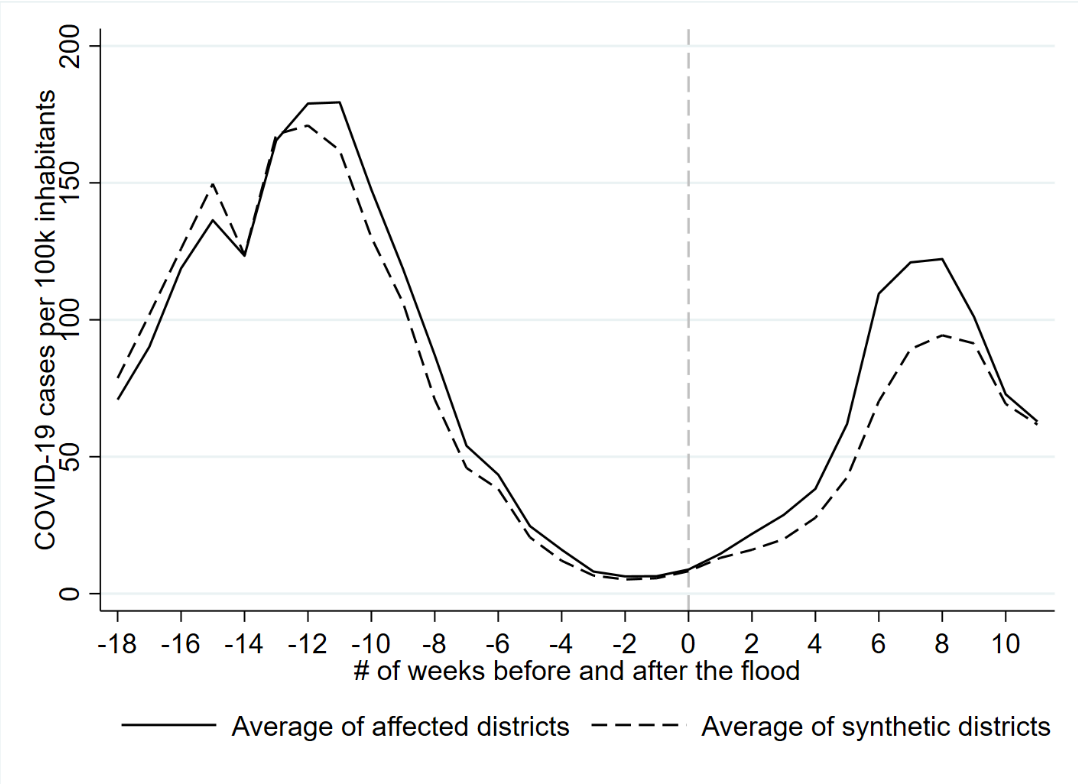
Figure A.1 illustrates the estimated effects for each of the 29 affected districts considered. While most districts exhibit a pattern similar to Figure 3, some, such as Hof and Oberbergischer Kreis, demonstrate a poor pre-treatment fit. Therefore, to further assess the validity of the estimated effects, we utilize the ratio of the RMSPE from the pre-treatment period to the RMSPE after the flood. This ratio serves as a robust measure because, as noted by Abadie et al. (2015, p. 505), "[a] large post-intervention RMSPE is not indicative of a large effect of the intervention if the synthetic control does not closely reproduce the outcome of interest prior to the intervention. That is, a large post-intervention RMSPE is not indicative of a large effect of the intervention if the pre-intervention RMSPE is also large."

Figure 4 presents two boxplots: One for the ratio derived from the placebo SCMs, where non-affected districts are treated as if they were affected, and one for the actual affected districts. As shown in the left boxplot, approximately 75% of affected districts have a ratio greater than or equal to 1, while around 25% exhibit a ratio suggesting a twofold larger post-flood variation compared to pre-flood differences.

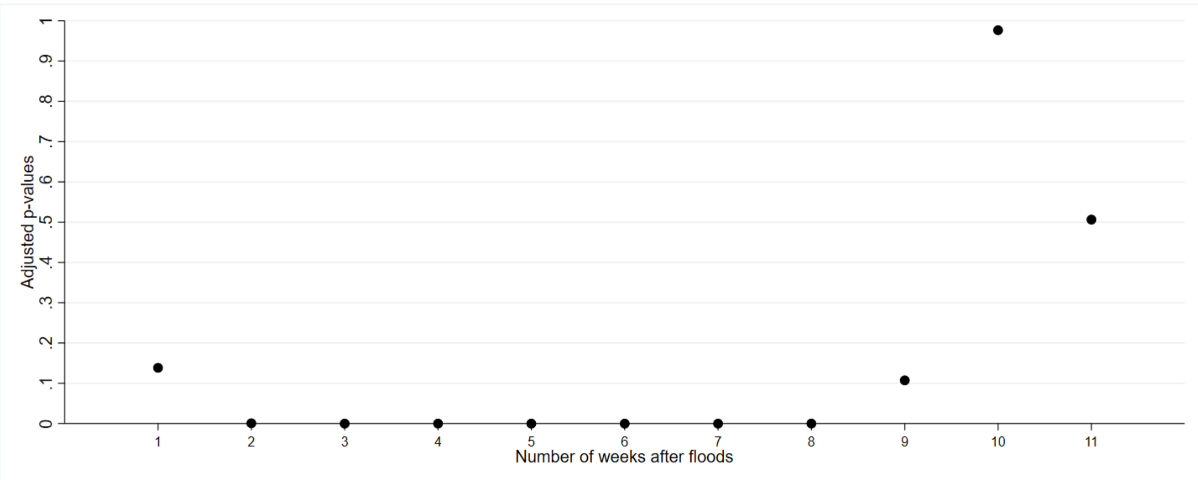
e.g. Schilling et al., 2022)

Figure 3: COVID-19 cases per 100,000 residents

a) Averages of Weekly COVID-19 cases: Affected and Synthetic Districts



b) Adjusted p-values

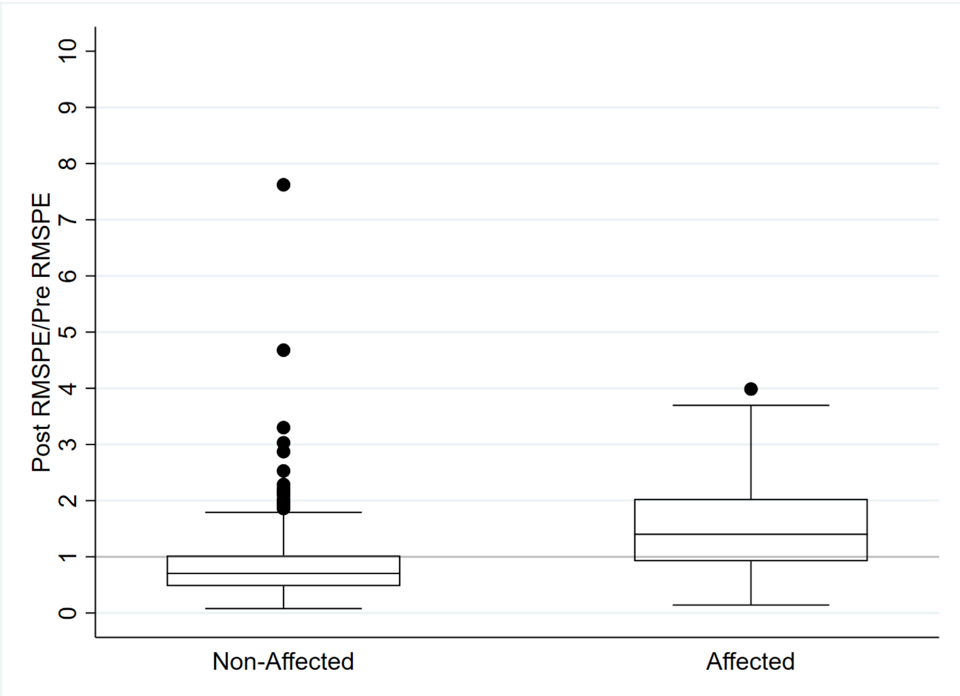


Note: Panel a) plots the weekly average of COVID-19 cases per 100,000 residents for all 29 affected districts (straight line) against the same average from all synthetic counterparts (dashed line). The x-axis represents the distance in weeks from the week of the flood (marked with a vertical dashed gray line). Panel b) shows the weekly p-values for the estimated effect. The p-values represent the proportion of average control units with an estimated effect at least as large as that of the average treatment unit. The values are adjusted for the pre-treatment fit.

Conversely, about 25% of affected districts report a ratio below 1, indicating that the observed differences in COVID-19 cases between the district and its synthetic counterpart after the flood are within the range of pre-flood deviations.

Moreover, some non-affected districts report relatively high ratios.[14] Nonetheless, the quartiles for affected districts are consistently higher than those for non-affected districts. A one-sided t-test strongly rejects the null hypothesis that the mean ratios are equal, supporting the alternative hypothesis that affected districts exhibit significantly higher ratios (p-value = 0.0000).

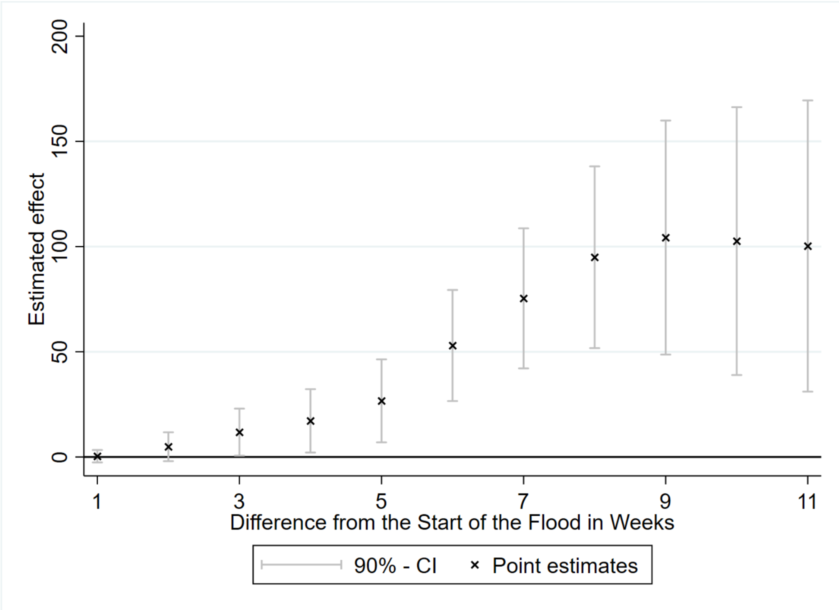
Figure 4: Boxplots for the RMSPE-Ratio – COVID-19 cases



Notes: Graph includes two boxplots of the pre-treatment to post-treatment ratio of the RMSPE for all donor districts and all affected districts.

[14]We explored potential factors contributing to increased case numbers in districts with high ratios but found little anecdotal evidence. Local health offices and other sources, such as newspapers, primarily reported raw COVID-19 case numbers without additional context. One exception was the health office of *Landkreis Rosenheim*, which reported the highest ratio (7.62). Their data indicated that the increase in cases following the flood was due to returning travelers from Southeast Europe visiting family (see Landratsamt Rosenheim, 2021). Since no substantial justification for exclusion was found, we retained these outliers in the baseline analysis.

Figure 5: Estimated Differences in COVID-19 Cases



Notes: This figure presents coefficient estimates and 90% confidence intervals for a dummy variable identifying the 29 districts impacted by the flood. The estimates represent the difference in cumulative COVID-19 cases between July 14 and each subsequent Tuesday until September 28.

Building on the general model in Equation 6, Figure 5 illustrates how the association between the treatment indicator and COVID-19 cases evolves over time. The figure reports coefficient estimates and 90% confidence intervals for the treatment variable, measuring the difference in cases on each Tuesday from July 20 to September 28 relative to July 14, the day storm Bernd began. The findings are in line with those presented in Figure 3. Notably, the association becomes statistically significant at the 10% level on August 3 and remains significant for all subsequent Tuesdays.

While using COVID-19 case numbers may initially appear to be a suitable choice for assessing the impact of the flood on the severity of the pandemic, we chose not to adopt it as our only outcome variable. This decision stems from our expectation that the number of registered COVID-19 cases at this stage of the pandemic was heavily influenced by varying testing behaviors across districts. This would be less of a concern if the flood had no influence on any affected district. We consider this scenario unlikely for three reasons: First, given the potential anticipation of a health threat resulting from the interaction between the flood and the pandemic, increased emphasis may have been placed on testing after the flood. Second, testing infrastructure was likely disrupted in the affected areas, reducing the likelihood of detecting COVID-19 cases. Third, the opportunity costs of testing positive may have risen for residents in the damaged regions. Faced with the overwhelming burden of destruction, affected individuals may simply lacked the time to get tested or to recover from COVID-19.

Another additional concern in this context is the influence of school holidays on infection dynamics. Empirical evidence indicates that summer breaks can contribute to the spread of infectious diseases (Plümper & Neumayer, 2021). During the flood in calendar week 28 (see Figure A.2 in the appendix), the federal states containing the affected districts had either already started their summer school holidays (North Rhine-Westphalia) or were set to begin in the subsequent weeks (calendar week 29 for Rhineland-Palatinate and calendar week 31 for Bavaria).

One potential solution to address these issues would involve using a variable that measures the number of SARS-CoV-2 tests performed. However, to the best of our knowledge, such district-level data is unavailable.[15] An alternative outcome variable that may be less affected by testing discrepancies is the weekly number of COVID-19-related deaths. Nevertheless, as shown in the maps in Figure A.3 in the appendix — and likely due to the already high vaccination coverage among the German population at that time, as reflected in Table 1— the overall baseline for reported deaths per week during this period in Germany was low, with minimal variation across districts. Consequently, any analysis based on COVID-19 death counts would rely disproportionately on a small subset of districts, whether affected or unaffected, that reported slightly higher-than-zero deaths.

Instead, we use the weekly average of COVID-19 patients receiving intensive care per day as another outcome variable. We believe that hospital testing protocols during this time were relatively consistent both over time and across districts. Patients hospitalized with COVID-19 symptoms would have been tested for the virus regardless of whether they had undergone testing prior to hospitalization. Figure 6 presents the weekly average number of ICU patients per day per 100,000 inhabitants around the time of the flooding, once again highlighting a highly conservative context for our research question given the low number of ICU patients.

Figure 7 displays the SCM results for the districts affected beginning on July 14 for

[15] The German National Association of Statutory Health Insurance Physicians (*Kassenärztliche Bundesvereinigung*) provided some data on the number of tests conducted, which was available on their website. However, this data on the federal level is no longer public.

the weekly average of registered COVID-19 patients per day in need of intensive health care.[16] Panel a) provides some insights: First, comparing the evolution of the average number of ICU patients per day for an average affected district with the development for an average synthetic control district suggests a good fit during the pre-treatment period - especially for the weeks close to the week of the flood. The RMSPE for the pre-treatment period is around 0.17. Second, we notice a growing disparity beginning around the week of the flood. At 3 weeks post-flood, the measured average effect is around 87% greater than the synthetic equivalent (0.53 observed patients on average per day to 0.31 average synthetic patients per day), with approximately 0.27 more intensive care patients per 100,000 inhabitants having COVID-19 in that week. The estimated weekly effect increases in size until it peaks of around 0.60 six weeks after the start of the flood.

To assess whether the observed difference could be due to chance, we again conducted placebo-in-space tests. Panel b) reveals that the observed difference for the first two weeks after the beginning of the flood is not unusual given the average of ICU patients per day by the districts in the placebo pool. However, three weeks after the flood the weekly adjusted p-values drop to around zero signaling significant effects. This is in line with the difference between the average synthetic and average affected district becoming more pronounced around three weeks after the start of the flood.

Although we argued that the number of reported ICU COVID-19 patients is more robust against changes in the testing behavior, the variable could suffer from a particular measurement error. As stated by DIVI (see DIVI, 2021), ICU patients with confirmed positive COVID-19 tests could be counted multiple times when transferred from one ICU to another. Each ICU admission, whether new or due to a transfer, is potentially included in the count. While those multiple counts would not threaten our design in general, if there would not be any differences in admission and readmission across affected and unaffected districts, our results in Figure 7 would be biased if the flood itself changed hospital discharge and transfer patterns in the affected regions. Unfortunately, due to the flood, hospitals had to close.[17] This is reflected in the data, as the average number of

[16]The donors receiving the biggest weights for each affected district can be seen in Table A.2 in the appendix.

[17]In its *Report on the 2021 Flood Disaster* (Bericht zur Hochwasserkatastrophe 2021), the German Ministry of the Interior states that five hospitals and two rehabilitation centers in Rhineland-Palatinate, as well as three hospitals in North Rhine-Westphalia, were forced to shut down at least temporarily

reporting units per 100,000 residents, shown in Figure A.4, drops sharply after the onset of the flood, whereas no changes are observable for the averages of all other districts. Consequently, patients requiring intensive care may have been transferred within the affected district, potentially leading to an artificial increase in reported ICU COVID-19 cases. Additionally, Figure A.5 illustrates the average number of ICU beds occupied per 100,000 residents. Similar to the trend observed for reporting ICU units, the average number of occupied ICU beds declines in the affected districts, while it remains largely unchanged on average in all other districts.

At the same time, neighboring or other districts may have expanded their ICU capacities to accommodate the influx of patients from affected areas. This could, in turn, lead to an underestimation of the true effect (ceteris paribus), especially since a substantial share of donors receiving the biggest weights in Table A.2 are neighboring at least one of the affected districts. Although the weekly averages shown in Figure A.4 and Figure A.5 do not provide descriptive evidence of an increase in ICU units or occupied ICU beds per 100,000 residents, the counterfactual development of ICU capacities remains unobservable. Beyond this reasoning, excluding neighboring districts likely reduces potential spillover effects. In the aftermath of the flood, a higher number of infections in the affected districts could lead to increased infections — and consequently, more COVID-19 hospitalizations — in adjacent, unaffected districts.

To address these potential issues, we first identified and subsequently excluded all districts where the number of reporting intensive care units changed in the DIVI data within the three weeks before and after the onset of the flood. Districts that reported changes in ICU units prior to the flood were also excluded to account for the possibility that hospital executives immediately anticipated the effects of the flood and proactively expanded capacity in preparation. Additionally we excluded all districts neighboring at least one affected district. Those steps restrict the sample to 18 affected and 289 non-affected districts. The results for the rerun of the SCM on this restricted pool of districts can be seen in Figure 8. Given the restrictions imposed on the donor pool, the pre-treatment fit only slightly decreases (Pre-RMSPE=0.23). This outcome is expected, as the restrictions exclude districts located closer to the affected areas, which are arguably more comparable to the affected districts in terms of the predictor variables used.[18]

(Bundesministerium des Innern und für Heimat, 2022).

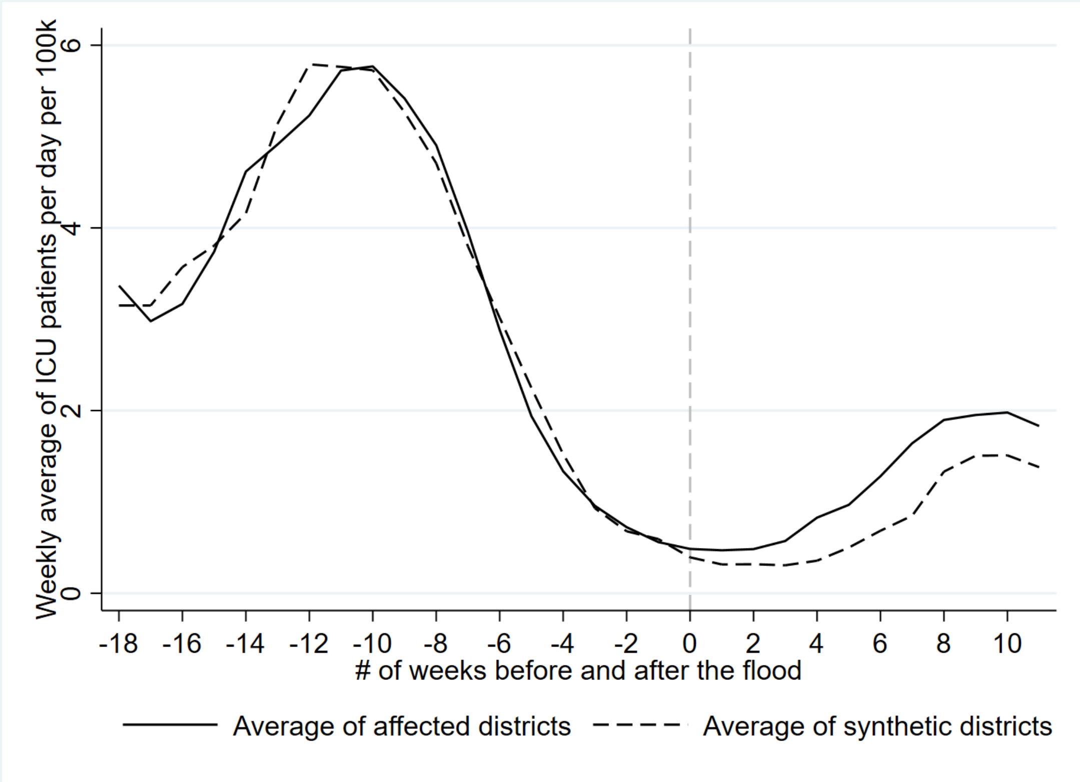
[18]Table A.3 in the appendix again displays the top three donors for each remaining affected district.

Figure 6: Weekly average of ICU patients per day per 100,000 residents per district

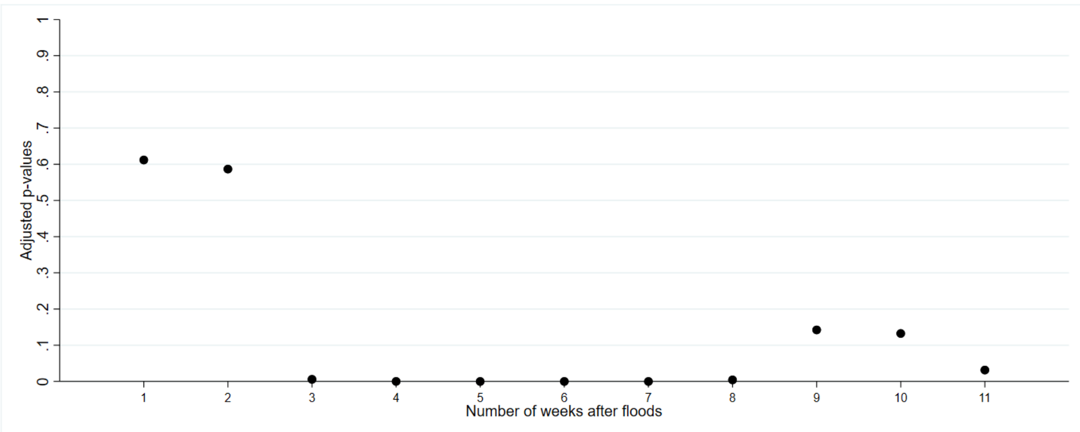
Notes: Red dots mark districts labeled as treated by the BBK. The class breaks are defined as the respective 75%, 95% and 99% percentile. We do not report data for the excluded districts mentioned in section 4.2.

Figure 7: Weekly average of ICU patients per day per 100,000 residents

a) Averages of Weekly COVID-19 patients: Affected and Synthetic Districts



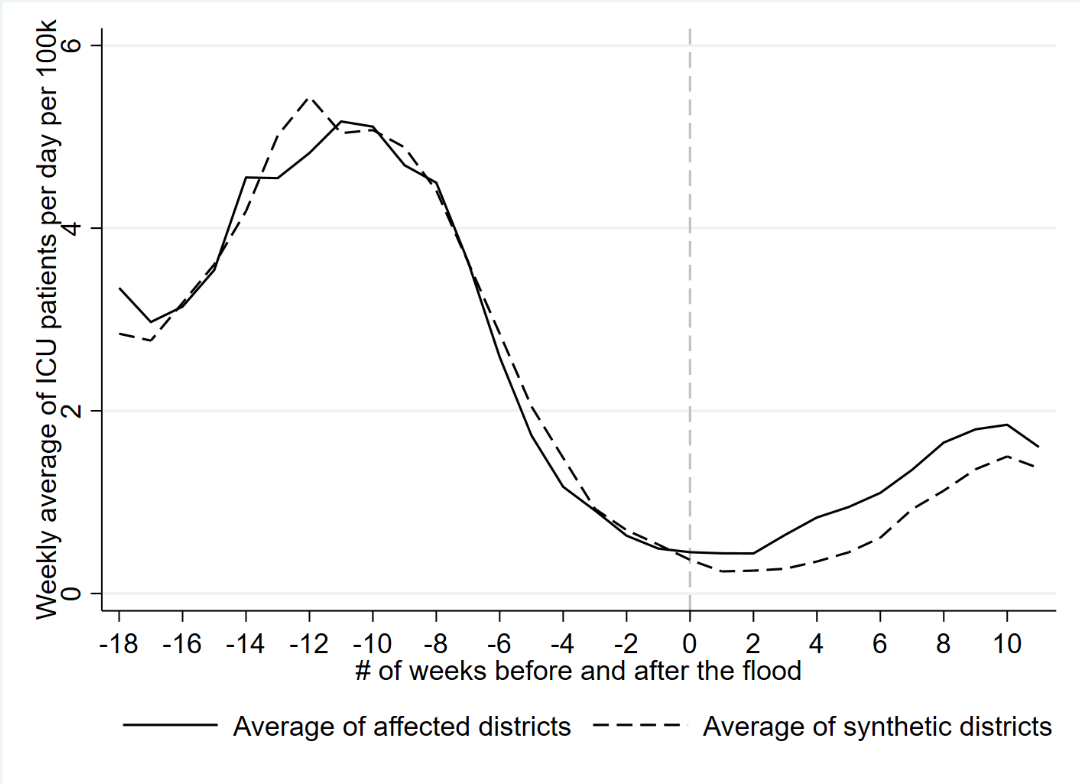
b) Adjusted p-values



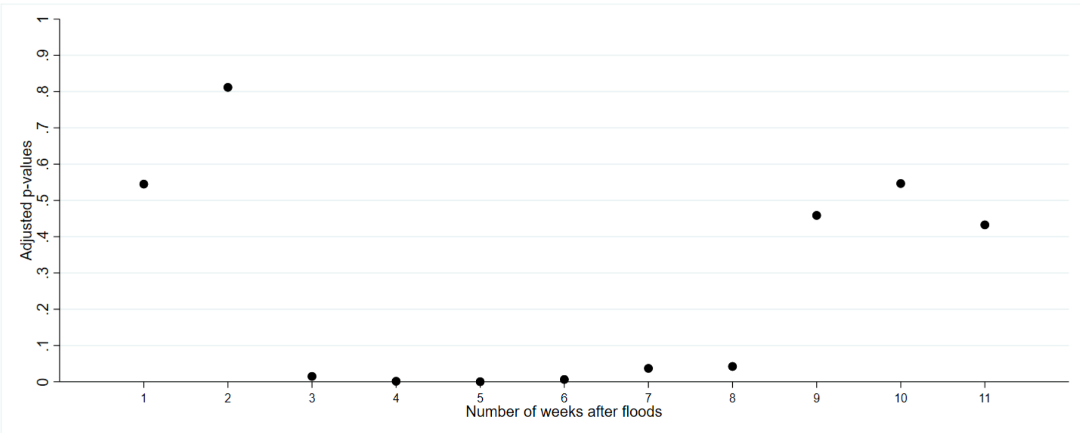
Note: Panel a) plots the weekly average of COVID-19 ICU patients per day per 100,000 residents for all 29 affected districts (straight line) against the same average from all synthetic counterparts (dashed line). The x-axis represents the distance in weeks from the week of the flood (marked with a vertical dashed gray line). Panel b) shows the weekly p-values for the estimated effect. The p-values represent the proportion of average control units with an estimated effect at least as large as that of the average treatment unit. The values are adjusted for the pre-treatment fit.

Figure 8: ICU patients per day - no changes in ICU units and no neighboring districts

a) Averages of Weekly ICU COVID-19 cases: Affected and Synthetic Districts



b) Adjusted p-values



Note: Panel (a) displays the weekly average of COVID-19 ICU patients per day per 100,000 residents across 18 affected districts that did not experience changes in the number of ICU units (solid line), compared to the corresponding average from their synthetic counterparts (dashed line). To construct the donor pool, all districts reporting changes in the number of ICU units, as well as those bordering at least one affected district, were excluded. The x-axis represents the number of calendar weeks relative to the week of the flood, indicated by a vertical dashed gray line. Panel (b) presents the p-values for the estimated effect with adjustments made for pre-treatment fit.

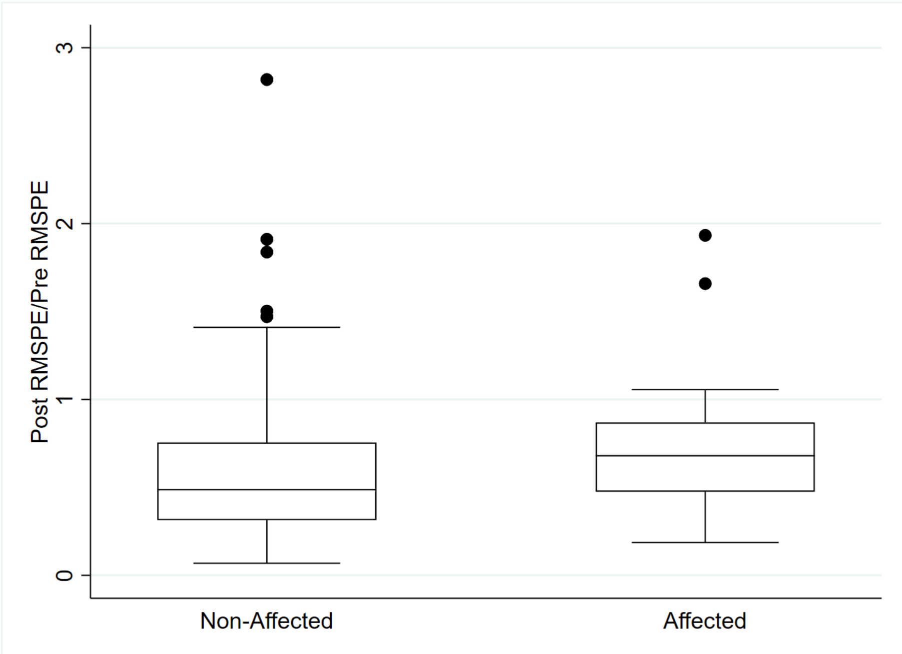
The estimated effects decrease in size. Compared to the previous three weeks post-flood difference, the estimated average effect increases to a difference of around 137% (ca. 0.37 more patients per day compared to an average of around 0.27 for all synthetic counterparts) in that week. At the peak of the difference six weeks after the start of the flood, the estimated difference is equal in size to around 109% of the synthetic average.

As for the specification concerning COVID-19 cases, we again illustrate the individual trajectories of the mean weekly ICU patients per day for each of the 18 affected districts against the development of their synthetic counterparts in Figure A.6. It reveals, while certain districts (such as Wuppertal or Essen) follow a pattern similar to the average treatment effect on the treated observed in Figure 8, others exhibit differences in the pre-treatment fit, post-treatment effects, or both.

Those ambiguous results are further illustrated in Figure 9, which again displays two boxplots of the RMSPE ratios: one for the donor pool districts serving as placebos and another for the 18 affected districts. Although the one-sided t-test, testing the null hypothesis that the mean difference in ratios is zero against the alternative that affected districts have a higher average ratio, results in a p-value of 0.0115, only a small subset of affected districts — Solingen, Essen, Wuppertal, and Rheinisch-Bergischer Kreis — exhibit a ratio above 1. Consequently, 14 out of 18 affected districts report a ratio below this threshold, indicating that a high post-intervention RMSPE does not necessarily suggest a significant effect of the intervention given the relatively high pre-intervention RMSPE.

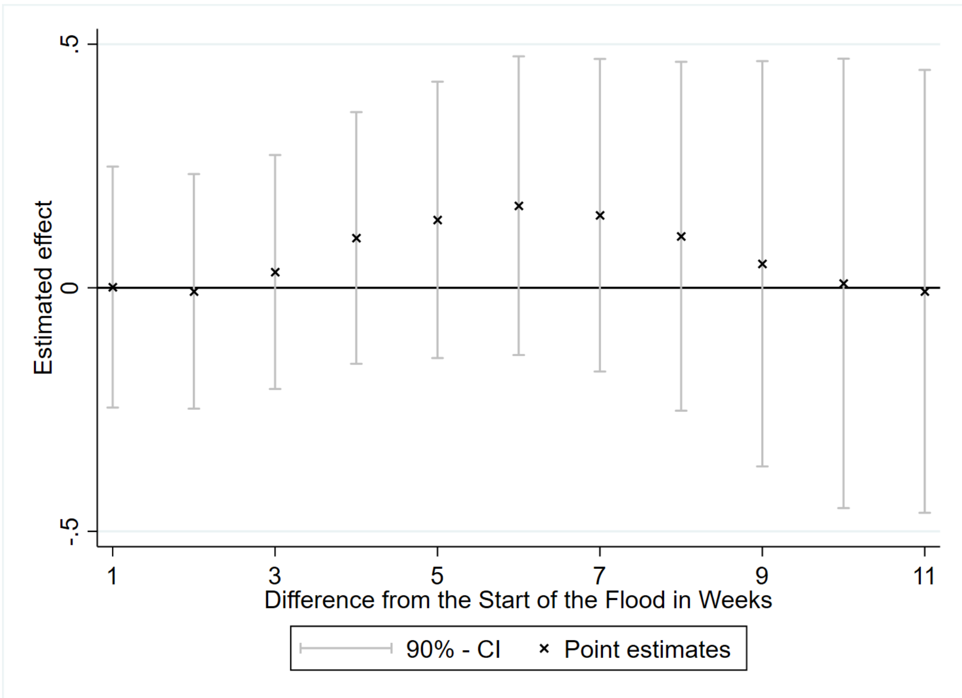
Finally, expanding on the general model in Equation 6, Figure 10 depicts the evolution of the association between the treatment indicator and the average number of ICU patients over time in this sample. The figure presents coefficient estimates along with 90% confidence intervals for the treatment variable. While the average treatment effect increases in magnitude two weeks after the flood, the confidence intervals indicate that the estimated effects are not significantly different from zero.

Figure 9: Boxplots – ICU patients with no change in ICU units and no neighboring districts



Notes: Graph includes two boxplots of the pre-treatment to post-treatment ratio of the RMSPE for all donor districts and all affected districts.

Figure 10: Average in ICU patients per day



Notes: This figure presents coefficient estimates and 90% confidence intervals for a dummy variable identifying the 29 districts impacted by the flood. The estimates represent the difference in the average of ICU patients per day per 100,000 residents for every period between July 14 and each subsequent Tuesday until September 28.

4.5 Discussion

Our data and methodological results provide insights into the potential impact of natural disasters on the spread of infectious diseases during a pandemic. Regarding COVID-19 cases per 100,000 residents, the average difference following the flood that began on July 14 is moderate for the first two weeks (week 0 and week 1 in Figure 3). This suggests either a lack of or a very small immediate direct influence from the catastrophe due to factors such as people staying indoors for extended periods or evacuees being sheltered in confined spaces.

However, a noticeable increase in cases emerges approximately two weeks after the flood, with the magnitude continuing to rise until around week 8. This aligns with our hypothesis that volunteers and aid workers arriving in the affected areas may have contributed to the spread of the virus. After approximately eight weeks, the difference declines sharply, which could be attributed to successful vaccination campaigns initiated in the immediate aftermath of the disaster.[19] Suppose that an individual receives her first dose around one week after the flood and her second shot around six weeks later[20], full immunity against COVID-19 infection (see Andrews et al., 2022) would be reached approximately eight weeks after the flood began.

As already discussed in Chapter 4.4, the results for COVID-19 cases may be affected by unobserved variation in testing behavior. This could introduce bias in two directions: First, restricted access to tests due to damage to testing centers, along with increased opportunity costs of getting tested, may have discouraged individuals in the affected regions from seeking tests. This would likely lead to an underestimation of the true effect, particularly in the immediate weeks following the flood, as testing infrastructure would not have been restored within just a few days. Second, both local authorities and residents were likely aware of the potential threat posed by COVID-19 and may have prioritized testing once the infrastructure was rebuilt. Furthermore, individuals who were unable to get tested in the first week after the flood might have done so later (if they were still symptomatic) once testing facilities reopened. These factors could contribute to an

[19]For instance, local authorities in the affected district of Ahrweiler began vaccinating residents as early as July 20 using mobile vaccination units (eifelschau.de, 2021).

[20]This timeline follows the recommendation in effect at the time by the *Standing Committee on Vaccination at the Robert Koch-Institute* (German: Ständige Impfkommission am Robert Koch-Institut) for the widely used BioNTech/Pfizer vaccine in Germany (Wichmann et al., 2021).

overestimation of the true effect, particularly in the weeks following the initial disruption.

Therefore, we used the average daily number of ICU patients per week as an alternative measure of the severity of the COVID-19 pandemic. In our most conservative approach - excluding neighboring districts that might be affected by spillovers and all districts that experienced changes in the number of reporting ICU units around the time of the flood - we find a small positive average treatment effect on the treated during the first two weeks after the flood. Beyond this period, the ATT increases in magnitude, peaking around the eighth week before gradually declining again. These results further support the impression that the influx of emergency responders likely contributed to an initial rise in pandemic severity, which was successfully mitigated by the immediate vaccination campaigns. However, a closer examination of district-level effects reveals that the overall effect is primarily driven by a small subset of the affected districts. A significant portion of the 18 affected districts showed no unusual effects when compared to their synthetic counterparts, given pre-treatment differences.

Moreover, the ICU-based severity measure may still be influenced by unobserved factors. Although we excluded all neighboring districts not classified as affected by the BBK to minimize potential spillover effects, this does not entirely eliminate the risk of spillovers caused by patient transfers between or within affected districts. While we also dropped districts that reported changes in the number of reporting ICU units, it is likely that hospitals receiving patients from other clinics did not need to establish additional ICU capacity in response to the flood, as the general number of COVID-19 ICU patients was relatively low at that time.

Overall, the timing of the flood provided a highly conservative setting for our analysis. While this was fortunate for those in the affected regions at the time, future natural disasters occurring during periods of higher baseline infection rates or in the absence of available vaccinations could have a much greater impact on the spread and severity of infectious diseases.

4.6 Conclusion

Our research has made contributions to understand the dynamics of infectious diseases during natural disasters, with a particular focus on the relationship between floodings and the spread of SARS-CoV-2. By controlling for factors such as demographics, economics, health care, child care characteristics as well as the disaster's intensity, our results imply that floods can have varying degrees of impact on the spread of infectious diseases.

A central aspect of our study is the implementation of a unique design that enables us to distinguish between treated and control districts affected by a flood in Germany. The unexpected occurrence of the catastrophe during the pandemic provides a quasi-experimental setting to compare affected and unaffected districts and benefits from extensive pre-disaster period to enhance result reliability. This approach allowed us to assess the specific impact of flooding on COVID-19 transmission and severity. However, due to the conservative nature of our setting and the limitations of the available data, we refrain from interpreting these results as causal. Future research utilizing alternative datasets, such as hospital statistics from the Research Data Center of the Statistical Offices of the German Federal States (German: Forschungsdatenzentrum der Statistischen Ämter der Länder), could help establish causal relationships.

Moreover, the hospital statistics could be leveraged to explore potential heterogeneity in effects based on variations in damage intensity across affected regions. For example, integrating claims data from the German Insurance Association (Gesamtverband der Deutschen Versicherungswirtschaft, GDV) for insurances covering natural disaster damages could provide deeper insights into the impact of natural disasters on disease dynamics.

To ensure effective policy guidance for managing compound disasters, it is critical to provide well-evidenced recommendations that take into account the complex dynamics involved. Our research has expanded the knowledge base in this field by providing initial evidence that, rather than gatherings occurring immediately around the time of a flood, those taking place in the following weeks (potentially driven by the influx of aid workers) may noticeably contribute to the spread of airborne diseases such as COVID-19. However, further research should prioritize the exploration of underlying mechanisms to better understand the complexity of transmission dynamics. This will require the establishment of reliable data frameworks and the implementation of rigorous monitoring systems during future disasters and pandemics, ultimately improving the ability to measure disaster preparedness and response. Additionally, exploring optimal strategies for monitoring such complex events is an important area for future research. By continuing to deepen our understanding of these dynamics, societies can improve their ability to develop effective policies and strategies for managing compound disasters.

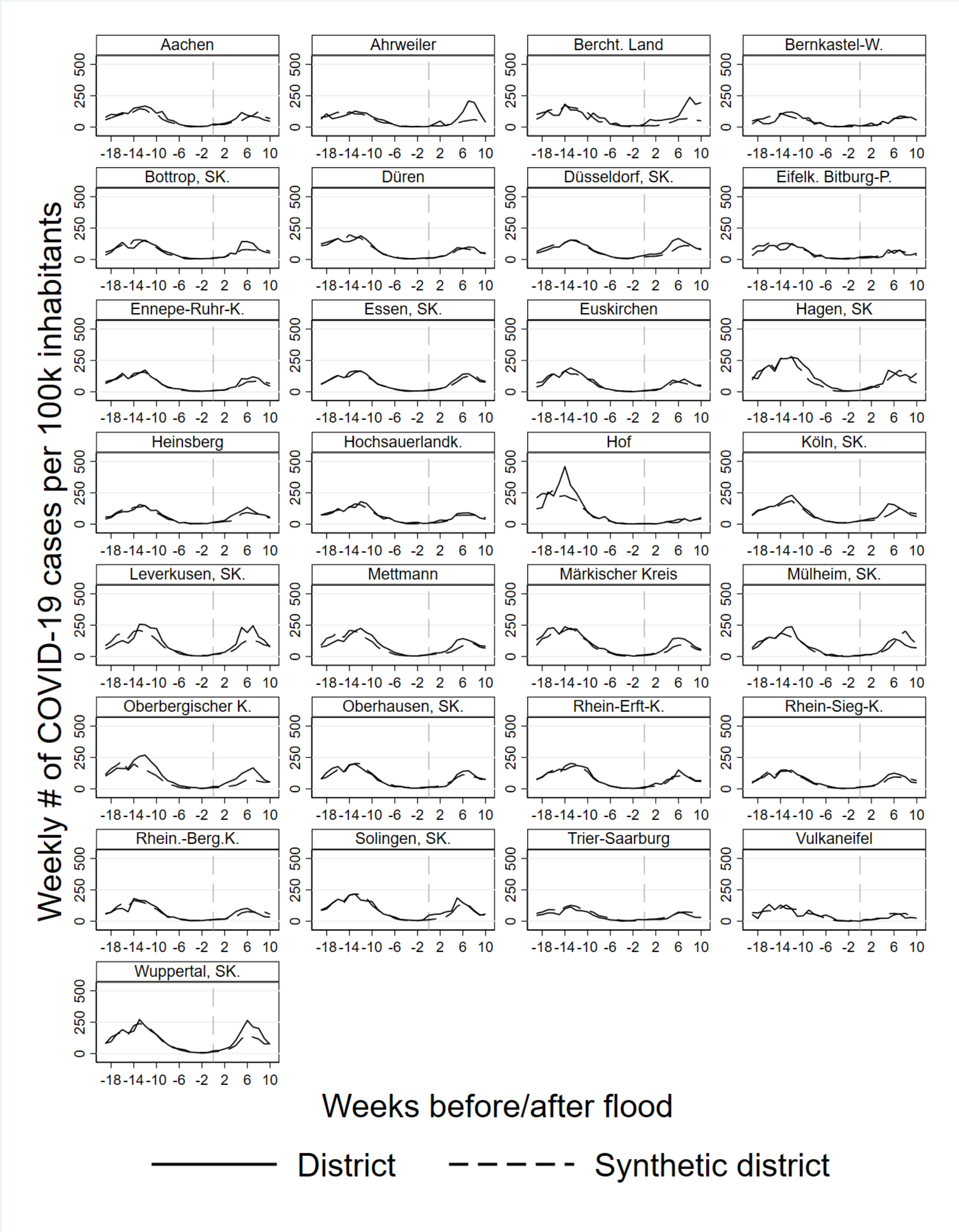
Appendix

Table A.1: Weights for COVID-19 cases

Affected district	First	Second	Third	Donors
Düsseldorf, SK.	München, SK (.289)	Berlin, SK. (.273)	Frankfurt a.M. (.223)	7
Essen, SK.	Gelsenkirch., SK.* (.275)	Wilhelmsh., SK. (.212)	Berlin, SK. (.184)	6
Mülheim, SK.	Neustadt, SK (.588)	Offenbach, SK. (.177)	Wilhelmsh., SK. (.089)	8
Oberhausen, SK.	Neunkirchen (.427)	Gelsenkirch., SK.* (.288)	Krefeld, SK. (.106)	8
Solingen, SK.	Remscheid, SK.* (.319)	Viersen* (.244)	Krefeld, SK. (.155)	10
Wuppertal, SK.	Gelsenkirch., SK.* (.259)	Krefeld, SK. (.242)	Heilbronn, SK. (.108)	12
Mettmann	Kaiserslautern (.14)	Greiz (.117)	Salzgitter, SK. (.101)	16
Köln, SK.	Frankfurt a.M. (.569)	Oldenburg, SK. (.223)	Kaiserslautern, SK (.072)	6
Leverkusen, SK.	Krefeld, SK. (.341)	Lörrach (.147)	Offenbach, SK. (.146)	10
Aachen	Landau, SK (.432)	Marburg-Biedenkopf (.213)	Flensburg, SK. (.138)	6
Düren	Viersen* (.284)	Neustadt (.176)	Gelsenkirch., SK.* (.144)	13
Rhein-Erft-K.	Rhein-K. Neuss* (.414)	Gelsenkirch., SK.* (.19)	Aichach-Friedberg (.146)	14
Euskirchen	Viersen* (.532)	Cloppenburg (.107)	Stendal (.075)	11
Heinsberg	Viersen* (.385)	Coesfeld (.17)	Erding (.122)	11
Oberbergischer K.	Borken (.252)	Wesermarsch (.244)	Cloppenburg (.219)	7
Rhein.-Berg.K.	Coesfeld (.19)	Lippe (.188)	Viersen* (.185)	12
Rhein-Sieg-K.	Lippe (.186)	Starnberg (.177)	Viersen* (.173)	14
Bottrop, SK.	Neunkirchen (.405)	Mönchengladbach, SK.* (.159)	Märkisch-Oder. (.116)	10
Hagen, SK	Salzgitter, SK. (.437)	Gelsenkirch., SK.* (.249)	Krefeld, SK. (.137)	8
Ennepe-Ruhr-K.	Bad Dürkheim (.213)	Neunkirchen (.161)	Wunsiedel* (.145)	14
Hochsauerlandk.	Höxter* (.306)	Freudenstadt (.194)	Borken (.16)	9
Märkischer Kreis	Viersen* (.284)	Borken (.258)	Salzgitter, SK. (.199)	6
Ahrweiler	Wunsiedel* (.217)	Cochem-Zell* (.187)	Kaiserslautern (.141)	9
Bernkastel-W.	Breisgau-H. (.236)	Südwestp. (.164)	Neustadt, SK (.148)	9
Eifelk. Bitburg-P.	Borken (.443)	Südwestp. (.246)	Neustadt (.169)	6
Vulkaneifel	St. Wendel* (.354)	Höxter* (.329)	Cochem-Zell* (.124)	11
Trier-Saarburg	Lörrach (.416)	Potsdam-M. (.192)	St. Wendel* (.164)	6
Bercht. Land	Garmisch (.45)	Waldshut (.148)	Chemnitz, SK. (.11)	9
Hof	Wunsiedel* (.534)	Kronach* (.274)	Wesermarsch (.104)	5

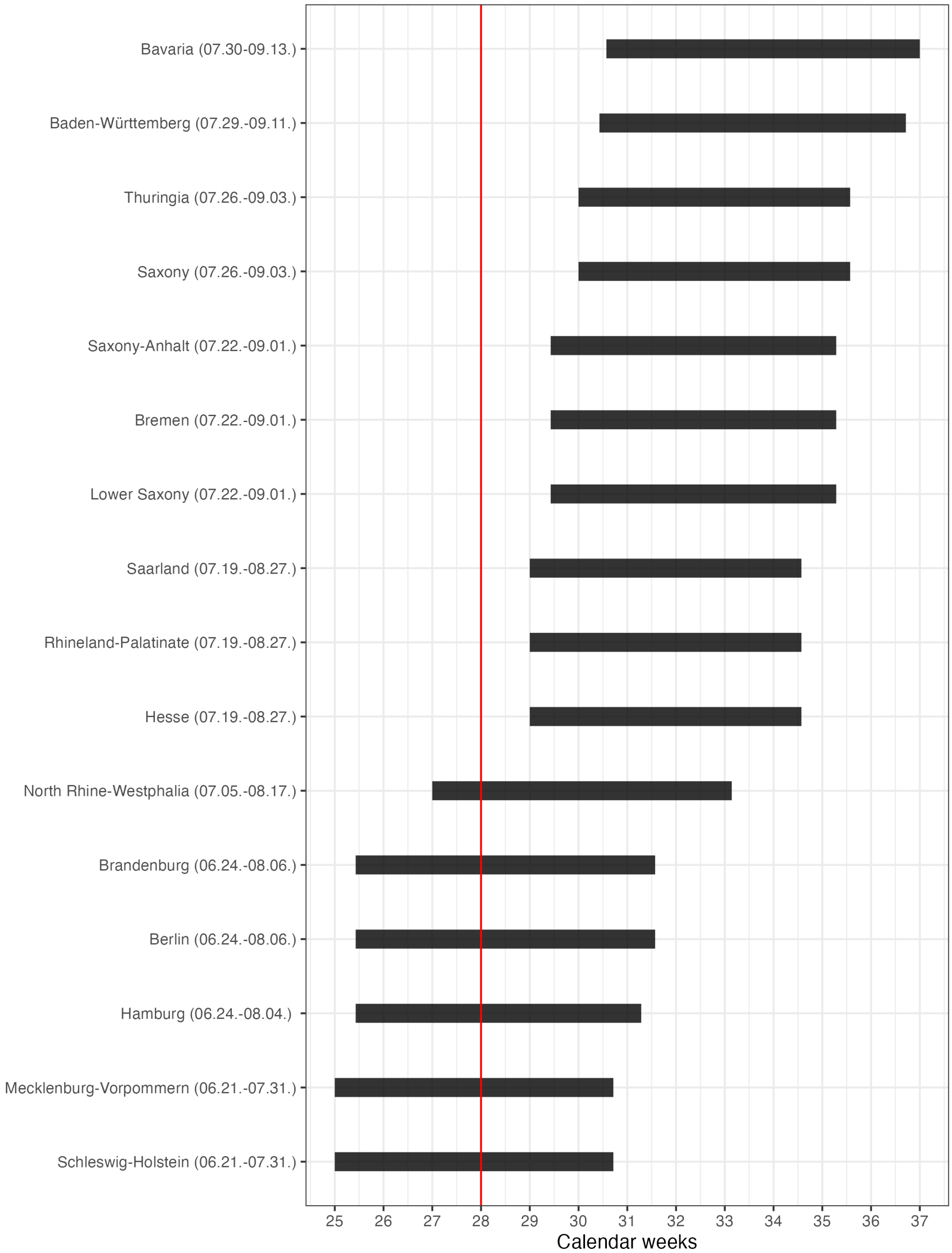
Note: The table reports up to three of the biggest non-affected districts (column two until four), receiving the biggest weights for each affected district (column one) for the SCM of the COVID-19 cases per 100,000 residents. Column five includes the overall number districts receiving a weight bigger than 0. * marks districts neighboring at least one affected district.

Figure A.1: Development COVID-19 cases of all affected districts



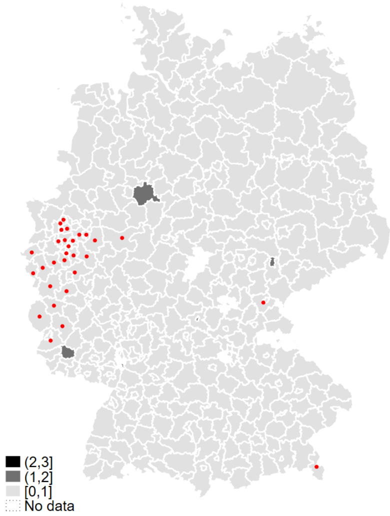
Notes: Graphs show the development of COVID-19 cases per week per 100,000 residents (black line) for each of the 29 affected districts together with its synthetic counterpart (dashed line). The gray vertical dashed line marks the week of the flood.

Figure A.2: Summer Holidays 2021 in Germany

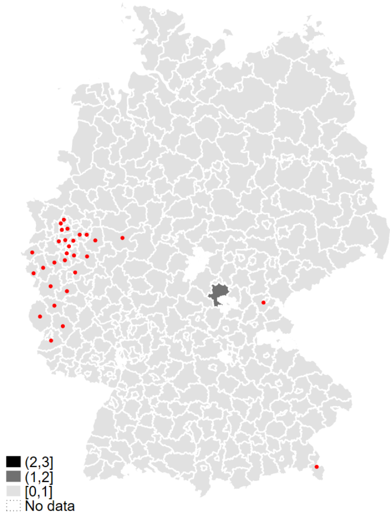


Graph contains a bar chart representing the duration of the summer school holidays in 2021 for each of the 16 German federal states. The x-axis contains the calendar weeks. The red vertical lines marks the week of the flood.

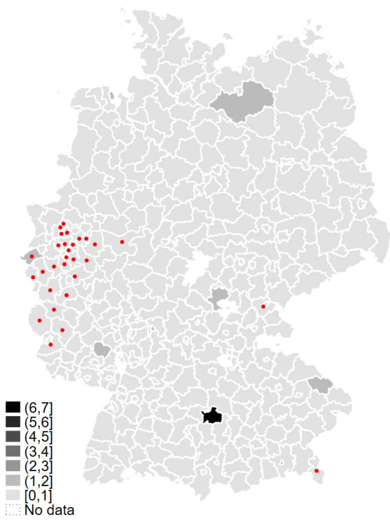
Figure A.3: Weekly number of COVID-19 deaths per 100,000 residents per district



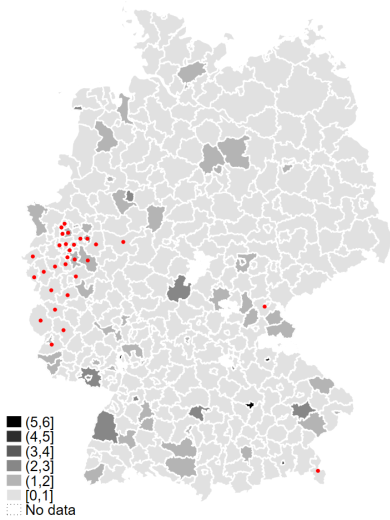
One week before



Week of the flood



Three weeks after



Six weeks after

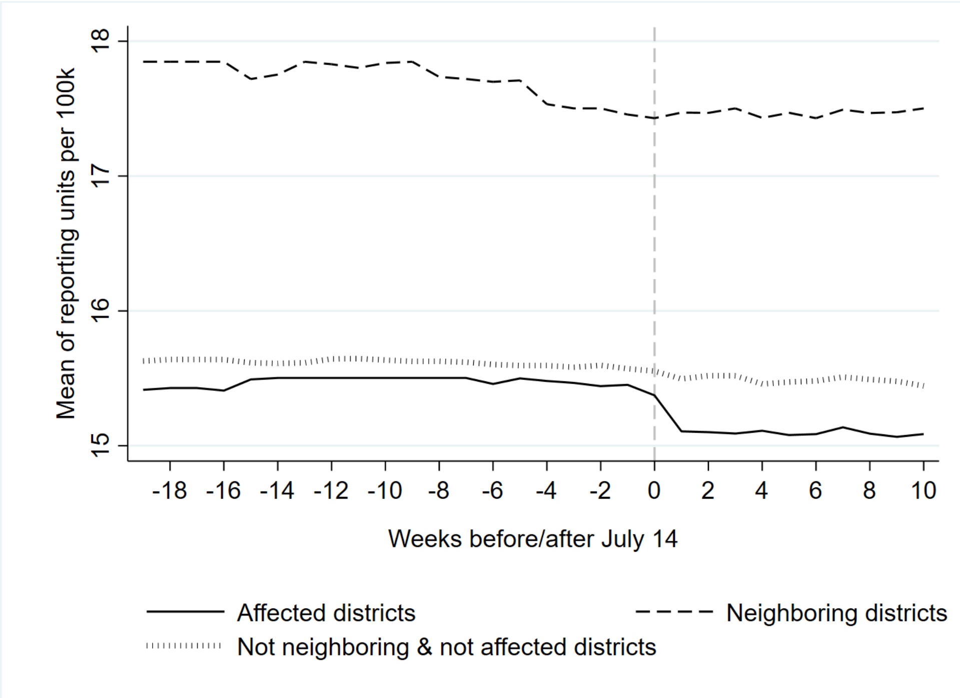
Notes: Red dots mark districts labeled as treated by the BBK. We do not report data for the excluded districts mentioned in section 4.2.

Table A.2: Weights for ICU patients

Affected district	First	Second	Third	Donors
Düsseldorf, SK.	Frankfurt a.M., SK. (.399)	München, SK (.307)	Uckermark (.114)	7
Essen, SK.	Wilhelmsh., SK. (.251)	Offenbach a.M., SK. (.229)	Berlin, SK. (.229)	9
Mülheim, SK.	Neustadt adW., SK (.607)	Offenbach a.M., SK. (.122)	Gelsenkirch., SK.* (.081)	9
Oberhausen, SK.	Neunkirchen (.389)	Gelsenkirch., SK.* (.257)	Offenbach a.M., SK. (.119)	8
Solingen, SK.	Rhön-Grabfeld (.386)	Gelsenkirch., SK.* (.227)	Viersen* (.213)	6
Wuppertal, SK.	Gelsenkirch., SK.* (.362)	Bonn, SK.* (.127)	Lörrach (.123)	10
Mettmann	Kaiserslautern (.237)	Hochtaunusk. (.222)	Pirmasens, SK (.098)	13
Köln, SK.	Frankfurt a.M., SK. (.601)	Oldenburg, SK. (.22)	Kaiserslautern, SK (.08)	7
Leverkusen, SK.	Krefeld, SK. (.254)	Hochtaunusk. (.221)	Neustadt adW., SK (.16)	9
Aachen	Flensburg, SK. (.419)	Landau, SK (.228)	Steinburg (.167)	8
Düren	Borken (.203)	Viersen* (.148)	Gelsenkirch., SK.* (.147)	13
Rhein-Erft-K.	Rhein-K. Neuss* (.354)	Gelsenkirch., SK.* (.185)	Aichach-Friedberg (.145)	15
Euskirchen	Viersen* (.455)	Borken (.16)	Stendal (.099)	10
Heinsberg	Viersen* (.392)	Kleve (.189)	Cloppenburg (.104)	12
Oberbergischer K.	Nienburg (Weser) (.398)	Borken (.191)	Kaiserslautern (.159)	8
Rhein.-Berg.K.	Nienburg (Weser) (.162)	Uckermark (.162)	Coesfeld (.158)	11
Rhein-Sieg-K.	Lippe (.286)	Viersen* (.259)	Mainz-Bingen (.154)	10
Bottrop, SK.	Neunkirchen (.39)	Gelsenkirch., SK.* (.164)	Fürth, SK. (.14)	14
Hagen, SK	Gelsenkirch., SK.* (.468)	Salzgitter, SK. (.214)	Rhön-Grabfeld (.129)	6
Ennepe-Ruhr-K.	Neunkirchen (.274)	Bad Dürkheim (.245)	Recklinghausen* (.144)	10
Hochsauerlandk.	Höxter* (.205)	Steinburg (.204)	Olpe* (.136)	10
Märkischer Kreis	Steinburg (.213)	Gelsenkirch., SK.* (.192)	Borken (.154)	9
Ahrweiler	Friesland (.253)	Kusel (.235)	Cochem-Zell* (.149)	10
Bernkastel-W.	Südwestp. (.265)	Waldshut (.158)	Altötting (.144)	11
Eifelk. Bitburg-P.	Borken (.348)	Dingolfing-Landau (.29)	Südwestp. (.215)	7
Vulkaneifel	St. Wendel* (.294)	Höxter* (.264)	Cochem-Zell* (.19)	9
Trier-Saarburg	Lörrach (.409)	Potsdam-M. (.19)	St. Wendel* (.168)	6
Bercht. Land	Bodenseek. (.287)	Kempten (Allgäu) (.282)	Sonneberg (.121)	9
Hof	Cochem-Zell* (.58)	Südwestp. (.124)	Saarpfalz-K. (.091)	7

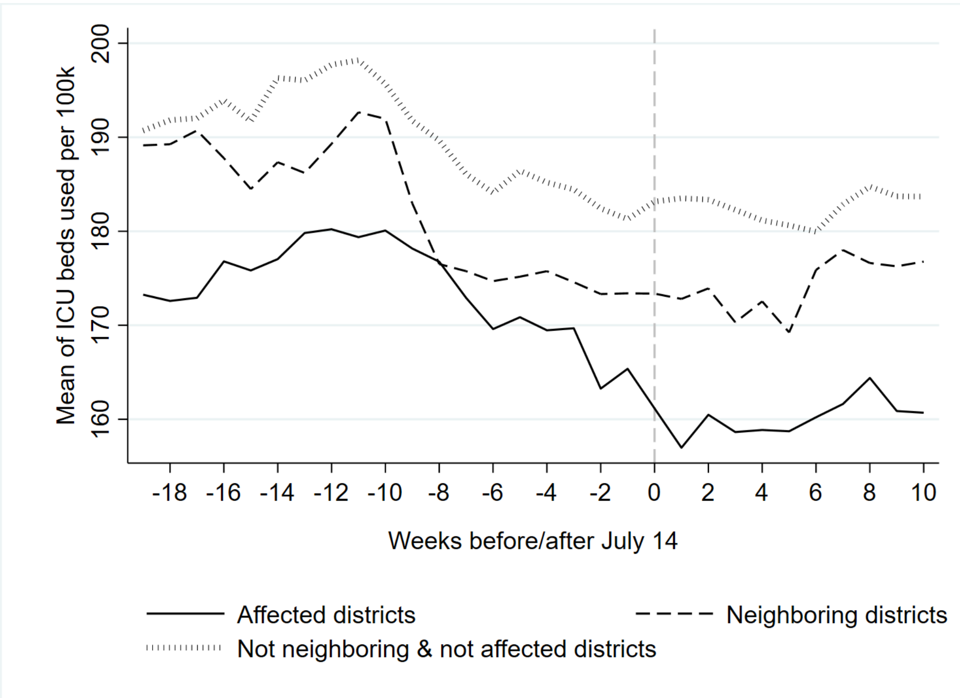
Note: The table reports up to three of the biggest non-affected districts (column two until four), receiving the biggest weights for each affected district (column one) for the SCM of the weekly average ICU patients per day per 100,000 residents. Column five includes the overall number districts receiving a weight bigger than 0. * marks districts neighboring at least one affected district.

Figure A.4: Development Average of ICU units per 100,000 residents



Notes: Graph shows the weekly average or reporting ICU units per 100,000 residents before and after the flood starting on July 14 (gray vertical dashed line) for all affected districts (black line), all neighboring districts (black dashed line) and all others (black dotted line).

Figure A.5: Development Average of ICU beds used per 100,000 residents



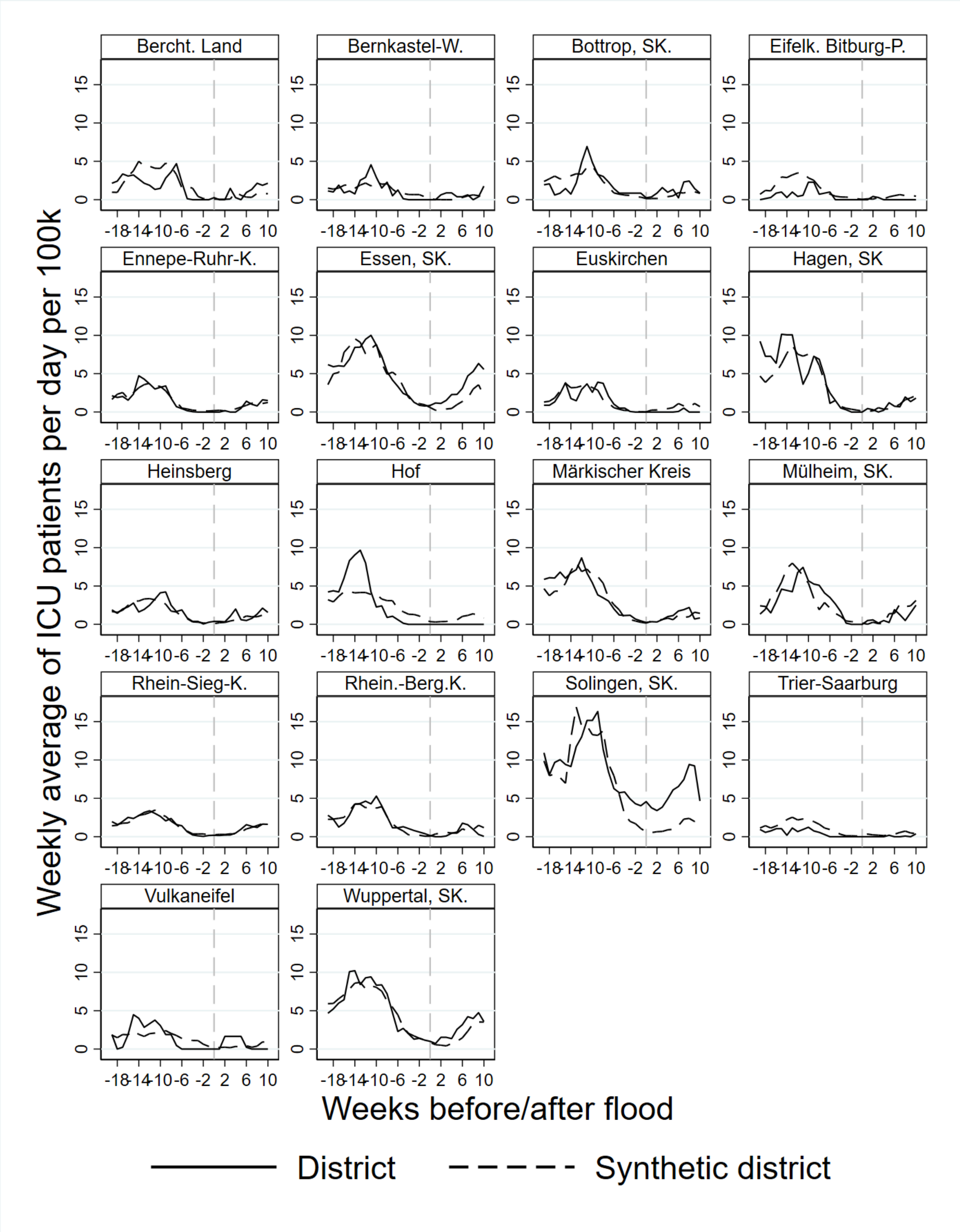
Notes: Graph shows the weekly average of ICU beds used per 100,000 residents before and after the flood starting on July 14 (gray vertical dashed line) for all affected districts (black line), all neighboring districts (black dashed line) and all others (black dotted line).

Table A.3: Weights for ICU patients - no changes in ICU units and no neighboring districts

Affected district	First	Second	Third	Donors
Essen, SK.	Pirmasens, SK (.463)	Offenbach, SK. (.215)	München, SK (.193)	6
Mülheim, SK.	Neustadt, SK (.561)	Frankenthal, SK (.144)	Offenbach, SK. (.109)	8
Solingen, SK.	Rhön-Grabfeld (.362)	Krefeld, SK. (.275)	Borken (.109)	7
Wuppertal, SK.	Krefeld, SK. (.366)	Bremerhaven, SK. (.19)	Lippe (.111)	9
Euskirchen	Steinburg (.27)	Borken (.26)	Amberg-Sulzbach (.116)	10
Heinsberg	Kleve (.185)	Alzey-W. (.163)	Steinburg (.153)	10
Rhein.-Berg.K.	Steinburg (.241)	Coesfeld (.199)	Uckermark (.137)	11
Rhein-Sieg-K.	Mainz-Bingen (.301)	Lippe (.277)	Krefeld, SK. (.115)	11
Bottrop, SK.	Neunkirchen (.47)	Fürth, SK. (.128)	Krefeld, SK. (.094)	10
Hagen, SK	Salzgitter, SK. (.387)	Krefeld, SK. (.255)	Bremerhaven, SK. (.154)	8
Ennepe-Ruhr-K.	Neunkirchen (.311)	Bad Dürkheim (.222)	Neustadt, SK (.103)	12
Märkischer Kreis	Steinburg (.386)	Borken (.125)	Krefeld, SK. (.122)	9
Bernkastel-W.	Südwestp. (.294)	Altötting (.174)	Waldshut (.156)	11
Eifelk. Bitburg-P.	Borken (.364)	Dingolfing-Landau (.279)	Südwestp. (.233)	6
Vulkaneifel	Südwestp. (.319)	Dithmarschen (.248)	Steinburg (.166)	8
Trier-Saarburg	Lörrach (.354)	Dingolfing-Landau (.244)	Potsdam-M. (.217)	5
Bercht. Land	Kempten (Allgäu) (.336)	Bodenseek. (.261)	Nordfriesland (.145)	9
Hof	Südwestp. (.479)	Saarpfalz-K. (.133)	Northeim (.13)	7

Note: The table reports up to three of the biggest non-affected districts (column two until four), receiving the biggest weights for each affected district (column one) for the SCM of the weekly average ICU patients per day per 100,000 residents. Column five includes the overall number districts receiving a weight bigger than 0.

Figure A.6: Development ICU patients per day of all affected districts - no changes in ICU units and no neighboring districts



Notes: Graphs show the development of the weekly averages of ICU patients per day per 100,000 residents (black line) for each of the 18 affected districts together with its synthetic counterpart (dashed line). The gray vertical dashed line marks the week of the flood.

5 Overarching Conclusions

The thesis provides evidence that critical events, such as elections and natural disasters, influence infectious disease transmission and therefore public health risks.

The first study quantifies the impact of the Bavarian municipal elections held on March 15, 2020, on COVID-19 transmissions. Using the synthetic control method and regression analysis, the study finds that voter participation was positively correlated with COVID-19 cases and deaths. The findings further indicate that, in addition to in-person voting, election-related activities occurring near election day — such as campaigning and vote counting — may have also played a role in the spread of the disease.

The second study expands this analysis to elections outside of pandemic periods and their role in the spread of respiratory diseases in general. It finds an increase in sick leaves due to respiratory diseases following regional elections in Hesse and Thuringia, implying a broader link between elections and disease transmission. However, the absence of an effect for the Bavarian election in October 2018, right at the beginning of the Influenza season in Germany, suggests that seasonality and baseline disease prevalence are key factors in determining the risk associated with elections.

The third study examines the impact of natural disasters on the spread of COVID-19. By comparing districts affected and non-affected by floodings while controlling for demographic and economic factors, it finds evidence that natural disasters can influence disease dynamics. While the results potentially do not establish causality, they emphasize how natural disasters can heighten health risks associated with respiratory diseases by facilitating transmission not only among affected victims but also, most likely, through interactions with helpers.

While each study contributes specific insights, together they paint a broader picture of how critical events can shape public health. These findings not only suggest that critical events can contribute to the spread of airborne diseases (like COVID-19), but additionally provide novel evidence that the mechanisms through which such events influence disease transmission are context-dependent. This dependence is shaped by the specific (social) gatherings that ensue. In the case of elections, not only the election day itself but also related activities, such as campaign events and gatherings of election workers for vote counting, can facilitate the spread. Evidence from the July 2021 flood in Western

Germany suggests that the influx of volunteers in the aftermath of natural disasters played a significant role in the spread of disease. Furthermore, the baseline transmission of respiratory diseases, which influences the scale of the impact, does not necessarily need to reach a severe outbreak level. The second study, which examines recurring influenza seasons, and the third study, which analyzes data from a summer plateau of the COVID-19 pandemic in Germany, both demonstrate that the influence of critical events can remain significant even when baseline transmission rates are relatively low. Consequently, the findings from these studies can be interpreted as lower-bound estimates of the effect during periods of exceptionally high disease spread. Finally, the second essay additionally provides evidence that the impact of elections extends beyond the widely in the literature recognized respiratory disease, COVID-19, to other respiratory illnesses such as Influenza.

While the findings provide valuable insights into the impact of critical events on the spread of infectious diseases, some limitations must be considered when interpreting the results. First, a potential bias produced by missing information about the tests conducted can not be entirely ruled out. While the data provided by the RKI and DIVI was sufficient for the analyses conducted in this thesis, the success of future empirical research - whether focused on estimating causal effects, making reliable predictions or else - likely depends on access to currently unavailable data. Without data on the number of *PCR tests* and *Rapid Antigen Tests* conducted, analyses must either rely on in that regard less bias-prone indicators (such as COVID-19 hospitalizations) or assume that testing behavior does not vary systematically due critical events between treated and untreated units of interest.[21] Secondly, it cannot be entirely ruled out that factors unrelated to the elections may have influenced the results. However, since this thesis examines multiple elections occurring at different times and in the context of different respiratory diseases, I am confident that the findings provide substantial evidence of an effect. Thirdly, this thesis focuses on a specific subset of critical events, while other occurrences — such as military defense activities or

[21] In theory, data on the number of tests conducted should be available for Germany, as most tests during the COVID-19 pandemic were funded by the German federal budget. According to Article 7 (1) of the *Regulation on the Entitlement to Testing for Direct Detection of the SARS-CoV-2 Coronavirus* (German: Verordnung zum Anspruch auf Testung in Bezug auf einen direkten Erregernachweis des Coronavirus SARS-CoV-2) from June 2021 (see Bundesministerium für Gesundheit, 2021), the *German National Association of Statutory Health Insurance Physicians* (German: Kassenärztliche Bundesvereinigung) was responsible for collecting such data. However, despite numerous attempts to contact the relevant authorities, my coauthors and I were unable to gain access.

responses to other natural disasters like volcanic eruptions or earthquakes — may also have an impact. Fourthly, the results do not always provide robust evidence of an effect on hospitalizations or deaths due to COVID-19. This may be due to data limitations related to hospital admissions, the effectiveness of COVID-19 vaccines that were already widely distributed at the time the analyses were conducted (both in the third study) or insufficient pre-treatment COVID-19 death counts (study one). However, this does not mean that no relationship exists. Further steps are already planned to address this limitation.[22]

Despite its limitations, the insights presented offer several important scientific implications for future research. First, the extent of their impact depends on the timing of such events relative to baseline disease prevalence. While it is clear that countermeasures against disease transmission should be considered for elections and natural disasters during pandemic periods, precise thresholds of disease prevalence for different events at which such measures become advisable remains an open question for future research. Second, since the thesis examines only a subset of critical events that should be taken into account, future studies could investigate whether empirical evidence supports a connection between disease transmission and events. Third, although the second study provides broad estimates of the economic costs associated with holding elections during periods of high virulence, further research is needed to assess not only indirect costs, such as productivity losses due to absenteeism from work, but also direct costs, including for example increased expenditures on outpatient and inpatient care. Finally, building on the previous points, future research should explore cost-effective countermeasures for such events. For instance, while face masks have been shown to significantly reduce the transmission of respiratory diseases (see, e.g., Mitze et al., 2020), research on their cost-effectiveness remains scarce, particularly in the context of pandemics and even more so for the seasonal spread of infectious diseases outside of pandemic scenarios. In that regard, future research should pay attention to the different contexts of different critical events. While distinguishing between non-critical and critical events in existing research illustrates that postponement or cancellation is not a viable option for critical

[22] In the next phase of study three, my coauthor and I will analyze the German *Hospital Statistics* (German: Krankenhausstatistik), an annual comprehensive survey of hospitals in Germany. This dataset allows for a more detailed examination of morbidity and mortality related to respiratory diseases. Additionally, it enables us to account for organizational structures of German hospitals, staffing, equipment, and provided services.

events during a pandemic, it does not imply that all countermeasures against viral transmission are equally suitable for every critical event. For instance, providing face masks to election workers and in-person voters on voting days is likely an effective strategy to limit the spread of Influenza and COVID-19, as this measure can be planned in advance. However, in the context of natural disasters, distributing face masks may be less practical, as such events are unpredictable, and wearing masks while performing physically demanding tasks — particularly for relief workers — may not be feasible. Instead, for natural disasters, closely monitoring viral spread through frequent testing of all involved individuals and immediately isolating those with symptoms or positive test results may be a more reasonable approach. In addition, the results for the flood indicate that the heightened focus on vaccination efforts in the affected area successfully curbed the virus's spread within the expected timeframe. While vaccine distribution in response to such unforeseen events can only mitigate long-term effects, these findings suggest that similar intensified efforts could help contain the spread foreseeable critical events, such as elections. Finally, enhancing airflow, such as through the use of ventilators (see Morawska et al., 2024), could be an effective measure for elections — particularly in voting booths — as well as for the sheltering of victims and aid workers following natural disasters.

Beyond its scientific contributions, this thesis also holds important implications for policymakers and public health officials. The evidence of a potential link between critical social events and the spread of infectious diseases could result in a trade-off between the societal relevance of such events (e.g., *the human right to vote*) and the public health. While further research is needed to get more precise estimates, the findings indicate that these dynamics could result in substantial social and economic costs. Policymakers should therefore not only assess whether non-critical events should be postponed or canceled but also explore measures to ensure the safe(r) execution of critical events. Additionally, investing in research on this topic could enhance societies' resilience to future respiratory disease outbreaks by, for example, further examining the cost-effectiveness of potential countermeasures to develop effective preparedness or response plans. Finally, as previously discussed, effectively mitigating the spread of viral diseases depends on a comprehensive analysis of the available data. Policymakers should prioritize making currently unavailable data, such as the number of tests conducted, accessible to researchers. In this context, the continuous monitoring of such diseases and ensuring that this data is available to the research community will also provide significant benefits.

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Affidavit

I confirm that I wrote this thesis independently and on my own without using any other sources and aids as I stated. Where I used other sources, I clearly marked them as not my own. Parts of the essays included in this thesis were written in cooperation with coauthors. These collaborations are set out in separate declarations of authorship, which are attached. This thesis has not been received by any examination board, neither in this nor in a similar form.

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