Title:

The effect of a nighttime curfew on the spread of COVID-19

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Abstract

While nighttime curfews are less severe restrictions compared to around-the-clock curfews in mitigating the spread of Covid-19, they are nevertheless highly controversial, with the scarce literature on their effectiveness providing mixed evidence. We study the effectiveness of the nighttime curfew in Hamburg, Germany's second largest city, in mitigating the spread of Covid-19. This curfew forbid people from leaving their home between 9 p.m. and 5 a.m. for non-essential businesses. Applying both difference-in-differences and synthetic control group methods, we find that the curfew was effective in reducing the number of Covid-19 cases. As it is unclear whether and how the virus will mutate in the next time, policy-makers might have to resort to non-pharmaceutical interventions again. Nighttime curfews should be kept in the toolbox of policy-makers to fight Covid-19.

Keywords: Covid-19, Curfews, Non-pharmaceutical interventions, Synthetic control, Germany

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1. Introduction

Governments around the world have implemented various non-pharmaceutical interventions (NPI) to slow the spread of Covid-19. Most of these interventions severely restrict the lives of the affected populations (including, e.g., school closures, stay-at-home orders, closings of restaurants, travel restrictions). Hence, governments constantly face a trade-off between containing the pandemic and restricting civil rights. While strict lockdown measures that allow people to leave the house only for essential businesses are found to be effective in reducing the spread of Covid-19 (see, e.g., Flaxman et al., 2020; Hsiang et al., 2020), they come with high social and economic costs (see, e.g., Bartik et al., 2020; Chetty et al., 2020). For this reason, many governments (e.g., in Canada, France, Germany, Greece, and Spain) implemented *nighttime* curfews, in which citizens are required to stay at home during the night except for specific, well-defined reasons. The idea of a nighttime curfew is to reduce private indoor gatherings, where the risk of infections is way higher than outdoors as people are not allowed to leave their homes for non-essential reasons.

While nighttime curfews are less severe restrictions than around-the-clock curfews, they are highly controversial. First, nighttime curfews nevertheless substantially restrict the affected individuals. Second, as these curfews do not affect daytime activities, they may not be effective in fighting the spread of Covid-19. They might be even counterproductive if they increase contact density during earlier hours of the day (Sprengholz et al., 2021). Generally, governments should not implement ineffective measures with substantial side-effects. However, it can strengthen the public adherence to an intervention if people are convinced of its effectiveness. For all these reasons, it is crucial to understand the effectiveness of nighttime curfews.

This paper examines the effectiveness of nighttime curfews, based on the curfew in the German city of Hamburg that came into effect on April 2, 2021. The curfew prohibited people from leaving their home between 9 p.m. and 5 a.m., except for physical training, walking dogs, and commuting to work. We compare the development of Covid-19 cases in Hamburg, Germany's second largest city with almost 2 million inhabitants, around the introduction of the curfew with the development of Covid-19 cases in other German regions over the same period. Applying both difference-in-differences and synthetic control group methods, our empirical analyses show that the nighttime curfew was effective in reducing the number of Covid-19 cases.

This study contributes to the literature on the effectiveness of different non-pharmaceutical interventions to mitigate the spread of Covid-19 by focusing on nighttime curfews, an intervention that restricts the lives of citizens in a less harsh manner than full lockdowns and around-the-clock stay-at-home orders. Although many countries and regions have implemented curfews at some point, there is surprisingly little evidence on the effectiveness of nighttime curfews to contain the Covid-19 pandemic – and the existing literature provides rather mixed evidence.

Sprengholz et al. (2021) examine public perceptions of nighttime curfews: The majority of participants in their survey did not perceive nighttime curfews as an effective measure to contain the pandemic and stated that they would not reduce private contacts due to a nighttime curfew. A substantial share of participants reported that they would rather shift a private dinner meeting forward in time, suggesting that nighttime curfews may increase contact density in the hours of the day when the curfew is not in place. While these statements refer to stated behaviors and not to actual behavior, the study by Sprengholz et al. (2021) highlights that nighttime curfews might backfire and actually increase Covid infections.

However, several studies find that nighttime curfews reduce mobility. For instance, Ghasemi et al. (2021) provide evidence that a nighttime curfew in Ontario/Canada substantially reduced nighttime mobility. Similarly, Velias et al. (2022) show that a temporal extension of a nighttime curfew in the Attica region of Greece led to a small decrease in mobility. These are important insights, but mobility data are not a direct measure of the pandemic situation and reductions in mobility do not necessarily translate into fewer Covid infections. For instance, nighttime curfews might induce two households to meet at one household's home instead of outdoors in a park. In this case, there would be a decline in mobility (as one household stayed at home), while Covid cases might still increase as the risk of infection is higher indoors than outdoors. Therefore, it is important to directly examine measures of the pandemic situation like incidence rates or virus reproduction numbers as well.

In a modeling study that examines several NPIs at the same time, Sharma et al. (2021) find that curfews might be effective in slowing the spread of Covid-19. Andronico et al. (2021) obtain similar results for French Guiana in another modeling study, while Dimeglio et al. (2021) provide evidence that a temporal extension of a nighttime curfew in Toulouse, France, actually increased infection rates slightly. de Haas et al. (2021) find in an *ex-post* evaluation that nighttime curfews in the German state of Hesse did not reduce Covid-19 infections. In Hesse, counties had to implement nighttime curfews when the incidence rate exceeded a certain threshold. This is different to the situation that we study: Hamburg made its own decision to implement a nighttime curfew. Further, given that there were many other regions in Germany with similar infection rates, the construction of a similar control group is feasible.

We contribute to this literature by focusing on a setting that offers several advantages for a clean identification of curfew effects. First, there were many other regions in Germany with a similar Covid-19 situation before the introduction of the curfew, allowing to use a control group with a very similar pre-treatment level and development of Covid-19 cases. Second, due to the analysis at the sub-national level, many other factors that affect the pandemic situation are very similar in Hamburg and the other German regions. For instance, other interventions were implemented or agreed upon at the national level (e.g., travel restrictions, vaccine availability). While our analysis relates to early 2021 when vaccines were not widely available, nobody knows whether and how the virus will mutate in the future (Ledford, 2022) and it is not unlikely that policy-makers will need to resort again to non-pharmaceutical interventions to contain the pandemic. Therefore, it is important to know about the effectiveness of previous non-pharmaceutical interventions.

The remainder of the paper is structured as follows. Section 2 provides further information about the curfew in Hamburg, the data, and the applied statistical methods. Section 3 presents the main empirical results, probes the robustness of the main findings, and examines effect heterogeneity. Section 4 presents a discussion of the findings and Section 5 concludes.

2. Materials and methods

2.1. Background

Hamburg introduced a nighttime curfew on April 2, 2021, as one of the first regions in Germany. The curfew came into effect on Good Friday, just before the Easter weekend.The curfew prohibited people from leaving their home between 9 p.m. and 5 a.m., except for physical training, walking dogs, and commuting to work. In Hamburg, the nighttime curfew was officially removed on May 12, one week after the incidence rate, the average number of daily new infections per 100,000 inhabitants, fell below the symbolic value of 100 for the first time since the curfew implementation.

Hamburg's curfew preempted the nationwide regulation for nighttime curfews in Germany: From April 23, 2021, regions with an incidence rate above 100 had to implement a nighttime curfew. The implementation of the curfews based on the federal regulation is endogenous in the sense that the curfews automatically follow from the number of weekly Covid-19 cases exceeding a specific threshold. This is an important difference to the nighttime curfew in Hamburg that we analyze. While the implementation of the curfew in Hamburg was also related to the development of the pandemic situation, it was at Hamburg's discretion to implement the curfew. At that time, many other cities in Germany had similar or even higher incidence rates but did not implement a curfew. Bavaria and Saxony implemented similar regulations before the nationwide regulations. Therefore, we exclude cities from these two states from our main analyses. Our results are, however, robust to including theses cities as well (see Section 3.2).

At the time of the curfew implementation, B.1.1.7 ("Alpha") was the dominant SARS-CoV-2 variant in Germany. Across Germany, bars and restaurants were closed, it was required to wear medical masks indoors, private gatherings were restricted, and testing capacities were scarce. Further, vaccines against Covid-19 were not widely available in Germany and were restricted to specific groups, including people older than 80, residents in nursing homes, and medical staff. About 9.5 million inhabitants (11.3 %) had received a first vaccine dosage, while about 4 million (5 %) had received two vaccinations (Robert Koch Institute, 2021).

2.2. Data and sample

The curfew was implemented on April 2, 2021, and our main analysis period begins on February 1, 2021. Hence, we have about two months of pre-treatment data, which should be a sufficient period to compare the development of the outcome variable before the implementation of the curfew. Having a longer pre-treatment period increases the risk that other regional policies were implemented in some of the control units. In Section 3.2, we show that our results are insensitive to different lengths of the pre-treatment period. Our analysis period ends on April 30, one week after the national night curfew regulations came into force on April 23. Hence, our analysis period consists of $t = 89$ days.

While Hamburg constitutes our treatment group, the pool of potential control units consists of all German cities that have a population of at least 100,000 inhabitants and that are independent counties (*Kreisfreie Städte*). Hence, we consider only urban regions in order to make the pool of potential control units more comparable in terms of population density and the provision of free Covid-19 testing stations. We further restrict this potential pool by excluding all cities that introduced nighttime curfews before the nationwide regulation for nighttime curfews in our observation period. This is done by first checking every Covid-related state-order (*Corona-Landesverordnung*) during our observation period. Further, we double-checked all remaining cities by working through local newspapers and local directives. We dropped Hagen, Halle (Saale), Heilbronn, Karlsruhe, Krefeld, Köln, Leverkusen, Mannheim, Mülheim an der Ruhr, Offenbach, Osnabrück, Potsdam, Remscheid, Salzgitter, Kaiserslautern, Koblenz, Ludwigshafen, Mainz, Stuttgart, Wolfsburg and Wuppertal because of local curfews as well as cities in Saxony and Bavaria because of state-wide curfew regulations.

In the end, the pool of potential control units consists of 35 cities (see Table A.1 in the Appendix for an overview over the included cities). Hence, our balanced panel consists of $N = 3204$ observations (89 days x (35+1) cities).

The main outcome variable of this study is based on the number of officially reported Covid-19 cases, including late reporting. More specifically, we use the so-called seven-day incidence rate, which is the most commonly used metric in Germany to measure Covid-19 cases as this measure allows to smooth-out differential reporting behaviors on different days of the week. For each city, we take the number of reported new infections across the past seven days. We normalize this number by the size of the city's population to take into account differences in population size across cities. We focus on the incidence rate in levels (and not in logs) as this is the metric used most commonly in the media and easier to interpret. Moreover, several other NPI studies rely on Covid cases in levels as well (e.g. Friedson et al., 2021; Isphording et al., 2021; Backhaus, 2022; Diederichs et al., 2022). However, in Section 3.2, we show that our conclusions do not change when we apply the log transformation to the incidence rate. The seven-day incidence rate is provided by *Corona Datenplattform* (`https://www.corona-datenplattform.de/`), which collects Covid-19 related data on behalf of the German Federal Ministry for Economic Affairs and Climate Action.

2.3. Empirical strategy

We employ both difference-in-differences and synthetic control methods. Both methods compare the change in Covid-19 cases before and after the introduction of the night-time curfew in Hamburg with the change in Covid-19 cases in a control group at the same time. The two methods are strongly related but differ in the selection of the control group.

For both methods, the causal interpretation depends on the assumption that Covid-19 cases in Hamburg would follow the same trend as in the control group – if Hamburg had not implemented the curfew. This is the common trend assumption. While this assumption cannot be directly tested, we provide additional evidence for their plausibility by examining whether Hamburg and the control group follow a similar trend **before** the introduction of the curfew.

2.3.1. Difference-in-differences

We first estimate difference-in-differences (DiD) regressions, in which we regress the seven-day incidence rate on (i) a binary indicator variable that takes on the value one for all observations in Hamburg, our treatment group, (ii) a binary indicator variable that takes on the values one for all observations after April 2, the date of the curfew implementation, and zero else, and (iii) the interaction of the first two indicator variables. The coefficient of this interaction term is the coefficient of interest and denotes the effect of the curfew on Covid-19 cases.

Our main DiD specification is based on a two-way fixed effects model with city and day fixed effects. The city fixed effects take into account general differences in the outcome variable between cities and the day fixed effects flexibly control for general changes in the outcome variable over time. Further, we include separate indicators for the first ten days of the curfew in Hamburg, the second ten days, and the remaining nine days, i.e. up to seven days after the implementation of the nationwide curfew regulations. This allows investigating the effect of the curfew at different points after its implementation. Due to the incubation period and the lag in reporting, we do expect little or no effects in the first 10 days after the curfew is in place. As in our setting the treatment starts at a single point in time, there is no benefit of applying any of the recently developed two-way fixed effects estimators that were specifically designed for settings with staggered treatment timing (see, e.g., Callaway and Sant'Anna, 2021).

2.3.2. Synthetic control method

The Synthetic Control Method (SCM), first introduced by Abadie and Gardeazabal (2003), gained wide popularity after Abadie et al. (2010) and is an extension of DiD (Cunningham, 2021). The idea behind the SCM is that a combination of units often provides a better comparison for the treatment unit than any single unit alone. Therefore, the method uses a weighted average of units in the donor pool to construct a synthetic version of the treatment group, i. e. a synthetic Hamburg. Several studies on NPIs to contain the Covid-19 pandemic apply SCM (e.g., Mitze et al., 2020; Born et al., 2021).

Our aim is that the synthetic Hamburg resembles the real Hamburg closely with respect to (i) the development of the incidence rate before the introduction of the curfew and (ii) pre-determined characteristics that are likely to relate to the development of the incidence rate after the curfew implementation. Therefore, we consider the incidence rate on each of the 21 days before April 2, 2021, (curfew implementation) in the construction of the synthetic Hamburg. We further include population density as a measure for the transmission of the virus as well as median age and life expectancy as measures for virulence. We also consider the share of people who recovered from Covid-19 as well as the share of twice vaccinated people. Additionally, we take into account the poverty rate to capture the ability to comply with movement restrictions as well as the share of voters of the right-wing party *Alternative für Deutschland* (AfD; Alternative for Germany) as a measure of the willingness to comply with Covid containment policies. The AfD is connected to the anti-vaccination movement and publicly opposes most of the Covid-19 measures. Information on population and population density, people recovered, as well as the election results are provided by *Corona Datenplattform*. All other control variables (median age, life expectancy, poverty rate) originate from Federal Statistical Office of Germany (2021).

The method of synthetic controls reweights the control cities in such a way that the reweighted control group closely matches Hamburg with respect to the aforementioned variables. Details of the SCM weighting procedure are described in Abadie et al. (2010) and Abadie et al. (2015). We implement SCM using version 0.0.7 of Stata's user-written package SYNTH (Abadie et al., 2010). Table A.1 in the Appendix shows that the synthetic Hamburg has multiple donors. The major ones are Frankfurt (Main) with 38.3%, Bremerhaven with 13.7%, Bonn with 11.6%, Ulm with 8.5% and Gelsenkirchen with 7.7% contribution. On top of that, six more cities contribute a little into the synthetic Hamburg.

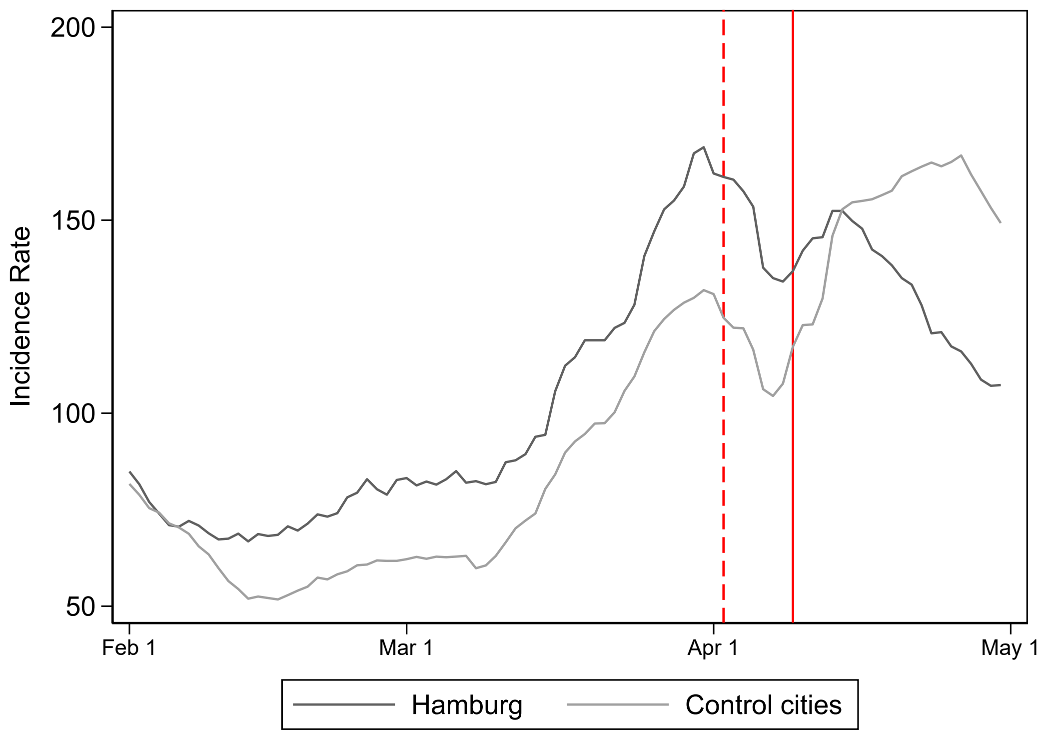
One criticism of SCM is that it might induce a regression-to-the-mean bias similar to matched difference-in-differences estimators (Daw and Hatfield, 2018) if treated and control cities are drawn from populations with different distributions of the outcome variable (Illenberger et al., 2020). While it is unclear why the distribution of the outcome variable should be generally different in treated and control cities in our (pandemic) setting, as a robustness exercise, we apply the recently developed synthetic difference-in-differences (SDiD) estimator (Arkhangelsky et al., 2021), which Illenberger et al. (2020) discuss as one potential solution to the regression-to-the-mean issue. SDiD aims to combine the merits of DiD and SCM by not only reweighting the control cities to match the treatment group but by also reweighting days from the pre-treatment period to match the average outcome in the post-treatment period for each of the control cities. We implement SDiD using Stata's user-written program SDID in the version 1.2.0 (Pailañir and Clarke, 2022).

3. Results

3.1. Main Results

We begin the empirical analysis by plotting our outcome variable, the 7-day incidence rate, separately for the treatment and control groups around the introduction of the curfew.

Figure 1: The development of Covid-19 incidence rates in the treatment and control groups



Note: The graph displays the development of our main outcome variable, the seven-day number of reported new infections per 100,000 inhabitants (incidence rate) separately for Hamburg and the 35 control cities (as the average) before and after the introduction of the nighttime curfew in Hamburg. The dashed vertical line indicates the date of the curfew implementation (April 2, 2021), while the solid vertical line indicates the date one week after curfew implementation as the curfew is unlikely to be effective immediately after is implementation due to Covid-19's incubation period and subsequent reporting lags.

Figure 1 shows several noteworthy features. First, the development of the outcome variable is rather similar in Hamburg and the control group before the implementation of the curfew, supporting the common trend assumption. Only in the first half of February 2021 do the two trends differ. In Section 3.2, we show that our findings are robust to excluding February from the pre-treatment period. Second, the incidence rate in the pre-treatment period is higher in Hamburg compared to the control group. As the level of the outcome variable might relate to the future development of the outcome, we show in the next section that our findings are confirmed by SCM, where we work with a control group that has a very similar pre-treatment outcome level as Hamburg. Third, already shortly before the curfew, Covid-19 cases start to decrease in Hamburg. This is likely due to the Easter holidays, which covers the week before and the week after Easter (i.e., from March 29 to April 9, 2021) in most states. During the Easter holidays, there is no Covid testing in schools and many people take off days from work. Fourth, this decline in Hamburg matches a similar decline in the control group. Fifth, developments in the treatment and control groups are very similar in the first days after the implementation of the curfew. This is not surprising given the incubation period and the lag in reporting and, hence, further supports the common trend assumption. Lastly, and most importantly, several days after the implementation of the curfew, trends diverge and the number of Covid cases declines in Hamburg relative to the cases in the control group. This suggests that the curfew reduced Covid cases.

We now turn to the DiD regression results in Table 1. The first column reports the DiD coefficient of the baseline DiD. This column indicates that the curfew reduced the incidence rate in Hamburg by 25.9 cases per 100,000 inhabitants. This effect is statistically significant at the 1% level. The size of the estimated effect stays constant when we include city and day fixed effects in column (2). Column (3) shows that the curfew did not reduce Covid cases in the first ten days after its implementation. This was to be expected given the incubation period and the lag in reporting. However, the estimated effect of the nighttime curfew is large and statistically significant 11-20 days and 21-29 days after the

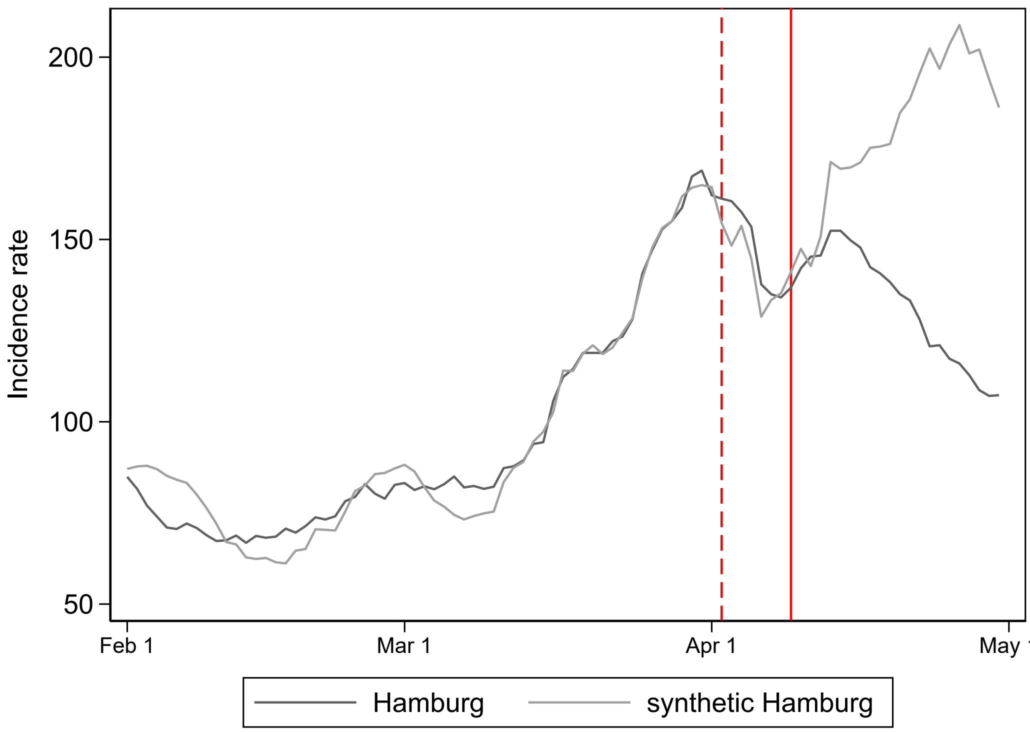
Table 1: The effect of Hamburg's nighttime curfew: DiD estimation

	Difference-in-differences			Synthetic control	Synthetic DiD
	(1)	(2)	(3)	(4)	(5)
Curfew	-25.9***	-25.9***	—	—	—
	(5.4)	(5.4)			
Curfew_{1-10}	—	—	13.2***	3.7	-1.0
			(3.0)	(6.8)	(12.7)
Curfew_{11-20}	—	—	-26.6***	-29.1**	-45.1
			(5.9)	(10.5)	(29.5)
Curfew_{20+}	—	—	-68.4***	-83.1***	-86.8**
			(9.0)	(12.9)	(43.3)
N	3204	3204	3204	1068	704
City fixed effects		✓	✓		✓
Day fixed effects		✓	✓	✓	✓

Note: The table displays the effect of the nighttime curfew in Hamburg on the reported Covid-19 incidence rate. Column (1) is based on the baseline DiD estimation, while columns (2) and (3) control for a full set of day and city fixed effects. Column (3) splits the overall curfew indicator in indicators for the curfew in the first 10 days, the second ten days, and the next nine days. Column (4) applies the city-specific weights from the synthetic control approach to the specification in column (3), while column (5) weights this specification with the weights from the synthetic difference-in-differences approach. Standard errors in parentheses (* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$).

curfew implementation, suggesting that the night curfew had a substantial negative effect on the Covid-19 incidence rate in Hamburg.

Figure 2: The evolution of the incidence rate in Hamburg and its synthetic version



Note: The graph displays the development of the incidence rate separately for Hamburg and its synthetic counterpart. The dashed vertical line indicates the date of the curfew implementation (April 2, 2021), while the solid vertical line indicates the date one week after the curfew implementation.

Figure 2 provides the SCM results graphically. It is evident that the level and trend of the outcome variable are even more similar between Hamburg and its synthetic counterpart in the pre-treatment period than in Figure 1. The trends are also very similar in the days following the curfew, when the curfew is unlikely to have a direct impact on Covid cases. This suggests that the synthetic control group manages to mimic quite well what would have happened in Hamburg if there was no curfew in place. About 6-8 days after the curfew came into effect, the incidence rate increases much stronger in the synthetic Hamburg than in the real Hamburg. In Hamburg the incidence rate decreases after Mid-April, while it increases in the synthetic Hamburg until almost the end of April. To quantify the SCM effects, column (4) in Table 1 applies the city-specific SCM weights to our main specification. The SCM estimates are slightly more negative than the main DiD results. When we apply the SDiD approach, the estimated effects are even more negative for 11-20 days and 21-29 days after the curfew implementation. However, the point estimate for 11-20 days after the ban is no longer statistically significant (with a p-value of 0.126), which might be due to the fact that in our case SDiD makes only use of three days in the pre-treatment period, thereby substantially reducing the sample size.

3.2. Robustness

This section examines the robustness of our findings with respect to (i) different time periods, (ii) different control groups, and (iii) various estimation issues. Columns (1) and (2) of Table A.2 change the length of the pre-treatment period by minus and plus one month, respectively. Column (3) disregards the week before and the week after Easter (i.e., March 29 to April 11, 2021) as the implementation of the curfew in Hamburg was just before the Easter weekend. These two weeks are school holidays in most German states, in which many people take days off from work for holidays or family visits. Goodman-Bacon and Marcus (2020) discuss spillover effects as a threat for the identification of causal effects of NPIs. For this purpose, column (4) excludes all cities from the northern German states (Schleswig-Holstein, Bremen, Lower Saxony, Mecklenburg-West Pomerania) from the control group. Note that the neighboring regions of Hamburg are not part of our main control group. The nearest city in the main control group is Kiel, which is about 100 kilometers away from Hamburg. All other control cities are even further away.

Columns (5) and (6) add further cities to the control group. Column (5) also considers the eight cities with a population of more than 100,000 that are not independent counties, while column (6) also includes cities from Bavaria and Saxony, the two states that implemented similar curfew regulations before the nationwide regulations. Curfews in cities in these states were implemented both in the pre-treatment period and the post-treatment period as well as in parallel to the curfew in Hamburg. Column (7) weights all cities by their population, while column (8) takes the natural logarithm of the incidence rate to analyze whether our findings are robust to this transformation of the dependent variable. The latter robustness test is particularly relevant as the common trend assumption generally holds either in levels or in logs. When using the outcome in levels, we implicitly assume that the outcome develops linearly, while the log-specification is more directed to exponential growth rates. While the latter might be seen as more appropriate for a pandemic with exponential growth rates, Figure 1 shows that the linearity assumption provides a good approximation for the actual development of the incidence rate in the weeks preceding the curfew. It is reassuring that the results in column (8) show that the conclusion of our study does not depend on the scaling of the outcome variable. The point estimates suggest that the curfew reduced the incidence rate by about 25% in the second 10 days and by about 45% 21-29 days after the curfew implementation. Column (9) uses daily new cases as outcome variable in order to facilitate a comparison of results to other studies. The obtained daily point estimates imply that in the first 29 days after its implementation the curfew reduced cumulative Covid cases per 100,000 by about 160. As Hamburg has a population of about 1.84 million inhabitants, this means that the curfew prevented almost 3,000 reported infections in this time span.

All robustness checks confirm our main findings: The curfew did not reduce reported Covid cases during the first ten days after its implementation, while it substantially reduced reported Covid cases in the subsequent period.

Additionally, we perform placebo tests for the SCM results, following the idea of the "In-space-placebo"-tests by Abadie et al. (2010). More specifically, we construct a synthetic version of each city in the control group and plot the outcome difference between each city and its synthetic version over time. Figure A.1 in the appendix shows the curfew effect in Hamburg (black thick line) compared to the placebo curfew effects in all other cities (gray thin lines). It is striking that in the second half of the post-treatment period the curfew effect in Hamburg is more negative than all the placebo curfew effects.

Abadie et al. (2010) and Abadie et al. (2015) suggest to use the distribution of these placebo effects for an alternative method of statistical inference, similar to randomization inference. If there was no curfew effect, the probability that Hamburg would exhibit the most negative difference between the actual city and its synthetic counterpart would be $p = 1/36 = 0.028$. This is highly unlikely and, therefore, increases our confidence that the obtained effect is indeed due to the curfew and not due to random.

3.3. Effect heterogeneity

Table 2: Heterogeneity in the curfew effect

		By age					By sex	
	Main	5-14	15-34	35-59	60-79	80+	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Curfew_{1-10}	13.2	62.7***	10.6	6.5	-2.6	1.9	15.1*	9.9
	(8.2)	(13.8)	(12.0)	(10.0)	(6.8)	(14.0)	(8.3)	(8.8)
Curfew_{11-20}	-26.6***	6.5	-37.6***	-37.9***	-20.2***	-11.6	-25.0***	-30.4***
	(8.2)	(13.8)	(12.0)	(10.0)	(6.8)	(14.0)	(8.3)	(8.8)
Curfew_{20+}	-68.4***	-113.8***	-87.8***	-75.2***	-32.0***	-15.7	-69.9***	-65.8***
	(8.6)	(14.4)	(12.6)	(10.4)	(7.1)	(14.7)	(8.7)	(9.2)
N	3204	3204	3204	3204	3204	3204	3204	3204

Note: The table displays the effect of the nighttime curfew on the reported Covid-19 incidence rate separately for different age groups and for women and men. Standard errors in parentheses (* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$).

Information on the number of reported Covid cases is provided separately by age groups and sex, allowing to examine whether the curfew affected these groups differently. Table 2 shows the results for different age groups. The curfew did not reduce the incidence rate in any of the age groups in the first ten days after its implementation. However, over the next ten days we observe strong declines in the incidence rate for the age groups 15-34 and 35-59, the age groups in which people most often have private gatherings in the evening. The effects for the other age groups are either insignificant (ages 5-14 and 80+)

and/or clearly smaller (ages 60-79). In the following period (more than 20 days after the curfew implementation), we see stronger reductions across all age groups. This is in line with the idea of a trickle-down effect in the sense that non-infections in one age group will be also beneficial for other age groups in subsequent periods.

Finally, Table 2 shows that the effects are similar for women and men and, hence, provides little evidence for effect heterogeneity with respect to sex.

4. Discussion

This paper addresses the effectiveness of nighttime curfews in mitigating the spread of Covid-19. Based on the curfew in Germany's second largest city, Hamburg, in April 2021, the paper provides evidence that this curfew substantially reduced Covid-19 cases. About one month after its implementation, the curfew decreased cumulative Covid cases by about 160 per 100,000 population; put differently, the curfew prevented about 3,000 reported infections in Hamburg in this time span. We provide several arguments that support a causal interpretation of our results. First, before the implementation of the curfew, Hamburg and the control group exhibited very similar trends with respect to the outcome variable, making it more plausible that trends would have been similar in the period after if Hamburg had not implemented the curfew (common trend assumption). Second, our finding is confirmed both by difference-in-differences and synthetic control group methods. Third, we observe several patterns in the data that are in line with the effectiveness of the curfew: The curfew does not reduce reported Covid cases in the first days after its implementation, when the curfew is unlikely to be effective due to Covid-19's incubation period and subsequent reporting lags. Further, we find that the curfew has the strongest effect in the age groups that are most likely to have private gatherings in the evening.

In order to better understand the magnitude of our estimates, it is helpful to compare the estimated curfew effect to the effect of other NPIs. However, such a comparison is complicated by several factors. First, different studies use different outcome measures to evaluate the effectiveness of specific NPIs (e.g., case numbers, reproduction number, death rates). Second, studies that look at a wide range of different NPIs do not take into account the endogeneity of the policy and co-treatments in the form of other NPIs implemented at the same time or only a few days apart. Therefore, we focus on studies that evaluate the effectiveness of single interventions as these studies often apply more convincing strategies to identify causal effects. Third, studies focus on different stages of the pandemic and the magnitude of the NPI effects might differ substantially across the different stages (e.g., due to different outcome levels, different variants of the virus, vaccine availability etc.). For all these reasons, the following comparisons have to be taken with a grain of salt.

Friedson et al. (2021) analyze the shelter-in-place order in California in March 2020, which was the first in the U.S. and included the closure of non-essential businesses and a stay-at-home order for all non-essential activities. They find that this shelter-in-place order reduced cumulative Covid cases per 100,000 population by 160.9 to 194.7 about one month after its implementation. While our estimates for the curfew effect imply a similar magnitude on cumulative cases (see Section 3.2) as the lower bound of the shelter-in-place order effect, case numbers were way lower in California in March 2020, meaning that the proportionate effect of the SIPO was clearly larger. Backhaus (2022) studies the partial relaxation of travel restrictions in Europe in summer 2020, exploiting the different start of school holidays across German states. He finds that daily new cases increased by about 0.5 per 100 000 population 16-30 days after the school break. This point estimate is less than one tenth of the daily curfew effect (see Table A.2). Also exploiting differences in school holidays across German states, Isphording et al. (2021) show that school re-openings in summer 2020 did not increase the number of Covid infections. All in all, the comparison with previous findings on different NPIs suggests that the curfew is more effective than international travel restrictions and school closures but less effective than stay-at-home orders (but also less restrictive).

Our results refer to a situation when vaccines were not widely available and a variant of SARS-CoV-2 dominated (Alpha) that was more contagious than the original type but clearly less contagious than the Omicron variant that dominated in most countries at the beginning of 2022. A crucial question is what can be learned from our results for the future handling of the pandemic. It is unclear whether and how the virus will mutate in the next months and weeks (Ledford, 2022). It is not unlikely, however, that a new mutation of the virus will emerge with a high degree of immune evasion that causes severe disease. As it may take time to adjust the vaccine production to these new mutants, policy-makers might have to resort to non-pharmaceutical interventions again. Moreover, there might be situations where the number of sick people approaches a threshold where critical infrastructure like hospitals, police force, public transport or child care and education threatens to collapse. Such a collapse could not only lead to higher death rates and the postponement of necessary medical interventions but also to severe economic costs. Therefore, it is important to know about the effectiveness of previous non-pharmaceutical interventions in curbing the pandemic and controlling disease intensity. Because of the high share of vaccinated people and the case of multiple infections, the aims of non-pharmaceutical interventions shift from providing mostly medical protection to the maintenance of critical infrastructure. In line with this, several virologist (e.g., Germany's leading coronavirus expert Christian Drosten) think that in autumn/winter 2022/23 policy-makers will have to resort to harsher measures again (Der Spiegel, 2022). Further, several studies provide evidence that political trust induces higher rates of compliance with Covid-related containment policies (see e.g. Bargain and Aminjonov, 2020; Brodeur et al., 2021). However, in order to have trust in policy-makers, it is crucial that citizens are convinced that governments implement effective and reasonable policies. Therefore, it is important to provide evidence about the effectiveness of specific policies.

5. Conclusion

We conclude that the April 2021 nighttime curfew in Hamburg was effective in mitigating the spread of Covid-19 and that nighttime curfews should be kept in the toolbox of policy-makers to fight Covid-19. Curfews might be particularly useful in order to prevent situations, in which critical infrastructure like hospitals and the police force are overburdened due to a high share of infections among employees. While nighttime curfews restrict affected individuals, these restrictions are less severe than around-the-clock curfews.

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Appendix

Table A.1: City weights in the construction of the synthetic Hamburg

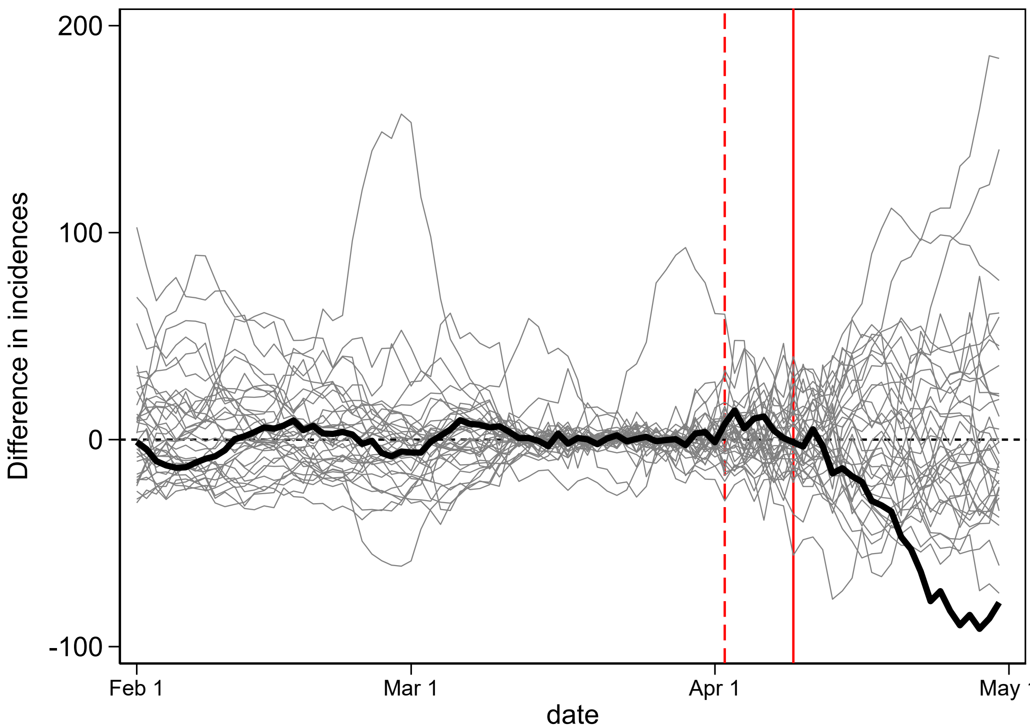
City	**Weight**	**City**	**Weight**
Berlin	0.008	Herne	0.062
Bielefeld	0.053	Jena	0.006
Bochum	0	Kiel	0
Bonn	0.116	Darmstadt	0
Bottrop	0	Kassel	0
Braunschweig	0	Wiesbaden	0
Bremen	0	Lübeck	0
Bremerhaven	0.137	Magdeburg	0
Dortmund	0	Mönchengladbach	0
Duisburg	0	Münster	0
Düsseldorf	0	Oberhausen	0.056
Erfurt	0	Oldenburg	0
Essen	0	Pforzheim	0
Frankfurt	0.383	Rostock	0
Freiburg	0	Solingen	0
Gelsenkirchen	0.077	Trier	0
Hamm	0	Ulm	0.085
Heidelberg	0.019		

Table A.2: Sensitivity analyses

	Alternative time periods			Alternative control groups			Estimation		
Main	- 1 month (1)	+ 1 month (2)	excl. Easter (3)	w/o 4 states (4)	more cities (5)	more cities 2 (6)	weights (7)	log(y) (8)	daily infect. (9)
Curfew_{1-10}	5.7**	17.4***	0.0	9.4***	14.9***	0.6	12.1***	-0.0	0.4
	(2.7)	(3.3)	(.)	(3.3)	(3.0)	(3.8)	(2.5)	(0.0)	(0.4)
Curfew_{11-20}	-34.1***	-22.3***	-25.2***	-33.9***	-25.7***	-49.6***	-27.1***	-0.3***	-6.8***
	(5.5)	(5.6)	(6.0)	(5.9)	(5.5)	(5.7)	(4.9)	(0.0)	(1.0)
Curfew_{20+}	-75.9***	-64.2***	-67.0***	-81.6***	-67.6***	-83.8***	-62.9***	-0.6***	-10.8***
	(8.6)	(8.3)	(9.0)	(9.1)	(7.9)	(6.3)	(10.0)	(0.1)	(1.4)
N	2196	4320	2700	2581	3916	6052	3204	3204	3204

Note: The table displays various robustness tests for the effect of the nighttime curfew on the reported Covid-19 incidence rate. Columns (1) and (2) change the length of the pre-treatment period by minus and plus one month, while column (3) disregards the week before and the week after Easter. Column (4) excludes all cities from the northern German states (Schleswig-Holstein, Bremen, Lower Saxony, Mecklenburg-West Pomerania) from the control group. Column (5) also considers the eight cities with a population of more than 100,000 that are not independent counties, while column (6) also includes cities from Bavaria and Saxony, the two states implemented similar regulations before the nationwide regulations. Column (7) weights all cities by their population, while column (8) takes the natural logarithm of the incidence rate. Column (9) uses as outcome daily new cases. Standard errors in parentheses (* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$).

Figure A.1: In-space-placebo results



Note: The graph displays the differences in incidences between every city and their synthetic control. The thick black line represents Hamburg and the thin gray lines represent all 35 other cities. The dashed vertical line indicates the date of the curfew implementation (April 2, 2021), while the solid vertical line indicates the date one week after the curfew implementation.