I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14334

Is Large-Scale Rapid Cov-2 Testing a Substitute For Lockdowns? The Case of Tübingen

Marc Diederichs
Peter G. Kremsner
Timo Mitze
Gernot J. Müller
Dominik Papies
Felix Schulz
Klaus Wälde

APRIL 2021

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14334

Is Large-Scale Rapid Cov-2 Testing a Substitute For Lockdowns? The Case of Tübingen

Marc Diederichs
Johannes Gutenberg University Mainz

Peter G. Kremsner
University of Tübingen

Timo Mitze
University of Southern Denmark

Gernot J. Müller
University of Tübingen, CESifo and CEPR

Dominik Papies
University of Tübingen

Felix Schulz
Johannes Gutenberg University Mainz

Klaus Wälde
Johannes Gutenberg University Mainz, CESifo and IZA

APRIL 2021

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Is Large-Scale Rapid Cov-2 Testing a Substitute For Lockdowns? The Case of Tübingen

Various forms of contact restriction have been adopted in response to the Covid-19 pandemic. Only recently, rapid testing appeared as a new policy instrument. If sufficiently effective, it may serve as a substitute for contact restrictions. Against this background we evaluate the effects of a unique policy experiment: on March 16, the city of Tübingen set up a rapid testing scheme while relaxing lockdown measures—in sharp contrast to its German peers. Comparing case rates in Tübingen county to an appropriately defined control unit over a four-week period, we find an increase in the reported case rate, robustly across alternative specifications. However, the increase is temporary and about one half of it reflects cases that would have gone undetected in the absence of extra testing.

JEL Classification: I18, C23

Keywords: COVID-19, number of tests, reported number of CoV-2 infections, (correcting the) bias, SIR model, unbiased epidemiological severity index

Corresponding author:
Klaus Wälde
Johannes Gutenberg University Mainz
Mainz School of Management and Economics
Jakob-Welder-Weg 4
55131 Mainz
Germany

E-mail: waelde@uni-mainz.de

Can large-scale CoV-2 testing strategies substitute for restrictive public health measures (aka lockdowns)? In theory, the idea is straightforward. If, first, every socially active person is subjected to a rapid CoV-2 test on a regular basis and, second, quarantined if tested positive, there is zero infection risk arising from social interactions. In this way, one would achieve the same outcome as a perfectly effective lockdown—but at much lower costs as, in contrast to a lockdown, it would be possible to maintain social interactions. Against this background, there have been calls for comprehensive and large-scale testing schemes early in the pandemic (*1*).

In practice, however, there are several possible complications. Perhaps most importantly, even an ideal testing procedure would generate false negatives, that is, some infections will necessarily go undetected (*2*). Moreover, its timing is critical for the testing strategy to work: if testing takes place too early, infected persons go undetected, if it takes place too late, the transmission of the disease may have already taken place. In fact, some observers suggest that for these reasons rapid tests do more harm than good (*3*). Lastly, testing and quarantining may be not sufficiently comprehensive, for instance, because of lack of compliance.

Lockdowns on the other hand are unlikely to prevent new infections altogether. First and foremost, they cannot be complete because some social interactions are essential. Second, their effectiveness also suffers from lack of compliance (*4*, *5*).

So, eventually, the question of whether large-scale CoV-2 testing strategies can substitute—fully or partially—for lockdown measures calls for an empirical assessment. A number of countries have opted for large-scale testing in response to the pandemic. For instance, by early April 2021, Denmark and Slovakia, both had cumulatively performed more than 3500 tests per 1000 people and thus about 6 times more than Germany. However, in these instances testing was not systematically introduced as a substitute for lockdown measures, but often as a complement. Second, we lack an appropriate control group against which we can benchmark infection dynamics in these countries.

This is why we turn to a uniquely suited policy experiment set up in the German town of Tübingen in mid-March 2021. It introduced a large-scale rapid testing scheme while simultaneously relaxing lockdown measures. Each person that tested negative was permitted to shop as well as to join other people in restaurants (although outdoors only). In order to set up this experiment, Tübingen got a special permit from the state government. And while several towns tried to obtain similar permits elsewhere in Germany, the case of Tübingen is unique in that it switched from lockdown to testing while other German municipalities were still in the lockdown mode.

We rely on these municipalities as a reference point to assess infections dynamics in Tübingen. This is essential for our evaluation of the experiment because infection dynamics gained considerable momentum all over Germany in March 2021. In order to perform a systematic comparison, we apply the synthetic control method (SCM) which allows us to construct a synthetic control unit against which we can benchmark the developments in Tübingen. SCM allows us to mimic an experimental setup and to study social phenomena in context where controlled experiments are not feasible (*6*). Moreover, SCM is used in the context of the Covid-19 pandemic to study the effect of making face masks mandatory or to quantify the effect of lockdown measures (*7*, *8*). But it is also used in other context, for instance, to quantify the impact of the Brexit referendum on economic performance in the UK (*9*).

1 The experiment

In order to appreciate the experiment under study, we briefly consider the developments in Germany prior to the experiment under study. In Germany the policy measures in response to the Corona pandemic are set at the state level and while policies differed somewhat across the 16 states, all states agreed to a range of measures in response to the second wave in December 2020.

In particular, non-essential shops, restaurants, and schools were closed. These measures were partly reversed in early March against the backdrop of rising infections numbers, presumably because the second wave of infections had died off by late February. Tübingen is located in the state of Baden-Württemberg (BW, for short). Here non-essential shops were opened on March 8 provided that the case rate in the county was below 50. Otherwise, a 'click & meet' scheme was put in place. Teaching at primary schools resumed on March 15. These measures were announced on March 5 by the state government and hence implemented on short notice.

On March 15 the state government also announced that starting the next day (March 16), the town of Tübingen would embark on a special experiment, centered around a large-scale rapid testing scheme, officially labeled 'Opening under Safety' ('Öffnen mit Sicherheit'), or 'OuS' for short. The town set up 9 testing posts where everybody would queue for about 5-30 minutes to be subjected to a rapid antigen test free of charge. After another 15 minutes the result of a test would be released and in the case it was negative, the subject was provided with a 'day ticket' entitling the holder to shop in non-essential stores, attend bars and restaurants (outdoors), cinemas and theaters (the OuS activities). In case the test was positive, people were asked to take a PCR test which is supervised by the public health office (Gesundheitsamt). These tests form the basis for the official statistics on which our analysis is based. The capacity for daily testing was 9000 and there were more than 30K tests per week (*10*).

At the regional level, Germany is organized in 16 states, which are subdivided in a total of 401 counties ("Landkreise" and "kreisfreie Städte"). Tübingen city (pop: 91K) is part of Tübingen county (pop: 229K). In total, there are 44 counties in BW. The experiment under study took place in Tübingen city only. Still, everyone living in Tübingen county was allowed to participate. Hence, spillovers from the city to the countryside may have potentially been significant. Also, detailed data is available only at the county level. In what follows, we therefore compare data for Tübingen county to those in other counties. In our baseline, we focus on the seven-day CoV-2 case notification rate per 100,000 ("case rate", for short), that is, the number of new CoV-2 cases per 100K people in the past 7 days.

To measure the causal impact of OuS, it is important to note that Tübingen is not exceptional in terms of fundamentals. However, it performed relatively well compared to its BW peers regarding CoV-2 case numbers (see appendix A.5.5 for more background). At some point, Tübingen county was indeed enjoying the lowest case rate in all of BW. Still, there have been many counties which did similarly well during the period. The experiment taking place in Tübingen rather than elsewhere is most likely a result of local idiosyncrasies and politics that are orthogonal to infection dynamics. The experiment, while approved by the state government, was devised jointly by the town's major and his Corona-commissioner. Both have gained prominence in national media as a result of vocal and eloquent interventions regarding the handling of the pandemic and, more importantly, because of their personalities. It seems that these personalities, rather than any special developments in Tübingen, have been causal for setting up the Tübingen experiment. It thus comes close to a randomized control trial.

2 Findings

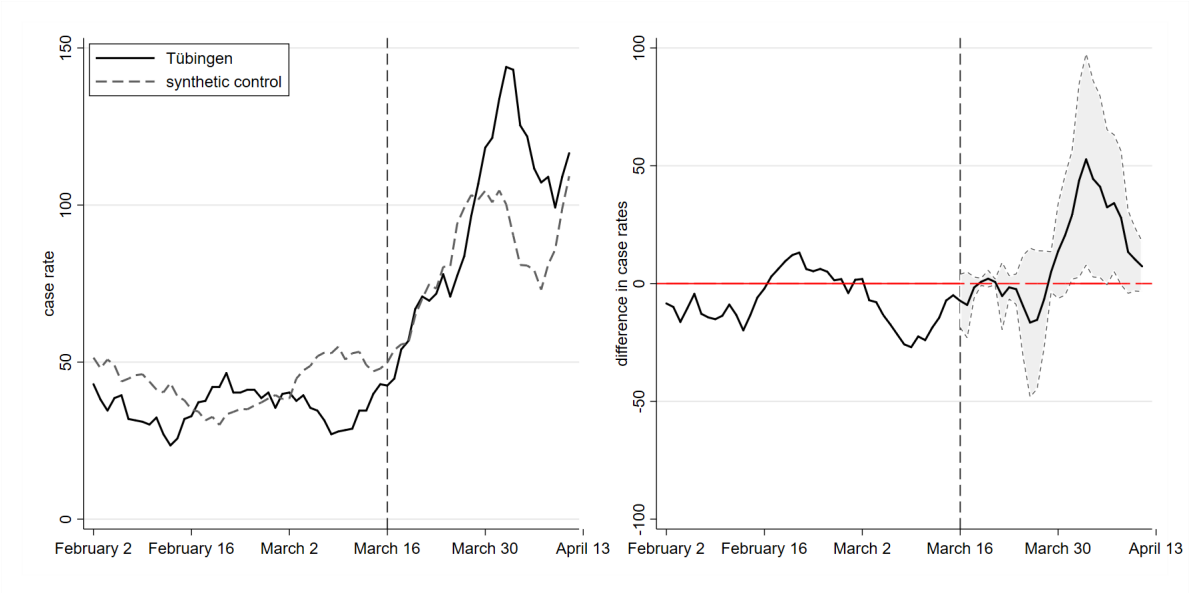
What is the effect of opening under safety (OuS) on infection dynamics in Tübingen?

2.1 Seven-day case rate

We start our analysis by describing the pandemic state by the most popular measure: the seven-day case rate. As the solid black curve in the left panel of figure 1 shows, the case rate in Tübingen was below 50 before the start of the project and increased to almost 150 at the

beginning of April during the Easter weekend. This increase was associated with OuS and led to wide public claims that "Tübingen failed".

Figure 1: Seven-day case rates for donor pool Baden-Württemberg



Notes: The left panel shows the seven-day case rate, the right panel shows the seven-day case rates between Tübingen and the synthetic control county. Control counties were chosen by SCM where the donor pool was restricted to counties from BW only, excluding neighboring counties of Tübingen.

It is clear that one cannot judge the success or failure of a project by comparing some measure (the case rate in our case) before and after the start of the project. Other factors than OuS might have affected pandemic dynamics in Tübingen over this period. We therefore need to compare the pandemic in Tübingen to other counties sharing various characteristics. This selection of counties should display comparable pandemic dynamics before the start of OuS in Tübingen, should share certain fundamental characteristics (like population density, age structure or medical services) and should be subject to very similar if not identical public health measures.

We identify such a set of control counties using our statistical method (for details, see the method section below) and by restricting the set of control counties from which to choose to counties in BW, excluding direct neighbors of Tübingen (listed in appendix A.5.4) given a high likelihood of spillovers. The restriction to the state of BW makes sure that all counties are subject to the same public health measures before OuS. The resulting counties and their weights constituting our synthetic control county are presented in table 1.

Table 1: Control counties and their weights for figure 1

Name	Weight
SK Heidelberg	0.431
SK Freiburg i.Breisgau	0.300
LK Enzkreis	0.254
LK Heilbronn	0.0160

As the table shows, the synthetic control county consists of two cities, Heidelberg and Freiburg, and two counties, Enzkreis and Heilbronn (which, however, is almost negligible with a weight of around 1%). Similar to Tübingen, Heidelberg and Freiburg are major university towns

that have similar population levels of between 160-230K and comparable socio-demographic structures (average age, share of highly educated inhabitants and similar job in-commuting structures). Local health care system (number of registered doctors and hospital beds) are also similar. The Enzkreis has a lower population density and thus complements the smaller and less agglomerated communities belonging to Tübingen county.

Given this background, we can now again turn to figure 1. We observe a good fit in the pandemic history since February 2021. Table 4 shows that the fit with respect to other criteria is also convincing.

If we want to enter into a detailed interpretation of day-to-day differences between Tübingen and its control county, we need to remind ourselves that the effects of any policy measure are visible in the data only with a certain delay. This is the result of incubation and a reporting delay. If 100 new infections arose on day 1, 50 of them (median) would be visible in the data between day 1 and around day 9, the rest later (see appendix A.3.2). Hence, given a 'real-world' treatment date of March 16, we need to study whether effects in the data are visible as of around March 24.

The best way to see the difference between Tübingen and its control is to consider the right panel of figure 2. We indeed find a strong increase in the difference as of March 24. This looks like a clear treatment effect for Tübingen due to OuS. The difference peaks three weeks after the start, that is, around April 1, just before the Easter weekend. The right panel also shows that this difference, while clearly visible, is hardly statistically different from zero at the 10% level. Nevertheless, a treatment effect is visible, OuS seems to increase the case rate – at least temporarily. Towards the end of our observation period, Tübingen and its synthetic control county hardly show any difference. Case rates are back to the synthetic control county's level. OuS seems to raise case rates only temporarily.

We note that Tübingen restricted the participation in OuS activities to inhabitants of Tübingen county as of April 1st. At the same time, outdoor areas by restaurants were closed and only pick-up was allowed. Given the previously discussed delay, this can *not* possibly be the reason behind the drop as of April 1st. It should have contributed, generally speaking, to the decline in case rates in Tübingen one to two weeks later.

2.2 Case rates and testing

There is one issue related to OuS which is relevant for all projects of this type. This issue is also of a much larger concern and has been discussed for a long time: does the number of reported infections increase when there is more testing?

One can argue that the answer is 'no' when a test is undertaken when a patient with Covid-19 symptoms visits a doctor. If the test follows from the examination of the patient by the doctor, the number of tests depends on the number of patients with Covid-19 symptoms. The number of reported infections therefore increases only when there are more patients with symptoms. Tests increase as a function of the state of the pandemic.

The argument is different when testing is the outcome of projects as, for example, the one of Tübingen. In this case, the number of tests does not depend on the state of the pandemic but on the number of participants and, on the national scale, on the number and scale of OuS projects. Similar arguments can be made with respect to testing travellers, testing sport professionals or all other preventive testing (see appendix A.3.3 for more background). In this case, more infected individuals are found when there is more testing.

To understand the effect of more testing during the project period, we start from the number of positive rapid tests. They amount to 45 (15 to 21 March), 39 (22 to 28 March), 29 (29 March to 4 April) and 30 (5 to 11 April) per week (*10*, *11*). While clinical studies are being undertaken, a good estimate about the share of positive rapid tests that is confirmed by a positive PCR

tests is lacking. A reasonable range seems to lie between 50% and 80%.

When we translate these weekly numbers of positive cases due to rapid testing into weekly rates (see section A.4.2 for details), we can compute the seven-day case rate that would have been observed in Tübingen in the case of OuS but in the absence of the positive cases which occur only because of rapid testing.

Table 2: The increase in the case rate in Tübingen due to OuS and the effects of rapid testing

	March 21	March 28	April 4	April 11
Difference	2.75	8.93	45.54	28.19
low predictive value (50%)	-11.75	-3.57	36.04	18.69
high predictive value (80%)	-20.45	-11.07	30.34	12.99

Notes: The first row ('difference') shows the increase in the seven-day case rates in Tübingen due to OuS as plotted in the right panel of figure 1. Case rates are based on reports of positive PCR tests. Assuming that 50% of the number of positive rapid tests are PCR confirmed, the second row shows the corrected effect of OuS. A negative sign indicates that OuS reduces the case rate. The third row shows the case where 80% of positive rapid tests are PCR confirmed.

Table 2 shows the differences in case rates for those four days for which we have weekly positive test counts. It subtracts the case-rate counterpart of positive test counts according to equation (A.7) for the case of a low and for a high predictive value. The case of a low predictive value of rapid tests assumes that only 50% of all positive rapid tests are confirmed by a positive PCR test. Under the assumption of a high predictive value, there are only few false positive cases, i.e. 80% of positive rapid tests are confirmed to be PCR positive.

These corrected case rates suggest a conclusion that OuS could actually *reduce* the case rate. Yet, overall, table 2 does show that OuS in Tübingen on average increased case rates. This is true especially around Easter (April 4), but case rates returned almost to the level of its control county afterwards.

We emphasize that this issue is of importance beyond OuS projects: Correcting reported cases by the number of positive tests from rapid testing should become routine when regulations and potentially even laws are based on case rates. Otherwise each region following a testing strategy to identify asymptomatic cases punishes itself by higher reported cases.

2.3 Understanding our findings

Our main message is that OuS in Tübingen did not lead to a substantial increase in case rates. On the contrary, OuS even provides built-in mechanisms that possibly reduce the number of cases. How can this be understood? Understanding means that we need some theory. Numbers are numbers and a comparison of numbers does not explain differences. What is the effect of OuS from a conceptual perspective?

First, OuS implies, by definition, more testing. Second, participants in the OuS activities have contacts in the activities constituting the project and beyond (see appendix A.3.3 for more details). More testing allows the identification of asymptomatic cases. This clearly has a positive effect on the pandemic: Imagine a group of, say, five visitors. Imagine further that one of these five visitors is infectious. If these five visitors meet in private, it is likely that some non-infected of this group gets infected during this meeting. If these five visitors participate in some OuS activities, the infectious individual is sorted out and would not infect the others.

The downside of OuS is the potential increase in risky contacts. Testing does not identify exposed individuals (they are infected but not yet infectious) and there are false negative test results. Hence, some infection risk is always left. More contacts should therefore lead to more

infections. (As a side remark, if contacts in the context of OuS substitute for otherwise private contacts, the number of contacts due to OuS might actually not be higher than without OuS.)

A priori, it is therefore unclear whether OuS leads to more or less reported infections. These simple theoretical considerations also show, however, that one can easily imagine a scenario in which OuS possibly even reduces the number of infections.

3 Method

To estimate the causal effect of OuS (the 'treatment'), on infection dynamics in Tübingen (the 'treated unit'), we require a control unit that is comparable to Tübingen in terms of relevant socioeconnomic factors as well as in terms of pre-treatment trends. To this end, we rely on the synthetic control method (SCM), proposed for the causal assessment of policy interventions on the basis of aggregate outcome measures (*12*).

At the heart of this method lies an estimator which identifies, in our application, counties in Germany to which Tübingen county can be compared. This comparison is based on information observable prior to treatment and summarized by a set of predictor variables. In our case, this set includes several observations for the infection rate in the weeks prior to treatment and other relevant characteristics such as, for instance, the old-age dependency ratio. Table 4 in the appendix reports the full list of predictors for our preferred specification.

The control unit is constructed by minimizing the 'Root Mean Square Percentage Error Loss' (RMSPE) which quantifies the distance of the (weighted sum of) comparison counties to Tübingen prior to treatment. SCM requires an a-priori list of counties from which to construct the control unit (the 'donor pool'). In our preferred specification, the donor pool consists of all counties of BW, except for Tübingen county and its direct neighbors. In robustness checks (see appendix A.5), we extend the donor pool to include all counties in Germany. In terms of outcome variable, we focus on the 7-day case rate and provide robustness checks for alternatives in appendix A.6. We provide more details on the method in appendix A.4.1.

4 Discussion

As we emphasise in the method section above, results of a comparison of a county without synthetic county depend on (a) the measure used (outcome variable), (b) the criteria employed to find comparable counties (predictor variables) and (c) the donor pool, i.e. the group of counties from which to choose comparable counties. This section therefore discusses the impact of variations in our choices. A short preview would reveal that changes in outcome variables and predictor variables have no major effect on our overall evaluation. Changes in the donor pool do however influence results significantly. For this reason, we want to start off our discussion with the latter.

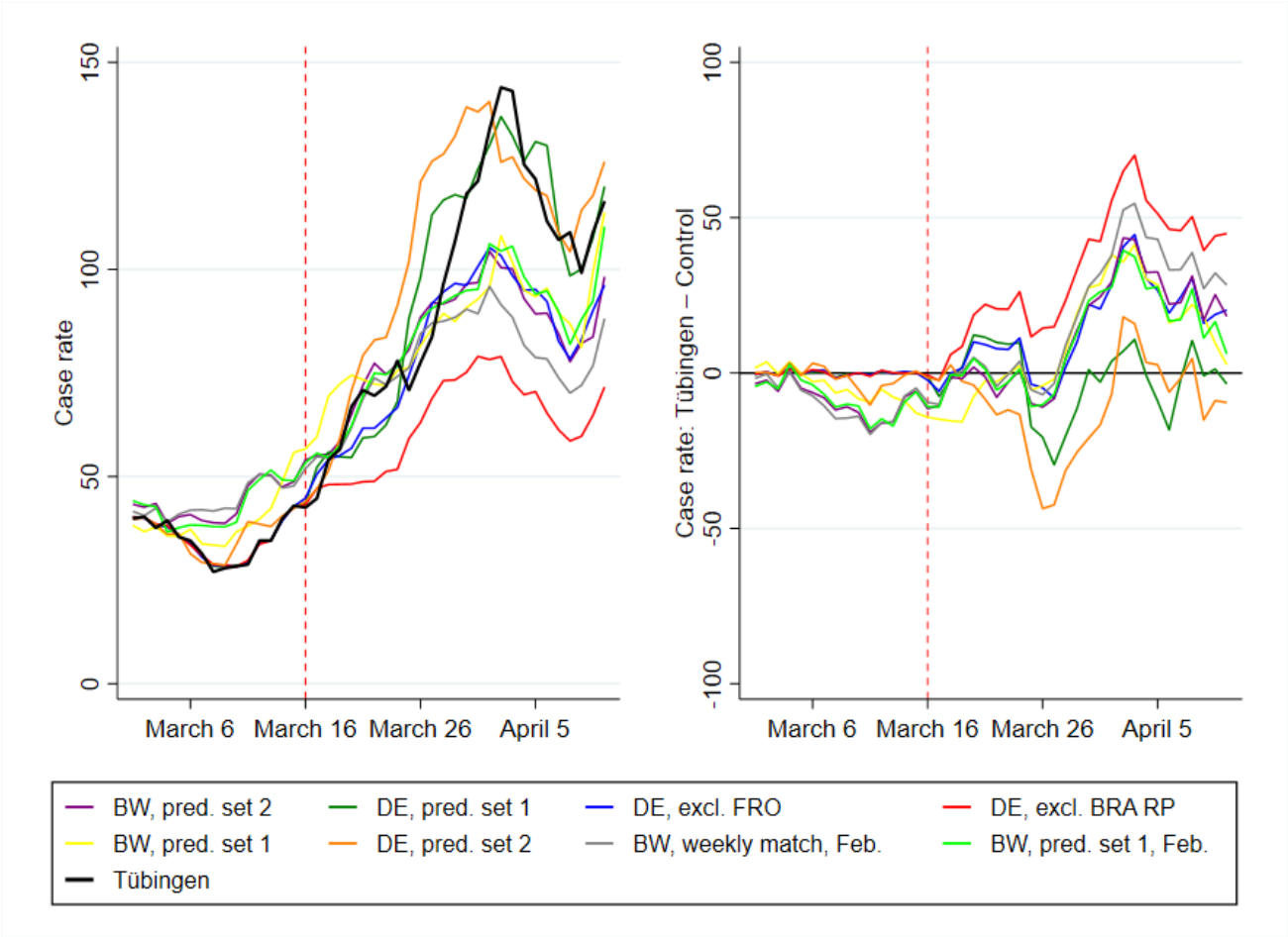
4.1 The role of the donor pool

- No optimistic story

We could have told a very optimistic story about Tübingen. It is the outcome of a SCM analysis that allows all counties in Germany to be part of the donor pool and puts a strong emphasis on short-run dynamics in the predictor set (our 'predictor set 1' is shown in table 10). The larger the donor pool, the larger the choice among counties, the larger the "chance" that a county similar to Tübingen is found and the better the outcome of the minimisation problem of the SCM (see appendix A.4.1). The evolution of the case rate for the resulting synthetic control

region is displayed as the green line ('DE, pred. set 1') in figure 2. It basically tracks Tübingen. OuS would have no effect.

Figure 2: Seven-day case rates for all specifications



Notes: Case rates and their difference between Tübingen and the respective synthetic control counties are shown for all relevant SCM specifications. Apart from two perfect but not highly robust fits ('DE, pred. set 1' and 'DE, pred. set 2'), all other specifications confirm the baseline specification in figure 1

The downside of this approach consists in the risk that some counties chosen by SCM for the synthetic control county might have experienced opening measures similar to OuS in Tübingen. In fact, the control county for this specification contains Frankfurt/Oder, a region that also allowed shops to open in mid March (see (*13*) or (*14*)). We therefore exclude Frankfurt/Oder from the donor pool. The strong effect of Frankfurt/Oder is confirmed by a leave-one-out and leave-all-out analysis in section A.5.2. Given this, we do not consider this optimistic story to be statistically convincing.

When we put more emphasis on longer-term predictors (our predictor set 2) we achieve a similarly good fit and a zero-effect of OuS in Tübingen. This is the yellow-ochre ('DE, pred. set 2') curve in figure 2. Even though the synthetic control county does not include Frankfurt/Oder, a leave-all-out analysis (see appendix A.5.3) also shows that this specification is not robust. Hence, both findings that OuS does not lead to additional cases turned out to be non-robust. We therefore conclude that OuS does lead to some increase in cases, at least temporarily.

- Conservative Germany-wide approaches

Given the experience with Frankfurt/Oder as another treated region in Germany, we let SCM choose control counties from restricted donor pools. The first restricted donor pool we

propose includes all 401 German counties without Tübingen and Frankfurt/Oder. We then excluded all of Brandenburg (where Frankfurt/Oder is situated) plus Rhineland-Palatinate, as the latter also, at least temporarily, allowed restaurants to serve outdoors. Both specifications lead to developments of case-rates (see 'DE, excl. FRO' in blue and 'DE, excl. BRA RP' in red in figure 2) that are similar to the evolution in our baseline specification. The specification excluding Brandenburg and Rhineland-Palatinate is the one according to which OuS would have the worst effects.

- The robustness of our baseline specification

We also investigate the stability of our baseline specification for BW in figure 1. We change the predictor set to predictor set 2 (see table 12), putting more emphasis on long-run stable predictors. We also vary the pre-treatment period employed to construct Tübingen's synthetic control county. As figure 18 shows in detail and figure 2 joint with our other robustness checks, none of these variations led to a substantial change in our prediction.

4.2 The role of the pandemic measure

The seven-day case rate as employed in figure 1 is the measure of the pandemic state that receives most of the attention around the world. It is not clear, however, whether this is the best measure for a pandemic. It is also not clear, whether this is the best measure to compare the evolution of the pandemic across regions. A moving average over a period of seven days is much more short-run in nature than for example the simple sum of all new infections since some starting point.

We therefore investigate the total number of reported infections since January 2021 per 100,000 inhabitants as dependent variable. While the details are in appendix A.6.1, we find that using this definition, the synthetic twin of Tübingen consists of different counties than our benchmark analysis above. The fit was however much better compared to the specification above, as cumulative infections over a longer period than only 7 days are less volatile. Finding similar counties is therefore easier. What is most important, however, is the evaluation of OuS: We confirm the findings from above. There is a small but not prohibitive difference between Tübingen and comparison counties. OuS appears to be working.

By contrast, we find significantly different results whenever we do not normalize the infections numbers. If not caculated per capita, Tübingen fares worse than its synthetic twin. This is true for when we observe the total number of reported infections since January 2021 (*not* per 100,000 inhabitants) in appendix B.2 or the total number of reported infections over the previous 7 days (hence the non-normalized counterpart of the standard seven-day case rate) in appendix A.6.2.

We do not believe, however, that these findings contradict our previous results, nor that they provide any understanding of the pandemic in general and the effects of OuS in detail. In almost any SIR-type model, infection risk depends on both (i) the number of contacts and (ii) the probability that the contact takes place with an infectious person. (See (*15*) for a detailed discussion of the specification of an infection rate in SIR models.) The probability of meeting an infectious person in a certain region does therefore plausibly *not* depend on the absolute number of infectious individuals in the region alone. It is rather a function of the *share* of infectious individuals in this region.

There is a second reason why non-normalized cases do not seem plausible: All public health discussions center around normalized cases. Many regulations are based on case rates, i.e. normalized cases. Hence, cases in a region must plausibly be normalized by population size also for a statistical analysis. We therefore do not attach too much importance to findings based on non-normalized cases.

4.3 The role of predictors

It is clear that the choice of predictors, i.e. the variables by which we compare counties determines what regions end up being part of our synthetic and untreated Tübingen. Depending on what counties are chosen by SCM, we achieve different differences between Tübingen and the synthetic county. We therefore begin with an explanation of the predictors for our baseline specification and then report the effects of varying the predictor set.

Table 4 in the appendix shows our predictors. They consist of two subsets. First, lagged outcome variables and, second, fundamental regional characteristics (like for example population density, age structure or medical services). The choice of predictor variables is driven (partly by their availability and) mainly by the desire to identify comparable counties based on fundamental determinants driving the outcome variable. In an ideal world, one would include those measures as predictors which determine the evolution of the pandemic in a county. As these ideal predictors are not available, regional characteristics and lagged outcome variables serve as proxies for the latent true variables.

As most SCM applications we are aware of work with low-frequency (like annual or quarterly) data, we experiment here with adding more high-frequency predictors. Appendix 4.3 shows that including high-frequency predictors improves the fit between Tübingen and its twin. At the same time, it detaches Tübingen from its twins with respect to the more stable long-term characteristics.

Concerning our robustness concerns, we were relieved to see in figure 17 that Tübingen fares just as well as its synthetic county as in our baseline specification. Varying predictors therefore does not change our basic conclusion.

4.4 The future of Opening under Safety

What do our findings mean from a more general perspective? If we should draw lessons for future OuS projects, the following would be our top priorities: Replicating the experiment in Tübingen elsewhere should be done with care. Tübingen had an excellent starting point with a very low initial case rate compared to its peers (see figures 5 and 9). Running OuS-projects in high incidence regions both poses the risk of a fast increase of cases and the chance of finding more asymptomatic cases. If such a project was monitored on a daily basis (which would be very simple if existing case data at the community level were made available publicly), it would be worth a try.

The effect of rapid testing on reported cases needs to be taken into account. Test centres should be strongly encouraged to publish data on positive cases by postcode. This would allow to draw a distinction between cases resulting from the dynamics of the pandemic and cases resulting from rapid testing. The latter could be achieved if health authorities recorded *and* reported the reason for a test (symptoms, contact person, on the job, rapid test etc). If additional cases discovered through (PCR confirmed) rapid testing of asymptomatic individuals are not subtracted from overall cases, regions undertaking rapid testing would punish themselves by higher cases.

Can OuS experiments be justified in times of high and increasing case rates? Various studies based on SIR models (inter alia (*16*), (*17*), (*18*)) estimate the effects of public health regulations. Some conclude from these studies that lifting contact restrictions must worsen the pandemic state. As these findings were obtained at a time when rapid testing was *not* available, these conclusions appear premature.

Whether OuS experiments should be undertaken in times of increasing case rates also depends on one's view where infections take place. If infections mostly occur because of private contacts, additional regulation of public contacts is of little use. The issue of health policy is then an issue of compliance and enforcement. If enforcement of rules for private contacts is

not possible, individuals need incentives to cooperate. If rapid testing is more acceptable with a reward (like visiting a restaurant), many people will accept rapid testing. If vaccination is more acceptable with a reward, more people would get a vaccination. OuS might be a way to increase fast testing rates and thereby help identify asymptomatic cases. If the latter accept quarantine (given the issue of enforcement and compliance), case numbers will fall through OuS.

Data on repeated testing would be very informative. What is known about individuals that take part in OuS events? Is the share of infected individuals higher after the event compared to individuals who did not take part? Test outcomes of one and the same person should be merged by testing centers. If data protection prevents this, data protection helps the pandemic to continue.

Acknowledgments

We would like to thank Peter Martus for guidance to testing data and Viola Priesemann for discussions and comments. Authors are funded by their universities, contributed equally and declare no competing interests. All data and code is available in the manuscript or the supplementary appendix.

References

1. P. Romer, "Roadmap to responsibly reopen America", tech. rep., (https://roadmap.paulromer.net/paulromer-roadmap-report.pdf).
2. J. Dinnes *et al.*, en, *Cochrane Database of Systematic Reviews*, Publisher: John Wiley & Sons, Ltd, ISSN: 1465-1858, (2021; https://www.cochranelibrary.com/cdsr/doi/10.1002/14651858.CD013705.pub2/full) (2021).
3. G. Guglielmi, "Rapid coronavirus tests: a guide for the perplexed", Nature, news feature.
4. G. Graffigna *et al.*, *PLoS ONE 15(9): e0238613.* (2020).
5. N. B. Masters *et al.*, *PLoS ONE 15(9): e0239025*, https://doi.org/10.1371/journal.pone.0239025 (2020).
6. A. Abadie, A. Diamond, J. Hainmueller, *American Journal of Political Science* **59**, 495–510 (Apr. 2015).
7. T. Mitze, R. Kosfeld, J. Rode, K. Wälde, *Proceedings of the National Academy of Sciences* **117**, 32293–32301, ISSN: 0027-8424, (https://www.pnas.org/content/117/51/32293) (2020).
8. B. Born, A. M. Dietrich, G. J. Müller, *PLOS ONE* **16**, 1–13, (https://doi.org/10.1371/journal.pone.0249732) (Apr. 2021).
9. B. Born, G. Müller, M. Schularick, P. Sedláček, *The Economic Journal* **129**, 2722–2744, (https://doi.org/10.1093/ej/uez020) (May 2019).
10. B. Palmer, L. Federle, *Stadt Tübingen: Der Oberbürgermeister*, (www.tuebingen.de/Dateien/modellprojekt_zweiter_zwischenbericht_land.pdf) (2021).
11. Gesundheitsamt Tübingen, *Landkreis Tübingen*, (https://www.kreis-tuebingen.de/Abteilung+33+_+Gesundheit.html) (2021).

12. A. Abadie, A. Diamond, J. Hainmueller, *SYNTH: Stata module to implement Synthetic Control Methods for Comparative Case Studies*, Statistical Software Components, Boston College Department of Economics, Oct. 2011, (https://ideas.repec.org/c/boc/bocode/s457334.html).

13. *Allgemeinverfügung der Stadt Frankfurt (Oder) Nr. 08/2021 - erweiterte Schutzmaßnahmen Inzidenz über 100*, (https://www.frankfurt-oder.de/PDF/Allgemeinverf%C3%BCgung_der_Stadt_Frankfurt_Oder_Nr_08_2021_erweiterte_Schutzma%C3%9Fnahmen_Inzidenz_%C3%BCber_100.PDF?ObjSvrID=2616&ObjID=9503&ObjLa=1&Ext=PDF&WTR=1&_ts=1617345595).

14. *Brandenburg zwingt Frankfurt (Oder) zur Notbremse*, (2021; https://www.rbb24.de/studiofrankfurt/panorama/coronavirus/beitraege_neu/2021/03/brandenburg-frankfurt-eindaemmungsverodnung-kassiert.html).

15. J. R. Donsimoni, R. Glawion, B. Plachter, K. Wälde, *German Economic Review* **21**, 181–216 (2020).

16. J. Dehning *et al.*, *Science* **369** (2020).

17. R. Kosfeld, T. Mitze, J. Rode, K. Wälde, *Journal of Regional Science* **forthcoming**, eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/jors.12536, (https://onlinelibrary.wiley.com/doi/abs/10.1111/jors.12536).

18. S. Hsiang *et al.*, *Nature* **584**, 262–267 (2020).

A Supplementary appendix

A.1 Data

A.1.1 General information

Data on reported SARS-CoV-2 infections are taken from the Robert Koch Institute (*1*). Infections are identified by PCR tests. For our empirical analysis we use aggregate case numbers for each county and day based on the reporting date by local health authorities. Time-varying predictors are the average daily temperature and daily mobility changes for each county during the pre-treatment period until March 16, 2020. Mobility changes (in percent) based on individual mobile phone data are computed as difference in mobility patterns between a specific date and the average value for the corresponding weekday from the same month in 2019 (pre-COVID benchmark period). To give a specific example: The mobility change for Wednesday, March 10, 2021 is calculated as difference in the number of regional trips for this date and the average number of trips on Wednesdays in March 2019. We use data on daily temperatures from Deutscher Wetterdienst (*2*) and updated data on mobility changes per county and day are obtained from (*3*).

We further include time-constant cross-sectional predictors characterizing regional demographic structures and the regional health care system as in (*4*) based on data from the INKAR online database of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (*5*). We use the latest year available in the database, which is 2017, and rely on the following cross-sectional predictor variables: population density (Population/km2), the share of female in population (in %), the average age of female and male population (in years), old- and young-age dependency ratios (in %), the number of medical doctors per 10,000 of population and pharmacies per 100,000 of population, the regional settlement structure (categorical dummy), and the share of highly educated population (in %).

A.1.2 Descriptive statistics

Table 3 shows descriptive statistics for all variables used in our analyses. The variables are measured on the district level and the underlying population is Germany without direct neighboring counties of Tübingen, listed in appendix A.5.4. The latter are excluded from all analyses. Panel A contains all variables related to measuring the development of the pandemic. Panel B displays information on the time varying predictors mobility and average air temperature and panel C shows all predictors related to the districts demographic structure and their health care coverage.

Table 3: Descriptive statistics

	Mean	S.D.	Min.	Max.
A: Data on reported CoV-2 cases				
Seven-day CoV-2 case notification rate per 100,000	106.85	68.91	3.74	66.77
Cumulative infections per 100,000 inhabitants since January 1st	5895.28	8317.93	343	151095
Cumulative cases over previous 7 days	211.50	286.32	2	7338
B: Time-varying predictors				
Mobility	-.11	.14	-.74	.73
Average temperature	3.02	4.88	-17.50	19
C: Regional demographic structure and local health care system				
Population density (inhabitants/km^2)	535.44	705.34	36.13	4686.17
Share of females in population (in %)	50.60	.64	48.39	52.74
Average age of female population (in years)	45.88	2.12	40.70	52.12
Average age of male population (in years)	43.18	1.84	38.80	48.20
Old-age dependency ratio (persons aged 65 years and above per 100 of population aged 15-64 years)	34.39	5.49	22.40	53.98
Young-age dependency ratio (persons aged 14 years and under per 100 of population aged 15-64 years)	20.53	1.44	15.08	24.68
Medical doctors per 10,000 of population	14.62	4.42	7.33	30.48
Pharmacies per 100,000 population	27.04	4.91	18.15	51.68
Categorical variable$^{\$}$ for population density of NUTS3 region	2.60	1.05	1	4
Share of highly educated* persons in regional population (in %)	13.05	6.21	5.59	42.93

Notes: * = International Standard Classification of Education (ISCED) Level 6 and above; $^{\$}$ = included categories are 1) larger cities (kreisfreie Großstädte), 2) urban districts (städtische Kreise), 3) rural districts (ländliche Kreise mit Verdichtungsansätzen), 4) sparsely populated rural districts (dünn besiedelte ländliche Kreise).

A.2 Literature

In theory, it is clear that testing and quarantining can dramatically reduce the costs of an epidemic (*6*). A systematic empirical assessment, however, of the benefits of widespread rapid testing based on antigen tests is still missing (*7*). In the present paper, we seek to contribute to such an assessment by studying a unique policy experiment, in which widespread rapid antigen tests were coupled with opening of non-essential infrastructure. We estimate the causal effect of this intervention using the synthetic control method (*8–10*). This method, SCM, for short, is the vehicle for our empirical identification strategy.

SCM has been frequently used in the social sciences to study the effect of policy interventions, broadly defined, on political, social, and economic outcomes (*10*). In these contexts, SCM has been shown to be a flexible and robust estimation tool. In addition, it has also been applied to COVID-related research, for instance, to study the effectiveness of lockdown measures by means of a counterfactual analysis for Sweden (*11*, *12*) and to study the effect of shelter-in-place policies in California (*13*). In addition, (*4*) use SCM to study the effect of face masks on SAR-CoV-2 cases in Germany. The SCM approach has also been used in the interim evaluation of the Liverpool mass-scale testing project (*14*). While similar to the Tübingen experiment, this pilot was centered around repeated testing of asymptomatic individuals, those with a negative result were not allowed to participate in otherwise restricted activities. Compared to the synthetic control region, they find that large scale testing does not significantly decrease case numbers and hospitalization.

Under the SCM, identification is based on a counterfactual that mimics a situation in which the treatment in treated regions (here: a re-opening of public life and the local economy in conjunction with a large-scale rapid testing scheme) would not have taken place. In Section

A.4.1, we explain in detail how we implement SCM in the context of the present study.

A.3 Findings

A.3.1 Our baseline result

The synthetic twin county employed in figure 1 consists of 4 counties who are listed, jointly with their weights, in the main text in table 1. Their fit with respect to predictors and the RMSPE is in table 4.

Table 4: Pre-treatment predictor balance and RMSPE for Figure 1

	e(X_balance)	
	Treated	Synthetic
cum_cases7(68)	63	78.117
cum_cases14(74)	158	163.439
i7_rate(32)	44.29581	49.93466
i7_rate(39)	31.45002	42.83748
i7_rate(46)	31.89298	30.47054
i7_rate(53)	40.30919	35.23315
i7_rate(60)	39.86623	41.55996
i7_rate(67)	27.02044	41.37256
i7_rate(74)	42.96693	47.02528
mobility(68(1)74)	.0011557	-.1706146
average_temperature(68(1)74)	4.214286	7.101671
Population density	434.8634	1171.571
Share of females in population	51.25601	51.5463
Average age of female population	41.67062	41.9366
Average age of male population	40.03484	39.91151
Old-age dependency ratio	24.57881	25.2647
Young-age dependency ratio	20.20369	18.35703
Medical doctors per population	15.63642	22.25934
Pharmacies per population	23.47678	29.47752
Categorial variable for population density of NUTS3 region	2	1.273
Share of highly educated persons in regional population	26.46966	32.70743
RMSPE (pre-treatment)	8.75	

Table 4 displays the criteria (predictors) which we selected for SCM to choose control regions based on predictor values in the pre-treatment period before March 16, 2021. Predictors can be split into groups: lagged pandemic measures (the outcome variable) and structural regional characteristics, which are expected to influence the local infection dynamics over time. As the table shows, we place a strong emphasize on lagged values of the seven-day case rate as predictor in order to ensure that Tübingen and the selected control regions follow a common pre-trend in the last two weeks before the OuS experiment stated in Tübingen. We also include an average measure for the cumulative number of SARS-CoV-2 cases in the two weeks before treatment start.

With regard to structural regional characteristics, we use both time-varying and time-constant predictors. As such, we use average levels for daily temperature and intra-regional mobility changes in the week prior to the treatment. The link between seasonality and infection dynamics has recently been studied (*15*). Including mobility effectively controls for social

interaction as a driver of local infection dynamics and also as a measure how closely people follow prevailing (lockdown) policy rules (*16*).

Additionally, we control for the share of females in population, average age of female population, average age of male population, old-age dependency ratio, young-age dependency ratio, medical doctors per population, pharmacies per population, categorial variable for population density of NUTS3 region and share of highly educated persons in regional population as suggested in (*4*). The rationale behind the inclusion of these predictors is to match Tübingen as closely as possible to its synthetic control group in terms of socio-demographic factors and factors related to the local health care system. Previous research has shown that these factors are significantly related to differences in COVID-19 incidence and death rates at the sub-national level (*17*).

The overall inspection of the pre-treatment prediction error (RMSPE) for the SCM specification shown in Table 4 underlines the good fit between the seven-day case rate development in Tübingen and its syntethic control group as already visualized in figure 16.

A.3.2 The reporting delay

Imagine a public health measure is implemented on a certain day and that it is effective. When should we see the effects in the data? This delay between measure and statistical visibility depends on the usual incubation period and on the reporting delay. The incubation period is well-studied and has a median of 5.2 days and 95% of all delays lie in the range of around 2 to 12 days. They seem to be approximately log-normally distributed (1, 2). The reporting delay was studied in general and applied to Germany in (*18*). It consists of a delay due to diagnosis, testing and reporting of the test. We update the findings on (*18*) for our purposes here.

Again, let D_I denote a random variable that describes the incubation period. Let D_R denote a second random variable that describes the delay between perceptible symptoms and reporting to authorities of a positive SARS-CoV2 test. We are interested in the distributional properties of the overall delay defined as $D = D_I + D_R$. We will take the median of D as our measure for how long it takes before effects of public health measures are visible in the data. Information on the date of reporting and on the day of first symptoms is provided in (3). The difference between these two dates gives a vector of realizations of the random variable D_R.

Findings for incubation. (*19*) and (*20*) describe the delay between infection and symptoms, i.e. the incubation period, by a lognormal distribution. To be precise about parameters in what follows, a lognormal distribution $f(x)$ of a random variable X is characterized by a dispersion parameter σ and scale parameter μ. (*20*) report $m = 5.1$ and that 95% of all cases lie between 2.2 and 11.5 days. The latter reads, more formally $\int_{2.2}^{11.5} f(x)dx = 0.95$. This implies $\sigma = 0.4149$. The scale parameter is given by $\mu = ln5.1 = 1.63$.

Table 5: Descriptive statistics for the reporting delay D_R

Sample	Period	Mean	Median	Variance	Standard Deviation
A	Jan 7 to May 6, 2020	6.80	6	30.92	5.56
B	May 7, 2020 to March 16, 2021	5.38	4	80.21	8.96

Note: The RKI data set downloaded on June 7, 2020 (April 8, 2021) contains 119,917 (851,576) observations with information on day of infection until re-porting day May 6, 2020 (March 16, 2021). We focus on 118,618 (831,328) with $D_R \geq 0$.

Findings for reporting. The mean, median (50% percentile), variance and standard deviation of DR in (*18*) are in the first row of table 5. The second row displays the same summary

statistics from May 7, 2020 to March 16, 2021.

Merging the two. When we merge incubation and reporting, we consider the sum of two random variables, $D = D_I + D_R$. The mean is $ED = ED_I + ED_R$ and the variance reads $VarD = VarD_I + VarD_R$ assuming independence between the two random variables. More distributional in-formation can be obtained from a convolution analysis (*18*). We obtain the following percentiles.

Table 6: Percentiles of total delay D

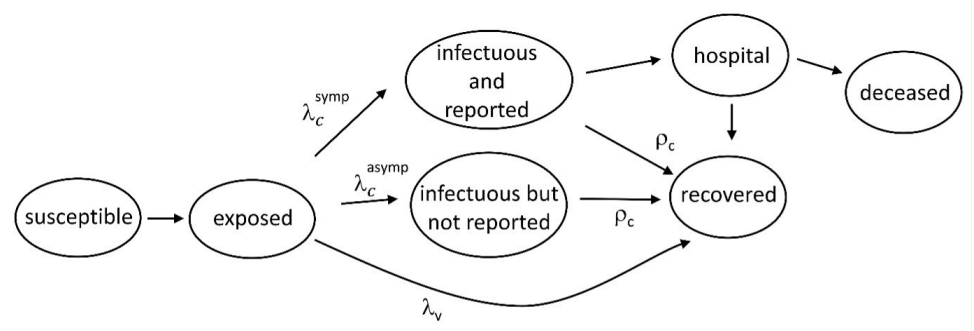
Sample	1	2.5	5	10	25	50	75	80	90	95	97.5	99
A	3.42	4.09	4.78	5.70	7.65	10.52	14.30	15.41	18.74	22.22	26.29	34.23
B	3.21	3.8	4.38	5.16	6.76	9.07	12.08	12.96	15.69	18.54	21.86	28.51

A.3.3 Opening under Safety in a SIR framework

- Sketch of a model

To understand the effects of opening under safety (OuS), we start from a fairly standard description of a pandemic illustrated in figure 3. Each circle represents the (expected) number of individuals of a certain region in the respective state. When individuals are infected, they are in state E like exposed. When infectious, they are either symptomatic or asymptomatic. Thereafter, they can recover, enter hospital or even die. Models of this type have been employed e.g. by (*21*), (*18*) or (*22*). We assume for illustration purposes that tests are undertaken only if individuals visit a doctor and display symptoms related to Covid-19. All reported infections are therefore symptomatic infections (Covid-19 cases). Tests employed in this case are PCR tests.

Figure 3: An extended SIR model

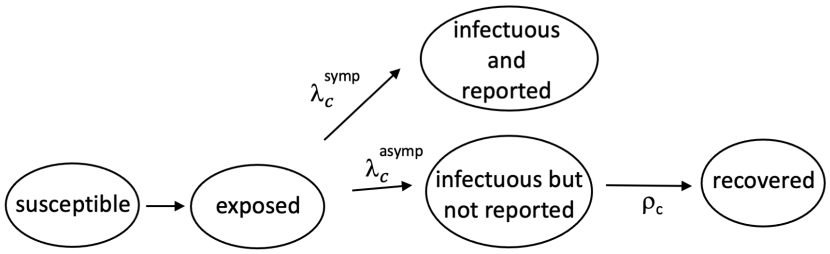


What is the effect of rapid testing (which are not PCR tests) in such a framework? We assume that symptomatic individuals, individuals in hospital (and deceased individuals) do not show up for rapid testing. Hence, tests are applied to susceptible, exposed, asymptomatic infectious and recovered. Identified infectious individuals do not receive a day pass such that visitors with a day pass are under much lower infection risk. What is more, the rest of the population is also subject to lower infection risk due to the discovery of asymptomatic cases (assuming they enter quarantine).

Due to false negative tests and as exposed cannot be detected, individuals holding a day pass include susceptible, exposed, asymptomatic infectious (at a much lower share than before testing) and recovered individuals. The dynamics of the pandemic of a negatively tested group

therefore follows an adjusted SIR model as illustrated in figure 3. Exposed individuals can turn infectious with or without symptoms, susceptible individuals can turn exposed and infectious individuals can recover.

Figure 4: A SIR model for an OuS project



- The effects on the number of reported infections

What happens to the number of reported infections in an OuS region? (See (*23*) for a more general analysis of the testing bias in reported infections.) Following our model sketch, rapid testing identifies asymptomatic infectious individuals. A certain share of them will be confirmed to be positive by a subsequent PCR test. The number of reported infections therefore no longer just includes symptomatic but also asymptomatic infectious individuals. The number of reported infections therefore rises by the number of discovered asymptomatic cases. If no OuS had taken place, the number of reported infections would still consist only of the number of symptomatic infectious individuals.

From a theoretical perspective, we should therefore expect that OuS leads to more reported infections (due to discovered asymptomatic cases). At the same time, it implies a drop in (symptomatic and asymptomatic) infections as discovered asymptomatic cases enter quarantine and the infection rate falls.

If we want to correct the artificially increased number of reported infections in an OuS region caused by the additionally identified asymptomatic cases, one should subtract the number of asymptomatic cases from the reported number of infections. This adjusted measure counts the number of symptomatic infectious individuals after OuS. This adjusted measure should be compared with the reported number of infections in the OuS region before OuS and in control regions where no testing takes place. As Tübingen was the first county in Germany to start with OuS, we assume (doing some robustness checks) that it is appropriate to subtract (PCR confirmed) asymptomatic cases from reported number of infections.

A.4 Methods

A.4.1 The Synthetic control method

The synthetic control method (SCM) is by now a well established strategy to measure the *treatment effect* of specific policy measures (see Section A.2 for references). Here we provide the details regarding SCM that are relevant for our analysis. First, we set up the *donor pool*: it includes 400 Germany counties ("Landkreise" und "kreisfreie Städte"). 34 of these are located in BW and hence in the same state as Tübingen county. We consider alternative donor pools in order to robustify our results.

Second, we construct a *synthetic control unit* as a weighted average across the counties in the donor pool. Note that the number of counties with non-negligible weight is not restricted by our procedure and may vary across specifications. The weights are selected on the basis of a minimum distance approach. Specifically, we target a set of *predictor variables* for Tübingen

county in the pre-treatment period (that is, before March 16) in order to determine county weights. The predictor set includes observations for the *outcome variable* (infection rate). We choose the weights on the counties in the donor pool such that the control unit resembles Tübingen in terms of these variables as closely as possible. In this way, we ensure that pre-treatment differences in trends of the outcome variable are equalized. Table 4 lists all predictor variables. They include all socio-economic characteristics that are a) available at the county level and b) may matter for infection dynamics. In addition, we include weekly averages for infection rates in the six weeks prior to treatment.

Formally, let $\mathbf{x_1}$ denote the $(k \times 1)$ vector of predictor varibles in Tübingen and let $\mathbf{X_0}$ denote a $(k \times n)$ matrix with observations in the counties included in the donor pool consisting of n counties. Let $\mathbf{w}$ denote a $(n \times 1)$ vector of country weights w_j, $j = 1, \ldots, n$. Then, the control unit is defined by a $\mathbf{w}^*$ which minimizes the mean squared error

$$(\mathbf{x_1} - \mathbf{X_0}\mathbf{w})'\mathbf{V}(\mathbf{x_1} - \mathbf{X_0}\mathbf{w}) \ , \tag{A.1}$$

subject to $w_j >= 0$ for $j = 1, \ldots, n$ and $\sum_{j=1}^{n} w_j = 1$. In this expression, $\mathbf{V}$ is a $(k \times k)$ symmetric and positive semidefinite matrix. Here, $\mathbf{V}$ is a weighting matrix assigning different relevance to the characteristics in $\mathbf{x_1}$ and $\mathbf{X_0}$. Although the matching approach is valid for any choice of $\mathbf{V}$, it affects the weighted mean squared error of the estimator (*24*). We choose a diagonal $\mathbf{V}$ matrix such that the mean squared prediction error of the outcome variable (and the covariates) is minimized for the pre-treatment period (*24*, *25*).

We conduct all SCM estimations in STATA using the SYNTH (*26*) and SYNTH_RUNNER (*27*) packages. Our implementation follows largely (*4*).

Confidence intervals (CIs) are calculated from one-sided pseudo p-values obtained on the basis of comprehensive placebo-in-space tests. The latter tests calculate pseudo-treatment effects for all counties in the donor pool assuming that they, rather than Tübingen would have been treated with OuS on March 16, 2021. We calculate one-sided pseudo p-values as the share of placebo-treatment effects that are larger than the observed treatment effects for treated counties and thus indicate the probability that the increase in the outcome variable was observed by chance given the distribution of pseudo-treatment effects in the donor pool.

To account for differences in pre-treatment match quality of the pseudo-treatment effects, only donors with a good fit in the pre-treatment period are considered for inference. Specifically, we do not include placebo effects in the pool for inference if the match quality of the control region, measured in terms of the pre-treatment root mean squared prediction error (RMSPE), is greater than 10 times the match quality of the treated unit (*28*). Based on the obtained pseudo p-values we calculate confidence intervals as described in (*29*).

A.4.2 Case rates, comparisons and growth rates

Some or our arguments require a little bit of algebra. Especially the comparison between the number of weakly positive rapid tests and the seven-day case rate in section 2.2 becomes clearer when the idea behind the difference shown in table 2 is clearly shown.

- The basics

We start by defining c_{it} as the number of new cases on day t in region i. Let N_i denote the population size of region i. This allows us to compute the sum of cases over the last seven days as $c_{it}^7 \equiv \Sigma_{t=t-7}^{t-1} c_{it}$ and the seven-day CoV-2 case notification rate per 100,000 (the case rate) as

$$c_{it}^{7r} \equiv c_{it}^7 / N_i * 100,000. \tag{A.2}$$

This expression is shown everywhere in this paper whenever we display 'case rate' on the axes of the figures or write about seven-day case rates.

- Comparing regions

It is often useful to see the absolute difference in cases between Tübingen and its synthetic control county. The daily difference is

$$\Delta_t = c_{\text{Tüb},t} - c_{\text{cont},t}, \tag{A.3}$$

where the number of cases in the synthetic control county is a weighted sum of members m of the synthetic control county,

$$c_{\text{cont},t} = \Sigma_{m=1}^{M} \pi_m c_{mt}. \tag{A.4}$$

The weight π_m of member m of the control county is given by the outcome of SCM.

The difference per week, i.e. over the previous seven days, is

$$\Delta_t^7 = c_{\text{Tüb},t}^7 - c_{\text{cont},t}^7. \tag{A.5}$$

Defining

$$\Delta_t^{7r} \equiv \Delta_t^7 / N_i * 100,000 \tag{A.6}$$

yields the expression shown in all figures with a panel B where the axis is labeled by 'differences in case rates'.

- Positive rapid testing and case rates

Imagine we have data on cases discovered via rapid testing. We denote these cases by c_{it}^{test}. Cases that would have been reported if rapid testing had not taken place can then be approximated by $\tilde{c}_{it} \equiv c_{it} - c_{it}^r$. How can we approximate the seven-day case rate, being based on positive PCR tests, that would have been observed in the absence of rapid testing? The seven-day case rate is defined above in (A.2). The number of positive rapid tests over a period of seven days (the weekly number of positive tests, simply speaking) is given by $c_{it}^{7,\text{ test}} \equiv \Sigma_{t=t-7}^{t-1} c_{it}^{\text{test}}$. Hence, assuming that each positive rapid test is confirmed by a positive PCR test, the corrected case rate is given by

$$\tilde{c}_{it}^{7r} \equiv \frac{c_{it}^7 - c_{it}^{7,\text{ test}}}{N_i} * 100k = \frac{c_{it}^7}{N_i} * 100k - \frac{c_{it}^{7,\text{ test}}}{N_i} * 100k. \tag{A.7}$$

As the equality sign shows, we can either correct cases and then compute the rate or compute the difference between a case rate and a "positive-test" rate. This rate is displayed in table 2 in the main text. When we do so, we make two assumptions about the share of positive rapid tests that is confirmed by PCR tests. (Data on the result of a PCR test following a positive rapid test is not available.)

A.5 Discussion of donor pool

A.5.1 Donor pool Germany

The least restrictive donor pool consists of all German counties, excluding neighboring counties of Tübingen to avoid spillover effects. As can be seen in figure 5, Tübingen is at the lower end of the German case rate distribution before March 16. However, there are many other districts with case rates slightly above or below the case rate of Tübingen that can potentially serve as synthetic control group. This is in contrast to figure 9 that depicts the case rate of Tübingen in the BW donor pool. Tübingen sometimes has the lowest case rate which makes finding a suitable control group difficult.

Figure 5: Seven-day case rates in German counties

Notes: Data for all counties in Germany. Tübingen county indicated by black bold line.

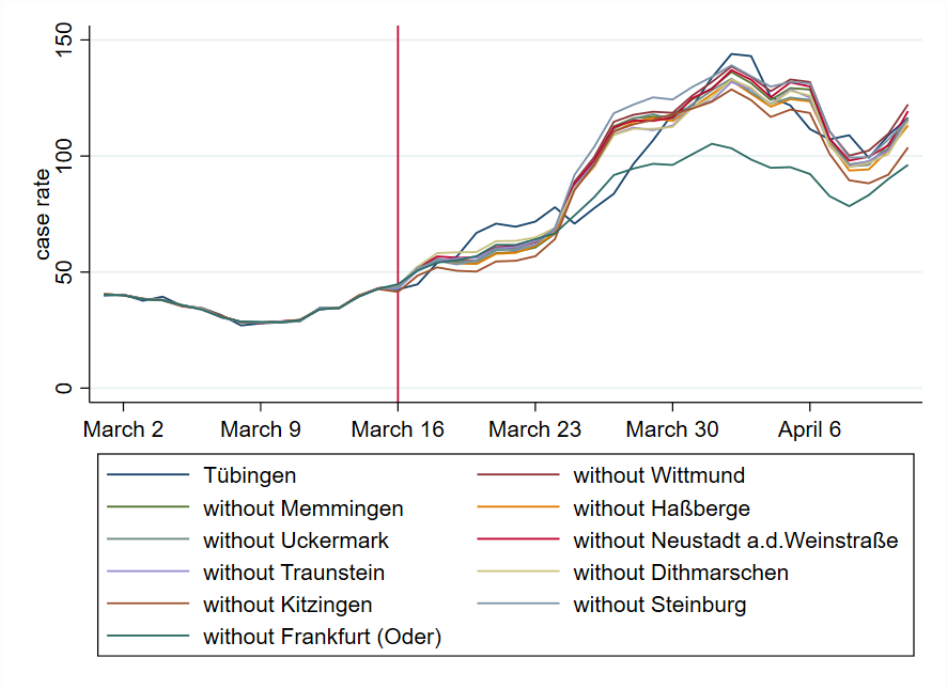
A.5.2 'Leave 1 out' and 'leave all out'

We now see why we do not tell the optimistic story from section A.7.1.

- Leave one out

Leave each county that was included in the control group of our baseline visible in figure 16 out at a time. The results are in figure 6.

Figure 6: Leave one out for figure 16



As figure 6 impressively shows, the results are robust apart from leaving out Frankfurt (Oder). Rechecking public health regulations (see (*30*) or the newspaper report in (*31*)), it turned out that shops in Frankfurt (Oder) were open and various sports and cultural activities were allowed up to April 1. We conclude that Frankfurt (Oder) was treated as well and needs to be excluded from the donor pool.

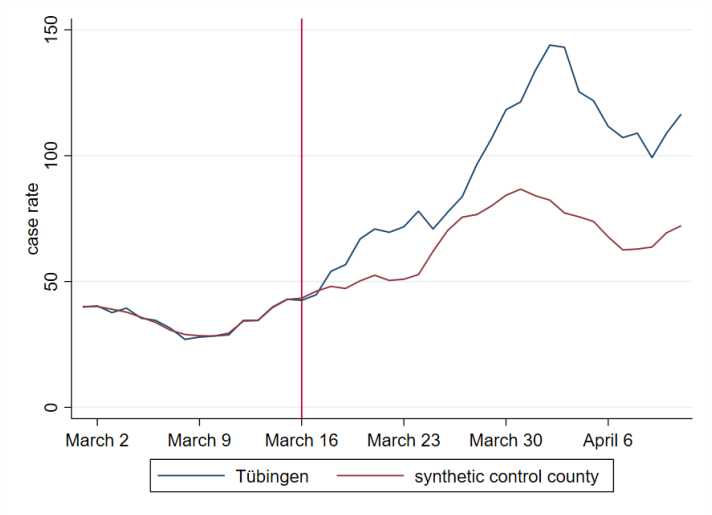
- Leave all out

We now run an SCM where we exclude all counties that are part of the originally selected synthetic control county of figure 16. The corresponding composition of the new synthetic control county is in table 7. This brings us to figure 7.

Table 7: Control counties and their weights for figure 7

Name	Weight
LK Friesland	0.29
LK Aurich	0.20
LK Nordfriesland	0.20
LK Eichstätt	0.13
SK Bamberg	0.050
LK Donnersbergkreis	0.048
LK Heidekreis	0.039
LK Neumarkt i.d.OPf.	0.018
SK Zweibrücken	0.017
SK Pforzheim	0.0080
LK Lüchow-Dannenberg	0.0050
LK Bad Dürkheim	0.0030

Figure 7: Leave all out for figure 16



A.5.3 'Leave all out' and predictor set 2

We now perform a standard robustness-check known as 'leave all out'. Given that SCM found a good fit, is this good fit the outcome of luck or a more stable result? We therefore perform SCM for the predictor set in section A.7.2 again, excluding all counties identified there (see table 21) as control counties. The results are in the following figure.

Figure 8: Seven-day case rates for predictor set 2 and 'leave all out'

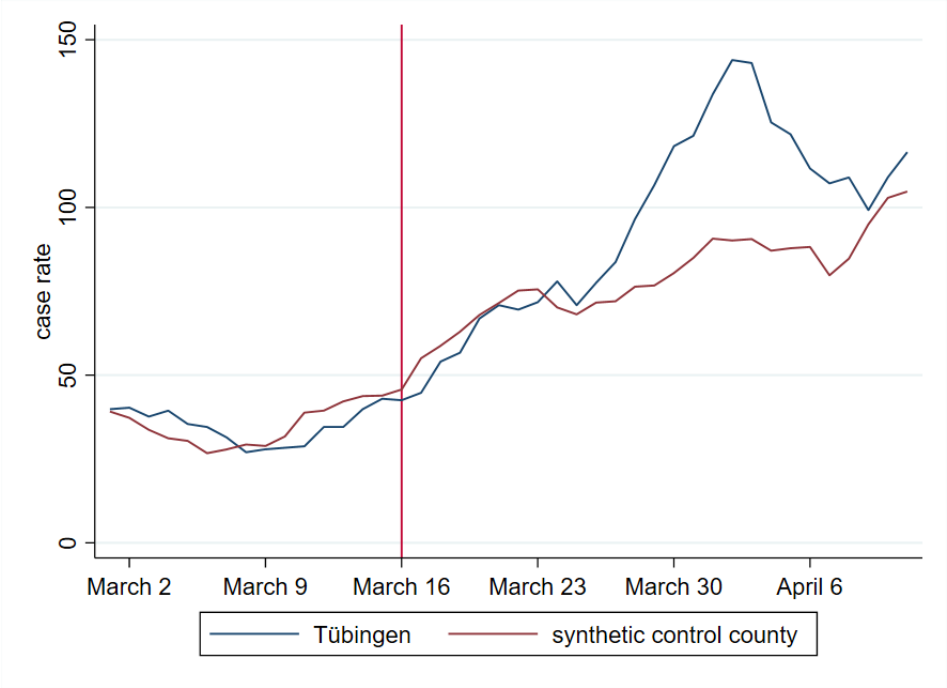


Table 8: Control counties and their weights for figure 8

Name	Weight
SK Trier	0.41
SK Ingolstadt	0.27
SK Darmstadt	0.15
LK Kitzingen	0.10
SK Münster	0.048
SK Hamburg	0.016

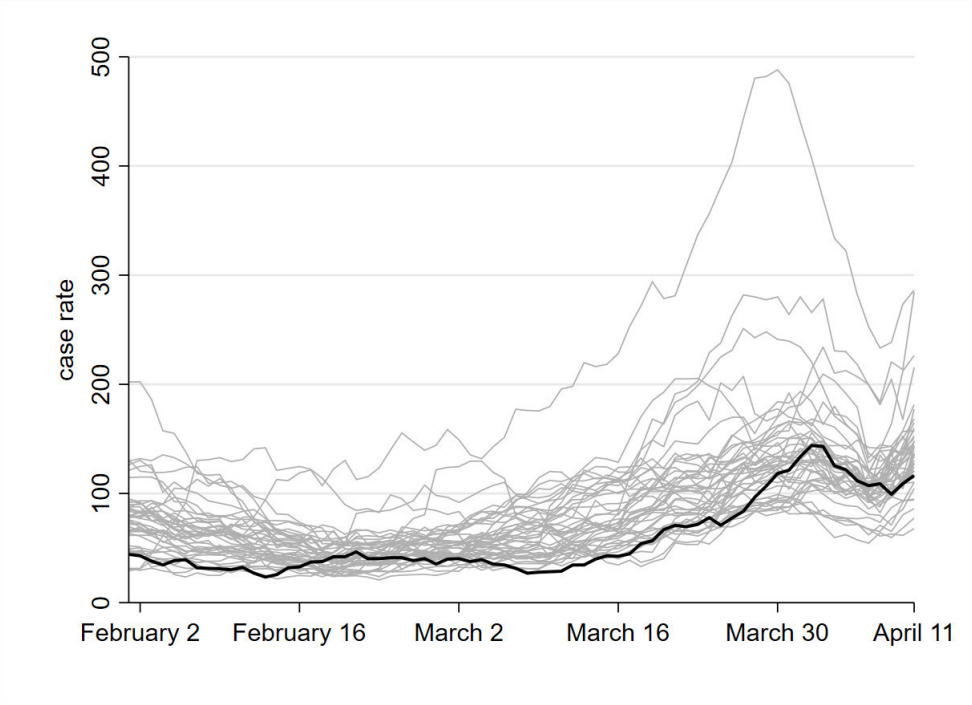
A.5.4 Neighboring counties of Tübingen

Neigboring counties are Böblingen, Esslingen, Reutlingen, Zollernalb, Freudenstadt and Calw. None of them is in any of our donor pools. Spillovers from Tübingen county might bias results if they appeared in the synthetic control county.

A.5.5 Donor pool Baden-Württemberg only

We now focus on a donor pool consisting of counties from BW only. We first show the case rates in all BW countries for January to April 2021 in figure 9. We see that Tübingen county had usually been within the lower range of case rates, especially shortly before the beginning of OuS on 16 March. Afterwards, it moved into the middle range among all counties in BW:

Figure 9: Seven-day case rates in BW counties



Notes: Data for all counties in BW. Tübingen county indicated by black bold line.

- Preditor set 1 for BW controls

When we run SCM with a donor pool consisting of all BW counties (apart from neighbours), we obtain just two control counties as seen in table 9.

Table 9: Control counties and their weights for figure 10

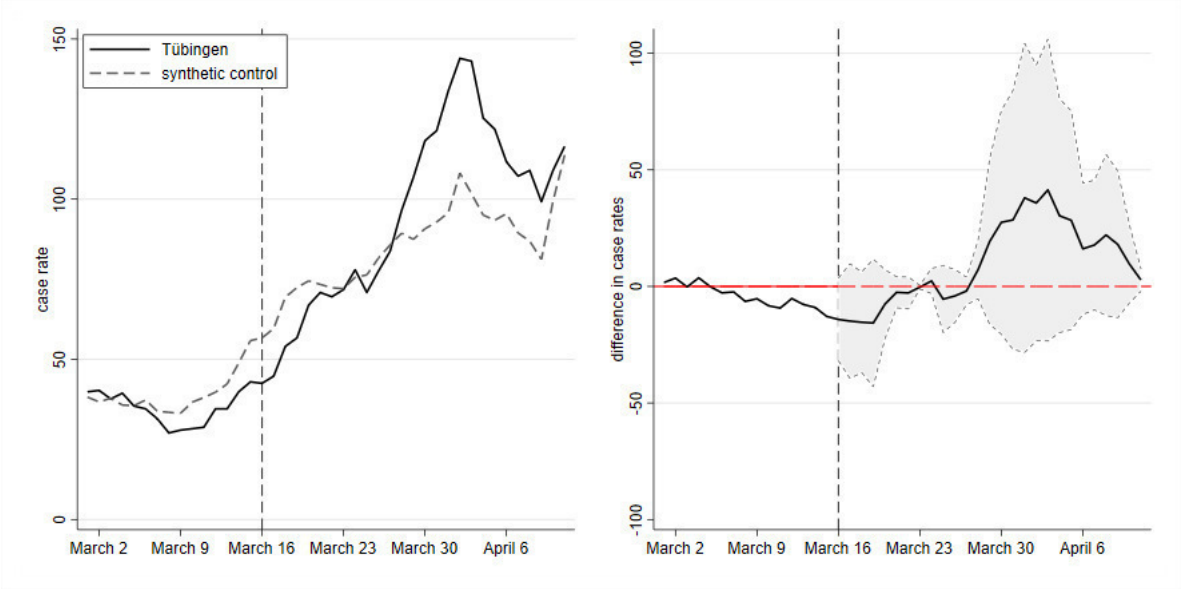
Name	Weight
LK Schwarzwald-Baar-Kreis	0.67
SK Pforzheim	0.34

The corresponding predictor balance is in table 10 which also shows our 'predictor set 1' in the first column.

Table 10: Pre-treatment predictor balance ('predictor set 1') and RMSPE for figure 10

	e(X_balance)	
	Treated	Synthetic
cum_cases14(68)	154	124.93
cum_cases14(74)	158	160.235
i7_rate(60)	39.86623	38.21198
i7_rate(61)	40.30919	36.73075
i7_rate(62)	37.65144	37.8542
i7_rate(63)	39.42327	35.73844
i7_rate(64)	35.43665	35.56197
i7_rate(65)	34.55073	37.31514
i7_rate(66)	31.45002	33.8088
i7_rate(67)	27.02044	33.44862
i7_rate(68)	27.90636	33.17908
i7_rate(69)	28.34932	36.64251
i7_rate(70)	28.79227	38.08323
i7_rate(71)	34.55073	39.74576
i7_rate(72)	34.55073	42.30753
i7_rate(73)	39.86623	48.87179
i7_rate(74)	42.96693	55.83915
mobility(68(1)74)	.0011557	.0504839
average_temperature(68(1)74)	4.214286	3.922643
Population density	434.8634	561.5437
Share of females in population	51.25601	50.49082
Average age of female population	41.67062	45.02182
Average age of male population	40.03484	42.06438
Old-age dependency ratio	24.57881	33.25497
Young-age dependency ratio	20.20369	21.49127
Medical doctors per population	15.63642	15.55456
Pharmacies per population	23.47678	27.82893
Categorial variable for population density of NUTS3 region	2	1.665
Share of highly educated persons in regional population	26.46966	11.15118
RMSPE (pre-treatment)	6.33	

Figure 10: Case rates for donor pool BW and predictor set 1

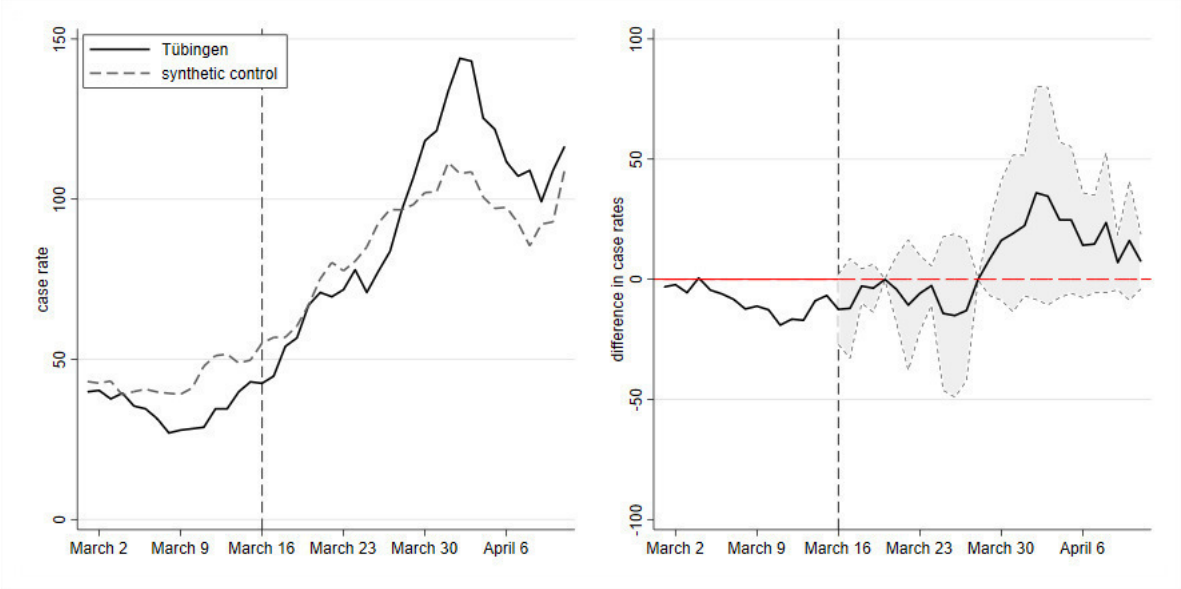


Notes: Left shows infection rate (new cases per 100K people during last seven days) in Tübingen (in blue) and synthetic control country (in red). Right panel shows difference of infection rate in Tübingen and control county. Dashed lines indicate 90 percent confidence interval calculated from one-sided pseudo p-values obtained on the basis of comprehensive placebo-in-space tests, see Section A.4.1 for details.

- Preditor set 2 for BW controls

When we compare our findings from predictor set 1 with predictor set 2, we obtain figure 11 with a predictor set 2 shown in table 12.

Figure 11: Case rates for donor pool BW and predictor set 2



Notes: Left shows infection rate (new cases per 100K people during last seven days) in Tübingen (in blue) and synthetic control country (in red). Right panel shows difference of infection rate in Tübingen and control county. Dashed lines indicate 90 percent confidence interval calculated from one-sided pseudo p-values obtained on the basis of comprehensive placebo-in-space tests, see Section A.4.1 for details.

Table 11: Counties and weights for figure 11

Name	Weight
SK Trier	0.41
SK Ingolstadt	0.27
SK Darmstadt	0.15
LK Kitzingen	0.10
SK Münster	0.048
SK Hamburg	0.016

Table 12: Pre-treatment predictor balance ('predictor set 2') and RMSPE for figure 11

	e(X_balance)	
	Treated	Synthetic
i7_rate(60)	39.86623	43.07041
i7_rate(68)	27.90636	39.10397
i7_rate(74)	42.96693	49.74245
cum_cases14(68)	154	139.398
cum_cases14(74)	158	155.045
mobility(68(1)74)	.0011557	-.1906342
average_temperature(68(1)74)	4.214286	7.151386
Population density	434.8634	1229.796
Share of females in population	51.25601	51.24652
Average age of female population	41.67062	41.81767
Average age of male population	40.03484	39.68116
Old-age dependency ratio	24.57881	25.1085
Young-age dependency ratio	20.20369	18.16761
Medical doctors per population	15.63642	22.69643
Pharmacies per population	23.47678	30.2666
Categorial variable for population density of NUTS3 region	2	1.182
Share of highly educated persons in regional population	26.46966	33.56344
RMSPE (pre-treatment)	10.60	

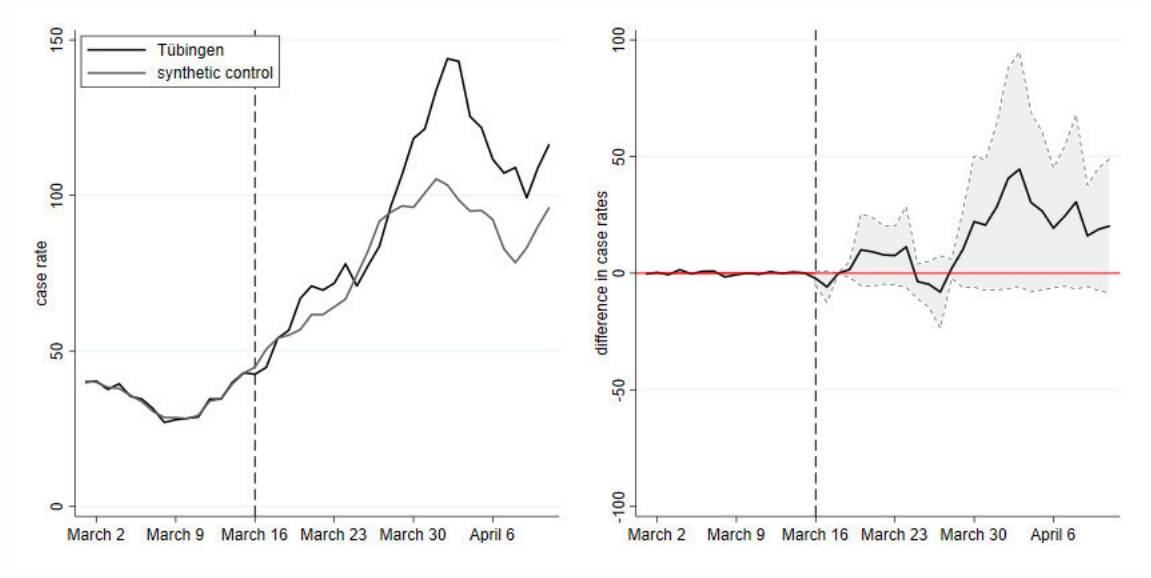
A.5.6 Donor pool without Frankfurt (Oder)

Having identified Frankfurt (Oder) as a treated region, we now run SCM with a donor pool consisting of 399 counties: all counties in Germany leaving out Tübingen and Frankfurt (Oder). The results are in figure 12.

A.5.7 Donor pool without Rheinland-Pfalz and Brandenburg

To be even more careful, we now exclude all counties in Brandburg (the state of Frankfurt (Oder)) and all counties from Rheinland-Pfalz. The latter state also announced (and partly implemented) opening measures for restaurants allowing them to serve outdoors.

Figure 12: Leave out Frankfurt (Oder) from donor pool for figure 16



Notes: Left shows infection rate (new cases per 100K people during last seven days) in Tübingen (in blue) and synthetic control country (in red). Right panel shows difference of infection rate in Tübingen and control county. Dashed lines indicate 90 percent confidence interval calculated from one-sided pseudo p-values obtained on the basis of comprehensive placebo-in-space tests, see Section A.4.1 for details.

A.6 Discussion of pandemic measure

A.6.1 Normalized: Cumulative infections per 100,000 inhabitants since January 1st

An alternative measuring of the pandemic state consists in looking at the total number of infections per 100,000 inhabitants since January 1st, 2021. We therefore do not look at a moving average (like the seven-day case rate) but simply add up the number of infections over time.

Searching for an appropriate comparison group for this dependent variable and comparing the evolution of infections over time provides a very surprising finding.

As the red and blue curve before treatment on March 16 in figure 14 show, the fit between Tübingen and its synthetic twin county is almost perfect here.[†]

What is much more important for our question, however, is the basically parallel evolution between Tübingen and its comparison county. In plain words, testing and opening does not have any effect whatsoever on infections.

Table 16 shows the fit between Tübingen and the synthetic twin city for cumulative infections as dependent variable.

[†]The better fit compared to case rates is not surprising as adding up infections since some starting date (1 January 2021 here) implies a smoother time series than adding infections over the previous 7 days. See table 14 for details on the fit.

Table 13: Control counties and their weights for figure 12

Name	Weight
LK Dithmarschen	0.16
LK Kitzingen	0.13
SK Neustadt a.d.Weinstraße	0.099
LK Steinburg	0.098
LK Friesland	0.091
LK Traunstein	0.083
LK Eichstätt	0.071
LK Bad Dürkheim	0.052
LK Wittmund	0.046
SK Bamberg	0.045
LK Donnersbergkreis	0.044
LK Uckermark	0.029
LK Neumarkt i.d.OPf.	0.028
SK Zweibrücken	0.027

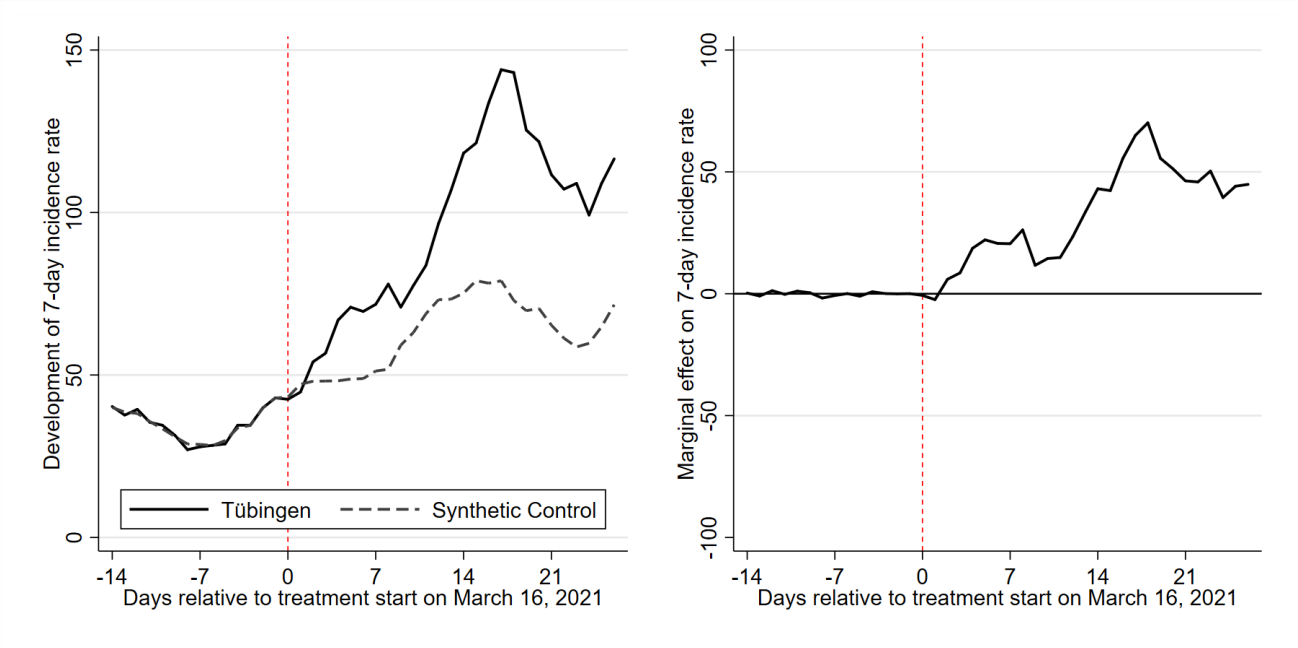
Table 14: Control counties and their weights for figure 13

Name	Weight
LK Friesland	0.29
LK Nordfriesland	0.17
LK Steinburg	0.17
LK Dithmarschen	0.11
LK Kitzingen	0.080
LK Wittmund	0.065
LK Neumarkt i.d.OPf.	0.059
LK Traunstein	0.039
SK Bamberg	0.023

Table 15: Control counties and their weights for figure 14

Name	Weight
SK Trier	0.33
LK Eichstätt	0.31
SK Heidelberg	0.16
SK Oldenburg	0.14
SK Münster	0.040
LK Pfaffenhofen a.d.Ilm	0.016

Figure 13: Donor pool without Rheinland-Pfalz and Brandenburg for figure 16



Notes: Left shows infection rate (new cases per 100K people during last seven days) in Tübingen (in blue) and synthetic control country (in red). Right panel shows difference of infection rate in Tübingen and control county. Dashed lines indicate 90 percent confidence interval calculated from one-sided pseudo p-values obtained on the basis of comprehensive placebo-in-space tests, see Section A.4.1 for details.

Figure 14: Cumulative cases per 100,000 since January 1st

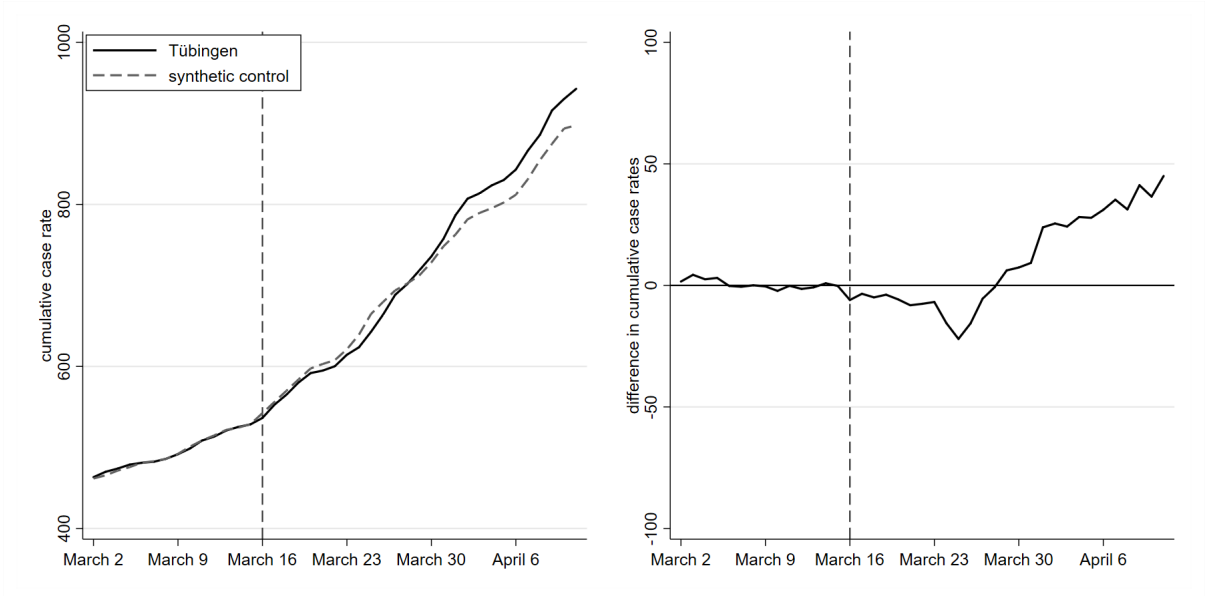


Table 16: Pre-treatment predictor balance and RMSPE for SCM in Figure 14

	e(X_balance)	
	Treated	Synthetic
cum_incidence_rate_jan_norm(60)	458.0186	457.2947
cum_incidence_rate_jan_norm(68)	491.6835	492.0916
cum_incidence_rate_jan_norm(74)	528.449	528.6675
i7_rate(74)	42.96693	41.55176
mobility(68(1)74)	.0011557	-.1601087
average_temperature(68(1)74)	4.214286	5.600533
Population density	434.8634	854.1957
Share of females in population	51.25601	50.6651
Average age of female population	41.67062	42.36237
Average age of male population	40.03484	40.06036
Old-age dependency ratio	24.57881	25.29022
Young-age dependency ratio	20.20369	19.01475
Medical doctors per population	15.63642	18.93145
Pharmacies per population	23.47678	29.82542
Categorial variable for population density of NUTS3 region	2	1.66
Share of highly educated persons in regional population	26.46966	22.10394
RMSPE (pre-treatment)	1.79	

A.6.2 Non-normalized: Cumulative cases over previous 7 days

This is a pandemic measure which follows from multiplying the standard seven-day case rate by the number of inhabitants per county and dividing by 100,000.

Figure 15: Cumulative cases over previous 7 days

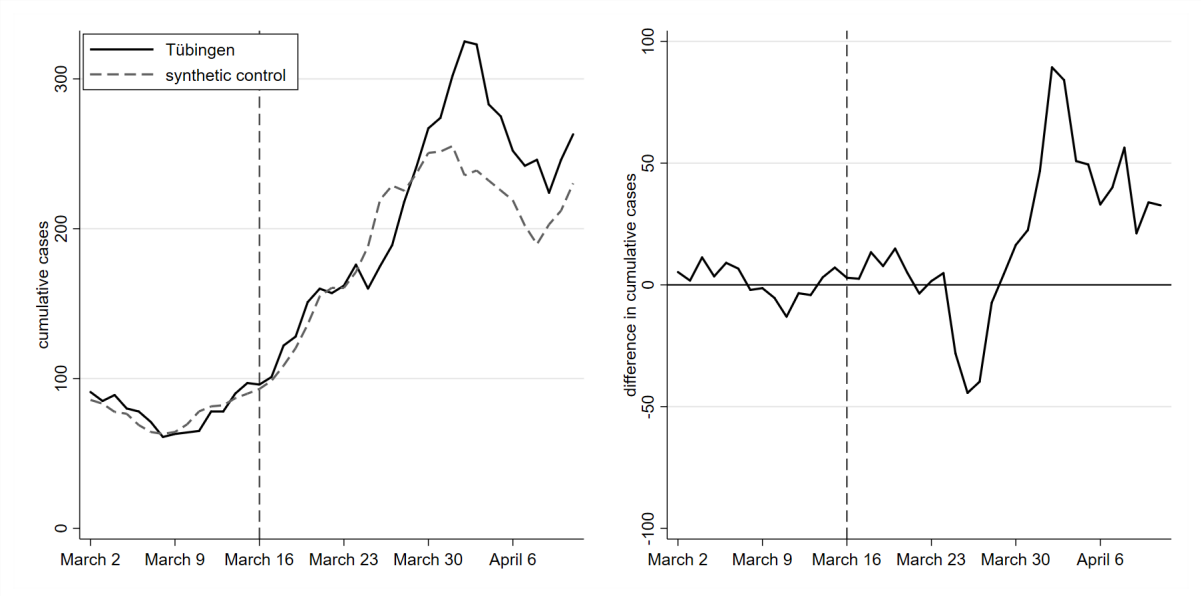


Table 17: Control counties and their weights for figure 15

Name	Weight
LK Eichstätt	0.53
SK Heidelberg	0.35
LK Heilbronn	0.087
LK Recklinghausen	0.017
SK Köln	0.013

Table 18: Pre-treatment predictor balance and RMSPE for SCM in Figure 15

	e(X_balance)	
	Treated	Synthetic
cum_cases7(60)	90	85.616
cum_cases7(67)	61	63.056
cum_cases(74)	6296	4971.405
i7_rate(74)	42.96693	38.81958
mobility(68(1)74)	.0011557	-.1342543
average_temperature(68(1)74)	4.214286	5.63929
Population density	434.8634	647.947
Share of females in population	51.25601	50.16586
Average age of female population	41.67062	41.97374
Average age of male population	40.03484	40.0914
Old-age dependency ratio	24.57881	25.0439
Young-age dependency ratio	20.20369	20.3595
Medical doctors per population	15.63642	16.1092
Pharmacies per population	23.47678	24.64824
Categorial variable for population density of NUTS3 region	2	2.169
Share of highly educated persons in regional population	26.46966	24.31394
RMSPE (pre-treatment)	6.27	

A.7 Discussion of predictor set

This is where the challenge begins. Counties can be compared by fundamental characteristics like population density, educational background and medical services or by more pandemic-related features such as case rates prior to treatment. Interestingly, results do not differ in any relevant way by this dimension.

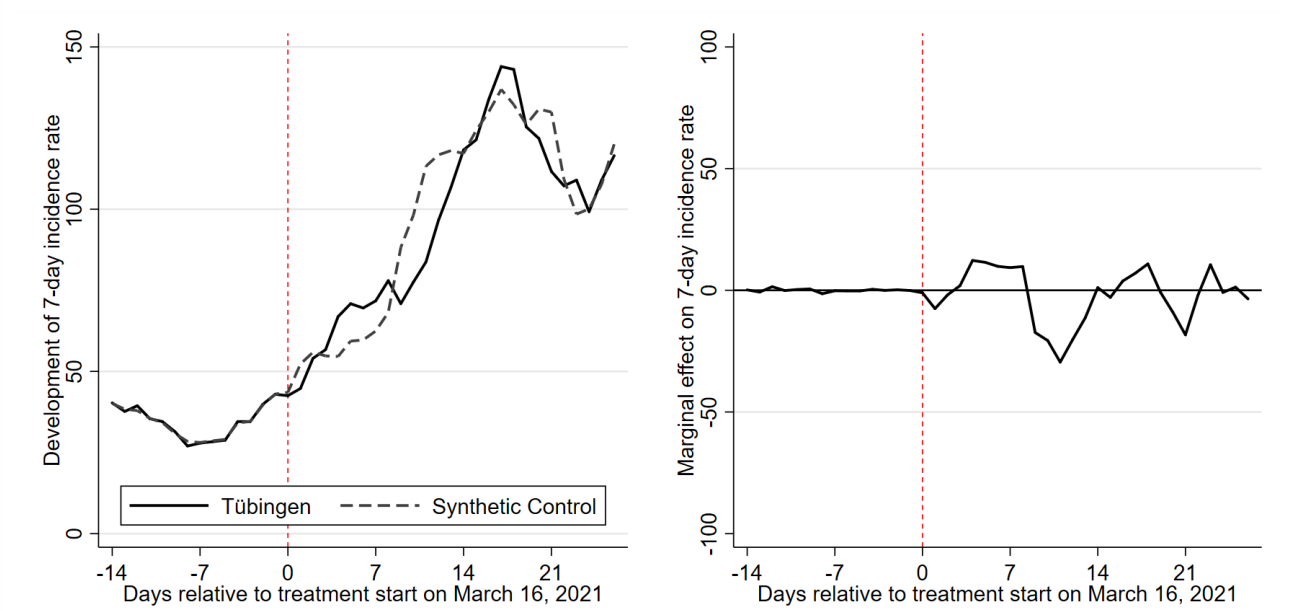
A.7.1 Full donor pool and predictor set 1

We could have told a very optimistic story about Tübingen. It results from a full donor pool and a strong emphasis on short-run dynamics. This finding is displayed in figure 16.

With this in mind, Tübingen performing worse in the left panel of figure 16 before March 24 can hardly be attributed to OuS. Around the Easter weekend (starting on April 3), however, Tübingen clearly exceeds its comparison group in terms of case rates. Public concerns seem to be justified. By contrast, in other periods and after Easter, case rates in Tübingen basically evolved in the same way as in the synthetic control counties. Hence, we are far away from a clear-cut result that OuS failed, quite to the contrary: OuS seems to work.

To be on the safe side, the right panel of figure 16 plots the difference between Tübingen and its synthetic twin. We also plot 90% confidence intervals. This suggests even more strongly that OuS seems to work. Tübingen performs at least as good as its twin regions.

Figure 16: Seven-day case rate with predictor set 1



Notes: Left shows infection rate (new cases per 100K people during last seven days) in Tübingen (in blue) and synthetic control country (in red). Right panel shows difference of infection rate in Tübingen and control county. Dashed lines indicate 90 percent confidence interval calculated from one-sided pseudo p-values obtained on the basis of comprehensive placebo-in-space tests, see Section A.4.1 for details.

Table 19: Control counties and weights for figure 16

Name	Weight
SK Frankfurt (Oder)	0.26
LK Steinburg	0.15
LK Kitzingen	0.12
LK Dithmarschen	0.11
LK Traunstein	0.11
SK Neustadt a.d.Weinstraße	0.087
LK Uckermark	0.073
LK Haßberge	0.045
SK Memmingen	0.027
LK Wittmund	0.020

A.7.2 Full donor pool and predictor set 2

We consider an alternative specification to the predictor set employed for our baseline specification in figure 16. Our predictor set 2 is shown in table 20, where we put less weight on seven day case rates in matching period.

Table 20: Pre-treatment predictor balance and RMSPE for SCM in Figure 17

	e(X_balance)	
	Treated	Synthetic
cum_cases7(68)	63	63.521
cum_cases14(74)	158	154.927
i7_rate(68)	27.90636	28.42382
i7_rate(74)	42.96693	42.15899
mobility(68(1)74)	.0011557	-.1468311
average_temperature(68(1)74)	4.214286	5.266146
Population density	434.8634	681.5408
Share of females in population	51.25601	50.10295
Average age of female population	41.67062	42.14188
Average age of male population	40.03484	40.12551
Old-age dependency ratio	24.57881	25.30402
Young-age dependency ratio	20.20369	20.45575
Medical doctors per population	15.63642	16.32629
Pharmacies per population	23.47678	25.87873
Categorial variable for population density of NUTS3 region	2	2.18
Share of highly educated persons in regional population	26.46966	23.62033
RMSPE (pre-treatment)	3.64	

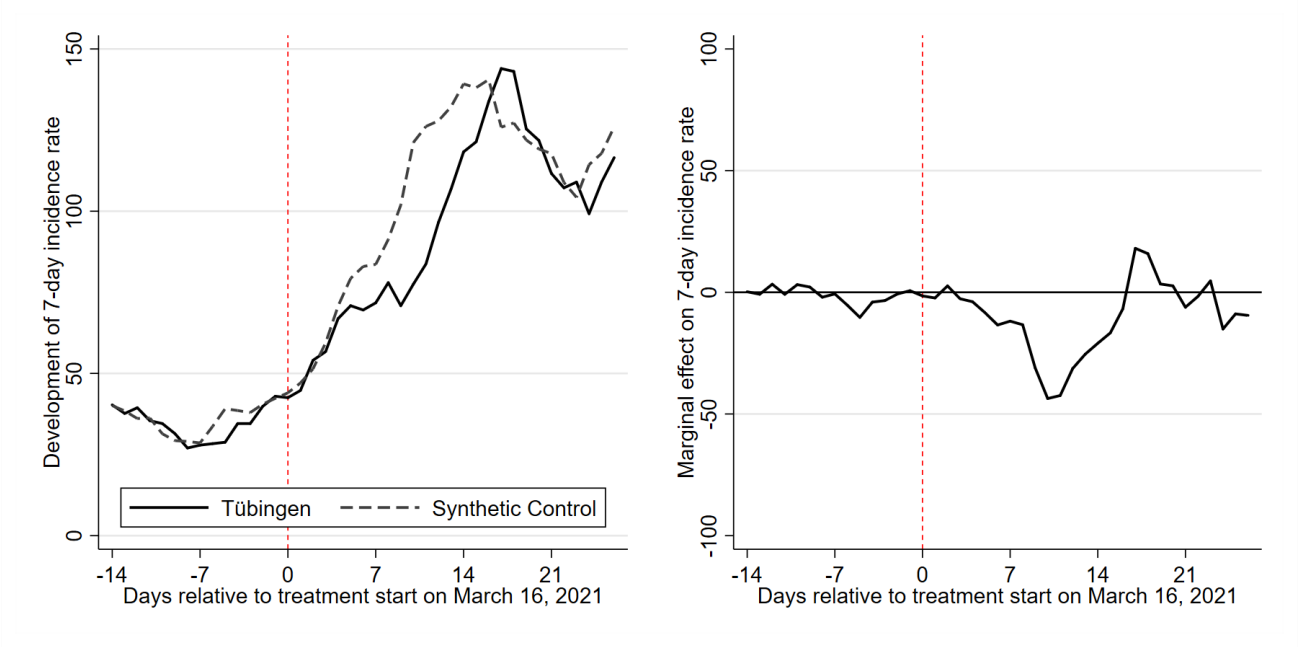
Like Figure 2 but different Weighting on i7_rate

Not surprisingly, we get a different set of comparison counties in table 21. The result is in figure 17.

Table 21: Control counties and their weights for figure 17

Name	Weight
LK Eichstätt	0.48
SK Heidelberg	0.25
SK Erlangen	0.16
LK Neuburg-Schrobenhausen	0.096
SK Berlin	0.012
LK Pfaffenhofen a.d.Ilm	0.0050

Figure 17: Seven-day case rates for predictor set 2

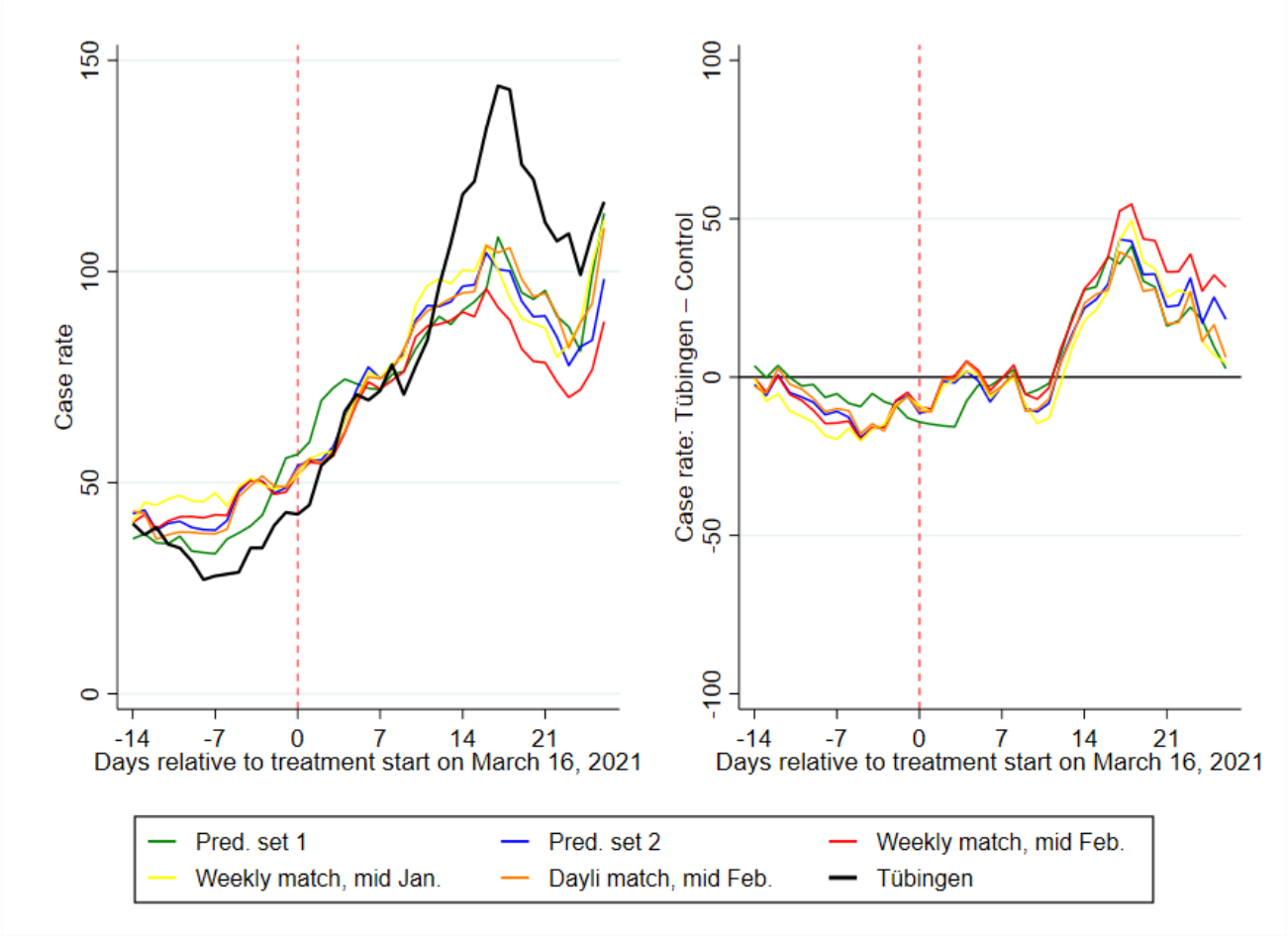


Notes: Left shows infection rate (new cases per 100K people during last seven days) in Tübingen (in blue) and synthetic control country (in red). Right panel shows difference of infection rate in Tübingen and control county. Dashed lines indicate 90 percent confidence interval calculated from one-sided pseudo p-values obtained on the basis of comprehensive placebo-in-space tests, see Section A.4.1 for details.

A.7.3 Various predictor sets for BW only

We varied the predictor set when selecting control counties from BW in various ways. We started from our predictor set 1 and 2 with outcomes visible already in section A.5.5. The case rates of the corresponding synthetic control counties is reproduced in figure 18. We then varied the length of the pre-treatment matching period and allowed for daily case rates to enter (as in predictor set 1) or for weekly case rates only. The results are also shown in figure 18 and further confirm the robustness of our benchmark result.

Figure 18: Seven-day case rates for all BW specifications



References for appendix

1. Robert Koch Institute, *Covid-19 Infektionen, General Website (NPGEO Corona Hub):* `https://npgeo-corona-npgeo-de.hub.arcgis.com/`, Accessed: February 8, 2021, 2021.
2. Deutscher Wetterdienst (German Weather Service), *Climate Data Center (CDC), General Website:* `https://www.dwd.de/EN/climate_environment/cdc/cdc.html`, Accessed: April 12, 2021, 2021.
3. F. Schlosser *et al.*, *Proceedings of the National Academy of Sciences* **117**, 32883–32890, ISSN: 0027-8424, eprint: `https://www.pnas.org/content/117/52/32883.full.pdf`, (`https://www.pnas.org/content/117/52/32883`) (2020).
4. T. Mitze, R. Kosfeld, J. Rode, K. Wälde, *Proceedings of the National Academy of Sciences* **117**, 32293–32301, ISSN: 0027-8424, (`https://www.pnas.org/content/117/51/32293`) (2020).
5. INKAR, *Indikatoren und Karten zur Raum- und Stadtentwicklung: Bundesinstitut für Bau-, Stadt- und Raumforschung (Federal Institute for Research on Building, Urban Affairs and Spatial Development), General Website:* `https://www.inkar.de/`, Accessed: June 4, 2020, 2021.
6. M. Eichenbaum, S. Rebelo, M. Trabandt, "The Macroeconomics of Testing and Quarantining", mimeo.

7. G. Guglielmi, "Rapid coronavirus tests: a guide for the perplexed", Nature, news feature.
8. A. Abadie, J. Gardeazabal, *American Economic Review* **93**, 113–132, (https://www.aeaweb.org/articles?id=10.1257/000282803321455188) (2003).
9. A. Abadie, A. Diamond, J. Hainmueller, *Journal of the American Statistical Association* **105**, 493–505, eprint: https://doi.org/10.1198/jasa.2009.ap08746, (https://doi.org/10.1198/jasa.2009.ap08746) (2010).
10. A. Abadie, *Journal of Economic Literature*, (https://www.aeaweb.org/content/file?id=12409) (2020).
11. B. Born, A. M. Dietrich, G. J. Müller, *PLOS ONE* **16**, 1–13, (https://doi.org/10.1371/journal.pone.0249732) (Apr. 2021).
12. S.-W. Cho, *The Econometrics Journal* **23**, 323–344 (Aug. 2020).
13. A. I. Friedson, D. McNichols, J. J. Sabia, D. Dave, *Journal of Policy Analysis and Management* **40**, 258–283, (https://onlinelibrary.wiley.com/doi/abs/10.1002/pam.22267) (2021).
14. The University of Liverpool *et al.*, "Liverpool Covid-19 Community Testing Pilot: Interim Evaluation Report", tech. rep. December (The University of Liverpool, Liverpool, 2020), (https://www.liverpool.ac.uk/media/livacuk/coronavirus/Liverpool,Community,Testing,Pilot,Interim,Evaluation.pdf).
15. R. E. Baker, W. Yang, G. A. Vecchi, C. J. E. Metcalf, B. T. Grenfell, *Nature Communications* **12**, 846, ISSN: 2041-1723, (https://doi.org/10.1038/s41467-021-20991-1) (2021).
16. S. M. Kissler *et al.*, *Nature Communications* **11**, 4674, ISSN: 2041-1723, (https://doi.org/10.1038/s41467-020-18271-5) (2021).
17. M. Karmakar, P. M. Lantz, R. Tipirneni, *JAMA Network Open* **4**, e2036462–e2036462, ISSN: 2574-3805, eprint: https://jamanetwork.com/journals/jamanetworkopen/articlepdf/2775732/karmakar_2021_oi_201090_1611286825.95028.pdf, (https://doi.org/10.1001/jamanetworkopen.2020.36462) (Jan. 2021).
18. T. Mitze, R. Kosfeld, J. Rode, K. Wälde, *Proceedings of the National Academy of Science (PNAS), December 3* (2020).
19. N. M. Linton *et al.*, eng, *Journal of clinical medicine* **9**, 538, ISSN: 2077-0383, (https://pubmed.ncbi.nlm.nih.gov/32079150%20https://www.ncbi.nlm.nih.gov/pmc/articles/PMC7074197/) (Feb. 2020).
20. eng, *Annals of internal medicine* **172**, 577–582, ISSN: 1539-3704, (https://pubmed.ncbi.nlm.nih.gov/32150748%20https://www.ncbi.nlm.nih.gov/pmc/articles/PMC7081172/) (May 2020).
21. J. R. Donsimoni, R. Glawion, B. Plachter, K. Wälde, *German Economic Review* **21**, 181–216 (2020).
22. J. Dehning *et al.*, *Science* **369** (2020).
23. K. Wälde, *IZA DP* **No. 13785,** http://ftp.iza.org/dp13785.pdf (2020).
24. A. Abadie, A. Diamond, J. Hainmueller, *Journal of the American Statistical Association* **105**, 493–505 (2010).
25. A. Abadie, J. Gardeazabal, *American Economic Review* **93**, 113–132 (Mar. 2003).

26. A. Abadie, A. Diamond, J. Hainmueller, *SYNTH: Stata module to implement Synthetic Control Methods for Comparative Case Studies*, Statistical Software Components, Boston College Department of Economics, Oct. 2011, (https://ideas.repec.org/c/boc/bocode/s457334.html).

27. S. Galiani, B. Quistorff, *The Stata Journal* **17**, 834–849, eprint: https://doi.org/10.1177/1536867X1801700404, (https://doi.org/10.1177/1536867X1801700404) (2017).

28. E. Cavallo, S. Galiani, I. Noy, J. Pantano, *The Review of Economics and Statistics* **95**, 1549–1561, (https://doi.org/10.1162/REST_a_00413) (2013).

29. D. G. Altman, J. M. Bland, *BMJ* **343**, eprint: https://www.bmj.com/content/343/bmj.d2090.full.pdf, (https://www.bmj.com/content/343/bmj.d2090) (2011).

30. *Allgemeinverfügung der Stadt Frankfurt (Oder) Nr. 08/2021 - erweiterte Schutzmaßnahmen Inzidenz über 100*, (https://www.frankfurt-oder.de/PDF/Allgemeinverf%C3%BCgung_der_Stadt_Frankfurt_Oder_Nr_08_2021_erweiterte_Schutzma%C3%9Fnahmen_Inzidenz_%C3%BCber_100.PDF?ObjSvrID=2616&ObjID=9503&ObjLa=1&Ext=PDF&WTR=1&_ts=1617345595).

31. *Brandenburg zwingt Frankfurt (Oder) zur Notbremse*, (2021; https://www.rbb24.de/studiofrankfurt/panorama/coronavirus/beitraege_neu/2021/03/brandenburg-frankfurt-eindaemmungsverodnung-kassiert.html).