# The lockdown effect: A counterfactual for Sweden*

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**Abstract**

While most countries imposed a lockdown in response to COVID-19, Sweden did not. To quantify the lockdown effect, we approximate a counterfactual lockdown scenario for Sweden through the outcome in a synthetic control unit. We find, first, that a 9-week lockdown in the first half of 2020 would have reduced infections and deaths by about 75% and 50%, respectively. Second, the lockdown effect starts to materialize with a delay of 3-4 weeks only. Third, the actual adjustment of mobility patterns in Sweden suggests there has been substantial voluntary social restraint, although the adjustment was less strong than under the lockdown scenario. Lastly, we find that a lockdown would not have caused much additional output loss.

*Keywords:* COVID-19, lockdown, counterfactual, synthetic control unit, voluntary social restraint, Google mobility reports, output loss

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# 1 Introduction

As the COVID-19 pandemic spreads across the globe, policy makers are faced with a trade-off. By imposing a lockdown, they may limit the spread of COVID-19 infections and deaths. But a lockdown might also entail severe economic and social costs (Bonaccorsi et al., 2020; Frey and Osterloh, 2020). In order to inform policy makers, it is essential to quantify both the costs and the benefits of lockdowns. In this study, we focus on the health benefits and quantify the extent to which a lockdown limits the spread of COVID-19. We only make a first pass at quantifying the economic costs and ignore the social costs altogether.

A major challenge in quantifying the lockdown effect is that infection dynamics are bound to change over time even in the absence of a lockdown—at least for two reasons. First, the basic Susceptible-Infectious-Recovered (SIR) model assumes a constant infection probability, but predicts that infection growth slows over time as the pool of infected and susceptible people shrinks (Kermack and McKendrick, 1927). Second, people may adjust their behavior in the face of infection risk and restrain their social interactions voluntarily. This, too, impacts infection growth. Several studies neglect these aspects and attribute any change in the growth rate of infections to lockdown measures or, more broadly speaking, to non-pharmaceutical interventions (NPI). NPI are thus found to be very effective in reducing infection growth (Zhanga et al., 2020) and to reduce the number of deaths by about 95% according to an influential study (Flaxman et al., 2020).

In the present study, we explicitly account for changing infection dynamics as we quantify the lockdown effect on the basis of a purely empirical approach. Specifically, we focus on Sweden: it stands out from its European peers in that its government opted against a lockdown in the first half of 2020, although its exposure to COVID-19 was not systematically different from the rest of Europe. Still, the Swedish authorities merely advised—rather than ordered—citizens to adjust their behavior in the face of the pandemic. For instance, people were told "to avoid unnecessary traveling and social events, to keep distance to others, and to stay at home" if they had any symptoms. In addition, those above age 70 were "advised to avoid social contact" and "visits to retirement homes" were banned (Krisinformation, 2020).

In order to quantify the lockdown effect, we benchmark actual developments in Sweden against a counterfactual lockdown scenario approximated through the outcomes in a synthetic control unit (Abadie, Diamond, et al., 2010). We construct it on the basis of a donor pool of European countries that actually imposed a lockdown and by making sure that it resembles Sweden in terms of infection dynamics *before* the lockdown. In the counterfactual scenario, Sweden would have imposed a first lockdown, just like in the control unit, running for 9 weeks from March 16 to May 19, 2020.

We find that it takes 3 to 4 weeks for the lockdown to make a difference. Only after this period do we observe fewer COVID-19 infections and deaths in the control unit. The cumulative difference grows over time, but only up to the end of August. For this reason, we limit our assessment of the lockdown effect to the period up to September 1, 2020. We find that a lockdown would have reduced infections and deaths by about 75% and 50%, respectively. We obtain similar numbers for various robustness tests, just like a recent study that follows our empirical strategy closely (Cho, 2020). Our results are also in the same order of magnitude as those reported in a case study of Norway and Denmark, which uses Sweden as a control group (Juranek and Zoutman, 2020).

We also analyze Google mobility reports and find a profound change of actual mobility patterns in Sweden, even as no lockdown is imposed: people travel less and spend more time at home or in parks. The extent of this adjustment is sizable, but not as strong as in the control unit under the lockdown. This finding suggests that voluntary social restraint limits infection growth to a considerable extent—in line with recent work that augments the basic SIR model to account for behavioral adjustments in the face of infection risk (Eichenbaum et al., 2020; Farboodi et al., 2020; Krueger et al., 2020)—and may rationalize why the lockdown effect becomes manifest with a considerable delay only. Yet our results also support the notion that there is a non-trivial infection externality—people fail to internalize the costs they impose on others as they become infectious and, hence, voluntary social restraint may fail to deliver the same extent of social distancing as a lockdown. Lastly, we find that, against the backdrop of the large actual output loss in the first half of 2020, a lockdown would have caused little additional output loss in Sweden.

The remainder of the paper is organized as follows. The next section details our approach, while Section 3 presents the results. A final sections concludes with a number of caveats.

# 2 The approach

Our empirical strategy is centered around the notion that one may approximate a counterfactual lockdown scenario for Sweden by the outcomes in a synthetic control unit. We construct the *control unit* on the basis of a *donor pool* of countries in such a way that it behaves just like Sweden *before* the lockdown. In contrast to Sweden, all countries in the donor pool imposed a lookdown in response to the first wave of COVID-19 infections during the first half of 2020. Any difference in infection dynamics in Sweden and the control unit *since* the lockdown may then be attributed to the lockdown in the control unit. In what follows we first provide details on the donor pool before turning to the construction of the control unit.

## 2.1 The donor pool

We construct the control unit below as a weighted average of the countries in the donor pool. To ensure a high degree of homogeneity between Sweden and the control unit, we restrict the donor pool to Norway and western EU countries with more than 1 million inhabitants. In total, it includes 13 countries. Table 1 provides details on each country in the donor pool, including the timing of the lockdown and the most important measures. The first lockdown was imposed in Italy on March 9, the last in the Netherlands on March 24. These lockdowns typically involved the closing of non-essential shops as well as a ban on gatherings of more than two people. In some instances, the ban applied only to gatherings of 10 people or more. In Sweden only gatherings of more than 50 people were banned.

Table 1: Donor pool countries and Sweden: lockdown measures and dates

| Country | Lockdown Start | Lockdown End | Containment Measures | Day 1 | Days to Lockdown | Days in Lockdown | Lockdown Stringency | % of control unit |
|---|---|---|---|---|---|---|---|---|
| Austria | 16.03. | 01.05. | non-essential shops closed, ban on gatherings of more than 5 people | 29.02. | 16 | 46 | 83 | 00.0 |
| Belgium | 18.03. | 11.05. | non-essential shops closed, ban on gatherings of more than 2 people, stay-at-home order | 03.03. | 15 | 54 | 81 | 00.0 |
| Denmark | 18.03. | 11.05. | non-essential shops closed, ban on gatherings of more than 50 people | 03.03. | 15 | 54 | 70 | 29.5 |
| Finland | 16.03. | 01.06. | government agencies closed, ban on gatherings of more than 10 people, stay-at-home advice | 01.03. | 15 | 77 | 56 | 24.7 |
| France | 17.03. | 11.05. | non-essential shops closed, ban on gatherings of more than 2 people, stay-at-home order | 29.02. | 17 | 55 | 90 | 00.0 |
| Germany | 23.03. | 06.05. | non-essential shops closed, ban on gatherings of more than 2 people | 01.03. | 22 | 44 | 73 | 00.0 |
| Greece | 23.03. | 11.05. | non-essential shops closed, ban on gatherings of more than 10 people, stay-at-home-order | 05.03. | 18 | 46 | 82 | 00.0 |
| Ireland | 28.03. | 08.06. | non-essential shops closed, stay-at-home-order | 04.03. | 24 | 72 | 87 | 00.0 |
| Italy | 09.03. | 18.05. | non-essential shops closed, stay-at-home-order | 22.02. | 16 | 70 | 85 | 00.0 |
| Netherlands | 24.03. | 11.05. | non-essential shops closed, ban on gatherings | 02.03. | 22 | 48 | 79 | 24.3 |
| Norway | 13.03. | 01.06. | restaurants, bars closed, ban on gatherings of more than 5 people (24.03) | 28.02. | 14 | 80 | 66 | 17.2 |
| Portugal | 19.03. | 01.06. | no shops closed, government agencies closed, stay-at-home advice | 06.03. | 13 | 74 | 79 | 00.0 |
| Spain | 14.03. | 08.06. | non-essential shops closed, stay-at-home-order | 01.03. | 13 | 86 | 78 | 04.3 |
| Sweden | - | - | No Lockdown imposed, ban on gatherings of more than 50 people | 28.02. | - | - | 42 | - |

Notes: donor pool includes western EU countries with population size of at least one million and Norway. Day 1 is the day when the number of infections surpasses a threshold of one infection per one million inhabitants. Sources for lockdown dates and details are provided in Table A.1 in the supplementary appendix. Lockdown ends when shops were reopened, except for Finland and Norway (restaurant reopening). Lockdown stringency is average value during lockdown period based on data from the Coronavirus Government Response Tracker (Hale et al., 2020). For Sweden, average is for counterfactual lockdown period from March 16 to May 19. "% of control unit" pertains to baseline (specification A), rounded to first digit.

As a comprehensive measure, we also report the average stringency of the lockdown based on data from the Coronavirus Government Response Tracker (Hale et al., 2020). As can be seen in the second-to-last column of Table 1, there is some variation in lockdown stringency across countries. According to this measure, there has been some response of the Swedish authorities to the Coronavirus, but—consistent with the premise of our analysis—the index number is considerably lower than for the other countries in the donor pool.

In order to assess the impact of the lockdown, it is essential to ensure that infection dynamics, displayed in Figure A.1, are comparable across countries prior to the lockdown. And because the virus arrived at different dates in each country, we may not compare the infection dynamics on a calendar-day basis. Therefore, we initialize observations for each country using a common reference point: day 1 is when the number of infections surpasses a threshold of one infection per one million inhabitants. Day 1 varies from country to country, see Table 1. For instance, in Sweden it is February 28, in Norway it happens to be the same day, while in Denmark it is March 3. Given day 1, we find that it takes countries at least 13 days since day 1 to impose a lockdown. Its duration also varies across countries from a minimum of 44 days in Germany to a maximum of 86 days in Spain, see again Table 1.

## 2.2 Constructing the control unit

For the construction of the control unit, we require it to track the infection dynamics in Sweden during the first 13 days as closely as possible, that is, before any country in the donor pool imposed a lockdown. Given the speed at which COVID-19 spreads, this is a rather long time frame and the infection dynamics during this period are likely to be informative about a country's overall exposure to the virus. Moreover, since the number of infections are initially very low, we target the log of infections rather than the level. In this way, we make sure that the early observations within the 13-day window play a non-negligible role for the construction of the control unit.

A widely discussed shortcoming of the available data is that the number of reported infections is not independent of the number of tests, since infections may go undetected if symptoms are mild or even absent. Hence, we verify—once we have constructed the control unit—that the frequency of testing in Sweden does not change systematically relative to the control unit in our sample period. Importantly, while we observe some variation in the relative testing frequency, there is no systematic variation that would account for our results: see Figure A.2 in the appendix.

In addition, we require the control unit to be comparable to Sweden in terms of population size and in terms of the urban population share because these factors may play an important role for infection dynamics, too. In total, we target 15 observations in order to construct the control unit: log infections at daily frequency within the 13 day window prior to the first lockdown, population size, and the urbanization rate.

Formally, we construct the control unit in such a way that it tracks the infection dynamics in Sweden during the first 13 days as closely as possible, that is, before any country in the donor pool imposed a lockdown. Formally, we construct the control unit by selecting weights on the countries in the donor pool for which we obtain the best match between the control unit and Sweden for the 15 target observations. That is, we let $\mathbf{x_1}$ denote the $(15 \times 1)$ vector of observations in Sweden and let $\mathbf{X_0}$ denote a $(15 \times 13)$ matrix with observations in the countries included in the donor pool. Finally, we let $\mathbf{w}$ denote a $(13 \times 1)$ vector of country weights $w_j$, $j = 1, \ldots, 13$. Then, the control unit is defined by $\mathbf{w}^*$ which minimizes the following mean squared error:

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

subject to $w_j >= 0$ for $j = 1, \ldots, 13$ and $\sum_{j=1}^{13} w_j = 1$. In this expression, $\mathbf{V}$ is a $(15 \times 15)$ 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 (Abadie, Diamond, et al., 2010). 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 (Abadie, Diamond, et al., 2010; Abadie and Gardeazabal, 2003).

# 3 Results

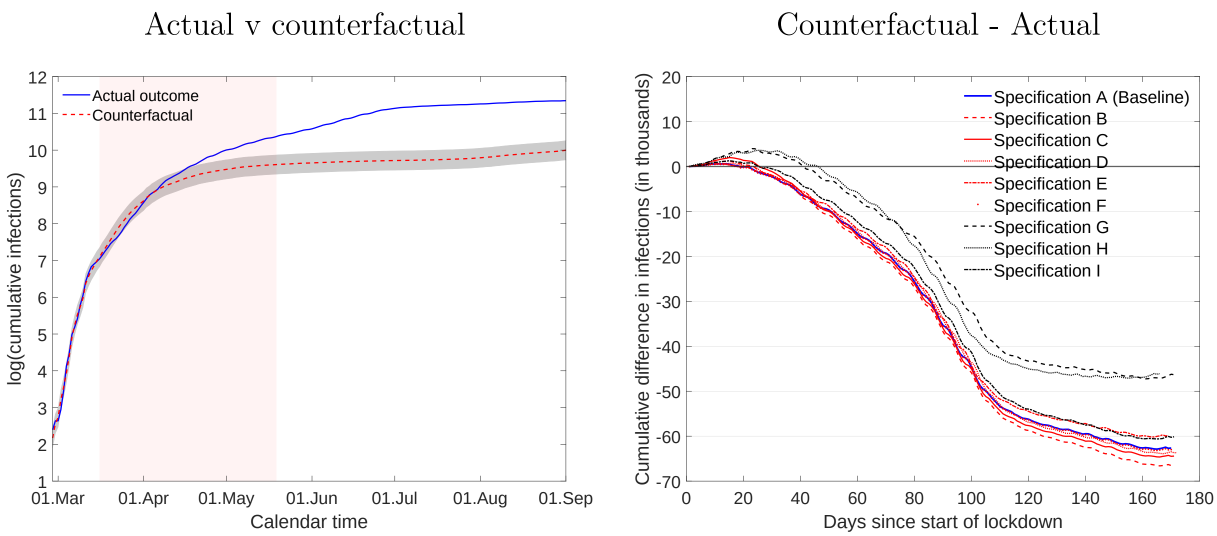
In what follows, we present three sets of results. First, we quantify the lockdown effect as the difference in infection outcomes in Sweden and the control unit which approximates the lockdown counterfactual for Sweden. Second, we present results regarding the extent of voluntary social restraint in Sweden and the control unit. Lastly, we make a first pass at assessing the potential economic costs of a lockdown in Sweden.

Our results are based on a control unit which is composed of five countries: Denmark (29.5%), Finland (24.7%), the Netherlands (24.3%), Norway (17.2%), and Spain (4.3%). The other countries in the donor pool receive negligible weights only, as reported in the right column of Table 1. Note that the number of countries with non-negligible weight is not restricted by our procedure and may vary across specifications. Using these weights, we compute the average outcome across the countries in the control unit in order to approximate the counterfactual infection outcome for Sweden. On average, it takes 17 days after day 1 before the a 64-day lockdown is imposed. Its stringency is 68 and hence well above the actual value for Sweden. Since day 1 in Sweden is February 28, 2020, we assume that a first counterfactual lockdown would have run from March 16 to May 19, 2020, that is, for 9 weeks.

## 3.1 The lockdown effect

The left panel of Figure 1 shows infection dynamics in Sweden (blue solid line) and the control unit that approximates the counterfactual outcome (red dashed line). The vertical axis measures cumulative infections in logs, the horizontal axis represents calendar days. The lockdown period is indicated by the pink shaded area. By construction, the control unit tracks infection dynamics closely before the lockdown. We also obtain a good fit for population size (10.175 millions in Sweden and 10.185 in the control unit) and for the urbanization rate (0.874 and 0.869, respectively). The gray shaded area represents two standard deviations of the difference between log infections in Sweden and the control unit during the matching period,

Figure 1: COVID-19 infections in Sweden




Notes: Left panel compares actual outcome (blue solid line) and control unit/counterfactual (red dashed line) in logs for baseline specification. Right panel shows difference of infections between counterfactual and actual outcome in thousands. Table 1 reports country weights for control unit shown in left panel and used in specification A (baseline) in right panel. Gray shaded area in left panel shows two standard deviations of difference between infections in Sweden and control unit during the first 13 days (matching period). Pink shaded area: lockdown period (March 16–May 19). Specifications B-F in right panel: alternative control units based on restricted donor pools. Specification G: day 1 determined on the number of deaths; Specification H: control unit determined by matching number of deaths instead of log infections; Specification I: control unit determined by matching the infections rate in percent.

an informal measure to put deviations since the start of the lockdown into perspective. We observe that infection dynamics start to diverge visibly midway through the lockdown period: at this point infection growth slows markedly in the control unit, that is, in the counterfactual lockdown scenario relative to the actual developments in Sweden.

The right panel of Figure 1 displays the difference in infections between the control group (counterfactual) and Sweden, measured as the cumulative difference in terms of the number of new infections since the beginning of the lockdown in the counterfactual. Initially, the difference is small, but over time, infection growth in Sweden is higher, such that the difference turns negative. The blue solid line represents our baseline (specification A) for which we report country weights in Table 1. These weights are plausible in the sense that Sweden's neighbors receive a lot of weight.

Table 2: The lockdown effect on COVID-19 infections and deaths

| | | Infections | | Deaths | |
|---|---|---|---|---|---|
| | | Actual | Counterfactual | Actual | Counterfactual |
| A) | Baseline (lockdown ends May 19) | 83,375 | 20,738<br>-75% | 5,805 | 3,038<br>-48% |
| B) | W/o Denmark in donor pool (lockdown ends May 29) | 83,458 | 16,940<br>-80% | 5,806 | 3,271<br>-44% |
| C) | W/o Finland in donor pool (lockdown ends May 18) | 83,458 | 19,028<br>-77% | 5,806 | 1,501<br>74% |
| D) | W/o Netherlands in donor pool (lockdown ends May 22) | 83,527 | 19,879<br>-76% | 5,808 | 4,080<br>-30% |
| E) | W/o Norway in donor pool (lockdown ends May 15) | 83,256 | 23,383<br>-72% | 5,799 | 2,683<br>-54% |
| F) | W/o Spain in donor pool (lockdown ends May 17) | 83,375 | 20,384<br>-76% | 5.805 | 2,693<br>-54% |
| G) | Day 1 determined using number of deaths (lockdown ends May 19) | 83,527 | 37,343<br>-55% | 5,808 | 1,902<br>-67% |
| H) | Matching number of deaths (lockdown ends May 14) | 82,788 | 36,699<br>-56% | 5,775 | 4,067<br>-30% |
| I) | Matching infection rates (lockdown ends May 26) | 83,458 | 23,322<br>-72% | 5,806 | 1,333<br>-77% |

Notes: New infections and deaths since start of lockdown up until September 1, 2020. Figures for the actual outcome differ across specifications because the start of the lockdown period varies with the composition of the control unit. Panel A: baseline (see Figure 1). Panels B-F: alternative control units, for which each of the countries with non-negligible weight in baseline control unit, in turn, is excluded from donor pool. Panel G: day 1 determined on the number of deaths, Panel H: control unit determined by matching number of deaths instead of log infections. Panel I: control unit determined by matching the infection rates in percent. See text for details.

Nevertheless we consider a number of alternative specifications. For specifications B to F we construct alternative control units based on a restricted donor pool: in each case, we exclude, in turn, each country that receives a non-negligible weight in the baseline specification. Specification G uses an alternative criterion to specify day 1, namely the day 30 days before the number of deaths exceeds one per 100,000 inhabitants. In this way we address concerns regarding the accuracy of infection data. Similarly, in specification H, we match the total number of deaths instead of log infections. In this case we define day 1 as the day 10 days before a country suffers at least one death per 100,000 inhabitants. For specification I, we match infection rates (rather than log infections), calculated as the fraction of the absolute number of infections divided by a country's population size.

In each instance, we obtain a new control unit that differs from the baseline in terms of composition and also somewhat in the timing and the stringency of the lockdown, see Table A.2 and Figures A.3 to A.11 in the appendix. Yet, as the right panel of Figure 1 shows, a robust picture emerges across specifications. In particular, two results stand out. First, the lockdown effect emerges with considerable delay of 20-30 days. This is noteworthy because the incubation period for COVID-19 is on average only 5-6 days, and at most up to 14 days (WHO, 2020). Second, the effect of the lockdown levels off some 170 days after it is imposed (for as long as 64 days in the baseline specification).

For this reason we assess the overall effect of the lockdown up until September 1, that is, over a 170 day period since March 16. Specifically, Table 2 reports the total number of new infections and deaths between the start of the lockdown (which differs slightly across specifications because of the changing composition of the control unit), contrasting once more the developments in Sweden and the control unit (counterfactual). Panel A shows numbers for the baseline specification: 83,375 new infections in Sweden vs 20,738 in the counterfactual lockdown scenario. This is our main result: a lockdown would have reduced the number of infections in Sweden by 75%. At the same time, it would have reduced the number of deaths by 48%, from 5,805 to 3,038. Table 2 also reports results for alternative specifications: the impact of the lockdown ranges from -55% to -80% for infections and from -30% to -77% for deaths.

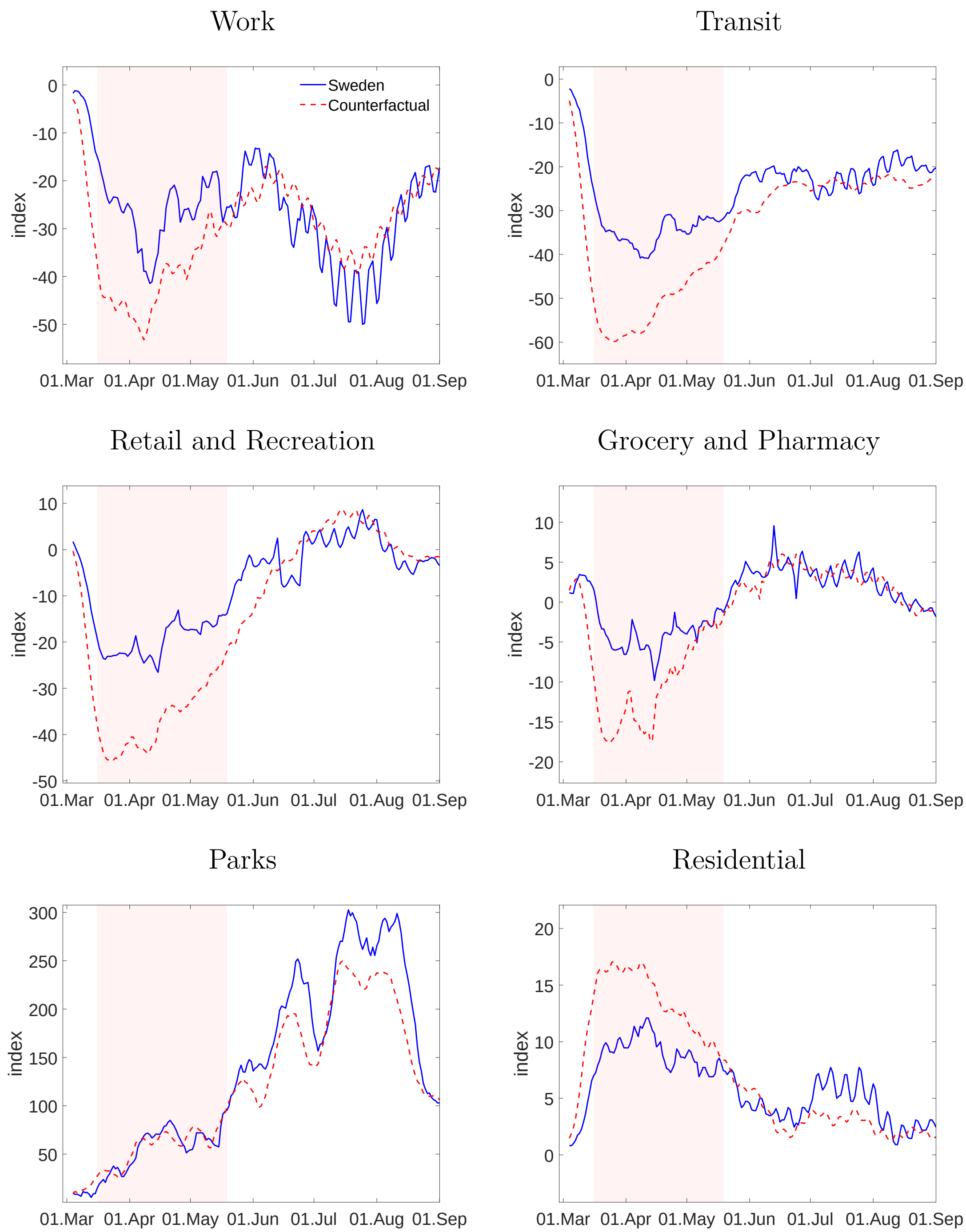
### 3.2 Social distancing

Our results show that a lockdown would have reduced the number of COVID-19 infections and deaths in Sweden, but this lockdown effect starts to materialize with a delay of 3 to 4 weeks only. In order to rationalize this result, we compare the extent of social distancing in Sweden and in the control unit on the basis of Google COVID-19 Community Mobility Reports (Google, 2020). The reports classify locations on the basis of six distinct categories, namely, Work, Transit, Retail and Recreation, Grocery and Pharmacy, Parks, and Residential and measure the change in the number and the length of stays at these locations relative to the median value of the same weekday between January 3 and February 6, 2020.

Figure 2 displays mobility adjustments for each location, contrasting once more actual data for Sweden (blue solid line) and for the control unit (red dashed line) that is meant to approximate the counterfactual outcome in case of a lockdown. We measure observations using 11-day symmetric averages along the horizontal axis, and the percentage change relative to the pre-COVID-19 period along the vertical axis. As before, the pink shaded area indicates the lockdown period. Several findings stand out. First, we observe a pronounced decline of mobility in the top four panels. They provide a measure of activities associated with travel and work as well as shopping and dining in restaurants ("recreation"). At the same time, people spend more time in parks and at home (bottom panels). More importantly still, the adjustment starts to take place before the lockdown period and can be observed both in Sweden and in the control unit. Consistent with these findings, it has been documented elsewhere that COVID-19 has induced job seekers to reduce their search intensity and employers to reduce vacancy postings in the absence or prior to a lockdown (Hensvik et al., 2020; Kahn et al., 2020). Last, we observe that while the adjustment of activities follows roughly the same pattern, it is more pronounced for the control unit during the lockdown period.

These findings are consistent with the hypothesis that it takes time for the lockdown to make a difference because people adjust their behavior even in the absence of a lockdown: they reduce their social interactions in ways which limit the spread of the virus—there is,

Figure 2: Mobility adjustment in Sweden




Notes: actual outcome (blue solid line) vs control unit/counterfactual (red dashed line). Vertical axis measures percentage change relative to median in early 2020. Horizontal axis measures 11-day symmetric moving average. Pink shaded area indicates lockdown period. Construction of counterfactual: see Figure 1 (Specification A, Baseline). Data source: Google mobility reports (Google, 2020).

in other words, voluntary social restraint. This point emerges also clearly from a number of recent model-based contributions that have extended the basic SIR model to allow for peoples' (rational) adjustment of work, consumption and leisure activities in the face of infection risk. Model simulations illustrate that voluntary social restraint can have large effects, both in terms of limiting the maximum share of infections in the population and in terms of inducing an economic contraction (Eichenbaum et al., 2020). This behavioral response can also rationalize the observation that individuals across the United States reduced their social interactions well before lockdown restrictions were imposed (Farboodi et al., 2020). Importantly, the extent to which people adjust their behavior depends critically on the extent to which there are possibilities to substitute low-risk activities, such as working from home or take-away dining, for high-risk ones. There can be little doubt that actual economies offer a fairly rich array of substitutes for most goods and activities (Krueger et al., 2020).

That said, economic theory also identifies an infection externality: while people adjust their behavior to limit their exposure to the virus, they fail to internalize the costs they impose on others as they become infectious. This provides a rationale for government-imposed NPI and an explanation why the outcomes under "laissez faire" and the optimal policy can be quite different. In Sweden it takes a considerable amount of time for the difference to become visible—likely because lockdown policies in practice are fairly blunt and less well targeted than the optimal policy in model-based simulations. Still, over time, there is a sizeable lockdown effect—in line with the notion of a non-trivial infection externality (Chudik et al., 2020).

## 3.3 The economic costs of a lockdown

Based on our approach, we also compute the economic costs of the lockdown in terms of forgone output growth. For this purpose we compare GDP growth in Sweden in the first two quarters of 2020 to GDP growth in the control unit. To ensure comparability, we normalize GDP growth by subtracting the average growth rate in 2019 in both instances. For the first and second quarter of 2020, we find that on year-on-year basis GDP growth in Sweden has been 0.61 and 9.03 percentage points lower compared to 2019. In the control unit, the numbers are 2.24 and 9.71, respectively. Hence, here too we find a lockdown effect: a lockdown in Sweden would have reduced annualized output growth by 1.62 and 0.68 percentage points, in the first and second quarter of 2020, respectively.

This effect appears moderate, but in line with the finding that the Swedish labor market performed only slightly better than that of its neighbors (Juranek, Paetzold, et al., 2020). Two considerations may rationalize this result. First, as documented above, there has been considerable voluntary social distancing in Sweden. This has already taken a toll on output growth, even as no lockdown was imposed and a lockdown may not have raised the costs much further. In fact, there is recent evidence, comparing mobility adjustments and unemployment across U.S. states, that mobility is a good proxy for how social distancing—voluntary or mandatory—affects economic activity (Yang et al., 2020).

Second, the economic costs of a lockdown in any given country depend on what happens in the rest of the world. If, as in the case of Sweden, all major trading partners are put under lockdown, economic growth declines even as no lockdown is imposed. Against this background it would be interesting to investigate an alternative counterfactual in which no country imposes a lockdown. Such an assessment, however, is beyond the scope of the present study.

# 4 Conclusion

Our study focuses on the Swedish case to quantify the effect of a lockdown on COVID-19 infections and deaths. We may summarize our results in four findings: First, we find the effect to be sizable and robust across specifications: it ranges from -55% to -80% for infections and from -30% to -77% for deaths. Second, the lockdown effect starts to materialize with a delay of 3-4 weeks only. Third, the actual adjustment of mobility patterns in Sweden suggests there has been substantial voluntary social restraint, although the adjustment was less strong than under the lockdown scenario. Lastly, we find that a lockdown would not have caused much additional output loss.

At this point, we should point out a number of limitations of our analysis. First, we use data on COVID-19 infections and deaths even though there are serious issues related to measurement, not least the fact that the number of reported infections depends on the number of tests. Still, our analysis is based on the same data that informs public discussions and actual policy design.

Second, we assume a macro perspective throughout and study the effect of "a" lockdown (with stringency 68). Clearly, specific lockdown measures may differ strongly in terms of effectiveness. For instance, it has been shown on the basis of an approach similar to ours that making face masks mandatory is fairly effective in limiting the spread of infections (Mitze et al., 2020).

Third, we focus on the immediate effects of the lockdown and do not assess to what extent a lockdown alters the dynamics of a "second wave", say, because in the absence of a lockdown a larger fraction of the population will have gained immunity (Giesecke, 2020).

Fourth, there is the issue of external validity: we have developed a counterfactual for Sweden and cannot be sure that results carry over to other contexts and countries. In particular, we cannot rule out that the behavioral adjustment in Sweden was influenced by the fact that other countries in Europe imposed a lockdown. We note, however, that our results are similar to those obtained for California, the first state in the US to issue a shelter-in-place order, on the basis of an approach similar to ours (Friedson et al., 2020).

Last, we stress that our study focuses on benefits of a lockdown in terms of limiting COVID-19 infections and deaths. We find that these benefits are not trivial. While we also quantify the costs of a lockdown in terms of output and find the effect to be rather moderate, our analysis is altogether silent on the social, political, and psychological costs of a lockdown. Hence, the final verdict on lockdowns as a policy tool is still out. We would hope that our results inform a broad-based debate on the best policy response to the COVID-19 pandemic.

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# Appendix

**Data sources** Our data source for the number of COVID-19 infections and deaths up to September 1, 2020 is the Johns Hopkins University (Dong et al., 2020). Observations are available at daily frequency. They are assembled using national (e.g. ministry of health, government) as well as international official sources (e.g. WHO, European Centre for Disease Prevention and Control). We display the time series data for each country in the donor pool in Figure A.1. Finally, our data for population size and urbanization rate is provided by the World Bank (Worldbank, 2019 Revision[a],[b]).

We use Google COVID-19 Community Mobility Reports to measure mobility changes due to the pandemic (Google, 2020). They are available for each country in our donor pool and provide a measure for how long and how frequently certain types of locations are visited. Google collects location data in various ways using mobile phone positions (via mobile networks or GPS data), a user's IP address, search queries, or navigation requests. Google uses this information only if users actively agree to share their "Location History". Last, in order to compare GDP growth in Sweden and the control unit we use OECD data (OECD, 2020).

To control for the frequency of testing in Sweden and the donor pool countries, we rely on data from the European Center for Disease Prevention and Control (European Centre for Disease Prevention and Control, 2020).

**Time series Data for Countries in Donor Pool: Infections and Deaths** Figure A.1 presents the raw data series for deaths and infections of the countries in the donor pool, as made available by the Johns Hopkins University (Dong et al., 2020). Data displayed runs from February 22 to September 1.

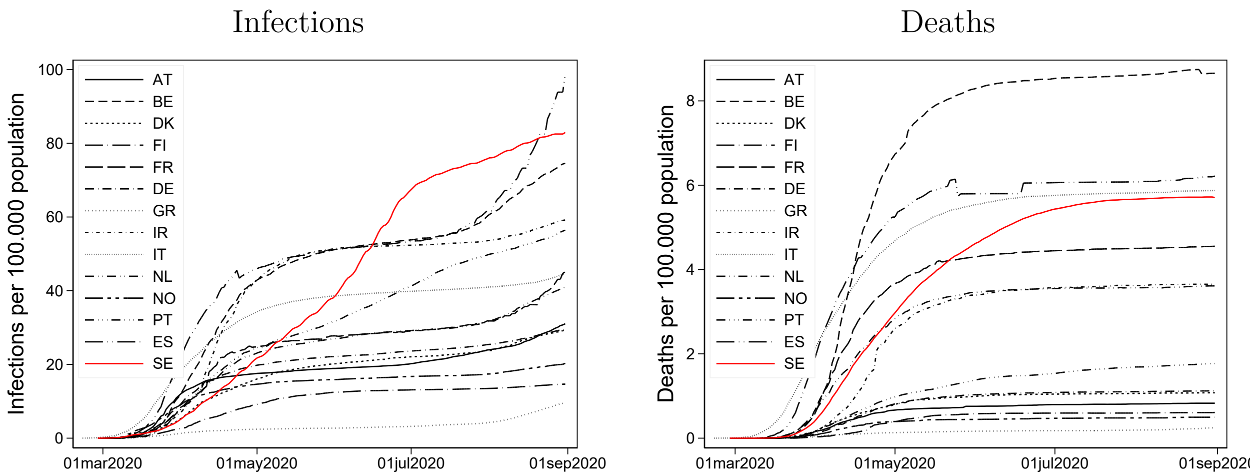


Figure A.1: Left panel shows cumulative infections per 100.000 population in countries of donor pool. Right panel shows cumulative COVID-19 deaths per 100.000 population in countries of donor pool. Red line represents data for Sweden. Data source: (Dong et al., 2020).

**Ratio of Tests** In Figure A.2, we display the ratio of weekly tests between Sweden and the control group. We use weekly data provided by European Centre for Disease Prevention and Control (2020). Data for each country is then shifted in time so that the countries' day 1 is in the same week as for Sweden.

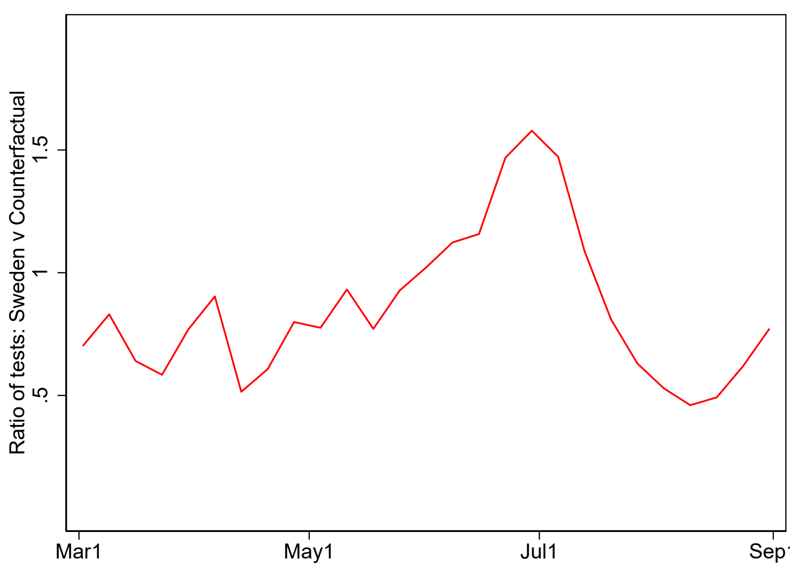


Figure A.2: Weekly ratio of tests: Sweden vs the control unit (counterfactual) for baseline. Data source: European Centre for Disease Prevention and Control (2020).

**Lockdown Sources**

Table A.1: Lockdown measures in Europe: sources

| Country | Lockdown Start Source | Lockdown End Source |
|---|---|---|
| Austria | *www.parlament.gv.at* | *www.wko.at* |
| Belgium | *www.belgium.be* | *www.belgium.be/en* |
| Denmark | *politi.dk* | *politi.dk* |
| Finland | *valtioneuvosto.fi* | *valtioneuvosto.fi* |
| France | *www.diplomatie.gouv.fr* | *www.tagesschau.de* |
| Germany | *www.bundesregierung.de* | *www.bundesregierung.de* |
| Greece | *gr.usembassy.gov/covid-19* | *www.visitgreece.gr* |
| Ireland | *www.gov.ie* | *www.gov.ie* |
| Italy | *www.trovanorme.salute.gov.it* | *www.salute.gov.it* |
| Netherlands | *www.government.nl* | *www.government.nl* |
| Norway | *www.helsedirektoratet.no* | *www.regjeringen.no* |
| Portugal | *www.acm.gov.pt* | *www.acm.gov.pt* |
| Spain | *www.gov.uk/foreign-travel-advice/spain/coronavirus* | *www.bbc.com* |

## Weights of Counterfactual Scenario

Table A.2: Country weights

| Specification | A (Baseline) | B | C | D | E | F | G | H | I |
|---|---|---|---|---|---|---|---|---|---|
| Austria | 00.0 | 00.0 | 20.1 | 00.0 | 00.2 | 00.0 | 00.0 | 00.0 | 00.9 |
| Belgium | 00.0 | 00.0 | 00.0 | 09.2 | 00.0 | 00.0 | 04.9 | 16.5 | 01.3 |
| Denmark | 29.5 | NA | 16.5 | 31.0 | 31.8 | 29.9 | 34.1 | 35.7 | 07.7 |
| Finland | 24.7 | 14.8 | NA | 22.9 | 29.0 | 24.1 | 00.0 | 0.00 | 49.0 |
| France | 00.0 | 00.0 | 00.0 | 00.0 | 00.0 | 02.2 | 00.0 | 00.0 | 10.7 |
| Germany | 00.0 | 00.0 | 03.1 | 00.0 | 00.0 | 00.0 | 02.2 | 00.0 | 01.7 |
| Greece | 00.0 | 00.0 | 00.0 | 01.0 | 00.0 | 00.0 | 00.0 | 00.0 | 00.6 |
| Ireland | 00.0 | 0.01 | 00.0 | 00.0 | 00.0 | 00.0 | 00.0 | 16.4 | 00.8 |
| Italy | 00.0 | 00.0 | 00.0 | 00.0 | 00.0 | 00.0 | 00.0 | 00.0 | 01.0 |
| Netherlands | 24.3 | 14.1 | 13.6 | NA | 39.0 | 27.9 | 00.0 | 31.4 | 00.5 |
| Norway | 17.2 | 63.4 | 46.7 | 26.1 | NA | 16.0 | 05.3 | 00.0 | 24.0 |
| Portugal | 00.0 | 00.0 | 00.0 | 00.0 | 00.0 | 00.0 | 53.5 | 00.0 | 00.8 |
| Spain | 04.3 | 07.6 | 00.0 | 09.8 | 00.0 | NA | 00.0 | 00.0 | 01.0 |

Table gives the weights in percent for each country in the optimized control group for each specification (A to I), as outlined in Table 1 in the main part. If table entry is NA country is excluded from the donor pool for the respective specification.

## Specification A

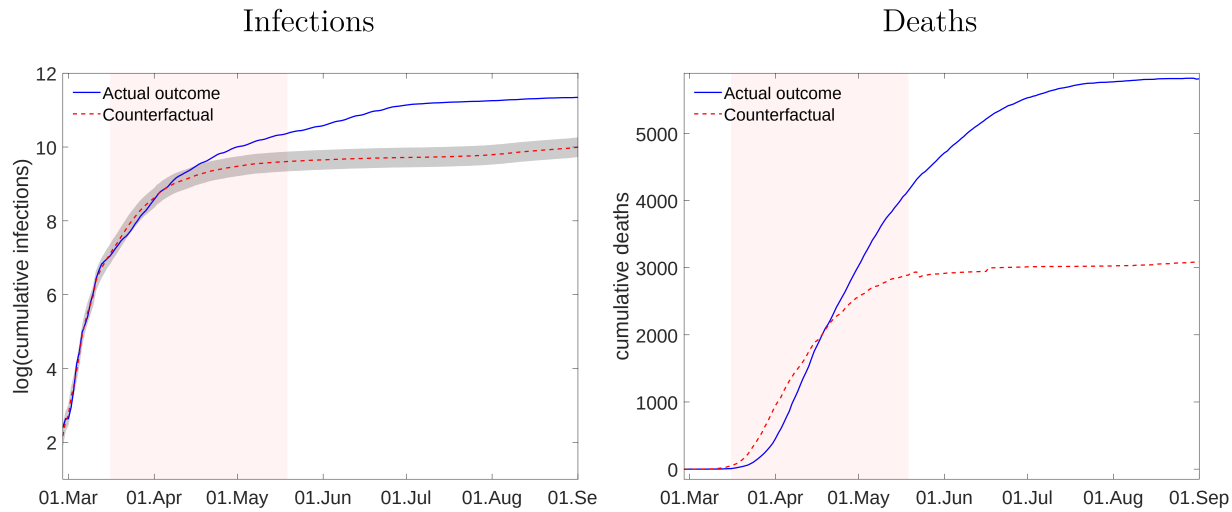


Figure A.3: Specification A (Baseline). Left panel shows infections, actual outcome (blue solid line) v counterfactual (red dashed line) in logs. Right panel shows deaths, actual outcome (blue solid line) v counterfactual (red dashed line). Counterfactual approximated by outcome of control unit, see Table A.2 for country weights of specification A (baseline). Gray shaded area: two standard deviations of difference between infections in Sweden and control unit during the first 13 days. Pink shaded area: lockdown period.

## Specification B

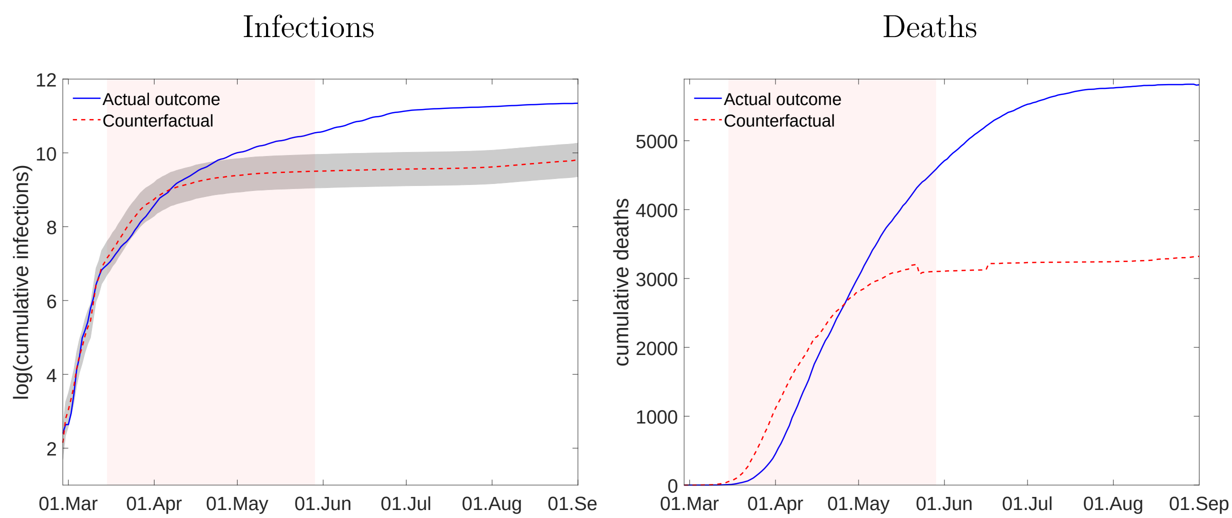


Figure A.4: Specification B (w/o Denmark). Left panel shows infections, actual outcome (blue solid line) v counterfactual (red dashed line) in logs. Right panel shows deaths, actual outcome (blue solid line) v counterfactual (red dashed line). Counterfactual approximated by outcome of control unit, see Table A.2 for country weights of specification B. Gray shaded area: two standard deviations of difference between infections in Sweden and control unit during the first 13 days. Pink shaded area: lockdown period.

**Specification C**

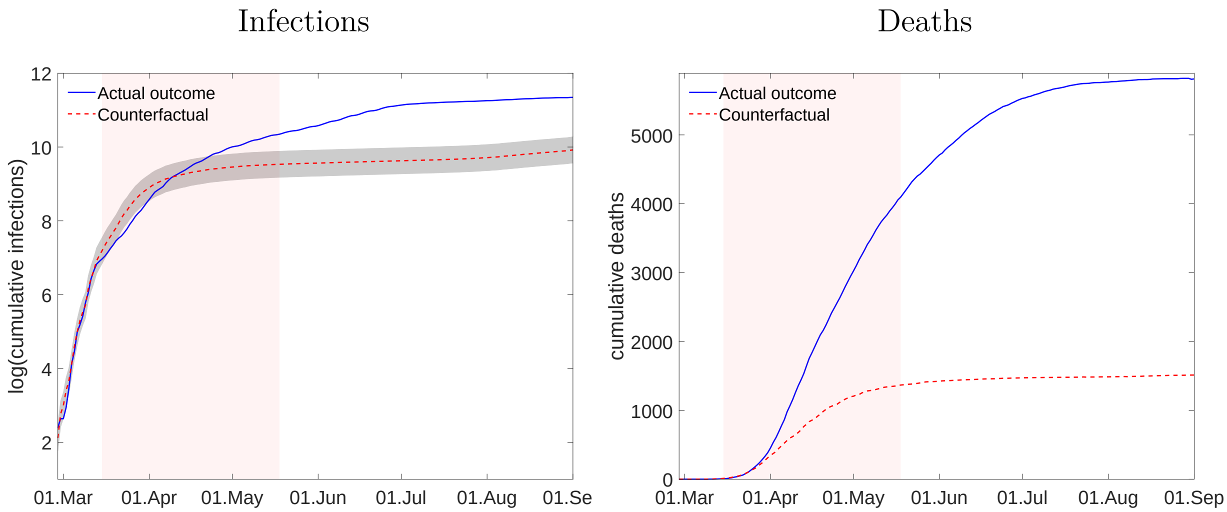


Figure A.5: Specification C (w/o Finland). Left panel shows infections, actual outcome (blue solid line) v counterfactual (red dashed line) in logs. Right panel shows deaths, actual outcome (blue solid line) v counterfactual (red dashed line). Counterfactual approximated by outcome of control unit, see Table A.2 for country weights of specification C. Gray shaded area: two standard deviations of difference between infections in Sweden and control unit during the first 13 days. Pink shaded area: lockdown period.

**Specification D**

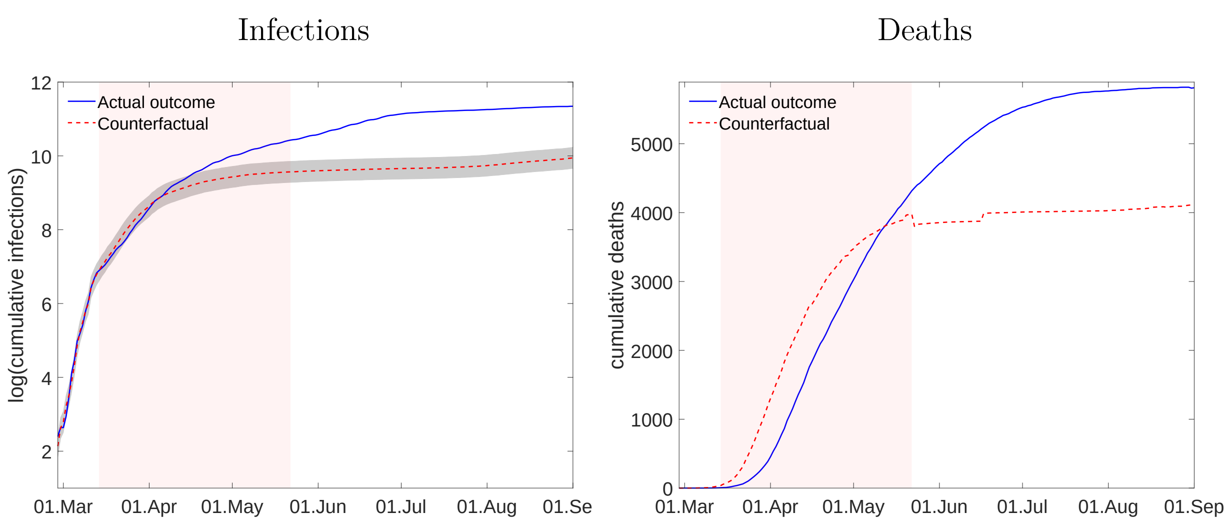


Figure A.6: Specification D (w/o Netherlands). Left panel shows infections, actual outcome (blue solid line) v counterfactual (red dashed line) in logs. Right panel shows deaths, actual outcome (blue solid line) v counterfactual (red dashed line). Counterfactual approximated by outcome of control unit, see Table A.2 for country weights of specification D. Gray shaded area: two standard deviations of difference between infections in Sweden and control unit during the first 13 days. Pink shaded area: lockdown period.

## Specification E

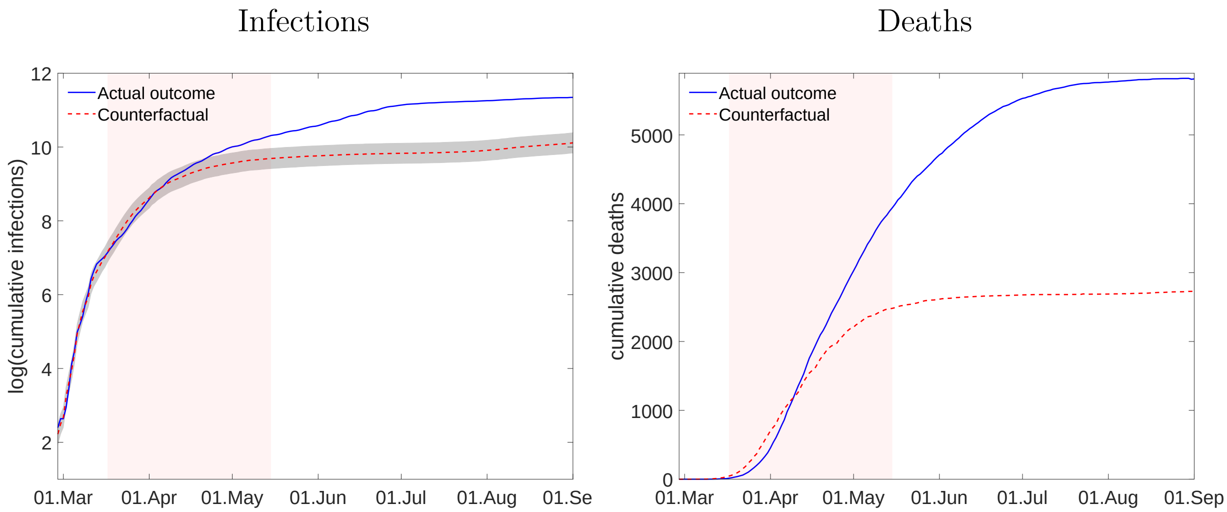


Figure A.7: Specification E (w/o Norway). Left panel shows infections, actual outcome (blue solid line) v counterfactual (red dashed line) in logs. Right panel shows deaths, actual outcome (blue solid line) v counterfactual (red dashed line). Counterfactual approximated by outcome of control unit, see Table A.2 for country weights of specification E. Gray shaded area: two standard deviations of difference between infections in Sweden and control unit during the first 13 days. Pink shaded area: lockdown period.

## Specification F

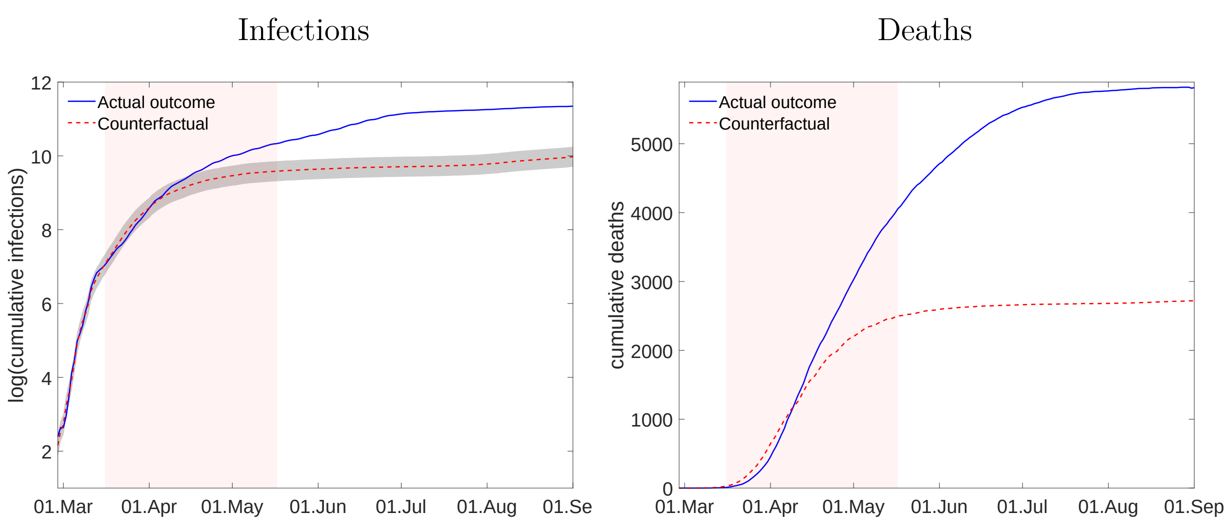


Figure A.8: Specification F (w/o Spain). Left panel shows infections, actual outcome (blue solid line) v counterfactual (red dashed line) in logs. Right panel shows deaths, actual outcome (blue solid line) v counterfactual (red dashed line). Counterfactual approximated by outcome of control unit, see Table A.2 for country weights of specification F. Gray shaded area: two standard deviations of difference between infections in Sweden and control unit during the first 13 days. Pink shaded area: lockdown period.

### Specification G

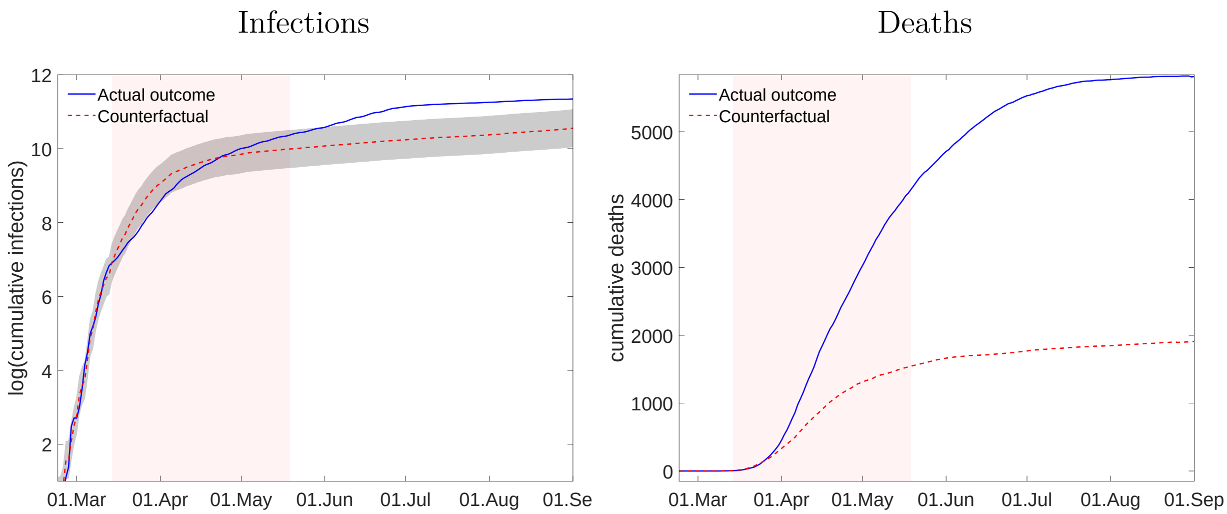


Figure A.9: Specification G. Left panel gives infections, actual outcome (blue solid line) v counterfactual (red dashed line) in logs. Right panel shows deaths, actual outcome (blue solid line) v counterfactual (red dashed line). Counterfactual approximated by outcome of control unit, see Table A.2 for country weights of specification G. Gray shaded area: two standard deviations of difference between infections in Sweden and control unit during the first 13 days. Pink shaded area: lockdown period.

### Specification H

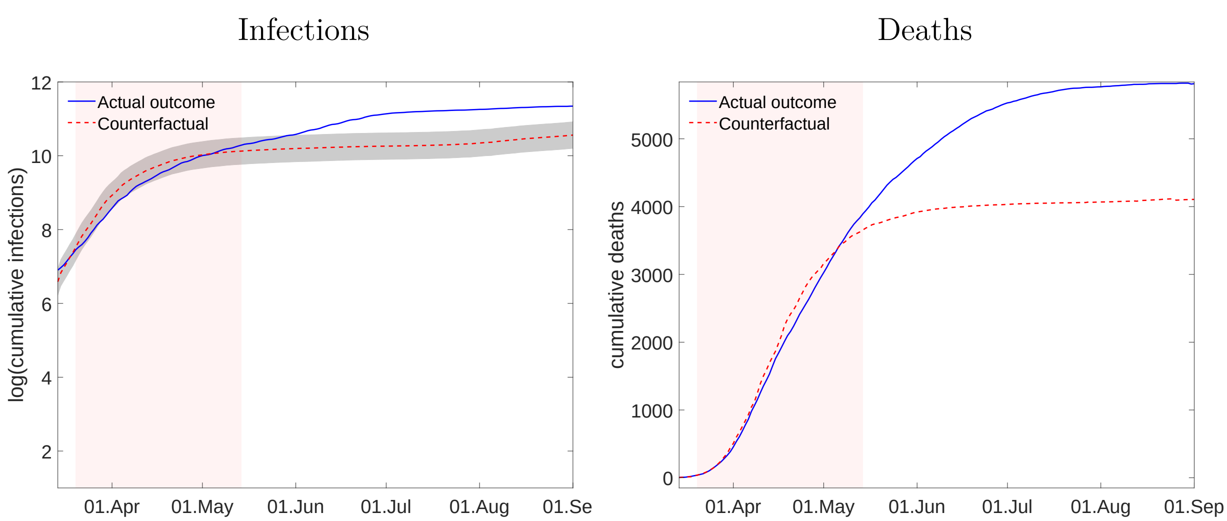


Figure A.10: Specification H. Left panel shows infections, actual outcome (blue solid line) v counterfactual (red dashed line) in logs. Right panel shows deaths, actual outcome (blue solid line) v counterfactual (red dashed line). Counterfactual approximated by outcome of control unit, see Table A.2 for country weights of specification H. Pink shaded area: lockdown period.

### Specification I

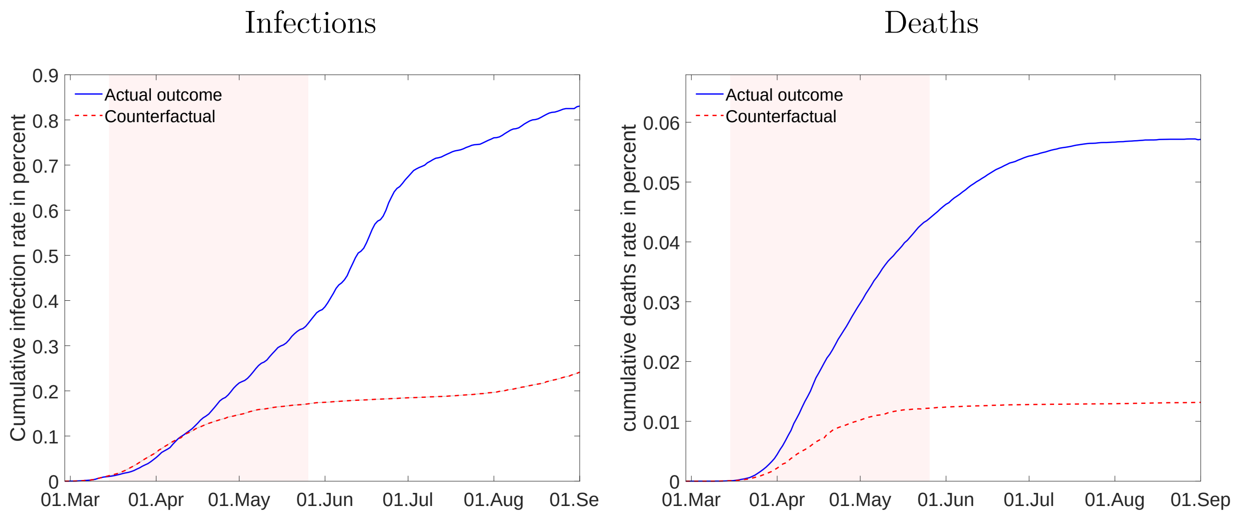


Figure A.11: Specification I. Left panel shows infections, actual outcome (blue solid line) v counterfactual (red dashed line) in logs. Right panel shows deaths, actual outcome (blue solid line) v counterfactual (red dashed line). Counterfactual approximated by outcome of control unit, see Table A.2 for country weights of specification I. Pink shaded area: lockdown period.