IEA Perspectives 1

# DID LOCKDOWNS WORK?

## The verdict on Covid restrictions

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Steve H. Hanke

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Economic Affairs

This book is a revised and extended version of a working paper by the same authors that carried the title ‘A Literature Review and Meta-Analysis of the Effects of Lockdowns on COVID-19 Mortality – II’, which was published on 20 May 2022.

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# About the authors

**Jonas Herby** is a special adviser at the Center for Political Studies (CEPOS), an independent classical liberal think tank based in Copenhagen, Denmark, where his research focuses on law and economics. Until mid-April 2020, Jonas had been confident that restrictions were effective and even published two reports where he assumed that the restrictions in Denmark were the direct cause of the differences in death tolls between Denmark and Sweden. But after observing that the first Covid waves peaked almost simultaneously in Denmark and Sweden, his interest in the effect of Covid restrictions grew. His interest turned into skepticism, and he has since then published numerous articles and reports on the subject. Herby holds a master's degree in Economics from the University of Copenhagen and has studied one year as a special student at the University of Wisconsin – Madison.

**Lars Jonung** is professor emeritus at the Knut Wicksell Centre for Financial Studies, Department of Economics, Lund University, Sweden. He served as chairperson of the Swedish Fiscal Policy Council 2012-13. He was Research Advisor at DG ECFIN, European Commission, Brussels, 2000-2010. Inspired by the threat of a bird flu pandemic, he initiated in 2006 a model-based study of the macroeconomic effects of a pandemic on the European economy. The basic conclusion was that a pandemic would take a huge toll in human suffering but it would not cause a major economic downturn. This result was based on the implicit assumption that no restrictions on business, trade, and the movement of workers were enforced. In Swedish public debate, he opposed the use of lockdowns during the COVID-19 pandemic, using the evidence from his earlier work. Eventually, he teamed up with Steve Hanke and Jonas Herby to prepare this IEA book. Prior to moving to Brussels, Jonung was professor in economics at the Stockholm School of Economics, Stockholm. He served as chief economic adviser to Prime Minister Carl Bildt in 1991-94. His research covers monetary and fiscal policy, inflationary expectations, the euro, and European integration. He holds a PhD in Economics from the University of California, Los Angeles, 1975. He is the author of several books in English and Swedish.

**Steve H. Hanke** is a Professor of Applied Economics and Founder & Co-Director of The Johns Hopkins Institute for Applied Economics, Global Health, and the Study of Business Enterprise in Baltimore. He is a well-known currency reformer and a currency and commodity trader. Prof. Hanke served on President Reagan's Council of Economic Advisers, has been an adviser to five foreign heads of state and five foreign cabinet ministers, and has held a cabinet-level rank as a state counsellor in both Lithuania and Montenegro. He has been awarded seven honourary doctorate degrees and is an Honourary Professor at four foreign institutions. He was President of Toronto Trust Argentina in Buenos Aires in 1995, when it was the world's best-performing mutual fund. Currently, he serves as Chairman of the Supervisory Board of Advanced Metallurgical Group N.V. in Amsterdam. In 1998, he was named one of the twenty-five most influential people in the world by *World Trade Magazine.* In 2020, Prof. Hanke was named a Knight of the Order of the Flag.

# Abstract

The purpose of this systematic review and meta-analysis is to determine the effect of lockdowns, also referred to as ‘Covid restrictions’, ‘social distancing measures’ etc., on COVID-19 mortality based on available empirical evidence. We define lockdowns as the imposition of at least one compulsory, non-pharmaceutical intervention (NPI). We employ a systematic search and screening procedure in which 19,646 studies are identified that could potentially address the purpose of our study. After three levels of screening, 32 studies qualified. Of those, estimates from 22 studies could be converted to standardised measures for inclusion in the meta-analysis. They are separated into three groups: lockdown stringency index studies, shelter-in-place-order (SIPO) studies, and specific NPI studies. Stringency index studies find that the average lockdown in Europe and the United States in the spring of 2020 only reduced COVID-19 mortality by 3.2 per cent. This translates into approximately 6,000 avoided deaths in Europe and 4,000 in the United States. SIPOs were also relatively ineffective in the spring of 2020, only reducing COVID-19 mortality by 2.0 per cent. This translates into approximately 4,000 avoided deaths in Europe and 3,000 in the United States. Based on specific NPIs, we estimate that the average lockdown in Europe and the United States in the spring of 2020 reduced COVID-19 mortality by 10.7 per cent. This translates into approximately 23,000 avoided deaths in Europe and 16,000 in the United States. In comparison, there are approximately 72,000 flu deaths in Europe and 38,000 flu deaths in the United States each year. When checked for potential biases, our results are robust. Our results are also supported by the natural experiments we have been able to identify. The results of our meta-analysis support the conclusion that lockdowns in the spring of 2020 had a negligible effect on COVID-19 mortality. This result is consistent with the view that voluntary changes in behaviour, such as social distancing, did play an important role in mitigating the pandemic.

**Key Words:** COVID-19, Covid restrictions, social distancing measures, lockdowns, non-pharmaceutical interventions, mortality, systematic review, meta-analysis

**JEL Classification:** I18; I38; D19

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# List of acronyms

| | |
|---|---|
| COVID-19….. | Coronavirus Disease 2019 |
| NPI………….. | Non-pharmaceutical intervention |
| OxCGRT……. | Oxford COVID-19 Government Response Tracker |
| PHEIC………. | Public Health Emergency of International Concern |
| PRISMA…….. | Preferred Reporting Items for Systematic Reviews and Meta-Analyses |
| PWA………… | Precision-weighted average |
| $R_t$…………… | The effective reproductive number. This is the expected number of new infections caused by an infectious individual in a population at time *t* |
| SE…………… | Standard errors |
| SIPO………… | Shelter-in-place order |
| SIR………….. | Susceptible-Infected-Recovered |

# Foreword

The path that led to this meta-analysis of the effects of COVID-19 lockdowns (also referred to as ‘Covid restrictions’, ‘social distancing measures’, etc.) on mortality began in Sweden. Early in the pandemic, Sweden embraced a relatively modest response to the coronavirus with remarkably few mandatory restrictions compared to the international pattern. The response was based on advice and recommendations concerning individual behaviour, not on binding compulsory measures such as lockdowns.

Why this Swedish exceptionalism? This was a question Lars Jonung and Steve Hanke began to ponder back in May 2020. We discovered that the cornerstone of the Swedish response was found in its constitution, specifically its most important part, the Regeringsform. Chapter 2, Article 8 states: ‘Everyone shall be protected in their relations with the public institutions against deprivations of personal liberty. All Swedish citizens shall also in other respects be guaranteed freedom of movement within the Realm and freedom to depart the Realm.’ The Regeringsform makes exceptions only for prisoners and military conscripts, and there is no provision for a peacetime state of emergency. While the constitutions of neighbouring Finland and Norway also guarantee freedom of movement, neither juxtaposes that provision with a broad protection of ‘personal liberty.’

The Swedish constitution comes into play in another, perhaps more significant, way, namely the strong independence of public authorities from government interference. This unique feature was first introduced in the Regeringsform of 1634, which followed the death of King Gustavus Adolphus II in the Thirty Years War. It insulates Sweden’s public institutions from political meddling to a much greater degree than in any other democracy.

The Public Health Agency of Sweden – like other public bodies, such as the world's oldest central bank, the Riksbank – operates with an incomparably high degree of independence from the government. Chapter 12, Article 2 of the Regeringsform spells this out: 'No public authority, including the Riksdag' – the Parliament – 'or decision-making body of any local authority, may determine how an administrative authority shall decide in a particular case relating to the exercise of public authority vis-à-vis an individual or a local authority, or relating to the application of law.'

So, the Public Health Agency of Sweden is directed and operated by experts – not government political appointees. These experts were the architects of Sweden's exceptional low-key response to the coronavirus pandemic in the early stages of the pandemic.

Sweden's exceptionalism rests on both its formal, written constitution and the high degree of trust infused in the country's customs and habits. It is one thing to have rules, but another to follow them (Jonung and Hanke 2020).[1]

As it turns out, Sweden's approach with few restrictions on individual rights of free movement ran counter to the more authoritarian approaches to the pandemic that were taken in other countries. As a result, a firestorm of criticism was levelled at Sweden.

This led Jonung and Hanke to the next questions: Do lockdowns work to reduce mortality? And if so, to what extent do they work? (Jonung and Röger 2006).[2] In our search for experts who could answer that question, Jonung became aware of ongoing research by Jonas Herby in Denmark. Shortly thereafter, Jonung, Hanke, and Herby decided that the best way to address the question of the efficacy of lockdowns would be to conduct a meta-analysis. To this end, Herby filed a formal protocol with the Social Science Research Network, which was published on 15 July 2021, and with that our research accelerated at top speed (Herby et al. 2021).

What did we discover? Our initial search identified 19,646 studies that could potentially address the problems that we are researching. But, only

1 On this account see also Lars Jonung 2020 Sweden's Constitution Decides Its Exceptional Covid-19 Policy. *VoxEU*, 18 June 2020 (https://cepr.org/voxeu/columns/swedens-constitution-decides-its-exceptional-covid-19-policy).

2 Here Jonung was inspired by his early work for the European Commission on forecasting the economic effects of a pandemic in Europe.

22 of those studies contained data that could be converted into standardised, comparable measures that could be included in our meta-analysis. Using these measures, we found, among other things, that lockdowns in the spring of 2020 in Europe resulted in 6,000 to 23,000 deaths avoided. To put those numbers into context, during an average flu season, approximately 72,000 deaths are recorded in Europe. Our results made clear that lockdowns had negligible public health effects when measured by mortality.

Our research was published by The Johns Hopkins Institute for Applied Economics, Global Health, and the Study of Business Enterprise as a Studies in Applied Economics working paper ‘A Literature Review and Meta-Analysis of the Effects of Lockdowns on COVID-19 Mortality’ on 21 January 2022 (Herby et al. 2022a). This publication attracted immediate attention, making its way to the White House press briefing room and the halls of the U.S. Congress.

Since our results flew in the face of the lockdown narrative generated by officialdom, they were controversial and generated a great deal of media attention, most of which was negative and repetitive. ‘Appendix II: Public response to the first edition of our working paper’ documents the biased, treatment that our research received in the media. And the media was not the only biased treatment that our work received. After publishing our research protocol ‘Protocol for “What Does the First XX Studies Tell Us about the Effects of Lockdowns on Mortality? A Systematic Review and Meta-Analysis of COVID-19 Lockdowns”’ on 15 July 2021, the Social Science Research Network refused to publish a second edition of our working paper (Herby et al. 2022b).[3] In our view, this is unprecedented. For the correspondence of that sorry episode, see: ‘Appendix III: Our Letter to Social Science Research Network (SSRN).’

After publishing the second edition of our Johns Hopkins working paper, ‘A Systematic Literature Review and Meta-Analysis of the Effects of Lockdowns on COVID-19 Mortality – II’ on 20 May 2022 and further polishing our work, we are pleased that the Institute of Economic Affairs has, after appropriate peer-review, decided to offer our work to a wider audience.

For those who value liberty, our findings will be sobering, if not depressing. Indeed, the COVID-19 pandemic gave rise to widespread lockdowns and some of the greatest infringements on personal liberties under peacetime

3 Published by The Johns Hopkins Institute for Applied Economics, Global Health, and the Study of Business Enterprise as a Studies in Applied Economics.

conditions in history. In the final analysis, these infringements generated negligible public health benefits while imposing a set of massive costs on society. As we interpret the available evidence, a cost–benefit analysis of the lockdowns applied suggests that the policy of lockdowns represents a global policy failure of gigantic proportions. Of course, research on the effects of lockdowns does not stop with our report. Still, we are convinced that future work will not significantly modify the conclusions presented here.

December 2022

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# 1. Introduction*

Social distancing works. If you keep distance from others, your risk of being infected with a communicable disease is reduced. However, the fact that social distancing works does not imply that compulsory non-pharmaceutical interventions (NPIs), commonly known as 'lockdowns' – policies that restrict internal movement, close schools and businesses, ban international travel and/or other activities – work. If governments primarily close activities that hardly anyone wants to participate in during an ongoing pandemic, the effect of the lockdown will be modest. If there is too much non-compliance, the effect of the lockdown will be modest. If government only regulates a fraction of the activities where people can become infected, the effect of the lockdown will be modest. If people react strongly to lower infection rates following lockdowns by being much less careful, the effect of the lockdown will be modest. If, if, if…

Although many people perceive lockdowns as extremely effective in reducing Coronavirus Disease 2019 (COVID-19) infections and mortality, it is today – from a research perspective – unknown to what extent lockdowns did in fact reduce COVID-19 infections and COVID-19 mortality. The goal of this study is to answer the following research question:

* We have received helpful comments from Douglas Allen, Torben M. Andersen, Fredrik N. G. Andersson, Andreas Bergh, Jonas Björk, Anders Björkman, Christian Bjørnskov, Joakim Book, Gunnar Brådvik, Kristoffer Torbjørn Bæk, Dave Campbell, Bernard Casey, Kevin Dowd, Ulf Gerdtham, Nicholas Hanlon, Caleb Hofmann, Olga B. Jonas, Daniel B. Klein, Fredrik Charpentier Ljungqvist, Christian Heebøl-Nielsen, Martin Paldam, Jonas Ranstam, Spencer Ryan, John Strezewski, Roger Svensson, Ulf Persson, Anders Waldenström, and Joakim Westerlund. We also thank several proofreaders who reviewed the first version of this study and helped us find ways in which we could clarify our methodology and facilitate the understanding of our results. We thank Line Andersen, Troels Sabroe Ebbesen, and Anders Lund Mortensen for excellent research assistance. Needless to say, the usual disclaimer holds: All remaining errors are our own.

Were lockdowns effective in reducing COVID-19 mortality? We also examine if some NPIs were more effective than others.

### *Definition of 'lockdown' and 'NPI'*

We use 'NPI' to describe *any government mandate that directly restricts people's possibilities*. Our definition does *not* include governmental recommendations, governmental information campaigns, access to mass testing, voluntary social distancing, etc., but *do* include mandated interventions such as closing schools or businesses, mandated face masks, etc.

During the COVID-19 pandemic, lockdowns have mainly been used to describe two different things. Some use 'lockdown' under the definition of 'a period of time in which people are not allowed to leave their homes or travel freely'. Others use 'lockdown' more broadly to describe governments' responses to the pandemic in terms of less or more strict interventions.[4] We follow the latter use and define *lockdown* as any policy consisting of at least one NPI as described above.[5] We use shelter-in-place orders (SIPOs) to describe the former use of the term 'lockdown'.

Our focus is on the effect of compulsory NPIs, policies that, for example, restrict internal movement, close schools and businesses, ban international travel, etc. We do not look at the effect of voluntary behavioural changes (e.g., voluntary mask wearing), the effect of recommendations (e.g., recommended mask wearing), or governmental services (e.g., voluntary mass testing).

The first NPIs were implemented in China, starting in early 2020. From there, the pandemic and NPIs spread first to Italy and later to virtually all other countries (see Figure 1). Of the 186 countries covered by the Oxford

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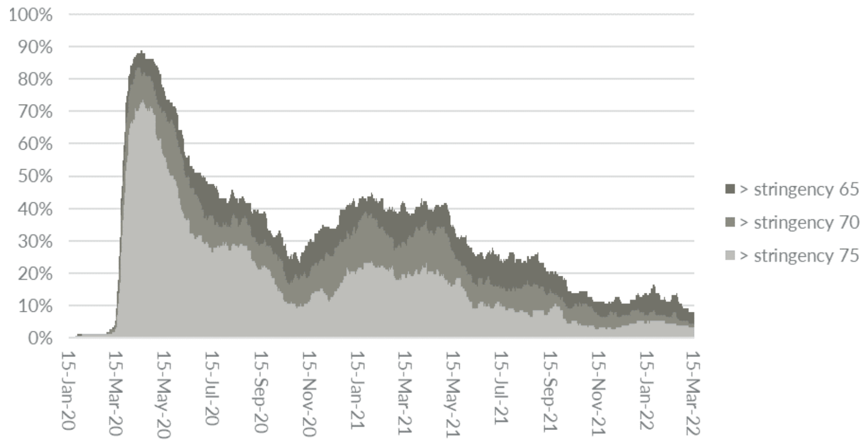
4 See https://dictionary.cambridge.org/dictionary/english/lockdown and https://ig.ft.com/coronavirus-lockdowns/ for two different examples of how the term 'lockdown' is defined and used.

5 For example, we will say that the government of Country A introduced the *non-pharmaceutical interventions* of school closures and shelter-in-place orders as part of the country's *lockdown.*

COVID-19 Government Response Tracker (OxCGRT),[6] only Comoros, an island country in the Indian Ocean with a population below 1 million, did not impose at least one NPI (as defined by OxCGRT) before the end of March 2020. Since virtually all countries have implemented some sort of restrictions, we are essentially studying how the degree of lockdowns affect mortality rates.

**Figure 1: Percentage of countries with Oxford COVID-19 Government Response Tracker (OxCGRT) stringency index readings above thresholds 65, 70, and 75, respectively**




*Source:* Our World in Data (2022).

*Comment:* The OxCGRT stringency index measures the stringency of lockdowns on a scale from 0 to 100, where a higher value corresponds to stricter lockdowns. *The figure shows the share of countries where the* OxCGRT *stringency index on a given date surpassed index 65, 70 and 75 respectively. Only countries with more than one million citizens are included (153 countries in total).* The OxCGRT *stringency index records the strictness of NPI policies that restrict people's behaviour. It is calculated using all ordinal containment and closure policy indicators (i.e., the degree of school and business closures, etc.), plus an indicator recording public information campaigns.*

6 The Oxford Covid-19 Government Response Tracker (OxCGRT) collects systematic information on policy measures that governments have taken to tackle COVID-19. The different policy responses have been tracked since 1 January 2020, cover more than 180 countries, and are coded into 23 indicators, such as school closures, travel restrictions, and vaccination policies. These policies are recorded on a scale to reflect the extent of government action, and scores are aggregated into a suite of policy indices.

### *Do lockdowns work?*

One could question the necessity of examining the effectiveness of certain NPIs that have been used for centuries. However, although NPIs such as school and workplace closures were recommended by the World Health Organization (WHO) before the COVID-19 pandemic in the event of an extraordinarily severe pandemic influenza, the quality of the evidence regarding the effectiveness of such measures was, in general, very low (see Table 1).

**Table 1: Quality of evidence for selected NPIs as assessed by WHO before the COVID-19 pandemic.**

| NPI | Quality of evidence as assessed by WHO before COVID-19 |
|---|---|
| School measures and closures | Very low |
| Workplace measures and closures | Very low |
| Avoiding crowding | Very low |
| Entry and exit screening (travellers) | Very low |
| Internal travel restrictions | Very low |
| Border closure | Very low |

Source: WHO (2019)

Despite the very low quality of the evidence (see Table 1), early epidemiological studies predicted that NPIs would have large effects. An often cited model simulation study by researchers at Imperial College London (Ferguson et al. 2020) predicted that a suppression strategy would reduce COVID-19 mortality by up to 99 per cent.[7] Ferguson et al. (2020) state that 'it is highly likely that there would be significant spontaneous changes in population behaviour even in the absence of government-mandated interventions' but also that 'any one intervention in isolation is likely to be limited, requiring multiple interventions to be combined to have a substantial impact on transmission' and 'we predict that transmission will quickly rebound if interventions are relaxed' causing many to perceive their projections as a forecast in the case of no lockdown.[8] And many commentators hold the results from Ferguson et al. (2020) responsible for the subsequent lockdown in the United Kingdom (Woolhouse 2022; Sumption 2022; Baker and Hanke 2022).[9]

Already early in the pandemic, there was reason to question whether lockdowns were as effective as promised.[10] First, there was no clear negative correlation between the degree of lockdown and actual outcomes on fatalities in the spring of 2020 (see Figure 2). Although lack of correlation does not

7 With $R_0$ = 2.0 and trigger on 60, the number of COVID-19-deaths in Great Britain could be reduced to 5,600 deaths from 410,000 deaths (-99%) with a policy consisting of case isolation + home quarantine + social distancing + school/university closure, see Table 4 in Ferguson et al. (2020:13). $R_0$ (the basic reproduction rate) is the expected number of cases directly generated by one case in a population where all individuals are susceptible to infection. The lowest effect of lockdowns modelled by Ferguson et al. (2020) was with $R_0$ = 2.6, trigger on 200-400, and case isolation + home quarantine + social distancing. In this case, deaths were predicted to be reduced from 550.000 to 120.000 (-78%).

8 This perception was supported by statements from the study's authors and Imperial College London. 17 March 2020, Imperial College tweeted that 'without more action, the virus would have overwhelmed intensive care units.' And, Neil Ferguson is cited for saying that 'we're going to have to suppress this virus – frankly, indefinitely – until we have a vaccine.' See https://www.nytimes.com/2020/03/16/us/coronavirus-fatality-rate-white-house.html.

9 For example, see https://www.theguardian.com/world/2020/mar/16/new-data-new-policy-why-uks-coronavirus-strategy-has-changed. Woolhouse (2022), who in January 2020 was appointed to a SAGE sub-committee called the Scientific Pandemic Influenza Group on Modelling (SPI-M) in the United Kingdom, describes how the reaction was due to a misinterpretation of the report from Ferguson et al. (2020): 'The report should have been a wake-up call that we needed to invest quickly and heavily in other ways to control novel coronavirus or – according to the model – we'd end up in lockdown. This implication was barely mentioned – lockdown was accepted as a necessity the first time it was proposed. When Report 9 [i.e., Ferguson et al. (2020)] was published, the details of the scenarios modelled were quickly forgotten, as were any mentions of the assumptions, caveats, and uncertainties of the analysis. Report 9 was condensed to the simple but misleading message that, if the government did not impose full lockdown immediately, over half a million people would die.'

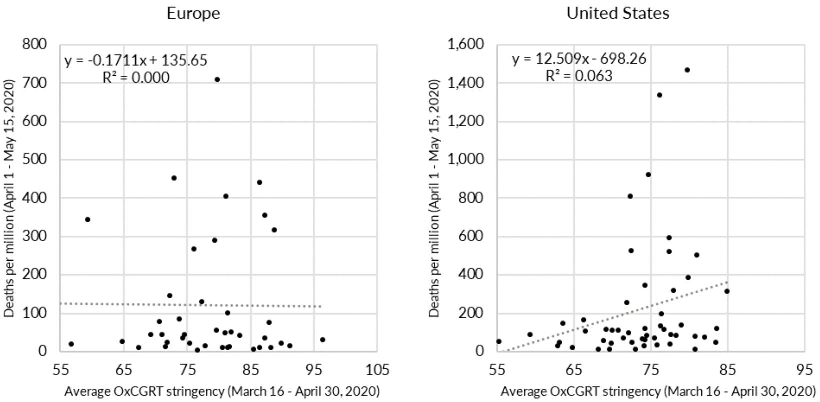
10 Given Ferguson and his Imperial College team's track record, it is surprising that questions were not raised before jumping on the lockdown bandwagon, see Hanke and Dowd (2022).

necessarily imply lack of causality, one would – given the large perceived effect of lockdowns that studies such as Ferguson et al. (2020) have suggested – expect at least to observe a simple negative correlation between COVID-19 mortality and the degree to which lockdowns were imposed.

Second, several studies pointed to facts questioning the effect of lockdowns. For example, Atkeson et al. (2020) showed in August 2020 that ‘across all countries and US states that we study, the growth rates of daily deaths from COVID-19 fell from a wide range of initially high levels to levels close to zero within 20-30 days after each region experienced 25 cumulative deaths.’ Goolsbee and Syverson (2021) found in June 2020 that

> legal shutdown orders account for only a modest share of the massive changes to consumer behavior […]. While overall consumer traffic fell by 60 percentage points, legal restrictions explain only 7 percentage points of this. Individual choices were far more important and seem tied to fears of infection. Traffic started dropping before the legal orders were in place; was highly influenced by the number of COVID deaths reported in the county; and showed a clear shift by consumers away from busier, more crowded stores toward smaller, less busy stores in the same industry.

**Figure 2: The positive correlation between the OxCGRT stringency index and COVID-19 mortality in 44 European countries and the 50 U.S. states (and Washington, DC) during the first wave in 2020**




Source: Our World in Data (2022).

Note: The OxCGRT stringency index measures the stringency of lockdowns on a scale from 0 to 100, where a higher value corresponds to stricter lockdowns. Isle of Man and North Macedonia are not included, because there is no stringency index data for these two countries before 30 April 2020.

Although the externalities associated with a communicable disease such as COVID-19 are evident, it is less clear how these externalities can be regulated effectively.[11] Specifically, it remains an open question as to whether lockdowns have had a large, significant effect on COVID-19 mortality.

### *Our contribution*

We address the question 'Were lockdowns effective in reducing COVID-19 mortality?' by evaluating the current academic literature on the relationship between lockdowns and COVID-19 mortality rates.[12] Our analysis is based on the evidence found in studies published between 1 January 2020 and 21 February 2022.

11 In economics, an externality is an indirect cost or benefit to a third party that arises as an effect of another party's (or parties') activity.

12 We use 'mortality' and 'mortality rates' interchangeably to mean COVID-19 deaths as a percentage of the population.

We are still in the early phases of the scientific and quantitative evaluation of the effects of lockdowns, and future research will continue to improve our understanding of lockdowns. Still, we find it valuable to summarise in a consistent way the evidence available from the first two years of the pandemic.

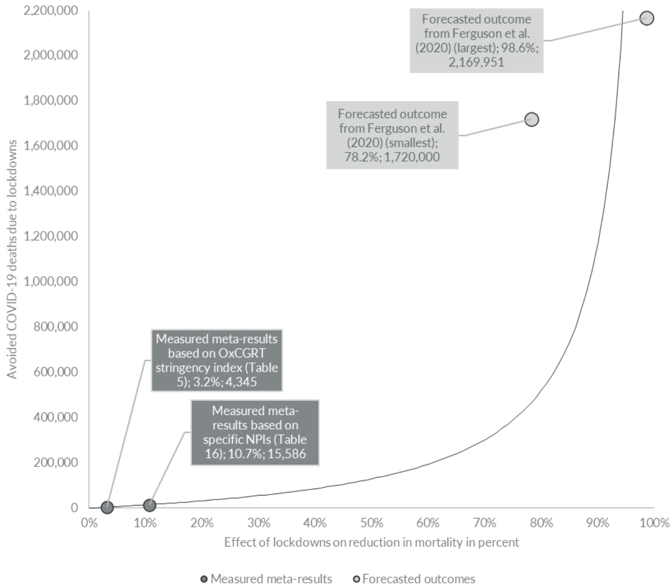
Compared to other reviews such as Herby (2021) and Allen (2021), the main difference in our approach is that we carry out a systematic and comprehensive search strategy to identify all papers potentially relevant, and carry out a meta-analysis combining evidence from several existing studies to answer the question we pose. Results need repeated replication to be unambiguous and credible, but replication is unfortunately rare.

Mueller-Langer et al. (2019) find that only 0.1 per cent of publications in the top 50 economics journals were replication studies. However, as described by Paldam (2022), the same question is often analysed in many studies, which use different datasets, estimation models, control variables, etc. Thus, instead of strict replication, there are often several partial replications.

A meta-analysis is a technique developed to analyse if the aggregation of the evidence from studies that analyse the same question leads to a general result. Thus, in our meta-analysis, we aim to replace replication by presenting results in such a way that they can be systematically assessed and used to derive overall conclusions.

In Figure 3 below, we compare the measured results from our meta-analysis to the forecasts derived from the models used in Ferguson et al. (2020). Overall, the meta-analysis does not support the notion that lockdowns in the spring of 2020 had a large effect on COVID-19 mortality, as many modelling studies had concluded they would.

**Figure 3: Divergence between avoided number of deaths in the United States as measured by our meta-results and the forecasted outcome from Imperial College London**




Note: The effect of lockdowns on total mortality based on the meta-study's precision-weighted averages (PWA) is calculated as total COVID-19 deaths by 1 July 2020 (128,063 COVID-19 deaths) x (1/(1-PWA)-1). The relative effect of lockdowns on total mortality based on Ferguson et al. (2020) is calculated as the largest and smallest predicted relative effect multiplied with their mortality estimate of 2.2 million deaths in a 'do nothing' scenario in the United States. For more details see Figure 10 p.112.

### *Updates in this version*

The first version of this literature review and meta-analysis (Herby et al. 2022a) generated countless comments, ranging from ad hominem, wrong, and irrelevant to of use and of constructive value. For more on the discussion of the first version of this literature review and meta-analysis, see Appendix II (also see Appendix III for an overview of letters exchanged between SSRN and the authors regarding the updated version).

In this updated version, we have responded to criticism and incorporated all constructive comments. Paragraphs have been rewritten for clarity. For example, it was not clear to some commentators that we distinguished between the effect of social distancing and the effect of lockdowns. We have tried to make this distinction more transparent, and the first sentence in the introduction is now ‘Social distancing works’. Our definition of lockdown also created confusion among some critics, so we have elaborated our definition to make it clearer. We have also added more examples to support the understanding of our results and conclusions. For example, we have elaborated on section 5.2.3 where we discuss why the effect of lockdowns – as our measured meta-results show – was limited.

Our results have also changed for three reasons. First, we have excluded some studies that we now believe to be ineligible. Second, we have updated our literature search, so more studies are now included. Finally, we have changed some calculations. It is worth noting that we, not the commentators, did identify and correct one computational error that was contained in the first version of this study. Its correction had no significant effect on our overall conclusion.

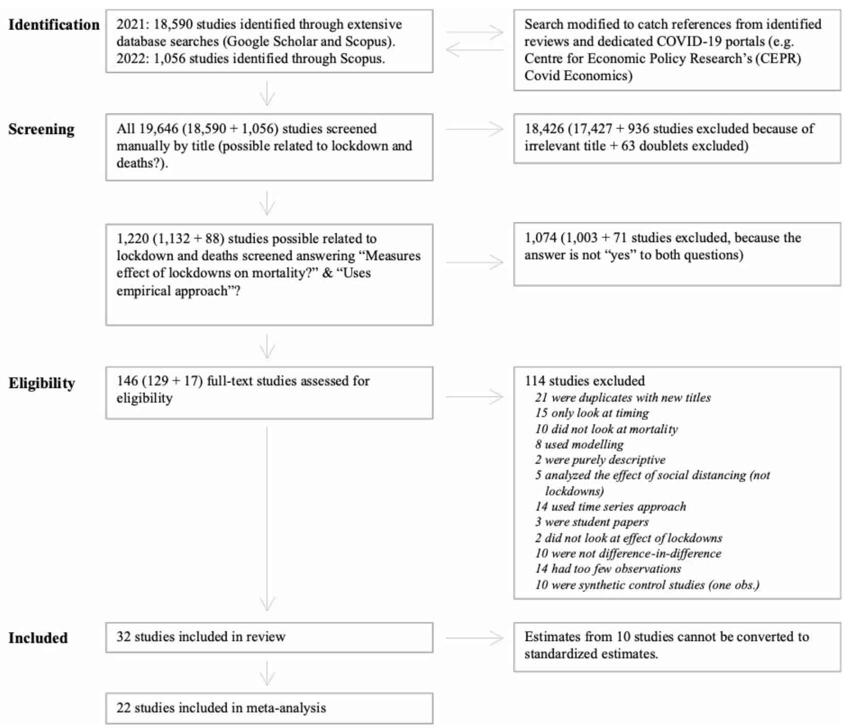
We have also expanded the section on specific NPIs to give a more in-depth analysis of the results. These updates have changed our estimates, but not the overall conclusion. We believe that one major mistake in our first version was our failure to explain that the overall conclusions do not depend on whether the impact of lockdowns on COVID-19 mortality was 0.2 per cent, 3.0 per cent or 15 per cent. As Figure 3 illustrates, all cases based on actual measurements of saved lives due to lockdowns are much smaller and far removed from the promises made by many epidemiologists, politicians, and the media.

Our literature review and meta-analysis are organised in the following way. In section 2, we describe our identification process for selecting relevant studies. That is, we explain our search strategy and eligibility criteria. In section 3, we present an account of the empirical evidence. Section 4 contains our meta-analysis of the impact of lockdowns on COVID-19 mortality. Section 5 contains our concluding observations and discussion.

# 2. Identification process: Search strategy and eligibility criteria

Figure 4 presents an overview of our identification process. It uses a flow diagram designed according to Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines by Moher et al. (2009). Of the 19,646 studies identified during our database searches, 1,220 remained after a title-based screening. Then 1,074 studies were excluded because they either did not measure the effect of lockdowns on mortality or did not use an empirical approach. This left 146 studies that were read and inspected carefully. After a more thorough assessment, 114 of the 146 were excluded, leaving 32 eligible studies. The 114 studies excluded in the final step are listed in Table 19 in Appendix I.

## Figure 4: The PRISMA flow diagram for the selection of studies




The inclusion rate of 32 eligible studies out of 19,646 identified studies (0.2 per cent) is in the range of other systematic literature reviews on the topic of COVID-19 lockdowns. Our inclusion rate is similar to Talic et al. (2021), who include 72 of 36,729 identified studies (0.2 per cent) and Iezadi et al. (2021) (35 of 12,523, 0.3 per cent), while is relatively low compared to, e.g., Rezapour et al. (2021)a  (26 of 2,176 studies, 1.2 per cent), Zhang et al. (2021) (47 of 1,649 studies, 2.9 per cent), and Johanna et al. (2020) (14 of 623 studies, 2.2 per cent).

A major reason for the difference in the inclusion rate is the choice of search strategy. Rezapour et al. (2021), Zhang et al. (2021) and Johanna et al. (2020) identify studies by searching publication databases such as PubMed, Scopus, Web of Science, SAGE, etc., while our search in Google Scholar is broader. For example, our search also includes presentations and books. Performing our search only with Scopus, instead of both Google Scholar and Scopus, results in an inclusion rate of 0.7 per cent.

Below, we present our search strategy and eligibility criteria. They follow the PRISMA guidelines and are specified in detail in our protocol at the Social Science Research Network (SSRN). Our protocol was first posted on 15 July 2021 (Herby et al. 2021).[13]

### *2. 1. Search strategy*

The studies we reviewed were identified by scanning Google Scholar and Scopus for English-language studies. We used a wide range of search terms that are combinations of three search strings: a disease search string (‘covid’, ‘corona’, ‘coronavirus’, ‘sars-cov-2’), a government response search string,[14] and a methodology search string.[15] We identified papers based on 1,360 search terms. We also required mentions of ‘deaths’, ‘death’, and/or ‘mortality’. The search terms were continuously updated (by adding relevant terms) to fit our criteria.[16]

We also included all papers published in *Covid Economics*. Our first search was performed between 1 July and 5 July 2021 and resulted in 18,590 unique studies. All studies identified using Scopus and *Covid Economics* were also found using Google Scholar. This made us comfortable that including other sources such as VoxEU and SSRN would not materially change the result. Indeed, many papers found using Google Scholar were from these sources. On 21 February 2022, we repeated our search on Scopus resulting in another 1,056 studies.

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13 The protocol was first published on 23 June 2021 and updated last on 28 October 2021.

14 The government response search string used was: ‘non-pharmaceutical’, ‘nonpharmaceutical’, ‘NPI’, ‘NPIs’, ‘lockdown’, ‘social distancing orders’, ‘statewide interventions’, ‘distancing interventions’, ‘circuit breaker’, ‘containment measures’, ‘contact restrictions’, ‘social distancing measures’, ‘public health policies’, ‘mobility restrictions’, ‘covid-19 policies’, ‘corona policies’, ‘policy measures’.

15 The methodology search string used was: ‘fixed effects’, ‘panel data’, ‘difference-in-difference’, ‘diff-in-diff’, ‘synthetic control’, ‘counterfactual’ , ‘counter factual’, ‘cross country’, ‘cross state’, ‘cross county’, ‘cross region’, ‘cross regional’, ‘cross municipality’, ‘country level’, ‘state level’, ‘county level’, ‘region level’, ‘regional level’, ‘municipality level’, ‘event study’.

16 If a potentially relevant paper from one of the 13 reviews (see eligibility criteria) did not show up in our search, we added relevant words to our search strings and ran the search again. The 13 reviews were: Allen (2021); Brodeur et al. (2021); Gupta et al. (2020); Herby (2021); Johanna et al. (2020); Nussbaumer-Streit et al. (2020); Patel et al. (2020); Perra (2020); Poeschl and Larsen (2021); Pozo-Martin et al. (2020); Rezapour et al. (2021); Robinson (2021); Zhang et al. (2021).

All 19,646 (18,590 from July 2021 and 1,056 from February 2022) studies were first screened based on the title. Studies clearly not related to our research question were deemed irrelevant.[17]

After screening based on the title, 1,220 papers remained. These papers were manually screened by answering two questions:

1. Does the study measure the effect of lockdowns on mortality?
2. Does the study use an empirical *ex post* difference-in-difference approach (see eligibility criteria below)?

Studies to which we could not answer 'yes' to both questions were excluded. When in doubt, we made the assessment based on reading the full paper, and in some cases, we consulted colleagues.[18]

After the manual screening, 146 studies were retrieved for a full, detailed inspection. These studies were carefully examined, and metadata and empirical results were stored in an Excel spreadsheet. All studies were assessed by at least two researchers. During this process, another 114 papers were excluded because they did not meet our eligibility criteria. A table with all 114 studies excluded in the final step can be found in Appendix I, Table 19. Below we explain the most important of our eligibility criteria. A full list can be found in our protocol (Herby et al. 2021).

### *2.2. Eligibility criteria*

#### *Focus on mortality and lockdowns*

We only include studies that attempt to establish a relationship (or lack thereof) between lockdown policies and COVID-19 mortality or excess mortality. Following our protocol (Herby et al. 2021), we exclude studies that use cases, hospitalisations, or other measures.

17 This included studies with titles such as 'COVID-19 outbreak and air pollution in Iran: A panel VAR analysis' and 'Dynamic Structural Impact of the COVID-19 Outbreak on the Stock Market and the Exchange Rate: A Cross-country Analysis Among BRICS Nations.'

18 Professor Christian Bjørnskov of the University of Aarhus was particularly helpful in this process.

While we regard analysis based on cases as problematic because of large data problems,[19] one could argue that including studies examining the effect of lockdowns on hospitalisation could improve the quality of our review and meta-analysis because it would allow us to include more studies. Using the same search strings at Scopus, but replacing 'deaths', 'death', and/or 'mortality' with 'hospitalization', 'intensive care', and/or 'ICU', indicates that including hospitalisations would yield another 1-2 eligible studies.[20]

Although including studies examining the effect of lockdowns on hospitalisation would potentially strengthen our results by adding more studies to the review and meta-analysis, we see little reason to believe that doing so would change our results significantly. It is true that a key argument for locking down countries around the world was to protect the healthcare sector and keep hospitalisations down. But one of the arguments for protecting the healthcare sector was that if hospitalisations were high and hospitals were overcrowded, there would be an unusually high excess mortality rate because COVID-19 patients would not be able to receive treatment.[21] Given this relationship between hospitalisations and deaths, we should see the effect of hospitalisations in our analysis of mortality.

19 Analyses based on cases pose major problems, as testing strategies for COVID-19 infections vary enormously across countries (and even over time within a given country). In consequence, cross-country comparisons of cases are, at best, problematic. Although these problems exist with death tolls as well, they are far more limited. Also, while cases and death tolls are correlated, there may be adverse effects of lockdowns that are not captured by the number of cases. For example, an infected person who is isolated at home with family under a SIPO may infect family members with a higher viral load, causing a more severe illness. So even if a SIPO reduces the number of cases, it may theoretically increase the number of COVID-19 deaths. Adverse effects like this may explain why studies such as Chernozhukov et al. (2021) find that a SIPO reduces the number of cases but has no significant effect on the number of COVID-19 deaths. Finally, mortality is hierarchically the most important outcome, see GRADEpro (2013).

20 Scopus returns 947 hits on mortality, etc. between 1 January 2020 and 30 June 2021. Searching for hospitalisation etc. yields another 35 hits corresponding to 3.7 per cent more studies.

21 E.g. Madsen et al. (2014) find that 'high bed occupancy rates were associated with a significant 9 percent increase in rates of in-hospital mortality and thirty-day mortality, compared to low bed occupancy rates. Being admitted to a hospital outside of normal working hours or on a weekend or holiday was also significantly associated with increased mortality.'

***Assessment of actual outcomes (in contrast to modelled outcomes)***

There are two different approaches to examine the relationship between mortality rates and lockdown policies. The first approach uses actual, measured mortality data. These are *ex post* studies based on actual mortality outcomes. The second approach uses simulated data on mortality and infection rates generated from models.[22] These are *ex ante* studies based on modelled outcomes.

In this review and meta-analysis, we include all studies from the former group but exclude all *ex ante* studies, as the results from these studies are determined by model assumptions and calibrations and cannot be the basis for solid empirical evidence for policy purposes, before these models have been empirically validated which is exactly the point of our study. This means that we exclude, e.g., the much-cited Ferguson et al. (2020) from Imperial College.

***Counterfactual difference-in-difference approach***

We exclude studies that do not use a counterfactual difference-in-difference approach (called 'controlled before-and-after studies' in some social sciences).[23]

Difference-in-difference is a quasi-experimental design that makes use of longitudinal data from treatment and control groups to obtain an appropriate counterfactual to estimate a causal effect. Difference-in-difference is typically used to estimate the effect of a specific intervention (for example, a non-pharmaceutical intervention) by comparing the changes in outcomes over time between a treatment group (where a specific NPI was in place) and a control group (where the NPI was not in place).

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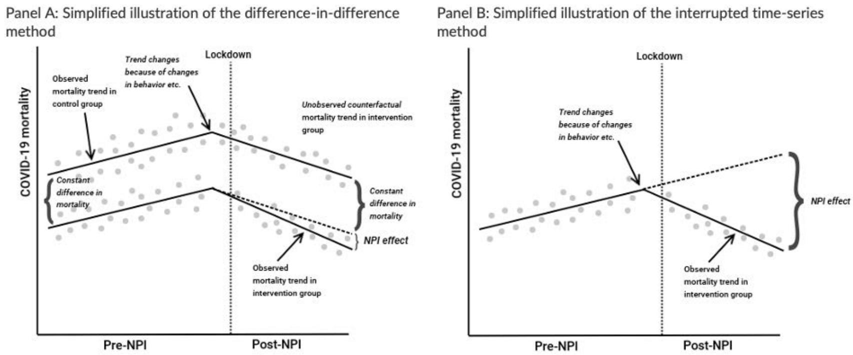
22 These simulations are often made in variants of the susceptible-infected-recovered (SIR) model, which can simulate the progress of a pandemic in a population consisting of people in different states (susceptible, infectious, or recovered) with equations describing the process of moving between these states. A common problem with epidemiological models is that they do not take spontaneous behavior changes into account. And even when they do, these behavior changes and consequently the results are based on the authors' assumptions, see Toxvaerd (2020).

23 See Mailman School of Public Health, Columbia (2007) which has also inspired Figure 5 and other parts of the description of the difference-in-difference method.

**Figure 5: Simplified illustration of the difference-in-difference approach compared to interrupted time-series approach when trend changes**




Difference-in-difference is used in observational settings where exchangeability between the treatment and control groups cannot be assumed, as is the case with COVID-19 mortality, where mortality rates differ between countries and over time. The difference-in-difference method relies on a somewhat weaker – although not negligible – exchangeability assumption known as the parallel, or common trends, assumption, as illustrated in Figure 5, Panel A.[24]

24 For a more in-depth discussion of the parallel assumption, we refer to David McKenzie 2020a Revisiting the Difference-in-Differences Parallel Trends Assumption: Part I Pre-Trend Testing. *World Bank* (blog), 21 January 2020 (https://blogs.worldbank.org/impactevaluations/revisiting-difference-differences-parallel-trends-assumption-part-i-pre-trend), David McKenzie 2020b Revisiting the Difference-in-Differences Parallel Trends Assumption: Part II What Happens If the Parallel Trends Assumption Is (Might Be) Violated? *World Bank* (blog), 3 February 2020 (https://blogs.worldbank.org/impactevaluations/revisiting-difference-differences-parallel-trends-assumption-part-ii-what-happens), and David McKenzie 2021 An Adversarial or 'Long and Squiggly' Test of the Plausibility of Parallel Trends in Difference-in-Differences Analysis. *World Bank* (blog), 10 March 2021 (https://blogs.worldbank.org/impactevaluations/adversarial-or-long-and-squiggly-test-plausibility-parallel-trends-difference). Also see David McKenzie 2022a A New Synthesis and Key Lessons from the Recent Difference-in-Differences Literature. *World Bank* (blog), 10 January 2022 (https://blogs.worldbank.org/impactevaluations/new-synthesis-and-key-lessons-recent-difference-differences-literature) and David McKenzie 2022b Explaining Why We Should Believe Your DiD Assumptions. *World Bank* (blog), 24 January 2022 (https://blogs.worldbank.org/impactevaluations/explaining-why-we-should-believe-your-did-assumptions).

Difference-in-difference is a useful technique to employ when examining the effect of lockdowns where randomisation is not possible. The approach removes biases in post-intervention period comparisons between the treatment and control groups that could be the result of permanent differences between those groups (e.g., caused by coincidences early in the pandemic[25]), as well as biases from comparisons over time in the treatment group that could be the result of trends due to other causes of the outcome (e.g., changes in behaviour or seasonality).

The exclusion of studies that do not use a counterfactual difference-in-difference approach means that we exclude all studies where the counterfactual is based on forecasting (for example, using a SIR model calibrated on mortality data). This means that we exclude studies such as Duchemin et al. (2020) and Matzinger and Skinner (2020). We also exclude all studies based on interrupted time-series designs. Interrupted time-series designs are useful when there is a stable long-term period before and after the time of the intervention examined (lockdowns) and where other things are relatively constant and/or can be controlled for. This is not the case with COVID-19 and lockdowns, where the period before (and often after) the intervention is short, where things are far from constant, and where changes in behaviour cannot easily be controlled for. As illustrated in Figure 5, Panel B, interrupted time-series risk overestimating the effect of lockdowns, if, for example, voluntary behavioural changes are important. Excluding interrupted time-series studies rules out works such as Bakolis et al. (2021) and Siedner et al. (2020).

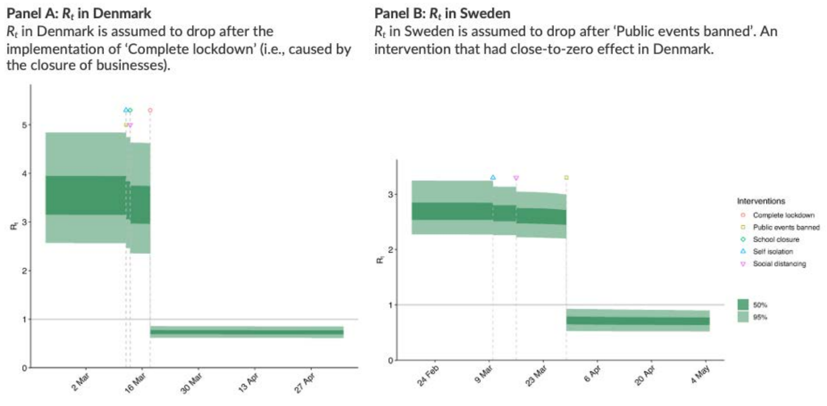
Given our criteria, we also exclude the much-cited paper by Flaxman et al. (2020), which claimed that lockdowns saved three million lives in Europe. Flaxman et al. (2020) assume that the pandemic will follow an epidemiological curve unless countries lock down. However, this assumption means that the only interpretation possible for the empirical results is that lockdowns are the only factor that matters, even if other factors such as changes in voluntary behaviour, seasonality, etc. caused the observed change in the reproduction rate, $R_t$. Figure 6 illustrates how problematic Flaxman's assumption is. The figure shows Flaxman et al. (2020)'s estimate of the effect of various NPIs on the effective reproduction number, $R_{t,}$[26] in

25 As we describe on page 140, Arnarson (2021) and Björk et al. (2021) show that areas in Europe where the winter holiday was relatively late (in week 9 or 10 rather than week 6, 7 or 8) were hit especially hard by COVID-19 during the first wave because the virus outbreak in the Alps could spread to those areas with ski tourists.

26 The effective reproductive number, denoted $R_t$, is the expected number of new infections caused by an infectious individual.

Denmark and Sweden. According to the results, banning public events had a close-to-zero effect in Denmark (Panel A) but huge effects in Sweden (Panel B).

**Figure 6: The assumptions used by Flaxman et al. (2020) lead to two contradictory conclusions: That banning public events had no effect in Denmark but were extremely effective in Sweden in March 2020**




Source: Flaxman et al. (2020), Extended Data Fig. 1 & Fig. 2.

Flaxman et al. (2020) are aware of this problem and state that 'our parametric form of $R_t$ assumes that changes in $R_t$ are an immediate response to interventions rather than gradual changes in behavior'. Despite stating that the results cannot be interpreted as an effect of lockdowns, media around the globe – supported by statements by the authors[27] – reported these findings as 'proof' that lockdowns had saved millions of lives.[28]

27 For example, Dr. Seth Flaxman said 'Lockdown averted millions of deaths', see e.g. https://www.bbc.com/news/health-52968523, and Samir Bhatt said 'Our estimates show that lockdowns had a really dramatic effect in reducing transmission,' and 'Without them [lockdowns] we believe the toll would have been huge,', see e.g. https://news.wgcu.org/2020-06-09/modelers-suggest-pandemic-lockdowns-saved-millions-from-dying-of-covid-19.

28 For example, see https://www.reuters.com/article/us-health-coronavirus-lockdowns-idUSKBN23F1G3, https://www.bbc.com/news/health-52968523, https://www.imperial.ac.uk/news/198074/lockdown-school-closures-europe-have-prevented/, https://www.france24.com/en/20200609-covid-19-lockdowns-saved-millions-of-lives-and-easing-curbs-risky-studies-find, and https://www.washingtonpost.com/health/2020/06/08/shutdowns-prevented-60-million-coronavirus-infections-us-study-finds/.

Similar interpretation problems are typical and are found in Brauner et al. (2021)[29] and Hsiang et al. (2020) for example.

In our view, Flaxman et al. (2020), Brauner et al. (2021), Hsiang et al. (2020), etc. illustrate how problematic it is to force data to fit a certain model and certain assumptions if you want to infer the effect of lockdowns on COVID-19 mortality, and how these assumptions – while not being academically incorrect, as they are readily available in their paper – can lead to misguided perceptions in the media.[30] Including the estimates from these studies and interpreting them as the effect of lockdowns would without a doubt greatly overstate the effectiveness of lockdowns.

### *Jurisdictional variance – few observations*

We also exclude studies with little jurisdictional variance.[31] For example, we exclude Conyon et al. (2020) who 'exploit policy variation between Denmark and Norway on the one hand and Sweden on the other' and, thus, only have one jurisdictional area in the control group. Although this is a difference-in-difference approach, there is a non-negligible risk that differences are caused by much more than just differences in lockdowns. (As of matter of fact, research has shown that Sweden was particularly unlucky in the spring of 2020.) Arnarson (2021) and Björk et al. (2021) show that areas in Europe – such as Sweden – where the winter holiday was relatively late (in week 9 or 10 rather than week 6, 7 or 8) were hit especially hard by COVID-19 during the first wave because the virus outbreak in the Alps could spread to those areas with ski tourists).

29 Brauner et al. (2021) state that 'our approach cannot distinguish direct effects on transmission in schools and universities from indirect effects, such as the general population behaving more cautiously after school closures signaled the gravity of the pandemic' and Hsiang et al. (2020) write that 'if increasing availability of information reduces infection growth rates, it would cause us to overstate the effectiveness of anti-contagion policies'.

30 Several scholars have criticised Flaxman et al. (2020), e.g., Homburg and Kuhbandner (2020), N. Lewis (2020), and Lemoine (2020).

31 A jurisdictional area can be countries, U.S. states, or counties. With 'jurisdictional variance' we refer to variation in mandated lockdowns across jurisdictional areas.

Another example is Wu and Wu (2020), who use all U.S. states, but pool groups of states so they end up with basically three observations. None of the excluded studies covers more than ten jurisdictional areas.[32]

### *Synthetic control studies*

The synthetic control method is a special case of the difference-in-difference method used in comparative case studies to evaluate the effect of an intervention. It involves the construction of a synthetic control that functions as the counterfactual and is constructed as an (optimal) weighted combination of a pool of donors. For example, Born et al. (2021) create a synthetic control for Sweden which consists of 30.0 per cent Denmark, 25.3 per cent Finland, 25.8 per cent Netherlands, 15.0 per cent Norway, and 3.9 per cent Sweden. The effect of the intervention is derived by comparing the actual developments to those derived through the synthetic control. We exclude synthetic control studies because of too little jurisdictional variance, as these studies examine the effects of lockdowns based on one country/state compared to a synthetic counterfactual.

But – as discussed by Bjørnskov[33] – synthetic control studies also have empirical problems in relation to studying the effect of lockdowns. Bjørnskov finds that the synthetic control version of Sweden in Born et al. (2021) deviates substantially from 'actual Sweden', when looking at the period before mid-March 2020, when Sweden decided not to lock down. He estimates that *actual Sweden* experienced approximately 500 fewer deaths in the first 11 weeks of 2020 and 4,500 fewer deaths in 2019 compared to *synthetic Sweden*.

Such empirical problems are inherent to all synthetic control studies of COVID-19 because the synthetic control should be fitted based on a long

32 One could argue that Mader and Rüttenauer (2022), who used a generalised synthetic control method (GSCM), should not have been excluded from our study as the GSCM allowed them to examine the effect of lockdowns with multiple countries in the treatment and control groups. They found no significant effect on mortality rates of SIPOs, business closures, school closures, travel restrictions, mask mandates, public transportation closures, and internal movement restrictions. In many cases, their estimates are positive (lockdowns increase mortality). However, since we specifically exclude synthetic control studies in our protocol (see Herby et al. (2021)), we did not include this study but note that it supports our conclusions.

33 Christian Bjørnskov 2021 Born et al. Om Epidemien i Sverige – Hvad Er Der Galt Og Hvordan Ser Det Ud Nu? *Punditokraterne* (blog). 14 June 2021 (http://punditokraterne.dk/2021/06/14/born-et-al-om-epidemien-i-sverige-hvad-er-der-galt-og-hvordan-ser-det-ud-nu/).

period of time before the intervention or the event one is studying the consequences of – i.e., the lockdown (see Abadie 2021). This is not possible for the coronavirus pandemic, as there clearly is no long period with coronavirus *before* the lockdown. Hence, the synthetic control method is *by design* not well suited for studying the effects of lockdowns.

In retrospect, excluding studies with little jurisdictional variance and studies that used the synthetic control method in our protocol may have been unnecessary. For example, one or two of the excluded studies could have been included in our meta-study without imposing selection bias (see Appendix IV). However, the inclusion of these studies would have not altered our meta-results substantially; in fact, their inclusion would have only strengthened, not detracted from, our conclusion. Changing a protocol after the fact is not, in our view, a sound practice, as it would only open up a Pandora's Box of potential criticisms of the original work.

### *The role of optimal timing*

One important thesis on the effect of lockdowns is that timing is important for a lockdown to be effective. The rationale behind this thesis is straightforward (assuming lockdowns are effective): If an epidemic is growing exponentially, the benefit of intervening sooner rather than later is disproportionately large. For example, if the doubling-time is one week, then locking down one week earlier will more than halve the total number of deaths, assuming that the pre-lockdown reproduction number is larger than two and *if* the lockdown brings the reproduction number below one. On the other hand, locking down too strongly and too early can result in a resurgence when restrictions are lifted if there is a failure to completely eliminate the virus, with potentially higher deaths than if it were permitted to spread to a small extent prior to the lockdown. Hence, the argument goes that there is an optimal timing for lockdowns (see e.g., Abernethy and Glass 2022 and Oraby et al. 2021).

We, however, evaluate the general effect of lockdowns, i.e., whether lockdowns on average have been effective in reducing COVID-19 mortality. We therefore exclude studies that solely analyse the effect of optimally-timed lockdowns in contrast to less well-timed lockdowns. There are several reasons for this exclusion.[34]

First, studies searching for the optimal timing of lockdowns will by design find inflated effects of the average lockdown, because they – if optimal timing is important – will neglect all the less well-timed lockdowns implemented around the world. And hence, these studies will not result in an unbiased estimate of the average effect of lockdowns.

Secondly, it is inherently difficult to differentiate between the effect of public awareness and the effect of lockdowns when looking at timing because people and politicians are likely to react to the same information. In fact, it is difficult for a democratic country's political leaders to impose and enforce a lockdown, unless there is a widespread belief that a danger is imminent.

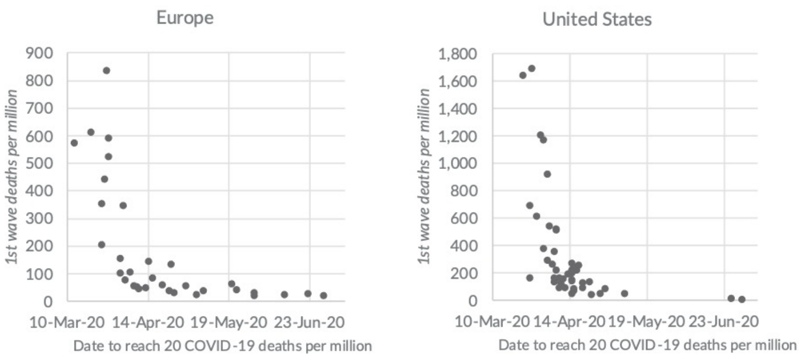
Björk et al. (2021) illustrate the difficulties in analysing the effect of timing in Europe. They find that a 10-stringency-points-stricter lockdown would reduce COVID-19 mortality by a total of 200 deaths per million[35] if done in week 11, 2020, but would only have approximately 1/3 of the effect if implemented one week earlier or later, and close to no effect if implemented three weeks earlier or later. One interpretation of this result is that lockdowns do not work if people either find them unnecessary and fail to obey the mandatory restrictions or if people voluntarily lock themselves down. This is the argument Allen (2021) uses for the ineffectiveness of the lockdowns he identifies. If this interpretation is correct, what Björk et al. (2021) find is that information and signalling are far more important than the strictness of the lockdown. There may be other interpretations, but our point is that studies focusing on timing cannot differentiate between these two conflicting interpretations.

34 This exclusion criteria was mistakenly not made public in our protocol in Herby et al. (2021), but 'only looks at timing' was decided upon as an exclusion criteria in mid-September 2021 (documentation can be provided on request). Also see Jonas Herby 2021 Hvad Betyder Timingen Af Nedlukninger? Virker Det, Hvis Man Lukker Ned Tidligt? *Punditokraterne* (blog), September 16, 2021 (https://punditokraterne.dk/2021/09/16/hvad-betyder-timingen-af-nedlukninger-virker-det-hvis-man-lukker-ned-tidligt/).

35 They estimate that 10-point higher stringency will reduce excess mortality by 20 'per week and million' in the 10 weeks from week 14 to week 23.

This view is also supported by Figure 7, which illustrates that all European countries and U.S. states that were hit hard and early by COVID-19 in the spring of 2020 experienced high overall mortality rates, whereas all countries hit relatively late experienced low mortality rates.[36] The figure shows that there is no doubt that being prepared for a pandemic and knowing when it arrives at your doorstep is vital. But to what degree this can be attributed to well-timed lockdowns or simply to alerting citizens is a question that is not easily answered and may previously have been misunderstood or neglected in prior research on, e.g., the 1918 Spanish Flu pandemic (we will get back to this issue in section 5.2.4 on p. 126).

**Figure 7**: **All countries and states that were hit late by the pandemic experienced lower COVID-19 mortality rates**




Comment: The figure shows the relationship between early pandemic strength and total first wave of COVID-19 mortality. On the X-axis is 'Date to reach 20 COVID-19 deaths per million. The Y-axis shows mortality (deaths per million) by 30 June 2020.

Source: Reported COVID-19 deaths and OxCGRT stringency for European countries and U.S. states with more than one million citizens. Data from Our World in Data (2022).

36 Equivalently, Ylli et al. (2020) find that 'mortality and incidence were strongly and inversely intercorrelated with days from January 22, respectively −0.83 ($p<0.001$) and −0.73 ($p<0.001$). Adjusting for average life expectancy and outpatients contacts per person per year, between days 33 to 50 from the 22nd of the January, the average mortality rate decreased by 30.1/million per day (95% CI: 22.7, 37.6, $p<0.001$). During interval 51 to 73 days, the change in mortality was no longer statistically significant but still showed a decreasing trend. A similar relationship with time interval was found for incidence.' They conclude that 'countries in Europe that had the earliest COVID-19 circulation suffered the worst consequences in terms of health outcomes, specifically mortality.'

We are aware of three reviews (Iezadi et al. (2021), Perra (2020), and Stephens et al. (2020)), which opine on the importance of timing. Stephens et al. (2020) find 22 studies that look at policy and timing with respect to mortality rates, however, only four were multi-country, multi-policy studies, which could possibly account for the problems described above. Stephens et al. (2020) conclude that ‘the timing of policy interventions across countries relative to the first Wuhan case, first national disease case, or first national death, is not found to be correlated with mortality.’ Iezadi et al. (2021) write that ‘it is very important to contain the spread of the infection at the very early stage of the outbreak. At later stages, no NPIs, even if implemented harshly, might be very effective’, while Perra (2020) writes that ‘countries that acted early, with respect to the local spread, were most successful in controlling the spread and reported markedly lower death tolls’. But these three reviews do not distinguish between the effect of information (which is the effect of being hit late by the pandemic) and the effect of lockdowns.

Although the verdict on optimal timing is still out, we would like to stress the importance of alternative interpretations here. As Figure 7 illustrates, one can easily interpret the lower mortality rates as an effect of early lockdowns even when they are caused by changes in voluntary behaviour or – not unlikely – by a combination of both. One should be careful concluding that early lockdown is important when alternative conclusions – such as changing information and voluntary behaviour – may explain outcomes equally well.

Even if future research finds that the timing of lockdowns *is* crucial, such knowledge may not be useful for future policymakers.

First, it is not easy to know when the right timing is. When COVID-19 hit Europe and the United States, it was virtually impossible to determine the right timing. The World Health Organization declared the COVID-19 pandemic a Public Health Emergency of International Concern (PHEIC) on 30 January 2020, WHO (2020a). However, this was the sixth PHEIC in just 11 years, and it could not reasonably justify a lockdown.[37] The first time the WHO characterised COVID-19 as a pandemic was on 11 March 2020 (WHO 2020b). But at that date, Italy had already registered 13.7 COVID-19 deaths per million. On 29 March 2020, 18 days after the WHO

37 Wilder-Smith and Osman (2020) state that ‘Six events were declared PHEIC between 2007 and 2020: the 2009 H1N1 influenza pandemic, Ebola (West African outbreak 2013-2015, outbreak in Democratic Republic of Congo 2018-2020), poliomyelitis (2014 to present), Zika (2016) and COVID-19 (2020 to present).’

declared the outbreak of a pandemic and the earliest date that a lockdown response to the WHO's announcement could potentially have a large effect due to the lag between infection and death, the mortality rate in Italy was a staggering 178 COVID-19 deaths per million, with an additional 13 per million dying each day.[38]

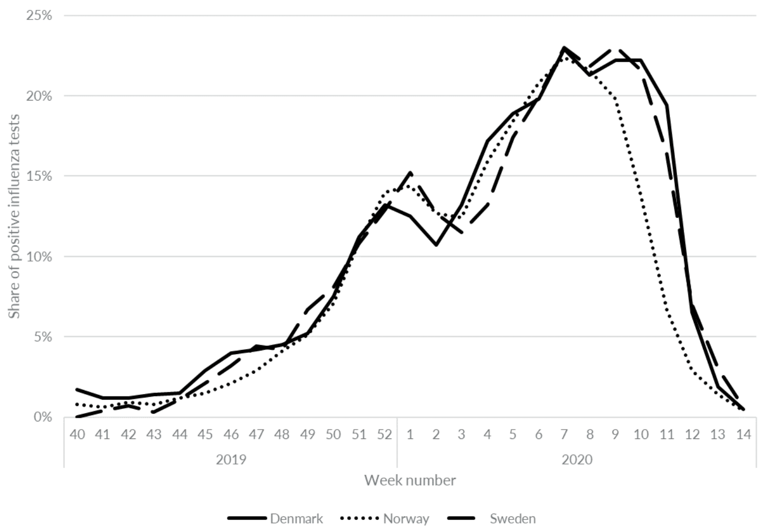
Second, we already pointed to the fact that policymakers (at least in democratic countries) need support from the electorate to impose and enforce lockdowns. So even if a few people know the right time to impose a lockdown, this information is only useful if citizens and politicians agree that there is a dangerous and threatening infectious disease and act upon that threat.[39] But under these conditions, they are also likely to respond significantly (and voluntarily) to recommendations, making a lockdown less necessary.

In fact, data from the influenza surveillance programme in Denmark from Statens Serum Institut (2020) show that the influenza vanished *before* lockdowns were implemented but possibly *coinciding with* the announcement of coming lockdowns, which spurred significant voluntary behavioural changes. However, the influenza vanished at the exact same time in Norway and Sweden, as illustrated in Figure 8, suggesting that *if* lockdowns spurred significant voluntary behavioural changes, the Swedish 500-person limit on public gatherings effective by 12 March 2020, may have been sufficient to spur these changes.

38 There's approximately a three- to four-week lag between infection and deaths. See footnote 47.

39 In his book *The Premonition: A Pandemic Story*, M. Lewis (2021) describes how White House experts wanted to close schools in the United States at the beginning of the 2009 swine flu, but did not have the necessary support to implement what the group thought was the right decision. It turned out that closing schools would in fact have been a mistake. Later, in early 2020, the same group again wanted to close schools, etc. Again, they did not have the necessary political support, but this time it remains unknown if it could potentially have saved a significant number of lives.

**Figure 8**: **Influenza disappeared at the same time in Denmark, Norway, and Sweden in March 2020 despite radical differences in lockdown policies**




Source: Data from Emborg et al. (2021)

Note: In Sweden, gatherings were limited to 500 persons from Thursday, 12 March 2020 (week 11), while high schools and higher education was closed from Wednesday, 18 March 2020 (week 12).

We conclude that most – if not all – studies focusing on timing fail to distinguish between the effects of lockdowns and the effects of voluntary behavioural changes.

# 3. The empirical evidence

In this section we present the empirical evidence found through our identification process. We describe the eligible studies and their results. In addition, we comment on the methodology and possible identification problems and biases.

### *3.1. Preliminary considerations*

Before we turn to the eligible studies, we present some considerations that we adopted when interpreting the empirical evidence.

#### ***Our interpretation and conclusions are based solely on the empirical findings contained in the studies we reviewed***

While the policy conclusions in some studies are based on statistically significant results, many of these conclusions are ill-founded due to the little impact associated with the statistically significant results. For example, Ashraf (2020) states that ‘social distancing measures has proved effective in controlling the spread of [a] highly contagious virus.’ However, their estimates show that the average lockdown in Europe and the U.S. only reduced COVID-19 mortality by 2.4 per cent.[40]

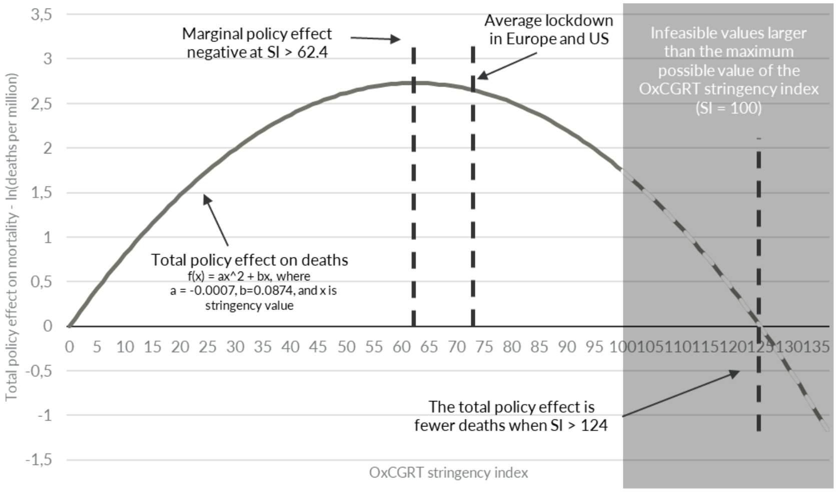
Another example is Chisadza et al. (2021), where the authors argue that ‘less stringent interventions increase the number of deaths, whereas more severe responses to the pandemic can lower fatalities.’ Their conclusion is based on a negative estimate for the squared term of *stringency*, which results in a total negative effect on mortality rates (i.e., fewer deaths) for stringency values larger than 124. This means that for lockdowns with a

40 We describe how we arrive at the 2.4 per cent in Section 4.

stringency of at least 124, the lockdown theoretically reduces mortality. However, the stringency index is limited to values between 0 and 100 by design, so for all possible values of the stringency index, the total effect of lockdowns on mortality is positive (more deaths), so Chisadza et al.'s conclusion is infeasible.

This is illustrated in Figure 9 below. The figure describes the total policy effect based on Chisadza et al. (2021) estimates for their squared specification. Starting from a lockdown with a stringency of 0 (no lockdown) and increasing stringency from there, a stricter lockdown increases mortality. But, at stringency 62.4, a stricter lockdown reduces mortality at the margin. However, the total effect is still an increase in mortality for stringency values below 124. And because stringency values are capped at 100, there are no lockdowns that decrease mortality overall.

**Figure 9: The total policy effect, including infeasible values outside the range of the OxCGRT stringency index, as estimated by Chisadza et al. (2021)**




Source: Chisadza et al. (2021)

Note: SI = OxCGRT stringency index. The OxCGRT stringency index measures the stringency of lockdowns on a scale from 0 to 100, where a higher value means stricter lockdowns.

Again, to avoid any such biases, we base our interpretations solely on the empirical estimates and not on the authors' own interpretation of their results.

### *Handling multiple models, specifications, and uncertainties*

Several studies adopt a number of models to understand the effect of lockdowns. For example, Bjørnskov (2021) estimates the effect after one, two, three, and four weeks of lockdowns. For these studies, we select the longest time horizon analysed to obtain the estimate closest to the long-term effect of lockdowns.

Several studies also use multiple specifications, including and excluding potentially relevant variables. For these studies, we choose the model that the authors regard as their main specification.

Finally, some studies have multiple models which the authors regard as equally important. One interesting example is Chernozhukov et al. (2021), who estimate two models with and without national case numbers as a variable. They show that including this variable in their model substantially reduces the efficacy of lockdowns on mortality. The explanation could be that people responded to information about national conditions. For these studies, we present both estimates in Table 2, but – following Doucouliagos and Paldam (2008) – we use an average of the estimates in our meta-analysis to avoid giving more weight to a study with multiple models relative to studies with just one principal model (for one study – Chernozhukov et al. (2021) – we can only include the model estimating large effects of lockdowns, because they report the counterfactual effect for this model only – see Table 20).

For studies that look at different classes of countries (e.g., rich and poor), we report both estimates in Table 2 but use the estimate for rich Western countries in our meta-analysis, where we derive standardised estimates for Europe and the United States.

### *Effects are measured relative to 'doing the least in the spring of 2020'*

Virtually all countries in the world implemented some kind of lockdown in response to the COVID-19 pandemic. Hence, most estimates are relative to 'doing the least', which in many Western countries means relative to doing as Sweden did during the first wave, when Sweden, due to constitutional constraints (see Jonung and Hanke 2020; Jonung 2020), implemented very few restrictions compared to other Western countries. However, some studies state that they *do* compare the effect of doing something to the effect of doing absolutely nothing (e.g., Bonardi et al. 2020).

The consequence is that some estimates are relative to 'doing the least' while others are relative to 'doing nothing'. This may lead to biases if 'doing the least' works as a signal (or warning) that alters the behaviour of the public. For example, Gupta et al. (2020) find a large effect of emergency declarations, which they argue 'are best viewed as an information instrument that signals to the population that the public health situation is serious and they act accordingly', on social distancing but not of other policies such as SIPOs. Thus, if we compare a country issuing a SIPO to a country doing nothing, we may overestimate the effect of a SIPO because it is the sum of the signal *and* the SIPO. Instead, we should compare the country issuing the SIPO to a country 'doing the least' to estimate the *marginal* effect of the SIPO.

To take an example, Bonardi et al. (2020) find relatively large effects of doing *something* but no effect of doing *more*. They find no extra effect of stricter lockdowns relative to less strict lockdowns and state that 'our results point to the fact that people might adjust their behaviors quite significantly as partial measures are implemented, which might be enough to stop the spread of the virus'. Hence, whether the baseline is 'doing the least', or 'doing nothing' can affect the magnitude of the estimated impacts. There is no obvious right way to resolve this issue, but since estimates in most studies are relative to doing less, we report results as compared to 'doing the least' when available. Hence, for Bonardi et al. (2020), we state that the effect of lockdowns is zero (compared to 'doing the least').

This also means that our results cannot say much about the importance of *signalling*. One could imagine that lockdown may serve as a signal to citizens that now is the time to be careful. If signalling is thought to be important, future research should focus on finding the least costly signals.

### *3.2. Overview of the findings of the eligible studies*

Table 2 covers the 32 studies eligible for our review.[41] Out of these 32 studies, nineteen were peer-reviewed and thirteen were working papers. The studies analyse lockdowns during the first wave. Most of the studies (25) use data collected before 1 September 2020, and six use data collected before 1 May 2020. Only two studies use data collected after 1 January 2021. All studies are cross-sectional, ranging across jurisdictions. Geographically, fifteen studies cover countries worldwide, two cover European countries, thirteen cover the United States, one covers Europe and the United States, and one covers the OECD member countries. Seven studies analyse the effect of SIPOs, eleven studies analyse the effect of stricter lockdowns (measured by the OxCGRT stringency index), thirteen studies analyse specific NPIs independently, and one study analyses other measures.[42]

Several studies find no statistically significant effect of lockdowns on mortality. This includes Bjørnskov (2021) and Goldstein et al. (2021), who find no significant effect of stricter lockdowns (a higher OxCGRT stringency index), Sears et al. (2020) and Dave et al. (2021), who find no significant effect of SIPOs, and An et al. (2021) and Guo et al. (2021) who find no significant negative (fewer deaths) effect of any of the analysed NPIs, including business closures, school closures, and border closures.

Other studies find a significant *negative* relationship between lockdowns and mortality. Fowler et al. (2021) conclude that SIPOs reduce COVID-19 mortality by 35 per cent, while Chernozhukov et al. (2021) state that employee mask mandates reduce mortality by 34 per cent and closing businesses and bars reduces mortality by 29 per cent.

A few studies find a significant *positive* relationship between lockdowns and mortality. This includes Chisadza et al. (2021), who find that stricter lockdowns (higher OxCGRT stringency index) increase COVID-19 mortality, and Berry et al. (2021), who find that SIPOs increase COVID-19 mortality by 1 per cent after 14 days.

Most studies use the number of official COVID-19 deaths as the dependent variable. Only one study, Bjørnskov (2021), looks at total excess mortality,

41 The following numbers are based on data presented in Table 4.

42 Yue Li et al. (2021) use 'data on the stringency of state social distancing measures from wallethub.com'.

which we believe to be the best (albeit still imperfect) measure, as it overcomes the measurement problems related to proper reporting of COVID-19 deaths.

## Table 2: Summary of eligible studies

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Alderman and Harjoto (2020); 'COVID-19: U.S. shelter-in-place orders and demographic characteristics linked to cases, mortality, and recovery rates' | COVID-19 mortality | The study uses state-level data from all U.S. states published by the COVID-19 Tracking Project, and a multivariate regression analysis to empirically investigate the impacts of the duration of shelter-in-place orders on mortality. | The study finds that shelter-in-place orders are – for the average duration – associated with 1% (insignificant) fewer deaths per capita. | The study does not use a stringent difference-in-difference approach; but this approach is included as the study uses panel data with a time dimension. |
| An et al. (2021); 'Policy design for COVID-19: worldwide evidence on the efficacies of early mask mandates and other policy interventions' | COVID-19 deaths | The authors use 164 countries to estimate the long-run efficacy of early mandate adoption. They use both a country fixed-effects model and a country random-effects model. They also use several other methods in order to estimate the specific NPIs' effect on infection rates and mortality, and they look specifically at timing for each NPI. | With different modelling approaches and alternative measurements (for both the focal independent and dependent variables), the analysis shows that domestic lockdowns and restaurant closures do not display any consistent associations with new infection and mortality rates in the short term. Mass gathering bans and school closures need more time to manifest their short-term efficacies. Both cross-sectional and longitudinal analyses provide consistent evidence that only mask mandates demonstrate persistent long-run efficacy from early adoption. | The study uses several different methods to estimate how effective timing and specific NPIs are and finds that the only NPI that shows consistent reduction of infection rates and mortality both in the short and long term is mask mandates. |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
| --- | --- | --- | --- | --- |
| Ashraf (2020); 'Socioeconomic conditions, government interventions and health outcomes during COVID-19' | COVID-19 mortality | The study's focus is on the effectiveness of policies targeted to diminish the effect of socioeconomic inequalities (economic support) on COVID-19 deaths. The study uses data from 80 countries worldwide and includes the OxCGRT stringency as a control variable in its models. The paper finds a significant negative (fewer deaths) effect of stricter lockdowns. The effect of lockdowns becomes insignificant when the author includes an interaction term between the socioeconomic conditions index and the economic support index in the study's model. | For each 1-unit increase in OxCGRT stringency index, the cumulative mortality changes by –0.326 deaths per million (fewer deaths). The estimate is –0.073 deaths per million but becomes insignificant when including an interaction term between the socioeconomic conditions index and the economic support index. | |
| Berry et al. (2021); 'Evaluating the effects of shelter-in-place policies during the COVID-19 pandemic' | COVID-19 mortality | The authors use U.S. county data on COVID-19 deaths from Johns Hopkins University and SIPO data from the University of Washington to estimate the effect of SIPOs. They find no detectable effects of SIPOs on deaths. The authors stress that their findings should not be interpreted as evidence that social distancing behaviours are not effective. Many people had already changed their behaviours before the introduction of shelter-in-place orders, and shelter-in-place orders appear to have been ineffective precisely because they did not meaningfully alter social distancing behaviour. | A SIPO increases the number of deaths by 0,654 per million after 14 days (see Fig. 2). | The authors conclude that 'We do not find detectable effects of these policies [SIPO] on disease spread or deaths.' However, this statement does not correspond to their results. In Figure 2 they show that the effect on deaths is significant after 14 days. The paper looks at the effect 14 days after SIPOs are implemented, which is a short lag given that the time between infection and deaths is at least 2-3 weeks. |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Bjørnskov (2021); ‘Did lockdown work? An economist’s cross-country comparison’ | Excess mortality | The study uses excess mortality and OxCGRT stringency data from 24 European countries to estimate the effect of lockdowns on the number of deaths in the subsequent one, two, three and four weeks. | A stricter lockdown (OxCGRT stringency) does not have a significant effect on excess mortality. Specifications using instrument variables yields similar results. | The study finds a positive (more deaths) effect after one and two weeks, which could indicate that other factors (omitted variables) affect the results. |
| Blanco et al. (2020); ‘Do coronavirus containment measures work? Worldwide evidence’ | COVID-19 mortality | The study uses data for deaths and NPIs from Hale et al. (2020) covering 158 countries between January and August 2020 to evaluate the effect of eight different NPIs: stay-at-home orders, bans on gatherings, bans on public events, school closures, lockdowns of workplaces, interruption of public transportation services, and international border closures. They address the possible endogeneity of the NPIs by using instrumental variables. | When using the naïve dummy variable approach, all parameters are statistically insignificant. On the contrary, estimates using the instrumental variable approach indicate that NPIs are effective in reducing the growth rate in the daily number of deaths 14 days later. | The study runs the same model four times for each of the different NPIs (stay-at-home orders, bans on meetings, bans on public events, and mobility restrictions). These NPIs were often introduced almost simultaneously so there is a high risk of multicollinearity with each run capturing the same underlying effect. Indeed, the size and standard errors of the estimates are worryingly similar. The study looks at the effect 14 days after NPIs are implemented, which is a fairly short lag given that the time between infection and deaths is 2-3 weeks; see e.g., Flaxman et al. (2020), which according to Bjørnskov (2021) appears to be the minimum typical time from infection to death. |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Bonardi et al. (2020); 'Fast and local: How did lockdown policies affect the spread and severity of the Covid-19' | Growth rates | The study uses NPI data collected from news headlines from LexisNexis and death data from Johns Hopkins University up to 1 April 2020 in a panel structure with 184 countries. The study controls for country fixed-effects, day fixed-effects, and within-country evolution of the disease. | The study finds that certain interventions (SIPO, regional lockdown and partial lockdown) work (in developed countries), but that stricter interventions (SIPOs) do not have a larger effect than less strict interventions (e.g., restrictions on gatherings). It finds no effect of border closures. | The study finds a positive (more deaths) effect on day 1 after lockdown which may indicate that their results are driven by other factors (omitted variables). We rely on their publicly available version submitted to CEPR *Covid Economics*, but estimates on the effect of deaths can be found in Supplementary material, which is available in an updated version hosted on the Danish Broadcasting Corporation's webpage: https://www.dr.dk/static/documents/2021/03/04/managing_pandemics_e3911c11.pdf |
| Chernozhukov et al. (2021); 'Causal impact of masks, policies, behavior on early Covid-19 pandemic in the U.S.' | Growth rates | The study uses COVID deaths from the *New York Times*, Johns Hopkins University, and data on U.S. states' policies from Raifman et al. (2020) to estimate the effect of SIPOs, closed non-essential businesses, closed K-12 schools, closed restaurants except takeout, closed movie theatres, and face mask mandates for employees in public facing businesses. | The study finds that mandatory masks for employees and closing K-12 schools reduces deaths. SIPO and closing business (average of closed businesses, restaurants and movie theatres) have no statistically significant effect. The effect of school closures is highly sensitive to the inclusion of national case and death data. | The authors state that 'our regression specification for case and death growths is explicitly guided by a SIR model although our causal approach does not hinge on the validity of a SIR model.' We are uncertain if this means that data are managed to fit a SIR model (and thus should fail our eligibility criteria). |
| Chisadza et al. (2021); 'Government effectiveness and the COVID-19 pandemic' | COVID-19 mortality | The study uses COVID-19-deaths and OxCGRT stringency from 144 countries to estimate the effect of lockdowns on the number of COVID-19-deaths. It finds a significant positive (more deaths) non-linear association between government response indices and the number of deaths. | In the authors' linear model, an increase by 1 on 'stringency index' increases the number of log deaths by 0.0130 per million (corresponding to 1.3%). In their non-linear model, the sign of the squared term is negative, but the combined non-linear estimate is positive (increases deaths) and larger than the linear estimate for all values of the OxCGRT stringency index. | The authors state that 'less stringent interventions increase the number of deaths, whereas more severe responses to the pandemic can lower fatalities.' However, according to their estimates this is not correct, as the combined non-linear estimate cannot be negative for relevant values of the OxCGRT stringency index (0 to 100). |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Clyde et al. (2021); 'A Study of the effectiveness of governmental strategies for managing mortality from COVID-19' | COVID-19 mortality | The study uses data for NPIs in 37 OECD countries from the OxCGRT to examine the impact of policy variable changes between 15 March and 31 October 2020. | Results indicate that only school closings and public transportation closings have a persistently significant impact. Stay-at-home policies only show a significant impact after 70 days. Workplace closings, restrictions on the size of gatherings, and restrictions on internal travel show no significant impact on mortality rates. Moreover, stricter measures are not significantly associated with lower growth rates in mortality. | |
| Dave et al. (2021); 'When do shelter-in-place orders fight Covid-19 best? Policy heterogeneity across states and adoption time' | COVID-19 mortality | The study uses smartphone location tracking and state data on COVID-19 deaths and SIPO data (supplemented by their own searches) collected by the *New York Times* to estimate the effect of SIPOs. The authors find that a SIPO was associated with a 9%–10% increase in the rate at which state residents remained in their homes full-time, but overall, they do not find a significant effect on mortality after 20+ days (see Figure 4). They indicate that the lacking significance may be due to long-term estimates being identified of a few early adopting states. | The study finds no overall significant effect of a SIPO on deaths but does find a negative effect (fewer deaths) in early adopting states. | The study finds large effects of a SIPO on deaths after 6-14 days in early adopting states (see Table 8), which is before a SIPO-related effect would be seen. This could indicate that other factors rather than SIPOs drive the results. |
| Dergiades et al. (2022); 'Effectiveness of government policies in response to the COVID-19 outbreak' | COVID-19 mortality | The study uses daily deaths from the European Centre for Disease Prevention and Control and OxCGRT stringency from 32 countries worldwide (including U.S.) to estimate the effect of lockdowns on the number of deaths. | The study finds that the greater the strength of government interventions at an early stage, the more effective these are in slowing down or reversing the growth rate of deaths. | The focus is on the effect of early stage NPIs and thus does not absolutely live up to our eligibility criteria. However, we include the study as it differentiates between lockdown strength at an early stage. |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Ertem et al. (2021); ‘The impact of school opening model on SARS-CoV-2 community incidence and mortality’ | COVID-19 deaths | The study uses 459 U.S. counties to estimate the impact of school opening on mortality. It uses multivariate Poisson regressions with robust standard errors. The authors define three school modes, as they focus on the difference between traditional and virtual. | ‘There was no impact of school opening mode on subsequent COVID-19-related deaths during the entire 12-week period after school opening in any region (Fig. 4 and Extended Data Fig. 2).’ The authors also conclude that there are major limitations because the underlying reason for regional differences could not be delineated. | |
| Fakir and Bharati (2021); ‘Pandemic catch-22: The role of mobility restrictions and institutional inequalities in halting the spread of COVID-19’ | COVID-19 mortality | The study uses data from 127 countries, combining high-frequency measures of mobility data from Google’s daily mobility reports, country-date-level information on the stringency of restrictions in response to the pandemic from OxCGRT, and daily data on deaths attributed to COVID-19 from Our World in Data and Johns Hopkins University. The study instruments stringency using day-to-day changes in the stringency of the restrictions in the rest of the world. | The study finds large causal effects of stricter restrictions on the weekly growth rate of recorded deaths attributed to COVID-19. It shows that more stringent interventions help more in richer, more educated, more democratic, and less corrupt countries with older, healthier populations and more effective governments. | The authors find a larger effect on deaths after 0 days than after 14 and 21 days (Table 3). This is surprising given that it takes 2-3 weeks from infection to death, and it may indicate that their results are driven by other factors. |
| Fowler et al. (2021); ‘Stay-at-home orders associate with subsequent decreases in COVID-19 cases and fatalities in the United States’ | COVID-19 mortality | The study uses U.S. county data on COVID-19 deaths and SIPO data collected by the *New York Times* to estimate the effect of SIPO’s using a two-way fixed-effects difference-in-differences model. It finds a large and early (after few days) effect of SIPOs on COVID-19 related deaths. | Stay-at-home orders are also associated with a 59.8 per cent (18.3 to 80.2) average reduction in weekly fatalities after three weeks. These results suggest that stay-at-home orders might have reduced confirmed cases by 390,000 (the 95 per cent confidence interval spans from 170,000 to 680,000) and fatalities by 41,000 (from 27,000 to 59,000) within the first three weeks in localities that implemented stay-at-home orders. | The study finds the largest effect of SIPOs on deaths after 10 days (see Figure 4), before a SIPO-related effect could possibly be seen as it takes 2-3 weeks from infection to death. This could indicate that other factors drive their results. |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Fuller et al. (2021); 'Mitigation policies and COVID-19–Associated mortality – 37 European countries, January 23–June 30, 2020' | COVID-19 mortality | The study uses COVID 19-deaths and OxCGRT stringency in 37 European countries to estimate the effect of lockdowns on the number of COVID-19 deaths. It finds a significant negative (fewer deaths) effect of stricter lockdowns after the mortality threshold is reached. The threshold is a daily rate of 0.02 new COVID-19 deaths per 100,000 population, based on a 7-day moving average. | For each 1-unit increase in OxCGRT stringency index, the cumulative mortality decreases by 0.55 deaths per 100,000. | |
| Gibson (2020); 'Government mandated lockdowns do not reduce Covid-19 deaths: implications for evaluating the stringent New Zealand response' | COVID-19 mortality | The study uses data for every county in the United States from March through 1 June 2020, to estimate the effect of SIPOs (called 'lockdowns') on COVID-19 mortality. Policy data are acquired from American Red Cross reporting on emergency regulations. The author's control variables include county population and density, the elder share, the share in nursing homes, nine other demographic and economic characteristics and a set of regional fixed-effects. The author handles causality problems using instrument variables (IV). | The author finds no statistically significant effect of SIPOs. | The author uses the word 'lockdown' as synonym for SIPO (writes 'technically, government-ordered community quarantine') |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Goldstein et al. (2021); 'Lockdown fatigue: The diminishing effects of quarantines on the spread of COVID-19 ' | COVID-19 mortality | The study uses panel data from 152 countries with data from the onset of the pandemic until 31 December 2020. It finds that lockdowns tend to reduce the number of COVID-19 related deaths, but also that this benign impact declines over time: after four months of strict lockdown, NPIs have a significantly weaker contribution in terms of their effect in reducing COVID-19 related fatalities. | Stricter lockdowns reduce deaths for the first 60 days, whereafter the cumulative effect begins to decrease. If reintroduced after 120 days, the effect of lockdowns is smaller in the short run, but after 90 days the effect is almost the same as during the first lockdown (only approximately 10 per cent lower). | There is little documentation in the study (e.g., no tables with estimates). |
| Guo et al. (2021); 'Mitigation Interventions in the United States: An exploratory investigation of determinants and impacts' | COVID-19 mortality | The study uses policy data of 1,470 executive orders from the state–government websites for all 50 U.S. states and Washington D.C. and COVID-19 deaths from Johns Hopkins University in a random-effect spatial error panel model to estimate the effect on COVID-19 deaths of nine NPIs: SIPO, strengthened SIPO, public school closure, all school closure, large-gathering ban of more than ten people, any gathering ban, restaurant/bar limit to dining out only, non-essential business closure, and mandatory self-quarantine of travellers. | Two mitigation strategies (all school closure and mandatory self-quarantine of travellers) showed a positive (more deaths) impact on COVID-19 deaths per 10,000. Six mitigation strategies (SIPO, public school closure, large-gathering bans [>10], any gathering ban, restaurant/bar limit to dining out only, and non-essential business closure) did not show any impact (Table 3, Proportion of Cumulative Deaths Over the Population). | The study only concludes on NPIs which reduce mortality. However, the conclusion is based on one-tailed tests, which means that all positive estimates (more deaths) are deemed insignificant. Thus, in their mortality-specification (Table 3, Proportion of Cumulative Deaths Over the Population), the estimate of all school closures (.204) and mandatory self-quarantine of travellers (0.363) is deemed insignificant based on schools CI [.029, .379] and quarantine CI [.193, .532]. We believe these results should be interpreted as a significant increase in mortality and that these results should have been part of their conclusion. |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Hale, Hale, et al. (2020); 'Global assessment of the relationship between government response measures and COVID-19 deaths' | COVID-19 mortality | The study uses the OxCGRT stringency and COVID-19-deaths from the European Centre for Disease Prevention and Control for 170 countries. It estimates both cross-sectional models in which countries are the unit of analysis, as well as longitudinal models on time-series panel data with country-day as the unit of analysis (including models that use both time and country fixed-effects). | The study finds that higher stringency in the past leads to a lower growth rate in the present, with each additional point of stringency corresponding to a 0.039%-point reduction in daily deaths growth rates six weeks later. | |
| Hale et al. (2021); 'Government responses and COVID-19 deaths: Global evidence across multiple pandemic waves' | COVID-19 mortality | The study uses the OxCGRT stringency to analyse the effect of stricter lockdowns in 113 countries through 1-3 waves. | The study finds that stricter lockdowns reduce mortality and that the effect was particularly large – even during the first wave – in countries which experienced three waves. | The authors' results on three wave countries are based on just ten countries. However, since there is no obvious reason why lockdowns in countries, which later experienced a third wave, should be particularly effective, this could indicate that some model misspecification is driving the results. |
| Leffler et al. (2020); 'Association of country-wide coronavirus mortality with demographics, testing, lockdowns, and public wearing of masks' | COVID-19 mortality | The study uses COVID-19 deaths from Worldometer and info about NPIs (mask/mask recommendations, international travel restrictions and lockdowns [defined as any closure of schools or workplaces, limits on public gatherings or internal movement, or stay-at-home orders]) from Hale et al. (2020) for 200 countries to estimate the effect of the duration of NPIs on the number of deaths. | The study finds that masking (mask recommendations) reduces mortality. For each week that masks were recommended the increase in per-capita mortality was 8.1% (compared to 55.7% increase when masks were not recommended). It finds no significant effect of the number of weeks with internal lockdowns and international travel restrictions (Table 2). | The authors' 'mask recommendation' category includes some countries where masks were mandated (see Supplemental Table A1) and may (partially) capture the effect of mask mandates. The study looks at duration, which may cause a causality problem, because politicians may be less likely to ease restrictions when there are many cases or deaths. |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Li et al. (2021); ‘Association of state social distancing restrictions with nursing home COVID-19 and non-COVID-19 outcomes’ | COVID-19 mortality | The study uses data on the stringency of state social distancing measures from wallethub.com (based on seventeen state COVID-19 policy metrics) to categorise U.S. states into either ‘high’ or ‘low’ stringency. The study then uses linear regression to estimate cumulative numbers of deaths among residents in long-term care in low and high stringency states based on mortality data from 14,046 nursing homes. | The study finds that stricter lockdowns reduced COVID-19 mortality and across-facility disparities in COVID-19 outcomes, but also caused more deaths due to non-COVID reasons among nursing home residents. | |
| Mccafferty and Ashley (2021); ‘Covid-19 social distancing interventions by statutory mandate and their observational correlation to mortality in the United States and Europe’ | Other | The study uses data from 27 U.S. states and twelve European countries to analyse the effect of NPIs on peak mortality rate using general linear mixed-effects modelling. | The study finds that no mandate (school closures, prohibition on mass gatherings, business closures, stay-at-home orders, severe travel restrictions, and closure of non-essential businesses) was effective in reducing the peak COVID-19 mortality rate. | |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Pan et al. (2020); 'Covid-19: Effectiveness of non-pharmaceutical interventions in the United States before phased removal of social distancing protections varies by region' | COVID-19 mortality | The study uses county-level data for all U.S. states. Mortality data are obtained from Johns Hopkins University, while policy data are obtained from official governmental websites. It categorises twelve policies into four levels of disease control; Level 1 (low) - State of Emergency; Level 2 (moderate) - school closures, restricting access (visits) to nursing homes, or closing restaurants and bars; Level 3 (high) – non-essential business closures, suspending non-violent arrests, suspending elective medical procedures, suspending evictions, or restricting mass gatherings of at least ten people; and Level 4 (aggressive) – shelter in place or stay at home, public mask requirements, or travel restrictions. It uses a stepped-wedge cluster-randomised trial (SW-CRT) for clustering and negative binomial mixed model regression. | The study concludes that only (duration of, see comment in next column) level 4 restrictions are associated with reduced risk of death, with an average 15 per cent decline in the COVID-19 death rate per day. Implementation of level 3 and level 2 restrictions increased death rates in 6 of 6 regions, while longer duration increased death rates in 5 of 6 regions. | The authors focus on the negative estimate of duration of Level 4. However, their implementation estimate is large and positive, and the combined effect of implementation and duration is unclear. |
| Pincombe et al. (2021); 'The effectiveness of national-level containment and closure policies across income levels during the COVID-19 pandemic: an analysis of 113 countries' | COVID-19 mortality | The study uses daily data for 113 countries on cumulative COVID-19 death counts over 130 days between 15 February 2020 and 23 June 2020, to examine changes in mortality growth rates across the World Bank's income group classifications following shelter-in-place recommendations or orders (the authors use one variable covering both recommendations and orders). | The study finds that shelter-in-place recommendations or orders reduces mortality growth rates in high-income countries (although insignificant) but increases growth rates in countries in other income groups. | |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Sears et al. (2020); 'Are we #stayinghome to flatten the curve?' | COVID-19 mortality | The study uses cellular location data from all 50 states and the District of Columbia to investigate mobility patterns during the pandemic across states and time. The authors estimate the effect of stay-at-home policies on COVID-19 mortality by adding COVID-19 death tolls and the timing of SIPOs for each state.<br><br>'We obtain travel activity and social distancing data from the analytics company Unacast.'<br><br>'To denote periods before or after a state implemented a "stay at home order", we obtain the date each statewide policy was issued [45] for all 50 states and the District of Columbia' – from the *New York Times*. | The study finds that SIPOs lower deaths by 0.13-0.17 per 100,000 residents, equivalent to death rates 29-35 per cent lower than in the absence of policies. However, these estimates are insignificant at a 95 per cent confidence interval (see Table 4). The study also finds reductions in activity levels prior to mandates. The human encounter rate fell by 63 percentage points and non-essential visits by 39 percentage points relative to pre-COVID-19 levels, prior to any state implementing a statewide mandate. | In the abstract the authors state that death rates would be 42-54 per cent lower than in the absence of policies. However, this includes averted deaths due to pre-mandate social distancing behaviour (p. 6). The effect of a SIPO is a reduction in deaths by 29-35 per cent compared to a situation without a SIPO but with pre-mandate social distancing. These estimates are insignificant at a 95 per cent confidence interval. |
| Shiva and Molana (2021); 'The luxury of lockdown' | COVID-19 mortality | The study uses COVID-19 deaths and OxCGRT stringency from 169 countries to estimate the effect of a lockdown on the number of deaths 1-8 weeks later. It finds that stricter lockdowns reduce COVID-19 deaths 4 weeks later (but insignificantly after 8 weeks) and have the greatest effect in high-income countries. It finds no effect of workplace closures in low-income countries. | A stricter lockdown (1 stringency point) reduces deaths by 0,1 per cent after 4 weeks. After 8 weeks the effect is insignificant. | |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Spiegel and Tookes (2022); 'All or Nothing? Partial business shutdowns and COVID-19 fatality growth' | Growth rates | The study uses hand-collected policy data for all U.S. counties from March through December 2020 to estimate the effect of capacity limits on spas, bars, restaurants, and gyms. | The study finds mixed results, where partial capacity restrictions on restaurants and bars are more effective than full shutdowns, and full shutdowns of gyms reduce fatality growth rate, while partial capacity restrictions are counterproductive. | |
| Spiegel and Tookes (2021); 'Business restrictions and Covid-19 fatalities' | COVID-19 mortality | The study uses data for every county in the United States from March through December 2020 to estimate the effect of various NPIs on the COVID-19 deaths growth rate. Derives causality by 1) assuming that state regulators primarily focus on the state's most populous counties, so state regulation in smaller counties can be viewed as a quasi-randomised experiment, and 2) conducting county pair analysis, where similar counties in different states (and subject to different state policies) are compared. | The study finds that some interventions (e.g., mask mandates, restaurant and bar closures, gym closures, and high-risk business closures) reduces mortality growth, while other interventions (closures of low- to medium-risk businesses and personal care/spa services) did not have an effect and may even have increased the number of deaths. | In total the authors analyse the lockdown effect of 21 variables. Fourteen of 21 estimates are significant, and of these 6 are negative (reduces deaths) while eight are positive (increases deaths). Some results are not readily intuitive; e.g., mask recommendations increase deaths by 48 per cent while mask mandates reduce deaths by 12 per cent, and closing restaurants and bars reduces deaths by 50 per cent, while closing bars but not restaurants only reduces deaths by 5 per cent, although the latter could potentially be explained by more crowding in open venues. They handle early effect (within 1-2 weeks) in Table 5. |

| 1. Study (Author & title) | 2. Measure | 3. Description | 4. Results | 5. Comments |
|---|---|---|---|---|
| Stokes et al. (2020); 'The relative effects of non-pharmaceutical interventions on early Covid-19 mortality: natural experiment in 130 countries' | COVID-19 mortality | The study uses daily COVID-19 deaths for 130 countries from the European Centre for Disease Prevention and Control (ECDC) and daily policy data from the OxCGRT. It looks at all levels of restrictions for each of the nine sub-categories of the OxCGRT stringency index: school, work, events, gatherings, transport, SIPO, internal movement, and travel, as well as Public information campaigns. . | Of the nine sub-categories in the OxCGRT stringency index, only travel restrictions are consistently significant (with level 2 'Quarantine arrivals from high-risk regions' having the largest effect, and the strictest level 4 'Total border closure' having the smallest effect). Restrictions on very large gatherings (>1,000) have a large significant negative (fewer deaths) effect, while the effect of stricter restrictions on gatherings are insignificant. The authors recommend that the closing of schools (level 1) has a very large (in absolute terms it is twice the effect of border quarantines) positive effect (more deaths) while stricter interventions on schools have no significant effect. Required cancelling of public events also has a significant positive (more deaths) effect. We focus on their 14-38 days results, as they catch the longest time frame (the authors' 0–24-day model returns mostly insignificant results). | The authors' results are counter intuitive and somewhat inconclusive. Why does limiting very large gatherings (>1,000) work, while stricter limits do not? Why does recommending school closures cause more deaths? Why is the effect of border closures before the first death insignificant, while the effect of closing borders after the first death is significant (and large)? Why does quarantining arrivals from high-risk regions work better than total border closures? With 23 estimated parameters in total, these counter intuitive and inconclusive results could be caused by multiple test bias (we correct for this in the meta-analysis) but may also be caused by other factors such as omitted variable bias. |
| Yang et al. (2021); 'What is the relationship between government response and COVID-19 pandemics? Global evidence of 118 countries' | COVID-19 deaths | The study uses OxCGRT stringency covering 118 countries from 1 January to 13 April 2021 to estimate the effect of lockdowns on mortality. | The study finds a statistical negative (fewer deaths) effect of stricter lockdowns on COVID-19 mortality rates 21 days later. | The study finds a (relatively large) effect after just seven days before any policy could be effective, which could indicate that other factors (omitted variables) affect the results. |

Note: All comments on the significance of estimates are based on a 5 per cent significance level unless otherwise stated.

It is difficult to draw any firm conclusions based on the overview presented in Table 2. For example, is –0.073 to –0.326 deaths per million per stringency point, as estimated by Ashraf (2020), a large or a small effect relative to the 98 per cent reduction in mortality predicted by the study published by Imperial College London (Ferguson et al. 2020)? This question is the subject of our meta-analysis in the next section. Here, it turns out that Ashraf's (2020) –0.073 to –0.326 deaths/million per stringency point represents a relatively modest effect – one that corresponds to only a 2.4 per cent reduction in COVID-19 mortality on average in the U.S. and Europe.

# 4. Meta-analysis: The impact of lockdowns on COVID-19 mortality

We now turn to the meta-analysis, where we focus on the impact of lockdowns on COVID-19 mortality.

In the meta-analysis, we include 22 studies from which we can derive a *standardised estimate,* which is the relative effect of lockdowns on COVID-19 mortality. That is, we obtain an estimate of the percentage of deaths that were avoided due to lockdowns. For some studies, the authors state the relative effect, and our standardised estimate is thus readily available. For other studies, their estimate must be converted to our standardised estimate.[43] Doing so is fairly straightforward for most studies, and our calculations are explained in Table 20. However, the estimates from ten studies cannot be converted to our standardised estimates. These include studies estimating the effect of lockdowns on mortality growth rates, unless the authors, such as Chernozhukov et al. (2021), calculate a counterfactual scenario.[44] Also, Mccafferty and Ashley (2021) estimate the effect of lockdowns on peak mortality, but an estimate of peak mortality cannot be meaningfully converted to our standardised estimate, which measures the relative impact on COVID-19 mortality.

43 This approach implicitly assumes that the results also hold for units outside the sample used in the difference-in-difference analysis, and thus the assumptions required are stronger.

44 One reason that we cannot calculate a counterfactual based on the estimated effect on growth rates is that we do not know the effect on the distribution and vertex of the death curve. If lower growth rates simply flatten the curve, the effect on total mortality can be limited even if the effect on growth rates is substantial.

The conclusions in the excluded studies are, overall, in correspondence with the conclusions in the included studies (see Table 3). Five excluded studies find lockdowns reduce mortality, two find mixed effects of NPIs, with some NPIs working and others not, and three find no or positive (more deaths) effects of NPIs. In comparison, eight of the included studies find lockdowns reduce mortality, eleven find mixed or insignificant effects of NPIs, and three studies find no or positive (more deaths) effects of lockdowns. Our reasons for not including a study are described in Table 20 in Appendix I.

**Table 3: Conclusions from included and excluded studies in the meta-analysis are similar**

| **Conclusion** | **Number (share) of studies included in meta-analysis** | **Number (share) of studies *not* included in meta-analysis** |
|---|---|---|
| Find that lockdowns reduce mortality | 8 (36%) | 5 (50%) |
| Find mixed effects of NPIs | 11 (50%) | 3 (30%) |
| Find that lockdowns have no effect (or increases mortality) | 3 (14%) | 2 (20%) |
| Total | 22 | 10 |

Note: Mixed effects means that some of the examined NPIs reduce mortality while others increase mortality. The category 'Find that lockdowns reduce mortality' includes studies which find at least one significant negative (fewer deaths) estimate ($p < 0.05$) and no significant positive estimates (more deaths). The category 'Find mixed effects of NPIs' includes studies which find both significantly negative and significantly positive estimates. The category 'Find that lockdowns have no effect (or increases mortality)' includes studies that find no significant estimates or find at least one significant positive (more deaths) estimate ($p < 0.05$) and no significant negative (fewer deaths) estimates.

The studies we examine are placed in three categories. Eight studies analyse the effect of stricter lockdowns based on the OxCGRT stringency indices, twelve studies analyse the effect of SIPOs (six studies only analyse SIPOs and six analyse SIPOs together with other interventions), and eight studies analyse the effect of specific NPIs independently.[45] Each of these categories is handled so that comparable estimates can be made across categories.

### *Bias dimensions*

Not all eligible studies are of the same quality. One way to handle this problem is to evaluate the quality of each study and use this evaluation to weigh or group the studies. However, there is currently no consensus as to best practices and/or an established scientific framework for evaluating the effectiveness of NPIs and lockdowns (see Banholzer et al. 2022). As a result, such evaluation risks being subjective. Instead, we investigate whether there are any biases in the reviewed studies that can affect the studies' conclusions. We do this by dividing them into different 'bias dimensions'. Below, we describe the dimensions as well as our reasons to believe they could describe important biases. We also describe which group we perceive as 'better' (meaning 'probably less biased'). However, it should be noted that the primary objective of these dimensions is to identify and understand any biases in the studies, which can affect our overall results.

- *Peer-reviewed vs. working papers*: We distinguish between peer-reviewed studies and working papers. All else being equal, we perceive peer-reviewed studies as better than working papers.[46]
- *Long vs. short data periods*: We distinguish between studies based on long time periods (with data series ending after 31 May 2020) and short time periods (data series ending at or before 31 May 2020), because the first wave did not fully end until late June in the U.S. and Europe. Thus, studies relying on short data periods omit the last part of the first wave and may yield biased results if lockdowns only 'flatten the curve' and do not prevent deaths. All else being equal, we perceive studies based on long periods as better than studies based on short periods.

45 The total is larger than 22 because the 12 SIPO studies include six studies that look at multiple measures.

46 Vetted papers from CEPR *Covid Economics* are considered working papers in this regard.

- *No early effect vs. early effect on mortality*: On average, it takes approximately three to four weeks from infection to death.[47] However, several studies find effects of lockdown on mortality almost immediately, which is clearly inconsistent with the standard view of COVID-19 transmission. Fowler et al. (2021) find a significant effect of SIPOs on mortality after just four days and the largest effect after 10 days. An early effect may indicate that other factors (omitted variables) drive the results. Thus, we distinguish between studies that find an effect on mortality sooner than 14 days after lockdown and those that do not.[48] Several studies do not look at the short term and are placed in the latter category by default. All else being equal, we perceive no early effect as better than an early effect (or no information).

- *Lag vs. no lag of policy measures*: On average, it takes approximately three to four weeks from COVID-19 infection to a possible death. Hence, the effect of a lockdown policy on mortality should be seen about three weeks after the policy measure is implemented, and a specification with a 2-4 week lagged policy variable is expected to be better than a specification with no lag, because the latter also captures the development around the policy decision, which is not influenced by the policy.[49] All else being equal, we perceive a lagged effect as being better than no lag.
- *Panel vs. no panel estimation:* The development of a pandemic in a country is affected by several factors and therefore may be inherently different from country to country. One way to handle these intrinsic differences is to exploit the panel data structure using fixed- or random-effects regression. Thus, we distinguish between studies that use fixed or random effects and those that do not. All else being equal, we perceive fixed/random effects regression as better than those that do not use fixed/random effects.

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47 Leffler et al. (2020) write: ‘On average, the time from infection with the coronavirus to onset of symptoms is 5.1 days, and the time from symptom onset to death is on average 17.8 days. Therefore, the time from infection to death is expected to be 23 days.’ Meanwhile, Stokes et al. (2020) state that ‘evidence suggests a mean lag between virus transmission and symptom onset of 6 days, and a further mean lag of 18 days between onset of symptoms and death.’

48 Some of the authors are aware of this problem. E.g. Bjørnskov (2021) writes ‘when the lag length extends to three or fourth weeks, that is, the length that is reasonable from the perspective of the virology of Sars-CoV-2, the estimates become very small and insignificant’ and ‘these results confirm the overall pattern by being *negative* and significant when lagged one or two weeks (the period when they cannot have worked) but turning positive and insignificant when lagged four weeks.’

49 See Chernozhukov et al. (2022) for a discussion of why this may affect estimates.

- *Verified vs. unverified data:* We distinguish between studies using verified data (e.g., from OxCGRT or the *New York Times*) and unverified data (e.g., data collected by researchers but not readily publicly available and not updated on a continuous basis). All else being equal, we perceive studies using verified data as better than studies using unverified data.[50]
- *Address vs. do not address causality*: Not all studies address the causality/endogeneity question. We distinguish between studies addressing the causality question and studies that do not address the question. We consider the question addressed if the authors handle causality technically (e.g., using instrument variables or lagged dependents) or argue for the causality of their results. All else being equal, we perceive studies that address causality as better than studies that do not address causality.
- S*ocial sciences vs. other sciences*: While it is true that epidemiologists and researchers in the natural sciences should, in principle, know much more about COVID-19 and how it spreads than social scientists, social scientists are, in principle, experts in evaluating the effect of various policy interventions. Thus, we distinguish between studies published by scholars in the social sciences and by scholars in other fields of research. For each study, we have registered the research field for the corresponding author's associated institute (e.g., for a scholar from 'Institute of Economics', the research field is registered as 'Economics'). Where no corresponding author was available, the affiliation of the first author has been used. Afterwards, all research fields have been classified as either 'Social Science' or 'Other'.[51] All else being equal, we perceive the social sciences as better suited for examining the effect of policies than other sciences.

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50 Eight studies are based on stringency data from OxCGRT, two are based on specific NPI data from OxCGRT, three are based on data from the *New York Times*, one on data from COVID-19 Tracking Project, and one on data from Response2covid19. These 15 studies are considered to be based on verified data. Three studies collect their own data, two studies rely on data from other studies, one study uses purchased data from Burbio.com, one study uses American Red Cross reporting on emergency regulation. These seven data sources are considered to be unverified (the latter two because they require login credentials and thus are not easily verified by external researchers).

51 Research fields classified as social sciences are economics, public health, management, political science, government, international development, and public policy, while research fields not classified as social sciences are ophthalmology, environment, medicine, evolutionary biology and environment, human toxicology, epidemiology, and anesthesiology.

We also considered including a bias dimension to distinguish between studies based on excess mortality and studies based on COVID-19 mortality, as we believe that excess mortality is potentially a better measure for two reasons. First, data on total deaths in a country is far more accurate than data on COVID-19-related deaths, which may be both underreported (due to a lack of tests) or overreported (because some people die *with* – but not *because of* – COVID-19). Second, a major goal with lockdowns was to save lives. To the extent lockdowns shift deaths *from* COVID-19 *to* other causes (e.g., suicide), estimates based on COVID-19 mortality will overestimate the effect of lockdowns. Likewise, if lockdowns save lives in other ways (e.g., fewer traffic accidents), lockdowns' effect on mortality will be underestimated. However, as only one of the 32 studies, Bjørnskov (2021), is based on excess mortality, we have to disregard this bias dimension.

Metadata used for our bias dimensions as well as other relevant information are shown in Table 4.

## Table 4: Bias dimension data for the studies included in the meta-analysis

| 1. Study (Author & title) | 2. Publication status | 3. End of data period | 4. Earliest effect | 5. Policy/death lag | 6. Fixed/random effects | 7. Verified data | 8. Handling of causality | 9. Field of research | 10. Quality index |
|---|---|---|---|---|---|---|---|---|---|
| Alderman and Harjoto (2020); 'COVID-19: US shelter-in-place orders and demographic characteristics linked to cases, mortality, and recovery rates' | Peer-review | 11-Jun-20 | n/a | No | No | Other official source (Verified) | Does not handle causality | Economics (Social science) | 0.25 |
| An et al. (2021); 'Policy design for COVID-19: Worldwide evidence on the efficacies of early mask mandates and other policy interventions' | Peer-review | 15-Jul-20 | n/a | Yes | Yes | Other official source (Verified) | Does not handle causality | Public policy (Social science) | 0.14 |
| Ashraf (2020); 'Socioeconomic conditions, government interventions and health outcomes during COVID-19' | WP | 20-May-20 | n/a | No | Yes | OxCGRT stringency index (Verified) | Other model handling | Economics (Social science) | 0.25 |
| Berry et al. (2021); 'Evaluating the effects of shelter-in-place policies during the COVID-19 pandemic' | Peer-review | 30-May-20 | 8-14 days | No | Yes | Other research data (Unverified) | Lag dependent (>2 weeks) | Public policy (Social science) | 0.00 |
| Bjørnskov (2021); 'Did lockdown work? An economist's cross-country comparison' | Peer-review | 30-Jun-20 | <8 days | Yes | Yes | OxCGRT stringency index (Verified) | Instrument variable | Economics (Social science) | 0.77 |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Bonardi et al. (2020); 'Fast and local: How did lockdown policies affect the spread and severity of the Covid-19' | WP | 13-Apr-20 | <8 days | Yes | Yes | Own data (Unverified) | Argumentation | Economics (Social science) | 0.39 |
| Chernozhukov et al. (2021); 'Causal impact of masks, policies, behavior on early covid-19 pandemic in the U.S.' | Peer-review | 03-Jun-20 | n/a | Yes | Yes | Other research data (Unverified) | Other model handling | Economics (Social science) | 0.14 |
| Chisadza et al. (2021); 'Government effectiveness and the COVID-19 pandemic' | Peer-review | 01-Sep-20 | n/a | No | no | OxCGRT stringency index (Verified) | Does not handle causality | Economics (Social science) | 0.25 |
| Dave et al. (2021); 'When do shelter-in-place orders fight Covid-19 best? Policy heterogeneity across states and adoption time' | Peer-review | 20-Apr-20 | Finds no effect | Yes | Yes | New York Times (Verified) | Argumentation | Economics (Social science) | 0.77 |
| Ertem et al. (2021); 'The impact of school opening model on SARS-CoV-2 community incidence and mortality' | Peer-review | 21-Dec-20 | >21 days | Yes | Yes | Other data (Unverified) | Does not handle causality | Engineering (Other) | 0.77 |
| Fowler et al. (2021); 'Stay-at-home orders associate with subsequent decreases in COVID-19 cases and fatalities in the United States' | Peer-review | 07-May-20 | <8 days | No | Yes | *New York Times* (Verified) | Other model handling | Public Health (Social science) | 0.39 |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Fuller et al. (2021); ‘Mitigation policies and COVID-19–associated mortality – 37 European countries, January 23–June 30, 2020’ | WP | 30-Jun-20 | n/a | Yes | No | OxCGRT stringency index (Verified) | Does not handle causality | Epidemiology (Other) | 0.14 |
| Gibson (2020); ‘Government mandated lockdowns do not reduce Covid-19 deaths: Implications for evaluating the stringent New Zealand response’ | Peer-review | 01-Jun-20 | Finds no effect | Yes | Yes | Other data (Unverified) | Instrument variable | Economics (Social science) | 0.77 |
| Goldstein et al. (2021); ‘Lockdown fatigue: The diminishing effects of quarantines on the spread of COVID-19‘ | WP | 31-Dec-20 | <8 days | Yes | Yes | OxCGRT stringency index (Verified) | Lag dependent (>2 weeks) | International Development (Social science) | 0.56 |
| Guo et al. (2021); ‘Mitigation interventions in the United States: An exploratory investigation of determinants and impacts’ | Peer-review | 07-Apr-20 | n/a | No | Yes | Own data (Unverified) | Does not handle causality | Social work (Social science) | 0.06 |
| Hale et al. (2021); ‘Government responses and COVID-19 deaths: Global evidence across multiple pandemic waves’ | Peer-review | 11-Mar-21 | n/a | Yes | Yes | OxCGRT stringency index (Verified) | Other model handling | Government (Social science) | 0.77 |
| Leffler et al. (2020); ‘Association of country-wide coronavirus mortality with demographics, testing, lockdowns, and public wearing of masks’ | Peer-review | 09-May-20 | n/a | No | No | OxCGRT data (Verified) | Does not handle causality | Ophthalmology (Other) | 0.39 |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Sears et al. (2020); 'Are we #stayinghome to flatten the curve?' | WP | 29-Apr-20 | Finds no effect | No | Yes | New York Times (Verified) | Argumentation | Economics (Social science) | 0.39 |
| Shiva and Molana (2021); 'The luxury of lockdown' | Peer-review | 08-Jun-20 | 15-21 days | Yes | Yes | OxCGRT stringency index (Verified) | Does not handle causality | Government (Social science) | 0.77 |
| Spiegel and Tookes (2021); 'Business restrictions and Covid-19 fatalities' | Peer-review | 31-Dec-20 | 15-21 days | Yes | No | Own data (Unverified) | Other model handling | Management (Social science) | 0.25 |
| Stokes et al. (2020); 'The relative effects of non-pharmaceutical interventions on early Covid-19 mortality: Natural experiment in 130 countries' | WP | 01-Jun-20 | n/a | Yes | Yes | OxCGRT data (Verified) | Other model handling | Economics (Social science) | 0.06 |
| Yang et al. (2021); 'What is the relationship between government response and COVID-19 pandemics? Global evidence of 118 countries' | Peer-review | 03-Dec-20 | <8 days | Yes | No | OxCGRT stringency index (Verified) | Does not handle causality | Economics (Social science) | 0.39 |

Note: Research fields classified as social sciences were economics, public health, health science, management, political science, government, international development, and public policy, while research fields not classified as social sciences were ophthalmology, environment, medicine, evolutionary biology and environment, human toxicology, epidemiology, and anesthesiology.

### *Interpreting and weighting estimates*

The estimates used in the meta-analysis are not always readily available in the studies shown in Table 4. In Table 20 in Appendix I, we describe for each paper how we interpret the estimates and how they are converted to a standardised estimate (the relative effect of lockdowns on COVID-19 mortality), which is comparable across all studies.

Following Paldam (2015) and Stanley and Doucouliagos (2010), we also convert standard errors (SE)[52] and use the precision of each estimate (defined as 1/SE) to calculate the precision-weighted average (PWA) of all estimates. The PWA is our primary indicator of the efficacy of lockdowns, but we also report arithmetic averages and medians in the meta-analysis.

### *Sensitivity analyses*

Given the relatively low number of studies in the meta-analysis, a study with an outlier estimate or outlier weight may influence our primary indicator of the efficacy of lockdowns. One way to deal with this uncertainty and illustrate the robustness of our estimates is to cap the estimates and weights at the end of the tails.

We therefore carry out four sensitivity analyses, where we replace the outlier (min/max) estimate/weight with the nearest estimate/weight and recalculate the PWA. For instance, the conclusion of Chisadza et al. (2021) is an outlier, which finds that the average lockdown *increases* COVID-19 mortality. In one sensitivity analysis, we replace the estimate from Chisadza et al. (2021) with the nearest estimate from Bjørnskov (2021) and recalculate the PWA. We report the result of our four sensitivity analyses as a span (from/to) at the bottom of each table.

### *Quality-adjusted precision-weighted average*

As a supplement to the PWA and the sensitivity analyses, we also calculate a quality-adjusted PWA based on our bias dimensions displayed in Table 4. The quality-adjusted PWA is calculated as the PWA weighted by a quality index, where the score on the quality index for each study is the

52 Standard errors are converted such that the t-value, calculated based on standardised estimates and standard errors, is unchanged. When confidence intervals are reported rather than standard errors, we calculate standard errors using a t-distribution with ∞ degrees of freedom (i.e., 1.96 for a 95 per cent confidence interval).

squared number of bias dimensions, where the study is of ‘better’ quality. Hence, each study can score between 0 and 64 on the index (because it includes eight bias dimensions). Finally, the index is normalised to 0-1 by dividing by 64.

In the following sections, we present the meta-analysis for each of the three groups of studies: stringency index studies, SIPO studies, and studies analysing specific NPIs.

### *4.1. Stringency index studies*

Eight eligible studies examine the link between lockdown stringency and COVID-19 mortality.[53] The results from these studies, converted to standardised estimates, are presented in Table 5 below. All studies are based on the COVID-19 Government Response Tracker’s (OxCGRT) stringency index of Oxford University’s Blavatnik School of Government (Hale et al. 2020).

The OxCGRT stringency index measures neither the expected effectiveness of the lockdowns nor the expected costs. Instead, it describes the stringency based on nine equally weighted parameters.[54] Many countries followed similar patterns, and almost all countries closed schools, while only a few countries issued SIPOs without closing businesses. Hence, it is reasonable to perceive the stringency index as continuous, although not necessarily linear. The index includes recommendations (e.g., ‘workplace closing’ is 1 if the government recommends closing (or working from home) (see Hale et al. 2021), but the effect of including recommendations in the index is primarily to shift the index parallelly upwards and should not alter the results relative to our focus on mandated NPIs.

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53 An earlier version of our meta-study also included Stockenhuber (2020). However, Stockenhuber (2020) does not use a difference-in-difference approach and is excluded in this version.

54 The nine parameters are ‘C1 School closing’, ‘C2 Workplace closing’, ‘C3 Cancel public events’, ‘C4 Restrictions on gatherings’, ‘C5 Close public transport’, ‘C6 Stay at home requirements’, ‘C7 Restrictions on internal movement’, ‘C8 International travel controls’ and ‘H1 Public information campaigns’. ‘H1 Public information campaigns’ is not an intervention following our lockdown definition, as it is not a mandatory requirement. However, of 97 European countries and U.S. states in the OxCGRT database, only Andorra, Belarus, Bosnia and Herzegovina, Faroe Islands, and Moldova – less than 1.6 per cent of the population – did not get the maximum score by 20 March 2020, so the parameter simply shifts the index parallelly upwards and should not have a notable impact on our conclusions.

It is important to note that the index is far from perfect. As pointed out by Book (2020), it is certainly possible to identify errors and omissions in the index. However, the index is objective and unbiased and, as such, useful for cross-sectional analysis with several observations, even if it is not suitable for comparing the overall strictness of lockdowns across two countries. In any case, there is no better available measure to adopt for cross-country comparisons.

Since the studies examined use different units of estimates, we have created standardised estimates for Europe and the United States to make them comparable. The standardised estimates show the effect of the average lockdown in Europe and the United States (with average stringencies of 76 and 74, respectively, between 16 March and 15 April 2020)[55] compared to the most lenient lockdown, which we define as a COVID-19 policy based solely on recommendations (stringency 44).[56]

For example, Ashraf (2020) estimates that the effect of stricter lockdowns is –0.073 to –0.326 deaths per million per stringency point. We use the average of these two estimates (–0.200) in the meta-analysis. The average lockdown in Europe between 16 March and 15 April 2020, was 32 points stricter than a policy solely based on recommendations (76 vs. 44). In the United States, it was 30 points. Hence, the total effect of the lockdowns compared to the recommendation policy was (using rounded numbers) –6.37 deaths per million in Europe (32 x –0.200) and –5.91 (30 x –0.200) deaths per million in the United States. With populations of 748 million and 333 million, respectively, the total effect as estimated by Ashraf (2020) is 4,766 averted COVID-19 deaths in Europe and 1,969 averted COVID-19 deaths in the United States. By the end of the study period in Ashraf (2020), which is 20 May 2020, 164,600 people in Europe and 97,081 people in the United States had died of COVID-19. Hence, the 4,766 averted COVID-19 deaths in Europe and the 1,969 averted COVID-19 deaths in the United States correspond to 2.8 per cent and 2.0 per cent of all COVID-19 deaths, respectively, with an arithmetic average of 2.4 per cent.

55 Unless otherwise noted, we use these values in our calculations. The average stringency index is relatively stable during the first wave until the end of June 2020. For instance, the average stringencies are 73 and 72, respectively, between 16 March and 30 June 2020.

56 In reality, the most lenient lockdown varies from study to study depending on the group of countries and/or states included in the study. However, for practical purposes our definition is sufficient to calculate standardised estimates.

Our standardised estimate is thus –2.4 per cent (see Table 5). Our approach is not unproblematic. First of all, the level of stringency varies over time for all countries. Secondly, OxCGRT has changed the index over time, and a 10-point difference today may not be the exact same as a 10-point difference when the studies were finalised. However, we believe these problems are small and unlikely to significantly alter our results.

Table 5 demonstrates that the studies find that lockdowns, on average, have reduced COVID-19 mortality rates by 3.2 per cent (precision-weighted average) and the sensitivity analysis shows a range from 4.4 per cent to 3.0 per cent. The results yield an arithmetic average of 8.9 per cent and a median of 5.8 per cent. To put the estimate in perspective, there were 188,542 registered COVID-19 deaths in Europe and 128,063 COVID-19 deaths in the United States by 30 June 2020. Thus, the 3.2 per cent PWA (8.9 per cent arithmetic average, 5.8 per cent median) corresponds to 6,000 (18,000, 12,000) avoided deaths in Europe and 4,000 (13,000, 8,000) avoided deaths in the United States.[57] In comparison, there are approximately 72,000 flu deaths in Europe and 38,000 flu deaths in the United States each year.[58]

Hence, based on the stringency index studies, we find that mandated lockdowns in Europe and the United States had a negligible effect on COVID-19 mortality rates.

57 The estimate from Fuller et al. (2021), the only study finding a substantial effect of lockdowns (–35 per cent), corresponds to 103,000 avoided deaths in Europe and 70,000 avoided deaths in the United States.

58 The average estimated flu deaths in the United States in the five years prior to COVID-19 were 38,400 according to the CDC (2022), and the WHO (2022) states that there are 72,000 flu deaths in Europe each year.

**Table 5: Estimates of the effect on COVID-19 mortality of the average lockdown in Europe and in the United States from studies based on the OxCGRT stringency index**

| Effect on COVID-19 mortality | Standardised estimate (Estimated Averted Deaths / Total Deaths) | Standard error | Weight (1/SE) |
|---|---|---|---|
| Bjørnskov (2021) | –0.3% | 0.822% | 122 |
| Shiva and Molana (2021) | –4.0%** | 0.395% | 253 |
| Chisadza et al. (2021) | 11.7%** | 1.442% | 69 |
| Goldstein et al. (2021) | –7.5% | 1.964% | 51 |
| Fuller et al. (2021) | –35.3%** | 9.085% | 11 |
| Ashraf (2020) | –2.4%** | 0.391% | 256 |
| Yang et al. (2021) | –16.3%** | 4.523% | 22 |
| Hale et al. (2021) | –16.9%** | 2.812% | 36 |
| **Precision-weighted average (arithmetic average / median)** | **–3.2% (–8.9%/–5.8%)** | | |
| **Sensitivity analysis (quality-adjusted PWA)** | **–4.4% to –3.0% (–3.8%)** | | |

Note: ** (*) denote significance at $p < 0.01$ ($p < 0.05$). The table shows the estimates for each study converted to a standardised estimate, i.e., the implied effect on COVID-19 mortality in Europe and United States. A negative number corresponds to fewer deaths, so –5% means 5 per cent lower COVID-19 mortality. For details on how the estimates are converted to standardised estimates see Table 20 in Appendix I. The quality-adjusted PWA is calculated as the PWA weighted by a quality index, where the score on the quality index for each study is the number of bias dimensions squared (except 'peer-reviewed' and 'social sciences'), where the study is of 'better' quality, see Table 4.

We now turn to the bias dimensions. Table 6 presents the results differentiated by the bias dimensions. We find no evidence of significant biases. Although the effect is generally of a larger magnitude (more negative, i.e., fewer deaths) for studies we – all else being equal – perceive as better, the difference is marginal.

Only one bias dimension, social sciences vs. other sciences, shows a large difference. This is because Fuller et al. (2021) find that lockdowns reduced COVID-19 mortality rates by 35.3 per cent, and is the only study from the non-social sciences. Fuller et al. (2021) do not exploit the panel structure of their data by using fixed or random effects, nor do they address the causality question. There are three studies that both lag policy implementation, exploit panel estimation, and address the causality question. These three studies find that lockdowns reduced COVID-19 mortality rates by 4.9 per cent (compared to 2.6 per cent in the other studies).

**Table 6: Estimates of the effect on COVID-19 mortality of the average lockdown in Europe and in the United States from studies based on the OxCGRT stringency index classified according to the bias dimensions**

| *Values show effect on COVID-19 mortality* | **Precision-weighted average** | **Arithmetic average** | **Median** |
|---|---|---|---|
| **Peer-reviewed vs. working papers** | | | |
| Peer-reviewed [5] | –2.4% | –5.2% | –4.0% |
| Working paper [3] | –5.0% | –15.1% | –7.5% |
| **Long vs. short data period** | | | |
| Data period ends after 31 May 2020 [7] | –3.5% | –9.8% | –7.5% |
| Data period ends before 31 May 2020 [1] | –2.4% | –2.4% | –2.4% |
| **No early effect on mortality** | | | |
| Does not find an effect within the first 14 days [1] | –4.0% | –4.0% | –4.0% |
| Finds effect within the first 14 days (including n/a) [7] | –2.8% | –9.6% | –7.5% |
| **Lag vs. no lag of policy measures** | | | |
| Lag of policy implementation [6] | –5.6% | –13.4% | –11.9% |
| No lag of policy implementation [2] | 1.5% | 4.7% | 4.7% |
| **Panel vs. no panel estimation** | | | |
| Panel estimation [5] | –3.8% | –6.2% | –4.0% |
| No panel estimation [3] | 0.6% | –13.3% | –16.3% |
| **Verified vs. unverified data** | | | |
| Verified data [8] | –3.2% | –8.9% | –5.8% |
| Unverified data [0] | n/a | n/a | n/a |
| **Address vs. do not address causality** | | | |
| Address causality [4] | –3.7% | –6.8% | –5.0% |
| Do not address causality [4] | –2.7% | –11.0% | –10.2% |
| **Social sciences vs. other sciences** | | | |
| Social sciences [7] | –2.8% | –5.1% | –4.0% |
| Other sciences [1] | –35.3% | –35.3% | –35.3% |

Note: The table shows the standardised estimate as described in Table 5 for each bias dimension. The number of studies in each category is in square brackets. A negative number corresponds to fewer deaths, so –5% means 5 per cent lower COVID-19 mortality.

### *Overall conclusion on stringency index studies*

Compared to a policy based solely on recommendations, we find little evidence that *stricter* lockdowns had a noticeable impact on COVID-19 mortality. Only one study, Fuller et al. (2021), finds a substantial effect of stricter lockdowns compared to the most lenient lockdowns, while the remaining studies find a negligible effect. Indeed, according to the stringency index studies, the average lockdown in Europe and the United States only reduced COVID-19 mortality by 3.2 per cent using the precision-weighted average. The sensitivity analysis ranges from 4.4 per cent to 3.0 per cent, and overall, our bias dimensions do not suggest that biases are important. We stress that this result does not imply that lockdowns do not work. It simply indicates that the most lenient lockdowns had virtually the same effect on mortality as stricter lockdowns. Since no country did nothing, we cannot reject the thesis that some NPI would be required, e.g., to spur voluntary behavioural changes.[59]

It should also be noted that the eight stringency studies are all based on the same index (OxCGRT stringency index). Although OxCGRT is widely recognised as the best index recording the strictness of 'lockdown style' policies that restrict people's behaviour and tracks and compares policy responses around the world, rigorously and consistently, we cannot rule out the possibility that the lack of evidence of the efficacy of lockdowns is caused by the limitations of the index.[60] In the following section, we will look at the effect of one of the strictest NPIs used during the COVID-19 pandemic, SIPO, following the same structure as the current section.

## *4.2. Shelter-in-place-order (SIPO) studies*

We have identified twelve eligible studies that estimate the effect of shelter-in-place orders (SIPOs) on COVID-19 mortality (see Table 7).[61] Six of these studies look at multiple NPIs of which a SIPO is just one, while six studies estimate the effect of a SIPO vs. no SIPO in the United States. According to the containment and closure policy indicators from OxCGRT,

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59 As noted earlier, Sweden limited public gatherings to 500 people on 12 March 2020, and – at the same time as Denmark and Norway – eliminated influenza, see Figure 8. Based on this experience, limiting gatherings to 500 people could be sufficient, if some NPI is required to spur voluntary behavioural changes.

60 Morgenstern (1963) describes in detail how such indices can be 'fuzzy' metrics containing lots of errors in measurement.

61 An earlier version of our meta-analysis also included Aparicio and Grossbard (2021) and Chaudhry et al. (2020). However, these studies do not use a difference-in-difference approach and have been excluded in this version.

41 states in the United States issued SIPOs in the spring of 2020. Usually, these were introduced after implementing other NPIs, such as school closures or workplace closures.

On average, SIPOs were issued 7½ days after both schools and workplaces closed and 12 days after the first of the two closed. Only one state, Tennessee, issued a SIPO before schools and workplaces closed. The ten states that did not issue SIPOs all closed schools. Moreover, of those ten states, three closed some non-essential businesses, while the remaining seven closed all non-essential businesses. Because of this, we perceive estimates for SIPOs based on U.S. data as the marginal effect of SIPOs on top of other restrictions, although we cannot rule out that the estimates may capture the effects of other NPI measures as well.

The results of eligible studies based on SIPOs are presented in Table 7. This table demonstrates that the studies generally find that SIPOs have reduced COVID-19 mortality by 2.0 per cent (precision-weighted average) and the sensitivity analysis shows a span from 4.1 per cent to 1.4 per cent. The arithmetic average estimate is 7.8 per cent and the median is 0.5 per cent. To put these numbers into perspective, there were 188,542 registered COVID-19 deaths in Europe and 128,063 COVID-19 deaths in the United States by 30 June 2020. Thus, the reduction of 2.0 per cent PWA (7.8 per cent arithmetic average, 0.5 per cent median) corresponds to approximately 4,000 (16,000, 1,000) avoided deaths in Europe and 3,000 (11,000, 1,000) avoided deaths in the United States, had all countries and states implemented SIPOs. In comparison, there are approximately 72,000 flu deaths in Europe and 38,000 flu deaths in the United States each year.[62]

There is an apparent difference between studies in which a SIPO is one of multiple NPIs and studies in which a SIPO is the only examined intervention. The former group generally finds that SIPOs *increase* COVID-19 mortality by 6.0 per cent, whereas the latter finds that SIPOs *decrease* COVID-19 mortality by 5.1 per cent. As we will see below, this difference may – at least partly – be explained by the data period covered by each study.

62 The average estimated flu deaths in the United States in the five years prior to COVID-19 were 38,400 according to CDC (2022), and WHO (2022) writes that there are 72,000 flu deaths in Europe each year.

## Table 7: Estimates of the effect on COVID-19 mortality of shelter-in-place orders (SIPOs)

| *Values show effect on COVID-19 mortality* | **Standardised estimate (Estimated Averted Deaths / Total Deaths)** | **Standard error** | **Weight (1/SE)** |
|---|---|---|---|
| **Studies where SIPO is one of several examined interventions and not (as) likely to capture the effect of other interventions** | | | |
| Chernozhukov et al. (2021) | –17.7% | 14.3% | 7 |
| Stokes et al. (2020) | 4.9% | 2.8% | 36 |
| Spiegel and Tookes (2021) | 13.1%* | 6.6% | 15 |
| Bonardi et al. (2020)* | 0.0% | n/a | n/a |
| Guo et al. (2021) | 4.6% | 14.8% | 7 |
| An et al. (2021)** | 15.6% | 7.8% | 13 |
| Precision-weighted average (arithmetic average / median) where SIPO is one of several variables | 6.0% (3.4%/4.7%) | | |
| **Studies where SIPO is the only examined intervention and may capture the effect of other interventions** | | | |
| Sears et al. (2020) | –32.2% | 17.6% | 6 |
| Alderman and Harjoto (2020) | –1.0% | 0.6% | 169 |
| Berry et al. (2021)* | 1.1% | n/a | n/a |
| Fowler et al. (2021) | –35.0%** | 7.0% | 14 |
| Gibson (2020) | -6.0% | 24.3% | 4 |
| Dave et al. (2021) | –40.8% | 36.1% | 3 |
| Precision-weighted average (arithmetic average / median) where SIPO is the only variable | –5.1% (–19.0%/–19.1%) | | |
| Precision-weighted average (arithmetic average / median) for all studies | –2.0% (–7.8%/–0.5%) | | |
| Sensitivity analysis (quality-adjusted PWA) | –4.1% to –1.4% (–1.8%) | | |

Note: ** (*) denote significance at $p < 0.01$ ($p < 0.05$). A negative number corresponds to fewer deaths, so –5% means 5 per cent lower COVID-19 mortality. The quality-adjusted PWA is calculated as the PWA weighted by a quality index, where the score on the quality index for each study is the number of bias dimensions squared (except 'peer-reviewed' and 'social sciences'), where the study is of 'better' quality, see Table 4.

* Bonardi et al. (2020) and Berry et al. (2021) do not affect the precision-weighted average, as we do not know the standard error.

** An et al. (2021) report estimates for a SIPO introduced both early and late.[63] For simplicity we only report the average estimate and standard error (detailed estimates are +13% and +18%). Stokes et al. (2020) report estimates for a SIPO implemented before first death and within 14 days after first death. Again, for simplicity we only report the average estimate and standard error (detailed estimates are +1.8% and +8.0%%, average SE is calculated as $\text{sqrt}((SE_1{}^2+SE_2{}^2+\ldots+SE_n{}^2)/k)$, where k is the number of estimates).

Table 8 presents the results differentiated by bias dimensions. One bias dimension, 'long vs. short data period', shows a large potential bias driven by relatively short data periods. The four studies with relatively short data periods find a very large effect of SIPOs (a 25.9 per cent reduction in mortality rates), while studies based on longer data periods find a modest increase in mortality rates of 1.0 per cent. The last data points for the three studies that find the – by far – largest effects of SIPOs (Sears et al. 2020, Fowler et al. 2021, and Dave et al. 2021) are 29 April, 7 May, and 20 April in 2020, respectively. These findings could indicate that SIPOs can delay deaths but not eliminate them. However, these studies were also done very early in the pandemic and could not – as do the other studies – 'stand on the shoulders of giants'. The bias dimensions 'Lag vs. no lag of policy implementation', 'Panel vs. no panel estimation', and 'Address vs. do not address causality' also find some potential bias, although of a lesser magnitude.

63 An et al. (2021) define early mandate adoption as being taken within 14 days (the median in their dataset) after the first reported infection in each country.

**Table 8: Estimates of the effect on COVID-19 mortality of shelter-in-place orders (SIPOs) classified according to the bias dimensions**

| *Values show effect on COVID-19 mortality* | **Precision-weighted average** | **Arithmetic average** | **Median** |
|---|---|---|---|
| **Peer-reviewed vs. working papers** | | | |
| Peer-review [8] | –2.3% | –8.4% | –3.5% |
| Working paper [2] | –0.2% | –13.6% | 0.0% |
| **Long vs. short data period** | | | |
| Data period ends after 31 May 2020 [6] | 1.0% | 1.5% | 1.9% |
| Data period ends before 31 May 2020 [4] | –25.9% | –25.8% | 0.0% |
| **No early effect on mortality** | | | |
| Finds effect within the first 14 days [4] | –4.3% | –16.5% | –19.1% |
| Does not find an effect within the first 14 days (including n/a) [6] | –1.7% | –4.8% | 0.0% |
| **Lag vs. no lag of policy measures** | | | |
| Lag policy implementation [6] | 3.8% | –5.2% | –0.6% |
| No lag of policy implementation [4] | –4.2% | –15.9% | 0.0% |
| **Panel vs. no panel estimation** | | | |
| Panel estimation [8] | –6.4% | –13.3% | –11.9% |
| No panel estimation [2] | 0.2% | 6.0% | 0.0% |
| Verified vs. unverified data | | | |
| Verified data [6] | –2.6% | –14.7% | –16.6% |
| Unverified data [4] | 2.5% | –1.5% | 0.0% |
| **Address vs. do not address causality** | | | |
| Address causality [7] | –6.7% | –16.2% | –17.7% |
| Do not address causality [3] | 0.2% | 6.4% | 0.6% |
| **Social sciences vs. other sciences** | | | |
| Social sciences [10] | –2.0% | –9.4% | –3.5% |
| Other sciences [0] | n/a | n/a | n/a |

Note: The table shows the standardised estimate as described in Table 7 for each bias dimension. The number of studies in each category is in square brackets (the numbers do not include Bonardi et al. (2020) and Berry et al. (2021), because they do not affect the precision-weighted average, as we do not know the standard error). A negative number corresponds to fewer deaths, so –5% means 5 per cent lower COVID-19 mortality.

There are three studies – Chernozhukov et al. (2021), Stokes et al. (2020), and Gibson (2020) – that use long data series, lag the policy implementation, exploit panel estimation, and address the causality question. These studies find that SIPOs *increased* COVID-19 mortality rates by 0.6 per cent (compared to an average reduction in mortality by 2.5 per cent in the other studies).

### *Overall conclusion on SIPO studies*

We find that SIPOs had a negligible effect on COVID-19 mortality. On average, countries in Europe and states in the United States that used SIPOs only reduced COVID-19 mortality by 2.0 per cent (precision-weighted average). The sensitivity analysis ranges from 1.4 per cent to 4.1 per cent, and our bias dimensions suggest that using long data series is important and that this will further reduce the estimated effect of SIPOs, possibly making the effect of a SIPO on COVID-19 mortality positive (more deaths).

Multiple studies find a small *positive* effect of SIPOs on COVID-19 mortality. Although such a result might appear to be counterintuitive, it could, for example, be the result of an (asymptomatic) infected person being isolated at home under a SIPO who can infect family members with a higher viral load causing more severe illness.[64,65] Our result is in line with Nuzzo et al. (2019), who state that ‘in the context of a high-impact respiratory pathogen, quarantine may be the least likely NPI to be effective in controlling the spread due to high transmissibility’ and World Health Organization Writing Group (2006), which concludes that ‘forced isolation and quarantine are ineffective and impractical.’[66]

64 See Guallar et al. (2020), who conclude that ‘our data support that a greater viral inoculum at the time of SARS-CoV-2 exposure might determine a higher risk of severe COVID-19.’

65 One could imagine that SIPOs also affected other types of deaths, including deaths by despair, impacts of deferred diagnoses, etc. However, all studies in Table 7 examine the effect of SIPOs on COVID-19 deaths – not overall mortality – so these are not likely explanations.

66 Both Nuzzo et al. (2019) and World Health Organization Writing Group (2006) focus on quarantining infected persons. However, if quarantining infected persons is not effective, it should come as no surprise that quarantining uninfected persons could be ineffective too.

In the following section, we will look at the effects found in studies analysing other specific NPIs.

### *4.3. Studies of other specific NPIs*

A total of eight eligible studies examine the effect of specific NPIs.[67] The definition of these specific NPIs varies from study to study, which makes comparison difficult. The variety of definitions can be seen in the analysis of non-essential business closures and bar/restaurant closures. Chernozhukov et al. (2021) focus on a combined parameter (the average of business closure and bar/restaurant closure in each state), Spiegel and Tookes (2021) examine bar and/or restaurant closure but not business closure, and Guo et al. (2021) look at both business closures and bar/restaurant closures independently.

Some studies include several NPIs (e.g., Stokes et al. 2020 and Spiegel and Tookes 2021), while others cover very few. For example, Leffler et al. (2020) look at internal lockdowns of any type, mask recommendations, and international travel restrictions. Too few NPIs in a model are potentially a problem because they can capture the effect of excluded NPIs.[68] On the other hand, several NPIs in a model increase the risk of multiple test bias. Also, looking at one NPI at a time may be problematic, as behavioural spillover effects may not be fully captured. For example, if we show that closing bars works because people who go to bars are more likely to be infected than people who do not go to bars, this finding does not automatically imply that closing bars will have a significant impact on the overall number of infections, if people adjust their behaviour according to official case numbers and are more careful when case numbers rise.

The differences in the choice of NPIs and in the number of NPIs generally make it challenging to create an overview of the results. In the following, we go through the evidence for the effectiveness of specific NPIs. First, we cover business closures, then school closures, limiting gatherings, border closures,

67 Based on our search strategy we did not search on specific measures such as 'school closures' but on words describing the overall political approach to the COVID-19 pandemic, such as 'non-pharmaceutical', 'NPIs', 'lockdown' etc.

68 Say two studies, A and B, examine the effect of lockdowns. Study A examines school closure and business closure, whereas study B examines business closure and a SIPO. Then, the estimates from study A could capture the effect of the omitted variable SIPO and the estimates from study B could capture the effect of school closures. Based on study A and B, we would report precision-weighted averages on three estimates, but since they all potentially capture the effect of omitted variables, our precision-weighted average would be biased towards larger effects.

and face masks, as these NPIs are all covered by at least four studies. Last, we cover NPIs covered by one or two studies (cancellation of public events, closing public transport, and restrictions on internal movement).

### *Business closures*

Five studies examine the effect of business closures on COVID-19 mortality.[69] Table 9 presents an overview of the estimates in these studies. Closing businesses reduced COVID-19 mortality rates by 7.5 per cent (precision-weighted average), and an arithmetic average of 10.5 per cent and a median of 5.5 per cent. The sensitivity analysis shows rates ranging from 6.6 per cent to 9.3 per cent reduction in mortality.

Three studies find a negligible effect, one study (Spiegel and Tookes 2021) finds some effect, and another study (Chernozhukov et al. 2021) finds a relatively large effect. It should be noted that the estimate from Chernozhukov et al. (2021) is based on their model without the national death-variable, which may be interpreted as an information signal (see their Table 7). They also run a model with national deaths, where they find that business closures *increase* mortality (see their Table 9). However, since they do not calculate a counterfactual for this model, this estimate is not included in Table 9. If there is a large effect, it seems related to closing bars and restaurants. Indeed, the 'close businesses' category in Chernozhukov et al. (2021) is an average of closed businesses, restaurants, and movie theatres. And, the 'closing bars and restaurants' submeasure (in grey in Table 9) in Spiegel and Tookes (2021) delivers the largest relative effect. The overall estimate of business closures for Spiegel and Tookes (2021) is much smaller than the estimate for just its 'closing bars and restaurants' submeasure.

69 An earlier version of our meta-analysis included Bongaerts et al. (2021), which looked at business closures in Italy and thus did not meet our eligibility criteria of sufficient jurisdictional variance.

## Table 9: Estimates of the effect on COVID-19 mortality of business closures

| Effect on COVID-19 mortality | Description | Standardised estimate (Estimated Averted Deaths / Total Deaths) | Standard error | Weight (1/SE) |
|---|---|---|---|---|
| **Chernozhukov et al. (2021)[*]** | **Measure is average of business and bar/restaurant closure** | **–28.6%** | **20.842%** | **5** |
| **Spiegel and Tookes (2021)** | **Business closure (average)** | **–13.3%[*]** | **5.812%** | **17** |
| | Bars & restaurants | –50.2%[**] | 8.735% | 11 |
| | Bars but not restaurants | –4.9% | 4.208% | 24 |
| | Gyms closed | –13.8%[**] | 4.272% | 23 |
| | Spas closed | 15.9%[**] | 4.782% | 21 |
| **Stokes et al. (2020)[**]** | **Workplace closing** | **–4.9%[**]** | **1.822%** | **55** |
| | Implemented before 1st death | –4.8%[*] | 1.955% | 51 |
| | Implemented 14 days after 1st death | –5.0%[**] | 1.678% | 60 |
| **Guo et al. (2021)** | **Business closure (average)** | **–0.4%** | **15.616%** | **6** |
| | Business closure | 4.7% | 13.600% | 7 |
| | Bars & restaurants | –5.5% | 17.400% | 6 |
| **An et al. (2021)** | **Business closure (average)** | **–5.5%** | **13.062%** | **8** |
| | Early business closure | –7.5% | 5.760% | 17 |
| | Late business closure | –3.6% | 17.551% | 6 |
| **Precision-weighted average (arithmetic average / median)** | | **–7.5% (–10.5%/–5.5%)** | | |
| **Sensitivity analysis (quality-adjusted PWA)** | | **–9.3% to –6.6% (–11.9%)** | | |

Note: ** (*) denote significance at $p < 0.01$ ($p < 0.05$). For studies with several estimates related to the measure, we report all submeasures in grey but focus on the average of these measures and use the average when calculating the PWA. The average standard error is calculated as $\mathrm{sqrt}((SE_1^2 + SE_2^2 + \ldots + SE_n^2)/k)$, where k is the number of estimates. The quality-adjusted PWA is calculated as the PWA weighted by a quality index, where the score on the quality index for each study is the number of bias dimensions squared (except 'peer-reviewed' and 'social sciences'), where the study is of 'better' quality, see Table 4. Values in grey are not included in the calculations of the precision-weighted average, the arithmetic average, and the median. A negative number corresponds to fewer deaths, so –5% means 5 per cent lower COVID-19 mortality.

[*] The estimate from Chernozhukov et al. (2021) is based on their model without the national deaths-variable (see their Table 7). They also run a model with national deaths, where they find that business closures increase mortality slightly (see their Table 9). However, since they do not calculate a counterfactual for this model, this estimate is not included in Table 9.

[**] Stokes et al. (2020) consider two distinct time periods of 24 days to account for the changing magnitude of effects of an exponentially transmitted virus: i) 0-24 days and ii) 14-38 after the first COVID-19 death. We report both and use the average in the precision-weighted average.

### *School closures*

Four studies examine the effect of school closure on COVID-19 mortality.[70] Table 10 presents an overview of the estimates in the studies. Closing schools reduced COVID-19 mortality rates by 5.9 per cent (precision-weighted average) with an arithmetic average of 0.2 per cent and a median of 0.0 per cent. The sensitivity analysis shows a range of 2.5 per cent to 6.2 per cent.

Since schools in the United States were closed almost simultaneously, the estimate from Guo et al. (2021) suffers from a lack of variation in school closures, but has little impact on the precision-weighted average due to the low precision/weight.[71] Ertem et al. (2021) look at school re-openings and find a (small) increase in mortality rates following school re-openings.

70 An earlier version of our meta-analysis included Auger et al. (2020), which looked at school closures in the United States. However, Auger et al. (2020) represent an interrupted time series analysis and, thus, did not meet our eligibility criteria.

71 According to Auger et al. (2020), all 50 states closed schools between 13 March 2020 and 23 March 2020, which means that all difference-in-difference is based on a maximum of seven school days (44 states closed schools in just four school days (15 March 2020 (Sunday) to 19 March 2020 (Friday)), see Table 1 in Auger et al. (2020)).

Assuming that the effect of closing and reopening is identical, this corresponds to a (small) decrease in mortality rates following school closures.

**Table 10: Estimates of the effect on COVID-19 mortality of school closures**

| Effect on COVID-19 mortality | Description | Standardised estimate (Estimated Averted Deaths / Total Deaths) | Standard error | Weight (1/SE) |
|---|---|---|---|---|
| **Stokes et al. (2020)*** | **School closures (average)** | **–10.9%**** | **1.567%** | **64** |
| | School closing implemented before 1st death | –2.2% | 1.681% | 59 |
| | School closing implemented after 1st death | –19.7%** | 1.443% | 69 |
| **Guo et al. (2021)**** | **School closures (average)** | **10.1%** | **20.662%** | **5** |
| | Public school closure | –0.2% | 23.400% | 4 |
| | All school closure | 20.4% | 17.500% | 6 |
| **An et al. (2021)** | **School closures (average)** | **3.8%** | **7.374%** | **14** |
| | Early school closure | 2.8% | 7.590% | 13 |
| | Late school closure | 4.7% | 7.152% | 14 |
| **Ertem et al. (2021)***** | **Opening schools (virtual vs. traditional)** | **–3.7%**** | **1.982%** | **50** |
| **Precision-weighted average (arithmetic average / median)** | | **–5.9% (–0.2%/0.0%)** | | |
| **Sensitivity analysis (quality-adjusted PWA)** | | **–6.2% to –2.5% (–1.2%)** | | |

Note: ** (*) denote significance at $p < 0.01$ ($p < 0.05$). For studies with several estimates related to the measure, we report all submeasures in grey but focus on the average of these measures and use the average when calculating the PWA. The average standard error is calculated as $\mathrm{sqrt}((SE_1^2 + SE_2^2 + \ldots + SE_n^2) / k)$, where k is the number of estimates. Chernozhukov et al. (2021) also examine the effect of school closures but does not report a counterfactual estimate. Based on a back-of-the-envelope sensitivity analysis it is unlikely that including Chernozhukov et al. (2021) would have a considerable effect on the result. The quality-adjusted PWA is calculated as the PWA weighted by a quality index, where the score on the quality index for each study is the number of bias dimensions squared (except 'peer-reviewed' and 'social sciences'), where the study is of 'better' quality, see Table 4. Values in grey are not included in the calculations of the precision-weighted average, the arithmetic average, and the median. A negative number corresponds to fewer deaths, so –5% means 5 per cent lower COVID-19 mortality.

* Stokes et al. (2020) consider two distinct time periods of 24 days to account for the changing magnitude of effects of an exponentially transmitted virus: i) 0-24 days and ii) 14-38 after the first COVID-19 death. We report both and use the average in the precision-weighted average.

** The estimate from Guo et al. (2021) is based on school closures in United States where all 50 states closed schools between 13 March 2020 and 23 March 2020, and 44 states closed schools in just four school days (15 March 2020 (Sunday) to 19 March 2020 (Friday)), see Table 1 in Auger et al. (2020). Hence, the difference-in-difference effect is based on a maximum of seven school days. As noted by Chernozhukov et al. (2021), this lack of cross-sectional variation can lead to considerable uncertainty over the effects of school closures, and the results from Guo et al. (2021) should be treated with this in mind.

*** The standard error for Ertem et al. (2021) is underestimated and, thus, the weight, 1/SE, is overestimated. See the description for Ertem et al. (2021) in Table 20 in Appendix I for further details.

The absence of a notable effect of school closures is in line with Irfan et al. (2021), who – based on a systematic review and meta-analysis of 90 published or preprint studies of transmission in children – concluded that 'risks of infection among children in educational-settings was lower than in communities. Evidence from school-based studies demonstrate it is largely safe for young children (<10 years of age) to be at schools, however, older children (between 10 and 19 years of age) might facilitate transmission.'

UNICEF (2020) and ECDC (2020) reach similar conclusions. UNICEF (2020) concludes,

> The preliminary findings thus far suggest that in-person schooling – especially when coupled with preventive and control measures – had lower secondary COVID-19 transmission rates compared to other settings and do not seem to have significantly contributed to the overall community transmission risks.

Similarly, ECDC (2020) concludes,

> there is a general consensus that the decision to close schools to control the COVID-19 pandemic should be used as a last resort. The negative physical, mental health and educational impact of proactive school closures on children, as well as the economic impact on society more broadly, would likely outweigh the benefits […] School closures can contribute to a reduction in SARS-CoV-2 transmission, but by themselves are insufficient to prevent community transmission of COVID-19 in the absence of other non-pharmaceutical interventions (NPIs) such as restrictions on mass gathering.[72]

Even though UNICEF (2020) and ECDC (2020) published their reviews in December 2020, there were still at least 160 countries that closed schools during 2021, according to the Oxford COVID-19 Government Response Tracker (see Hale et al. 2021).

### *Limiting gatherings*

Four studies examine the effect of limiting gatherings. Table 11 presents an overview of the estimates in the studies. Limiting gatherings *increased* COVID-19 mortality rates by 5.9 per cent (precision-weighted average) and the sensitivity analysis shows a span from a 4.9 per cent to an 8.9 per cent *increase* in mortality rates. The arithmetic average is 8.5 per cent and the median is 7.0 per cent, while the quality-adjusted PWA is 9.8 per cent.

It is worth noting that no studies have provided estimates showing that limiting gatherings reduced COVID-19 mortality. Indeed, all four studies find positive – and sometimes rather large – effects of limiting gatherings on mortality.

72 Isphording et al. (2021) apply a quasi-experimental approach where they use the staggered timing of summer breaks across federal states in Germany to estimate the causal impact of school re-openings after the summer break in 2020. They find no evidence of a positive effect of school re-openings on case numbers. Rather, they find that the end of summer breaks had a negative but insignificant effect on the number of new confirmed cases.

## Table 11: Estimates of the effect on COVID-19 mortality of limiting gatherings

| Effect on COVID-19 mortality | Description | Standardised estimate (Estimated Averted Deaths / Total Deaths) | Standard error | Weight (1/SE) |
|---|---|---|---|---|
| **Stokes et al. (2020)** | **Limiting gatherings (average)** | **3.8%**** | **1.307%** | **77** |
| | Restrictions implemented before 1st death | 2.4% | 1.403% | 71 |
| | Restrictions implemented after 1st death | 5.2%** | 1.204% | 83 |
| **Spiegel and Tookes (2021)** | **Limiting gatherings (average)** | **16.0%*** | **7.014%** | **14** |
| | Gatherings <=10 | 4.5% | 8.034% | 12 |
| | Gatherings 11-100 | 19.1%** | 6.249% | 16 |
| | Gatherings (limit > 100) | 24.4%** | 6.631% | 15 |
| **Guo et al. (2021)** | **Limiting gatherings (average)** | **5.7%** | **13.100%** | **8** |
| | Gatherings <=10 | 8.7% | 13.050% | 8 |
| | Gatherings 11-100 | 2.7% | 13.150% | 8 |
| **An et al. (2021)** | **Limiting gatherings (average)** | **8.4%** | **12.442%** | **8** |
| | Early gathering limits | 1.1% | 8.149% | 12 |
| | Late gathering limits | 15.7% | 15.595% | 6 |
| **Precision-weighted average (arithmetic average / median)** | | **5.9% (8.5%/7.0%)** | | |
| **Sensitivity analysis (quality-adjusted PWA)** | | **4.9% to 8.9% (9.8%)** | | |

Note: ** (*) denote significance at $p < 0.01$ ($p < 0.05$). For studies with several estimates related to the measure, we report all submeasures in grey but focus on the average of these measures and use the average when calculating the PWA. The average standard error is calculated as $\text{sqrt}((SE_1^2 + SE_2^2 + \ldots + SE_n^2)/k)$, where k is the number of estimates. A negative number corresponds to fewer deaths, so –5% means 5 per cent lower COVID-19 mortality. The quality-adjusted PWA is calculated as the PWA weighted by a quality index, where the score on the quality index for each study is the number of bias dimensions squared (except 'peer-reviewed' and 'social sciences'), where the study is of 'better' quality, see Table 4. Values in grey are not included in the calculations of the precision-weighted average, the arithmetic average, and the median. Stokes et al. (2020) consider two distinct time periods of 24 days to account for the changing magnitude of an exponentially transmitted virus: i) 0-24 days and ii) 14-38 after the first COVID-19 death. We report both and use the average in the precision-weighted average.

### *Travel restrictions*

Five studies examine the effect of travel restrictions.[73] Table 12 presents an overview of the estimates in these studies. Travel restrictions reduced COVID-19 mortality rates by 3.4 per cent (precision-weighted average) and the sensitivity analysis shows a span from 0.4 per cent to 4.7 per cent. The arithmetic average is a 5.3 per cent *increase* in mortality and the median is 0.0 per cent, while the quality-adjusted PWA shows an *increase* of 2.1 per cent.

The description of the NPI varies greatly between studies and may not be comparable. This may partly explain the large span of estimates (from a *reduction* of 15.6 per cent to an *increase* of 36.3 per cent).

73 An earlier version of our meta-analysis also included Toya and Skidmore (2021). However, Toya and Skidmore (2021) does not use a difference-in-difference approach and is excluded in this version.

### Table 12: Estimates of the effect on COVID-19 mortality of travel restrictions

| Effect on COVID-19 mortality | Description | Standardised estimate (Estimated Averted Deaths / Total Deaths) | Standard error | Weight (1/SE) |
|---|---|---|---|---|
| **Leffler et al. (2020)** | **International travel restrictions** | **–15.6%** | **5.737%** | **17** |
| **Stokes et al. (2020)** | **Travel restrictions (average)** | **–6.1%**** | **1.320%** | **76** |
| | Travel restrictions implemented before 1st death | –2.1% | 1.417% | 71 |
| | Travel restrictions implemented after 1st death | –10.0%** | 1.216% | 82 |
| **Bonardi et al. (2020)** | **Border closures** | **0.0%** | **n/a** | **n/a** |
| **Guo et al. (2021)** | **Quarantine of travellers** | **36.3%** | **16.950%** | **6** |
| **An et al. (2021)** | **Travel restrictions (average)** | **12.1%** | **8.440%** | **12** |
| | *Early travel restrictions* | 5.5% | 5.366% | 19 |
| | *Late travel restrictions* | 18.7% | 10.662% | 9 |
| **Precision-weighted average (arithmetic average / median)** | | **–3.4% (5.3%/0.0%)** | | |
| **Sensitivity analysis (quality-adjusted PWA)** | | **–4.7% to –0.4% (1.1%)** | | |

Note: ** (*) denote significance at $p < 0.01$ ($p < 0.05$). For studies with several estimates related to the measure, we report all submeasures in grey but focus on the average of these measures and use the average when calculating the PWA. The average standard error is calculated as $\mathrm{sqrt}((SE_1^2 + SE_2^2 + \ldots + SE_n^2)/k)$, where k is the number of estimates. A negative number corresponds to fewer deaths, so –5% means 5 per cent lower COVID-19 mortality. The quality-adjusted PWA is calculated as the PWA weighted by a quality index, where the score on the quality index for each study is the number of bias dimensions squared (except ‘peer-reviewed’ and ‘social sciences’), where the study is of ‘better’ quality, see Table 4. Values in grey are not included in the calculations of the precision-weighted average, the arithmetic average, and the median. Stokes et al. (2020) consider two distinct time periods of 24 days to account for the changing magnitude of effects of an exponentially transmitted virus: i) 0-24 days and ii) 14-38 after the first COVID-19 death. We report both and use the average in the precision-weighted average.

Allow us to stress that the level of aggregation in Table 12 is high. It combines inherently different measures, such as border closures and the quarantine of travellers. Also, none of the studies specifically examine the cases in which travel restrictions are most likely to work. As Woolhouse (2022) explains, border closures are most likely to be effective when borders are closed early and the closure does not involve too many exemptions. In an open, internationally-oriented economy, where people and goods cross borders *en masse*, fully closed borders or effective quarantining may only be possible for islands.[74]

### *Mask mandates*

The three studies examining the effect of mask mandates – an intervention that was not widely used in the spring of 2020, and in many countries was even discouraged – on average find that mask mandates reduced COVID-19 mortality by 18.7 per cent (precision-weighted average), see Table 13. The sensitivity analysis shows a span from 12.5 per cent to 19.9 per cent, and the arithmetic average and median are 18.7 per cent and 13.5 per cent, respectively.

The description of the NPI varies greatly between studies and may not be comparable. Chernozhukov et al. (2021) find that ‘employee face masks’ reduces mortality by 34 per cent, and, thus, do not – such as An et al.

74 Indeed, many islands experienced very low COVID-19 mortalities during the pandemic, but this may also be related to a lower initial inflow of infections in the spring of 2020 (also see p. 137).

(2021) – look at a general mask mandate.[75] Spiegel and Tookes (2021) examine both 'employee face masks' and 'mandatory face masks,' finding similar effects. We do not include the estimate on 'mask recommendation' from Leffler et al. (2020) as it is not a mandated NPI, but interestingly Leffler et al. (2020) find that mask recommendations reduced mortality by 23 per cent, which is in the same order of magnitude we find for mask mandates.

***Table 13: Estimates of the effect on COVID-19 mortality of mask mandates***

| Effect on COVID-19 mortality | Description | Standardised estimate (Estimated Averted Deaths / Total Deaths) | Standard error | Weight (1/SE) |
|---|---|---|---|---|
| | **Employee face masks** | **–34.0%** | **8.511%** | **12** |
| **Spiegel and Tookes (2021)** | **Average of below measures** | **–13.5%** | **5.131%** | **19** |
| | *Employee face masks* | –12.2% | 4.272% | 23 |
| | *Mandatory face masks* | –14.9% | 5.866% | 17 |
| **An et al. (2021)** | **Mask mandates** | **–8.5%** | **12.697%** | **8** |
| | *Early mandates* | –8.0% | 17.246% | 5.8 |
| | *Late mandates* | –9.1% | 5.000% | 20 |
| Precision-weighted average (arithmetic average / median) | –18.7% (–18.7%/–13.5%) | | | |
| **Sensitivity analysis (quality-adjusted PWA)** | –19.9% to –12.5% (–18.2%) | | | |

75 Philippe Lemoine 2021 Lockdowns, Econometrics and the Art of Putting Lipstick on a Pig. *CSPI Center* (blog), July 29, 2021 (https://cspicenter.org/blog/waronscience/lockdowns-econometrics-and-the-art-of-putting-lipstick-on-a-pig/) writes about Chernozhukov et al. (2021), noting that 'another reason to regard even this result as dubious is that, when the same analysis is performed to evaluate the effect of mandating face masks for everyone and not just employees of public-facing businesses, the effect totally disappears and is even positive in many specifications. The authors collected data on this broader policy, so they could have performed this analysis in the paper, but they failed to do so despite speculating in the paper that mandating face masks for everyone could have a much larger effect than just mandating them for employees.'

Note: ** (*) denote significance at $p < 0.01$ ($p < 0.05$). For studies with several estimates related to the measure, we report all submeasures in grey but focus on the average of these measures and use the average when calculating the PWA. The average standard error is calculated as $\mathrm{sqrt}((SE_1^2 + SE_2^2 + \ldots + SE_n^2)/k)$, where k is the number of estimates. A negative number corresponds to fewer deaths, so –5% means 5 per cent lower COVID-19 mortality. The quality-adjusted PWA is calculated as the PWA weighted by a quality index, where the score on the quality index for each study is the number of bias dimensions squared (except 'peer-reviewed' and 'social sciences'), where the study is of 'better' quality, see Table 4. Values in grey are not included in the calculations of the precision-weighted average, the arithmetic average, and the median.

Our findings are in contrast to several other reports and studies. WHO (2019) concludes that 'ten RCTs were included in meta-analysis, and there was no evidence that face masks are effective in reducing transmission of laboratory-confirmed influenza.' UK Department of Health, Social Services, and Public Safety (2011) states in its Influenza Pandemic Preparedness Strategy that 'in line with the scientific evidence, the Government will not stockpile facemasks for general use in the community'. Liu et al. (2021) conclude in a review that 'fourteen of sixteen identified randomized controlled trials comparing face masks to no mask controls fail[ed] to find statistically significant benefit in the intent-to-treat populations.' Similarly, a pre-COVID Cochrane review,[76] Jefferson et al. (2020), finds that,

> There is low certainty evidence from nine trials (3,507 participants) that wearing a mask may make little or no difference to the outcome of influenza-like illness (ILI) compared to not wearing a mask (risk ratio (RR) 0.99, 95% confidence interval (CI) 0.82 to 1.18). There is moderate certainty evidence that wearing a mask probably makes little or no difference to the outcome of laboratory-confirmed influenza compared to not wearing a mask (RR 0.91, 95% CI 0.66 to 1.26; 6 trials; 3,005 participants).[77]

76 A Cochrane Review is a systematic review of research in health care and health policy that is published in the Cochrane Database of Systematic Reviews. See https://www.cochranelibrary.com/about/about-cochrane-reviews.

77 Lipp and Edwards (2014) also find no evidence of an effect and – looking at disposable surgical face masks for preventing surgical wound infection in clean surgery – conclude: 'Three trials were included, involving a total of 2113 participants. There was no statistically significant difference in infection rates between the masked and unmasked groups in any of the trials.' Meanwhile, Yanni Li et al. (2021) – based on six case-control studies – conclude: 'In general, wearing a mask was associated with a significantly reduced risk of COVID-19 infection (OR = 0.38, 95% CI: 0.21-0.69, $I^2$ = 54.1%)'.

However, it should be noted that even if no effect is found in controlled settings, this does not necessarily imply that mask mandates do not reduce mortality, as other factors may play a role (e.g., wearing a mask may function as a tax on socialising if people are bothered by wearing a face mask when they socialise, or masks may function as a constant reminder of the presence of the pandemic).

In a cluster-randomised trial of community-level mask promotion in Bangladesh, Abaluck et al. (2022) find that the intervention (which included free masks, information on the importance of masking, role modelling by community leaders, and in-person reminders for 8 weeks) reduced symptomatic seroprevalence by 9.3 per cent.

Another possible explanation is that masks reduce the viral inoculum and that this affects mortality. For example, Bielecki et al. (2021) – in a sort of natural experiment – find that in two groups of Swiss soldiers, soldiers in the one group – those who were *not* physically distancing or wearing surgical masks before being exposed to COVID-19 – had more symptoms (47 per cent) than the other group, who *were* physically distancing and wearing surgical masks before being exposed to COVID-19.

### *Other NPIs (cancelling public events, closing public transportation, restrictions on internal movement, 'lockdown vs. no lockdown')*

Table 14 presents the estimates for NPIs that are only covered by one study, as well as Leffler et al. (2020)'s estimate on lockdown vs. no lockdown. The estimates are all close to zero, but – needless to say – the uncertainty is large. We do note, however, that Leffler et al. (2020) support the other results in the meta-analysis, suggesting the effect of lockdowns of any type is limited.

## Table 14: Estimates of the effect on COVID-19 mortality of other NPIs

| Effect on COVID-19 mortality | Description | Standardised estimate (Estimated Averted Deaths / Total Deaths) | Standard error | Weight (1/SE) |
|---|---|---|---|---|
| **Stokes et al. (2020)** | **Cancelling public events (average)** | **2.0%** | **2.165%** | **46** |
| | *Cancelling public events before 1st death* | –1.5% | 2.324% | 43 |
| | *Cancelling public events after 1st death* | 5.5%** | 1.994% | 50 |
| **Stokes et al. (2020)** | **Closing public transport (average)** | **0.1%** | **2.478%** | **40** |
| | *Closing public transport before 1st death* | 2.0% | 2.659% | 38 |
| | *Closing public transport after 1st death* | –1.7% | 2.282% | 44 |
| **Stokes et al. (2020)** | **Restricting internal movement (average)** | **1.4%** | **2.412%** | **41** |
| | *Restricting internal movement before 1st death* | 0.5% | 2.588% | 39 |
| | *Restricting internal movement after 1st death* | 2.3% | 2.221% | 45 |
| **Leffler et al. (2020)** | **Lockdown of any type** | **1.7%** | **9.015%** | **11** |

Note: ** (*) denote significance at $p < 0.01$ ($p < 0.05$). For studies with several estimates related to the measure, we report all submeasures in grey but focus on the average of these measures and use the average when calculating the PWA. The average standard error is calculated as $\mathrm{sqrt}((SE_1^2 + SE_2^2 + \ldots + SE_n^2)/k)$, where k is the number of estimates. A negative number corresponds to fewer deaths, so –5% means 5 per cent lower COVID-19 mortality. Stokes et al. (2020) consider two distinct time periods of 24 days to account for the changing magnitude of effects of an exponentially transmitted virus: i) 0-24 days and ii) 14-38 after the first COVID-19 death. We report both and use the average in the precision-weighted average.

### *4.4. The overall effect of lockdown policies based on a SIPO and specific NPIs*

#### *Overview of specific NPIs*

Table 15 below summarises our results on a SIPO and the other specific NPIs. The central precision-weighted average in column 2 is small for most NPIs and even positive for limiting gatherings. Only mask mandates seem to have a notable effect on mortality rates, but the estimate is based on just three studies (column 3). Column 4 presents the results of the sensitivity analyses. The precision-weighted averages are generally robust to the sensitivity analyses, and the quality-adjusted PWA generally finds a less promising effect than the precision-weighted average ('business closures' is the only NPI where the quality-adjusted PWA is 'preferable' to the precision-weighted average).

**Table 15: Summary of estimates of specific non-pharmaceutical interventions (NPIs)**

| **1. NPI (number of studies)** | **2. Precision-weighted average (PWA)** | **3. Sensitivity analysis** | **4. Quality-adjusted PWA** |
|---|---|---|---|
| SIPO (12) | –2.0% | –4.1% to –1.4% | –1.8% |
| Business closures (5) | –7.5% | –9.3% to –6.6% | –11.9% |
| School closures (4) | –5.9% | –6.2% to –2.5% | –1.2% |
| Limiting gatherings (4) | 5.9% | 4.9% to 8.9% | 9.8% |
| Travel restrictions (5) | –3.4% | –4.7% to –0.4% | 1.1% |
| Mask mandates (3) | –18.7% | –19.9% to –12.5% | –18.2% |
| Public events cancellation (1) | 2.0% | n/a | 2.0% |
| Public transportation closures (1) | 0.1% | n/a | 0.1% |
| Internal movement restrictions (1) | 1.4% | n/a | 1.4% |

Note: The table summarises the precision-weighted averages from Table 7, Table 9, Table 10, Table 11, Table 12, Table 13, and Table 14.

***The average effect of lockdowns during spring of 2020***

The overview in Table 15 allows us to estimate the effect of average lockdown policies in the spring of 2020. First, we use OxCGRT data to calculate the share of the population that faced each of the NPIs from Table 15 in the spring of 2020. We focus on NPIs in the period between 16 March and 15 April 2020, which would have the greatest impact on deaths, since death rates flattened after this period.

We only look at whether each NPI was mandated or not, and not whether it was strict or more lenient (e.g., we code both '2 - Require closing only some levels or categories, e.g., just high school, or just public schools' and '3 - Require closing all levels' as 'closed schools'). This means that we overestimate the effect if stricter NPIs are more effective than more lenient NPIs. Also, as mentioned earlier, each precision-weighted average risks to be biased towards a larger effect, since the estimate in each study may capture the effect of multiple (omitted) NPIs (also see footnote 68).

Based on this approach and with the bias towards overestimating the effect of lockdowns in mind, Table 16 presents the effect of the average lockdown in the spring of 2020. Our calculations suggest that the average lockdown in Europe and the United States – based on estimates for specific NPIs – reduced COVID-19 mortality rates by 10.7 per cent (precision-weighted average) with a range in the sensitivity analysis of 0.7 per cent (worst case) to 16.0 per cent (best case). The quality-adjusted PWA is a 3.2 per cent reduction in mortality rates. This precision-weighted average of 10.7 per cent is larger than the effect found in the studies based on the OxCGRT stringency index (3.2 per cent reduction), but still relatively small and far from the large effects promised by many epidemiological models early in the pandemic, such as Ferguson et al. (2020). To put the estimate in perspective, there were 188,542 registered COVID-19 deaths in Europe and 128,063 COVID-19 deaths in the United States by 30 June 2020. Thus, the 10.7 per cent corresponds to 23,000 avoided COVID-19 deaths in Europe (best case: 26,000 avoided deaths, worst case: 1,000 avoided deaths) and 16,000 avoided COVID-19 deaths in the United States (best case: 25,000 avoided deaths, worst case: 1,000 avoided deaths). In comparison, there are approximately 72,000 flu deaths in Europe and 38,000 flu deaths in the United States each year.[78] Given these data, it is

78 The average estimated flu deaths in the United States in the five years prior to COVID-19 were 38,400 according to the CDC (2022), and the WHO (2022) writes that there are 72,000 flu deaths in Europe each year.

clear that the effects of lockdowns have been negligible from a health policy perspective.

**Table 16: Estimates of the effect on COVID-19 mortality of the average lockdown in Europe and in the United States from studies based on specific NPIs**

| 1. NPI (number of studies) | 2. Share of time with mandate (population weighted) | 3. Impact on mortality (PWA · share) | 4. Sensitivity analysis (best case to worst case) | 5. Quality-adjusted PWA |
|---|---|---|---|---|
| SIPO (12) | 70% | –1.4% | –2.9% to –1.0% | –1.2% |
| Business closures (5) | 92% | –6.9% | –8.6% to –6.1% | –10.9% |
| School closures (4) | 97% | –5.7% | –6.0% to –2.4% | –1.1% |
| Limiting gatherings (4) | 95% | 5.6% | 4.7% to 8.4% | 9.3% |
| Travel restrictions (5) | 93% | –3.1% | –4.3% to –0.4% | 1.0% |
| Mask mandates (3) | 10% | –1.9% | –2.1% to –1.3% | –1.9% |
| Public events cancellation (1) | 95% | 1.9% | 1.9% to 1.9% | 1.9% |
| Public transportation closures (1) | 14% | 0.0% | 0.0% to 0.0% | 0.0% |
| Internal movement restrictions (1) | 64% | 0.9% | 0.9% to 0.9% | 0.9% |
| **Total impact of the average lockdown policy** | | **–10.7%** | **–16.0% to –0.7%** | **–3.2%** |

Note: Column 2 shows the share of the time between 16 March and 15 April 2020, where each NPI was implemented. Column 3 shows the impact of the NPI given the precision-weighted average (PWA) in Table 15 and the share of the time the NPI was implemented, see Column 2. Column 4 shows the best case (where all PWAs are in the lower end of the sensitivity analysis) and the worst case (where all PWAs are in the upper end of the sensitivity analysis), and Column 5 shows the quality-adjusted PWA. The total impact of the average lockdown policy is calculated as the product of (1 – (estimates in column 3)) – 1. Implementing all NPIs has an impact of –26.4%.

# 5. Concluding observations

Public health experts and politicians have – based on forecasts in epidemiological studies such as that of Imperial College London (Ferguson et al. 2020) – embraced compulsory lockdowns as an effective method for arresting the pandemic. But, have these lockdown policies really been effective in curbing COVID-19 mortality? This question is answered by our meta-analysis.

Adopting a systematic search and title-based screening, we identified 1,220 studies that potentially look at the effect of lockdowns on mortality rates. To answer our question, we focused on studies that examine the actual impact of lockdowns on COVID-19 mortality rates based on registered cross-sectional mortality data and a counterfactual difference-in-difference approach. Out of the 1,220 studies, 32 met our eligibility criteria, and standardised estimates for our meta-analysis could be calculated for 22 of the eligible studies.

## *5.1. Conclusions*

Overall, our meta-analysis fails to confirm the notion that lockdowns – at least in the spring of 2020 – had a large, significant effect on mortality rates. Studies examining the relationship between lockdown strictness and mortality (based on the OxCGRT stringency index) find that the average lockdown in Europe and the United States only reduced COVID-19 mortality by 3.2 per cent compared to the most lenient COVID-19 policy. Shelter-in-place orders (SIPOs) were also ineffective. They only reduced COVID-19 mortality by 2.0 per cent. Based on nine specific NPIs, we estimate that the average lockdown in Europe and the United States in the spring of 2020 reduced mortality by 10.7 per cent. The 3.2 per cent to 10.7 per cent corresponds to 6,000-23,000 avoided deaths in Europe and 4,000-16,000 avoided deaths in the United States.

In comparison, there are approximately 72,000 flu deaths in Europe and 38,000 flu deaths in the United States each year.[79] Thus, lockdowns in Europe and the United States on average saved lives corresponding to 9 per cent to 35 per cent of an average flu season.

Of the specific NPIs, closing non-essential businesses seems to have had some effect (reducing COVID-19 mortality by 7.5 per cent), which is possibly related to the closure of bars. We find that mask mandates had the largest effect (reducing COVID-19 mortality by 18.7 per cent), but the estimate is based on just three studies with heterogeneity in the definition of the mandate. Limiting gatherings was counterproductive and *increased* mortality by 5.9 per cent.

Our measured meta-results are supported by the natural experiments we have been able to identify through our work and by searches in the abstract and citation database Scopus (see Table 17).

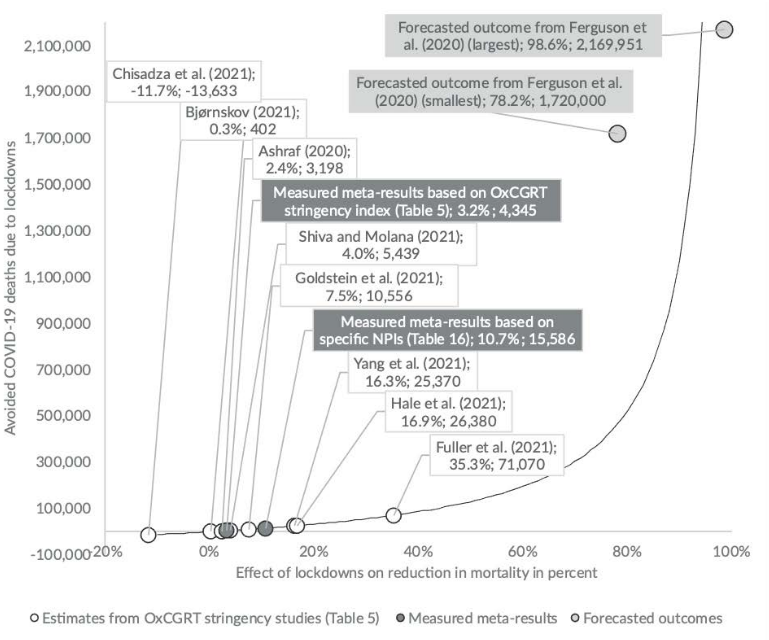
Overall, our meta-analysis supports the conclusion that lockdowns – at least in the spring of 2020 – had a negligible effect on COVID-19 mortality.

Throughout the meta-analysis, we have focused on the precision-weighted average as our primary indicator of the efficacy of lockdowns. However, as shown in Figure 10, the overall conclusion holds regardless of which studies or measures one chooses to emphasise. Figure 10 presents the effect on mortality in the United States based on the measured estimates from all stringency studies as well as our two central measured estimates for the effect of lockdowns in the spring of 2020 (the precision-weighted average from the stringency studies in Table 5 and the estimate based on specific NPIs in Table 16). We have added the maximum and minimum forecasted estimates from Ferguson et al. (2020) for comparison.

79 The average estimated flu deaths in the United States in the five years prior to COVID-19 were 38,400 according to the CDC (2022), and the WHO (2022) writes that there are 72,000 flu deaths in Europe each year.

Even if we cherry-pick the most preferable empirical estimate from Fuller et al. (2021), the effect on mortality is far from the effects promised based on Ferguson et al. (2020) and corresponds to the mortality from less than two influenza seasons.[80]

**Figure 10: Divergence between avoided number of deaths in the United States as measured by the meta-results, studies based on the OxCGRT stringency index, and the forecasted outcome from Imperial College London**




Note: The estimates in the group 'Estimates from stringency studies' are from Table 5. For the group 'Meta-study PWA' the estimate for 'PWA stringency studies' is from Table 5, and 'average lockdown spring 2020' is from Table 16. Both estimates illustrate the effect of the average lockdown in Europe and United States in the spring of 2020. The effect of lockdowns on total mortality based on the meta-study's precision-weighted averages (PWA) is calculated as total COVID-19 deaths by 1 July 2020 (128,063 COVID-19 deaths) x (1/(1-PWA)-1). The relative effect of lockdowns on total mortality based on Ferguson et al. (2020) is calculated as the

80 In the five years prior to COVID-19, the flu caused 38,400 deaths annually on average according to CDC (2022).

largest and smallest predicted relative effect multiplied with their mortality estimate of 2.2 million deaths in a 'do nothing' scenario in United States. The estimates from Ferguson et al. (2020) is for a two-year period, but the relative effect is largest early in the pandemic.

### *Limitations*

Our study has a number of limitations. Most importantly, we cannot say whether NPIs are crucial as a means of signalling the danger of the pandemic. It is possible that – even if stricter lockdowns and each individual NPI are ineffective – the government 'doing something' is necessary to spur voluntary behavioural changes. The question is whether people would have understood how serious the situation was if governments had not resorted to measures that went beyond the usual. If the answer to this question is that some sort of lockdown was indeed necessary (and there is some evidence suggesting this),[81] the next obvious question is: How little intervention does it take to send the necessary signal? Our results suggest that the answer to this question is 'relatively little', but how to send the best signal with as little cost to society as possible, is an obvious area for future research.

Another limitation is the limited number of studies. This is especially true for the specific NPIs where our estimates are often based on 3-5 studies. We hope that our work will inspire more researchers to think about ways to examine the effect of specific NPIs so that future meta-analyses have more studies to rely on.

Also, we do not analyse the role of timing. As pointed out in section 2.2, p. 42 , and section 5.2.4, p. 126 , we believe that many studies examining the role of timing are fundamentally flawed because they do not distinguish between voluntary behavioural changes and lockdowns (i.e., mandatory behavioural changes). We also point out that even if there *is* an optimal timing, we cannot be sure that democratic governments will ever be able to react in time to this information and implement the NPIs accordingly. However, these are problems that may be addressed by future research. In any case, researchers should find ways to distinguish between the

81 For example, using survey data collected immediately (same day) before and after Boris Johnson announced the UK lockdown, Eggers and Harding (2021) find that 'the lockdown announcement made people more supportive of the government's response to the crisis but also (perhaps surprisingly) more concerned about the pandemic.' E.g., people responding after the announcement were more likely to respond, 'I fear for my future' and 'I have started not going out at all.'

effects of voluntary behavioural changes (possibly spurred by signalling) and those of lockdowns.

Our meta-analysis shows that some NPIs may have a measurable effect on mortality. Mask mandates especially look relatively effective, although our estimate is based on just three studies and 18.7 per cent is still a relatively low effect compared to the effects promised by many epidemiological models early in the pandemic.[82] However, the fact that an NPI has a measurable effect on mortality is a necessary – but not sufficient – requirement to make the policy beneficial and desirable. Also, there is some evidence that mask recommendations can be sufficient to reap much of the effect of mask mandates. Thus, future research is needed to estimate the broader costs of mask mandates – including effects on welfare, trust etc. – before one can conduct an actual cost–benefit analysis, which can answer whether mask mandates are a desirable policy.

We only look at mortality. It is possible that there are other benefits related to lockdowns that are not captured in the studies looking at mortality rates. For example, Banholzer et al. (2022) believe that 'interventions that reduce the number of new infections can have downstream effects on various outcomes, including disease-related deaths, cases of severe illness and hospitalizations, cases with long-term health effects after infection, the efficiency of testing and contact tracing'. While this may be true, we believe it is unlikely that the effect of lockdowns on infections has been so different from the effect of lockdowns on mortality that it changes the overall conclusion. Even if the effect on infections is two or three times larger than the effect on deaths, the overall effect is limited and far from the effect promised based on model studies (see Figure 10). However, only future research can tell whether this immediate assessment holds true.

We also restrict our search strategy to studies using a 'counterfactual difference-in-difference approach'. We believe difference-in-difference studies are better suited than other widely used empirical methods to examine the true effect of lockdowns because they allow us to leave out the effect of voluntary behaviour changes. There is, however, no doubt that the results from other study methods are of great interest because

82 Some have argued that mask mandates may be an efficient regulation during future influenza seasons. However, an effect of 18.7% will only reduce the number of deaths by approximately 25.000 in Europe and the United States combined during an average flu season. Although this *is* a large benefit, it should be compared to the economic cost of mandating more than 1 billion people to wear face masks.

they can give us insights with regard to, e.g., the importance of voluntary behaviour changes. We welcome future research on these other methodologies.

### *5.2. Discussion*

#### *5.2.1 Conclusions are in line with other reviews*

Overall, we conclude that stricter lockdowns are not an effective way of reducing mortality rates during a pandemic, at least they were not during the first wave of the COVID-19 pandemic. Our results are in line with the World Health Organization Writing Group (2006), stating,

> Reports from the 1918 influenza pandemic indicate that social distancing measures did not stop or appear to dramatically reduce transmission […] In Edmonton, Canada, isolation and quarantine were instituted; public meetings were banned; schools, churches, colleges, theaters, and other public gathering places were closed; and business hours were restricted without obvious impact on the epidemic.

Our findings are also in line with the conclusion in Allen (2021): 'The most recent research has shown that lockdowns have had, at best, a marginal effect on the number of Covid-19 deaths.' Poeschl and Larsen (2021) conclude that 'interventions are generally effective in mitigating COVID-19 spread,' but 9 of the 43 (21 per cent) results they review find 'no or uncertain association' between lockdowns and the spread of COVID-19,[83] suggesting that the impact of lockdowns is limited and not that far from zero, which contradicts their conclusion.[84] Based on two interrupted time-series studies, Iezadi et al. (2021) find that overall NPIs reduced daily ICU admissions by 16.5 per cent. Mendez-Brito et al. (2021) find that school closing is the most effective measure, although only fourteen out of 24 studies (58 per cent) found an association between school closures and number of cases,

83 We are uncertain if these numbers are correct, as Poeschl and Larsen (2021) list only one study examining bar closures in their overview table, although they review two studies. Also, Poeschl and Larsen (2021) do not look at business closures, and at least one of their studies examines this. Weber (2020) writes that 'In an estimation without the non-positivity constraints, the sum of all sector closure effects is insignificant at the one percent level.'

84 If the true estimate was far from zero, we would expect to see relatively few estimates that are zero or positive (more deaths). If, on the other hand, the true value is around 0, we would expect to see that approximately half of the 'guesses' are greater than zero, while half are lower than zero.

suggesting a limited effect.[85] Herby (2021) concludes that ‘mandated behavior changes accounts for only 9% (median: 0%) of the total effect on the growth of the pandemic stemming from behavioral changes. The remaining 91% (median: 100%) of the effect was due to voluntary behavior changes.’

The findings contained in Johanna et al. (2020) are in contrast to our results. They conclude that ‘for lockdown, ten studies consistently showed that it successfully reduced the incidence, onward transmission, and mortality rate of COVID-19’. The driver of the difference is threefold. First, Johanna et al. (2020) include modelling studies (ten out of a total of fourteen studies), which we have explicitly excluded. Second, they included interrupted time-series studies (three of fourteen studies), which we also exclude. Third, the only study using a difference-in-difference approach (as we have done) is based on data collected before 1 May 2020. We should mention that our results indicate that early studies find relatively larger effects compared to later studies.

### *5.2.2. Causality or correlation?*

As pointed out by Bjørnskov (2021), there is a potential endogeneity problem (also referred to as reverse causality):

> which derives from the nature of political reactions to the virus that could rely on the reported number of infections. If an increase in the reported infection rate leads government to introduce lockdown policies, and if a declining reported infection rate subsequently leads them to ease the lockdown, the estimated association between policy stringency and mortality is biased.

Several studies explicitly claim that they examine the actual causal relationship between lockdowns and COVID-19 mortality. Some studies use instrumental variables (e.g., Bjørnskov 2021), lagged dependents (e.g., Goldstein et al. 2021), or other techniques to establish a causal

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85 Talic et al. (2021) is another systematic review and meta-analysis that looks at the effectiveness of public health measures in reducing the incidence of COVID-19. However, their focus is on voluntary measures. They state that ‘the findings of this review suggest that personal and social measures, including handwashing, mask wearing, and physical distancing are effective at reducing the incidence of covid-19. More stringent measures, such as lockdowns and closures of borders, schools, and workplaces need to be carefully assessed by weighing the potential negative effects of these measures on general populations. Further research is needed to assess the effectiveness of public health measures after adequate vaccination coverage.’

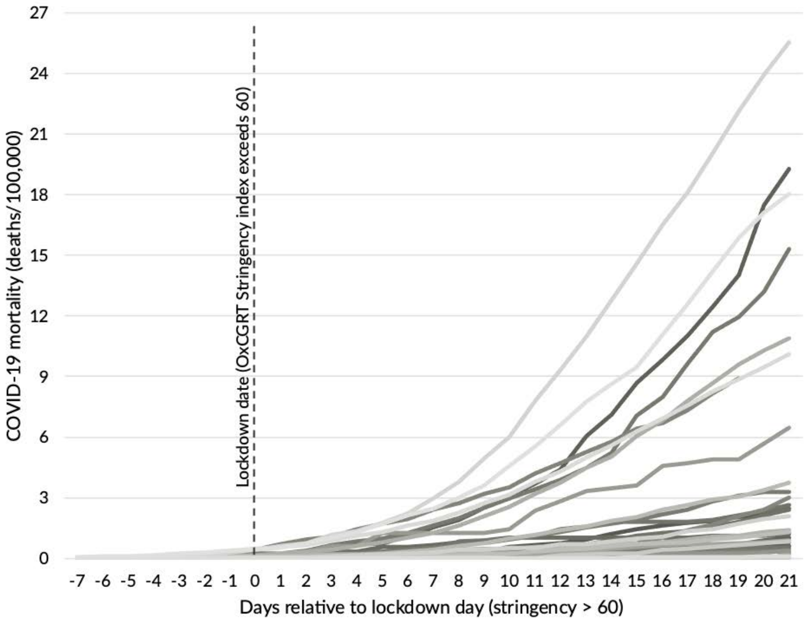
relationship, while others make causality probable using arguments.[86] Sebhatu et al. (2020) show that government policies are strongly driven by the policies initiated in neighbouring countries rather than by the severity of the pandemic in their own countries. In short, Sebhatu et al. (2020) show that it is not the severity of the pandemic that drives the adoption of lockdowns, but rather the propensity to copy policies initiated by neighbouring countries. Similar results are found by Engler et al. (2021). This suggests that an availability cascade, as described by Kuran and Sunstein (2007), where an idea gains widespread acceptance and influence because it is repeatedly and prominently presented in a self-reinforcing process, was driving public policy.

Sebhatu et al. (2020) also find that the death rate predicts the stringency of countries' policy adoptions, but the effect is small, explaining only 2.1 stringency points on average (in comparison, the gap between the strictest and most lenient lockdowns in Europe was between 67 and 92 stringency points in the period from 16 March to 15 April 2020).[87] The very low mortality rates on the day of lockdown (defined as the day when the OxCGRT stringency index exceeds 60) are illustrated in Figure 11.

86 E.g., Dave et al. (2021) state that 'estimated case reductions accelerate over time, becoming largest after 20 days following enactment of a SIPO. These findings are consistent with a causal interpretation.'

87 Sebhatu et al. (2020) estimate that the impact of 'death rate (/100,000)' on stringency is 11.706 (Table S3) and highly significant. On average, the death rate (/100,000) was 0.18 (Table S4) meaning that the average explanatory power was 2.1 stringency points. According to OxCGRT, the average death rate (/100,000) in Europe was approximately 0.08 and 0.19 when reaching stringency 60 and 70, respectively. Only San Marino had a substantial death rate and – given the estimate from Sebhatu et al. (2020) – a substantial impact on stringency when reaching stringency 60 (and Netherlands and Spain when reaching stringency 70). In San Marino, the Netherlands, and Spain the total impact measured as a death rate estimate was 34, 5, and 2 respectively when reaching stringency index 60 and 172, 15, and 13 respectively when reaching stringency index 70. No other country had an impact above 5 points.

***Figure 11: Mortality rates in European countries were very low prior to lockdown decisions***




*Source: Our World in Data (2022).*

Note: The figure shows the development in mortality in 37 European countries with more than 100,000 inhabitants. Day 0 is the day where the OxCGRT stringency index crosses 60.

It is worth noting that Figure 11 overstates the severity of the pandemic at the time when lockdown decisions were made. These decisions were often made days before implementation. So, mortality rates were lower on the date of decision than on the date of implementation. This lag between decision and implementation is non-negligible. On average, mortality rates were about 50 per cent lower three days prior to lockdown and 75 per cent lower five days prior to lockdown. In short, governments had very little hard data and were making decisions relative to lockdowns based on epidemiological modelling or, in many cases, just following the policies introduced by neighbouring countries.

Also, most eligible studies examine the first wave, when most countries – due to limited testing capabilities – had little information about the progress of the pandemic, and thus, the policy response in a given country is unlikely to be greatly affected by the severity of the pandemic. Bjørnskov (2021) points to governments' ability to react quickly and notes:

> Although one might think that policy making reacts quickly to changing mortality during an emergency, exploring the determinants of changes in the stringency indices reveals that an increase in the contemporaneous mortality or an increase in the reported number of Sars-CoV-2 cases was not associated with stricter lockdown measures.

And he concludes that:

> it is highly unlikely that there is a substantial endogeneity problem in the following as mortality changes only affect policy changes with a three-week lag, and as policy changes cannot affect the mortality rate before another two to three weeks have passed. As such, any bias is likely to be small and practically negligible.

Finally, eleven of the 22 studies in the meta-analysis address the causality question, but their results are not much different from the other eleven studies, implying that causality is not a major problem.

Hence, we believe there is a strong case for a causal relationship in our results and that what the studies examine is the effect (of the strictness) of lockdowns on mortality and not the opposite (mortality rates' effect on (the strictness of) lockdowns), although the issue can never be finally settled with observational studies.[88]

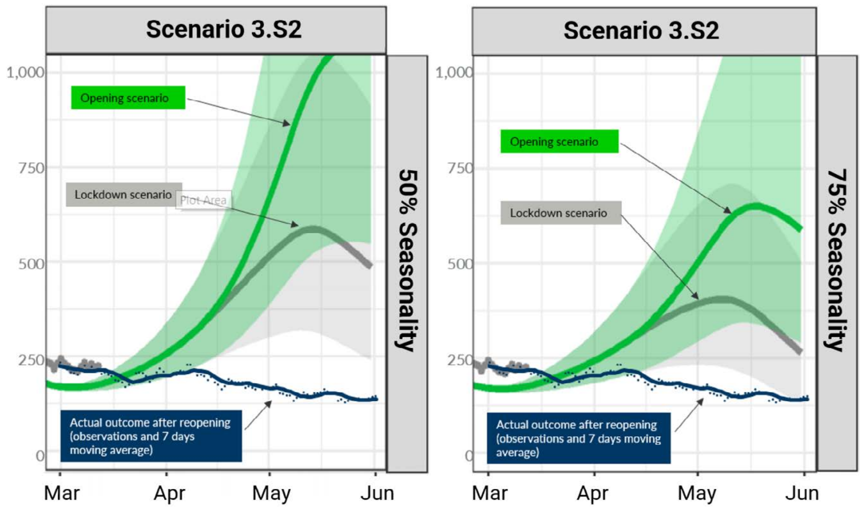
### *5.2.3. Why are the effects of lockdowns limited?*

Our main conclusion invites a discussion of some issues. Our review does not point out *why* lockdowns did not have the effect promised by the epidemiological models of Imperial College London (Ferguson et al. 2020). But it is evident that modellers around the globe failed to accurately forecast the development of the pandemic.

88 The 'RCT-like' studies, we have identified, support our conclusions based on observational studies. See Table 17.

One example is the projections of COVID-19 inpatients that were published during the Danish negotiations for a reopening in the spring of 2021. Figure 12 below compares the projections to actual data following the reopening. Not only did the modellers fail to project the number of COVID-19 inpatients following the reopening of the economy, but the actual outcome was below even the most optimistic lockdown scenario (the lower bound of the grey shaded area). It should be noted that this forecasting failure was in no way unique to the Danish health authorities. Most health authorities and expert modellers failed to correctly project the development of the pandemic.[89]

**Figure 12: Model forecasts of COVID-19 inpatients with and without reopening compared to actual outcomes (Denmark, 2021)**




Source: Statens Serum Institut (2021) and Danmarks Statistik (2022).

Note: The green line shows the projections if the economy was reopened, while the grey line shows the projections with continued lockdown. The blue dots and line show the actual development after the economy was reopened. '50% seasonality' and '75% seasonality' denotes how much seasonal variation is included. Text has been translated from Danish to English.

89 See, for example, https://www.spectator.co.uk/article/how-did-sage-scenarios-compare-to-reality-an-update and https://www.svd.se/hundratusen-skulle-do--modellen-slog-fel.

We propose four factors that might explain the difference between our conclusions and the views embraced by some epidemiologists.

### *People respond voluntarily to dangers*

First, people respond to dangers outside their door when they are aware of them. When a pandemic rages, people engage in social distancing regardless of what the government mandates. In economic terms, you can say that the demand for costly disease prevention efforts such as social distancing and increased focus on hygiene is high when infection rates are high.[90] On the contrary, when infection rates are low, the demand is low and it may even be morally and economically rational not to comply with mandates such as SIPOs, which are difficult to enforce.

Herby (2021) reviews studies that distinguish between mandatory and voluntary behavioural changes. He finds that – on average – voluntary behavioural changes are 10 times as important as mandatory behavioural changes in combating COVID-19. Andersen et al. (2020) find that consumer spending fell almost as much in Sweden as in Denmark despite Sweden having a very limited lockdown.[91] Interestingly, the response in Sweden was especially large among 70+-year-olds and even larger than in Denmark – possibly due to the larger outbreak in Sweden. Chetty et al. (2020) show that high-income individuals reduced spending sharply in mid-March 2020, particularly in areas with high rates of COVID-19 infection and in sectors that require physical interaction. By comparing counties with and without restrictions, Goolsbee and Syverson (2021) conclude that only 7 per cent-points of the 60 per cent-point decline in business activity could be attributed to legal restrictions and that the shift was highly tied to the number of COVID deaths in the county. Most of the decline resulted from consumers voluntarily choosing to avoid stores and restaurants. The point from Andersen et al. (2020), Chetty et al. (2020) and Goolsbee and Syverson (2021) is illustrated in Figure 13 from Maas (2020).[92]

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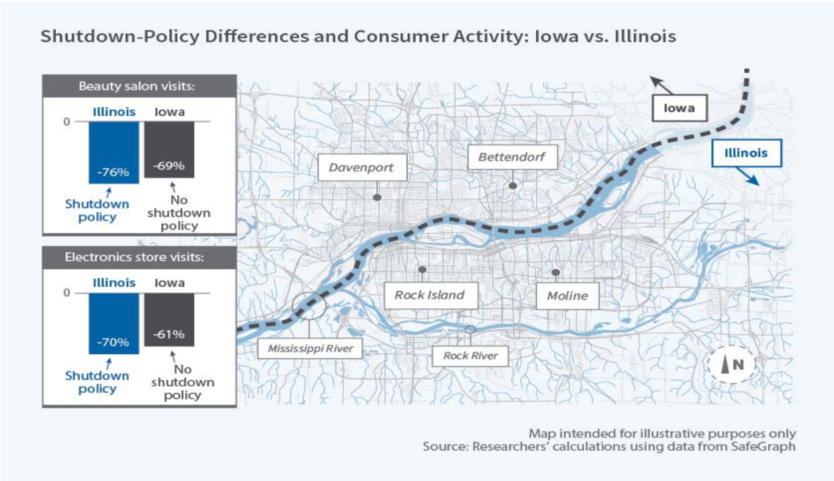
90 In a randomized control trial, Helsingen, Løberg et al. (2020) find that ‘provided good hygiene and social distancing measures, there was no increased COVID-19 spread at training facilities.’ Their study shows that many activities can be safe with a focus on hygiene and social distancing.

91 Andersen et al. (2020) analyse transaction data for Denmark and Sweden from a large bank in Scandinavia to reach this conclusion.

92 Steve Maas 2020 Consumers’ Fear of Virus Outweighs Lockdown’s Impact on Business. *NBER* (blog), August 2020 (https://www.nber.org/digest/aug20/consumers-fear-virus-outweighs-lockdowns-impact-business).

**Figure 13: Lockdown-policy differences and consumer activity in Iowa and Illinois**




Source: Maas (2020) based on data from SafeGraph.

Gupta et al. (2020) find that 'information-based policies and events such as first cases had the largest effects'. They also find that SIPOs do not affect social distancing (as measured by a mixing index which – based on cell phone data – measures the exposure of a smart device to other devices). At a first glance, this seems to be in conflict with the findings by Joshi and Musalem (2021) who find that SIPOs increase time spent at home, but one obvious explanation is that people stop mixing voluntarily before they are mandated to stay at home, so the SIPOs do not increase social distancing (and thus, reduce infections) but only increase time spent at home. After all, there are several ways you can leave your house without mixing with others. Bor et al. (2021b) find that intrinsic motivations related to the severity of the pandemic (as measured by national case numbers) play a significant role when citizens increase their attention to the health authorities' advice during an epidemic.

These voluntary behavioural changes may also explain why epidemiological model simulations such as Ferguson et al. (2020) – which do not model behaviour endogenously[93] – fail to forecast the effect of lockdowns. Voluntary behavioural changes also explain why the flu almost disappeared in Denmark in March 2020 before a single restriction was implemented. In Denmark, schools closed by 16 March and businesses by 18 March. But at these dates the share of positive influenza tests in Denmark had dropped from about 25 per cent – a level that had been more or less constant for two months – before 11 March, when the Danish prime minister held a press conference, to 5%-10% (Statens Serum Institut 2020). As we showed in Figure 8, p. 47 , the same pattern was seen in Norway and Sweden (Emborg et al. 2021).

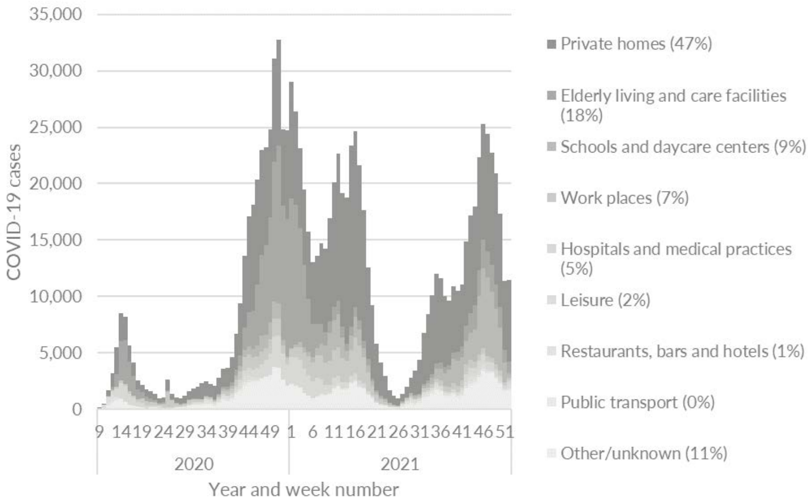
In the United States, Ziedan et al. (2020) find that ‘aggregate trends in outpatient visits show a 40% decline after the first week of March 2020, only a portion of which is attributed to state policy.’ Tsai and Tzu-Ting (2021) find similar results for Taiwan. Overall, we believe that Allen (2021) is correct when he concludes, ‘The ineffectiveness [of lockdowns] stemmed from individual changes in behavior: either non-compliance or behavior that mimicked lockdowns.’

### *Mandates only regulate a fraction of our potential contagious contacts*

Second, mandates only regulate a fraction of our potential contagious contacts. Figure 14 illustrates infection locations in Germany during the early pandemic. It shows that most of the infections in Germany assigned to an outbreak (defined as at least two cases) occurred in homes (including homes for the elderly), hospitals, and workplaces that were not subject to general restrictions applied throughout society and where potentially effective interventions, such as handwashing, coughing etiquette, ventilation, distancing etc. could neither be regulated nor enforced but relied solely on voluntary behavioural changes. In total, 77 per cent of infections occurred in homes, hospitals, and workplaces, and the share of infections in homes, hospitals, and workplaces was large (above 60 per cent) despite variations in the use of NPIs.

93 In fact, Ferguson et al. (2020) describe their results as ‘unlikely’, as they are based on the assumption of the ‘absence of any […] spontaneous changes in individual behavior’.

**Figure 14: Homes, hospitals, and workplaces were the main drivers of infections in Germany and the location for 77 per cent of all infections**




Source: Robert Koch Institut (2022)

Note: Laboratory-confirmed COVID-19 cases assigned to an outbreak by infection setting and reporting week. Number in parentheses is the share of total cases during the covered period. The data covers COVID-19 outbreaks with two or more cases which includes about 15 per cent of all cases of infection in Germany.

The data in Figure 14 only covers COVID-19 outbreaks with two or more cases, which includes about 15 per cent of all cases of infection in Germany. But data from other countries shows similar patterns. Lee et al. (2020) write that ‘early contact tracing studies and a large study of more than 59,000 case contacts in South Korea found household contacts to be greater than six times more likely to be infected with SARS-CoV-2 than other close contacts’ and Zhao et al. (2020) find that ‘69.2% of total cases were clustered in a home, apartment or residential estate.’

In a Danish matched case-control study based on data from November 2020, Munch et al. (2022) find that contact with an infected person at home or at work is a substantial risk factor.[94] They find no infection risk associated with community exposures such as shopping at supermarkets, travelling by public transport, dining at restaurants, and attending private social events with few participants.

The results were confirmed by Lendorf (2021) who found that 69 per cent were infected in places with no general restrictions.[95] Both Munch et al. (2022) and Lendorf (2021) are based on data from a period with relatively few infections and some restrictions (gatherings limited to ten persons, restaurants and bars etc. had to close at 10 p.m., and face masks were mandatory indoors, except when seated). Nevertheless, their results illustrate that people in countries such as Denmark, Finland, and Norway – countries that allowed people to go to work, use public transport, and meet privately at home during the pandemic – had ample opportunities to legally meet with – and get infected by – others. Still, these countries experienced relatively low COVID-19 mortality rates.

### *Behavioural responses may counteract any initial effect of lockdowns*

Third, even if lockdowns are successful in initially reducing the spread of COVID-19, the behavioural responses may counteract the effect as people respond to the lower risk by changing behaviour. As Atkeson (2021) points out, the economic intuition is straightforward. If closing bars and restaurants causes the prevalence of the disease to fall towards zero, the demand for costly disease prevention efforts such as social distancing and increased focus on hygiene also falls towards zero, and the disease will return.[96]

As pointed out by Deaton and Cartwright (2018), randomisation 'does not relieve us of the need to think about (observed or unobserved) covariates'. Also, this kind of second-order behaviour response may also explain why closing down non-essential businesses simply reallocates consumer visits away from 'nonessential' to 'essential' businesses, as shown by Goolsbee and Syverson (2021), with limited impact on the total number of

94 Munch et al. (2022) write 'contact (OR 4.9, 95% CI 2.4–10) and *close contact* (OR 13, 95% CI 6.7–25) with a person with a known SARS-CoV-2 infection were main determinants. Contact most often took place in the household or work place.'

95 In particular, 23 per cent were infected in the household, 27 per cent were infected at work, and 19 per cent were infected by close acquaintances.

96 This kind of behaviour response may also explain why Subramanian and Kumar (2021) find that increases in COVID-19 cases are unrelated to levels of vaccination across 68 countries and 2,947 counties in the United States. When people are vaccinated and protected against severe disease, they have less reason to be careful.

contacts.[97] And this probable behaviour response to changes in infection levels limits the knowledge we can obtain from randomised control trials examining specific NPIs (if, for example, masking children in schools reduces the infections among children and teachers, this does not necessarily imply that masking children reduces infection rates overall). Also, Joshi and Musalem (2021) find that the effect of SIPOs on mobility decreases as time passes and infection rates drop.

***Some NPIs may have led to unintended consequences***

Fourth, unintended consequences may play a larger role than recognised. We already pointed to the possible unintended consequence of SIPOs, which may isolate an infected person at home with his or her family where he or she risks infecting family members with a higher viral load, causing a more severe illness. But often, lockdowns have limited people's access to safe (outdoor) places such as beaches, parks, and zoos or included outdoor mask mandates or strict outdoor gathering restrictions, pushing people to meet at less safe (indoor) places. Indeed, we do find evidence that limiting gatherings was counterproductive and increased COVID-19 mortality by 5.9 per cent (see Table 11).

### *5.2.4. Objections to the results of the meta-analysis*

Our results and conclusions go against the conventional wisdom that lockdowns were effective in reducing COVID-19 mortality, and, indeed, the first version of our study actually gave rise to a wide range of objections to our measured meta-results. We address the most important of these in this section.

***The 'timing of lockdowns is crucial' objection***

One objection to our conclusions is that we do not look at the role of timing. If timing is very important, differences in timing may empirically overrule any differences in lockdowns. We first note that this objection does not necessarily contradict our results. If timing is very important relative to strictness, this suggests that well-timed, but very mild, lockdowns should work as well as, or better than, less well-timed but strict lockdowns.

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97 In economic terms, lockdowns are substitutes for – not complements to – voluntary behavioral changes.

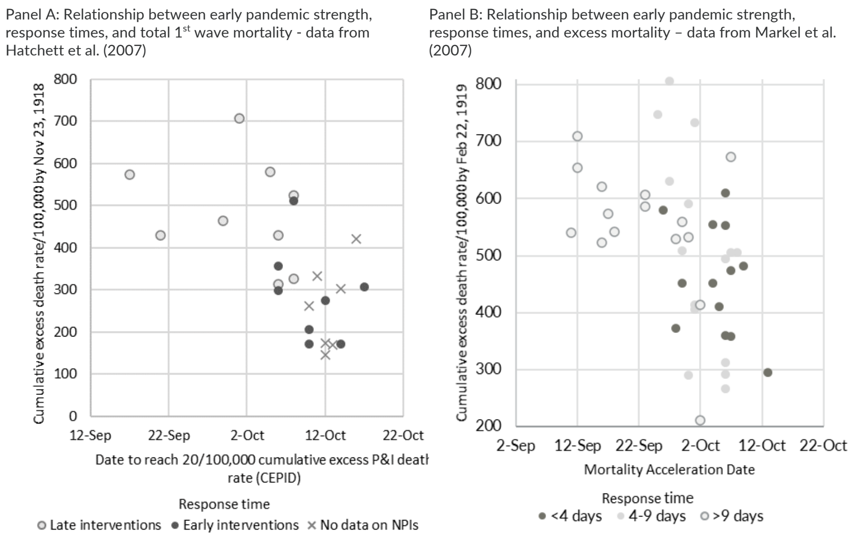
This is not in contrast to our conclusion, as the studies we reviewed analyse the effect of lockdowns when compared to doing very little (see section 3.1 p. 50 for further discussion). However, there is little solid evidence supporting the timing thesis, because it is inherently difficult to analyse (see Section 2.2 p. 42 for further discussion).

In Figure 7, we show that all countries and states that were hit late by the pandemic experienced low COVID-19 mortality rates. This pattern – where areas hit late in the pandemic also have lower death rates – was also found during the Spanish Flu in 1918. Figure 15 shows how cities in the United States that were hit early by the Spanish Flu in the autumn of 1918 also experienced high excess mortality. The data is from Hatchett et al. (2007) and Markel et al. (2007), who both conclude that the low excess mortality was due to lockdowns early in the pandemic. But, as Figure 7 clearly shows, cities that implemented lockdowns early in the pandemic were also hit relatively late compared to other cities, making it difficult to assess whether the measured effect of early lockdowns is related to lockdowns or is related to voluntary behaviour changes instead.

Hatchett et al. (2007) touch on the subject by noting that 'in addition, cities whose epidemics began later tended to intervene at an earlier stage of their epidemics […], presumably because local officials in these cities observed the effects of the epidemic along the Eastern seaboard and resolved to act quickly'. But if local officials observed the effects on the east coast, many ordinary people probably did too, spurring – or at least laying the groundwork for – voluntary behavioural changes. Indeed, if we exclude cities that were hit early, the average excess mortality in Markel et al. (2007) is similar across response times, indicating that information and voluntary behavioural changes could be driving their results.[98]

98 For cities with a response time <4 days, the average excess morality (unweighted) is 458 compared to 500 (4-9 days) and 486 (>9 days) if we only include cities with a 'mortality acceleration date' after 24 September 1918 (see Herby (2022) for details). Choosing later cut-off dates does not change the picture. For Hatchett et al. (2007), this comparison is more difficult because there is little overlap between early and late intervention cities.

**Figure 15: Cities hit late by the Spanish Flu in 1918 experienced lower excess mortality**

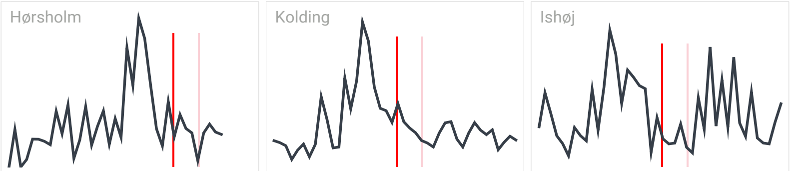



Source: Hatchett et al. (2007) and Markel et al. (2007)

Note: The figure illustrates that cities that were hit late by the 1918 Spanish Flu pandemic generally experienced lower excess death rates despite response times.

Also, even if it can be empirically stated that a well-timed lockdown is effective in combating a pandemic, it is doubtful that this information will ever be useful from a policy perspective. If lockdowns are effective as long as they are well timed, our results – which show the average effect of lockdowns in Europe and the United States – show that governments, on average, were unable to time lockdowns properly to obtain a substantial effect on mortality.[99] The problem of proper timing is well known from the debate about discretionary economic stabilisation policies. Selecting the proper timing for such measures has proved a disappointment.[100] Thus, discretionary approaches have largely been abandoned for rule-based stabilisation policies.

99 As an example of how difficult timing is, Jonas Herby 2021 Hvorfor Lukker Myndighederne Skolerne? *Punditokraterne* (blog), 31 March 2021 (https://punditokraterne.dk/2021/03/31/hvorfor-lukker-myndighederne-skolerne/) and Jonas Herby 2021 Nedlukningen Af Hørsholm Kommune Var Unødvendig. *Punditokraterne* (blog), 21 May 2021 (https://punditokraterne.dk/2021/05/21/nedlukningen-af-hoersholm-kommune-var-unoedvendig/) show how the Danish authorities – responding to local outbreaks in the fall of 2021 – in several occasions closed schools *after* case numbers had regressed to the level before the outbreak. The figures below illustrate this fact. The black line is test-corrected case numbers, the red vertical line is the day the schools closed, and the pink vertical line is the earliest day the effect of the school closure can be seen in cases (in Denmark in the autumn of 2021, people were advised to wait four days between close contact with an infected person and being tested).




100 This is partly due to policy lags, i.e., the lag between the time an epidemic problem arises and the effect of a policy intended to counteract it. In monetary and fiscal policies, the four lags are the *recognition lag*, the *decision lag*, the *implementation lag*, and the *effectiveness lag*. These lags are likely to be relevant for pandemic policies too. As described on p. 45, the first time WHO characterised COVID-19 as a pandemic was by 11 March 2020, 2½ months after the first case in China.

### *The 'your results do not apply to later waves and variants' objection*

Another objection has been that the studies included in the meta-analysis cover, for the most part, the first wave and that one can imagine that lockdowns may be effective against later waves or variants. This objection is a hypothesis, and only future research can show if this hypothesis is true or false. But even if future research falsifies this hypothesis, yet another hypothesis can then be proposed: that – again – the historical evidence is not relevant for future waves and variants. It is a type of hypothesis that has no logical end, as one can always propose new hypotheses that *could* potentially be true. In the end, it will be a political discussion about how heavy such speculations should weigh when making decisions when historical data finds limited effects of lockdowns.

### *The 'there are too few studies to know for sure' objection*

Several commentators have pointed out that our conclusion is based on relatively few studies. While more studies are always better, we have included all existing empirical evidence and covered far more studies than needed to, e.g., bring a new drug to the market.[101] It is worth noting that optimal sample sizes can be and are surprisingly small in number (see Hanke and Mehrez 1979). Communicable diseases can be handled using pharmaceutical interventions and/or non-pharmaceutical interventions. From a scientific and political perspective, the evidence required to implement either of these interventions should not differ. Hence, it is inconsistent to have a political regime where pharmaceutical interventions *may* only be used if one can prove they are *effective* and that negative side effects are *small*, while *non*-pharmaceutical interventions *will* be used unless one can prove they are *ineffective* and that negative side effects are *large*.

### *The 'we cannot be sure without randomised control trials' objection*

Another objection is that our findings are not based on randomised control trials (RCTs). The obvious response is that a RCT has never been conducted for lockdowns. Therefore, we are limited to observational studies.

101 In general, three to four clinical trials are required before a drug will be approved. Usually, a Phase 1 for safety, a Phase 2 to find the best dose (most effective while being safe), and one to two randomised Phase 3 trials to confirm the benefit seen in the Phase 2 trial.

We agree that, preferably, the effect of lockdowns should be tested using randomised control trials (RCTs), although Deaton and Cartwright (2018) argue that ‘the lay public, and sometimes researchers, put too much trust in RCTs over other methods of investigation’. Unfortunately, there are very few classic RCT studies except for some mask studies that are unfortunately of little relevance to our research question[102] (see Hirt et al. 2022).

Given the lack of RCTs, observational studies are our best way to know if lockdowns work. We only have one source of evidence: history, with all its covariates and missing/bad data. If we reject that source, we have nothing to rely on. RCT is not the only kind of research that can improve policy, and, as Caplan (2022) puts it: ‘What’s the empirical evidence that RCTs actually improve policy?’

However, we have – after we, among other things, conducted a search on Scopus[103] – knowledge of a few ‘RCT-like’ studies that use natural experiments to examine the effects of lockdowns, mask mandates, school closures, and SIPOs. The studies are described in Table 17, and overall the conclusions are very similar to the measured meta-results.

**Table 17: The results in the identified natural experiments are similar to our measured meta-results**

| Study | Conclusion | NPI and design | Description |
|---|---|---|---|
| Kepp and Bjørnskov (2021); ‘Lockdown effects on Sars-CoV-2 transmission – The evidence from Northern Jutland’ | ‘Efficient infection surveillance and voluntary compliance make full lockdowns unnecessary at least in some circumstances.’<br><br>This result is similar to the measured meta-results, see section 4. | Lockdown<br><br>Quasi-random policy change | Kepp and Bjørnskov (2021) use evidence from a quasi-natural experiment when seven of the eleven municipalities in Northern Jutland in Denmark went into extreme lockdown after the discovery of mutations of Sars-CoV-2 in mink (and not because of general level of infections). |

102 Our research question is ‘Were lockdowns effective in reducing COVID-19 mortality?’, cf. p. 22

103 The search was based on the same methodology as in our search strategy described in section 2.1, but we replaced the methodology search string with ‘natural experiment’ and ‘regression discontinuity’.

| Study | Conclusion | NPI and design | Description |
|---|---|---|---|
| Dave et al. (2020a); 'Did the Wisconsin Supreme Court restart a Covid-19 epidemic? Evidence from a natural experiment' | 'We find no evidence that the repeal of the state SIPO impacted social distancing, COVID-19 cases, or COVID-19-related mortality during the fortnight following enactment. Estimated effects were economically small and nowhere near statistically different from zero.'<br><br>This result is similar to the measured meta-results, see section 4. | SIPO<br><br>Quasi-random policy change | Dave et al. (2020a) use the Wisconsin Supreme Court abolishment of Wisconsin's 'Safer at Home' order (a SIPO) as a natural experiment. |
| Wang (2022); 'Stay at home to stay safe: Effectiveness of stay-at-home orders in containing the Covid-19 pandemic' | 'We find that although residents in both groups were staying at home even before the implementation of any order, these orders reduced the number of new COVID-19 cases by [5.4%][104].'<br><br>*This result is similar to the measured meta-results, see section* 4. | SIPO<br><br>Regression discontinuity design | Wang (2022) compares counties close to the border between states with SIPOs and states without SIPOs. |

104 In their abstract they write 7.6%, but that result is from their difference-in-differences model. The 5.4% is from their regression discontinuity design (see Supporting Information Table A14).

| Study | Conclusion | NPI and design | Description |
|---|---|---|---|
| Hansen and Mano (2021); 'Mask mandates save lives' | 'Our results imply that mandates saved 87,000 lives [in the United States] through December 19, 2020, while a nationwide mandate could have saved 58,000 additional lives.' This corresponds to an impact of 36% fewer deaths.[105]<br><br>*This result is relatively large compared to the measured meta-results, see section* 4*.* | Mask mandates<br><br>Regression discontinuity design | Hansen and Mano (2021) rely on the variation between counties across 'mask borders', i.e., a state border that separates two counties, in which one county is in a state with a mask mandate at a given time and the other county is in a state without a mask mandate at the same time. Interestingly, they find mask mandates are four times more effective in counties which are positively inclined to wearing masks which may indicate an important voluntary effect.[106] |

105 By 19 December 2020, 321,035 had died with COVID-19 in the United States. The results from Hansen and Mano (2021) imply that 263,035 would have died with a nationwide mandate and 408,035 without any mandates. Hence, the effect of masks is 36%.

106 Hansen and Mano (2021) write: 'Specifically, mask mandates reduce COVID-19 cases and deaths by -78.03 and -1.45, respectively, in the median county more positively inclined to wearing masks in our sample. While the same numbers for the median county more negatively inclined towards to wearing masks are -44.54 and -0.37, also respectively.'

| Study | Conclusion | NPI and design | Description |
|---|---|---|---|
| Abaluck et al. (2022); 'Impact of community masking on COVID-19: A cluster-randomized trial in Bangladesh' | 'The intervention reduced symptomatic seroprevalence by 9.5% (adjusted prevalence ratio [aPR] = 0.91 [0.82, 1.00]; control prevalence 0.76%; treatment prevalence 0.68%). […] In villages randomized to surgical masks (*N* = 200), the relative reduction was 11.1% overall ([aPR] = 0.89 [0.78, 1.00])'.<br><br>*This result is relatively small compared to the measured meta-results, see section 4.* | Mask mandates<br><br>Cluster-randomised trial of community-level mask promotion | Abaluck et al. (2022) carried out an experiment in Bangladesh with cross-randomised mask promotion strategies at the village and household level, including cloth vs. surgical masks. All intervention arms received free masks, information on the importance of masking, role modelling by community leaders, and in-person reminders for 8 weeks. The control group did not receive any interventions. Neither participants nor field staff were blinded to intervention assignment. |
| Fukumoto et al. (2021); 'No causal effect of school closures in Japan on the spread of COVID-19 in spring 2020' | 'We do not find any evidence that school closures in Japan reduced the spread of COVID-19. Our null results suggest that policies on school closures should be reexamined given the potential negative consequences for children and parents.'<br><br>*This result is similar to the measured meta-results, see section* 4. | School closures<br><br>Regression discontinuity design | Fukumoto et al. (2021) matches each municipality with open schools to a municipality with closed schools that is the most similar in terms of potential confounders to estimate the causal impact of closing schools. |

Note: We found Kepp and Bjørnskov (2021) and Dave et al. (2020a) during our initial search on Google Scholar. One of the authors pointed us towards Hansen and Mano (2021), and we knew Abaluck et al. (2022) from the media. The other studies were identified in a search on Scopus using the same disease search string and government response search string, but replacing the methodology search string with first 'natural experiment' then 'regression discontinuity design', see The Royal Swedish Academy of Sciences (2021). We did not find any additional relevant natural experiments in the Scopus search (several studies claim to be natural experiments, but as Digitale et al. (2021) note, 'the term "natural experiment" is somewhat of a misnomer. Policy responses being studied are not naturally occurring, but are decisions driven by the pandemic's trajectory and social and political will'). The Kepp and Bjørnskov (2021), Fukumoto et al. (2021), and Wang (2022) are not included in our review and meta-analysis, because they focus on cases and not on COVID-19 mortality. Dave et al. (2020a) is not included in our review, because it is a synthetic control method and lack jurisdictional variance. Hansen and Mano (2021) is not included, because their working paper was published after our search on Google Scholar.

Overall, the results from the natural experiments in Table 17 are similar to our own conclusions. They, for the most part, only find marginal effects of lockdowns and NPIs, except for Hansen and Mano (2021), who examine mask mandates. These studies do not meet our eligibility criteria and are therefore not included in the meta-analysis (see notes to Table 17). But, given their credibility, we do provide comments on these studies.

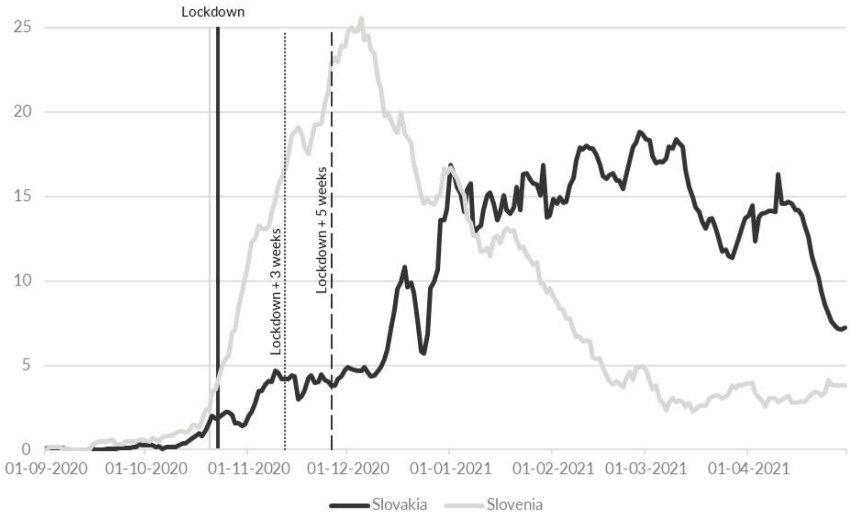
### *The 'every time a country has locked down, the mortality rate has dropped' objection*

We agree that the general pattern has been that mortality rates usually – but not always – drop after lockdowns are imposed. However, this does not imply causality, as people voluntarily change behaviour when responding to information.

Also, while this has been the general pattern, there are examples that, at the very least, question the causality in the argument. Figure 16 shows daily deaths in Slovenia and Slovakia during the 2020/21 winter. Both Slovenia and Slovakia introduced strict lockdowns in late October 2020. On 24 October, Slovakia issued SIPOs, limited gatherings, banned indoor eating and drinking at restaurants, closed schools, and issued mask mandates.[107] Nevertheless, the death toll continued to rise, and the death rate stayed above pre-lockdown levels for at least six months. Slovenia experienced a more classic wave, but the daily death rate did not peak until 6 weeks after the lockdown, making it unlikely to be caused by the lockdown. (Usually, it takes three to four weeks from infection to death.)

107 Source: https://crisis24.garda.com/alerts/2020/10/slovakia-authorities-to-introduce-partial-nationwide-lockdown-from-october-24-update-13.

**Figure 16: Lockdowns in Slovakia and Slovenia did not make mortality rates drop**




Source: Our World in Data (2022)

Note: Daily deaths are shown as seven days average. The black (Slovakia) and grey (Slovenia) vertical lines illustrates the day when the OxCGRT stringency index surpassed 70. The dotted and dashed lines show approximately three and five weeks after lockdowns were introduced.

### *The 'what about zero-covid-countries?' objection*

Some commentators have pointed to countries such as Australia and New Zealand, which have followed a strategy with very strict lockdowns as a response to even relatively few infections (known by many as a 'zero-covid' strategy). For example, Melbourne's SIPO in response to the Delta strain lasted 262 days.[108] Comparing COVID-19 mortality rates in Australia to mortality rates in Europe and the United States, this zero-covid strategy appears to be effective when measured by COVID-19 mortality rates.

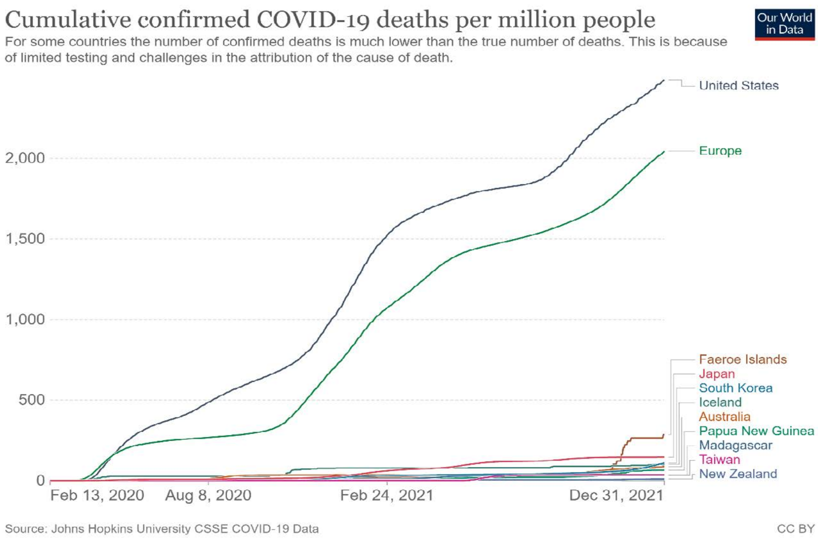
But as illustrated in Figure 17, the immediate effectiveness is less obvious compared to other island countries of which at least some have used more lenient COVID-19 policies (e.g., by 31 December 2021, Iceland had never issued a SIPO and only closed schools for 82 days in total, whereas New

108 See https://www.reuters.com/world/asia-pacific/melbourne-readies-exit-worlds-longest-covid-19-lockdowns-2021-10-20/.

Zealand (/Australia) had issued a SIPO for 191 (/363) days and closed schools for 200 (/396 days) (Hale, Angrist, Goldszmidt, et al. 2021).

We therefore caution against attributing the low mortality rates in New Zealand and Australia to strict lockdowns when more obvious explanations – such as being island countries – may explain the differences.

**Figure 17: COVID-19 mortality rates have been relatively low in several island countries despite significant differences in their lockdown policies (2020-2021)**




Source: Our World in Data (2022).

Note: South Korea is included as it is de facto an island.

Given their remoteness, island countries were particularly successful. One reason for this might have been their isolation and relatively low contact with foreign travellers. This may have slowed the initial inflow of infections. Indeed, island countries stand out with very few deaths even before lockdowns could possibly have had an effect. As illustrated in Figure 1, virtually all countries locked down in the middle of March. Given the three- to four-week lag between infection and death, this means that the possible effect of lockdowns (and simultaneous voluntary behaviour changes) would be visible in the second week of April 2020. At that time, the island countries did indeed have far fewer deaths, indicating that the initial inflow of infections in these countries was lower. [109]

### *5.2.5. Which factors explain the cross-country differences in COVID-19 mortality?*

But what else explains the differences between countries if not differences in lockdown policies? Differences in population age and health, the quality of the health sector, and the like are obvious factors. But several studies point at less obvious factors, such as culture, communication, and coincidences. For example, Frey et al. (2020) show that for the same policy stringency, countries with more obedient and collectivist cultural traits experienced larger declines in geographic mobility relative to their more individualistic counterparts.

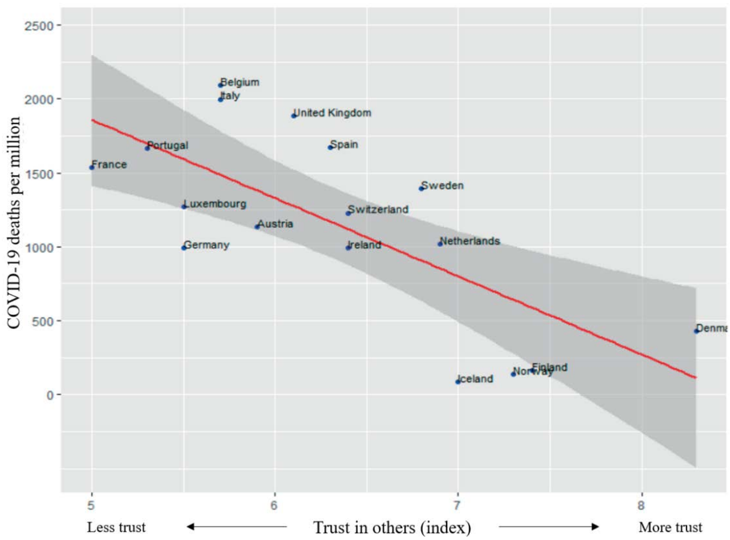
Using data from Germany, Laliotis and Minos (2020) show that the spread of COVID-19 and the resulting deaths in predominantly Catholic regions with stronger social and family ties were much higher when compared to non-Catholic ones at the local NUTS 3 level.[110] Albæk (2021) notes that

109 This can easily be seen at Our World In Data's website, see fx https://ourworldindata.org/explorers/coronavirus-data-explorer?zoomToSelection=true&time=2020-03-04..2020-04-25&Metric=Confirmed+deaths&Interval=Cumulative&Relative+to+Population=true&country=FRO~ISL~NZL~AUS~KOR~TWN~USA~Europe

110 The NUTS classification (Nomenclature of territorial units for statistics) is a hierarchical system for dividing up the economic territory of the EU and the UK. There are 1,215 regions at the NUTS 3-level.

trust in others seems to be an important factor, see Figure 18.[111] And Bollyky et al. (2022) find that 'measures of trust in the government and interpersonal trust, as well as less government corruption, had larger, statistically significant associations with lower standardised infection rates'. Thornton (2022) finds that 'if all societies had trust in government at least as high as Denmark, which is in the 75th percentile, the world would have experienced 13% fewer infections. If social trust – trust in other people – reached the same level, the effect would be even larger, with 40% fewer infections globally.' Similar results are found in several other studies.

**Figure 18: Countries with more trust in others experienced lower COVID-19 mortality rates**




Source: Albæk (2021)

Note: Axis titles have been translated from Danish to English.

111 It is remarkable that the five countries above the confidence interval in Figure 18 – Belgium, Italy, the United Kingdom, Spain, and Sweden – all can be found among the countries hit early by the pandemic, see Figure 7, which may indicate that the mortality rates in these countries could have been much lower if they had not been among the first countries to be hit by the COVID-19 pandemic. (There are no country labels in Figure 7, but the five countries all experienced more than 500 deaths per million during the first wave. The mortality rates by 30 June 2020 were 838 per million in Belgium, 576 per million in Italy, 593 per million in the United Kingdom, 615 per million in Spain, and 525 per million in Sweden).

Government communication may also have played a large role. Compared to its Scandinavian neighbours, the Swedish health authorities communication was far more subdued and embraced the idea of public health vs. economic trade-offs. An illustration of the differences in perspective on the coming pandemic was visible on 7 March 2020, when the Danish final in the European Song Contest – based on national health authorities' strong recommendations – was held *without* audience in Denmark but – again, based on national health authorities' recommendations – *with* audience in Sweden.[112]

This clearly illustrates differences in the health authorities' assessment of the risk, and this difference may explain why Helsingen, Refsum, et al. (2020), based on questionnaire data collected from mid-March to mid-April 2020, find that even though the daily COVID-19 mortality rate was more than four times higher in Sweden than in Norway, Swedes were less likely than Norwegians to not meet with friends (55 per cent vs. 87 per cent), avoid public transportation (72 per cent vs. 82 per cent), and stay home during spare time (71 per cent vs. 87 per cent). That is, despite a more severe pandemic, Swedes were less affected in their daily activities (legal in both countries) than Norwegians.

Many other factors may be relevant, and we should not underestimate the importance of coincidences. An interesting example illustrating this point is found in Arnarson (2021) and Björk et al. (2021), who show that areas in Europe where the winter holiday was relatively late (in week 9 or 10 rather than week 6, 7 or 8) were hit especially hard by COVID-19 during the first wave because the virus outbreak in the Alps could spread to those areas with ski tourists. Arnarson (2021) shows that the effect persisted in later waves.

The importance of the timing of the winter holiday is illustrated in Figure 19 borrowed from Andersson (2022), which illustrates 1) excess mortality in Swedish regions (län) where the winter holiday was in week 9, 2) excess mortality in Swedish regions where the winter holiday was in other weeks, and 3) excess mortality in other Nordic countries. Figure 19 illustrates how excess deaths in Sweden in the spring of 2020 were primarily driven by regions with winter holidays in week 9, while excess mortality in other regions was comparable to the excess mortality in other Nordic countries.

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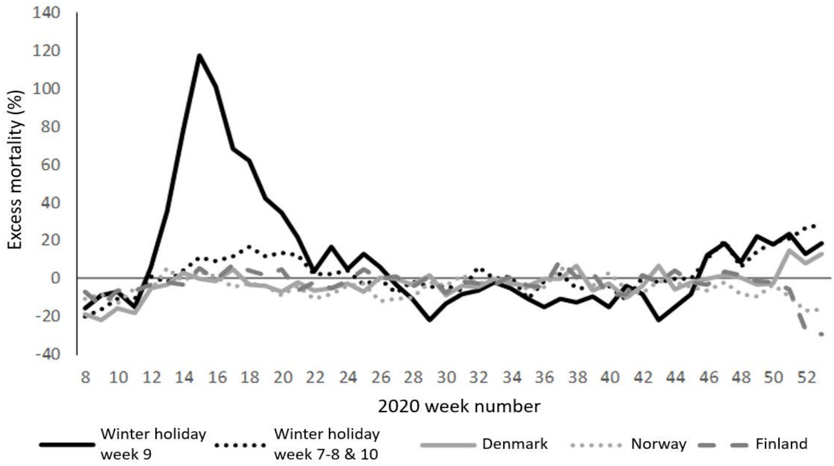
112 Another example is the Danish Prime Minister Mette Frederiksen, who has on several occasions said that every single death is a tragedy. A word choice that was very distinct from that of the Swedish state epidemiologist Anders Tegnell.

**Figure 19: The excess mortality in Sweden in the spring of 2020 emerged primarily in regions with winter holidays in week 9, when ski tourists were unknowingly exposed to a COVID-19 virus outbreak in the Alps**




Source: Andersson (2022)

Note: Axis titles and legends have been translated from Swedish to English.

Also, Sweden had more frail elders due to very mild flu seasons in 2018/19 and 2019/20 as well as very few deaths during the 2019 summer compared to earlier years and compared to other Nordic countries (see Herby 2020) which affected mortality in Sweden (see Juul et al. 2022; Zahran et al. 2022). Had the winter holiday in Sweden been in week 7 or week 8 as in Denmark and had mortality rates in Sweden in 2018/19 and 2019/20 been comparable to mortality rates in Denmark, the Swedish COVID-19 situation could have turned out very differently.[113]

### *5.2.6. The total costs of lockdowns to society*

A growing body of research argues that lockdowns have had devastating and far-reaching effects in many fields of society and through many channels (Gan et al. 2022). They have severely reduced economic activity,

113 Another case of coincidence is illustrated by Shenoy et al. (2022), who find that areas that experienced rainfall early in the pandemic realised fewer deaths because the rainfall induced social distancing.

raised unemployment, resulted in many enterprise bankruptcies, and increased government debt significantly. And they have contributed to raising inequality in a number of ways.

In addition to their immediate economic impact, lockdowns have reduced the time spent by children in school, decreasing the extent of education, and therefore reduced investment in human capital, increased mental disorders and domestic violence, and caused significant quality-of-life losses. Lockdowns have also reduced personal freedom, caused political unrest, strengthened authoritarian tendencies, increased government corruption, and undermined liberal democracy.

These wide effects of lockdowns and their subsequent costs have been captured by many researchers. For example, in a review, Onyeaka et al. (2021) conclude that

> the impact of the lockdown has had far-reaching effects in different strata of life, including; changes in the accessibility and structure of education delivery to students, food insecurity as a result of unavailability and fluctuation in prices, the depression of the global economy, increase in mental health challenges, wellbeing and quality of life amongst others.

Another commentator, Allen (2021) states that, aside from the common use of the decline in GDP as the cost of lockdowns, other costs should be analysed as well:

> It has been understood from the very beginning of the pandemic that lockdown caused a broad range of costs through lost civil liberty, lost social contact, lost educational opportunities, lost medical preventions and procedures, increased domestic violence, increased anxiety and mental suffering, and increased deaths due to despair and inability to receive medical attention.

Finally, Mulligan and Arnott (2022) find elevated levels of non-Covid excess deaths:

> From April 2020 through at least the end of 2021, Americans died from non-Covid causes at an average annual rate of 97,000 in excess of previous trends. Hypertension and heart disease deaths combined were elevated 32,000. Diabetes or obesity, drug-induced

> causes, and alcohol-induced causes were each elevated 12,000 to 15,000 above previous (upward) trends. Drug deaths especially followed an alarming trend, only to significantly exceed it during the pandemic to reach 108,000 for calendar year 2021. Homicide and motor-vehicle fatalities combined were elevated almost 10,000. Various other causes combined to add 18,000.

Although many of these excess deaths were a consequence of personal choices, it is likely that SIPOs, school closures, etc. made it difficult for people to handle the pandemic.[114]

To assist in an overview of the costs associated with lockdowns, we present a highly stylised picture in Table 18. It is little more than an impressionistic glance at the types of effects and subsequent costs that have been discussed in the literature.

It should be stressed that a great deal of confusion and misinterpretation of data arises during pandemics because, once people become aware of the dangers associated with the pandemic, they make voluntary changes in their behaviour to mitigate the chances of contracting the virus. It is what we refer to as the 'hot stove' effect. Once a person recognises that a stove is hot, that person will avoid placing his or her hand on the stove. This insight is important, and one should be very careful when claiming that lockdowns did this or that, because much of what we observed during the pandemic happened because of voluntary changes that had nothing to do with government mandates.

This logic can be equally applied to both the mortality rates and the costs associated with the pandemic. Note that the voluntary behavioural changes associated with the 'hot stove' effect explain why we would expect to see much lower mortality effects than epidemiological models, which assume no change in behaviour, predict. On the other hand, the voluntary behavioural changes certainly suggest that the costs of mandatory lockdowns will likely be much less than the total costs associated with the pandemic. A good portion of the costs are the result of voluntary behavioural changes.

114 Because Mulligan and Arnott (2022) do not distinguish between the effect of lockdowns and the effect of voluntary behaviour changes, the study is not included in Table 18.

If, due to voluntary social distancing, restaurant visits are down to 10 per cent of the pre-virus level and lockdowns then take this to 0 per cent, we cannot say that lockdowns have devastated the restaurant industry. The virus itself devastated it, and lockdowns just made it marginally worse.

Many of the studies of the costs of lockdowns make the same flawed implicit assumption as Flaxman et al. (2020), namely, that lockdowns – and not voluntary behavioural changes – are the only factor that affect society, and therefore lockdowns are the root of all effects and costs. And just as this assumption overestimates the effect of lockdowns on mortality, it runs the risk of overestimating the cost of lockdowns as well.

Table 18 mirrors the effects of lockdowns in several fields. We have tried to avoid studies making 'the Flaxman-mistake' and note when the studies do not immediately deal with this problem correctly. First, economic activity as measured by the decline in growth, disruption in global trade and production, and an increase in unemployment. Second, due to lockdowns and the decline in economic activity, governments stepped in to boost demand, which was contracting, through various programmes. All these support measures were financed by increasing government borrowing, raising public debt to exceptionally high levels in many countries. Fiscal policy became extremely expansionary, with fiscal deficits being monetised by central banks. This gave rise to a surge in the quantity of money held by the public, which, with a lag, produced record levels of inflation in many countries.

Andersson (2022) makes a constructive attempt to empirically separate the economic impact of lockdowns from the effects of voluntary behavioural adjustments. He focuses on the effects on economic growth (GDP) and on public debt from mandatory lockdowns and voluntary social distancing, using data from about 30 European countries. He concludes that mandatory lockdowns have had a significant and large impact on growth while voluntary adjustments are not significantly related to the decline in growth during the pandemic (Andersson 2022, Tables 3 and 4).

Andersson (2022) also finds the same pattern concerning the rise of public debt. Lockdowns are associated with a sharp rise in public debt while voluntary adjustments are not. Andersson (2022) concludes that the economic effects of lockdowns on growth and public finances are large and lasting. He also discusses the reasons why. According to Andersson, lockdowns hit the economy very hard by forcing everybody, private individuals and firms, to change their behaviour while voluntary adjustment allows for different behaviour, more flexibility, and less uniformity in the response to the pandemic.

The concept of loss of quality of life is worth a brief comment. Unfortunately, we are aware of only a few quality-of-life studies covering the COVID-19 pandemic. In a study of Sweden by Persson et al. (2021), the Health-Related Quality of Life (HRQoL) was measured by a web-based survey sent to randomised samples of the adult Swedish population before the outbreak of the pandemic in February 2020 and during the outbreak of the pandemic. The first-wave pandemic data was collected in April 2020, one month after the outbreak, and the second-wave data was collected in January 2021, after 10 months in which Swedes were living under the pandemic. The number of quality-adjusted life-years (QALYs) lost per month was calculated for both pandemic surveys.

The loss of health for the Swedish population for *one month* in April 2020 was 29-33,000 QALYs and for the month of January 2021 was estimated to be 21-44,000 QALYs. These monthly losses of health are of the same magnitude as the total loss of health due to excess mortality for the *entire year* of 2020, which was 42,800 QALYs. The results from Sweden – a country with relatively few government-mandated pandemic restrictions – underline the importance of looking at QALYs when assessing the cost of lockdowns. Hay et al. (2021) find an overall loss of 2.6 million QALYs in the U.S. compared to a pre-pandemic sample. It is difficult to say how much of the loss in QALYs documented by Persson et al. (2021) and Hay et al. (2021) was caused by lockdowns, but in another study, Fink et al. (2022) find that an estimated total of 3,259 million QALYs have been lost to date and that a longer time spent under severe restrictions is associated with a higher loss of QALYs.

A study of Israel by Yanovskiy and Socol (2022) attempts to estimate the loss of QALYs caused by lockdowns. They conclude for Israel that ‘it can be estimated that even if the lockdowns saved some lives, in the long term they killed 20 times more.’ The studies mentioned here imply that lockdowns represent a major cost in terms of loss of quality of life. These findings suggest that any analysis of the costs of lockdowns to society should include measures of the HRQoL.

## Table 18: Some costs of lockdowns to society. A stylised picture

| | | |
|---|---|---|
| Economic costs | Output, production, employment | Lockdowns contributed to a sharp decline in GDP, international trade, production, and a rise in unemployment and business failures according to the April 2020 World Economic Outlook, IMF (2020a). Here the Great Lockdown Recession is viewed as a major downturn in the global economy compared to the Great Depression of the 1930s. According to IFS Taxlab (2021) 'The economic lockdown that followed the outbreak of COVID-19 in the UK resulted in GDP being almost 10% lower in 2020 than in 2019. This is huge. Records suggest it is the biggest year-on-year decline in activity in over 300 years since the Great Frost of 1709.' König and Winkler (2021) studying GDP growth in 42 countries conclude that 'all efforts should be undertaken to avoid hard lockdowns as any rise in lockdown intensity has severely negative effects on economic activity.' See also IMF (2020b) and Andersson (2022). |
| | Fiscal and monetary policy | Lockdowns contributed to a sharp rise in public debt. See IMF (2020a), IMF (2020b), Makin and Layton (2021), and Andersson (2022). |
| | Inequality | IMF (2020b) stresses 'the unequal effects of lockdowns that severely affect economically vulnerable segments of the population'. Palomino et al. (2020a)'s analysis 'reveals that the lockdown and de-escalation periods will potentially increase poverty and inequality sizeably in all European countries,' (see Palomino et al. 2020b) while Caselli et al. (2022) find that 'lockdowns had a larger impact on the mobility of women and younger cohorts.' See also Abraham et al. (2022). |

| | | |
|---|---|---|
| Social costs | Public health | *A decline in the health of children*.<br>Rajmil et al. (2021) indicate that children's health has suffered in most of the world. Deoni et al. (2021) find, like several other studies, that the losses in early development are significantly greater among poor and poorly educated families. |
| | Schooling and schooling inequality | *Sharp and unequal decline in human capital formation*.<br>Engzell et al. (2021) find that children, on average, learn almost nothing in the weeks they received virtual education. The effect was particularly pronounced in less educated homes. Rose et al. (2021) find that school closures in the UK in the spring of 2020 had put six- and seven-year-old students about two months behind in reading and seven months in math. Lindberg (2021) find that half of all students in Denmark between 5th and 8th grade got significantly less out of online teaching. Halloran et al. (2021) find that passing rates declined 10.1% in districts without in-person teaching relative to districts with in-person teaching and that the effect was larger in districts with larger populations of students who are Black, Hispanic or eligible for free and reduced price lunch. Agostinelli et al. (2022) conclude that school closures have a large, persistent, and unequal effect on human capital accumulation. Gajderowicz et al. (2022) find that the economic loss in future student earnings due to learning losses may amount to 7.2 percent of Poland's gross domestic product. Hallin et al. (2022) find no COVID-19 related learning loss in reading in Swedish primary school students. |
| | Vaccine uptake | *Effects on future uptake of non-COVID vaccines*<br>Leuchter et al. (2022) find that influenza vaccine uptake has increased in states with high COVID-19 vaccine uptake and decreased in states with low COVID-19 vaccine uptake. For children influenza vaccine uptake has decreased uniformly regardless of COVID-19 vaccine uptake. Trujillo et al. (2022) observe that attitudes toward COVID-19 vaccination spillover onto general vaccine scepticism and attitudes toward hypothetical vaccines. |
| | Quality of life | *Lockdowns reduced the quality of life*<br>Persson et al. (2021) show a significant negative effect on the quality of life in Sweden during the pandemic. Hay et al. (2021) find a large negative effect on the quality of life in the U.S. Fink et al. (2022) conclude that that longer time with severe restrictions is associated with a higher loss of QALYs.<br>Using quality-of-life measures for Israel, Yanovskiy and Socol (2022) estimate that in the short run 'lockdowns saved some lives, in the long term they killed 20 times more.' |
| | Mental health | Patrick et al. (2022) find that during lockdowns and the pandemic the use of alcohol among U.S. young and middle adults increased to relax/relieve tension and because of boredom. Altindag et al. (2022) conclude that the SIPO-induced decline in mobility substantially worsened mental health outcomes. Zhou et al. (2020) find that UK women, especially mothers, experienced a more dramatic decline in wellbeing due to lockdowns and the pandemic, and single mothers got hurt the most in all aspects. Adams-Prassl et al. (2020) find that SIPOs lowered mental health. Serrano-Alarcón et al. (2022) show that easing lockdown measures rapidly improved mental health. Armbruster and Klotzbücher (2020) find that helpline contacts increased by around 20% in the first week of the lockdown and slowly decreased again after the third lockdown week. Greyling et al. (2021) show that a lockdown is associated with a decline in happiness, and that the more stringent the stay-at-home regulations are, the greater the decline seems to be. |
| | Crime | In a systematic review and meta-analysis including eighteen empirical studies, Piquero et al. (2021) conclude that incidents of domestic violence increased in response to SIPOs and that the effects were stronger when only U.S. studies were considered. Telles et al. (2021) describe how lockdowns during the COVID-19 pandemic have led to a surge in domestic violence in several countries, especially among females, minors, pets, and elders. |

| | | |
|---|---|---|
| Political costs | Threats to democracy | Wood et al. (2021) find that policies such as workplace and school closures, are associated with increases in dissent activities. According to Jørgensen et al. (2021), 'pandemic fatigue' significantly increases with time and the severity of interventions, and that 'fatigue elicited a broad range of discontent, including protest support and conspiratorial thinking'. Bor et al. (2021a) show that support for the political system markedly decreased already by April 2020 and fell further till December 2020. They find that 'pandemic fatigue' (specifically, the perceived subjective burden of the pandemic and feelings of anomie) correspond to decreases in system support and increases in extreme anti-systemic attitudes. The Armed Conflict Location & Event Data Project (ACLED) (2021) describes how COVID-19 caused many protests on different issues, from health workers, people suffering from the eviction crisis, school, restrictions, etc. |
| | Loss in freedom | Papadopoulou and Maniou (2021) find that the pandemic crisis have exacerbated existing obstacles to press freedom and have added new dimensions to the already documented threats. Governments have used the excuse of the pandemic to justify restrictions imposed on essential journalism and have worsened the condition of press freedom in both western democracies and authoritarian nations. Freedom House (2020) conclude that 'since the coronavirus outbreak began, the condition of democracy and human rights has grown worse in 80 countries. Governments have responded by engaging in abuses of power, silencing their critics, and weakening or shuttering important institutions, often undermining the very systems of accountability needed to protect public health.' |

Note: This table gives just a brief summary of the immense literature on the effects of lockdowns. Some of the studies are likely to catch both the effect of lockdown and voluntary behaviour changes (we note this for some studies by adding 'and the pandemic' in the description of the study).

Overall, lockdowns imposed huge costs on society wherever they were imposed. These costs are of both a short- and long-term nature. Indeed, many will linger for decades. The lockdowns will leave a long-lasting scar on the world economy.

### *5.3. Policy implications from comparing the benefits to the costs of lockdowns*

In the early stages of a pandemic, before the arrival of vaccines and new treatments, a society can respond basically in two ways: through mandated behavioural changes or voluntary behavioural changes. Our study finds a negligible positive health effect of mandated lockdowns. As a result, allow us to address voluntary behavioural changes. Here, more research is needed to determine how voluntary behavioural changes can be supported. But it should be clear that one important role for government authorities is to provide information so that citizens can voluntarily respond to the pandemic in a way that mitigates their exposure.[115] In short, they have to be effectively warned that the stove is hot and be given advice on how to avoid the hot stove.

When economists are faced with the choice of selecting the proper policy, their judgement is based on an analysis of both the benefits and costs. As far as we know, lockdowns have been adopted worldwide without the use of any explicit cost–benefit analysis. The same conclusion is found in Allen (2021), who state that 'no government has provided any formal cost/benefit analysis of their actions.'[116]

Epidemiologists have pushed for lockdowns with little consideration of the costs of their proposals to society. The United Kingdom is a prime example of this judging by Woolhouse (2022). Here, we are in line with Boettke and Powell (2021), who write that

> 'Follow the science' has been an oft-repeated phrase over the course of the COVID-19 pandemic. It is used mostly to implore people to do what epidemiologists recommend. However, epidemiologists have no expertise in weighing health benefits against other costs. Economics is the science that deals with evaluating the tradeoffs between costs and benefits.

Lockdowns were not used to such a large extent during any of the pandemics of the previous century, and – as illustrated in Table 18 – the costs of lockdowns to society are immense. These costs must be compared to the benefits of lockdowns, which our meta-analysis has shown to be negligible.

Much evidence points to people responding voluntarily to dangers as the main explanation for the negligible effect of lockdown. When a pandemic rages, people engage in social distancing regardless of what the government mandates. How much social distancing people demand depends on how severe the pandemic is perceived to be. The more transmissible and the higher the mortality, the greater the response, limiting the effect of – and need for – government intervention even in a situation where the virus is much more transmissible and deadly than anything witnessed during the COVID-19 pandemic, and where no vaccine is found.

115 As noted by Thaler and Sunstein (2009), it might be fruitful to consider how nudging can influence citizens' behaviour without coercion.

116 See Allen (2021) for a cost–benefit assessment of the experience of lockdowns in Canada.

A standard comparison of the costs and benefits of lockdowns leads to a strong conclusion: until future research based on credible empirical evidence proves that lockdowns have large and significant reductions in mortality, lockdowns should be rejected out of hand as a pandemic policy instrument.

# Appendix I: Supplementary information

### *Excluded studies*

Below is a list of the studies excluded during the eligibility phase of our identification process and a short description of our basis for excluding the study.

### Table 19: Studies excluded in the meta-study identification process

| 1. Study (Author & title) | 2. Reason for exclusion |
|---|---|
| Alemán et al. (2020); 'Evaluating the effectiveness of policies against a pandemic' | Too few observations |
| Alshammari et al. (2021); 'Are countries' precautionary actions against COVID-19 effective? An assessment study of 175 countries worldwide' | Is purely descriptive |
| Amuedo-Dorantes et al. (2020); 'Timing is everything when fighting a pandemic: COVID-19 mortality in Spain' | Duplicate |
| Amuedo-Dorantes et al. (2021); 'Early adoption of non-pharmaceutical interventions and COVID-19 mortality' | Only looks at timing |
| Amuedo-Dorantes et al. (2021); 'Timing of social distancing policies and COVID-19 mortality: county-level evidence from the U.S.' | Only looks at timing |
| Amuedo-Dorantes, Kaushal and Muchow (2020); 'Is the cure worse than the disease? County-level evidence from the COVID-19 pandemic in the United States' | Duplicate |
| Amuedo-Dorantes, Kaushal and Muchow (2021); 'Timing of social distancing policies and COVID-19 mortality: county-level evidence from the US' | Only looks at timing |
| Aparicio and Grossbard (2021); 'Are Covid fatalities in the US higher than in the EU, and if so, why?' | Not difference-in-difference |
| Arruda et al. (2021); 'Assessing the impact of social distancing on COVID-19 cases and deaths in Brazil: An instrumented difference-in-differences …' | Social distancing (not lockdowns) |

| 1. Study (Author & title) | 2. Reason for exclusion |
|---|---|
| Auger et al. (2020); 'Association between statewide school closure and COVID-19 incidence and mortality in the US' | Uses a time-series approach |
| Bakolis et al. (2021); 'Changes in daily mental health service use and mortality at the commencement and lifting of COVID-19 "lockdown" policy in 10 UK sites: a regression discontinuity in time' design' | Uses a time-series approach |
| Bardey, Fernández and Gravel (2021); 'Coronavirus and social distancing: do non-pharmaceutical-interventions work (at least) in the short run?' | Only looks at timing |
| Berardi et al. (2020); 'The COVID-19 pandemic in Italy: policy and technology impact on health and non-health outcomes' | Too few observations |
| Bhalla (2020); 'Lockdowns and closures vs COVID–19: COVID wins' | Uses modelling |
| Bharati and Fakir (2020); 'Pandemic Catch-22: How effective are mobility restrictions in halting the spread of COVID-19 in developing countries' | Duplicate |
| Björk et al. (2021); 'Impact of winter holiday and government responses on mortality in Europe during the first wave of the COVID-19 pandemic' | Only looks at timing |
| Bongaerts et al. (2021); 'Closed for business: The mortality impact of business closures during the Covid-19 pandemic' | Too few observations |
| Bongaerts et al. (2021); 'Closed for business: The mortality impact of business closures during the Covid-19 pandemic' | Duplicate |
| Bongaerts, Mazzola and Wagner (2020); 'Closed for business' | Duplicate |
| Born, Dietrich and Müller (2021); 'The lockdown effect: A counterfactual for Sweden' | Synthetic control study |
| Born, Dietrich and Müller (2021); 'The lockdown effect: A counterfactual for Sweden' | Duplicate |
| Borri et al. (2020); 'The "Great Lockdown": Inactive workers and mortality by Covid-19' | Too few observations |
| Bushman et al. (2020); 'Effectiveness and compliance to social distancing during COVID-19' | Social distancing (not lockdowns) |
| Canatay et al. (2021); 'Critical country-level determinants of death rate during Covid-19 pandemic' | Not difference-in-difference |
| Caselli et al. (2020); 'From the lockdown to the new normal: An analysis of the limitations to individual mobility in Italy following the Covid-19 crisis' | Do not look at mortality |
| Castaneda and Saygili (2020); 'The effect of shelter-in-place orders on social distancing and the spread of the COVID-19 pandemic: a study of Texas' | Uses a time-series approach |
| Cerqueti et al. (2021); 'The sooner the better: lives saved by the lockdown during the COVID-19 outbreak. The case of Italy' | Synthetic control study |
| Chaudhry et al. (2020); 'A country level analysis measuring the impact of government actions, country preparedness and socioeconomic factors on COVID-19 mortality and related health outcomes' | Not difference-in-difference |
| Chernozhukov, Kasahara and Schrimpf (2021); 'Mask mandates and other lockdown policies reduced the spread of COVID-19 in the US' | Duplicate |
| Chin et al. (2020); 'Effects of non-pharmaceutical interventions on COVID-19: A Tale of Three Models' | Uses modelling |

| 1. Study (Author & title) | 2. Reason for exclusion |
|---|---|
| Cho (2020); 'Quantifying the impact of nonpharmaceutical interventions during the COVID-19 outbreak: The case of Sweden' | Synthetic control study |
| Ciminelli and Garcia-Mandicó (2020); 'When and how do business shutdowns work? Evidence from Italy's first COVID-19 wave' | Too few observations |
| Ciminelli and Garcia-Mandicó (2021); 'Business shutdowns and covid-19 mortality' | Duplicate |
| Coccia (2020); 'The effect of lockdown on public health and economic system: findings from first wave of the COVID-19 pandemic for designing effective strategies to cope with future waves' | Only looks at timing |
| Coccia (2021); 'Different effects of lockdown on public health and economy of countries: Results from first wave of the COVID-19 pandemic' | Too few observations |
| Conyon and Thomsen (2021); 'COVID-19 in Scandinavia' | Synthetic control study |
| Conyon et al. (2020); 'Lockdowns and COVID-19 deaths in Scandinavia' | Too few observations |
| Dave et al. (2020); 'Did the Wisconsin Supreme Court restart a COVID-19 epidemic? Evidence from a natural experiment' | Synthetic control study |
| Delis, Iosifidi and Tasiou (2021); 'Efficiency of government policy during the COVID-19 pandemic' | Do not look at mortality |
| Dey et al. (2021); 'Lag time between state-level policy interventions and change points in COVID-19 outcomes in the United States' | Uses a time-series approach |
| Dreher et al. (2021); 'Policy interventions, social distancing, and SARS-CoV-2 transmission in the United States: a retrospective state-level analysis' | Do not look at mortality |
| Duchemin, Veber and Boussau (2020); 'Bayesian investigation of SARS-CoV-2-related mortality in France' | Uses modelling |
| Fair et al. (2021); 'Estimating COVID-19 cases and deaths prevented by non-pharmaceutical interventions in 2020-2021, and the impact of individual actions: a retrospective model …' | Uses modelling |
| Filias (2020); 'The impact of government policies effectiveness on the officially reported deaths attributed to covid-19' | Student paper |
| Fountoulakis et al. (2020); 'Factors determining different death rates because of the COVID-19 outbreak among countries' | Not difference-in-difference |
| Fowler et al. (2021); 'Stay-at-home orders associate with subsequent decreases in COVID-19 cases and fatalities in the United States' | Duplicate |
| Friedson et al. (2020); 'Did California's shelter-in-place order work? Early coronavirus-related public health effects' | Duplicate |
| Friedson et al. (2020); 'Shelter-in-place orders and public health: Evidence from California during the COVID-19 pandemic' | Synthetic control study |
| Fuss, Weizman and Tan (2020); 'COVID19 pandemic: How effective are interventive control measures and is a complete lockdown justified? A comparison of countries and states' | Do not look at mortality |
| Ghosh, Ghosh and Narymanchi (2020); 'A study on the effectiveness of lock-down measures to control the spread of COVID-19' | Synthetic control study |
| Glogowsky et al. (2021); 'How effective are social distancing policies? Evidence on the fight against COVID-19' | Only looks at timing |

| 1. Study (Author & title) | 2. Reason for exclusion |
|---|---|
| Glogowsky, Hansen and Schächtele (2020); 'How effective are social distancing policies? Evidence on the fight against COVID-19 from Germany' | Duplicate |
| Glogowsky, Hansen and Schächtele (2020); 'How effective are social distancing policies? Evidence on the fight against COVID-19 from Germany' | Duplicate |
| Gordon, Grafton and Steinshamn (2021); 'Cross-country effects and policy responses to COVID-19 in 2020: The Nordic countries' | Do not look at mortality |
| Gordon, Grafton and Steinshamn (2021); 'Statistical analyses of the public health and economic performance of Nordic countries in response to the COVID-19 pandemic' | Too few observations |
| Guo et al. (2020); 'Social distancing interventions in the United States: An exploratory investigation of determinants and impacts' | Duplicate |
| Hale et al. (2020); 'Global assessment of the relationship between government response measures and COVID-19 deaths' | Duplicate |
| Huber and Langen (2020); 'The impact of response measures on COVID-19-related hospitalization and death rates in Germany and Switzerland' | Duplicate |
| Huber and Langen (2020); 'Timing matters: The impact of response measures on COVID-19-related hospitalization and death rates in Germany and Switzerland' | Only looks at timing |
| Hunter et al. (2021); 'Impact of non-pharmaceutical interventions against COVID-19 in Europe: A quasi-experimental non-equivalent group and time-series' | Not difference-in-difference |
| Jain et al. (2020); 'A comparative analysis of COVID-19 mortality rate across the globe: An extensive analysis of the associated factors' | Do not look at mortality |
| Juranek and Zoutman (2021); 'The effect of non-pharmaceutical interventions on the demand for health care and mortality: evidence on COVID-19 in Scandinavia' | Too few observations |
| Juranek and Zoutman (2021); 'The effect of non-pharmaceutical interventions on the demand for health care and mortality: evidence on COVID-19 in Scandinavia' | Duplicate |
| Juranek and Zoutman (2021); 'The effect of non-pharmaceutical interventions on the demand for health care and mortality: evidence from COVID-19 in Scandinavia' | Duplicate |
| Kakpo and Nuhu (2020); 'Effects of social distancing on COVID-19 infections and mortality in the US' | Social distancing (not lockdowns) |
| Kapitsinis (2021); 'The underlying factors of excess mortality in 2020: A cross-country analysis of pre-pandemic healthcare conditions and strategies to cope with Covid-19' | Not difference-in-difference |
| Kapoor and Ravi (2020); 'Impact of national lockdown on COVID-19 deaths in select European countries and the US using a Changes-in-Changes model' | Too few observations |
| Khan et al. (2021); 'Assessing the impact of policy measures in reducing the COVID-19 pandemic: A case study of South Asia' | Too few observations |
| Khatiwada and Chalise (2020); 'Evaluating the efficiency of the Swedish government policies to control the spread of Covid-19' | Student paper |

| 1. Study (Author & title) | 2. Reason for exclusion |
|---|---|
| Korevaar et al. (2020); 'Quantifying the impact of US state non-pharmaceutical interventions on COVID-19 transmission' | Do not look at mortality |
| Kumar et al. (2020); 'Prevention- versus promotion-focus regulatory efforts on the disease incidence and mortality of COVID-19: A multinational diffusion study using functional data …' | Do not look at mortality |
| Langeland et al. (2021); 'The effect of state level COVID-19 stay-at-home orders on death rates' | Not difference-in-difference |
| Le et al. (2020); 'Impact of government-imposed social distancing measures on COVID-19 morbidity and mortality around the world' | Uses a time-series approach |
| Liang et al. (2020); 'Covid-19 mortality is negatively associated with test number and government effectiveness' | Not effect of lockdowns |
| Mader and Rütternauer (2021); 'The effects of non-pharmaceutical interventions on COVID-19-related mortality: A generalized synthetic control approach across 169 countries' | Synthetic control study |
| Matzinger and Skinner (2020); 'Strong impact of closing schools, closing bars and wearing masks during the Covid-19 pandemic: results from a simple and revealing analysis' | Uses modelling |
| Mccafferty and Ashley (2020); 'Covid-19 social distancing interventions by state mandate and their correlation to mortality in the United States' | Duplicate |
| Medline et al. (2020); 'Evaluating the impact of stay-at-home orders on the time to reach the peak burden of Covid-19 cases and deaths: Does timing matter?' | Only looks at timing |
| Mu et al. (2020); 'Effect of social distancing interventions on the spread of COVID-19 in the state of Vermont' | Uses modelling |
| Nakamura (2020); 'The Impact of rapid state policy response on cumulative deaths caused by COVID-19' | Student paper |
| Neidhöfer and Neidhöfer (2020); 'The effectiveness of school closures and other pre-lockdown COVID-19 mitigation strategies in Argentina, Italy, and South Korea' | Synthetic control study |
| Oliveira (2020); 'Does "staying at home" save lives? An estimation of the impacts of social isolation in the registered cases and deaths by COVID-19 in Brazil' | Social distancing (not lockdowns) |
| Palladino et al. (2020); 'Effect of implementation of the lockdown on the number of COVID-19 deaths in four European countries' | Uses a time-series approach |
| Palladino et al. (2020); 'Effect of timing of implementation of the lockdown on the number of deaths for COVID-19 in four European countries' | Duplicate |
| Palladino et al. (2020); 'Excess deaths and hospital admissions for COVID-19 due to a late implementation of the lockdown in Italy' | Uses a time-series approach |
| Pan et al. (2021); 'Heterogeneity in the effectiveness of non-pharmaceutical interventions during the first SARS-CoV2 wave in the United States' | Duplicate |
| Peixoto et al. (2020); 'Rapid assessment of the impact of lockdown on the COVID-19 epidemic in Portugal' | Uses modelling |
| Piovani et al. (2021); 'Effect of early application of social distancing interventions on COVID-19 mortality over the first pandemic wave: An analysis of longitudinal data from 37 countries' | Only looks at timing |

| 1. Study (Author & title) | 2. Reason for exclusion |
|---|---|
| Porto et al. (2022); ‘Lockdown, essential sectors, and Covid-19: Lessons from Italy’ | Too few observations |
| Reinbold (2021); ‘Effect of fall 2020 K-12 instruction types on CoViD-19 cases, hospital admissions, and deaths in Illinois counties’ | Synthetic control study |
| Renne, Roussellet and Schwenkler (2020); ‘Preventing COVID-19 fatalities: State versus federal policies’ | Uses modelling |
| Shanmugam et al. (2021); ‘A report card on prevention efforts of covid-19 deaths in US’ | Not difference-in-difference |
| Siedner et al. (2020); ‘Social distancing to slow the US COVID-19 epidemic: Longitudinal pretest–posttest comparison group study’ | Duplicate |
| Siedner et al. (2020); ‘Social distancing to slow the US COVID-19 epidemic: Longitudinal pretest–posttest comparison group study’ | Uses a time-series approach |
| Silva, Filho and Fernandes (2020); ‘The effect of lockdown on the COVID-19 epidemic in Brazil: Evidence from an interrupted time series design’ | Uses a time-series approach |
| Stamam et al. (2020); ‘Impact of lockdown measure on COVID-19 incidence and mortality in the top 31 countries of the world’ | Uses a time-series approach |
| Steinegger et al. (2021); ‘Retrospective study of the first wave of COVID-19 in Spain: Analysis of counterfactual scenarios’ | Only looks at timing |
| Stephens et al. (2020); ‘Does the timing of government COVID-19 policy interventions matter? Policy analysis of an original database’ | Only looks at timing |
| Stockenhuber (2020); ‘Did we respond quickly enough? How policy-implementation speed in response to COVID-19 affects the number of fatal cases in Europe’ | Not difference-in-difference |
| Supino et al. (2020); ‘The effects of containment measures in the Italian outbreak of COVID-19’ | Uses a time-series approach |
| Thayer et al. (2021); ‘An interrupted time series analysis of the lockdown policies in India: A national-level analysis of COVID-19 incidence.’ | Uses a time-series approach |
| Timelli and Girardi (2021); ‘Effect of timing of implementation of containment measures on Covid-19 epidemic. The case of the first wave in Italy’ | Only looks at timing |
| Toya and Skidmore (2021); ‘A cross-country analysis of the determinants of Covid-19 fatalities’ | Not difference-in-difference |
| Trivedi and Das (2020); ‘Effect of the timing of stay-at-home orders on COVID-19 infections in the United States of America’ | Only looks at timing |
| Tsai et al. (2021); ‘Coronavirus Disease 2019 (COVID-19) transmission in the United States before versus after relaxation of statewide social distancing measures’ | Uses a time-series approach |
| Umer and Khan (2020); ‘Evaluating the effectiveness of regional lockdown policies in the containment of Covid-19: Evidence from Pakistan’ | Too few observations |
| VoPham et al. (2020); ‘Effect of social distancing on COVID-19 incidence and mortality in the US’ | Do not look at mortality |
| Wu and Wu (2020); ‘Stay-at-home and face mask policies intentions inconsistent with incidence and fatality during US COVID-19 pandemic’ | Not difference-in-difference |

| 1. Study (Author & title) | 2. Reason for exclusion |
|---|---|
| Xu et al. (2020); ‘Associations of stay-at-home order and face-masking recommendation with trends in daily new cases and deaths of laboratory-confirmed COVID-19 in the United States’ | Do not look at mortality |
| Yehya et al. (2021); ‘Statewide interventions and Coronavirus Disease 2019 mortality in the United States: An observational study’ | Is purely descriptive |
| Yehya, Venkataramani and Harhay (2020); ‘Statewide Interventions and Coronavirus Disease 2019 Mortality in the United States: An Observational Study’ | Only looks at timing |
| Yilmazkuday (2021); ‘Stay-at-home works to fight against COVID-19: International evidence from Google mobility data’ | Social distancing (not lockdowns) |
| Ylli et al. (2020); ‘The lower COVID-19 related mortality and incidence rates in Eastern European countries are associated with delayed start of community circulation Alban Ylli1 …’ | Not effect of lockdowns |
| Zimmerman and Anderson (2021); ‘Association of the timing of school closings and behavioral changes with the evolution of the coronavirus disease 2019 pandemic in the US’ | Uses a time-series approach |

### *Interpretation of estimates and conversion to standardised estimates*

In Table 20 we describe for each study how we interpret their results and convert their estimates to our standardised estimate. For studies not included in the meta-analysis, we describe why. Standard errors are converted such that the t-value, calculated based on standardised estimates and standard errors, remains unchanged. When confidence intervals are reported rather than standard errors, we calculate standard errors using the t-distribution with ∞ degrees of freedom (i.e., 1.96 for a 95 per cent confidence interval).

## Table 20: Notes concerning the standardisation of results of the studies included in the meta-analysis

| 1. Study (Author & title) | 2. Date published | 3. Journal | 4. Notes concerning the calculation of standardised estimates |
|---|---|---|---|
| Alderman and Harjoto (2020); 'COVID-19: U.S. shelter-in-place orders and demographic characteristics linked to cases, mortality, and recovery rates' | 26-Nov-20 | Transforming Government: People, Process and Policy | We use the 1% effect noted by the authors in 'We find that the natural log of the duration (in days) that the state instituted shelter-in-place reduces percentages […] of mortality by […] 0.0001%, or approximately 1% of the means of percentages of […] deaths per capita in our sample.' The standard error is calculated on basis of the t-value in Table 3. |
| An et al. (2021), 19; 'Policy design for COVID-19: Worldwide evidence on the efficacies of early mask mandates and other policy interventions' | 06-Sep-21 | Public Administration Review | We use the country fixed-effects models as the authors state: 'To capture the dynamic nature of the relationships, namely, how policy adoption relates to infection and mortality rates over time, we turn to panel data using within-country variations (i.e., country fixed-effects).' |
| Ashraf (2020); 'Socioeconomic conditions, government interventions and health outcomes during COVID-19' | 1-Jul-20 | ResearchGate | It is unclear whether they prefer the model with or without the interaction term. In the meta-analysis, we use an average of –0.326 (Table 3, without) and –0.073 (Table 6, with) deaths per million per stringency point (i.e., –0.200). The standardised estimate is the average effect in Europe and United States respectively calculated as (Actual COVID-19 mortality) / (COVID-19 mortality with recommendation policy) –1, where (COVID-19 mortality with recommendation policy) is calculated as ((Actual COVID-19 mortality) – Estimate x Difference in stringency x population). Stringencies in Europe and United States are equal to the average stringency from 16 March to 15 April 2020 (76 and 74 respectively) and the stringency for the policy based solely on recommendations is 44 following Hale, Hale, et al. (2020). |
| Berry et al. (2021); 'Evaluating the effects of shelter-in-place policies during the COVID-19 pandemic' | 24-Feb-21 | PNAS | The estimated effect of SIPOs, an increase in deaths by 0,654 per million after 14 days (significant, see Fig. 2), is converted to a relative effect on a state basis based on data from Our World in Data. For states which did implement SIPO, we calculate the number of deaths without SIPO as the number of official COVID-19 deaths 14 days after SIPO was implemented minus 0,654 extra deaths per million. For states which did not implement SIPO, we calculate the number of deaths with SIPO as the number of official COVID-19 deaths 14 days after 31 March 2020, plus 0,654 extra deaths per million. We use 31 March 2020, as this was the average date on which SIPO was implemented in the 40 states which did implement SIPO. Using this approximation, the effect of SIPOs in the U.S. is 1,1% more deaths after 14 days. Standardised standard errors are not available. |

| 1. Study (Author & title) | 2. Date published | 3. Journal | 4. Notes concerning the calculation of standardised estimates |
|---|---|---|---|
| Bjørnskov (2021); 'Did lockdown work? An economist's cross-country comparison' | 29-Mar-21 | CESifo Economic Studies | We use estimates from Table 2 (four weeks). Bjørnskov (2021) uses a log-log specification which means that the standardised estimate can be calculated as the average of the effect in Europe and United States, where the effect for each is calculated as exp((ln(policy stringency) x estimate) – exp(ln(recommendation stringency)) x estimate). |
| Bonardi et al. (2020); 'Fast and local: How did lockdown policies affect the spread and severity of the covid-19' | 8-Jun-20 | CEPR Covid Economics | Find that, worldwide, internal NPIs have prevented about 650,000 deaths (3.11 deaths were prevented for each death that occurred, i.e., 76% effect). However, this effect is for any lockdown including a Swedish lockdown. They do not find an extra effect of stricter lockdowns and state that 'our results point to the fact that people might adjust their behaviors quite significantly as partial measures are implemented, which might be enough to stop the spread of the virus.' Hence, whether the baseline is Sweden, which implemented a ban on large gatherings early in the pandemic, or the baseline is 'doing nothing' can affect the magnitude of the estimated impacts. Since all Western countries did something and estimates in other reviewed studies are relative to doing less – and hence, not to doing nothing, we report the result from Bonardi et al. as compared to 'doing less'. Hence, for Bonardi et al. we use 0% as the standardised estimate in the meta-analysis for each NPI (SIPO, regional lockdown, partial lockdown, and border closure (stage 1, stage 2 and full)), because all NPIs are insignificant (compared to Sweden's 'doing the least' lockdown). |
| Chernozhukov et al. (2021); 'Causal impact of masks, policies, behavior on early covid-19 pandemic in the U.S.' | 1-Jan-21 | Journal of Econometrics | The study looks at the effect of NPIs on growth rates but does include an estimate of the effect on total mortality at the end of the study period for employee face masks (–34%), business closure (–29%), and SIPO (–18%), but not for school closures (which we therefore exclude). In reporting the results of their counterfactual, they alter between 'fewer deaths with NPI' and 'more deaths without NPI.' We have converted the latter to the former as estimate/(1+estimate) so 'without business closures deaths would be about 40% higher' corresponds to 'with business closures deaths would be about 29% lower.'<br>They have two model specifications. One *excluding* national case numbers (their Table 7) and one *including* national case numbers (their Table 9). The latter find much smaller effects of some NPIs on mortality, but since they only calculate the counterfactual for the specification excluding national case numbers, we do not include the estimates from their Table 9 in our analysis. |

| 1. Study (Author & title) | 2. Date published | 3. Journal | 4. Notes concerning the calculation of standardised estimates |
|---|---|---|---|
| Chisadza et al. (2021); ‘Government effectiveness and the COVID-19 pandemic’ | 10-Mar-21 | MDPI | They use a Poisson model, so a one-unit change in the predictor variable will change the log of the dependent variable (mortality) by respective regression coefficient. With the estimates from Table 2 (model 4), the change in ln(mortality) is equal to SI x 0.0874 – $SI^2$ x –0.0007, where SI is the value of the stringency index. Stringencies in Europe and United States are equal to the average stringency from 16 March to 15 April 2020 (76 and 74 respectively) and the stringency for the policy based solely on recommendations is 44 following Hale et al. (2020). Hence, the standardised estimate for Europe can be calculated as exp((76-44) x 0.0874 – ($76^2$–$44^2$) x 0.0007)–1 = 9%. The effect for United States is calculated similarly (14%). The standardised standard error is calculated as (standardised estimate / estimate x standard error). However, since Chisadza et al. (2021) use a quadratic term, we cannot calculate the standard error. Instead, we use the standard error from their linear model as a proxy for the standard error for the average effect.<br>Note: In an earlier version of this study, we used the linear estimate without taking the exponential. |
| Dave et al. (2021); ‘When do shelter-in-place orders fight Covid-19 best? Policy heterogeneity across states and adoption time’ | 3-Aug-20 | Economic Inquiry | The study looks at the effect of SIPOs on growth rates but does include an estimate of the effect on total mortality after 20+ days for model 1 and 2 in Table 7. Since model 3, 4 and 5 have estimates similar to model 2, we use an average of model 1 to 5, where the estimates of model 3 to 5 are calculated as (standardised estimate model 2) / (estimate model 2) x estimate model 3/4/5. |
| Ertem et al. (2021); ‘The impact of school opening model on SARS-CoV-2 community incidence and mortality’ | 27-Oct-21 | Nature Medicine | Include OxCGRT policy variables as covariates in their regression models, but do not present estimates for these variables. Since we do not have access to covariation between coefficients, the coefficients for different weeks are assumed independent. This results in an underestimation of standard error for our standardised estimate.<br>We report the results from these models from the school mode–week interaction terms as marginal effects that are interpreted as the adjusted absolute effect of school mode per week on the outcome. |
| Fowler et al. (2021); ‘Stay-at-home orders associate with subsequent decreases in COVID-19 cases and fatalities in the United States’ | 10-Jun-21 | PLOS ONE | The study looks at the effect of SIPOs on growth rates but does include an estimate of the effect on total mortality after three weeks (35% reduction in deaths) which is used in the meta-analysis. |

| 1. Study (Author & title) | 2. Date published | 3. Journal | 4. Notes concerning the calculation of standardised estimates |
|---|---|---|---|
| Fuller et al. (2021); 'Mitigation policies and COVID-19–associated mortality – 37 European countries, January 23–June 30, 2020' | 15-Jan-21 | Morbidity and Mortality Weekly Report | For each 1-unit increase in OxCGRT stringency index, the cumulative mortality decreases by 0.55 deaths per 100,000. The standardised estimate is the average effect in Europe and United States respectively calculated as (Actual COVID-19 mortality) / (COVID-19 mortality with recommendation policy) –1, where (COVID-19 mortality with recommendation policy) is calculated as ((Actual COVID-19 mortality) – Estimate x Difference in stringency x population). Stringencies in Europe and United States are equal to the average stringency from 16 March to 15 April 2020, (76 and 74 respectively) and the stringency for the policy based solely on recommendations is 44 following Hale, Angrist, Goldszmidt, et al. (2021). |
| Gibson (2020); 'Government mandated lockdowns do not reduce Covid-19 deaths: implications for evaluating the stringent New Zealand response' | 18-Aug-20 | New Zealand Economic Papers | We use the two graphs to the left in Figure 3, where we extract the data from the rightmost datapoint (i.e., % impact of county lockdowns on Covid-19 deaths by 1/06/2020). We then take the average of the estimates found in the two graphs, because it is unclear which estimate the author prefers. |
| Goldstein et al. (2021); 'Lockdown fatigue: The diminishing effects of quarantines on the spread of COVID-19' | 4-Feb-21 | CID Faculty Working Paper | We convert the effect in Figure 4 after 90 days (log difference −1.16 of a standard deviation change) to deaths per million per stringency following footnote 3, so the effect is e^−1.16 − 1 = −0.69 decline in weekly deaths per million per standard deviation. We convert to total effect by multiplying with 90 days and 'per point' by dividing with SD = 22.3 (corresponding to the SD for the 151 countries with data before 19 March 2020 – using all data yields similar results) yielding −0.03 deaths per week per million per stringency point. The standardised estimate is the average effect in Europe and United States respectively calculated as (Actual COVID-19 mortality) / (COVID-19 mortality with recommendation policy) −1, where (COVID-19 mortality with recommendation policy) is calculated as ((Actual COVID-19 mortality) − Estimate x Difference in stringency x population). Stringencies in Europe and United States are equal to the average stringency from 16 March to 15 April 2020, (76 and 74 respectively) and the stringency for the policy based solely on recommendations is 44 following Hale, Hale, et al. (2020). Actual COVID-19 mortality is cumulative mortality by 30 June 2020. Hence, the standardised estimate is calculated for the first wave. If we instead calculate the standardised estimate for the full data period used by Goldstein et al. (2021), the average stringency is 58 in Europe and 50 in United States and cumulative mortality is much larger. Both effects cause the estimated effect to be (much) lower. |

| 1. Study (Author & title) | 2. Date published | 3. Journal | 4. Notes concerning the calculation of standardised estimates |
|---|---|---|---|
| Guo et al. (2021); 'Mitigation interventions in the United States: An exploratory investigation of determinants and impacts' | 21-Sep-20 | Research on Social Work Practice | We use estimates for 'Proportion of Cumulative Deaths Over the Population' (per 10,000) in Table 3. We interpret this number as the change in cumulative deaths over the population in per cent and is therefore the same as our standardised estimate. |
| Hale, Angrist, Hale, et al. (2021); 'Government responses and COVID-19 deaths: Global evidence across multiple pandemic waves' | 09-Jul-21 | PLOS ONE | We use the estimate from Table 1 (1). Standardised estimate is calculated as the average of the effect in Europe and United States, where the effect for each is calculated as exp((policy stringency − recommendation stringency) x estimate) −1. Stringencies in Europe and United States are equal to the average stringency from 16 March to 15 April 2020 (76 and 74 respectively) and the stringency for the policy based solely on recommendations is 44 following Hale et al. (2020). |
| Leffler et al. (2020); 'Association of country-wide coronavirus mortality with demographics, testing, lockdowns, and public wearing of masks' | 26-Oct-20 | ASTMH | Their 'mask recommendation' includes some countries, where masks were mandated and may (partially) capture the effect of mask mandates. However, the authors' focus is on recommendation, so we do interpret their result as a voluntary effect – not an effect of mask mandate. Using estimates from Table 2 and assuming NPIs were implemented 15 March (8 weeks in total by end of study period), standardised estimates are calculated as 8^est-1. |
| Sears et al. (2020); 'Are we #stayinghome to flatten the curve?' | 6-Aug-20 | medRxiv | Find that SIPOs lower mortality by 29-35%. We use the average (32%) as our standardised estimate. Standardised standard errors are calculated based on estimates and standard errors from (Table 4) assuming they are linearly related to estimates. |
| Shiva and Molana (2021); 'The luxury of lockdown' | 9-Apr-21 | The European Journal of Development Research | The estimate with 8 weeks lag is insignificant, and preferable given our empirical strategy. However, they use the 4-week lag when elaborating the model to differentiate between high- and low-income countries, so the 4-week lag estimate for rich countries is used in our meta-analysis. Standardised estimate is calculated as the average of the effect in Europe and United States, where the effect for each is calculated as exp((policy stringency − recommendation stringency) x estimate) −1. Stringencies in Europe and United States are equal to the average stringency from 16 March to 15 April 2020 (76 and 74 respectively) and the stringency for the policy based solely on recommendations is 44 following Hale et al. (2020). |

| 1. Study (Author & title) | 2. Date published | 3. Journal | 4. Notes concerning the calculation of standardised estimates |
|---|---|---|---|
| Spiegel and Tookes (2021); 'Business restrictions and Covid-19 fatalities' | 18-Jun-21 | The Review of Financial Studies | We use weighted average of estimates for Table 4, 6, and 9. Since authors state that they place more weight on the findings in Table 9, Table 9 weights by 50% while Table 4 and 6 weights by 25%. We estimate the effect on total mortality from effect on growth rates based on authors calculation showing that estimates of −0.049 and −0.060 reduces new deaths by 12.5% 15.3% respectively. We use the same relative factor on other estimates. |
| Stokes et al. (2020); 'The relative effects of non-pharmaceutical interventions on early Covid-19 mortality: natural experiment in 130 countries' | 6-Oct-20 | medRxiv | We use estimates from Table 'Regression results, mean policy strictness (combination of timing and strictness)' in 'Additional file'). We use the average of their 24-day and 38-day specification from model 5. We calculate the effect of each NPI as the average effect in all of U.S./Europe. First, mortality rates without interventions are calculated for each country/state as the number of days each intervention was in effect before 8 May 2020 (24 days before end of study period). Based on this, we calculate the effect on mortality for each intervention in each country/state as 'days in in effect' x estimate x population (/mio). E.g., in Austria, workplaces were closed for 54 days in this period (workplaces closed on 16 March 2020). The total effect of workplace closures on mortality is then 54 days x −0.286 (average of −0.258 (24-day spec.) and −0.313 (38-day spec.) x 9 mio = 139 avoided deaths. Doing the same for other NPIs, the total effect of the Austrian lockdown was 336 avoided deaths by 1 June so 1,004 would have died *without* lockdown while 668 died *with* lockdown. This is done for all countries in Europe and all states in United States. In total, 204,709 would have died in Europe *without* lockdowns (compared to 172,714 *with* lockdowns) and 122,469 (compared to 108,293) in United States. The effect of each intervention in Europe and United States is then calculated assuming the intervention is in place from 15 March 2020 (54 days). Hence, for Europe, the effect of workplace closure is 54 days x −0.286 (est.) x 748 million = 11,529 avoided deaths out of 204,709 potential deaths which is equal to −5.6%. For United States the effect is 5,060 avoided deaths out of 122,469 potential deaths (−4.1%), and the average effect of workplace closure is thus 4.9%. |

| 1. Study (Author & title) | 2. Date published | 3. Journal | 4. Notes concerning the calculation of standardised estimates |
|---|---|---|---|
| Yang et al. (2021); 'What is the relationship between government response and COVID-19 pandemics? Global evidence of 118 countries' | 28-Aug-21 | Structural Change and Economic Dynamics | The standardised estimate is the average effect in Europe and United States respectively calculated as (Actual COVID-19 mortality) / (COVID-19 mortality with recommendation policy) −1, where (COVID-19 mortality with recommendation policy) is calculated as (Actual COVID-19 mortality + Avoided mortality). Avoided mortality is calculated as Estimate x Difference in stringency x number of days x population). Stringencies in Europe and United States are equal to the average stringency from 16 March to 15 April 2020 (76 and 74 respectively) and the stringency for the policy based solely on recommendations is 44 following Hale, Hale, et al. (2020). Actual COVID-19 mortality is cumulative mortality by 30 June 2020, and the number of days is 107 (from 16 March to 30 June 2020). Hence, the standardised estimate is calculated for the first wave. If we instead calculate the standardised estimate for the full data period used by Yang et al. (2021), the average stringency is 57 in Europe and 51 in United States and cumulative mortality is much larger. Both effects cause the estimated effect for the full period to be (much) lower. |

# Appendix II: Public response to the first edition of our working paper

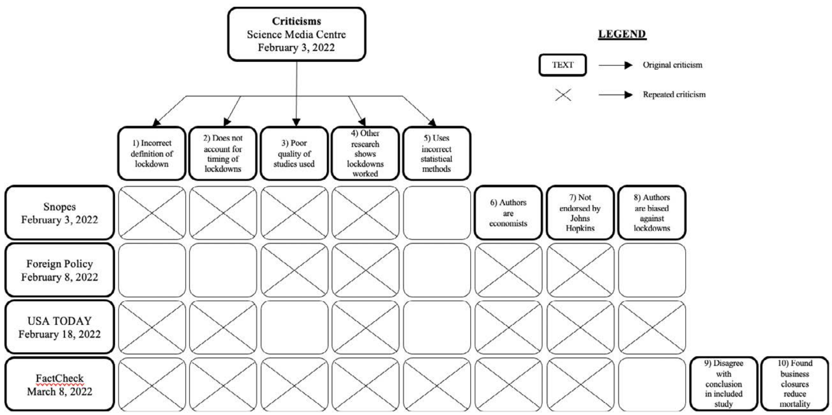
The Johns Hopkins Institute for Applied Economics, Global Health, and the Study of Business Enterprise, which one of us (Hanke) founded and co-directs, published ‘A Systematic Literature Review and Meta-Analysis of the Effects of Lockdowns on COVID-19 Mortality’ in its *Studies in Applied Economics* working paper series on 21 January 2022. The working paper’s findings – that lockdowns had a negligible public health effect measured by mortality – and its policy conclusions – that lockdown policies are ill-founded and should be rejected out of hand –attracted considerable attention in the media, in the White House and halls of the U.S. Congress, and among public health experts.

But, it was the strong endorsement of Dr. Marty Makary, a distinguished professor of medicine at the Johns Hopkins School of Medicine, during his 2 February appearance on *Tucker Carlson Tonight* that set off a media firestorm. Indeed, on 3 February, the Science Media Centre in London issued a press release, Science Media Centre (2022), with statements by Prof. Neil Ferguson, Dr Seth Flaxman, Prof. Samir Bhatt – all affiliated with Imperial College London and authors of two of the studies (Ferguson et al. 2020 and Flaxman et al. 2020) we implicitly criticised – and Prof. David Paton (Nottingham University Business School). The release contained several criticisms of our working paper from the Imperial College team. Those were authored by Prof. Ferguson, Dr Flaxman, and Prof. Bhatt. The press release also contained positive comments by Prof. Paton.

The accompanying Figure 20 denotes five of the most scientific criticisms raised in Science Media Centre (2022) as well as five criticisms raised by follow-up fact checkers. Although we believe that the criticisms have little or no merit, we are not going to engage in a critique of them in this Appendix, as the relevant criticisms are dealt with, either directly or indirectly, in the text of this book.[117] Our purpose is to illustrate how our working paper was handled by the media.

117 Referring to numbers in Figure 20: 1) The term ‘lockdown’ has mainly been used to describe two different things. We define our use of lockdown on p. 22. 2) We examine the average lockdown – not the optimal lockdown. Hence, there are good reasons to exclude studies focusing on timing. We discuss timing on p. 42 and p. 126. 3) The quality of the included studies is handled using bias dimensions (see p. 71). 4) We clearly describe which studies are included and – to handle the ‘Other research shows lockdowns prevent deaths’ critique –we now explain in more detail why some of the prominent studies such as Flaxman et al. (2020) are both very problematic and ineligible, see section 2.2 on p. 34. We also relate our results to the conclusions in other reviews in section 5.2.1 on p. 115. 5) We use more bias-dimensions to handle the ‘Used incorrect statistical methods’ critique (see p. 71). 6) The ‘authors are economists’ is an unscientific and irrelevant comment that we do not handle, but simply note that economists are skilled in handling meta-analyses in a wide range of contexts. 7) The ‘not endorsed by Johns Hopkins’ critique is an unscientific and irrelevant comment. As a matter of fact, Johns Hopkins University does not endorse specific research projects published by its faculty and staff. That said, our study was published by the Johns Hopkins Institute for Applied Economics, Global Health, and the Study of Business Enterprise, a research institute located at Johns Hopkins University. 8) The ‘authors are biased against lockdowns’ critique is an unscientific comment which was handled in the working paper where our search strategy and eligibility criteria were clearly described (see section 2 on p. 31 in this updated version). 9) We disagree with the conclusion in Chisadza et al. (2021). We explain why in section 3.1, p. 48. 10) We handle this critique on p. 31, where we write: ‘We believe that one major mistake in our first version was our failure to explain that the overall conclusions do not depend on whether the impact of lockdowns on COVID-19 mortality was 0.2 per cent, 3.0 per cent or 15 per cent.’

**Figure 20: Flow chart for criticisms raised in Science Media Centre**




A few hours after the Science Media Centre press release, *Snopes* published a report that contained eight criticisms (Snopes 2022). Of those, five were contained in the Science Media Centre press release. And three new 'criticisms' were added (see Figure 20 and footnote 117).

On 8 February, *Foreign Policy* published a critique (Garrett 2022). It used three of the Science Media Centre's criticisms and two of the new ones added by *Snopes*. In short, *Foreign Policy*'s article did not present any new arguments.

Then, *USA Today* entered the picture on 18 February (USA Today 2022). The fact checkers at *USA Today* offered seven criticisms – four were contained in the Science Media Centre press release and three in *Snopes*. On 8 March, *FactCheck* presented ten criticisms (FactCheck 2022). They included six from the Science Media Centre (including all five that we identified in Figure 20), two from *Snopes*, and two new criticisms (see Figure 20 and footnote 117).

There was, of course, extensive reportage about our working paper that appeared in the early February-April 2022 period. This material was highly repetitive, echoing material presented on 3 February in either the Science Media Centre press release or the *Snopes* report. There were several similar reports, but to avoid repetition ourselves, we limited our review to the five reports contained in Figure 20.

Of particular note is the fact that the post-Science Media Centre critiques exclude any mention of Prof. Paton's favourable appraisals of our working paper. For example, Prof. Paton made the following four points in Science Media Centre (2022):

1. 'Both parts of the paper (systematic review and the meta analysis) make a significant contribution to our understanding of lockdown effects.'

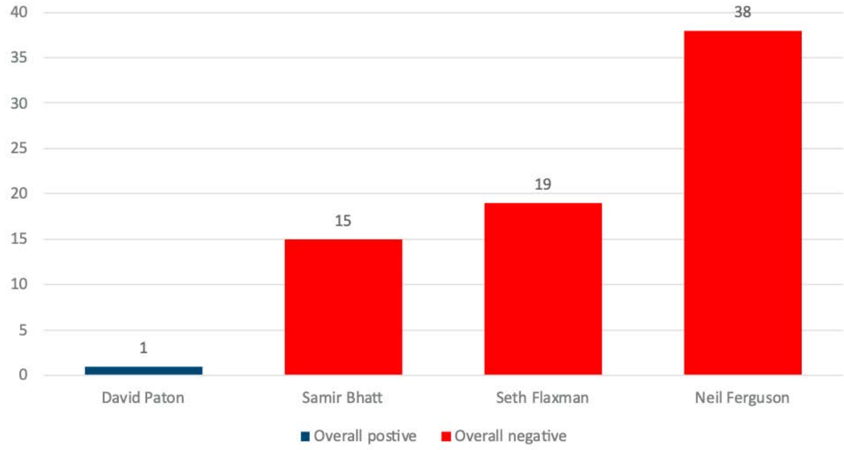
2. 'Key to a systematic review like this are the sets of search & exclusion criteria. The paper is very transparent about this which is good. They focus on difference-in-difference empirical studies. i.e. they look at papers which compare the impact of an intervention on mortality by looking before & after, but relative to other areas which did not have the intervention. As a result, modelling studies (like the well-known Flaxman Nature paper) are excluded. That is not controversial.'

3. '[The result] is pretty consistent with other, non-systematic reviews (e.g. Herby & Allen) which is reassuring. It is also consistent with the (few) studies which look at the impact on overall excess mortality.

4. 'More marginal in my view is their exclusion of synthetic control method (SCM) papers. Some of these paper[s] find a significant impact of NPIs on mortality so including them might have led to somewhat higher average mortality effects. The paper gives a robust defence of their exclusion, but I think you would get people on both sides of that debate.'

None of these positive comments were contained in the fact-checking articles that followed the Science Media Centre's press release on 3 February. Indeed, if you google 'David Paton Johns Hopkins lockdown' in February 2022, you will only get one single hit in English – a positive article in *The Spectator World*. If you do the same with Samir Bhatt, Seth Flaxman, and Neil Ferguson, you will get more than 70 hits, as illustrated in Figure 21.

**Figure 21: English Google hits on researcher name, 'Johns Hopkins', and 'lockdowns' in February 2022**




In closing this Appendix, we would like to indicate that we received extensive private reviews and comments on our working paper. After all, that is the purpose of publishing working papers. Our professional correspondents did engage in a serious primary reading of our working paper and made many useful comments and suggestions. Most of their names appear in our acknowledgements.

We engaged in a thorough review and revision of our 21 January 2022 working paper. We can report that one error of commission was found in the original. It was not detected by any fact checkers or by those we corresponded with, but by us. The error was a computational error that involved logarithms. It was 'small' and did not materially affect our results.

# Appendix III: Our letter to Social Science Research Network (SSRN)

On 5 August 2022, we received a letter from SSRN – a network ‘devoted to the rapid worldwide dissemination of research’[118] – after we had tried to upload our working paper to the network.

In short, SSRN did not want to publish our working paper. A decision we found quite disturbing and based on a very questionable basis.

Below is SSRN’s letter to us and our reply.

***SSRN’s letter to us, sent 5 August 2022***

The SSRN Processing Team has added the following comment to your submission, A Literature Review and Meta-Analysis of the Effects of Lockdowns on COVID-19 Mortality – II (Abstract ID 4170981):

> Thank you for your interest in submitting your paper to SSRN. Given the need to be cautious about posting medical content, SSRN is selective on the papers we post. Your paper has not been accepted for posting on SSRN.

118 From SSRN’s website. See https://www.ssrn.com/index.cfm/en/

***Our letter to SSRN, sent 28 August 2022***

To the SSRN,

Dear Sir/Madam,

We have submitted a working paper for posting with the SSRN, *A Literature Review and Meta-Analysis of the Effects of Lockdowns on COVID-19 Mortality – II*, initially published as a working paper at Johns Hopkins. See https://sites.krieger.jhu.edu/iae/files/2022/06/A-Systematic-Review-and-Meta-Analysis-of-the-Effects-of-Lockdowns-of-COVID-19-Mortality-II.pdf

Our submission was rejected based on the following argument: 'the need to be cautious about posting medical content'. See your letter below of August 5, 2022.

We object to this rejection. Our paper is authored by three economists. It belongs to the field of policy analysis, covering a truly unique natural experiment: the use of lockdowns as an instrument to influence mortality during the COVID-19 pandemic. We do not deal with medical drugs, prescriptions etc., we deal with restrictions that potentially inhibit the free movement of the public. We work in the field of health economics – a well-established field within economics and the social sciences. Indeed, SSRN has posted many papers in this field, including working papers on COVID-19 policy matters authored by one of us (Herby).

Moving from the general to the specific, allow us to itemize our arguments in support of our request to post our paper.

1. Our paper is published as a working paper at one of the leading research universities in the United States, if not the world, The Johns Hopkins University. It meets high academic standards. See https://sites.krieger.jhu.edu/iae/files/2022/06/A-Systematic-Review-and-Meta-Analysis-of-the-Effects-of-Lockdowns-of-COVID-19-Mortality-II.pdf.

2. Our paper is a meta-analysis. We have strictly followed the standard procedure for such a study by first publishing our protocol. It was posted on SSRN in the summer of 2021. It goes without saying that, at that time, the protocol was published by

SSRN and there was no rejection due to the fact that it contained 'medical content'. We find it remarkable that SSRN accepted our protocol but rejected the study that followed our protocol. See https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3872977.

In addition, SSRN has allowed a comment to the first version of our meta-study to be posted. We find it remarkable that you reject our updated version where we handle and reply to the comments. See: https://papers.ssrn.com/sol3/papers.cfm?abstract_id=4032477

3. Several of the papers included in our meta-analysis have been posted at SSRN as working papers. See https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3804077 and https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3665588 for examples.

4. One of us, Lars Jonung, posted in 2006 a co-authored working paper on the economic effects of a pandemic on the European economy. This paper has been on the top-ten list of SSRN in its category several times. We find it noteworthy that this paper remains posted while our most recent paper – which deals with a similar issue – is rejected. See https://papers.ssrn.com/sol3/papers.cfm?abstract_id=920851

5. Our paper is posted on the REPEC (Research Papers in Economics) website – a website with a similar role as SSRN but which specializes in papers that deal solely with economics. The first version of our paper realized a considerable number of downloads, more than 1,000 in February 2022. See https://logec.repec.org/scripts/paperstat.pf?h=repec:ris:jhisae:0200, https://econpapers.repec.org/paper/risjhisae/0200.htm (first version in February, 2022) and https://mpra.ub.uni-muenchen.de/113732/ (second version in June, 2022).

Allow us to conclude that we find the SSRN response of rejecting (read: censoring) our new, updated Johns Hopkins working paper objectionable. SSRN should serve the academic community – not censor academic work in health economics. Thus, we hope the rejection was simply a mistake that will be corrected.

We look forward to receipt of your swift response.

Yours sincerely,
Lars Jonung, Jonas Herby, and Steve H. Hanke

We never received any response from SSRN to the above letter in spite of several requests from us. The paper referred to in the letter above as posted in 2006 is Jonung and Röger (2006). This paper has been placed on SSRN's Top Ten download list for health economics (HEN) several times.

# Appendix IV: Evaluation of the Exclusion Criteria on synthetic control method and 'too few observations'

***Background***

In our protocol, Herby et al. (2021), we 'exclude synthetic control studies because of inherent empirical problems as discussed by Bjørnskov (2021b).' We also exclude studies with very few observations, e.g., Conyon et al. (2020), which 'exploit policy variation between Denmark and Norway on the one hand and Sweden on the other' and, thus, only have one observation in one group.

Our reasons to exclude these studies are twofold.

First, there are methodological problems related to the synthetic control method (SCM) in a COVID-19 setting. Abadie (2021) writes that 'the credibility of a synthetic control estimator depends in great part on its ability to steadily track the trajectory of the outcome variable for the affected unit before the intervention' and 'when designing a synthetic control study, it is of crucial importance to collect information on the affected unit and the donor pool for a large pre-intervention window.' As discussed by Bjørnskov (2021b), this lack of a large pre-intervention window is an inherent problem in the evaluation of the effect of lockdown on COVID-19 mortality.

Second, we worried that these studies could be biased because they would tend to focus on a few special places such as Italy and Sweden, which – possibly because they were hit early and were surprised by the pandemic – experienced very high death tolls.

Following the publication of our working paper, scholars such as Professor David Paton[119] have questioned this decision. One argument is that few observations is not a problem because the difference-in-difference method does not require a certain number of observations to be valid (if there are sufficient degrees of freedom). Hence, studies based on few jurisdictions can be assumed to produce useful knowledge regarding the effects of lockdown measures but are likely to have less precision than studies based on more cases. The key point is that ***if there is sufficient variation in the jurisdictions covered, the full set of estimates from studies with few observations are still unbiased***.

In general, it is best to avoid changing the research protocol after the results of the study have been obtained, as this can introduce bias and compromise the validity of the initial research plan. However, it is still valuable to assess whether our exclusion criteria are problematic in that they have excluded useful information or biased our results.

In this supplementary section, we explore whether:

1. the excluded studies are focusing solely on a few special places with many deaths during the first wave, such as Italy and Sweden, effectively preventing ‘sufficient variation’;

2. we find evidence that the SCM is as problematic in a COVID-19 setting as Bjørnskov (2021b) proposes.

### *The Synthetic Control Method (SCM) criteria*

We exclude the following ten SCM studies:

- Born et al. (2021)
- Cerqueti et al. (2021)
- Cho (2020)

119 See https://www.sciencemediacentre.org/expert-reaction-to-a-preprint-looking-at-the-impact-of-lockdowns-as-posted-on-the-john-hopkins-krieger-school-of-arts-and-sciences-website/

- Conyon and Thomsen (2021)
- Dave et al. (2020b)
- Friedson et al. (2021)
- Ghosh et al. (2020)
- Mader and Rüttenauer (2021)
- Neidhöfer and Neidhöfer (2020)
- Reinbold (2021)

***Geographical coverage of the ten studies***

Nine of the ten above-mentioned studies cover thirteen jurisdictions in total, whereas Mader and Rüttenauer (2021) use the Generalized Synthetic Control Method (GSCM) and cover 169 countries.

Seven of the thirteen jurisdictions examined in the nine studies are special areas with many COVID-19 deaths during the first wave, such as Sweden (three studies), Italy (three studies), and New York (one study). Two cover Delhi and South Korea and are not relevant given our protocol limiting our research to studies including ‘at least one EU-country, the United States or one US state or Latin America, and where at least one country/state is not an island’.[120] The last four jurisdictions covered by the above-mentioned studies are Illinois, California, Argentina, and Wisconsin.

It is clear that the variation in these studies is limited. Hence, the full set of estimates is possibly biased, if, for example, Sweden, Italy, and New York are special cases with unobserved confounders. And as we will discuss below, this is likely the case.

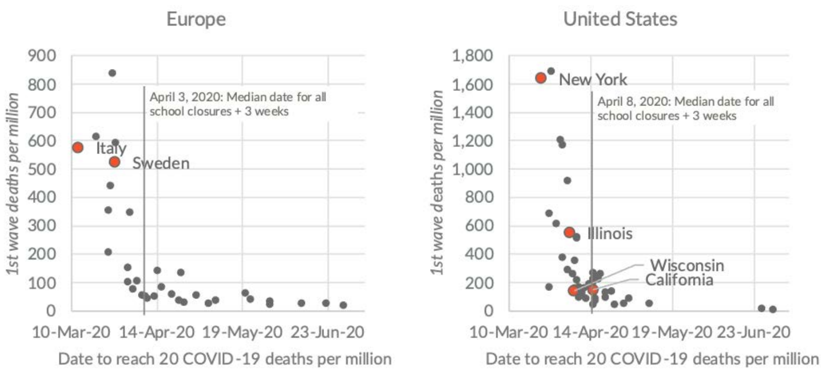
Figure 22 shows the total number of COVID-19 deaths per million during the first wave (Y-axis) and the date each jurisdiction reached 20 COVID-19 deaths per million. The vertical lines mark three weeks after school closures (‘all levels’, cf. Hale et al. 2020) which were typically during or under the first NPI implemented by governments. Since, on average, it takes

120 Ghosh et al. (2020) cover Delhi and Neidhöfer and Neidhöfer (2020) cover South Korea.

approximately three to four weeks from infection to death,[121] and since virtually all jurisdictions closed schools at the same time,[122] Figure 22 illustrates that some jurisdictions that were initially hit early and hard by the pandemic, encountered much higher death tolls than those locations in which populations had fair warning of an impending pandemic.

Since the counterfactuals in the SCM studies are typically estimated based on case numbers in a short period before lockdowns, there is a potential risk that the seven estimates based on Italy, Sweden, and New York do not illustrate the effect of lockdowns but simply the effect of being among the first jurisdictions to be hit by the pandemic.

**Figure 22: Many SCM studies covers European countries and U.S. states that were hit early and hard by the pandemic**




*Note: The figure is a replication of Figure 7. It shows the relationship between early pandemic strength and total first wave of COVID-19 mortality. On the X-axis is 'Days to reach 20 COVID-19 deaths per million (measured from 15 February 2020).' The Y-axis shows mortality (deaths per million) by 30 June 2020. Countries and states covered by the excluded SCM studies are marked with red.*

121 Leffler et al. (2020) write, 'On average, the time from infection with the coronavirus to onset of symptoms is 5.1 days, and the time from symptom onset to death is on average 17.8 days. Therefore, the time from infection to death is expected to be 23 days.' Meanwhile, Stokes et al. (2020) state that 'evidence suggests a mean lag between virus transmission and symptom onset of 6 days, and a further mean lag of 18 days between onset of symptoms and death.'

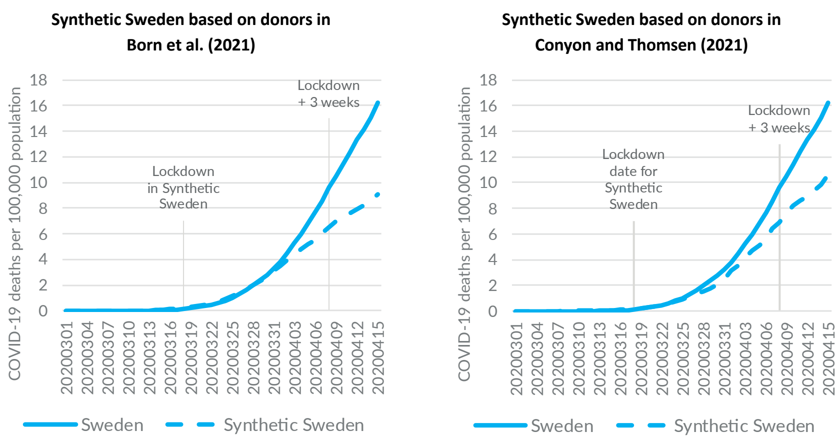
122 All 50 U.S. states closed schools between 13 March 2020 and 23 March 2020, and 44 states closed schools in just four school days (15 March 2020 (Sunday) to 19 March 2020 (Friday)), see Table 1 in Auger et al. (2020).

*Source: Reported COVID-19 deaths and OxCGRT stringency for European countries and U.S. states with more than one million citizens. Data from Our World in Data (2022).*

To reveal any potential bias, we have illustrated the development of COVID-19 deaths in Sweden and Synthetic Sweden for two studies in Figure 23 below.

Not only does Synthetic Sweden in Born et al. (2021) have 32 per cent fewer cumulative deaths on the date where the possible effect of the lockdown should be visible (29 per cent for Conyon and Thomsen 2021), the number of daily deaths is also 139 per cent (70 per cent) higher. This means that even if the reproduction rate, $R_t$, was exactly the same in Sweden and Synthetic Sweden after the lockdown date, the death toll in Sweden would have been substantially higher than in Synthetic Sweden at the end of the first wave.

**Figure 23: An illustration of cumulative COVID-19 deaths in Sweden and Synthetic Sweden**




Note: Lockdown date in Synthetic Sweden is 18 March 2020 in both figures based on the weighted average lockdown data from Born et al. (2021).

Based on the above, it should be clear that the seven studies do not reflect 'sufficient variation'. Rather, they are examinations of special cases in which the very short pre-intervention window is an important limitation of the SCM. Next, we examine the remaining four eligible studies and the GSCM-study by Mader and Rüttenauer (2021).

*The four SCM studies not examining jurisdictions that were initially hit early and hard*

In this section, we take a closer look at the four SCM studies that do not examine Sweden, Italy, and New York, which were surprised by the pandemic and therefore experienced many deaths during the first wave. The four studies are:

- Friedson et al. (2021) who examine a SIPO in California.
- Neidhöfer and Neidhöfer (2020) who examine school closures in Illinois.
- Reinbold (2021) who examines school closures and other pre-lockdown COVID-19 mitigation strategies in Argentina.
- Dave et al. (2020b) who exploit a natural experiment to examine the repeal of a SIPO in Wisconsin.

### *Friedson et al. (2021) – A SIPO in California*

Friedson et al. (2021) examine the effect of a SIPO in California. They find a large but insignificant[123] effect of California's SIPO on mortality, with 35 per cent to 56 per cent fewer COVID-19 deaths.[124]

Their reason for choosing California reveals a potential source of bias: 'With California being the first state in the nation to issue a statewide SIPO at a time when the COVID epidemic was still new, cases in the early periods, by definition, were low.'

They also write that 'matching on these relatively small values of pre-treatment COVID-19 cases may not fully leverage the construction of a valid counterfactual and end up minimizing meaningful differences prior

123 Friedson et al. (2021) write: 'While our estimated mortality decline is substantial in magnitude, permutation based p-values are insufficiently small to conclude definitively that there was a decline in COVID-19 deaths due to California's SIPO.'

124 Friedson et al. (2021) estimate that 'the adoption of a SIPO is associated with 636 to 1,556 fewer deaths across these specifications, with a median estimate of around 1,436 lives saved.' By the end of their study-period (20 April 2020) there were 1,201 COVID-19 deaths in California, so the effect of the SIPO is estimated at −35% to −56%, with a median estimate of −54%.

to policy adoption relative to post-treatment differences.' Indeed, their matching is based on a total of just 793 COVID-19 cases in California prior to the SIPO, corresponding to 20 cases per million.

While this does not in itself reveal any biases of concern, Friedson et al. (2021) is a perfect case to illustrate the inherent problems related to using SCM to evaluate the effect of lockdowns on COVID-19.

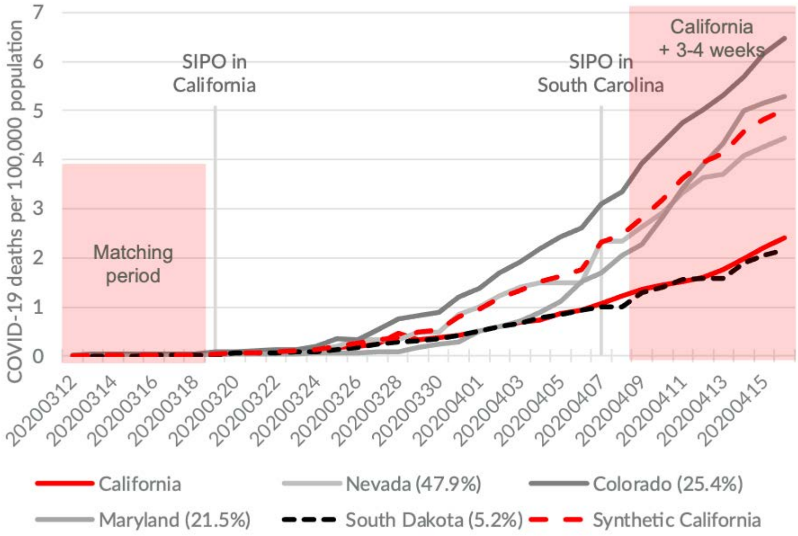
Figure 24 below illustrates basic information about Friedson et al. (2021). First, it illustrates that the Synthetic is based on a very short pre-intervention window, 11 March to 18 March 2020, before California imposed a SIPO.

Second, it illustrates that the short pre-intervention window may affect the results. After ten days, Synthetic California (Figure 24) has 54 per cent fewer deaths compared to California. After three weeks – at the time when the effect of the SIPO starts to be visible because of the three- to four-week lag between infection and death – Synthetic California has 51 per cent fewer deaths than California. This almost corresponds to the estimated effect of a SIPO. Hence, there is a high risk that a flaw in matching the synthetic control – caused by a very short pre-intervention window – drives the results, not the SIPO.

Interestingly, South Carolina (shown with a dashed line in Figure 24), which matches California almost 1:1 during the first three weeks after California's SIPO, and did not impose a SIPO until 7 April 2020,[125] has almost the exact same cumulative COVID-19 mortality rate by 30 June 2020.

125 See https://governor.sc.gov/news/2020-04/governor-mcmaster-issues-home-or-work-order

**Figure 24: COVID-19 mortality in California, Synthetic California, and donor states**




Source: Own calculations based on data from Folkhälsomyndigheten (2022), Our World in Data (2022) and Friedson et al. (2021).

Note: The figure is based on the weights from model 1 (see Table A1, Panel II(1)) in Friedson et al. (2021).

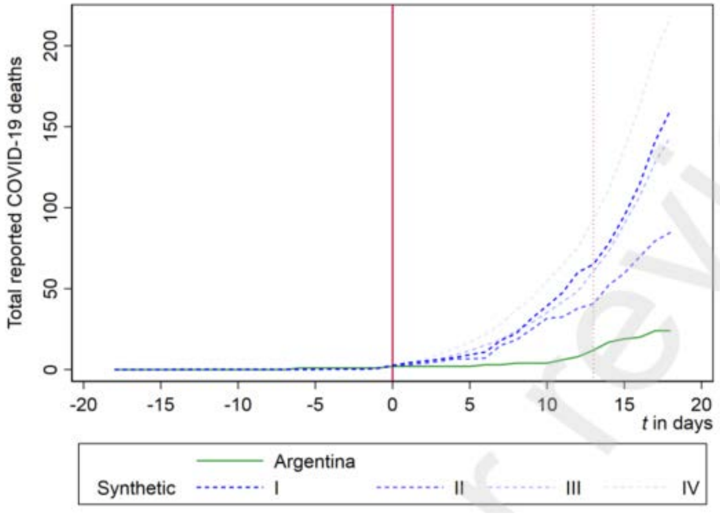
### *Neidhöfer and Neidhöfer (2020) – School closures in Argentina*

Neidhöfer and Neidhöfer (2020) examine the effectiveness of proactive school closures, and other early social distancing interventions, in three countries that reacted relatively early during the pandemic. In the following, we focus on Argentina, as Italy is a special case (see above) and South Korea is excluded given the criteria in our protocol.

The authors create synthetic controls based on COVID-19 cases and deaths in the 14-day period before school closures. They estimate that the ‘effect of the interventions ranges from a 63 per cent to a 90 per cent reduction in daily average deaths in Argentina’.

The very short pre-intervention window mirrors the same problems as do Friedson et al. (2021) (illustrated in Figure 23). Indeed, they find an enormous *and immediate* effect of nationwide school closures, which is incompatible with the fact that there is a long lag between infection and death (cf. Figure 25 below, which is a replica of the authors' Figure 1).

**Figure 25: COVID-19 mortality in Argentina and Synthetic Argentina**




Source: Figure 1 in Neidhöfer and Neidhöfer (2020)

### *Reinbold (2021) – School closures in Illinois*

Reinbold (2021) examines the effect of school closures in Illinois in August 2020 and thus does not suffer from the same 'surprise' problems as Friedson et al. (2021) and Neidhöfer and Neidhöfer (2020).

Reinbold (2021) does not explain the reason for using data from Illinois. In the period examined (24 August 2020 to 13 September 2020), the number of COVID-19 deaths was relatively stable in Illinois and the U.S. as a whole and revealed no potential source for bias regarding the geographical coverage.

Reinbold (2021) finds 'no significant differences in […] deaths between any of the three county groups' (majority hybrid, majority online-only and majority in-person counties).

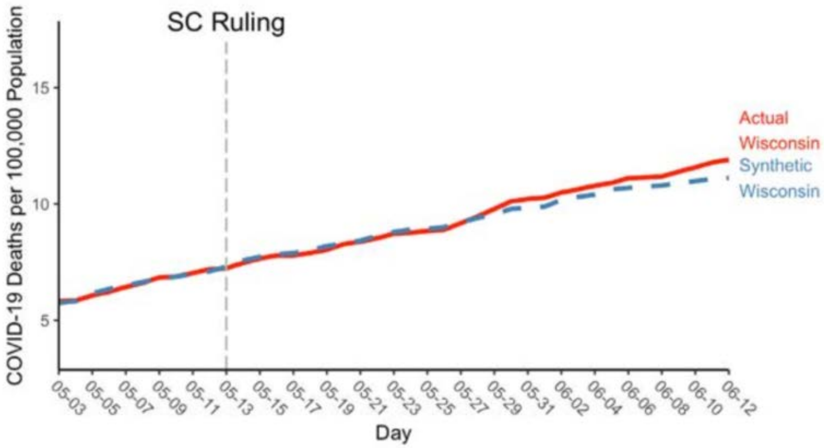
A major concern is that the period studied is only three weeks, so any effect on deaths from the school closures is likely to be omitted in the analysis because of the three- to four-week lag between infection and death. This may explain why Reinbold (2021) finds no significant effect on mortality.

***Dave et al. (2020b)*** *– Wisconsin*

Dave et al. (2020b) examine the effect of a SIPO, exploiting a natural experiment arising when the Wisconsin Supreme Court abolished the state's 'Safer at Home' order [SIPO] on 13 May 2020 and thus do not suffer from the same 'surprise' problems as Friedson et al. (2021) and Neidhöfer and Neidhöfer (2020). Since Dave et al. (2020b) rely on a natural experiment, there is no reason to believe that the selection of Wisconsin as a case was biased.

In their first version, Dave et al. (2020a), the authors only examine a two-week period following the Supreme Court decision, but in the authors' updated version, Dave et al. (2020b), they examine one full month. They conclude that 'we find no evidence that the Wisconsin Supreme Court decision impacted COVID-19 growth up to a month following the repeal.' Although insignificant, their Figure 7(a) shows that the total effect of a SIPO on COVID-19 mortality after one month is approximately 8 per cent fewer deaths, cf. Figure 26 below.

**Figure 26: Replication of Figure 7(a) in Dave et al. (2020b)**




Source: Figure 7(a) in Dave et al. (2020b).

Note: Synthetic WI is comprised of ME (.539), HI (.209) CA (.08) & PA (.048).

***The Generalized Synthetic Control Method (GSCM)***

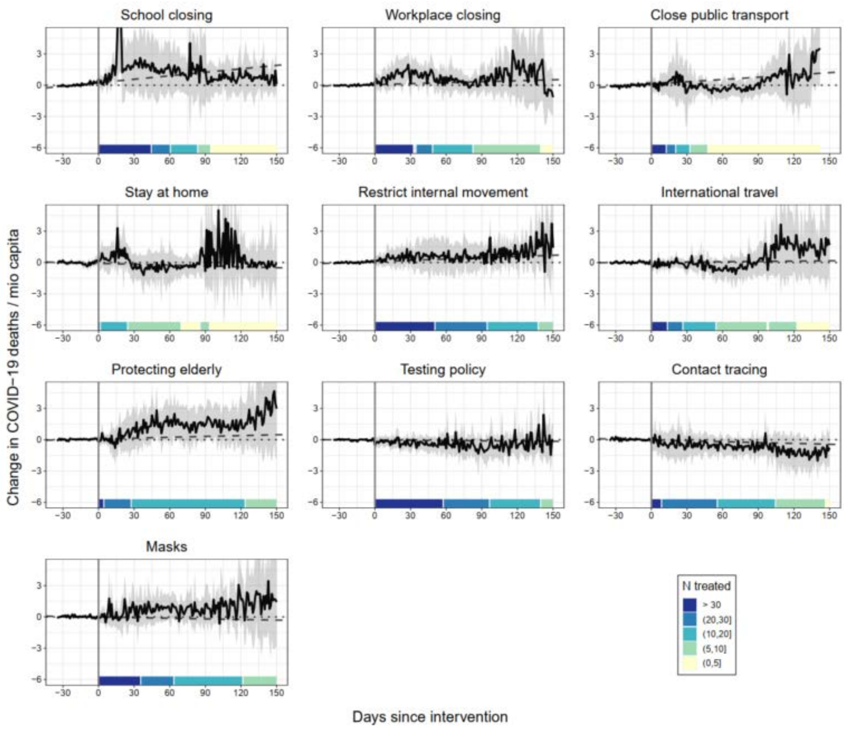
## *Mader and Rüttenauer (2021)*

Mader and Rüttenauer (2021) analyse data on daily confirmed COVID-19-related deaths per capita from Our World in Data, and on ten different NPIs from the Oxford COVID-19 Government Response Tracker for 169 countries from 1 July 2020 to 31 May 2021.

They use GSCM, thereby effectively avoiding any potential selection bias.

Mader and Rüttenauer (2021) 'do not find substantial and consistent mitigating effects of any NPI under investigation on COVID-19-related deaths per capita. We see a tentative change in the trend of COVID-19-related deaths around 30 days after workplace closing, public transport closing, and stay-at-home rules have been implemented, but none of them exerts a statistically significant effect.' Their results are summarised in Figure 27 below.

**Figure 27: Effect on COVID-19 deaths of various NPIs in Mader and Rüttenauer (2021)**




Source: Figure 1 in Mader and Rüttenauer (2021)

***Conclusion on the SCM criteria***

Born et al. (2021), Cerqueti et al. (2021), Cho (2020), Conyon and Thomsen (2021), and Ghosh et al. (2020) study special cases, i.e., jurisdictions that were hit early and surprised by the pandemic and would have experienced very high COVID-19 mortality even if they managed to reduce the reproduction number, $R_t$, to the same level at the same time as other places.

Friedson et al. (2021) and Neidhöfer and Neidhöfer (2020) find very large and very early effects which indicate that their synthetic control is flawed. The explanation may lie in the very short data period available for estimating the synthetic control, which is exactly the problem discussed by Bjørnskov (2021b) and initially pointed out by Abadie (2021).

Reinbold (2021) does not find any effect on mortality but only looks at a three-week period after treatment, hiding any potential effect of school closures on mortality due to the delay between infection and death.

Dave et al. (2020b) exploit a natural experiment to examine the repeal of a SIPO in Wisconsin and find a small (approximately −8 per cent) and insignificant effect of (repealing) SIPOs.

We conclude that the exclusion criteria in our protocol were well founded. The SCM studies in general do not show ‘sufficient variation’ but rather examine special cases where the very short pre-intervention window is an important limitation of the SCM. Only two studies, Mader and Rüttenauer (2021) and to some degree Dave et al. (2020b) do not suffer from the biases we expected when preparing our protocol. But, if these two studies had been included in our meta-study, our meta-results would not have been significantly altered.

Since it is prudent not to deviate from one’s research protocol *ex post facto*, we have adhered to our protocol with regard to the SCM studies. In the next section, we look at the studies excluded based on our ‘too few observations’ criteria.

### *The ‘too few observations’ criteria*

We exclude the following ten studies based on the ‘too few observations’ criteria:

- Bongaerts et al. (2021)
- Gordon et al. (2020)
- Berardi et al. (2020)
- Alemán et al. (2020)
- Conyon et al. (2020)
- Juranek and Zoutman (2021)
- Kapoor and Ravi (2020)
- Ciminelli and Garcia-Mandicó (2021)
- Porto et al. (2022)
- Borri et al. (2020)

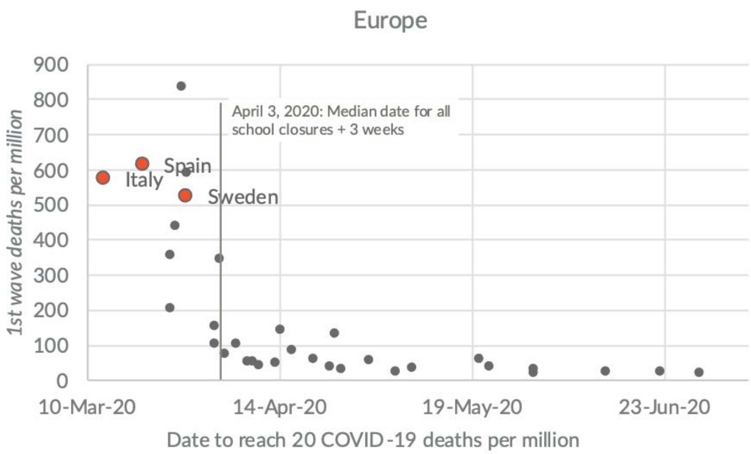
### *Geographical coverage of the ten studies*

Six of the ten studies examine variation within one country (five – Bongaerts et al. (2021), Berardi et al. (2020), Ciminelli and Garcia-Mandicó (2021), Porto et al. (2022), and Borri et al. (2020) – examine Italy, and one – Alemán et al. (2020) – examines Spain).

The remaining four studies – Gordon et al. (2020), Conyon et al. (2020), Juranek and Zoutman (2021), and Kapoor and Ravi (2020) – compare Sweden (control group) to other countries (primarily Scandinavian countries).

Given the data in Figure 28, these selections do not seem to be random. Indeed, the studies seem to focus on jurisdictions that were surprised by the pandemic and would have experienced very high COVID-19 mortality even if they managed to reduce the reproduction number, $R_t$, to the same level at the same time as other places.

**Figure 28: All 'too few observations' studies cover European countries that were hit early and hard by the pandemic**




*Note: The figure shows the relationship between early pandemic strength and total first wave of COVID-19 mortality. On the X-axis is 'Days to reach 20 COVID-19 deaths per million (measured from 15 February 2020)'. The Y-axis shows mortality (deaths per million) by 30 June 2020.*

*Source: Reported COVID-19 deaths and OxCGRT stringency for European countries and U.S. states with more than one million citizens. Data from Our World in Data (2022).*

### *Conclusion on the ‘too few observations’ criteria*

We conclude that the exclusion criteria in our protocol were in most cases well founded. The ‘too few observations’ studies in general do not show ‘sufficient variation’ but rather focus on special cases.

Since it is prudent not to deviate from one’s research protocol *ex post facto*, we have adhered to our protocol with regard to the ‘too few observations’ studies.

### *Conclusion*

In general, it is best to avoid changing the research protocol after the results of the study have been obtained, as this can introduce bias and compromise the validity and credibility of the research reported on the basis of the protocol.

The above evaluation of the exclusion criteria in our protocol shows that:

1. The excluded studies tend to focus on a few special cases, such as Italy and Sweden, which were hit early and surprised by the pandemic. As a result, they would have experienced very high COVID-19 mortality even if they had managed to reduce the reproduction number, $R_t$, to the same level at the same time as other places. These studies examine special cases and do not – even when combined – reflect ‘sufficient variation’ in the data.

2. Many SCM studies suffer from a very short pre-intervention window, which tends to render their synthetic control approach ineffective.

There are only two excluded studies where conclusion 1 and/or 2 do not apply (Mader and Rüttenauer 2021 and to some degree Dave et al. 2020a). These studies yield results similar to our meta-results and, thus, would not have significantly altered our meta-results if included.

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