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A Reply to “Comment on ‘A Literature Meta-Analysis of The Effects of Lockdowns on Covid-19 Mortality’”

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**A REPLY TO “COMMENT ON ‘A LITERATURE  
REVIEW AND META-ANALYSIS OF THE  
EFFECTS OF LOCKDOWNS ON COVID-19  
MORTALITY””**

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# **A reply to “Comment on ‘A Literature Review and Meta-Analysis of the Effects of Lockdowns on COVID-19 Mortality’”**

By Jonas Herby, Lars Jonung, and Steve H. Hanke

*August 7, 2023*

## **About the Series**

The Studies in Applied Economics series is under the general direction of Prof. Steve H. Hanke, Founder and Co-Director of The Johns Hopkins Institute for Applied Economics, Global Health, and the Study of Business Enterprise (hanke@jhu.edu). The views expressed in each working paper are those of the authors and not necessarily those of the institutions that the authors are affiliated with.

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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. He is a Senior Fellow at the Independent Institute, a contributor at National Review, 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 held a cabinet-level rank in both Lithuania and Montenegro. He has been awarded seven honorary doctorate degrees and is an Honorary 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

In our meta-study from 2022, "[\*A Literature Review and Meta-Analysis of the Effects of Lockdowns on COVID-19 Mortality – II\*](#)," we find that the effect of lockdowns—also referred to as 'Covid restrictions,' 'social distancing measures,' etc.—on mortality was negligible. The first version of our meta-study received criticism from Banholzer, et al. (2022). The authors raised "concerns regarding the subject and conduct" of our meta-analysis.

In this reply we respond to these objections. We demonstrate that the criticism brought forward by Banholzer et al. (2022) lacks a solid foundation, focuses at best on ancillary issues, and generally misses the entire point of our research. In short, Banholzer, et al.'s comments are immaterial and irrelevant.

# Introduction

In a meta-study, "A Literature Review and Meta-Analysis of the Effects of Lockdowns on COVID-19 Mortality – II," published in the Studies in Applied Economics working paper series at the Johns Hopkins Institute for Applied Economics, Global Health and the Study of Business Enterprise, Herby et al. (2022b), we find that the effect on mortality of lockdowns, also referred to as 'Covid restrictions,' 'social distancing measures,' etc. has been negligible. 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 percentage translates into approximately 6,000 avoided deaths in Europe and 4,000 in the United States. Additionally, based on specific non-pharmaceutical interventions (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, translating into approximately 23,000 avoided deaths in Europe and 16,000 in the United States. For comparison, each year approximately 72,000 in Europe and 38,000 in the United States die from the flu.<sup>1</sup>

Herby et al. (2022b) is an updated version of Herby et al. (2022a). It was recently published in an elaborated and extended version as a book entitled "Did Lockdowns Work? The verdict on Covid Restrictions" published by the Institute of Economic Affairs.<sup>2</sup>

The first version of our study received criticism from Banholzer et al. (2022) who raised "concerns regarding the subject and conduct of their meta-analysis." With this note, we would like to respond to these concerns.

Let us start by pointing out a deep problem that illustrates an important issue with many COVID-19 policies. Banholzer et al. (2022) believe that our strong findings "should be backed up by equally strong evidence." We believe the evidence *is* strong, but – more importantly – Banholzer et al. (2022) ought to view the imposition of lockdowns with the same mantra. Lockdowns are an extreme policy measure which, in liberal democracies, should be supported by equally strong evidence. But as Table 1 below illustrates, this evidence was and is still not available. The introduction of lockdowns was a major policy step into the unknown as there was no evidence to support it. The reason is simple. Lockdowns on the scale applied during the COVID-19 pandemic have not been tried before.

**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 of external travelers	Very low
Internal travel restrictions	Very low
Border closure	Very low

Source: WHO (2019)

Before we proceed, allow us to express our astonishment that the Social Science Research Network (SSRN) publishes both our study protocol, Herby et al. (2021), and Banholzer's critique of our meta-study, but has refused to publish our working paper, Herby et al. (2022b). In our view, this is unprecedented. For the correspondence of that sorry episode, see the appendix.

<sup>1</sup> 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.

<sup>2</sup> Herby et al. (2023) is available for download at [https://iea.org.uk/wp-content/uploads/2023/06/Perspectives-1\\_Did-lockdowns-work\\_June\\_web-1.pdf](https://iea.org.uk/wp-content/uploads/2023/06/Perspectives-1_Did-lockdowns-work_June_web-1.pdf).

## A response to Banholzer et al. (2022)'s critique

Banholzer et al. (2022) make four overall claims:

- A. "Lockdown is an unspecific, ill-defined term and thus an inappropriate starting point for meta-analyses".
- B. "If anything, the lockdown effect is not the effect of single NPIs but the combined effect of multiple NPIs".
- C. "Mortality is not the only relevant and not a conclusive measure of NPI effectiveness".
- D. "Highly restrictive eligibility criteria are no replacement for rigorous quality assessment".

In the following sections we will respond to each of these claims.

### A. On the term "lockdown"

However we choose to define the average level of restrictions in Europe and the U.S. during the first wave of the COVID-19 pandemic does not alter our results. Therefore, we wonder why Banholzer et al. (2022) even spent time on this matter when our results obviously raise more important questions.

However, we think Banholzer et al. (2022) completely misunderstand our definition. The responsibility for this misunderstanding falls back on us for failing to provide a thorough explanation.

**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 does include mandated interventions such as closing schools or businesses and face masks requirements. **We define a lockdown as "any policy consisting of at least one NPI."**

During the COVID-19 pandemic, lockdowns were mainly defined in two different manners. 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 government responses to the pandemic in terms of relatively less or more strict interventions.<sup>3</sup> Our definition follows the latter.<sup>4</sup>

For an example of our terminology, we will say that the government of Country A introduced the interventions of school closures and shelter-in-place orders as part of the country's lockdown. And we will say that Country A's lockdown was stricter than Country B's lockdown because Country B closed schools but did not issue shelter-in-place orders.

We agree with Banholzer et al. (2022) that our definition becomes less meaningful if a country only imposes very limited restrictions. With our definition, if Country C only cancelled public events, the requirements of a lockdown would be met. Country C's case is probably not what most people think of when they hear the word "lockdown." However, we are studying the effect of the average lockdown in Europe and the U.S. during the first wave, and the average lockdown is far from Country C.

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<sup>3</sup> 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.

<sup>4</sup> We use the term "shelter-in-place orders (SIPOs)" to describe the former use of the term 'lockdown.'

## B. On the “combined effect of multiple NPIs”

Banholzer et al. (2022) are stating the obvious when saying that the effect of lockdowns is the combined effect of the individual measures.

In the first version of our working paper (Herby et al. 2022a), we investigated these combined and individual effects. Our Table 3 shows the combined effect of NPIs as estimated in the studies based on the OxCGRT stringency index – the standard index used by researchers. In Table 7 we then present estimates from the studies examining the effect of individual NPI’s. Each effect estimate is relatively small. And even if all the effects of individual NPI’s are taken together, the combined effect is so small that it does not alter our conclusion.

In the updated version of our working paper, Herby et al. (2022b), this analysis was improved. We estimate that the average lockdown in Europe and the U.S. only reduced mortality by 10.7% when we use combined effect estimates of each individual measure.

Banholzer et al. (2022) argue that model-based studies such as Flaxman et al. (2020) – who claimed that lockdowns saved three million lives in Europe – are superior in estimating the effect of lockdowns. However, Flaxman et al. (2020) assume that the pandemic would have followed an epidemiological curve had countries not locked down. This crucial assumption means that the only interpretation possible for the empirical results is that lockdowns are the only factor that could have caused the observed change in the reproduction rate,  $R_t$ ,<sup>5</sup> ignoring the potential effects of other factors such as changes in voluntary behavior and seasonality. In fact, Flaxman implicitly assumes that nothing people did, which was not mandated by governments, had any effect. No effect from cancelling personal gatherings, keeping physical distance, washing hands, working from home, etc. This is clearly a misleading assumption given our knowledge of how people adjust in a dangerous situation.

Figure 1 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,  $R_t$ , in Denmark and Sweden. According to their results, banning public events had a close-to-zero effect in Denmark (Panel A) but huge effects in Sweden (Panel B).

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<sup>5</sup> The effective reproductive number, denoted  $R_t$ , is the expected number of new infections caused by an infectious individual.

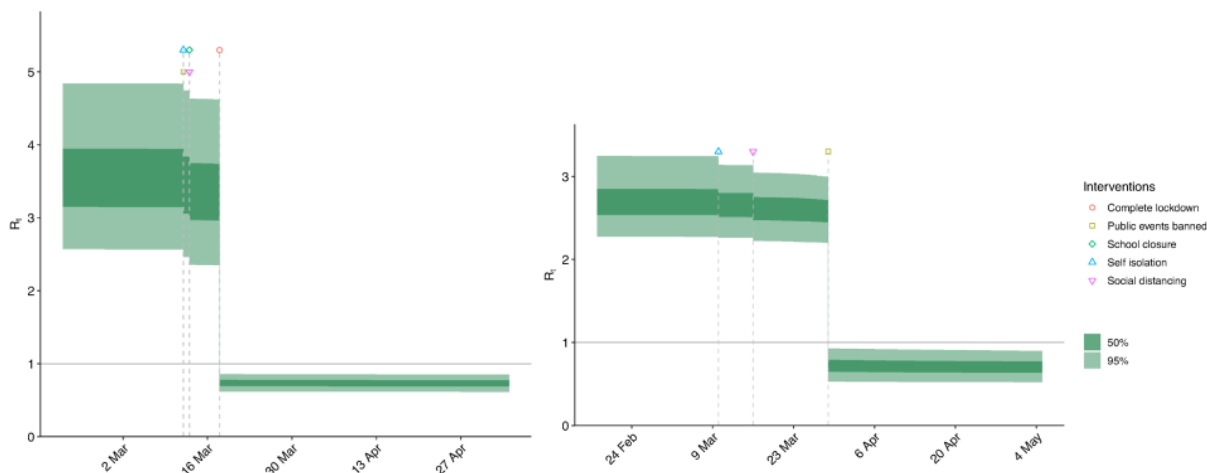
**Figure 1: The assumptions used by Flaxman et al. (2020) lead to two contradictory conclusions: That banning public events had no effect in Denmark but were extremely effective in Sweden in March 2020**

**Panel A:  $R_t$  in Denmark**

$R_t$  in Denmark is assumed to drop after the implementation of 'Complete lockdown' (i.e., caused by the closure of businesses).

**Panel B:  $R_t$  in Sweden**

$R_t$  in Sweden is assumed to drop after 'Public events banned'. An intervention that had close-to-zero effect in Denmark.



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<sup>6</sup> – reported these findings as ‘proof’ that lockdowns had saved millions of lives.<sup>7</sup>

Similar interpretation problems are also found in Brauner et al. (2021)<sup>8</sup> and Banholzer et al. (2021)<sup>9</sup>, which Banholzer et al. (2022) also promote. While these studies may tell us something about the total effect (that is the sum of mandated behavior changes and voluntary behavior changes) of societies’ response to the pandemic, they cannot tell us anything about solely the effectiveness of lockdowns. However, the studies included in our meta-analysis can give us information on this issue.

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.<sup>10</sup> Including the estimates from these studies in our meta-analysis and

<sup>6</sup> For example, Dr. Seth Flaxman stated that ‘lockdowns averted millions of deaths’, see e.g. <https://www.bbc.com/news/health-52968523>, and Samir Bhatt claimed that ‘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>.

<sup>7</sup> 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/>.

<sup>8</sup> Brauner et al. (2021) stated 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.” 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.”

<sup>9</sup> Banholzer et al. (2021) state that “any approach of explaining changes in the observed number of cases solely by specific NPIs makes the implicit assumption that these changes were not the result of some other factors. For instance, it is possible that additional measures or an increasing general awareness encouraged social distancing and hence lead to less infections.”

<sup>10</sup> Several scholars have criticised Flaxman et al. (2020), e.g., Homburg and Kuhbandner (2020), Lewis (2020), and Lemoine (2020).



interpreting them as the effect of lockdowns would without a doubt greatly overstate the effectiveness of lockdowns.

### **C. On “mortality as a conclusive measure”**

Banholzer et al. (2022) argue 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,” etc.

While we agree on this, unfortunately limited evidence exists that interventions actually reduced the number of infections. Our results indicate that, as Allen (2021) puts it, “the ineffectiveness [of lockdowns] stemmed from individual changes in behavior: either non-compliance or behavior that mimicked 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, hospitalizations, or other measures.

While we regard analysis based on cases as problematic because of large data problems,<sup>11</sup> one could argue that including studies examining the effect of lockdowns on hospitalization could improve the quality of our review and meta-analysis because it would allow us to include more studies. However, using the same search strings, but replacing ‘deaths,’ ‘death,’ and/or ‘mortality’ with ‘hospitalization,’ ‘intensive care,’ and/or ‘ICU,’ indicates that including hospitalizations would only yield another 1-2 eligible studies.<sup>12</sup>

Although including studies examining the effect of lockdowns on hospitalization would potentially strengthen our results, 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 hospitalizations down. But one of the arguments for protecting the healthcare sector was that if hospitalizations 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.<sup>13</sup> Given this relationship between hospitalizations and deaths, we should see the effect of hospitalizations in our analysis of mortality.

We welcome future research which can give us a valid estimate of the effect of lockdowns on cases and hospitalizations.

### **D. On “Highly restrictive eligibility criteria”**

First, we do not find our eligibility criteria “highly restrictive.” As mentioned, we exclude studies like Flaxman et al. (2020), Brauner et al. (2021), and Banholzer et al. (2021) because they cannot tell us

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<sup>11</sup> 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).

<sup>12</sup> Scopus returns 947 hits on mortality, etc. between 1 January 2020 and 30 June 2021. Searching for hospitalization, etc. yields another 35 hits (3.7 per cent more studies).

<sup>13</sup> 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.”

anything about the effectiveness of lockdowns. We also exclude studies based on very few observations, and as we show in our book, “Did Lockdowns Work? The verdict on Covid Restrictions”, Herby et al. (2023) Appendix IV, this criteria was well founded as these studies examined very special cases.

Second, Banholzer et al. (2022) criticize our “quality dimensions” because they do not “assess the concrete methods and models used in the study.” We regret that we used the term “quality dimension” – in the updated version of our working paper, we use “bias dimension.”

We apply the bias dimensions because 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 – as Banholzer et al. (2022) state – “there is currently no consensus on best practices or an established scientific framework in evaluating the effectiveness of NPIs.” 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.’ We did not find any important biases.

## Conclusions

In sum, we are of the opinion that the criticism brought forward by Banholzer et al. (2022) was ill founded and only to a very limited extend relevant for the conclusions in our study.

By focusing on vague arguments like our definition of lockdowns and misleading statements like “highly restrictive eligibility criteria,” Banholzer et al. (2022) missed the major question: if the studies that have the ability to handle the effect of voluntary behavior changes – unlike Flaxman et al. (2020), Brauner et al. (2021), and Banholzer et al. (2021) – find negligible effects of lockdowns, what does that tell us about how we should handle future pandemics? Are there ways we can give people better resources to protect themselves when lockdowns cannot?

In addition, we would like to stress that in any serious assessment of the use of lockdowns, the enormous economic, social, and political costs must be included.<sup>14</sup> With these in mind, the case for lockdowns becomes still weaker. They appear to be a major policy mistake. This issue is not explicitly dealt with by Banholzer, et al. (2022) but must be brought into the picture when we evaluate the use of lockdowns during the COVID-19 pandemic.

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<sup>14</sup> See the conclusions in Herby et al. (2023).

## **Appendix: 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”<sup>15</sup> – 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 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.

### **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

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<sup>15</sup> From SSRN’s website. See <https://www.ssrn.com/index.cfm/en/>

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](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](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](https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3804077) and [https://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=3665588](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](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://mpa.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.

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