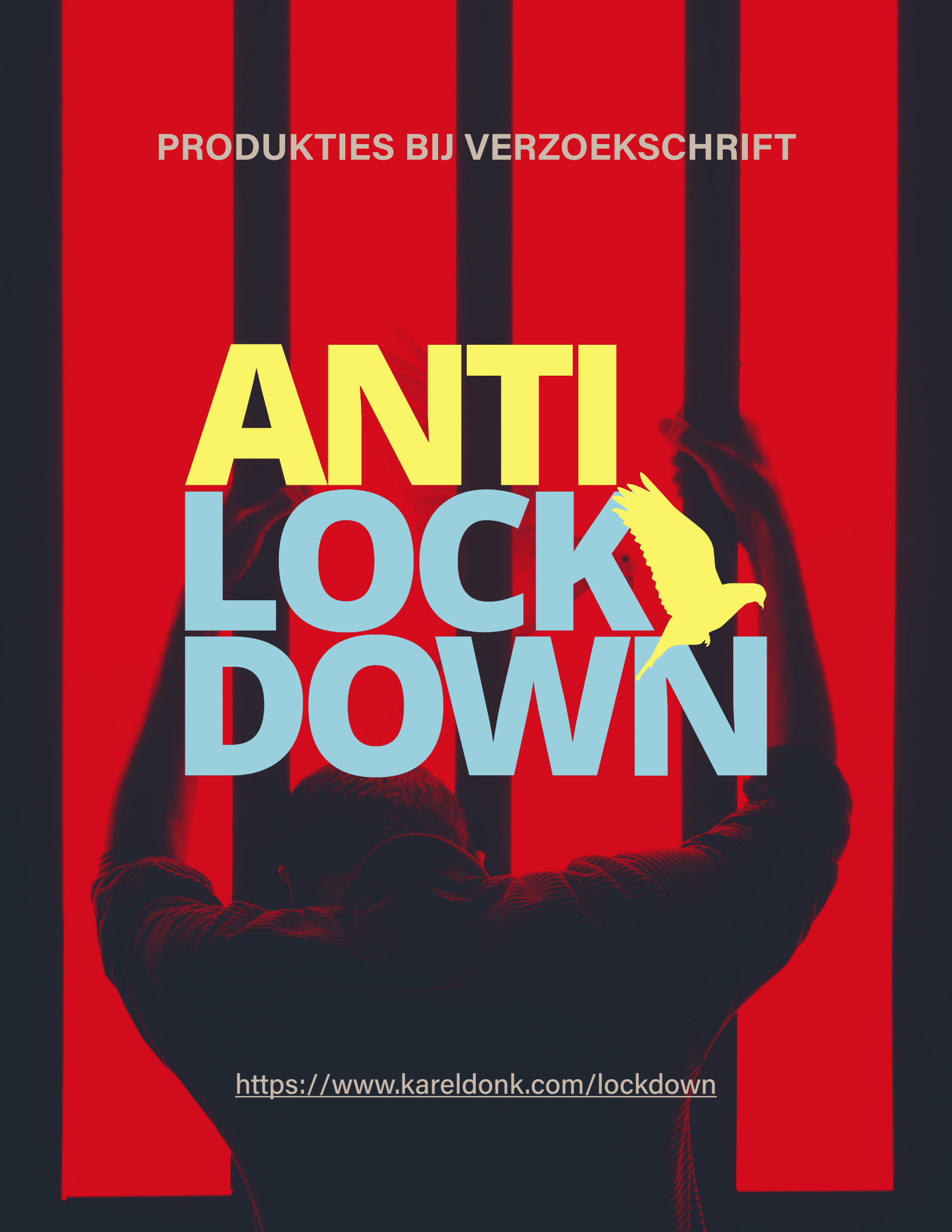
PRODUKTIES BIJ VERZOEKSCHRIFT

ANTI LOCKDOWN



https://www.kareldonk.com/lockdown

PRODUKTIE 1

Weekend lockdown, malls/kledingzaken/detailhandel/markten dicht

Maatregelen behoorlijk aangescherpt

De komende zaterdag en zondag is er volledige lockdown. Dit geldt ook voor zaterdag 29 en zondag 30 mei. Het uitgaansverbod voor de overige dagen is gesteld op 18.00 uur en duurt tot de volgende ochtend 5.00 uur. Alle overheidskantoren zijn vanaf nu gesloten. Slechts essentiële overheidsdiensten worden met minimale bezetting opengehouden. Vanaf vrijdag tot 3 juni zijn Malls, kledingzaken en overige detailhandelszaken, gesloten. Alle markten zijn eveneens gesloten. Scholen blijven dicht. Vluchten zullen slechts de volgende doelen hebben: Cargo, essentieel of repatriatie. Supermarkten, bakkerijen, apotheken, drogisterijen, poliklinieken, slagerijen zijn open met naleving protocollen.

De aankondiging van minister Amar Ramadhin van Volksgezondheid, via de Communicatiedienst Suriname:
Landgenoten,
De huidige maatregelen blijven van kracht tot donderdag 20 mei. Van vrijdag 21 mei 2021 tot donderdag 03 juni 2021 gelden de volgende maatregelen:
De algemene maatregelen blijven van kracht;
1. Het dragen van mond en neus bedekking buiten het eigen huis is verplicht, met name bij het betreden van ruimten. Kinderen beneden 12 jaar zijn niet verplicht een mondkap te dragen.

2. Neem de 1,5 meter fysieke afstand, de zogeheten Covid-19-afstand altijd in acht.
3.Was regelmatig uw handen met zeep en water of gebruik een hand sanitizer.

Het wordt zeer dringend geadviseerd om thuis te blijven en slechts voor noodzakelijke werkzaamheden, medische noodgevallen of het doen van noodzakelijke boodschappen uit huis te gaan.

Maatregel 1

Het uitgaansverbod is van vrijdag 21 mei 2021 tot donderdag 03 juni 2021 ingesteld van;

● Vrijdag 21 mei van 18:00 in de avond tot maandag 24 mei 05:00 in de ochtend. (Zaterdag 22 mei en zondag 23 mei is de zogenaamde full lockdown van kracht)

● Maandag 24 mei tot donderdag 27 mei dagelijks van 18:00 in de avond tot 05:00 in de ochtend.

● Vrijdag 28 mei van 18:00 in de avond tot maandag 31 mei 05:00 in de ochtend. (Zaterdag 29 mei en zondag 30 mei is de zogenaamde full lockdown van kracht)

● Maandag 31 mei tot donderdag 03 juni dagelijks van 18:00 in de avond tot 05:00 in de ochtend.

Deze en navolgende regels zijn onderhevig aan aanpassingen aan de hand van de informatie op dat moment!

Buiten de tijden van het uitgaansverbod gelden de volgende maatregelen;

Maatregel 2

Geen samenscholing van groepen groter dan vijf (5) personen op openbare plekken en in openbare ruimten. Dit geldt niet voor werk en sectoren of activiteiten waarvoor er een protocol is. Het is verboden feesten, met inbegrip van huisfeesten, te houden. Geen toestemming en dispensatie wordt verleend hiervoor. Deze zijn potentiële mass spreading events. Illegale feesten zullen gestopt worden. Iedereen zal volgens de Covid-19 WET beboet worden.

Maatregel 3

Bijeenkomsten voor rouwzittingen, uitvaarten en religieuze bijeenkomsten zijn uitsluitend toegestaan indien en voor zover niet meer dan tien (10) personen tegelijkertijd ter plaatse aanwezig zijn (indien de ruimte de anderhalve meter afstand toelaat). Dit op de meest veilige wijze en met inachtneming van de Covid-19 protocollen. Uitvaartbedrijven zijn verplicht om alle uitvaarten aan te melden bij het Covid-19 clusterteam.

Gebedshuizen zijn open en op geestelijke leiders en besturen, berust de verantwoordelijkheid om nauwlettend toe te zien op naleving van de protocollen en het aantal toegestane personen niet te overtreffen.

Maatregel 4

Alle groeps- en contactsporten zijn verboden. Individuele sportactiviteiten in de buitenlucht zijn toegestaan zolang er geen contact is met derden.

Maatregel 5

Personenvervoer door middel van het openbaar transport over de weg of over het water is uitsluitend toegestaan met inachtneming van het protocol openbaar vervoer.

Maatregel 6

In deze periode gelden voor publiekelijk toegankelijke commerciële lokaliteiten of ruimten, het volgende:
De navolgende sectoren zijn gesloten voor het publiek

a. Nachtclubs, dancings, discotheken en dergelijke;
b. Bordelen;
c. Bars;
d. Indoor dining
e. Outdoor dining (Terrassen)
f. Casino's;
g. Gok- en vermakelijkheidsgelegenheden;
h. Kansspelkantoren;
i. Sportscholen, yoga- en dansscholen, aerobics, zumba, sport- en fitnesscentra of –gelegenheden;
j. Kappers, kapsalons, schoonheidssalons en barbershops.
k. Malls, kledingzaken en overige detailhandelszaken.

De navolgende sectoren zijn open voor het publiek onder strikte naleving van de protocollen

a. Restaurants en andere commerciële gelegenheden voor het bereiden van eten.

● Afhaal; Dagelijks is afhalen mogelijk tot 17:00 uur

● Bezorging; Dagelijks is bezorging mogelijk tot 23:00 uur (Dus ook op zaterdag en zondag)

b. Supermarkten, bakkerijen, apotheken, drogisterijen, poliklinieken, slagerijen.

c. Recreatieoorden; Voor deze oorden geldt dat ze slechts mensen behorende tot hetzelfde gezin, individuen of groepen van maximaal 5 personen mogen toelaten. Deze groepen van 5 mogen niet in contact zijn met elkaar. Oorden, die lodges verhuren, kunnen dat doen, aan personen behorende tot een gezin. Ook hier geldt dat verschillende gezinnen niet bij elkaar mogen komen en dat ze strikte afstand moeten houden. De algemene Covid-19 maatregelen moeten op de oorden te allen tijde in acht worden genomen.

Maatregel 7

Repatrianten en essentieel personenverkeer is toegestaan om het land binnen te komen, met inachtneming van protocollen, die speciaal hiervoor zijn ontwikkeld. Ten aanzien van de overige maatregelen met betrekking tot het luchtruim zal in de komende dagen een volledige uitwerking plaatsvinden, na overleg met de luchtvaartsector. Vast staat wel dat de overheid alles in het werk zal stellen om de import van het Covid-19 virus en zijn varianten zoveel als mogelijk wil limiteren. Vluchten zullen slechts de volgende doelen hebben: Cargo, essentieel of repatriatie.

Maatregel 8

Binnenlandse vluchten zijn uitsluitend toegestaan voor cargo, repatrianten en voor noodgevallen.

Maatregel 9

Personen in quarantaine of isolatie, thuis of op een daartoe aangewezen locatie, of opgenomen in een ziekeninrichting, dienen zich strikt te houden aan de maatregelen. Het is hun strikt verboden om de vorengenoemde plekken te verlaten en zich elders te begeven of te bevinden.

Maatregel 10

Per heden 18 mei 2021 zijn alle overheidskantoren gesloten. Slechts essentiële overheidsdiensten worden met minimale bezetting opengehouden. Ambtenaren zullen via desbetreffende ministeries worden geïnformeerd in de komende dagen welke diensten open zullen zijn. De directies geven hiertoe de instructies aan het personeel schriftelijk. Dat geldt niet voor de hoge colleges van Staat (regering, DNA).

Maatregel 11

Het toezicht op de naleving van algemene Covid-19 protocollen op de werkvloer wordt opgevoerd, door de aanwijzing van een functionaris door de werkgever, die speciaal hierop moet toezien.

Maatregel 12

Alcoholgebruik voor en nabij winkels en supermarkten is niet toegestaan. Ook het onnodig ophouden rond winkels en supermarkten is niet toegestaan.

Maatregel 13

Er mag per gezin slechts 1 persoon toegelaten worden tot supermarkten en andere zaken die volgens het protocol zijn opengesteld.

Maatregel 14

Alle markten zijn in deze periode gesloten. Marktverkopers, groente- en fruitventers kunnen langs de straat/weg hun waar aanbieden.

Maatregel 15

Alle winkels en overige publieke plekken dienen een uur voor de aanvang van lockdown hun deuren te sluiten.

Maatregel 16

Scholen zijn tot nader order gesloten.

Alle individuele dispensatie formulieren, verstrekt door het Kabinet van de President, komen per 31 mei 2021 te vervallen. Verzoeken voor verlenging worden slechts gehonoreerd indien betrokkene beschikt over een bewijs van volledige vaccinatie. Collectieve dispensatie formulieren (bedrijfspersoneel etc.) blijven vooralsnog geldig.

Extra vermelding:
1.Op eenieder wordt een beroep gedaan om de regels en maatregelen strikt na te komen, teneinde de verspreiding van het Covid-19 virus te voorkomen en het beheersbaar te houden.
2. Op eenieder wordt het beroep gedaan om elke overtreding van deze maatregelen terstond te melden aan de POLITIE of andere handhavingsorganen (115, 178)

Opmerking;
Op de bevolking doen wij een beroep thuis te blijven en slechts voor noodzakelijke werkzaamheden, doktersbezoek, of boodschappen uit huis te gaan. Wij brengen nogmaals in herinnering dat het dragen van een mond-neusmasker verplicht is. Met name in openbare ruimten, voertuigen en in de nabijheid van groepen mensen. Laat u zich reeds bij beginnende symptomen zo snel mogelijk testen en isoleert u zich van collega's en familie totdat de test uitslag bekend is.
Covid-19 gerelateerde klachten of verschijnselen kunnen zijn:
– Koorts (temp vanaf 37,5°C) zonder duidelijke andere oorzaak (of koortsig gevoel)
– Hoesten
– Verlies van smaak of reuk
– Lichaamspijnen of spierpijnen zonder duidelijke andere oorzaak
– Diarree of braken
– Misselijkheid
– Vermoeidheid
– Koude rillingen
– Kortademigheid
– Keelpijn
– Verwardheid
– Hoofdpijn (relatief vaak achter de oogbollen)
– Loopneus
– Algehele malaise/algeheel onwel bevinden
– Ontsteking aan handen of voeten

Category: Actueel • By DNV-MAIN • May 18, 2021

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PREVIOUS
Avondklok om 18.00 uur; geen weekend lockdown

Overzicht Actueel

Weekend lockdown, malls/kledingzaken/detailhandel/markten dicht

May 18, 2021

Avondklok om 18.00 uur; geen weekend lockdown

May 13, 2021

Zaterdag van 5 tot 17.00 uur geen lockdown

May 6, 2021

PRODUKTIE 2

J.P.Morgan

Global Quantitative & Derivatives Strategy
20 May 2020

Market and Volatility Commentary

Political risks of pandemic, data favors further reopening

Global Quantitative and Derivatives Strategy
Marko Kolanovic, PhD [AC]
(1-212) 622-3677
marko.kolanovic@jpmorgan.com
Bram Kaplan, CFA
(1-212) 272-1215
bram.kaplan@jpmorgan.com
J.P. Morgan Securities LLC

When the pandemic struck the US, we knew that the timeline of the virus will be the most important, and perhaps the only relevant variable determining the path of the economy and financial markets. Hence, we put our efforts into forecasting the path of the pandemic and concluded that by mid-April, conditions will be met to start re-opening economies (see overview of our forecasts here). This conclusion, together with the extraordinary monetary and fiscal measures implemented (see our report here), informed our forecast that markets will recover much quicker than the consensus expects. While the epidemic and markets largely followed our forecasts, politics emerged as a new and significant risk. Despite the conditions for re-opening being mostly met across the US, it is not yet happening in the largest economic regions (e.g. CA, NY, etc.), and worrying populism related to the virus is putting at risk global cooperation and trade. As the virus risk is abating globally, political/geopolitical fallout is emerging as a new risk. For example, just today the US senate passed a bill to bar Chinese companies from being listed on US exchanges.

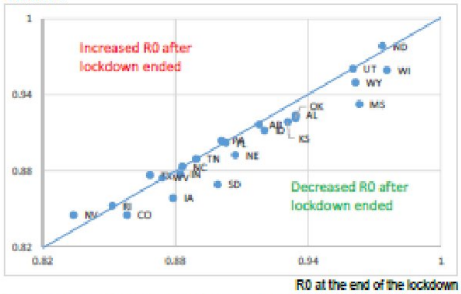
First let's see how the economic lockdowns evolved. At first, flawed scientific papers predicted several million virus deaths in the west. This on its own was odd, given that in China there were only several thousand deaths, and the mortality rate outside of Wuhan was very low. In the absence of conclusive data, these lockdowns were justified initially. Nonetheless, many of these efforts were inefficient or late. Indeed, recent studies indicate that full lockdown policies in some European countries did not produce any change pandemic parameters (such as growth rates R0) and hence might not have yielded additional benefits vs. less restrictive social distancing measures (see research paper). While our knowledge of the virus and lack of effectiveness of total lockdowns evolved, lockdowns remained in place and focus shifted to contact tracing, contemplating second wave outbreaks, and ideas about designing better educational, political and economic systems. At the same time, millions of livelihoods were being destroyed by these lockdowns. Unlike rigorous testing of potential new drugs, lockdowns were administered with little consideration that they might not only cause economic devastation but potentially more deaths than COVID-19 itself (see here, here).

See page 4 for analyst certification and important disclosures.

Marko Kolanovic, PhD
(1-212) 622-3677
marko.kolanovic@jpmorgan.com

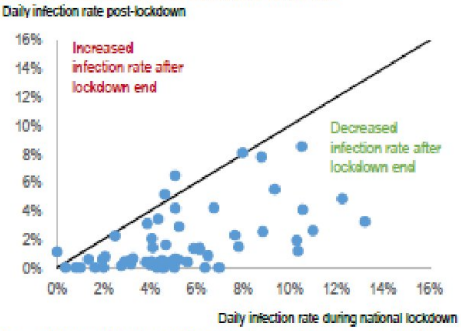
While we often hear that lockdowns are driven by scientific models, and that there is an exact relationship between the level of economic activity and spread of virus - this is not supported by the data. Figure 2 below show virus spread rates before and after lockdown for different countries around the world, and Figure 1shows the spread for US states that have re-opened (also see here). In particular, regression shows that infection rates declined, not increased, after lockdowns ended (for US states we show most recent R_0 vs R_0 on the day of lockdown end, and for countries we show infection rates). For example, the data in Figure 2 shows a decrease in infection rates after countries eased national lockdowns with >99% statistical significance. Indeed, virtually everywhere, infection rates have declined after reopening even after allowing for an appropriate measurement lag. This means that the pandemic and COVID-19 likely have its own dynamics unrelated to often inconsistent lockdown measures that were being implemented. The fact that re-opening did not change the course of pandemic is consistent with mentioned studies showing that initiation of full lockdowns did not alter the course of the pandemic either (see research paper). These virus dynamics are perhaps driven by the elimination of the most effective spreaders (e.g. see research paper), impact on the most vulnerable populations such as in nursing homes, common sense measures unrelated to full lockdowns (such as washing hands, etc.)and weather patterns in the northern hemisphere, etc.

Figure 1: R_0 during lockdown vs. after lockdown end by US state



Source: J.P. Morgan Quantitative and Derivatives Strategy

Figure 2: The vast majority of countries had decreased COVID-19 infection rates after national lockdowns were lifted



Source: J.P. Morgan Quantitative and Derivatives Strategy. Infection rate measured with a 7-day lag to allow for testing lags

So can one continue to justify stringent lockdowns in light of the above observations? This question has divided the country. Below we discuss some political implications of the lockdowns, including winners, losers, and the economic impact.

- US Elections – Even before the worst of the pandemic hit the US, the response of the current administration to COVID-19 became a focal point of election campaigns (e.g. COVID-19 ads by then candidate Michael Bloomberg). Election logic and backtests would say, the worse the virus impacts the US, the lower the chances of an incumbent's re-election given the economic pain, high unemployment and lack of health care during the pandemic. Indeed the initial response of the administration was to downplay the risk of the COVID-19 epidemic. However, since then, this simplistic thesis changed significantly. The administration shifted to forecasting a

larger negative impact (setting the stage for them to 'outperform', and e.g. 'hedging' the Georgia reopening), shifting the pandemic blame to China and the WHO, and at the same time shifting the blame for economic pain to large blue states that are perceived to be slowing down the reopening of the economy. Indeed, allowed economic activity across the country is now largely following partisan lines.

- Economic interest – Clearly there are economic winners and losers of prolonged shutdowns and social distancing. Working remotely, software/cloud, online shopping and socializing, etc. all benefit large technology firms. It should not come as a surprise that large tech stocks are near all-time highs. This could create (perhaps wrong) perceptions of conflicts of interest when the leading technology firms are influencing policies related to reopening (such as reimagining education, health care, vaccines, contact tracking and tracing etc.).

- Big vs. Small government – another political fault line exposed by COVID-19 is the role and scope of government in everyday life, encompassing questions such as: should lockdowns be recommended or mandated, how much of individual freedoms should be limited, etc. Government employees have been less affected by lockdowns than e.g. small private businesses, etc. Moreover, these ideological fault lines exposed by COVID-19 are to an extent replicated and exported to other countries in the west.

On the other side of the political spectrum, demagogues and radicals across the world will be tempted to use COVID-19 to blame immigrants, people of different race, or use the pandemic as a pretext to intensify geopolitical tensions. Blaming the pandemic on an ethnic group or country can provide a convenient excuse for various failings at home, or may provide pretext to push a geopolitical or protectionist agenda. This is perhaps even more dangerous than using the pandemic to further domestic political outcomes.

We will closely monitor how these risks evolve, but at this point see them as potential tail risks rather than an imminent threat, and thus maintain our positive outlook on markets.

PRODUKTIE 3

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ORIGINAL ARTICLE

WILEY

Assessing mandatory stay-at-home and business closure effects on the spread of COVID-19

Eran Bendavid[1,2] | **Christopher Oh**[1] | **Jay Bhattacharya**[2] | **John P. A. Ioannidis**[1,3,4,5,6]

[1]Department of Medicine, Stanford University, Stanford, CA, USA

[2]Center for Health Policy and the Center for Primary Care and Outcomes Research, Stanford University, Stanford, CA, USA

[3]Department of Epidemiology and Population Health, Stanford University, Stanford, CA, USA

[4]Department of Biomedical Data Science, Stanford University, Stanford, CA, USA

[5]Department of Statistics, Stanford University, Stanford, CA, USA

[6]Meta-Research Innovation Center at Stanford (METRICS), Stanford University, Stanford, CA, USA

Correspondence
Eran Bendavid, Department of Medicine, Stanford University, Stanford CA 94305, USA.
Email: ebd@stanford.edu

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Abstract

Background and Aims: The most restrictive nonpharmaceutical interventions (NPIs) for controlling the spread of COVID-19 are mandatory stay-at-home and business closures. Given the consequences of these policies, it is important to assess their effects. We evaluate the effects on epidemic case growth of more restrictive NPIs (mrNPIs), above and beyond those of less-restrictive NPIs (lrNPIs).

Methods: We first estimate COVID-19 case growth in relation to any NPI implementation in subnational regions of 10 countries: England, France, Germany, Iran, Italy, Netherlands, Spain, South Korea, Sweden and the United States. Using first-difference models with fixed effects, we isolate the effects of mrNPIs by subtracting the combined effects of lrNPIs and epidemic dynamics from all NPIs. We use case growth in Sweden and South Korea, 2 countries that did not implement mandatory stay-at-home and business closures, as comparison countries for the other 8 countries (16 total comparisons).

Results: Implementing any NPIs was associated with significant reductions in case growth in 9 out of 10 study countries, including South Korea and Sweden that implemented only lrNPIs (Spain had a nonsignificant effect). After subtracting the epidemic and lrNPI effects, we find no clear, significant beneficial effect of mrNPIs on case growth in any country. In France, for example, the effect of mrNPIs was +7% (95% CI: −5%-19%) when compared with Sweden and + 13% (−12%-38%) when compared with South Korea (positive means pro-contagion). The 95% confidence intervals excluded 30% declines in all 16 comparisons and 15% declines in 11/16 comparisons.

Conclusions: While small benefits cannot be excluded, we do not find significant benefits on case growth of more restrictive NPIs. Similar reductions in case growth may be achievable with less-restrictive interventions.

1 | INTRODUCTION

The spread of COVID-19 has led to multiple policy responses that aim to reduce the transmission of the SARS-CoV-2. The principal goal of these so-called nonpharmaceutical interventions (NPI) is to reduce transmission in the absence of pharmaceutical options in order to reduce resultant death, disease and health system overload. Some of the most restrictive NPI policies include mandatory stay-at-home and business closure orders ('lockdowns'). The early adoption of these more restrictive nonpharmaceutical interventions (mrNPIs) in early 2020 was justified because of the rapid spread of the disease, overwhelmed health systems in some hard-hit places and substantial uncertainty about the virus' morbidity and mortality.[1]

Because of the potential harmful health effects of mrNPI—including hunger,[2] opioid-related overdoses,[3] missed vaccinations,[4,5] increase in non-COVID diseases from missed health services,[6-9] domestic abuse,[10] mental health and suicidality,[11,12] and a host of economic consequences with health implications[13,14]—it is increasingly recognized that their postulated benefits deserve careful study. One approach to evaluating NPI benefits uses disease modelling approaches. One prominent modelling analysis estimated that, across Europe, mrNPIs accounted for 81% of the reduction in the effective reproduction number (R_t), a measure of disease transmission.[15] However, in the absence of empirical assessment of the policies, their effects on reduced transmission are assumed rather than assessed.[16,17] That analysis attributes nearly all the reduction in transmission to the last intervention, whichever intervention happened to be last, complete lockdowns in France or banning of public events in Sweden.[16]

Another, more empirically grounded approach to assessing NPI effects uses statistical regression models and exploits variation in the location and timing of NPI implementations to identify changes in epidemic spread following various policies.[18] These empirical studies find large reductions in the growth rate of new cases that are attributable to NPIs. An important challenge with these analyses is that they use pre-policy growth rates to determine the 'counterfactual' trajectory of new cases—the expected case growth rate in the absence of NPIs. This is problematic because it is widely recognized that epidemic dynamics are time-varying, and brakes on disease transmission occur without any interventions (through resolution of infections), as well as from behaviour changes unrelated to the NPIs.[19,20] These epidemic dynamics are demonstrated by an analysis showing that slowing of COVID-19 epidemic growth was similar in many contexts, in a way that is more consistent with natural dynamics than policy prescriptions.[21]

These challenges suggest that assessing the impact of mrNPIs is important, yet difficult. We propose an approach that balances the strengths of empirical analyses while taking into consideration underlying epidemic dynamics. We compare epidemic spread in places that implemented mrNPIs to counterfactuals that implemented only less-restrictive NPIs (lrNPIs). In this way, it may be possible to isolate the role of mrNPIs, net of lrNPIs and epidemic dynamics.

Here, we use Sweden and South Korea as the counterfactuals to isolate the effects of mrNPIs in countries that implemented mrNPIs and lrNPIs. Unlike most of its neighbours that implemented mandatory stay-at-home and business closures, Sweden's approach in the early stages of the pandemic relied entirely on lrNPIs, including social distancing guidelines, discouraging of international and domestic travel, and a ban on large gatherings.[22,23] South Korea also did not implement mrNPIs. Its strategy relied on intensive investments in testing, contact tracing and isolation of infected cases and close contacts.[24,25]

2 | METHODS

We isolate the effect of more restrictive NPIs (mrNPIs) by comparing the combined effect size of all NPIs in 8 countries that implemented more restrictive policies (England, France, Germany, Iran, Italy, the Netherlands, Spain and the United States) with the effect size of all NPIs in the 2 countries that only implemented less-restrictive NPIs (lrNPIs). In effect, we follow the general scheme:

$$\begin{aligned}\text{Effects of mrNPI} = {} & \text{Effects of (mrNPI} + \text{lrNPI} + \text{epidemic dynamics)} \\ & - \text{Effects of (lrNPI} + \text{epidemic dynamics)}\end{aligned}$$

We analyse only these countries because the analysis depends on subnational data, which were only available for those countries, as explained further below.

The conceptual model underlying this approach is that, prior to meaningful population immunity, individual behaviour is the primary driver of reductions in transmission rate, and that any NPI may provide a nudge towards individual behaviour change, with response rates that vary between individuals and over time. lrNPIs could have large anti-contagion effects if individual behavioural response is large, in which case additional, more restrictive NPIs may not provide much additional benefit. On the other hand, if lrNPIs provide relatively small nudges to individual behaviour, then mrNPIs may result in large behavioural effects at the margin, and large reductions in the growth of new cases. However, because underlying epidemic dynamics are imprecisely characterized and are important for estimating the policy effects, our models test the extent to which mrNPIs had additional effect on reducing transmission by differencing the sum of NPI effects and epidemic dynamics in countries that did not enact mrNPIs from the sum of NPI effects and epidemic dynamics in countries that did.

We estimate the unique effects of mrNPIs on case growth rate during the Northern Hemisphere spring of 2020 in England, France, Germany, Iran, Italy, the Netherlands, Spain and the United States by comparing the effect of NPIs in these countries to those in Sweden and South Korea (separately). The data we use build on an analysis of NPI effects and consist of daily case numbers in subnational administrative regions of each country (eg regions in France, provinces in Iran, states in the United States and counties in Sweden), merged with the type and timing of policies in each administrative region.[18,26] We use data from a COVID-19 policy databank and previous analyses of policy impacts to determine the timing and location of each NPI.[18,27] Each observation in the data, then, is identified by the subnational administrative region and the date, with data on the number of cases on that date and indicators characterizing the presence of each policy. We include indicators for changes in case definitions or testing technologies to capture abrupt changes in case counts that are not the result of the underlying epidemic (these are mostly single-day indicators), as suggested in a previous analysis.[18]

We define the dependent variable as the daily difference in the natural log of the number of confirmed cases, which approximates the daily growth rate of infections (g). We then estimate the following linear models:

$$g_{cit} = \theta_{0,ci} + \delta_{ct} + \sum_{p=1}^{p_c} (\gamma_{pc} Policy_{pcit}) + \mu_{cit} + \varepsilon_{cit}$$

The model terms are indexed by country (c), subnational unit (i), day (t) and NPI indicator (p). $\theta_{0,ci}$ is a series of fixed effects for the subnational unit, and δ_{ct} is country-specific day-of-week fixed effects. The parameters of interest are γ_{pc}, which identify the effect of each policy on the growth rate in cases. The parameter μ_{cit} is a single-day indicator that models changes in case definitions that result in short discontinuities in case counts that are not due to underlying epidemic changes.

We estimate these models separately for each pair of countries (one with mrNPIs, one without), for a total of 16 models. We then add the coefficients of all the policies for the country with mrNPIs (yielding the combined effects of all NPIs in the mrNPI country) and subtract the combined effects of all NPIs in the comparator country without mrNPI. As noted above, the difference isolates the effect of mrNPIs on case growth rates. We estimate robust standard errors throughout, with clustering at the day-of-week level to account for serial correlation.

It is important to note that because the true number of infections is not visible in any country, it is impossible to assess the impact of national policies on transmission or new infections.[28] Instead, we follow other studies evaluating the effects of NPIs that use case numbers, implicitly assuming that their observed dynamics may represent a consistent shadow of the underlying infection dynamics.[18]

The code for the data preparation, analysis and visualization is provided along with the article (Supplementary Material).

3 | RESULTS

The growth rate in new cases prior to implementation of any NPIs was positive in all study countries (Figure 1). The figure shows that, across all subnational units in all ten countries, the average growth rate prior to NPIs ranged from 0.23 in Spain (23% daily growth; 95% CI: 0.13 to 0.34) to 0.47 (95% CI: 0.39 to 0.55) in the Netherlands. The average across all 10 countries was 0.32, and in South Korea and Sweden, the 2 countries without mrNPIs, the pre-NPI growth rates were 0.25 and 0.33, respectively. The variation of pre-policy growth rates in cases may reflect epidemic intensity, testing coverage (higher growth may be a reflection of expanding testing capacity and of more people wishing to be tested) and pre-policy behaviour changes that led to increased or decreased transmission.

Figures 2 and 3 and demonstrate the effects of individual NPIs (Figure 2) and all NPIs combined (Figure 3) on daily growth in case counts. While the effects of 3 individual NPIs were positive—that is, contributing paradoxically to case growth—and significant (one in Germany, one in Italy and one in Spain, out of 51 individual NPIs in all 10 countries), the effects of about half of individual NPIs were negative and significant. The combined effects of all NPIs (Figure 3) were negative and significant in 9 out of 10 countries, where their combined effects ranged from −0.10 (95% CI: −0.06 to −0.13) in England to −0.33 (95% CI: −0.09 to −0.57) in South Korea. Spain was the only country where the effect of NPIs was not distinguishable from 0 (−0.02; 95% CI: −0.12 to 0.07).

Figure 4 shows the effect of mrNPIs in the 8 countries where mrNPIs were implemented, after accounting for the effects of lrNPIs and underlying epidemic dynamics. In none of the 8 countries and in none out of the 16 comparisons (against Sweden or South Korea) were the effects of mrNPIs significantly negative (beneficial). The point estimates were positive (point in the direction of mrNPIs resulting in increased daily growth in cases) in 12 out of 16 comparisons (significantly positive in 3 of the 12, in Spain and in England compared with Sweden). The only country where the point estimates of the effects of mrNPIs were negative in both comparisons was Iran (−0.07 [95% CI: −0.21 to 0.07] compared with Sweden; −0.02 [95% CI: −0.28 to 0.25] compared with South Korea). The 95% confidence intervals excluded a 30% reduction in daily growth in all 16 comparisons.

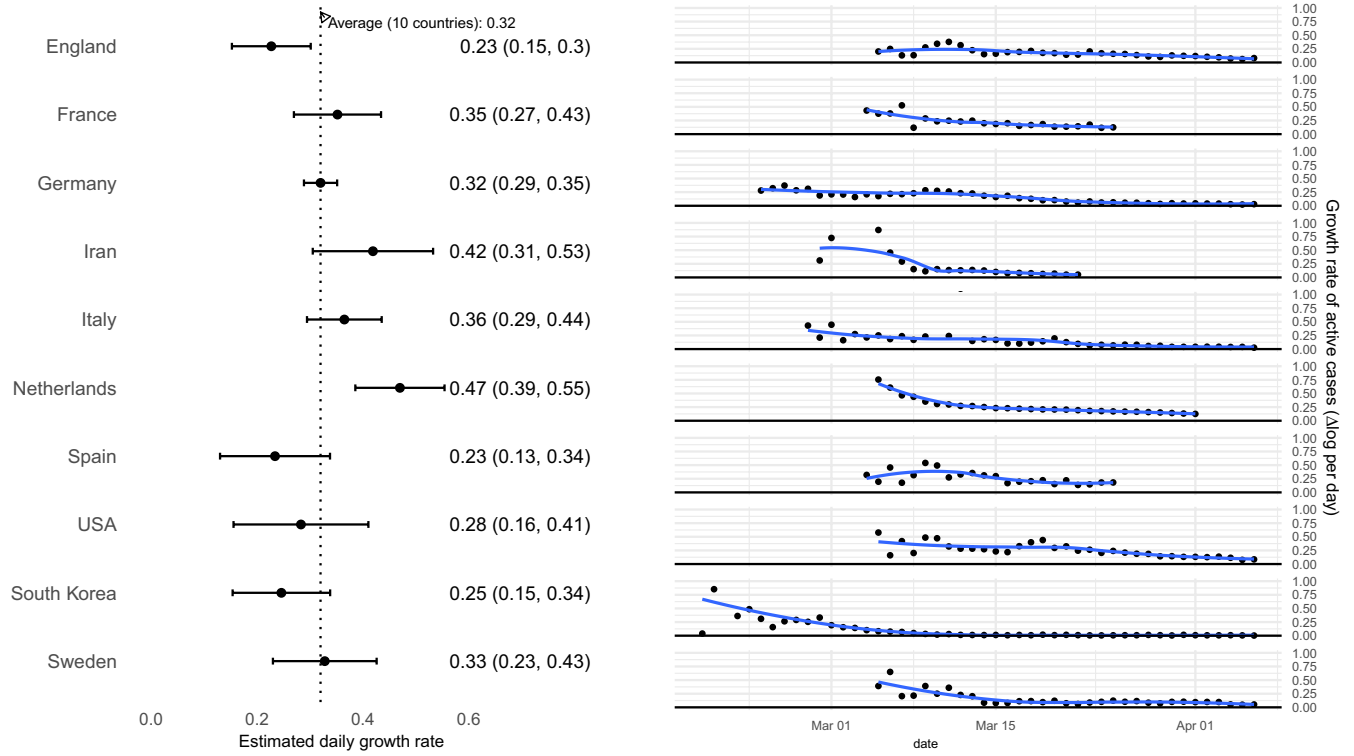

FIGURE 1 Growth rate in cases for study countries. The black bars demonstrate the average growth rate in cases in each subnational unit (95% CI) prior to any policies implemented. The figures to the right show the daily growth rate in cases for each of the countries and demonstrate the shared decline in case growth across all countries, including the countries that did not implement mrNPIs (South Korea and Sweden)

4 | DISCUSSION

In the framework of this analysis, there is no evidence that more restrictive nonpharmaceutical interventions ('lockdowns') contributed substantially to bending the curve of new cases in England, France, Germany, Iran, Italy, the Netherlands, Spain or the United States in early 2020. By comparing the effectiveness of NPIs on case growth rates in countries that implemented more restrictive measures with those that implemented less-restrictive measures, the evidence points away from indicating that mrNPIs provided additional meaningful benefit above and beyond lrNPIs. While modest decreases in daily growth (under 30%) cannot be excluded in a few countries, the possibility of large decreases in daily growth due to mrNPIs is incompatible with the accumulated data.

The direction of the effect size in most scenarios points towards an *increase* in the case growth rate, though these estimates are only distinguishable from zero in Spain (consistent with nonbeneficial effect of lockdowns). Only in Iran do the estimates consistently point in the direction of additional reduction in the growth rate, yet those effects are statistically indistinguishable from zero. While it is hard to draw firm conclusions from these estimates, they are consistent with a recent analysis that identified increased population-level transmission and cases in Hunan, China, during the period of stay-at-home orders, attributed to increased intra-household density and transmission.[29] In other words, it is possible that stay-at-home orders may facilitate transmission if they increase person-to-person contact where transmission is efficient such as closed spaces.

Our study builds on the findings of overall effectiveness of NPIs in reducing case growth rate. This has a plausible underlying behavioural mechanism: NPIs are motivated by the notion that they lead to anti-contagion behaviour changes, either directly through personal compliance with the interventions, or by providing a signal about disease risk, as communicated by policymakers, which is used in deciding on individual behaviours. The degree to which risk communications motivate personal behaviours has been used to explain South Korea's response to NPIs, where large personal behaviour changes were observed following less-restrictive NPIs.[30]

This analysis ties together observations about the possible effectiveness of NPIs with COVID-19 epidemic case growth changes that appear surprisingly similar despite wide variation in national policies.[31-33] Our behavioural model of NPIs—that their effectiveness depends on individual behaviour for which policies provide a noisy nudge—helps explain why the degree of NPI restrictiveness does not seem to explain the decline in case growth rate. Data on individual behaviours such

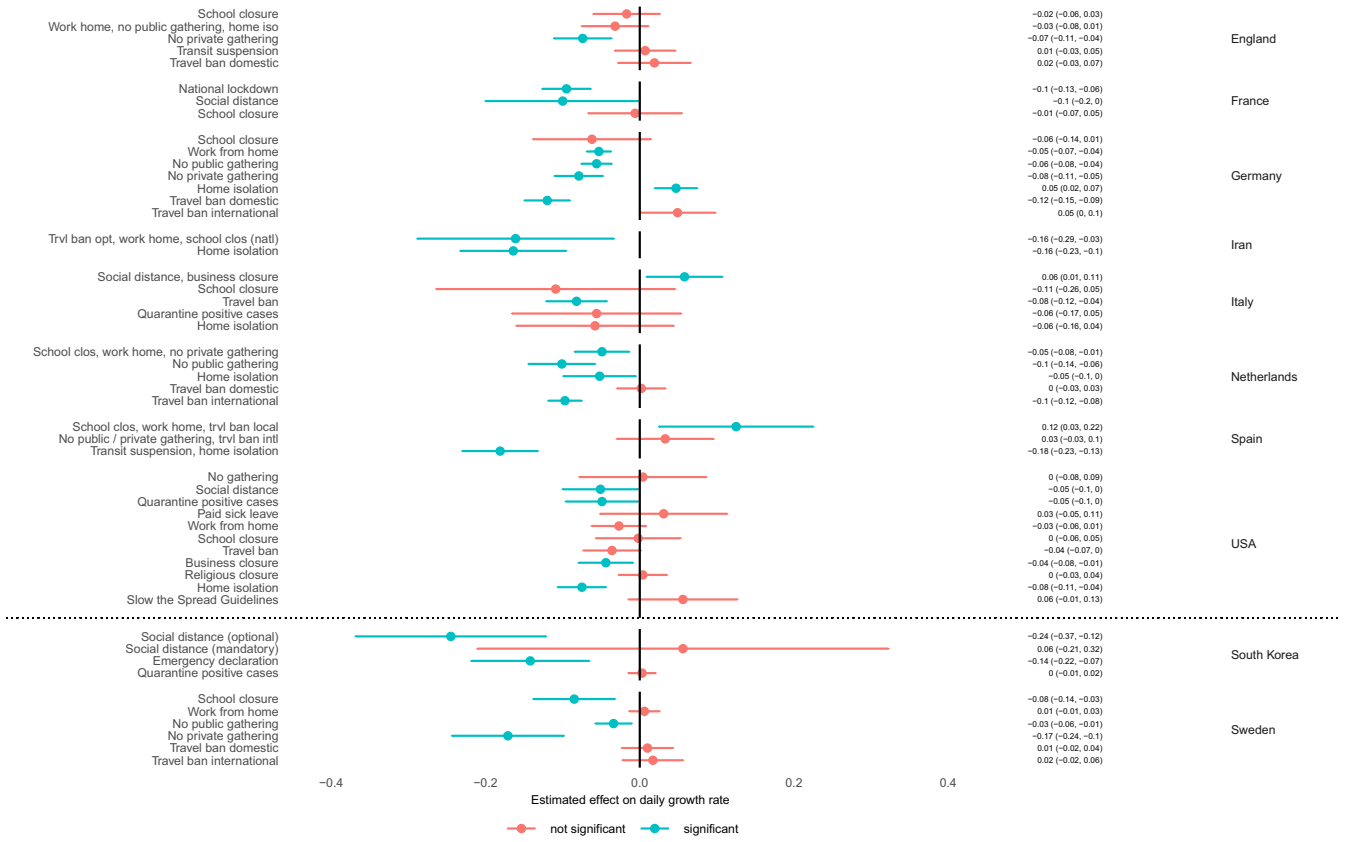

FIGURE 2 Effects of individual NPIs in all study countries. The variation in the timing and location of NPI implementation allows us to identify the effects of individual NPIs on the daily growth rate of cases. Where multiple NPIs were implemented simultaneously (in the same day) across all subnational units (eg school closure, work from home and no private gatherings in Spain), their overall effect cannot be identified individually and is shown combined

as visits to businesses, walking or driving show dramatic declines days to weeks prior to the implementation of business closures and mandatory stay-at-home orders in our study countries, consistent with the behavioural mechanisms noted above.[34-36] These observations are consistent with a model where the severity of the risk perceived by individuals was a stronger driver of anti-contagion behaviours than the specific nature of the NPIs. In other words, reductions in social activities that led to reduction in case growth were happening prior to implementation of mrNPIs because populations in affected countries were internalizing the impact of the pandemic in China, Italy and New York, and noting a growing set of recommendations to reduce social contacts, all of which happened before mrNPIs. This may also explain the highly variable effect sizes of the same NPI in different countries. For example, the effects of international travel bans were positive (unhelpful) in Germany and negative (beneficial) in the Netherlands (Figure 2).

While this study casts doubt on any firm conclusions about the effectiveness of restrictive NPIs, it also underscores the importance of more definitive evaluations of NPI effects. NPIs can also have harms, besides any questionable benefits, and the harms may be more prominent for some NPIs than for others. For example, school closures may have very serious harms, estimated at an equivalent of 5.5 million life years for children in the United States during the spring school closures alone.[37] Considerations of harms should play a prominent role in policy decisions, especially if an NPI is ineffective at reducing the spread of infections. Of note, Sweden did not close primary schools throughout 2020 as of this writing.

While we find no evidence of large anti-contagion effects from mandatory stay-at-home and business closure policies, we should acknowledge that the underlying data and methods have important limitations. First, cross-country comparisons are difficult: countries may have different rules, cultures and relationships between the government and citizenry. For that reason, we collected information on all countries for which subnational data on case growth were obtainable. Of course, these differences may also exist across subnational units, as demonstrated in the case of different states in the United States. Additional countries could provide more evidence, especially countries that had meaningful epidemic penetration and did not use mrNPIs for epidemic control. Second, confirmed case counts are a

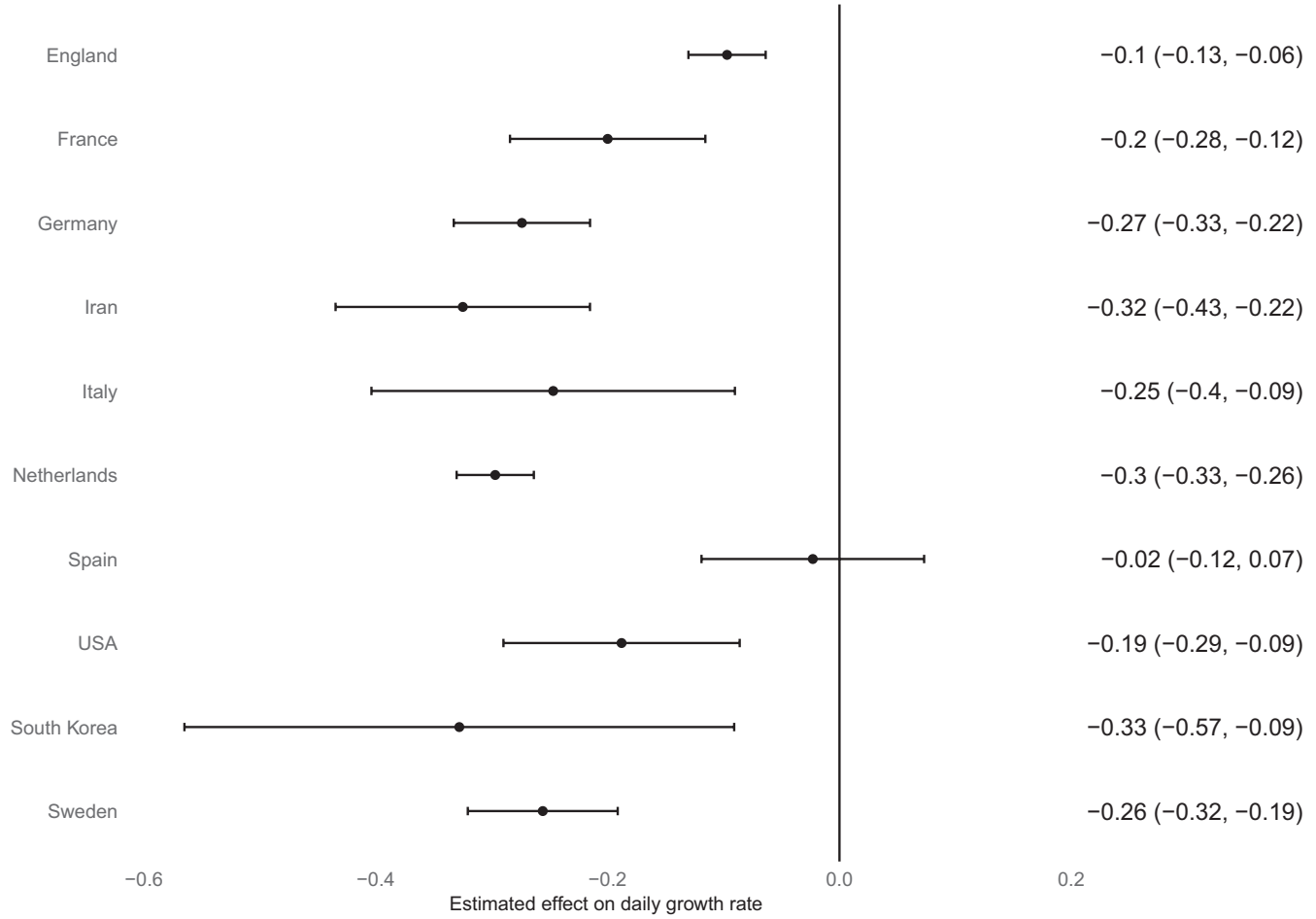

FIGURE 3 Combined effects of all NPIs in study countries. The point estimate and 95% CI of the combined effect of NPIs on growth rate in cases, estimated from a combination of individual NPIs. The estimates show significant effects in all countries except Spain and range from a 33% (9%-57%) decline in South Korea to 10% (6%-13%) in England. The point estimate of the effect in Spain is also negative but small (2%) and not significant

noisy measure of disease transmission. Testing availability, personal demand for or fear of getting tested, testing guidelines, changing test characteristics and viral evolution all interfere in the relationship between the underlying infections and case counts. Because the location and timing of policies are endogenous to perceived epidemic stage, the noise in case counts is associated with the policies, making bias possible and very difficult to eradicate. The fixed-effects approach provides unbiased estimates so long as the location or timing of policies is quasi-arbitrary with respect to the outcome. This may fail to hold in this assessment of NPI effects because the underlying epidemic dynamics are nonlinear, and the policies respond to—and modify—the epidemic stage. This limitation also holds for all other empirical assessments of NPI effects.[18]

Third, our findings rest on a conceptualization, common in the literature, of NPIs as 'reduced-form' interventions: an upstream policy has expected downstream effects on transmission. This allows us to use Sweden and South Korea as comparators, since they had applied less-restrictive interventions, which then enable netting out the combined effect of lrNPIs and the underlying epidemic dynamics. While contextual factors that mediate the effects of NPIs are important—countries implemented different variants of the same NPI, and the population responded differently—many analyses examining the effects of NPIs have a similar 'reduced-form' structure.[18,31,38] In that sense, our comparison is positioned squarely within the literature on the effects of NPIs.

During the Northern Hemisphere fall and winter of 2020, many countries, especially in Europe and the United States, experienced a large wave of COVID-19 morbidity and mortality. Those waves were met with new (or renewed) NPIs, including mrNPIs in some countries (eg England) and lrNPIs in others (eg Portugal) that had used mrNPIs in the first wave. The spread of infections in countries that were largely spared in the spring (eg Austria and Greece) further highlights the challenges and limited ability of NPIs to control the spread of this highly transmissible respiratory virus. Empirical data for the characteristics

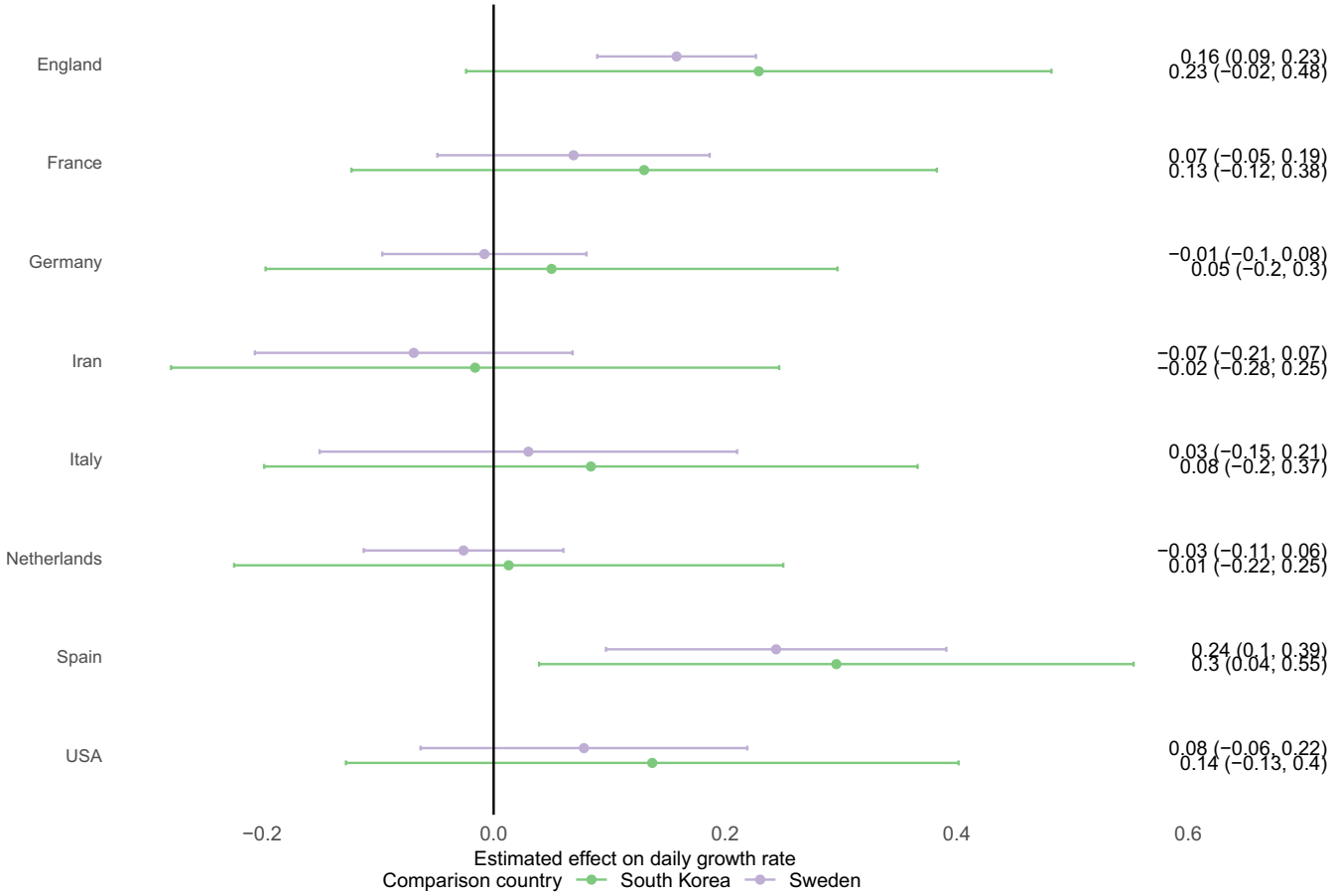

FIGURE 4 Effect of mrNPIs on daily growth rates after accounting for the effects of lrNPIs in South Korea and Sweden. Under no comparison is there evidence of reduction in case growth rates from mrNPIs, in any country. The point estimates are positive (point in the direction of mrNPIs resulting in *increased* daily growth in cases) in 12 out of 16 comparisons

of fatalities in the later wave before mrNPIs were adopted as compared with the first wave (when mrNPIs had been used) show that the proportion of COVID-19 deaths that occurred in nursing homes was often higher under mrNPIs rather than under less-restrictive measures.[39] This further suggests that restrictive measures do not clearly achieve protection of vulnerable populations. Some evidence also suggests[40] that sometimes under more restrictive measures, infections may be more frequent in settings where vulnerable populations reside relative to the general population.[40]

In summary, we fail to find strong evidence supporting a role for more restrictive NPIs in the control of COVID in early 2020. We do not question the role of all public health interventions, or of coordinated communications about the epidemic, but we fail to find an additional benefit of stay-at-home orders and business closures. The data cannot fully exclude the possibility of some benefits. However, even if they exist, these benefits may not match the numerous harms of these aggressive measures. More targeted public health interventions that more effectively reduce transmissions may be important for future epidemic control without the harms of highly restrictive measures.

5 | ROLE OF THE FUNDING ORGANIZATION OR SPONSOR

The funding organizations had no role in the design or execution of this analysis.

AUTHOR CONTRIBUTIONS

EB conceived the project; EB and CO designed the analyses, prepared the data and executed the analyses; JB and JPAI were involved in discussing and interpreting the results, and drafting, revising and improving the manuscript. All authors have approved the final manuscript.

ORCID

Eran Bendavid https://orcid.org/0000-0002-8364-4711
Jay Bhattacharya https://orcid.org/0000-0003-3867-3174
John P. A. Ioannidis https://orcid.org/0000-0003-3118-6859

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section.

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PRODUKTIE 4

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Why Is There No Correlation between Masks, Lockdowns, and Covid Suppression?

April 29, 2021 - 5:10 PM Anthony Rozmajzl [1]
Topics:
Bureaucracy and Regulation [2]

In the past couple of months, our esteemed public health experts have had a rough go of defending the supposedly settled science behind lockdowns and mask mandates.

White House covid-19 advisor Andy Slavitt was first on the chopping block back in mid-February, when he was reduced to parroting empty platitudes [3] about social distancing after failing to explain why a completely open Florida had numbers no worse than a strictly locked-down California. Then comes media darling Dr. Anthony Fauci, who has had a particularly embarrassing series of public appearances of late. During a recent MSNBC interview [4] Fauci expressed confusion and wasn't "quite sure" as to why Texas was experiencing falling cases and deaths an entire month after lifting its mask mandates and capacity restrictions. Moreover, during a hearing with Representative Jim Jordan, Fauci completely dodged [5] Jordan's question of why Texas has lower case rates than some of the most notable lockdown states. Fauci, refusing to answer the question, simply responded that having a lockdown is not the same thing as obeying lockdowns. Fauci was correct here, but he indirectly claimed that citizens of New York and New Jersey, two notorious lockdown states, were complying less with mitigation measures than a state that had, and still has, practically none. A quick check of Google's covid-19 mobility reports [6] lays this counterintuitive claim to rest.

The American Media's Agenda

When governments and media outlets around the world have successfully captured audiences by stoking fear of covid-19, the data that should so easily assuage this fear become irrelevant, and interviews like those mentioned above are simply brushed aside in favor of a fear-born allegiance to the "morally superior" government-mandated lockdowns, curfews, mask mandates, and more. This "scared straight" approach, as Bill Maher correctly described it [7], is the state's bludgeon of compliance.

As far as scaring citizens straight, Project Veritas has released footage showing CNN employees explaining [8] how the network plays up the covid-19 death toll to drive numbers. Especially disgraceful was CNN technical director Charlie Chester's admission that the network doesn't like to report recovery rates because "[t]hat's not scary…. If it bleeds it leads."

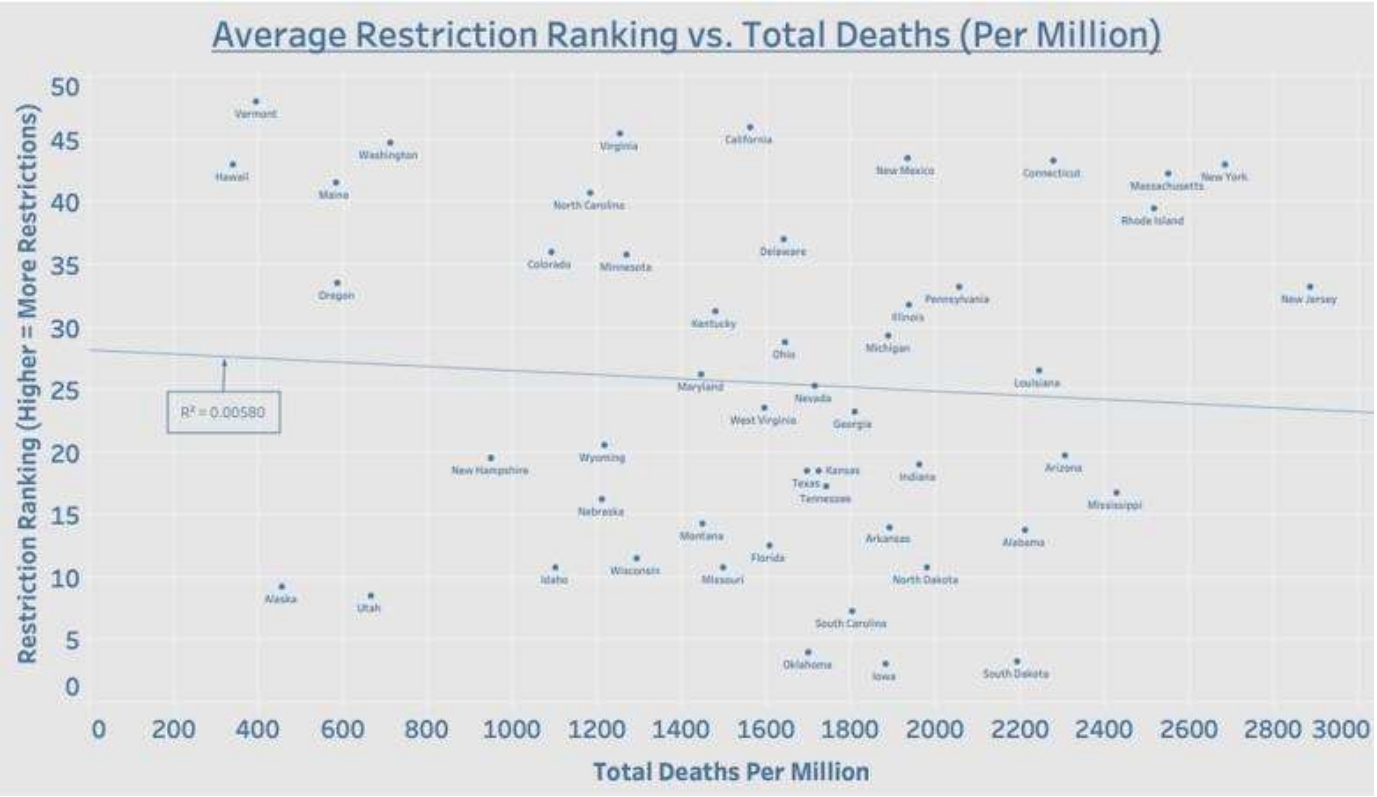
CNN isn't alone in the fearmongering business. Thanks to the surplus of United States media outlets willing to churn up a disproportionate amount of negative covid-19 headlines—roughly 90 percent [9] of covid-19 news in the United States is negative compared to 51 percent internationally—is it any surprise that nearly 70 percent of Democrats, 51 percent of Republicans, and almost 50 percent of independents [10] think the chances of being hospitalized with covid-19 range anywhere from 20 percent to over 50 percent?

Where's the Correlation?

Government- and media-induced panic have blinded us to the data, which for the past thirteen months have consistently shown zero correlation between the timing, strength, and duration of mitigation measures and covid-19 incidence. Nowhere could this lack of correlation be more prevalent than among lockdowns and mask usage.

Leaving aside the disastrous and deadly consequences of government lockdowns—see here [11], here [12], and here [13]—the evidence for lockdowns' ability to mitigate covid-19 mortality remains scant.

Looking at the United States, we can address the widely believed notion that states with more intense lockdowns will see fewer covid-19 deaths by plotting each state's average restriction ranking over the past thirteen months against the total number of covid-19 deaths for each state. To get the average ranking, the author averaged data from Oxford University's Blavatnik School of Government—this source ranked [14] each state by the average time spent at a stringency index measure greater than sixty up until mid-December 2020—and WalletHub, which also ranked each state by stringency using a weighted average of various measures [15] from January 2021 onward. Now, if the past year's worth of sanctimonious lectures from public health experts have any scientific weight behind them, we should see a very strong negative correlation between the intensity of states' restrictions and total covid-19 deaths.

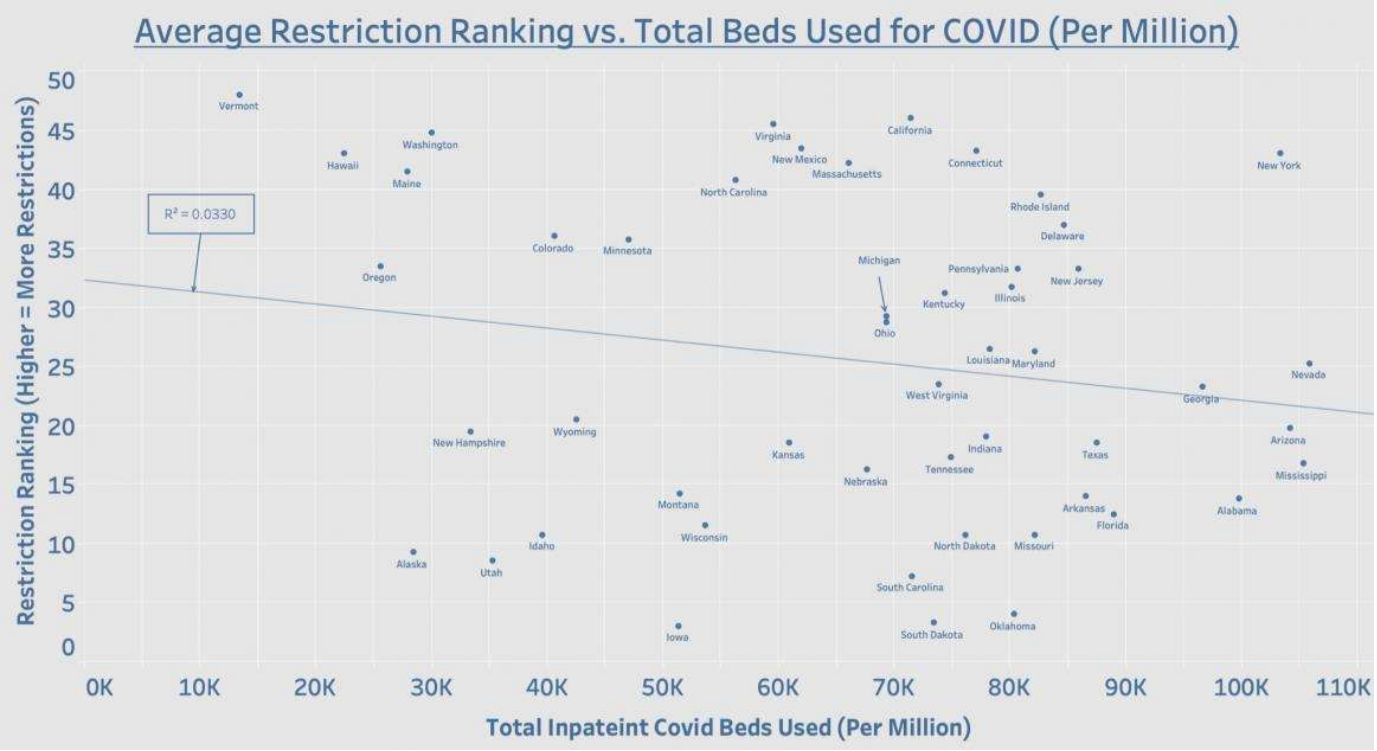


[16]

Source: Data on deaths (as of Apr. 28, 2021) from the NYTimes Covid-19 Data Bot [17]. Data on restriction rankings from the NYTimes Covid-19 Data Bot [17] (through December 2020); Adam McCann, "States with the Fewest Coronavirus Restrictions, [18]" WalletHub, Apr. 6, 2021 (since January 2021); and Laura Hallas, Ariq Hatibie, Saptarshi Majumdar, Monika Pyarali, and Thomas Hale, "Variation in US States' Responses to COVID-19" (Blavatnik School of Government Working Paper No. BSG-WP-2020/034, December 2020).

Contrary to what the public health experts have been telling us for more than a year, there is no correlation between the strength of a state's lockdown measures and total covid-19 deaths. In fact, notorious lockdown states such as New York and New Jersey have some of the worst mortality numbers to date. To blame noncompliance for these poor numbers is ridiculous on its face considering that states with no restrictions, such as Texas and Florida, have far fewer deaths than New York and New Jersey. In fact, you'll find that every state that has either removed its mask mandate or all covid-19 restrictions entirely [19] is outperforming New York and New Jersey in terms of deaths.

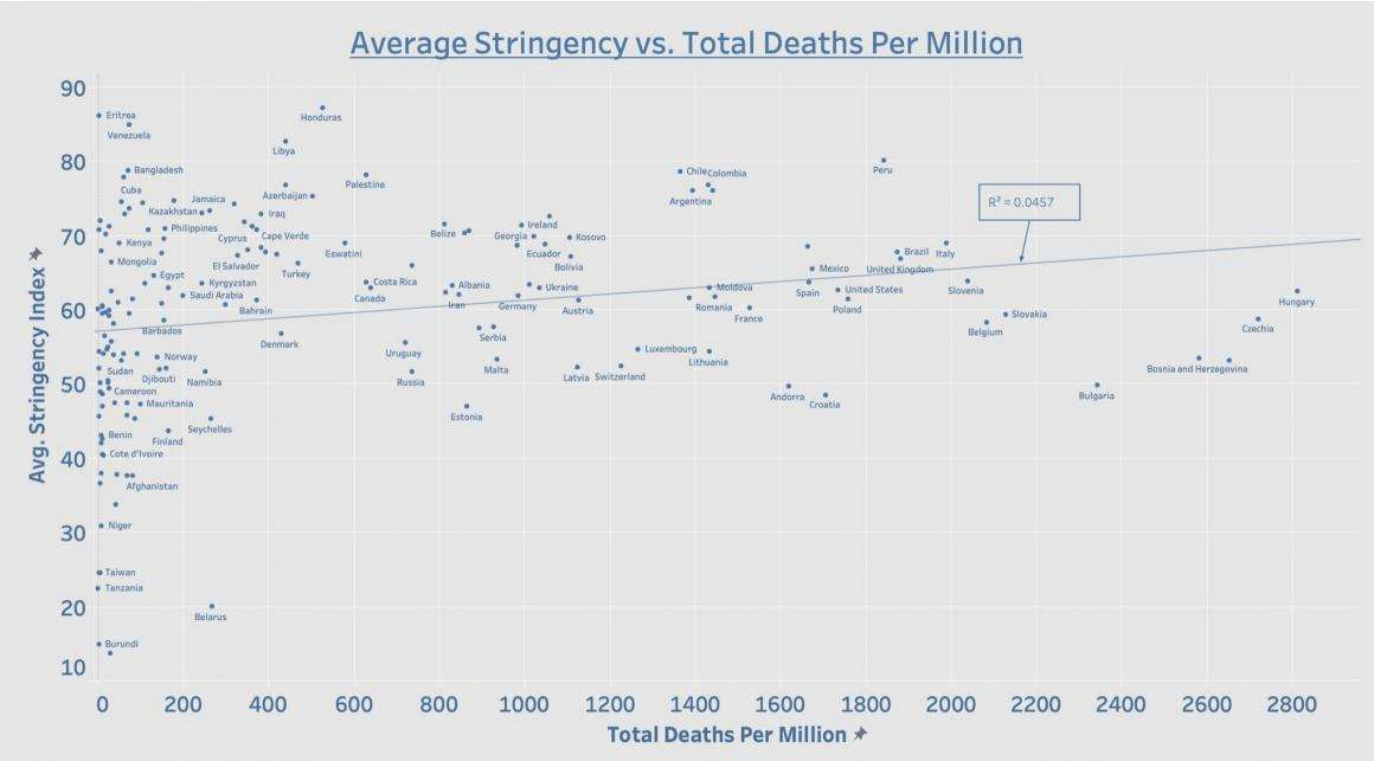
The same lack of correlation can be seen when comparing average lockdown stringency with the total number of patients hospitalized who have suspected or confirmed covid-19. As a point of clarification, the author summed the *current* number of patients hospitalized each day to arrive at the total number of patients hospitalized. This will result in slightly inflated total numbers, since patients may spend more than one day in the hospital, but having applied the same aggregation method across all states, the total hospitalization metric still provides an accurate assessment of covid-19 hospitalizations in each state.



[20]

Source: Data on hospitalizations (as of Apr. 24, 2021) from the US Department of Health and Human Services [21]. Data on restriction rankings from the NYTimes Covid-19 Data Bot [17] (through December 2020); Adam McCann, "States with the Fewest Coronavirus Restrictions, [18]" WalletHub, Apr. 6, 2021 (since January 2021); and Laura Hallas, Ariq Hatibie, Saptarshi Majumdar, Monika Pyarali, and Thomas Hale, "Variation in US States' Responses to COVID-19" (Blavatnik School of Government Working Paper No. BSG-WP-2020/034, December 2020).

Internationally speaking, the data continue to expose lockdowns as the single greatest public health failure in human history. Plotting lockdown stringency against total covid-19 death toll reveals, yet again, zero correlation between the two variables.



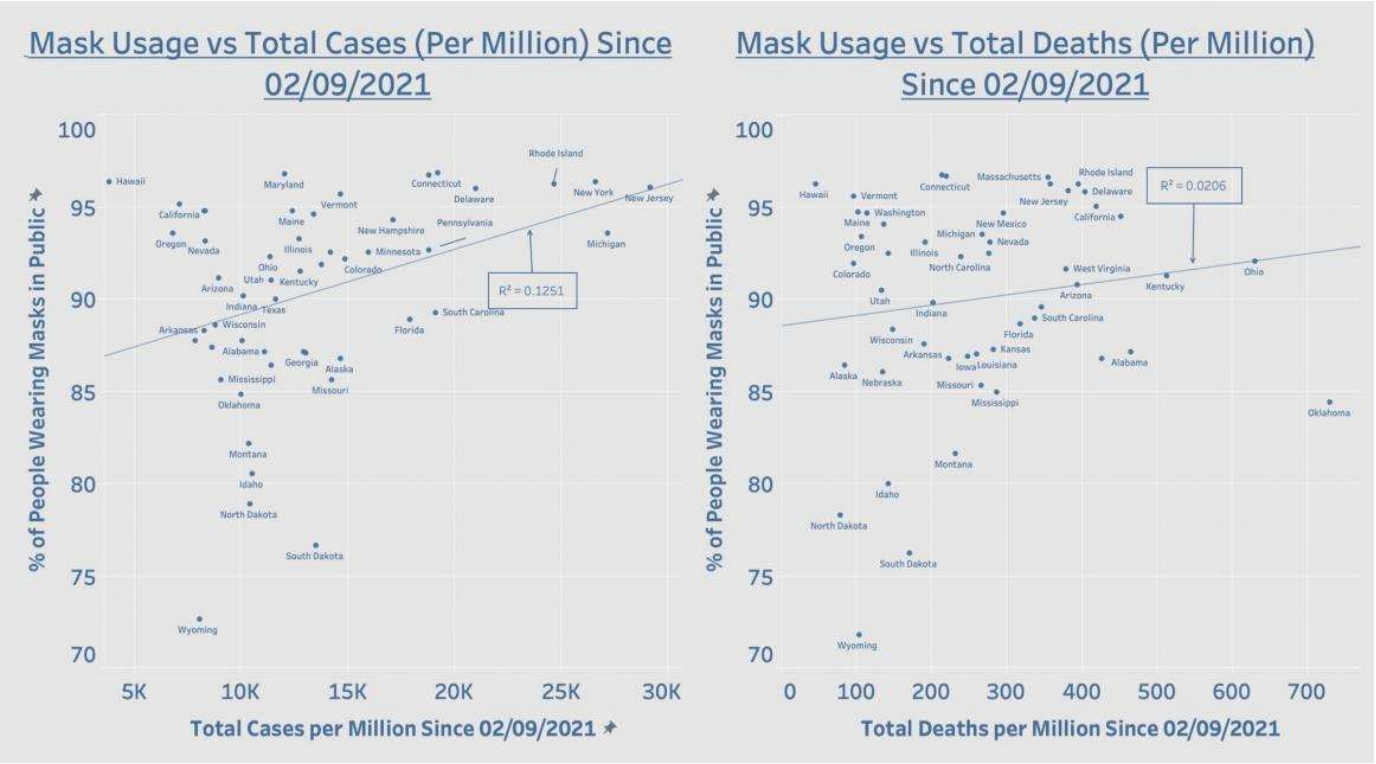
[22]

Source: Data on deaths (as of Apr. 28, 2021) and lockdown stringency (as of Apr. 28, 2021) from Our World in Data [23].

In light of a year's worth of data showing wildly different mortality and hospitalization outcomes for fifty states with fifty very different lockdown stringencies, as well as drastically different mortality outcomes for 166 countries with 166 different lockdown stringencies, one can only marvel that such a deadly and ineffective policy can be recommended by public health experts.

If the lockdowns failed to mitigate the spread of covid-19 in the United States just as in dozens of countries around the world—remember, the lockdowns fail without even taking their costs into account—it's possible that mask usage is the missing piece of the mitigation puzzle.

It wouldn't be fair to the reader to post quite literally hundreds of charts that show the exact opposite outcomes the media would have one expect after regions remove or institute mask mandates—Ian Miller [24] has done more work in this area than anybody else. It also wouldn't be fair to claim that mask mandates and mask usage are synonymous. However, based on reactions to states lifting their mask mandates, I don't think any proponent of mask wearing would seriously expect the same level of mask usage should mandates be lifted. Nevertheless, the claim that mask *usage* negatively correlates with cases and deaths is easily refuted with a quick look at the data. Given the data available, we'll again only be looking at the fifty states.

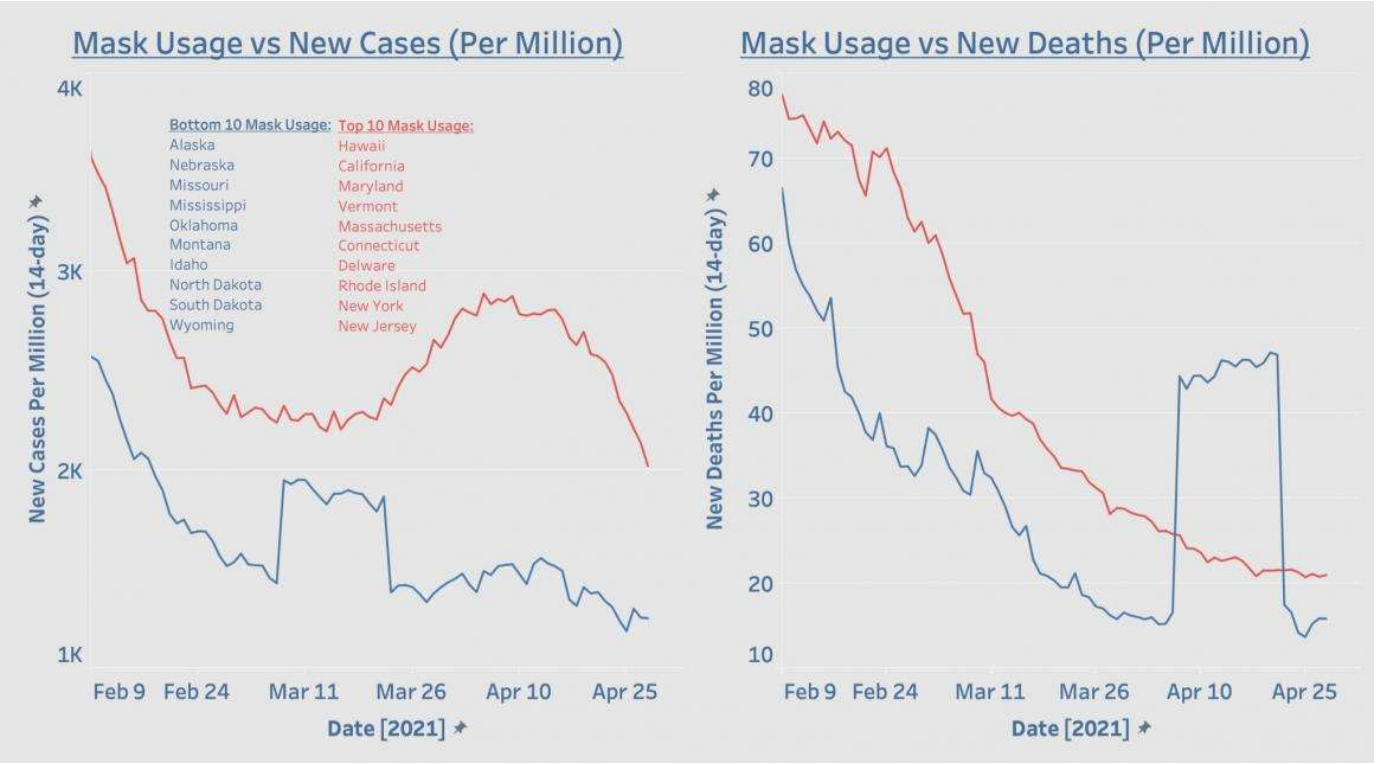


[25]

Source: Data for cases and deaths (as of Apr. 28, 2021) from the NYTimes Covid-19 Data Bot [17]. Mask usage data from the Delphi Group's COVIDcast [26].

Even though the trend lines travel in the exact opposite direction of what our public health experts would have us expect, the correlations are statistically meaningless. Note that the above chart only covers the 2.5-month period starting February 9, 2021, which is when COVIDcast began reporting mask usage numbers for each state. Therefore, the author included only the cases and deaths that occurred during this 2.5-month period. Despite this truncated time period, 2.5 months should have been more than enough to have exposed any sort of meaningful correlation between mask usage and both cases and deaths.

It is worth noting that Rhode Island and New York, each with some of the highest mask usage rates and lockdown stringencies in the country, are leading the pack with some of the largest case increases since early February. What is more, in the 2.5 months since early February the ten states with the highest rate of mask usage have been doing worse in both cases and deaths than the ten states with the lowest rate of mask usage.

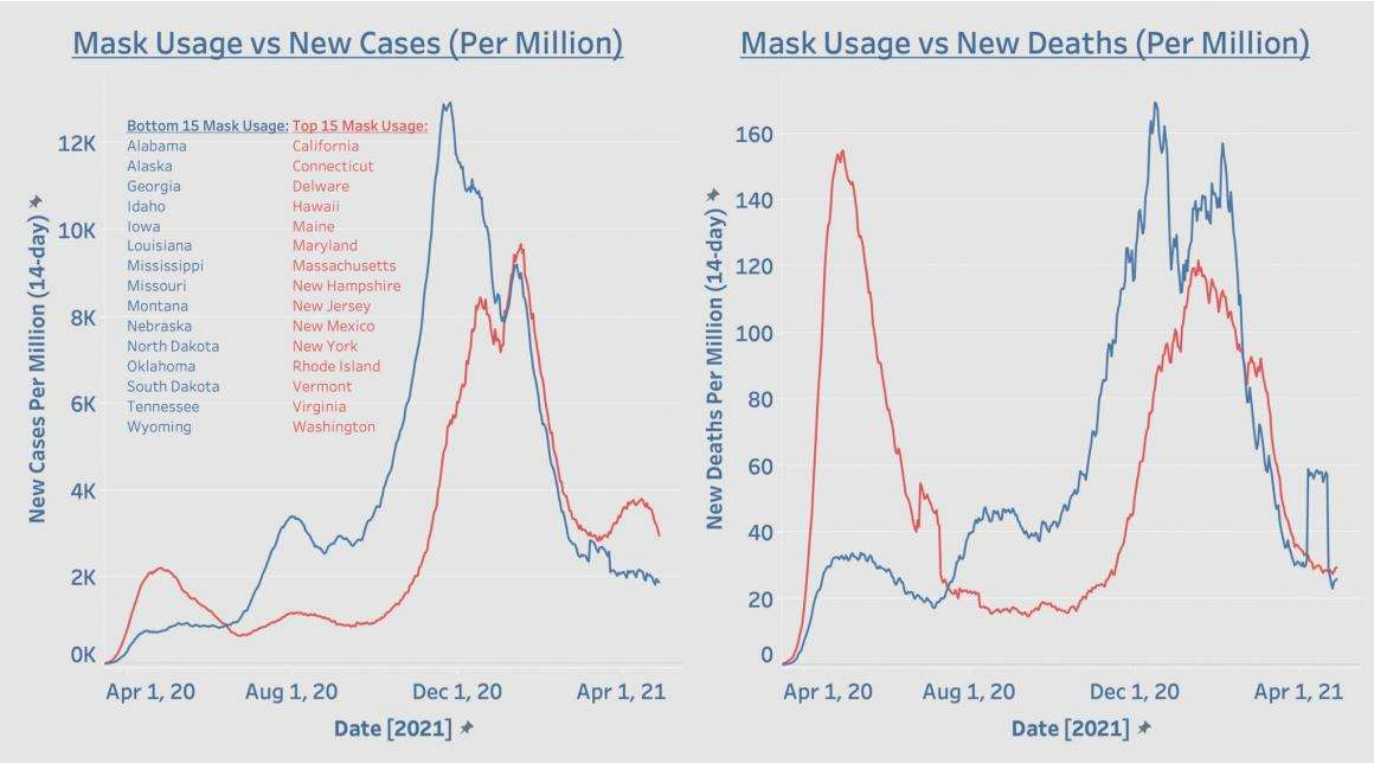


[27]

Source: Data for cases and deaths (as of Apr. 28, 2021) from the NYTimes Covid-19 Data Bot [17]. Mask usage data from the Delphi Group's COVIDcast [26].

Remember, we aren't measuring the amount of rules that simply say you have to wear a mask. What's being measured is the percentage of people actually *wearing* masks in public in each state. It's quite difficult to look at the trends depicted above and make the case not only for continuing mask mandates, but wearing masks at all.

Some may have an issue with the fact that the trends above only cover the couple of months since February. Let's assume, for the sake of a more complete picture, that mask usage trends were consistent for each state since the start of the pandemic. We can also expand our filter to the top and bottom fifteen states to account for some states' movement in and out of the top and bottom ten states.



[28]

Source: Data for cases and deaths (as of Apr. 28, 2021) from the NYTimes Covid-19 Data Bot [17]. Mask usage data from the Delphi Group's COVIDcast [26].

In terms of cases, from April to around mid-June, states with the lowest rates of mask usage were outperforming states with the highest rates of mask usage. This trend reversed from mid-June through mid-January and then reversed again in favor of states with the lowest rate of mask usage.

In terms of deaths, states with the lowest rates of mask usage outperformed states with the highest rates of mask usage from April until mid-July. From mid-July to mid-February, death trends were more favorable to states with the highest rates of mask usage, but after mid-February death trends again became more favorable to states with the lowest rates of mask usage. Again, if we are assuming fairly consistent rates of mask usage across the entire duration of the pandemic while also assuming that the science behind masks is truly settled, it's quite difficult to explain away any period of time in which states with the lowest rates of mask usage were outperforming states with the highest rates.

The supposedly settled science behind both lockdowns and mask mandates has always been in serious trouble but is even more so now. Completely leaving aside the incredible death toll of the lockdowns, their numerous social and psychological costs, the totalitarian denial of our most basic liberties, and the decimation of tens of thousands of small businesses, they would still be a miserable failure by nearly every covid-19 metric we have available. Though, to be fair, the lockdowns did make our cities quieter. But aside from that, the data continue to deny that either lockdowns or mask mandates are effective tools for mitigating the spread of covid-19.

Links
[1] https://mises.org/profile/anthony-rozmajzl
[2] https://mises.org/topics/bureaucracy-and-regulation
[3] https://twitter.com/tomselliott/status/1362048016560062466?s=20
[4] https://twitter.com/Breaking911/status/1381073146590208000?s=20
[5] https://twitter.com/BreitbartNews/status/1382818277399748611?s=20
[6] https://www.google.com/covid19/mobility/
[7] https://www.youtube.com/watch?v=Qp3gy_CLXho
[8] https://nypost.com/2021/04/14/cnn-staffer-tells-project-veritas-network-played-up-covid-19-death-toll-for-ratings/

[9] https://www.nytimes.com/2021/03/24/world/covid-coverage-by-the-us-national-media-is-an-outlier-a-study-finds.html
[10] https://www.nytimes.com/2021/03/18/briefing/atlanta-shootings-kamala-harris-tax-deadline-2021.html
[11] https://rationalground.com/lockdowns-pros-and-cons/
[12] https://collateralglobal.org/research
[13] http://thepriceofpanic.com
[14] https://www.bsg.ox.ac.uk/sites/default/files/2020-12/BSG-WP-2020-034-v2_0.pdf
[15] https://wallethub.com/edu/states-coronavirus-restrictions/73818
[16] https://cdn.mises.org/roz1.jpg
[17] https://github.com/nytimes/covid-19-data/blob/master/us-states.csv
[18] http://wallethub.com/edu/states-coronavirus-restrictions/73818
[19] https://pbs.twimg.com/media/EyfzUnXUcAIvi8a?format=jpg&name=large
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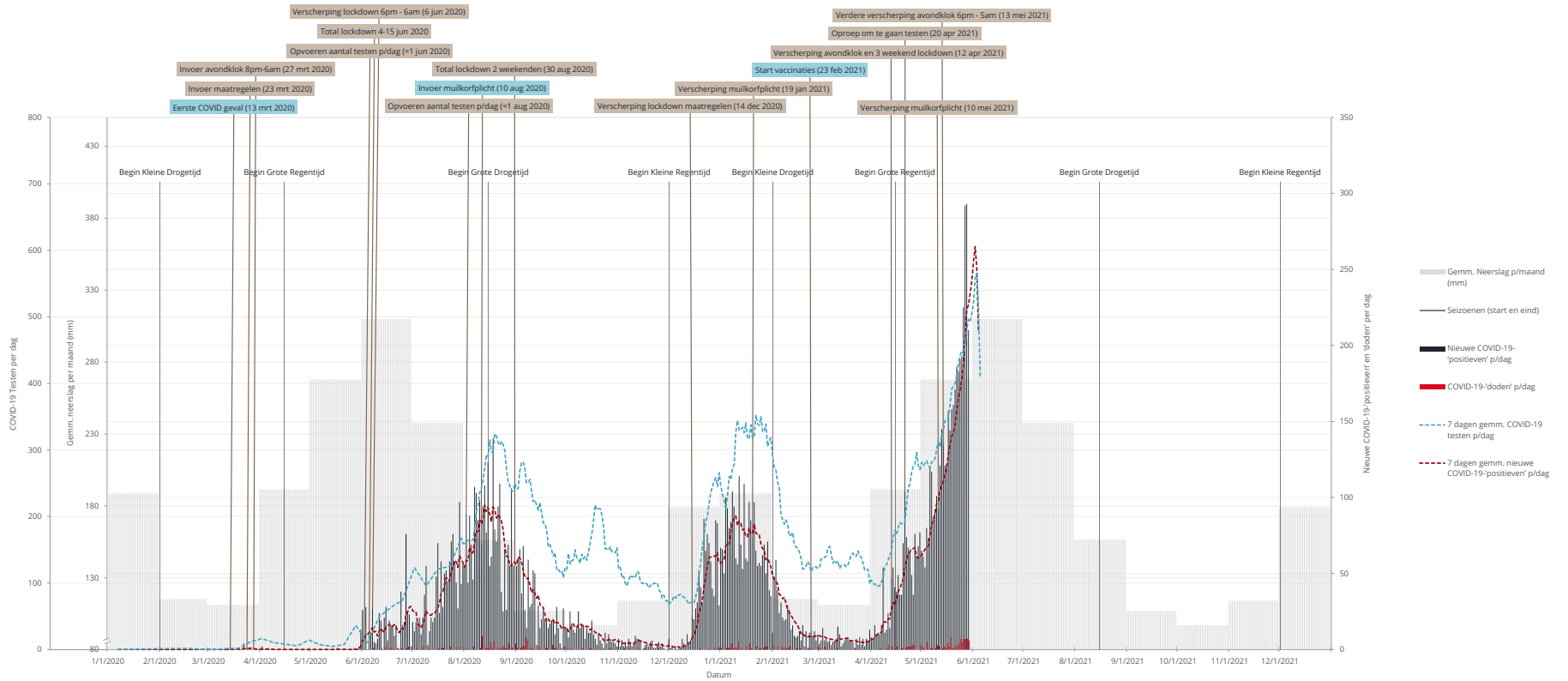
[22] https://cdn.mises.org/ar3.jpg
[23] https://github.com/owid/covid-19-data
[24] https://twitter.com/ianmSC/media
[25] https://cdn.mises.org/ar4.jpg
[26] http://delphi.cmu.edu/covidcast/
[27] https://cdn.mises.org/ar5.jpg
[28] https://cdn.mises.org/ar6.jpg

PRODUKTIE 6



Verloop COVID-19-'positieven' en 'doden' in Suriname vergeleken met de seizoenen en maatregelen (per 29 mei 2021)

De aantallen COVID-19-'positieven' en 'doden' zijn afkomstig van de WHO/PAHO en COVID-19.sr. De hoeveelheid neerslag is afkomstig van de Meteorologische Dienst, station Zorg en Hoop 1961-2005.

PRODUKTIE 10

PRODUKTIE 11

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REVIEW ARTICLE

WILEY

Reconciling estimates of global spread and infection fatality rates of COVID-19: An overview of systematic evaluations

John P. A. Ioannidis

Departments of Medicine, of Epidemiology and Population Health, of Biomedical Data Science, and of Statistics, and Meta-Research Innovation Center at Stanford (METRICS), Stanford University, Stanford, CA, USA

Correspondence
John P. A. Ioannidis, SPRC, 1265 Welch Road, Medical School Office Building Room X306, Stanford, CA 94305, USA.
Email: jioannid@stanford.edu

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Abstract

Background: Estimates of community spread and infection fatality rate (IFR) of COVID-19 have varied across studies. Efforts to synthesize the evidence reach seemingly discrepant conclusions.

Methods: Systematic evaluations of seroprevalence studies that had no restrictions based on country and which estimated either total number of people infected and/or aggregate IFRs were identified. Information was extracted and compared on eligibility criteria, searches, amount of evidence included, corrections/adjustments of seroprevalence and death counts, quantitative syntheses and handling of heterogeneity, main estimates and global representativeness.

Results: Six systematic evaluations were eligible. Each combined data from 10 to 338 studies (9-50 countries), because of different eligibility criteria. Two evaluations had some overt flaws in data, violations of stated eligibility criteria and biased eligibility criteria (eg excluding studies with few deaths) that consistently inflated IFR estimates. Perusal of quantitative synthesis methods also exhibited several challenges and biases. Global representativeness was low with 78%-100% of the evidence coming from Europe or the Americas; the two most problematic evaluations considered only one study from other continents. Allowing for these caveats, four evaluations largely agreed in their main final estimates for global spread of the pandemic and the other two evaluations would also agree after correcting overt flaws and biases.

Conclusions: All systematic evaluations of seroprevalence data converge that SARS-CoV-2 infection is widely spread globally. Acknowledging residual uncertainties, the available evidence suggests average global IFR of ~0.15% and ~1.5-2.0 billion infections by February 2021 with substantial differences in IFR and in infection spread across continents, countries and locations.

KEYWORDS
bias, COVID-19, global health, infection fatality rate, meta-analysis, seroprevalence

Highlights

- Six systematic evaluations have evaluated seroprevalence studies without restrictions based on country and have estimated either total number of people infected or aggregate infection fatality rates for SARS-CoV-2.
- These systematic evaluations have combined data from 10 to 338 studies (9-50 countries) each with partly overlapping evidence synthesis approaches.
- Some eligibility, design and data synthesis choices are biased, while other differing choices are defendable.
- Most of the evidence (78%-100%) comes from Europe or the Americas.
- All systematic evaluations of seroprevalence data converge that SARS-CoV-2 infection has been very widely spread globally.
- Global infection fatality rate is approximately 0.15% with 1.5-2.0 billion infections as of February 2021.

1 | INTRODUCTION

The extent of community spread of SARS-CoV-2 infection and the infection fatality rate (IFR) of COVID-19 are hotly debated. Many seroprevalence studies have provided relevant estimates. These estimates feed into projections that influence decision-making. Single studies create confusion, since they leave large uncertainty and unclear generalizability across countries, locations, settings and time points. Some overarching evaluations have systematically integrated data from multiple studies and countries.[1-6] These synthetic efforts probe what are typical estimates of spread and IFR, how heterogeneous they are, and what factors explain heterogeneity. An overview of these systematic evaluations comparing their methods, biases and inferences may help reconcile their findings on these important parameters of the COVID-19 pandemic.

2 | METHODS

2.1 | Eligible articles

Articles were eligible if they included a systematic review of studies aiming to assess SARS-CoV-2 seroprevalence; there were no restrictions based on country; and an effort was made to estimate either a total number of people infected or aggregate IFRs. Articles were excluded if they considered exclusively studies of particular populations at different risks of infection than the general population (eg only healthcare workers), if they focused on specific countries (by eligibility criteria, not by data availability), and if they made no effort to estimate total numbers of people infected and/or aggregate IFRs.

2.2 | Search strategy

Searches were updated until 14 January 2021 in PubMed, medRxiv and bioRxiv with 'seroprevalence [ti] OR fatality [ti] OR immunity [ti]' For feasibility, the search in PubMed was made more specific by adding '(systematic review OR meta-analysis OR analysis)'. Communication with experts sought potentially additional eligible analyses (eg unindexed influential reports).

2.3 | Extracted information

From each eligible evaluation, the following information was extracted:

1. Types of information included (seroprevalence, other)
2. Date of last search, search sources and types of publications included (peer-reviewed, preprints, reports/other)
3. Types of seroprevalence designs/studies included
4. Number of studies, countries, locations included
5. Seroprevalence calculations: adjustment/correction for test performance, covariates, type of antibodies measured, seroreversion (loss of antibodies over time)
6. Death count calculations: done or not; adjustments for over- or under-counting, time window for counting COVID-19 deaths in relationship to seroprevalence measurements
7. Quantitative synthesis: whether data were first synthesized from seroprevalence studies in the same location/country/other level; whether meta-analyses were performed across locations/countries and methods used; handling of heterogeneity, stratification and/or regression analyses, including subgroups

8. Reported estimates of infection spread, under-ascertainment ratios (total/documented infections) and/or IFR
9. Global representativeness of the evidence: proportion of the evidence (weight, countries, studies or locations, depending on how data synthesis had been done) from Europe and North America (sensitivity analysis: Europe and America)

2.4 | Comparative assessment

Based on the above, the eligible evaluations were compared against each other with focus on features that may lead to bias and trying to decipher the direction of each bias.

3 | RESULTS

3.1 | Eligible evaluations

Nine potentially eligible articles were retrieved[1-3,5-10] And four were rejected (Figure 1).[7-10] One more eligible report[4] was identified from communication with experts. The six eligible evaluations are named after their first authors or team throughout the manuscript.

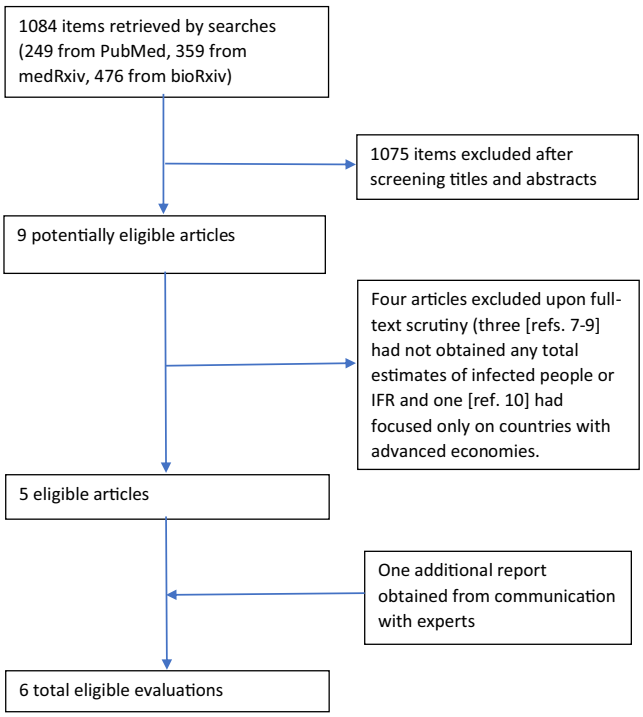

FIGURE 1 Flow diagram

3.2 | Information used

Five evaluations included only seroprevalence studies (Table 1). Meyerowitz-Katz also included non-serological and modelling papers; summary IFR was smaller in the seroprevalence studies (0.60% vs 0.84% in others). The six evaluations differed modestly in dates of last search (range, 6/16/2020-9/9/2020) and in sources searched. Given that few studies outside of Europe and Americas were released early, evaluations with earlier searches have a more prominent dearth of low-IFR studies from countries with younger populations and fewer nursing home residents.

Eligibility criteria varied and were sometimes unclear or left room for subjectivity. Consequently, eligible studies varied from 10 to 348 and countries covered with eligible data varied from 9 to 50. Two evaluations[1,4] excluded studies in overtly biased ways, leading to inflated IFR estimates.

Specifically, Meyerowitz-Katz excluded one study with low-IFR[5] alluding that the study itself 'explicitly warned against using its data to obtain an IFR'[1]; as co-investigator of the study, both myself and my colleagues are intrigued at this claim. They also excluded two more studies with low-IFR alluding that it 'was difficult to determine the numerator (ie number of deaths) associated with the seroprevalence estimate or the denominator (ie population) was not well defined',[1] while one even presented IFR estimates in its published paper. Another excluded paper[11] tabulated several seroprevalence studies with median IFR = 0.31%, half the Meyerowitz-Katz estimate.

The Imperial College COVID-19 Response Team (ICCRT) excluded studies with <100 deaths at the sero-survey mid-point.[4] This exclusion criterion introduces bias since number of deaths is the numerator in calculating IFR. Exclusion of studies with low numerator excludes studies likely to have low IFR. Indeed, five of six excluded studies with <100 deaths (Kenya, LA County, Rio Grande do Sul, Gangelt, Scotland)[12-16] have lower IFR than the 10 ICCRT-included studies; the sixth (Luxembourg)[17] is in the lower range of the 10 ICCRT-included studies.

The six evaluations varied on types of populations considered eligible. Table 2 summarizes biases involved in each study population type. General population studies are probably less biased, provided they recruit their intended sample. Conversely, studies of healthcare workers,[18] other high-risk exposure workers and closed/confined communities may overestimate seroprevalence; these studies were generally excluded, either upfront (5/6 evaluations) or when calculating key estimates (Bobrovitz). Other designs/populations may be biased in either direction, more frequently towards underestimating seroprevalence.[19-26] Three evaluations (Meyerowitz-Katz, ICCRT, O'Driscoll) were very aggressive with exclusions.

TABLE 1 Key features for eligible systematic data syntheses

Features	Meyerowitz-Katz	Rostami	Bobrovitz	Imperial college COVID-19 response team	Ioannidis	O'Driscoll
Types of information included	SP, non-serological and modelling studies	SP studies	SP studies	SP studies	SP studies	SP studies
Last search	16 June	14 August	28 August	Unclear	9 September	Unclear (1 September?)
Search sources	PubMed, preprints (medRxiv, SSRN), Google, Twitter searches, government agency reports eligible	PubMed, Scopus, EMBASE, medRxiv, bioRxiv, research reports eligible	MEDLINE, EMBASE, Web of Science, and Europe PMC, Google, communication with experts	SeroTracker searches (see Bobrovitz)	PubMed (LitCOVID), medRxiv, bioRxiv, Research Square, national reports, communication with experts for additional studies	Unclear
Types of SP studies included	Excluded targeted populations with selection bias, also four other studies[a]	Excluded at-risk populations (eg HCW), known diseases (eg dialysis, cancer)	All studies included if they reported on sample, date, region and SP estimate	Studies with defined sampling framework, defined geographic area, with availability of test performance, preferentially validation done as part of the study (not just by manufacturers), >100 deaths at SP study mid-point[b]; excluded healthcare workers, symptoms of COVID-19, self-referral or self-selection, narrow age range, confined settings, clinical samples	General population or approximations (including blood donors, excluding high risk, eg HCW, communities), sample size >500, area with population >5000	Unclear, but eventually it includes some general population studies, some blood donors and some hospital samples
Number of studies, countries, locations	24-27 studies[c], of which 16 serological from 14 countries	107 data sets from 47 studies from 23 countries	338 studies (184 from general population) from 50 countries (36 from general population)[d]	10 studies (six national, four subnational), nine countries[e]	82 estimates, 69 studies, 51 locations, 36 countries (main analysis at the location level)	25 studies from 20 countries (only 22 national representing 16 countries used in the ensemble model)
Studies published in peer-review journals at the time of the evaluation	1/16	61/107	4/40 included in final analysis of under-ascertainment ratio	5/10	35/82	6/20 countries

Abbreviations: HCW, healthcare workers; IFR, infection fatality rate; SP, seroprevalence.

[a]One study (LA County)[12] with very low IFR was excluded with the justification that it 'explicitly warned against using its data to obtain an IFR'; as a co-investigator of the study, both myself and my colleagues are intrigued at the rationale for exclusion; in the publication of the study in JAMA,[12] we did list limitations and caveats, as it is appropriate for any seroprevalence study to do; excluding studies that are honest to discuss limitations would keep only the worst studies that discuss no limitations. Two other studies with low IFR were excluded as well. One was done in Rio Grande do Sul[13] where its authors even report IFR estimates in their paper (0.29%, 0.23%, 0.38% in the three rounds of the serosurvey); the other was done in Boise,[85] where its authors properly discuss limitations but an approximation of IFR is possible; even if not perfectly accurate, it is certainly lower than the IFR estimates included in the Meyerowitz-Katz meta-analysis. For the fourth excluded study,[11] the justification offered for its exclusion is that it 'calculated an IFR, but did not allow for an estimate of confidence bounds'.[1] However, this study presents results of a New York study that Meyerowitz-Katz did include in their meta-analysis. Of note, that fourth study[11] also presents a cursory review of seroprevalence studies arriving at a median IFR = 0.31%, half of the summary estimate of Meyerowitz-Katz.; [b]Clear bias introduced since number of deaths is the numerator itself in the calculation of IFR, and exclusion of studies with low numerator is thus excluding studies likely to have low IFR; [c]Different numbers provided by the authors for total studies in abstract (n = 24), text of the paper (n = 25), tabulated studies (n = 27) and forest plot studies (n = 26); [d]39 estimates from 17 countries used in main calculation of median under-ascertainment ratio (N. Bobrovitz, personal communication); [e]One of the 10 included studies violates the eligibility criterion of the investigators having validated themselves the antibody test used; the ICCRT included this study invoking validation data for the same antibody kit done by a different team in a study in a completely different setting and continent (San Francisco); based on this rationale, perhaps many other studies could have been included, if the same violation of the eligibility criteria was tolerated. The included study was an Italian survey[30] which had only been released in the press with a preliminary report at the time of the ICCRT evaluation and which included crude results on only 64 660 of the intended 150 000 participants (missingness 57%). Its inferred IFR estimate (2.5%) is an extreme outlier, as it is 2- to 20-fold larger than other typical estimates reported from numerous European countries. Moreover, that IFR estimate even matches/exceeds case fatality rates, and thus, it is simply impossible. It is widely accepted that IFR must be several times smaller than case fatality rate, even in locations with substantial testing. Italy had very limited testing in the first wave and modest testing in the second wave. One estimate suggests that the number of infections in Italy at the peak of the first wave was 12 times more than the number of documented cases; that is, the IFR would be more than an order of magnitude lower than the case fatality rate.[31]

TABLE 2 Direction of potential bias in studies with different types of populations

Type of sampling	Direction of bias
General population (entire population or design for representative sample)	Depends on characteristics of individuals who cannot be reached and/or decline participation. If they are more likely to be more disadvantaged (eg have no address/phone/e-mail) and thus also at higher risk of infection, SP may be underestimated. Potential for bias is more prominent when non-response/non-participation is larger. Institutionalized populations and homeless people are typically not included, and these populations often have very high infection rates[19,20] ; thus, SP is underestimated
Convenience sample (including self-referral and response to adverts)	Bias could be in either direction. Volunteer bias is common and would tend to recruit more health-conscious, low-risk individuals,[21] leading to SP underestimation. Conversely, interest to get tested because of worrying in the presence of symptoms may lead to SP overestimation
Blood donors	Bias could be in either direction, but SP underestimation is more likely, since blood donors tend to be more health-conscious and thus more likely to avoid also risky exposures. An early classic assessment[22] described blood donors as 'low-risk takers, very concerned with health, better educated, religious, and quite conservative'—characteristics that would lead to lower infection risk. In countries with large shares of minorities (eg USA and UK), minorities are markedly under-represented among blood donors.[23,24] For example, in the USA, donation rates are 37%-40% lower in blacks and Hispanics versus whites[23] and in the UK, donation rates range from 1.59 per 1000 among Asian Bangladeshi origin, compared to 22.1 per 1000 among white British origin.[24] These minorities were hit the most by COVID-19. In European countries, donations are lower in low-income and low-education individuals[25,26] ; these are also risk factors for COVID-19 infection. Bobrovitz[3] found median seroprevalence of 3.2% in blood donor studies versus 4.1% in general community/household samples (risk ratio 0.80 in meta-regression). SP may be overestimated if blood donation is coupled to a free COVID-19 test in a poor population (as in the case of a study in Manaus, Brazil)
Clinical residual samples and patients (eg dialysis, cancer, other)	Bias could be in either direction, but SP underestimation is more likely since patients with known health problems may be more likely to protect themselves in a setting of a pandemic that poses them at high risk. Conversely, repeated exposure to medical facilities may increase risk. Demographic features and socio-economic status may also affect the size and direction of bias. Bobrovitz[3] found median seroprevalence of 2.9% in studies of residual samples versus 4.1% in general community/household samples (risk ratio 0.63 in meta-regression). Hospital visitors' studies had even lower seroprevalence (median 1.4%)
Healthcare workers, emergency response, other workers with obvious high risk of exposure	Bias very likely to lead to SP overestimation compared with the general population, because of work-related contagion hazard; however, this may not always be the case (eg most infections may not happen at work) and any increased risk due to work exposure sometimes may be counterbalanced by favourable socio-economic profile for some healthcare workers (eg wealthy physicians). Bias may have been more prominent in early days of the pandemic, especially in places lacking protective gear. Across eight studies with data on healthcare workers and other participants, seroprevalence was 1.74-fold in the former.[3]
Other workers	Bias could be in either direction and depends on work experience during the pandemic period and socio-economic background; for example, SP may be underestimated compared with the general population for workers who are wealthy and work from home during the pandemic and overestimated for essential workers
Communities (shelters, religious, other shared-living)	Likely very strong bias due to high exposure risk leading to SP overestimation compared with the general population. Some of these communities were saturated with very high levels of infection very early.[19,20]

Note: Abbreviations: SP, seroprevalence.

ICCRT had the most draconian exclusion criteria, excluding 165/175 identified seroprevalence studies. However, ICCRT actually dropped many general population studies (for various reasons), but included two blood donor studies[27,28] (out of many such) and one New York study[29] with convenience samples of volunteers recruited while entering grocery stores and through an in-store flyer. The latter inclusion goes against the stated ICCRT eligibility criteria where self-selection is reason for exclusion. The New York study[29] had high IFR (from the worst-hit state in the first wave). The preliminary press-released report from an Italian general population survey[30] was included in violation of ICCRT eligibility criteria[4] that a study should have performed its own antibody test validation; ICCRT 'salvaged' the Italian study by transporting validation data from another study in San Francisco. The Italian study report[30] showed data on only 64 660 of the intended 150 000 participants (missingness 57%). Its inferred IFR estimate (2.5%) is an extreme outlier (2- to 20-fold larger than other reported European estimates) and simply impossible: it matches/exceeds case fatality rates despite probably major under-ascertainment of infections in Italy.[31]

Finally, the six evaluations differed markedly on how many included seroprevalence estimates came from peer-reviewed publications (journal articles listed in the references) at the time of the evaluation: from only one peer-reviewed estimate in Meyerowitz-Katz to 61 in Rostami. Some included

TABLE 3 Adjustments and corrections for seroprevalence and death counts

Features	Meyerowitz-Katz	Rostami	Bobrovitz	Imperial College COVID-19 response team	Ioannidis	O'Driscoll
Adjustment of SP for test performance	Unclear selection rule	Unclear selection rule	Yes (Bayesian)	Yes	Yes, when done by authors of SP study	Yes (24/25 studies)
Adjustment of SP for confounders	Unclear selection rule	Unclear selection rule	Unclear selection rule	Unclear selection rule	Selecting most fully adjusted SP estimated	Unclear selection rule
Other SP correction	No	No	No	Seroreversion	Type of antibodies[a]	Seroreversion, in secondary analysis
Death count adjustments	No adjustments	Deaths not assessed	Deaths not assessed	No adjustments	No adjustments	No adjustments
Time window for death counts	10 d after completion of SP study	Deaths not assessed	Deaths not assessed	Distributional (truncated Gaussian and beta), mean 18.3 d from onset to seroconversion, 19.8 d from onset to death	7 d after mid-point of SP survey or as chosen by its authors	Distributional (gamma), mean 10 d from onset to seroconversion, 20 d from onset to death

Abbreviations: d, days; IFR, infection fatality rate; SP, seroprevalence.

[a]one-tenth adjustment per each not tested antibody (IgG, IgM, IgA).

seroprevalence estimates that came from preprints/reports published in peer-reviewed journals by 2/2021; final publications could have minor/modest differences versus preprints/reports. Even journal-published estimates may get revised; for example, a re-analysis increased Indiana seroprevalence estimates by a third.[32]

3.3 | Seroprevalence and death calculations

Three evaluations[3,4,6] routinely adjusted for test performance, one[5] adjusted for test performance when the authors of the studies had done so, and two were unclear (Table 3). Depending on test sensitivity/specificity, lack of adjustment may inflate or deflate seroprevalence. Ioannidis selected the most fully adjusted seroprevalence estimate, when both adjusted and unadjusted estimates existed; other evaluations were unclear on this issue. Ioannidis corrected the seroprevalence upward when not all three types of antibodies (IgG, IgM, and IgA) were assessed. ICCRT and O'Driscoll considered seroreversion adjustments.

Rostami and Bobrovitz did not collect death counts to estimate IFR. The other four evaluations did not systematically adjust death counts for under- or over-counting. Finally, ICCRT and O'Driscoll used distributional approaches on the time window for counting deaths (with means between seroconversion and death differing by 1.5 and 10 days, respectively), Ioannidis counted deaths until 7 days after the survey mid-point (or the date survey authors made a strong case for), and Meyerowitz-Katz counted deaths up until 10 days after survey end.

3.4 | Quantitative synthesis, heterogeneity and main estimates

The six evaluations differed in quantitative synthesis approaches with implications for the main results (Table 4).

Meyerowitz-Katz used random effects meta-analysis of 26 IFRs calculating a summary estimate despite extreme between-study heterogeneity (I^2 = 99.2%). Such extreme heterogeneity precludes obtaining meaningful summary estimates. Estimates from the same country/location were not combined first, and two multiply-counted countries (Italy and China) have high IFRs entered in calculations. Meta-analysis limited to seroprevalence studies yielded slightly lower summary IFR (0.60% vs 0.68%), but extreme between-study heterogeneity persisted (I^2 = 99.5%); thus, summary estimates remained meaningless. Extreme between-study heterogeneity persisted also within three risk-of-bias categories (I^2 = 99.6%, 98.8% and 94.8%, respectively), within Europe and within America. There was no between-study heterogeneity for four Asian estimates, but none came from

TABLE 4 Quantitative synthesis approaches, stratification and/or regression and main estimates

	Meyerowitz-Katz	Rostami	Bobrovitz	Imperial College COVID-19 response team	Ioannidis	O'Driscoll
Quantitative synthesis	26 IFR estimates combined at one step with D-L RE model, $I^2 = 99.4\%$	First step 107 SP estimates combined separately for each country with D-L RE model, then per region. Also D-L RE for all 107 estimates, $I^2 = 99.7\%$	Median SP calculated overall and per subgroup of interest.	Log-linear model for pooling age-stratified IFR, then age-stratified estimates extrapolated to the age structure of populations of typical countries	First step, sample size-weighted summary of SP per location; then median estimated across locations	The ensemble model eventually models age-stratified IFR in a total of 45 countries with available age-stratified death counts, but data are used as input from only 16 countries that have IFR data with some age stratification
Stratification and/or regression	Subgroup analyses per continent, month of publication, modelling versus serological and risk of bias	Subgroup analyses per age, gender, type of population, serological method, race/ethnicity, income, human development index, latitude/longitude, humidity, temperature, days from onset of pandemic; also RE meta-regressions	Subgroup analyses per GBD region, scope (national, regional, local, sublocal), risk of bias, days since 100th case (also explored in meta-regressions); RE inverse variance meta-analysis of prevalence ratios for demographics (age, sex, race, close contact, HCW status) with $I^2 = 85.1\%$-99.4% per grouping factor	Focus on age-strata, also IFR estimates with and without seroreversion, and (for some countries) excluding nursing home deaths	Separate analyses for age <70 years; also subgroup analyses according to level of overall mortality in the location	Focus on age-strata; also per sex/gender and per country
Main estimates	Summary IFR 0.68 (95% CI-0.53%-0.82%), 0.60 when limited to serological studies	263.5 million exposed/infected at the time of the study based on the pooled SP from all 107 data sets; when estimated per region the total is 641 million[a]	643 million infected as of 17 November, based on estimated median under-ascertainment factor of 11.9 (using 9 d before study end date for PCR counts)[b]	Overall IFR: LIC 0.22 (0.14, 0.39), LMIC) 0.37 (0.25, 0.61), UMIC 0.57 (0.38, 0.92), HIC 1.06 (0.73, 1.64)	Over 500 million infected as of September 12 (vs 29 million documented cases) globally; median IFR 0.23% in the available studies (0.09% in locations with <118 deaths/million), 0.20% in locations with 118-500 deaths/million, 0.57% in locations with >500 deaths/million	5.27% of the population of the 45 modelled countries had been infected by 1 September

Abbreviations: IFR, infection fatality rate; RE, random effects; SP, seroprevalence.

[a]In millions: Europe+North America 47, East+South-East Asia 47, Latin America 9, South America 6, Sub Saharan Africa 62, Central and South Asia 446, North Africa and West Asia 24; [b]Median under-ascertainment was 14.5 overall based on 125 study estimates and 11.9 in national estimates, 15.7 in regional estimates and 24.0 in local estimates.

seroprevalence data and their IFR estimate (0.46%) is far higher than many subsequent Asian studies (outside Wuhan) using seroprevalence data[5] instead of modelling.

Rostami also performed random effects meta-analyses but more appropriately combined at a first step seroprevalence data from studies in the same country, and in the same region, a summary estimate across all 107 estimates in all countries was also obtained. The step-wise approach avoids the Meyerowitz-Katz analysis flaw. However, seroprevalence estimates may still vary extremely even within the same location, for example if done at different times. Moreover, the main estimate of the evaluation ('263.5 million exposed/infected at the time of the study') extrapolated to the global population the pooled estimate from all 107 data sets. The more appropriate estimate is a sum of the infected per country, or at least per region. Actually, the authors did calculate numbers of people exposed/infected per world region. The sum was 641 million, 2.5-fold larger. Moreover, these numbers did not reflect 'the time of the study': the 107 seroprevalence studies were done 2-6 months before the Rostami evaluation was written.

Bobrovitz calculated medians (overall and across several subgroups of studies), and Ioannidis calculated sample size-weighted means per location and then medians across locations. Their approaches avoid multiple counting of locations with many estimates available. Bobrovitz also performed random effects inverse variance meta-analysis of prevalence ratios for diverse demographics (age, sex, race, close contact, healthcare workers). The approach is defendable, since prevalence ratios were calculated within each study, but still very large between-study heterogeneity existed ($I^2 = 85.1\%$-99.4% per grouping factor) making results tenuous. Bobrovitz and Ioannidis reach congruent estimates for total number infected globally (643 million by November 17 and at least 500 million by September 12, respectively) with under-ascertainment ratios of 11.9 in November and 17.2 in September. Only the latter evaluation calculated IFRs (0.23% overall; 0.05% for those <70 years old).

ICCRT and O'Driscoll focused on age-stratified estimates. ICCRT extrapolated age-stratified estimates to the age structure of populations of typical countries, obtaining separate overall IFR estimates for low-income countries (0.22%), lower-middle–income countries (0.37%), upper-middle–income countries (0.57%) and high-income countries (1.06%). O'Driscoll made extrapolations to 45 countries estimating 5.27% of their population infected by 1 September.

3.5 | Global representativeness

Seroprevalence data lacked global representativeness. 72%-91% of the seroprevalence evidence came from Europe and North America (78%-100% from Europe or Americas)

TABLE 5 Global representativeness

	Meyerowitz-Katz	Rostami	Bobrovitz	Imperial College COVID-19 response team	Ioannidis	O'Driscoll[b]
Estimates (countries)[a]						
Europe	11 (11)	52 (13)	33 (13)	8 (7)	22 (21)	13 (13)
North America	3 (1)	22 (1)	1 (1)	1 (1)	15 (2)	1 (1)
Latin America	1 (1)	17 (2)	3 (1)	1 (1)	3 (3)	1 (1)
Asia	1 (1)	14 (5)	2 (1)	0 (0)	10 (9)	0 (0)
Africa	0 (0)	2 (2)	1 (1)	0 (0)	1 (1)	1 (1)
Oceania	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)
Information from Europe and North America	91% of weight	72% of data sets	85% of data sets (82% of countries)	90% of data sets	73% of location estimates	87% of countries
Information from Europe and America	98% of weight	85% of data sets	93% of data sets (87% of countries)	100% of data sets	78% of location estimates	94% of countries

[a]Geographic location of estimates (countries) included in main calculations.; [b]The extrapolated 45 countries on which age-stratified IFR estimates are obtained also include countries outside the regions that have at least one country represented (Pakistan, Philippines, Bangladesh, Indonesia, China, Thailand, South Korea, Japan) even though not directly measured in any of them.

(Table 5). Lack of representativeness was most prominent in Meyerowitz-Katz (only one estimate from Asia, none from Africa), ICCRT (no estimates from Asia or Africa) and O'Driscoll (only one estimate from Africa, no estimate from Asia). However, ICCRT extrapolated to all countries globally and O'Driscoll extrapolated to 45 countries including eight in Asia.

4 | DISCUSSION

This overview of six systematic evaluations of global spread and/or IFR of SARS-CoV-2 utilizing seroprevalence data highlights differences in methods, calculations and inferences. Several choices made by some evaluations led to bias. Other choices are defendable and reveal some unavoidable variability on how evidence on these important questions should be handled.

Choices that led to biased inflated IFR estimates are the inclusion of modelling estimates, inappropriate exclusion of low-IFR studies despite fitting stated inclusion criteria of the evaluators, inappropriate inclusion of high-IFR studies despite not fitting stated inclusion criteria, and using low death counts as exclusion criterion. Two evaluations (Meyerowitz-Katz and ICCRT) suffered multiple such problems each. These biases contributed to generate inflated and, sometimes, overtly implausible results. These two evaluations also narrowly selected very scant evidence (16 and 10 studies, including only one and five peer-reviewed articles, respectively), while hundreds of seroprevalence estimates are available.

Differences in types of study designs and populations considered eligible may be defended with various arguments by each evaluator. Studies of healthcare workers were consistently excluded. No consensus existed on studies of blood donors, clinical samples, workers at no obvious high-risk occupations and various convenience samples; these designs have variable reliability. Reliability increases with careful adjustment for sampling, demographics and other key factors and when missing data are limited. General population sampling is theoretically best, but general population studies may still suffer large bias from selective missingness. Unreachable individuals, institutionalized people and non-participating invitees are typically at higher infection risk; if so, some general population studies may substantially underestimate seroprevalence (overestimate IFR). For example, Meyerowitz-Katz included a Danish government survey press release[33] where only 1071 of 2600 randomly selected invitees participated (missingness 59%); the estimated IFR (0.79%) is probably substantially inflated.[6,28]

Differences may also ensue from seroprevalence adjustments for test performance and other factors.[34,35] Sometimes the change in estimated seroprevalence is substantial.[36-38] Special caution is needed with low seroprevalence.[39] When not all types of antibodies are assessed, a correction may also be useful. Adjustment for test performance may seemingly suffice. However, control samples used to estimate test sensitivity come from PCR-tested diagnosed patients, while missed diagnoses typically reflect asymptomatic or less symptomatic patients not seeking testing. Sensitivity may be much lower in these people, as many develop no or low-titre antibodies.[40,41] Seroreversion has a similar impact. Preliminary evidence suggests substantial seroreversion.[29,42-45] For example, among healthcare personnel, 28.2% seroreverted in 2 months (64.9% in those with low titres originally).[45] Only ICCRT and O'Driscoll considered corrections for seroreversion, but still did not allow for high seroreversion. All these factors would result in underestimating seroprevalence (overestimating IFR).

Both over- and under-counting of COVID-19 deaths (the IFR numerator) may exist,[46,47] varying across countries with different testing and death coding. Correction of COVID-19 death counts through excess deaths is problematic. Excess reflects both COVID-19 deaths and deaths from measures taken.[46-49] Year-to-year variability is substantial, even more so within age-strata. Comparison against averages of multiple previous years is naïve, worse in countries with substantial demographic changes. For example, in the first wave, an excess of 8071 deaths (SMR 1.03, 95% CI 1.03-1.04) in Germany became a deficit of 4926 deaths (SMR 0.98, 95% CI 0.98-0.99) after accounting for demographic changes.[50] The exact timepoint when deaths are counted may affect IFR calculations when surveys happen while many deaths are still accruing. All evaluations that counted deaths allowed for greater time for death to occur than for seroconversion, but Meyerowitz-Katz used a most extreme delay, considering deaths until 10 days after survey end. Surveys take from one day to over a month; thus, inferred sampling-to-death delay may occasionally exceed 6 weeks. Meyerowitz-Katz defends this choice also in another paper[10] choosing 4 weeks after the serosurvey mid-point. However, the argument (accounting for death reporting delays) is weak. Several situational reports plot deaths according to date of occurrence rather than date of reporting anyhow.[51] Moreover, infection-to-death time varies substantially and may be shorter in developing countries where fewer people are long-sustained by medical support.

Some quantitative synthesis approaches were problematic, for example calculating summary estimates despite $I^2 > 99\%$ or no data combination within the same country/location before synthesis across countries/locations. Another generic problem with meta-analysis of such data is that it penalizes better studies that allow more appropriately for uncertainty in estimates (eg by accounting for test performance and adjusting for important covariates). Studies with less rigorous or no adjustments may have narrower CIs (smaller variance, thus larger weight).[5] Finally, for IFR meta-analysis, studies

with few deaths may have higher variance (lower weight) and these studies may have the lowest IFR.

Age stratification for IFR estimation and synthesis is a reasonable choice to reduce between-study heterogeneity driven by steep COVID-19 death risk age gradient.[52] However, both analyses[4,6] that capitalized on granular age stratification made tenuous extrapolations to additional countries from thin or no data. ICCRT lacked seroprevalence data on low-income and lower-middle–income countries (~half the global population); upper-middle–income countries (~35% of global population) were only represented by one estimate from Brazil assuming IFR = 1%, exceeding twofold to fivefold other peer-reviewed estimates from Brazil.[13,53] Estimates used from high-income countries included an impossible Italian estimate (IFR = 2.5%)[30] and mostly non–peer-reviewed data. O'Driscoll was more careful, but still some IFR extrapolations appear highly inflated versus data from subsequently accrued seroprevalence studies. Their ensemble model assumed highest IFR in Japan (1.09%) and lowest in Kenya (0.09%) and Pakistan (0.16%). Currently, available seroprevalence studies from these countries show markedly lower IFR estimates: =<0.03%,[54-56] =<0.01%[14] and 0.04%-0.07%,[57,58] respectively. In Japan, infections apparently spread widely without causing detectable excess mortality.[54] In Kenya, under-ascertainment compared with documented cases was ~1000-fold.[14] While some COVID-19 deaths are certainly missed in Africa, containment measures are more deadly.[59]

All six evaluations greatly over-represented Europe and America. Only two (Rostami and Ioannidis) included meaningful amounts of data from Asia and Africa (still less than their global population share) in main estimate calculations. Currently, extensive data suggest high under-ascertainment ratios in Africa and many Asian countries[5,14,54-61] and thus much lower IFR in Asia (outside Wuhan) and Africa than elsewhere.

Quality of seroprevalence studies varies. Risk-of-bias assessments in prevalence studies are difficult. There are multiple risk-of-bias scales/checklists,[62-65] but bias scores do not translate necessarily to higher or lower IFR estimates, while assessors often disagree in scoring (Appendix S1).

Acknowledging these caveats, four of the six evaluations largely reach congruent estimates of global pandemic spread. O'Driscoll estimated 5.27% of the population of 45 countries had been infected by 1 September 2020, that is 180 million infected among 3.4 billion. Excluding China, the proportion of population infected among the remaining 44 countries would be ~9%, likely >10% after accounting for seroreversion. Countries not included among the 45 include some of the most populous ones with high infection rates (India, Mexico, Brazil, most African countries). Therefore, arguably at least 10% of the non-China global population (ie at least 630 million) would be infected as of 1 September. This is very similar to the Ioannidis (at least 500 million infected as of 12 September) and Rostami (641 million infected by summer, when numbers are added per region) estimates. The Bobrovitz estimate (643 million infected as of 17 November) should be increased substantially given that only 2 of 17 countries informing the calculated under-ascertainment ratio were in Asia or Africa, continents with much larger under-ascertainment ratios. National surveys in India actually estimated 60% seroprevalence in November in urban areas.[66] Therefore, probably infected people globally were ~1 billion (if not more) by 17 November (compared with 54 million documented cases). By extrapolation, one may cautiously estimate ~1.5-2.0 billion infections as of 21 February 2021 (compared with 112 million documented cases). This corresponds to global IFR ~0.15%—a figure open to adjustment for any over- and under-counting of COVID-19 deaths (Appendix S2).

Meyerowitz-Katz and ICCRT reach higher estimates of IFR, but, as discussed above, these are largely due to endorsing selection criteria focusing on high-IFR countries, violations of chosen selection criteria and obvious flaws that consistently cause IFR overestimation. Similar concerns apply to another publication with implausibly high age-stratified IFRs by Meyerowitz-Katz limited to countries with advanced economies, again narrowly selected some of the highest IFR locations and estimates.[12]

Even correcting inappropriate exclusions/inclusion of studies, errors and seroreversion, IFR still varies substantially across continents and countries. Overall average IFR may be ~0.3%-0.4% in Europe and the Americas (~0.2% among community-dwelling non-institutionalized people) and ~0.05% in Africa[14] and Asia (excluding Wuhan). Within Europe, IFR estimates were probably substantially higher in the first wave in countries like Spain,[67] UK[68] and Belgium[69] and lower in countries such as Cyprus or Faroe Islands (~0.15%, even case fatality rate is very low),[70] Finland (~0.15%)[71] and Iceland (~0.3%).[72] One European country (Andorra) tested for antibodies 91% of its population.[73] Results[73] suggest an IFR less than half of what sampling surveys with greater missingness have inferred in neighbouring Spain. Moreover, high seroreversion was noted, even a few weeks apart[73]; thus, IFR may be even lower. Differences exist also within a country; for example within the USA, IFR differs markedly in disadvantaged New Orleans districts versus affluent Silicon Valley areas. Differences are driven by population age structure, nursing home populations, effective sheltering of vulnerable people,[74] medical care, use of effective (eg dexamethasone)[75] or detrimental (eg hydroxychloroquine)[76] treatments, host genetics,[77] viral genetics and other factors.

Infection fatality rate may change over time locally[78] and globally. If new vaccines and treatments pragmatically prevent deaths among the most vulnerable, theoretically global IFR may decrease even below 0.1%. However, there are still uncertainties

both about the real-world effectiveness of new options, as well as the pandemic course and post-pandemic SARS-CoV-2 outbreaks or seasonal re-occurrence. IFR will depend on settings and populations involved. For example, even 'common cold' coronaviruses have IFR~10% in nursing home outbreaks.[79]

Admittedly, primary studies, their overviews and the current overview of overviews have limitations. All estimates have uncertainty. Interpretation unavoidably has subjective elements. This challenge is well-known in the literature of discrepant systematic reviews.[80-84] Cross-linking diverse types of evidence generates even more diverse eligibility/design/analytical options. Nevertheless, one should separate clear errors and directional biases from defendable eligibility/design/analytical diversity.

Allowing for such residual uncertainties, reassuringly the picture from the six evaluations assessed here is relatively congruent: SARS-CoV-2 is widely spread and has lower average IFR than originally feared, and substantial global and local heterogeneity. Using more accurate estimates of IFR may yield more appropriate planning, predictions and evaluation of measures.

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CONFLICTS OF INTEREST

None.

DISCLOSURES

I am the author of one of the six evaluations assessed in this article.

ORCID

John P. A. Ioannidis https://orcid.org/0000-0003-3118-6859

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section.

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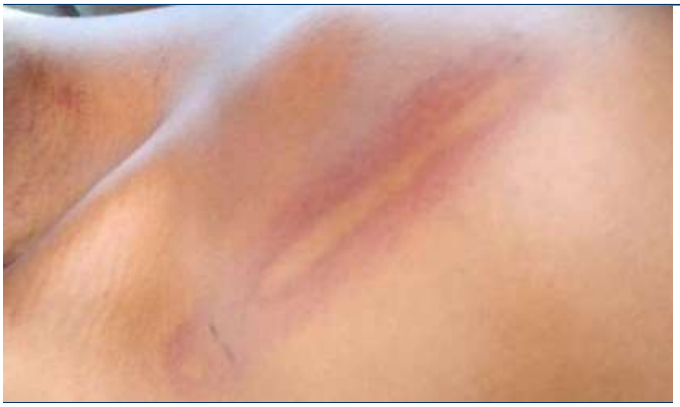
PRODUKTIE 18

Mishandelde lockdownovertreders nog onthutst over politieoptreden

15/05/2021 10:04 - Merredith Bruce



Letsels die Kevin Geerlings heeft overgehouden aan de mishandeling. **Foto:**

PARAMARIBO - "Ik wil dat de samenleving dit weet, want geen enkele lockdownovertreder verdient zo een behandeling", zegt Ryan Geerlings. Hij is op 20 april samen met zijn jongere broer Kevin slachtoffer geworden van een barbaars politieoptreden. De broers werden op die dag als lockdownovertreders door de politie aangehouden aan de Coesewijnestraat.

Het begon volgens Ryan - die ook namens Kevin praat - al toen ze werden gedirigeerd om in de politiebus te stappen. "Ze hebben me geduwd in mijn rug waardoor ik op de passagiersstoel viel." De twee werden naar de Politieacademie gereden maar kregen opeens te horen dat ze naar de politiepost aan de Keizerstraat zouden worden overgebracht. "Ik wilde weten waarom maar ze zeiden me dat ik niet te veel moet praten."

Op het politiebureau aangekomen werd hen opgedragen om zich van alle kleding te ontdoen. Van de politie kregen ze te horen dat dit de procedure is, anders lopen ze het risico dat arrestanten hun kleren zullen afpakken. "Flexi nanga mpp!" beet één van de manschappen hen toe, vertelt Ryan. Omdat het kennelijk niet snel genoeg ging, trokken de agenten zelf de kleren van hun lijf. Het slachtoffer vertelt dat hij en zijn broer vervolgens enkele keren zijn mishandeld. Ze incasseerden vuistslagen, schoppen, klappen en er kwam ook een gummistok aan te pas.

Een agent zou opdracht hebben gegeven aan arrestanten om -tegen een geldelijke 'beloning' - ook mee te doen. "*Den man musu bari leki meisje want den wan pley kwai man*", citeert Ryan het bevel naar de gevangenen. Zelf beweert hij dat er geen enkele aanleiding was voor zo een optreden. Hoewel hij vanwege zijn werk vrijstelling heeft van lockdown, maar echter niet in werkverband over straat was, rechtvaardigt dit de "criminele aanpak" door de agenten niet meent Ryan.

Hij en zijn broer hebben hen "slechts" erop gewezen dat hun aanpak niet door de beugel kan. "Daarin ben ik toch vrij?" stelt hij retorisch. Op de vraag waarom ze nu in de publiciteit treden, zegt Ryan dat ze eerst het onderzoek van de afdeling Onderzoek Politionele Zaken waarze een officiële klacht hebben ingediend, wilden afwachten. Er zijn naar aanleiding van dit onderzoek deze week vijf agenten in verzekering gesteld inzake mishandeling.

Naar verluidt zijn er daarnaast twee wetsdienaren op non-actief gesteld. Hoewel ze hiermee enigszins gerustgesteld zijn, heeft Ryan graag dat alle manschappen, onder wie twee militairen, die betrokken zijn geweest bij dit barbaarse optreden, worden aangepakt. "Het was geen kleine kliek." Door alsnog in de publiciteit te treden, willen de broers dat andere agenten hier lering uittrekken. "Laat dit een les voor ze zijn."

✚ Vandaag

- COMMENTAAR: Zeg nee!
- Openbaar Ministerie eist 5 jaar celstraf tegen Kromosoeto
- Caribische voetbaltitel binnen handbereik Inter Moengotapoe
- Geen retentiedollars voor ‘gelukzoeker’
- Consumentenprijzen met 3,5 procent gestegen
- 'Mensen willen geen pakketten'

PRODUKTIE 19

Feller laat ‘onheus’ politieoptreden niet zo

14/04/2021 05:59 - Merredith Bruce

PARAMARIBO - “Ik moest in bijzijn van anderen mijn broek zakken en vervolgens drie keer hurken,” vertelt Rendel Feller. Dit is naar zijn zeggen slechts een tipje van de onheuse behandeling die hij zondag op het politiebureau zou hebben gehad, tijdens zijn korte aanhouding.

Hij zal het niet hierbij laten en wil een schadevergoeding van de staat. Feller zegt deze week nog stappen daartoe te zullen ondernemen met zijn advocaat. De 24-jarige activist laat zich niet afschrikken, want al een dag na zijn aanhouding ging hij weer de straat op.

"Zolang ik weet dat ik oprecht bezig ben, heb ik niets te vrezen", klinkt het vastberaden en onbevreesd. Hij is zeker één keer via de telefoon met de dood bedreigd, maar ook dát weerhoudt hem niet om deze regering het vuur na aan de schenen te leggen. *"A wins den broko mi neki, nanga a her broko neki mi e go baka tap strati."*

Lees het uitgebreide interview in onze krant van woensdag

✚ Vandaag

- COMMENTAAR: Zeg nee!
- Openbaar Ministerie eist 5 jaar celstraf tegen Kromosoeto
- Caribische voetbaltitel binnen handbereik Inter Moengotapoe
- Geen retentiedollars voor ‘gelukzoeker’
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- 'Mensen willen geen pakketten'