Since January 2020 Elsevier has created a COVID-19 resource centre with free information in English and Mandarin on the novel coronavirus COVID-19. The COVID-19 resource centre is hosted on Elsevier Connect, the company’s public news and information website.

Elsevier hereby grants permission to make all its COVID-19-related research that is available on the COVID-19 resource centre - including this research content - immediately available in PubMed Central and other publicly funded repositories, such as the WHO COVID database with rights for unrestricted research re-use and analyses in any form or by any means with acknowledgement of the original source. These permissions are granted for free by Elsevier for as long as the COVID-19 resource centre remains active.
Facing an unfortunate trade-off: policy responses, lessons and spill-overs during the COVID-19 pandemic

Catalin Dragomirescu-Gaina

Center for European Studies (CefES), University of Milano-Bicocca, Italy

ARTICLE INFO

JEL Classifications:
E61
F6
I18

Keywords:
COVID-19
policy stringency
sign and magnitude restrictions
GVAR

ABSTRACT

Although COVID-19 emerged as a global shock, governments adopted non-pharmaceutical policy responses that were rather heterogeneous, depending on cultural and institutional characteristics. At the country level, the stringency of ‘lockdown’-type policies should be set to achieve the best possible trade-off between economic and fatality dynamics, obviously accounting for possible cross-border influences. To allow for policy learning, I assume that the first country implementing a policy initiative that is worth emulating must either get the best possible health or the best possible economic outcome. I propose a combination of sign and magnitude restrictions, embedded in a global VAR model, to identify idiosyncratic policy shocks that spill over and influence policy responses abroad. Once policy shocks are identified, I run a comparison exercise between two model specifications, i.e. with and without policy emulation. Within a given a sample, this methodology can be used to find when and where policy lessons can be identified. I find that, among 17 developed and developing countries, few can offer lessons based on their policy initiatives, but several others might get better trade-offs through policy emulation, although in reality this outcome is not guaranteed to have occurred.

1. INTRODUCTION

In January 2020 the entire world held its breath, watching China deal with the massive logistical and political challenges that emerged after quarantining the entire city of Wuhan. Then Italy followed suit in early March 2020, imposing a country-wide lockdown after a first death and several cases were confirmed in the Northern region of Lombardy. Despite large institutional and cultural differences, it seemed the only policy option Italy had available at that time to face a similar problem as China, although on a different scale. Fearing an uncontrollable outbreak, some European countries adopted similar measures before even registering a single death from the virus. Others instead have been rather sceptic regarding the benefits of such drastic restrictions (e.g. Sweden is a well-known example). While the COVID-19 shock has been a common one, inflicting severe economic and health damages on a global scale, policy responses and non-pharmaceutical interventions have come in various forms, with timing, composition and effectiveness varying by country, and region as well. Given this large heterogeneity in policy responses, I believe one simple question remains under-investigated in a rapidly expanding literature: Can policy emulation help countries to improve the main trade-off that each of them is facing between health and economic outcomes?

I try to empirically answer this important question by estimating a multi-country global VAR model in the spirit of Pesaran et al. (2004) and Dees et al. (2007), while paying particular attention to the identification of policy shocks and their cross-border transmission. As a proxy for (non-pharmaceutical) policy interventions I use the stringency index available from the Oxford COVID-19 Government Response Tracker (see Hale et al., 2020). From a time perspective, setting the right level of stringency (or lockdown-type) policy was a rather difficult task, as countries had to focus on balancing economic with infection risks, while downplaying other types of risk (such as social risks, which are currently rising due to misinformation, declining public trust, raising policy uncertainty etc.). This difficult balancing act prompted a so-called ‘wealth versus health’ trade-off that features extensively in the recent COVID-19 literature (e.g. Eichenbaum et al., 2020). Moreover, the level of policy stringency had to account for substantial cross-border influences that arise due to existing economic (i.e. trade) links and due to social...
mobility, which directly affects virus transmission.

In practice, policy emulation can explain the strong commonalities displayed by countries’ policy stringency responses, although policymakers were facing different domestic contexts, challenges and constraints. However, which countries can copy from which others, and whether this can help them to improve their ‘wealth vs. health’ trade-offs remain essentially empirical questions. In this paper, policy emulation is inferred indirectly, as a difference between two model specifications that either (i) fully prevent or (ii) explicitly allow for the transmission of cross-country policy influences (or spill-overs) from foreign idiosyncratic stringency shocks. Although in a strict sense, significant cross-border policy spill-overs are a necessary condition for policy emulation to arise, this is not sufficient for learning to occur. However, for lack of a better term, I will use ‘policy emulation’ when referring to significant domestic policy responses to foreign country-specific policy shocks, given that the later are properly identified to portray policy experiments or initiatives from which others can learn.

As a first rather methodological contribution, the paper proposes an identification strategy for policy shocks using a combination of sign and magnitude restrictions, which I describe in more details in the next section. The identification of policy shocks, which in my modelling setting can be interpreted as (discretionary) changes leading to policy lessons, is fundamental to our understanding of policy learning and diffusion. Therefore, to identify a successful policy initiative I assume that the first country implementing it must set an example by demonstrating it can get either the best possible health or the best possible economic outcome, within a short timeframe after its implementation.

Once policy shocks are identified, I rely on impulse responses to derive several comparative statistics and diagnostics for two main specifications of the model, i.e. with and without policy emulation. Therefore, a second contribution of the paper is to specify the transmission channel for cross-border policy spill-overs, which here rest on cultural similarities driving the institutional characteristics that determine the enforceability of such policies (see Frey et al., 2020; Yan et al., 2020). Note that stringency here refers to the adopted measures, rather than their enforceability; therefore, large cultural differences, for example, would hamper policy diffusion exactly because the domestic final outcomes of foreign policies would be more uncertain due to the unknown local population behaviour (that heavily influences the final outcomes of the adopted policies). My argument is therefore very similar to that used in Persson and Tabellini (2009), who show that policy learning from similar countries explains an important part of the observed cross-sectional heterogeneity in economic development (see also Buera et al., 2011; Bove and Gokmen, 2018).

To some extent, my empirical approach can be seen as an application of the theoretical model proposed in Acharya et al. (2020), who show that a better international risk-sharing arrangement addressing both health and economic risks can lead to improved outcomes. The main insight from my comparative exercise is that many countries can get better trade-offs for their health and economic outcomes through policy emulation, although this outcome is not guaranteed in reality. Moreover, I find that there is an implicit restriction to this ‘learning’ process because only few countries (e.g. Korea, Australia) offer learning opportunities due to their policy initiatives and experiences. The next Sections 2 and 3 describe the methodology, the data and the empirical approach in more details. Section 4 presents and discusses the main results, while section 5 describes some limitations and robustness checks. Finally, section 6 concludes.

2. EMPIRICAL METHODOLOGY

I adopt a global VAR (or GVAR) model specification that allows me to deal with both the time-series and cross-sectional dimensions of the various policy restrictions adopted in response to the COVID-19 pandemic. The GVAR is a collection of country-specific vector autoregressive (VAR) models, appropriately weighted to reflect cross-border transmission channels. In my setting, each country-specific model includes just three endogenous variables: the policy stringency index simply denoted as policy; an economic activity proxy denoted as economy; and the case fatality rate, denoted as fatality. This simple modelling framework allows me to capture both: (i) the policy instrument and (ii) the main trade-off that authorities had to deal with during the COVID crisis; Li and Meissner (2020) label this as ‘wealth versus health’ trade-off and document important policy spill-overs in, as well as from, neighbouring countries (or states in the case of U.S.). My analysis assumes a different channel for cross-border policy diffusion (i.e. based on cultural distance metrics), along with a more detailed identification of policy shocks (i.e. lessons arising from discretionary changes) – that I believe are fundamental to our understanding of policy learning and diffusion.

In particular, I require that a positive policy stringency shock in a given country: (i) has non-positive consequences on both domestic fatality and economic activity lasting for at least two weeks/periods after the shock; and (ii) has either the largest (in absolute terms) cumulated impact on that country’s fatality or the smallest (in absolute terms) cumulated impact on its economic activity; the cumulated impact is evaluated after two weeks/periods. The first condition merely indicates the direction of responses, while the second condition requires that the country’s outcomes set a benchmark (either in terms of fatality or economic dynamics) for others; I allow for a short two weeks lag (to approximate the period between infection and hospitalization) before evaluating the consequences of a given policy. These restrictions are conveniently summarised below:

Identifying restrictions for a positive policy stringency shock in country c

| Restrictions | Constraints |
|--------------|-------------|
| Sign restrictions imposed for two periods after impact | fatality responses ≤0 AND economy responses ≤0 |
| Magnitude restrictions evaluated after two periods | abs. magnitude of cumulated fatality response in country c > abs. magnitude of cumulated fatality response in all other countries OR abs. magnitude of cumulated economy response in country c < abs. magnitude of cumulated economy response in all other countries |

The intuition of the above restrictions is rather simple: to identify a policy lesson, the government delivering it must set an example by

2 Li and Meissner (2020) discuss policy spill-overs using a different dataset and empirical approach than the ones considered here.
3 While sign restrictions are regularly employed in empirical studies, magnitude restrictions are less common, being first introduced in De Santis and Zinic (2018). To the best of my knowledge, there is no other study that combines the two strategies, and more so in relation to the COVID-19 pandemic.
4 Several studies (e.g. Frey et al., 2020; Yan et al. 2020) document the importance of cultural and institutional factors in explaining the efficiency of the adopted policies in countering the consequences of the pandemic. Compared to all the above mentioned references, which deal with long-term economic/institutional/technological diffusion, an analysis of commonalities in policy responses to the COVID-19 crisis requires a short-term perspective. More arguments in support to this assumption are provided in section 3.
5 Despite some inherent challenges, fatality rates have been used in many studies published on COVID-19, e.g. Flaxman et al. (2020). I use the case fatality rate calculated based on international standards (see https://www.who.int/news-room/commentaries/detail/estimating-mortality-from-covid-19), using data on confirmed cases and deaths available from the Oxford dataset (see Hale et al., 2020).
6 While the table describes restrictions for positive policy shocks, the algorithm allows for negative shocks as well.
demonstrating it is able to either the best fatality or the best economic outcome, within a short timeframe after implementation. My sign restrictions merely require that a policy tightening has non-positive effects on the economy (e.g. due to restrictions on people movements, higher uncertainty on businesses, hiring and investments etc.) and on fatality dynamics (e.g. due to increased screening and testing, better organization of hospitals etc.). Magnitude restrictions (as in De Santis and Zimic, 2018) are then useful in separating changes within similar variables but across different jurisdictions (after accounting for cross-border spill-overs), especially when there is no local information available to allow for more precise identification.8

A three-variable model is thus the minimum specification allowing me to separate discretionary changes in stringency policies from those changes that are correlated with local health and economic dynamics.9 I use a highly parsimonious GVAR specification, with one lag, that is formulated as:

\[ Y_{t+1} = \alpha_t + \beta_t Y_{t-1} + \gamma_t X_t + \delta_t X_t + \epsilon_{t+1} \]  

(1)

where countries are indexed by \( c \), while \( t \) is a time subscript; \( Y_{t-1} \) is a vector of country-specific endogenous variables, while \( Y_{t-1} \) is a vector of country-specific foreign counterparts constructed based on a particular country-by-country weighting matrix (see below); \( \alpha_t \), \( \beta_t \), \( \gamma_t \), and \( \delta_t \) are parameters to be estimated, and \( \epsilon_{t+1} \) is a random noise. \( X_t \) is a set of global variables that are not specific to any particular country, but are used to summarize contributions from other factors not already captured by the endogenous variables of the model; the dynamics of \( X_t \) is kept as simple as possible, i.e. specified as a AR(2) process with direct feedbacks from the endogenous vector \( Y_t \) as:

\[ X_t = \theta + \sum_{i=1}^{k-2} \theta_i X_{t-i} + \sum_{j=0}^{p-1} \sigma_j S_{Y_{t-j}} + \epsilon_{X_t} \]  

(2)

where, \( \theta \), \( \theta_i \), and \( \sigma_j \) are coefficients, while \( S \) is a vector of country weights modulating the feedbacks from \( Y_t \) on \( X_t \), and defined by the average ratios of COVID-19 confirmed deaths over identified cases.

At this point, a short discussion is in order on how policy emulation is exactly inferred here. Even if in equation (1) the impact of \( Y \) is contemporaneous on \( Y \), this specification does not actually preclude policy emulation. Note that any discretionary policy change will be persistent since stringency enters the model in levels; this persistency in the policy instrument would then produce (persistent) effects on the other two variables included in vector \( Y \), allowing thus other countries to learn over time on the basis of the foreign realised (health and economic) outcomes, and then to decide whether emulating, or not, the same policy initiative. To completely shut off policy emulation in the model, I need therefore a more radical approach. Therefore, I introduce two distinct model specifications, with and without policy emulation and which I label as model specification (A) and (B) respectively:

| Model specification (A) with policy emulation | Model specification (B) without policy emulation |
|-----------------------------------------------|-----------------------------------------------|
| \( Y' = \text{policy}. \text{economy. fatality} \) | \( Y' = \text{policy}. \text{economy. fatality} \) |
| \( Y'' = \text{policy}. \text{economy. fatality} \) | \( Y'' = \text{economy}. \text{fatality} \) |

where, after ignoring the subscripts, we can write that: \( \text{policy} = W_1 \text{policy}_e \), \( \text{economy} = W_2 \text{economy} \), and \( \text{fatality} = W_3 \text{fatality} \); note that \( W_1, W_2, W_3 \) are weighting matrixes with zeros on the main diagonal, normalized by row (see the Appendix), and constructed to reflect the cross-sectional relationships of the three endogenous variables (see more details below).

The level of policy stringency in each country should be set in order to achieve the best possible trade-off between domestic economic dynamics and fatality rates at any given time. Whether (some) countries are able to internalize the large externalities arising from foreign policy responses is a theoretical issue that has been answered in other recent papers, e.g. Acharya et al. (2020); empirically though, this issue remains disputed. In case we assume that domestic policy-makers emulate foreign policies lessons (or avoid mistakes), we will be in model specification (A); otherwise specification (B) will apply. Differentiating between the two can be an interesting, but challenging empirical exercise since \( a-priori \) there is no clear way to identify which countries learnt from which others (except when based on timing, which nevertheless can still lead to inconclusive results). However, even in case that my models’ estimates prove to be inductive, a comparison or counterfactual analysis might still reveal other insights about policy learning.

3. DATA

To estimate the model, I rely on several data sources for my analysis, which employs weekly averages of several relevant indicators covering a total of 17 (both developed and developing) countries.10 The ISO codes of the countries included are the following: ARG, AUS, BRA, CAN, DEU, FRA, GBR, IDN, IND, ITA, JPN, KOR, MEX, RUS, TUR, USA, ZAF; unfortunately, I had to exclude China for lack of data availability. While such limited number of countries might reduce the set of available policy lessons one can identify in this data, the methodology here remains valid and can easily be extended to larger datasets. The sample begins on the first week of February 2020 and ends with the third week of November (42 observations), being therefore unaffected by vaccination campaigns that began in December 2020. Due to the obvious weekly seasonality of COVID-19 cases and testing practices, I use 7-days rolling averages (reported as of Saturday) of all the time-series reported on a daily basis.

Firstly, the data on policy non-pharmaceutical interventions comes from the Oxford COVID-19 Government Response Tracker (or OxCGRT) developed by Hale et al. (2020).11 The main findings presented in the next section focus on the policy stringency indicator that summarizes the strictness of ‘lockdown style’ policies on each date. Policy stringency index has been used in several recent studies, establishing itself as a relevant and effective indicator of government interventions (e.g. Li and Meissner, 2020; Amuedo-Durante et al., 2021). Also data on the number of confirmed cases and deaths, which are used to compute the case fatality rates, are available from the same source. As mentioned before, stringency enters the model in levels, since governments had a-priori a

---

8 Testing has been one of the key factor that allowed many countries to better calibrate their policy responses in the second wave that followed in late 2020. With their costs decreasing, the availability of testing increased, allowing countries to have better data and to better monitor the extent of the crisis (see Trudeau et al., 2020).

9 Assuming local information is available (e.g. from newspapers) policy errors might be more easily identifiable via a proxy VAR or using external instruments. This might be an interesting empirical extension of the analysis, but it would inherit an unavoidable bias since local media would be reporting on local policy errors as a function of political polarization, timing of elections and other political economy considerations that must be properly accounted for. Nevertheless, in section 4 I cross-check the identified policy shocks with local media news/events.

10 All country-specific equations (1) and equation (2) are stacked together and estimated jointly as a global VAR; see Pesaran et al. (2004) and Dees et al. (2007).

11 Data is available at https://www.bsg.ox.ac.uk/research/research-projects/coronavirus-government-response-tracker and was downloaded on January 10th, 2020.
strong preference for low stringency values; however, the case fatality rates enter in first difference due to high persistency, and the existence of several methodological (e.g. changes in the case definition) and data collection breaks in these time-series across various countries. 

Secondly, I use the newly available GDP weekly tracker on economic activity derived from Google Trends data; for methodological notes, see Woloszko (2020) and OECD (2020). Despite a multitude of empirical analyses employing several high-frequency, unconventional indicators (e.g. traffic data, pollution, energy consumption), this is the only dataset I am aware of that tracks economic activity on a weekly basis across a large sample of countries, and is constructed with a uniform methodology. Unfortunately, it also imposes the highest restrictions on my dataset in terms of (number of) countries and sample length. Since the OECD GDP tracker is shown to highly correlate with annual GDP growth rates, it enters the model directly in levels.

Thirdly, to link all the main country-specific indicators mentioned above and integrate them in my GVAR, I need several weighting matrices that can efficiently summarize the cross-sectional dimension of my dataset as well as the main transmission mechanisms at work. To reflect the diffusion of policy initiatives during the COVID-19 pandemic across different countries featuring different institutional regimes, I use the cultural distance\(^{12}\) proxy developed by Spolaore and Wacziarg (2009), Alesina and Giuliano (2015) show that cultural differences delay and hamper the diffusion of political as well as economic institutions conducive to economic development; in a similar vein, Persson and Tabellini (2009) and Buera et al. (2011) explain the observed cross-sectional heterogeneity in economic development based on policy learning from similar (as well as neighbouring) countries; Spolaore and Wacziarg (2009) and Bove and Gokmen (2018) link cultural distance to technology diffusion. Moreover, the OxCGRT policy stringency index merely captures the strictness of government adopted measures, but does not quantify their enforceability, population obedience, or compliance;\(^{13}\) therefore, large cultural differences would hamper policy diffusion due to the unknown local population behaviour, which is the last link in the transmission chain.

Next, to reflect cross-border economic linkages, i.e. \(W\), I use a standard weighting matrix based on annual export and import flows, averaged over the 2015-2019 period. Finally, in order to adequately capture the intensity of human-to-human transmission of the virus, i.e. \(W\), I use the Social Connectedness Index (SCI), which is based on Facebook data and available from Crowdtaggle.\(^{15}\) The SCI is provided as a dyadic matrix with each element reflecting the likelihood that a person from a country A has a Facebook connection in country B (with A and B not necessarily different). As a proxy for social mobility, I assume that the SCI\(^{16}\) reflects the likelihood (probably underestimated when based on official case count) of human-to-human transmission across borders in the absence of travel restrictions (see also Milani, 2021). In fact, recent medical studies (see La Rosa et al., 2021) show that COVID-19 was already circulating in Italy in late 2019, suggesting that the virus had already spread globally well before the first travel restrictions were enacted. By relying on data for 90 countries, Farzaneh et al. (2021) document a strong link between pre-crisis international tourism (from 2010 to 2019) and the cumulated number of confirmed cases and deaths by the end of April 2020.

Fourth, the model also features two global variables, jointly denoted by \(X\), in equations (1) and (2): VIX, which is a popular volatility index, and the EMV-ID index developed in Baker et al. (2020); the latter is based on specific keywords’ frequency and tracks the daily market volatility along with epidemic news. The two indexes are used as proxies for global market volatility and policy uncertainty that have both severely constrained (particularly highly indebted) governments from responding with adequate economic stimulus to the COVID-19 pandemic (e.g. Italy). This volatility/uncertainty compounds with the speed of the global economic slowdown spreading after the initial COVID-19 shock, thus hindering policy responses and pushing many governments in a worse position from the perspective of the ‘wealth vs. health’ trade-off (see Altig et al., 2020; Zimmermann et al., 2020; Milani, 2021).

\(\text{Table 1}\)

| Country | Equation: policy | Equation: economy | Equation: fatality |
|---------|-----------------|-------------------|-------------------|
| ARG     | 15.673 \(^\dagger\) | 5.815 \(^\dagger\) | 1.672 |
| AUS     | 2.032           | 1.739             | 0.302             |
| BRA     | 1.253           | 8.243             | 0.389             |
| CAN     | 25.477 \(^\dagger\) | 16.280 \(^\dagger\) | 0.390             |
| DEU     | 13.708 \(^\dagger\) | 0.472             | 0.018             |
| FRA     | 1.196           | 5.225             | 2.024             |
| GBR     | 19.171 \(^\dagger\) | 6.285 \(^\dagger\) | 1.486             |
| IDN     | 0.549           | 0.056             | 7.245 \(^\dagger\) |
| IND     | 2.683           | 0.273             | 5.058 \(^\dagger\) |
| ITA     | 5.559 \(^\dagger\) | 13.655 \(^\dagger\) | 1.166             |
| JPN     | 0.058           | 0.518             | 9.661 \(^\dagger\) |
| KOR     | 1.178           | 0.164             | 27.274 \(^\dagger\) |
| MEX     | 27.405 \(^\dagger\) | 0.148             | 0.005             |
| RUS     | 0.012           | 8.419 \(^\dagger\) | 0.064             |
| TUR     | 1.894           | 1.960 \(^\dagger\) | 0.068             |
| USA     | 18.170 \(^\dagger\) | 0.696             | 0.309             |
| ZAF     | 0.691           | 0.297             | 0.717             |

Note: The table displays the F-statistics computed by restricting the coefficients of policy variables to be exactly zero (see equation 1) in the country/equation indicated on the first column/row of the table. Countries are denoted by their ISO code. The \(^\dagger\) superscript denotes cases where the F-statistics is higher than the critical value of 4.139, and therefore the null can be rejected at a 5% significance level.

\(\text{\footnotesize Note:}\) the largest eigenvalues for model specifications (A) and (B) are 0.974 and 0.957 respectively.
India), foreign policies’ influence falls mainly on the fatality equation.18 For the remaining ones, the influence of foreign policies falls on the equations characterizing the domestic economy and/or policy stringency, though sometimes on both.

Secondly, lacking an undisputed proof on the statistical significance of policy19 for all countries is a rather unfortunate outcome, although it does suggest that policy ‘emulation’ – as defined here – has not been a universal strategy in reality. This heterogeneity in the estimated impact of policy20 leads to the following point: without a clear way to differentiate between the two model specifications (A) and (B), the best way to proceed forward is to compare their results and derive as many insights as possible from this simple comparison (or counterfactual) exercise.19 Despite my limited sample, this allows me to understand the possible benefits that countries would get from policy emulation and, more importantly, which countries learn best and which ones generate the most important policy lesson for others.

While Table 1 provides a first snapshot of some of interesting aspects, it does not adequately reveal the timing nor the origin of any policy lessons that are the main focus of this paper. Therefore, in the next step, I apply my identification restrictions to recover the structural policy shocks. The identification is performed separately, for each country in turn, using several bootstrap replications of the estimated model to construct median responses and confidence intervals. I start from the eigenvalue decomposition of the estimated variance-covariance matrix of the GVAR model, and then post-multiply it with the QR decomposition of a random orthogonal matrix, with elements drawn from a normal distribution; I allow up to 1000 QR draws for each bootstrap replication of the GVAR, with a success rate that varies21 depending on the specific country for which identification is performed; I use as many bootstraps as needed to make sure that the sign and magnitude restrictions are satisfied for at least 1000 times.

Using the peak values22 of the median responses to a positive policy shock, I draw scatter plots in Fig. 1 with the response in economy on the X axis and the response in fatality on the Y axis; the size of the bubbles represents the peak policy responses to a policy shock in a given country. These scatter plots provide the simplest representation of the ‘wealth vs. health’ trade-off each country faces when calibrating its policy stringency levels (with or without accounting for cross-border policy spill-overs). The relative position of the filled (black) bubble with respect to the empty (white) one appearing in each graph points to the relative benefits of policy emulation.23 The best relative outcomes (i.e. lower fatality and higher economy) obtain whenever the filled (black) bubble veers towards the left-lower corner, while the empty (white) one veers towards the left-upper corner; a better relative economic outcome obtains whenever the filled bubble stays on the right side of an empty one; a better relative health outcome obtains whenever the filled bubble seats below the empty one.

To save space, only policy shocks from two representative countries are depicted below for illustration purposes; a convenient summary of all these results is provided next in Table 2.

The main insights from Fig. 1 are the following. Emulating24 Italian policies brings worse economic outcomes for all countries (since filled bubbles always stay on the left side of the graphs), and better fatality dynamics for some countries (only where the filled bubbles lie below the empty ones; e.g. ARG, GBR, JP etc.). In the case of Japanese policy shocks, emulation always improves at least one outcome (i.e. economy or fatality rates), and sometimes even leads to better overall trade-offs, meaning both better fatality dynamics and better economic outcomes (when filled bubbles veer towards the right-lower corner, just opposite the empty bubbles; e.g. for ARG, AUS and DEU). These results match with the existing narrative. Italy was the first Western democracy to impose a full, country-wide lockdown in early March once it lost control of the outbreak; its strategy required draconic restrictions on social interactions and movements, leading to abrupt declines in economic activity (though fatality rates also dropped). Japan instead did not impose a full lockdown (the increase in its stringency levels is among the lowest across all countries), but instead used extensive testing and tracking policies to control the outbreak.

To more easily identify countries that provide the best learning opportunities within my dataset, Table 2 summarises the relative benefits derived under model specification (A) when compared to specification (B), for all country-pairs. Since I am just comparing two model specifications designed to be mutually exclusive, in reality, these benefits are not guaranteed to have occurred; they are relative in the sense that they make sense when compared one with each other.

Overall, Table 2 shows that countries like Australia, U.K., Indonesia, South Korea and Japan provide others with plenty of opportunities to improve the calibration of their own policy stringency responses; this is because following discretionary policy changes (i.e. shocks) in these countries, emulation25 allows others to improve their own trade-offs, i.e. obtain lower fatality and higher economic outcomes. While Australia, Japan, and South Korea are among those already mentioned for good policy practices, U.K. is a rather unexpected finding, at least due to its confusing public communication strategy regarding restrictions. While, to a large extent, narrative evidence suggests that emulating Italian or Brazilian policies can bring only fatality (but not economic) benefits, and only to a few countries, it is interesting to see this happening in the case of Germany as well. French policy initiative also did not fare better either, with countries emulating French policies being able to improve their fatality outcomes but lose on the economic dimension (except for India). Perhaps unsurprisingly, very few countries (i.e. only three) are able to improve any of their outcomes by emulating discretionary policy initiatives from U.S. though,26 suggesting that valuable lessons might be learnt from poorly designed policies as well, although the present analysis does not proceed along these lines.

The best learners in Table 2 are those able to improve their ‘wealth vs. health’ trade-offs along both dimensions by drawing on as many country-specific experiences as possible. The clear outlier here is Germany (potentially learning from Australia, U.K. and Japan); other outperformers include Brazil (potentially learning from U.K., and South Africa), and Russia (potentially learning from Australia and South

18 It suggests these countries were early movers in reacting to the spreading of the virus out of China (which is missing from my sample due to data unavailability) via the direct, human-to-human transmission channel. Since policy does not weight in China’s policy stringency levels, this means that my model is inherently underweighting the foreign policy stringency levels that would be relevant for these Asian countries.

19 To some extent, a similar modelling approach and comparison exercise is employed in Dragomirescu-Gaina and Philippas (2015) who compare fiscal policy coordination with fiscal policy discretion across a set of European countries. They start from the assumption that, within the European Union, true fiscal policy coordination most likely implies compromises reached in Brussels, which would be hard to identify in the data.

20 The success rate varies from a minimum of 0.2% per bootstrap, to 7.7% per bootstrap.

21 Peak values are computed based on responses displayed over the first six weeks after the shock.

22 Note that one country facing a bad position under specification (B), say high fatality and bad economic outcomes, might be able to improve it by policy emulation, although it would still have high fatality and/or bad economic outcomes even under specification (A).

23 There is an inherent drawback here in that the difference between policy ‘emulation’ and no policy ‘emulation’ is equivalent to the difference between taking and not taking into account foreign policy influences stemming from a country-specific policy shock.

24 Note that there exists an opposite mirror image of Table 2, showing the relative benefits of not emulating others. However, such an exercise would lack utility since not emulating would not offer any normative guidance and would have no policy implications.
Korea). In fact, all countries (except U.K.) can improve their trade-offs (i.e., obtain better fatality and economic outcomes) by emulating at least one other country’s policy initiatives; the key point though, and here lies the added value of the present analysis, is to identify which country’s policy initiative might be valuable to emulate. It should be stressed again that, even if this is merely a counterfactual exercise, it has the potential to show when and where the ‘good policies’ can be uncovered within any given dataset.

In particular, a very useful exercise can investigate the matching of the identified shocks series with the timeline of major COVID-related events, as available from various media outlets and news agencies. Table 3 below provides an example of such an exercise for U.K., showing...
some interesting matches. The largest identified shock corresponds to the week of 10-16 May 2020, and overlaps with a relaxation of “stay at home” restrictions – being actually the first reduction in ‘lockdown’ policies since the beginning of the pandemic in U.K.; at a disaggregated level, the OxCGRT stringency index includes 8 components (e.g. school closing, workplace closing, international travel restrictions etc.), out of which only for “stay at home” requirements there is a decline in the above-mentioned week. How can we interpret this? In the model’s language, the overall stringency level was set way too high with respect to the existing economic and health conditions, which were improving at that time in U.K. It is therefore this extra-cautiousness (i.e. relaxing some, but not all restrictions) that provides valuable policy lessons to other countries.

It is also possible that, in reality, the emulation of foreign policies (successful or not) was a matter of timing (see Eichenbaum et al., 2020), although my identifying restrictions are designed to reduce the impact of possible unaccounted contemporaneous spill-overs from abroad. However, due to the almost synchronous nature of the lockdown policies implemented from March to April 2020, the global economy entered into an abrupt slowdown that put additional constraints on the available resources.

Table 3
Identified policy shocks and the timeline of major events in U.K.

| Identified policy shocks | Timeline of major events across U.K. during 2020 |
|--------------------------|-------------------------------------------------|
| 22Feb2020                | 27-28 Feb. - First COVID-19 cases confirmed in Northern Ireland and Wales |
| 29Feb2020                | 5 Mar. - First deaths confirmed in U.K. |
| 07Mar2020                | 13 Mar. - Local elections are postponed. |
| 14Mar2020                | 18 Mar. - Announcement that schools will close on 20 Mar. |
| 21Mar2020                | 23Mar. - Restrictions begin |
| 28Mar2020                | 28 Mar. - P.M. Boris Johnson tests positive for COVID-19 |
| 04Apr2020                | 4 Apr. - P.M. was admitted to hospital |
| 11Apr2020                | 12 Apr. - P.M. leaves the hospital |
| 18Apr2020                | 16 Apr. D. Raab says lockdown will continue for ‘at least’ two weeks |
| 25Apr2020                | 10 May - Some restrictions are being relaxed (outdoor exercise are permitted, and driving to destinations in England) |
| 02May2020                | 11 May - A Covid alert system was introduced with a 5 colour scale |
| 09May2020                | 25 May - the P.M. adviser D. Cummings was criticised over hi alleged breaches of the lockdown |
| 16May2020                | 28 May - Scotland relaxes restrictions |
| 23May2020                | 1 Jun. - Primary schools reopen |
| 30May2020                | 6 Jun. - Thousand march for Black Lives Matter manifestation |
| 06Jun2020                | 8 Jun. - Quarantine becomes mandatory for travellers |
| 13Jun2020                | 24 Jul. - New rules making mask mandatory in public spaces |
| 20Jun2020                | 30 Jul. - New restrictions in Greater Manchester, east Lancashire and parts of West Yorkshire |
| 27Jun2020                | 31 Jul. - UK government announced they were delaying a further easing of restrictions in England until 15 August |
| 04Jul2020                | 14 Aug. - Massive queues and traffic jams at the harbours and Eurotunnel as UK holidaymakers rushed to return home before quarantine restrictions being imposed. |
| 11Jul2020                | 8-9 Sep. - New restrictions that will apply from 14 Sep. in England are published. Similar restrictions were announced for Wales and Scotland. |
| 18Jul2020                | 18 Sep. - New restrictions in different parts of U.K. |
| 25Jul2020                | 21 Sep. - The coronavirus alert level was upgraded from 3 to 4 |
| 01Aug2020                | 3-4 Oct. - ‘Technical errors’ are blamed for under-reporting of cases and deaths |
| 08Aug2020                | 12 Oct. - A three-tier legal framework was introduced, replacing the previous piecemeal local regulations |
| 15Aug2020                | 31 Oct. - P.M. B. Johnson announces a four-week lockdown in England; schools remain open |
| 22Aug2020                | 13 Nov. - Shorter quarantine and rapid testing for travellers |

Note: The values of the shocks in the graph above have been normalised. Timeline data source: https://en.wikipedia.org/wiki/COVID-19_pandemic_in_the_United_Kingdom. The dates of the identified shocks in the graph refer to the end of the corresponding week covering the events.
policy options of many countries. In theory, to prevent such an outcome, a better international risk-sharing arrangement would have been required, as explained in Acharya et al. (2020). To support this claim, Figure A2 from the Appendix depicts the confidence bands associated with peak responses, and shows that the most significant cross-border effects of policy shocks have been in the economic realm. Some countries, like Korea and Japan, were able to escape the ‘timing trap’, by being early adopters of restrictions well before the global economy nose-dived. However, this argument is only partially true since Korea and Japan managed to control the epidemic using a similar strategy (i.e. low stringency, extensive testing and tracking) during the entire year 2020, way after the initial outbreak. As the crisis progresses into 2021, countries will have more opportunities to offer and learn valuable lesson from each other, in which case the present analysis might offer relevant clues to policy-makers.

5. LIMITATIONS AND ROBUSTNESS CHECKS

Some clear limitations and drawbacks should be mentioned in relation to the present analysis. Firstly, Chinese data is missing from most datasets available to deal with the problem exposed here. Secondly, the time dimension of my sample is rather short (42 weekly observations), though I also exploit the cross-section dimension of the data, allowing me to improve the confidence of the estimates. Thirdly, case fatality rates suffer from frequent methodological (e.g. case definition) changes, and under-reporting of the confirmed cases and/or deaths. This is a problem that applies to many similar studies and will probably be overcome in time, as more reliable indicators become available; excess mortality rates have been proposed as a substitute, but such data is not available for many countries due to methodological and reporting differences; the reproduction rate, although widespread, is prone to large regional (i.e. within-country) differences that must account for population density, mobility etc., making this indicator less suitable for large cross-country analyses, and more adequate for regional/local analyses. Fourthly, the current model disregards the probabilistic development of vaccines throughout 2020, although Eichenbaum et al. (2020) show that optimal containment policy becomes more severe if vaccination is likely; to some extent, and rather indirectly, the inclusion of uncertainty and volatility proxies as global variables in the GVAr accounts for the probability of vaccine development (e.g. due to the forward looking behaviour of the stock market).

On a different note, the stringency index is constructed by aggregating several ‘types’ of policy restrictions, as we have seen in the case of U.K.; these policy restriction types could then allow for a more detailed analysis of which specific ‘types’ (e.g. travel restrictions, school closures etc.) are being emulated abroad. The main modelling constraint in this case is that the time-series of the more disaggregated policy ‘types’ include discrete values with much higher persistency (i.e. low time-variation) than the aggregated stringency index. Accordingly, including a specific ‘type’ of policy that captures only one out of many important policy dimensions would inherently restrict the explanatory power of the model, despite potentially bringing more insights.

Besides restrictions, many governments adopted early on in 2020 complementary policies, including economic government support. The combination of different policies probably affected compliance with respect to the ‘lockdown’-style restrictions that are summarised in the model by the stringency index. Recent studies, such as Bargain and Aminjnov (2020), or Schmelz (2021), show that trust was essential in balancing the effectiveness of voluntary compliance versus enforcement, when dealing with COVID-19 policies. But in contrast to stringency measures that were switched on and off during much of the 2020, the economic support programmes were one-off instruments that remained active and almost unchanged (in terms of scale, targeted measures etc.) even after many of the lockdown-styles restrictions were lifted; from a statistical perspective, the variation in OxCGRT economic support indexes registered between April and November 2020 is substantially lower (or even null in some countries) compared to the variation registered in stringency policy indexes. Therefore, apart from some conceptual arguments, there is little empirical value from adding a variable with such a low time-variation to a model intended to capture time-series dynamics over a short, but volatile time-span.

Interesting extensions of the present analysis remain possible though. As already mentioned before, theoretical extensions that consider a particular loss function specification to derive a policy rule for setting the stringency levels can be interesting, and can provide a clear theoretical backbone to the current empirical model. The existence of such a rule also seems to be supported by some evidence, given the many attempts at using colour-based systems (for cities, regions, countries) to communicate restrictions to the public in a more clear way. Replications of the current empirical model to larger datasets can also be considered, assuming data availability and quality improve along the lines mentioned above.

As a main robustness check motivated by the limited number (i.e. 17) of countries in the sample, I perform the following simple exercise: I exclude countries, one by one, from the sample and redo the analysis. Obviously, in reality countries can learn from others not included in my sample, so different policy lessons can be identified by drawing on a smaller/larger set of countries. In this robustness check, I try to deal with these issues, although the exercise here is inherently constrained by the available data. While the exclusion of some particular countries can change the policy discussion and the comparison of results to some extent, the present methodology remains effective in identifying the most relevant policy shocks from within any dataset. In the online Appendix, I present additional results for the case when the excluded country is Australia – one of the most important sources of policy lessons identified in the previous section. Despite some expected changes in the relative policy trade-offs in this case, there are for example very few changes in the magnitude of peak responses to the identified policy shocks. Moreover, policy shocks change in a predictable way, with those identified for countries that are culturally closer to Australia suffering more substantial changes due to its exclusion from the sample, while more culturally distant countries suffer less. If anything, this means that more heterogeneous samples allow better empirical insights, because robustness can be increased. The main implication of this exercise then is that one needs the most comprehensive dataset available in order to draw on as many relevant policy lessons as possible from a dataset.

6. CONCLUSIONS

In this paper I analyse the stringency responses across a sample of 17 countries, covering both developed and developing economies, and running on a weekly basis from the beginning of February, towards the end of November 2020. At a country level, policy stringency should be set in order to achieve the best possible trade-off between economic and mortality outcomes, after eventually accounting for possible cross-border influences that spread due to economic (i.e. trade) links, and due to social mobility (e.g. international travel); policies instead spread due to cultural similarities between countries – an assumption that I motivate based on studies in economic development. I exploit the significant cross-country heterogeneity in policy stringency responses to the COVID-19 crisis by using a combination of sign and magnitude
restrictions embedded in a global VAR model. The identified policy shocks then can be interpreted as policy lessons from which foreign governments can eventually learn through policy emulation. In technical terms, policy emulation is inferred indirectly, as the difference between two model specifications that either (i) fully prevent or (ii) explicitly allow for the transmission of cross-border policy influences (or spill-overs) from foreign idiosyncratic stringency shocks. I show that comparing the two model specifications can bring relevant policy insights. Moreover, since many of the statistically significant cross-border effects of policy shocks are in the economic realm, I think there is a need for better risk-sharing arrangements at the international level, as advocated by Acharya et al. (2020).

Within the limits of the existing dataset, the main utility of my exercise consists in pointing when and where the good (as well as the bad) policy initiatives can be uncovered. A simple example is provided for U. K., allowing readers to match the size of the identified policy shocks with relevant events timeline. Once the exact date is known, this type of identification can trigger more detailed investigations that can help narrowing down the large set of interventions and policies enacted (or changed). Although I run a comparison exercise based on model estimates from recent data, the policy insights I derive can be very relevant. Within the limits of my dataset (timespan and sample), the results of this exercise show that few countries, among which Japan, Korea and Australia, can offer valuable examples of policy interventions that can help others improve their trade-offs. On the other hand, Italy and U.S. offer policies examples that point in the wrong direction, since no country can improve any of the two relevant dimensions of its trade-off by following exactly the same policy prescriptions. One of the best possible learners in this dataset is Germany, which can improve its ‘wealth versus health’ trade-off along both its dimensions, via policy emulation from others. Because policy emulation here merely reveals a high, empirically relevant sensitivity to foreign policy shocks, learning can be seen as an artefact of my exercise that is not guaranteed to have occurred in reality. More in depth analysis is required to uncover the specific institutional and programme details that in reality made the difference between policy successes and policy mistakes.

Funding

This work has not benefited from any funding.

Declaration of Competing Interest

The authors report no declarations of interest.

Acknowledgements

I am grateful to the responsible editor, Joerg Baten, and to the two anonymous reviewers who provided me with several suggestions that helped improve the content, and made the presentation of the results more straightforward. Many of the Matlab codes used in the analysis leverage greatly on the GVAR toolbox written by Alessandro Galesi and Vanessa Smith, available from: https://sites.google.com/site/gvarmodelling/gvar-toolbox.

Appendix A. Supplementary data

Supplementary material related to this article can be found, in the online version, at doi:https://doi.org/10.1016/j.ehb.2021.101052.

References

Acharya, V.V., Jiang, Z., Richmond, R., von Thadden, E.L., 2020. Divided We Fall: International Health and Trade Coordination During a Pandemic. NBER, p. 28176. Working Paper.

Alesina, A., Giuliano, P., 2015. Culture and institutions. Journal of Economic Literature 53 (4), 898–944.

Altig, D., Baker, S., Barrero, J.M., Bloom, N., Bunn, P., Chen, S., Davis, S.J., Leather, J., Meyer, B., Mihaylov, E., Mizen, P., Parker, N., Renault, S., Smetsanka, T., Thwaites, G., 2020. Economic uncertainty before and during the COVID-19 pandemic. Journal of Public Economics 191, 104274.

Amuedo-Dorantes, C., Borra, C., Rivera-Garrido, N., Sevilla, A., 2021. Early Adoption of Non-Pharmaceutical Interventions and COVID-19 Mortality. Economics & Human Biology, 101005.

Baker, S.R., Bloom, N., Davis, S.J., Kost, K., Sammon, M., Viratyosin, T., 2020. The Unprecedented Stock Market Reaction to COVID-19. NBER. https://doi.org/10.3865/w26945. WP 26945.

Bargain, O., Aminjonov, U., 2020. Trust and compliance to public health policies in times of COVID-19. Journal of Public Economics 192, 104316.

Bove, V., Gokmen, G., 2018. Genetic distance, trade, and the diffusion of development. Journal of Applied Econometrics 33 (5), 727–747.

Dees, S., Mauro, F.D., Pesaran, M.H., Smith, L.V., 2007. Exploring the international linkages of the euro area: a global VAR analysis. Journal of applied econometrics 22 (1), 1–38.

Dragomirescu-Gaina, C., Philippas, D., 2015. Strategic insights of fiscal policies in Europe: A global VAR perspective. Journal of international Money and Finance 59, 43–65.

Eichenbaum, M.S., Rebelo, S., Trabandt, M., 2020. The macroeconomics of epidemics. NBER Working Paper Series, No. 26882.

Farzanegan, M.R., Ghoolipour, H.F., Feizi, M., Nunkoo, R., Andargoli, A.E., 2021. International tourism and outbreak of coronavirus (COVID-19): A cross-country analysis. Journal of Travel Research 60 (3), 687–692.

Fennell, A.A., Grossbard, S., 2020. Intergenerational residence patterns and COVID-19 fatalities in the EU and the US. Economics & Human Biology 39, 100934.

Frazzini, S., Mishra, S., Gandy, A., Unwin, H.L.T., Mellan, T.A., Coupland, H., Whitaker, C., Zhu, H., Berah, T., Eaton, J.W., Monod, M., 2020. Estimating the effects of non-pharmaceutical interventions on COVID-19 infections in Europe. Nature 584 (7820), 257–261.

Frey, C.B., Chen, C., Presidente, G., 2020. Democracy, Culture, and Contagion: Political Regimes and Countries Responsiveness to Covid-19. Covid Economics 18, 1–20.

Hale, T., Angrist, N., Bøty, T., Cameron-Blake, E., Hallas, L., Kira, B., Majumdar, S., Petherick, A., Phillips, T., Tatlow, H., Webster, S., 2020. Variation in government responses to COVID-19, Version 10. December 10, 2020. Bavarian School of Government. Working Paper.

La Rosa, G., Mancini, P., Ferraro, G.B., Veneri, C., Iaconelli, M., Bonadonna, L., Lucentini, L., Suffredini, E., 2021. SARS-CoV-2 has been circulating in northern Italy since December 2019: Evidence from environmental monitoring. Science of the Total Environment 750, 141711. https://doi.org/10.1016/j.scitotenv.2021.141711.

Mali, F., 2021. COVID-19 outbreak, social response, and early economic effects: a global VAR analysis of cross-country interdependencies. Journal of Population Economics 34 (1), 223–252.

OECD, 2020. The OECD Weekly Tracker of activity based on Google Trends, in: Issues notes on current policy challenges. OECD Publishing, Paris. https://doi.org/10.1787/133218ce-en.

Persson, T., Tabellini, G., 2009. Democratic capital: The nexus of political and economic change. American Economic Journal: Macroeconomics 1 (2), 88–126.

Pesaran, M.H., Schuermann, T., Weiner, S.M., 2004. Modeling regional interdependencies using a global error-correcting macroeconometric model. Journal of Business & Economic Statistics 22 (2), 129–162.

Schmelz, K., 2021. Enforcement may crowd out voluntary support for COVID-19 policies, especially where trust in government is weak and in a liberal society. Proceedings of the National Academy of Sciences 118 (9).

Spolaore, E., Wacziarg, R., 2009. The macroeconomics of epidemics. Quarterly Journal of Economics 124 (2), 469–529.

Tripathy, S.S., Bhatia, U., Mokhanty, M., Karmakar, S., Ghosh, S., 2021. Flood evacuation during pandemic: a multi-objective framework to handle compound hazard. Environmental Research Letters 16 (3), 034034.

Trudeau, J.M., Alcea-Planas, J., Vázquez, W.F., 2020. The value of COVID-19 tests in Latin America. Economics & Human Biology 39, 100931.

Van der Gaag, E., Zuidweg, R., Zijlstra, M., 2020. Why do countries respond differently to COVID-19? A comparative study of Sweden, China, France, and Japan. The American Review of Public Administration 50 (6-7), 762–769.

Zimmermann, K.F., Karabulut, G., Huseyin Bilgin, M., Cansin Doker, A., 2020. Inter-Country Distancing, Globalization and the Coronavirus Pandemic. The World Economy. https://doi.org/10.1111/twec.12969.

Woloszko, N., 2020. A Weekly Tracker of activity based on machine learning and Google Trends. OECD Economics Department Working Papers 1634. OECD Publishing, Paris.