COVID-19 anti-contagion policies and economic support measures in the U.S.

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ABSTRACT

Current literature assumes that non-pharmaceutical interventions (NPIs) reduce COVID-19 infections irrespective of their strength. The potential role of Economic Support Measures (ESM) towards controlling the virus is also overlooked. If anything, NPIs are more likely to control infections when economic support is in place. Using a panel threshold model of COVID-19 cases in U.S. states, we identify three distinct regimes of 'low', 'medium' and 'high' severity interventions; the latter being more effective towards reducing infections. The implemented NPIs (ESM) reduce the daily average percentage growth of infections by 21.4% (2.4%) compared to the case where no government action is taken.

Keywords: COVID-19, NPIs, Economic Support Measures, Panel Threshold model

JEL Classification: H51; C33; C51

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«Ἤρξατο δὲ τὸ πρῶτον, ὡς λέγεται, ἐξ Αἰθιοπίας τῆς ὑπὲρ Αἰγύπτου, ἔπειτα δὲ καὶ ἐς Αἴγυπτον καὶ Λιβύην κατέβη καὶ ἐς τὴν βασιλέως γῆν [Περσία] τὴν πολλήν.»

"It first began, it is said, in the parts of Ethiopia above Egypt, and thence descended into Egypt and Libya and into most of the King's country [i.e. Persia]"

Thucydides, 5th century B.C.

1. Introduction

The COVID-19 respiratory infection, caused by the SARS-CoV-2 virus first detected in Wuhan in late 2019, is continuing to spread globally with more than 103.3 million infections and 2.2 million deaths (World Health Organization, WHO; https://covid19.who.int/). Due to the virus rapid spread, Dr. Tedros Adhanom, WHO Director-General, declared COVID-19 a pandemic on March 11th, 2020. From the Great Plague of Athens (the first historically recorded epidemic in 430 B.C.) to the Black Death (the deadliest pandemic in the 14th century estimated to have killed 30% to 60% of Europe's population), humanity has faced several such fatal outbreaks. The most recent example of this magnitude is the 1918-19 influenza pandemic (the so-called "Spanish flu").

Lessons from previous pandemics reveal that timeliness and stringency are crucial aspects for maximizing the effectiveness of non-pharmaceutical interventions (NPIs) and minimizing the adverse social and economic consequences (Hatchett *et al.*, 2007; Martin *et al.*, 2007; Dasgupta *et al.*, 2020). Using historical data on the timing of 19 different types of NPIs in 17 U.S. cities during the Spanish flu pandemic, Hatchett *et al.* (2007) show that implementation of multiple interventions at an early phase of the epidemic reduced peak death rates at a substantial magnitude (~50%). Statistical and epidemiological analyses of past data from several U.S. cities also demonstrate a strong association between early, sustained, and layered application of public health measures in mitigating the consequences of the 1918-19 influenza pandemic in the U.S. (Martin *et al.*, 2007).

The U.S. is among the countries more severely hit by the COVID-19 pandemic. With more than 26 million coronavirus cases and 439,000 deaths, the U.S. has the highest number of confirmed infections and the highest official death toll in the world (WHO; https://covid19.who.int/). The first cases of COVID-19 occurred in January 2020 in travelers from China. Early travel restrictions imposed on February 2nd to non-U.S. citizens from China (later expanding to other countries with widespread transmission) failed to contain the virus, as the number of COVID-19 cases increased more than 1,000-fold during a three-week period in late February to early March (Schuchat, 2020). The early epicenter was New York and the Northeastern states (New Jersey, Connecticut, Massachusetts), where cases spiked in late March. Social distancing restrictions brought infections down; however, their gradual relaxation led to new outbreaks, shifting to the South and West regions of the country (i.e. Arizona, Florida, and California) and leading to a new countrywide peak in July. The U.S. is still struggling with the pandemic, with fast-moving outbreaks in North Dakota, South Dakota, and Wisconsin.

In the absence of a centralized federal response, there has been extreme variability in the timing and intensity of interventions in the U.S. states, and even at a county and city-level (Adolph *et al.*, 2020). Measures started being implemented only after March 10th, 13 days after the first report of community transmission. California was the first state to enact a lockdown, followed by the Midwest and parts of the Northeast, as well as Louisiana. Later adopters were largely concentrated in the Mid-Atlantic and upper Midwest. By April 20th, 40 out of the 50 states had adopted state-wide lockdowns. Dave *et al.* (2021) estimate a decline of up to 43.7% in COVID-19 cases three weeks after the implementation of state-wide quarantine, with significant heterogeneity in the response based on timing of the enactment and state characteristics. The social distancing effect of lockdown is estimated to be twice as large for early as compared to later-adopting states (2.6% vs 1.3%). The analysis of Dave *et al.* (2021) provides strong evidence that state-wide lockdowns are far more effective at decreasing the rate of coronavirus cases (including declines in the rate of COVID-19-related mortality) among early adopting states and states with higher population densities.

Chernozhukov *et al.* (2021) use data on confirmed COVID-19 cases and deaths for the U.S. states to estimate panel data models and find that nationally mandating face masks for employees early in the pandemic could have reduced the weekly growth rate of cases and deaths by more than 10 percentage points in late April 2020 and could have led to as much as 19% to 47% less deaths nationally by the end of May 2020, which roughly translates into 19,000 to 47,000 saved lives. Their findings also suggest that in the absence of stay-at-home orders, cases would have been larger by 6% to 63% and without business closures, cases would have been larger by 17% to 78%.

Drastic anti-contagion policy actions such as national lockdowns, though effective, lead to unprecedented negative economic impact. The U.S. economy experienced its deepest decline since official record keeping in 1947; indeed U.S. GDP shrank by an annualized rate of 32.9% in the second quarter of 2020 (https://fred.stlouisfed.org/). Using high-frequency proxy measures of economic activity (e.g. NO_X emissions) for Europe and Central Asia, Demirgüç-Kunt *et al.* (2020) find that national lockdowns are associated with a decline in economic activity of around 10%. This economic cost puts governments under enormous pressure to relax the intensity of NPIs. Consequently, understanding the exact pairwise relationship between NPIs and the spread of COVID-19 (considering issues such as threshold effects and model misspecification) is important for governments to timely plan effective short-run interventions to tame infections and, at the same time, minimize the adverse impact on economic activity.

However, the current fast-growing literature assesses the effect of NPIs by hypothesizing a homogeneous impact, irrespective of their strength (e.g. Hsiang *et al.* 2020; Haug *et al.* 2020). Moreover, the role of the deployed economic support measures (ESM) is largely overlooked. Here, we revel the heterogeneous relationship between NPIs and the growth of COVID-19 confirmed cases, conditioning on a set of variables such as ESM and climatic conditions. To do so, we use U.S. state-level data, and transform all variables through backward- or forward-looking rolling averages, thus accounting, to a certain extent, for errors in data measurement and most importantly for the endogenous nature of NPIs and COVID-19 infections. Moreover, as omitted variable bias may lead to invalid inferences, we estimate an augmented specification by including the ESM and the prevailing climatic conditions (temperature and relative humidity).[1] Indeed, in the presence of ESM, government interventions are likely to become more effective in bringing infection cases down. This is because employees, and the public in general, are more likely than not to stick to government intervention measures when economic support is in place. Finally, after ensuring that the estimated specification is robust against the typical violations of the error's sphericity assumption (heteroskedasticity, serial correlation and cross-sectional dependence), we identify the critical level of NPIs over which the growth rate of infections turns negative.

By fitting a two-threshold panel fixed-effects specification, we reach a number of findings. First, the impact of government NPIs on infections growth is significant and varying, depending on the stringency level. We identify three distinct regimes, i.e. regimes of 'low', 'medium' and 'high' severity interventions. A 10% increase in the level of the average NPIs (averaged over the previous 14 days) lowers the daily growth rate of infections by 0.349% in the low regime, by 0.492% in the medium regime, and by 0.546% in the high regime. Second, the ESM for employees and the whole population in general are statistically significant in bringing COVID-19 cases growth down. Furthermore, a 10%

[1] As ESM are positively correlated with conducted government interventions, non-inclusion of these measures in the specification will lead to biased and inconsistent estimates.

increase in the average ESM (averaged over the previous 14 days) lowers the daily growth rate of infections by 0.060%. Third, we identify a negative and significant impact of climatic conditions (i.e. an increase in temperature and relative humidity) on the growth of COVID-19 cases. Fourth, counterfactual analysis shows that the actual conducted NPIs significantly reduced the daily average percentage growth of infections by 21.4 percentage points compared to the scenario of no government action. At the other extreme, had government NPIs remained at the highest level throughout the sample, the daily average growth of infections would have been lower by 4.9 percentage points compared to the impact of the actual conducted NPIs. Fifth, the implemented ESM reduced the average daily percentage growth of infections by 2.4 percentage points compared to the scenario where no ESM were put in place. Finally, we find that only NPIs classified at the "high" regime can trigger a negative growth rate of infections.

The paper proceeds as follows: section 2 discusses the data and model specification; section 3 reports and discusses the model estimates and section 4 presents our counterfactual analysis. Finally, section 5 concludes.

2. Data and Model Specification

Being recently available by the Blavatnik School of Government of the University of Oxford, we use data on NPIs and ESM across all U.S. states for the period spanning January 1 to August 4, 2020. In more detail, we focus on the 50 U.S. states using daily observations on (i) the strength of the NPIs policies at state level, proxied by the OxCGRT index (*source*: Blavatnik School of Government of the University of Oxford)[2], (ii) the strength of the ESM (*source*: Blavatnik School of Government of the University of Oxford)[2], (iii) the number of confirmed COVID-19 cases (*source*: Centers for Disease Control and Prevention-CDC)[3] and the state population estimates as of July 2019 (*source*: United States Census Bureau)[4], in order to construct the number of daily cases per 100,000, (iv) the temperature (*source*: NASA Langley Research Center - LaRC, POWER Project) and (v) the relative humidity (*source*: NASA Langley Research Center - LaRC, POWER Project).[5]

[2] Blavatnik School of Government of the University of Oxford, see: https://www.bsg.ox.ac.uk/
[3] Centers for Disease Control and Prevention, see: https://www.cdc.gov/
[4] United States Census Bureau, see: https://www.census.gov/
[5] For temperature and relative humidity data, see NASA Langley Research Center, POWER Project, https://power.larc.nasa.gov/data-access-viewer/

To minimize the consequences of endogeneity and measurement error (Raftery *et al.* 2020), the variables are transformed through *forward-* or *backward-looking* rolling averages using a fixed window length.[6] We define the *forward-looking* transformation of a variable, at each time t, as the average value calculated by a fixed length rolling window with size equal to the 14 succeeding days ($t+1$ up to $t+14$).[7] Similarly, we define the *backward-looking* transformation by using the preceding fourteen days ($t-1$ up to $t-14$). In more detail, we first calculate the COVID-19 infections per 100,000 people and we then define, for each time t of the total sample, the *forward-looking* confirmed infections per 100,000 as the average of the succeeding 14 days. Based on the above transformation, we estimate the respective growth rate as the logarithmic difference of two subsequent observations. The *forward-looking* growth rate of infections per 100,000 (*growth of infections*, hereafter) for selected dates of the sample at state level is illustrated as a heat map in Fig. 1. Likewise, we define for each time t of the sample, the *backward-looking* OxCGRT index (*OxCGRT*, hereafter) and the *backward-looking* ESM index (*ESM*, hereafter) as the respective average of the preceding 14 days. The *OxCGRT* index for selected dates of the sample at state level is illustrated as column bars in Fig. 1 (in Fig. 1, each of the three regimes is signified with a different presentation colour; the estimation of the regimes is discussed in Section 3).

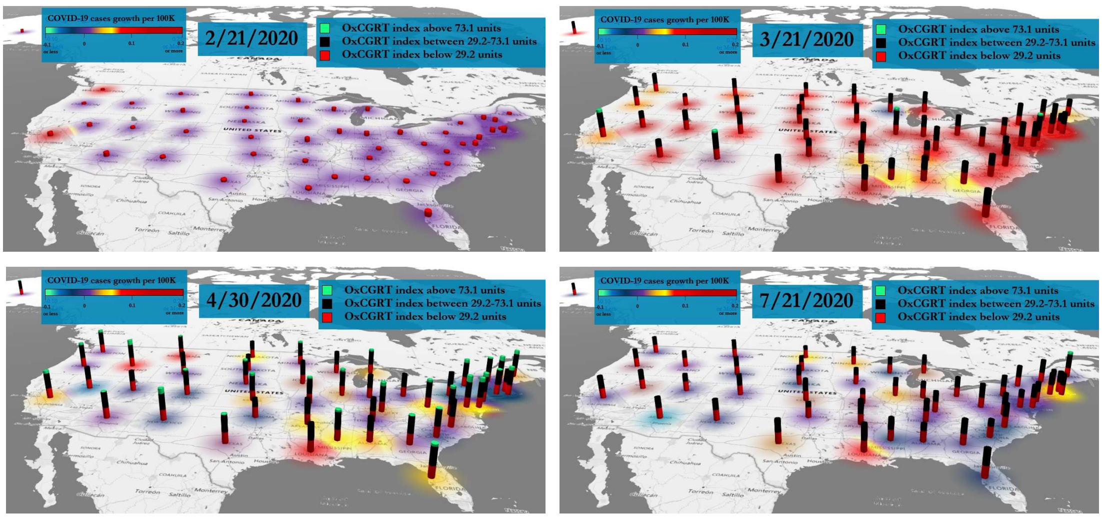

Fig. 1| *OxCGRT* index and growth rate of COVID-19 cases per 100,000

Notes: The time-lapse version of the figure is available at: **https://youtu.be/EXCo6LZd4w8**

[6] Dasgupta *et al.* (2020) note that under-reporting infectious disease statistics is a common characteristic of the current pandemic and the 1665 London plague 350 years ago.

[7] The window size is set to 14 days. Lauer *et al.* (2020) estimate that the virus incubation period is 14 days.

The ESM for selected dates of the sample at a state level is illustrated as column bars in Fig. 2 (Fig. 2, presents jointly as a heat map the *growth of infections* per 100,000). We finally define, for each time, t, of the total sample, the *backward-looking* temperature, as well as the *backward-looking* relative humidity. The *backward-looking* temperature variable for selected dates of the sample at state level is illustrated as column bars in Fig. 3 (Fig 3, presents, as a heat map, the *growth of infections* per 100,000, while the reflection of the temperature column bar implies negative temperatures). The *backward-looking* relative humidity variable for selected dates at a state level is illustrated as column bars in Fig. 4 (Fig. 4, presents jointly, as a heat map, the *growth of infections* per 100,000).

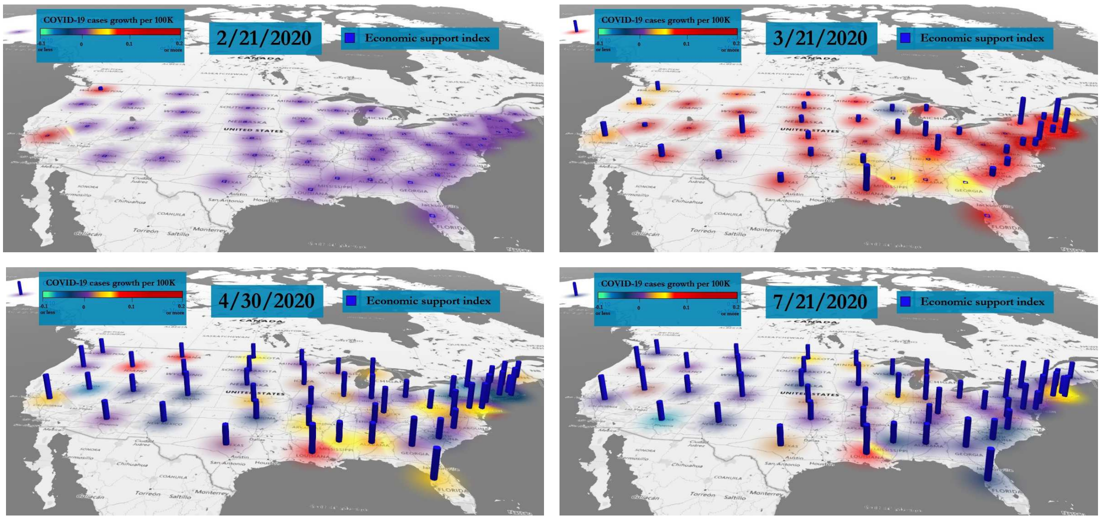

Fig. 2 | *ESM* index and growth rate of COVID-19 cases per 100,000

Notes: The time-lapse version of the figure is available at: **https://youtu.be/xM6x4PS24YE**

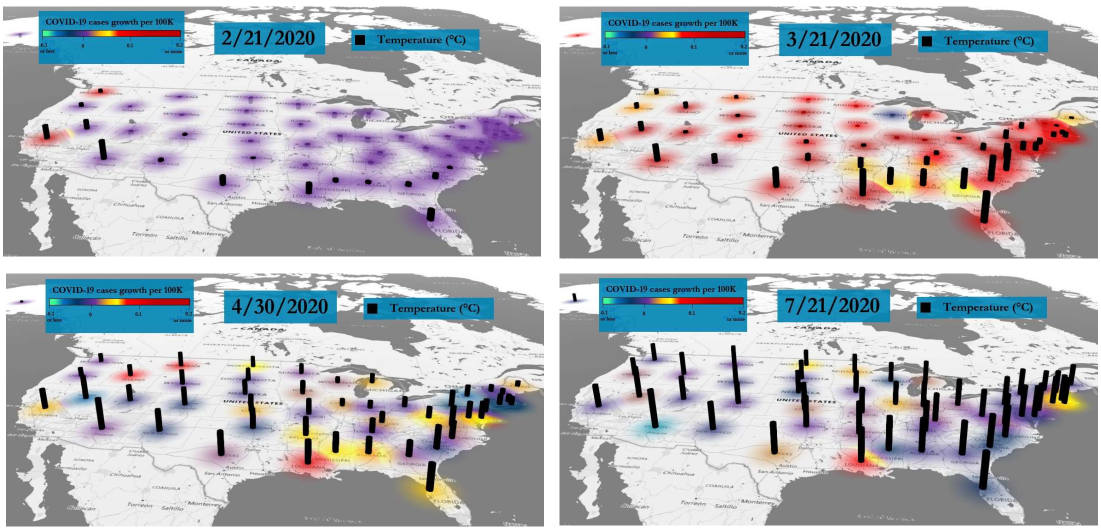

Fig. 3 | Temperature and growth rate of COVID-19 cases per 100,000

Notes: The time-lapse version of the figure is available at: **https://youtu.be/Kc9V-GTyn2I**

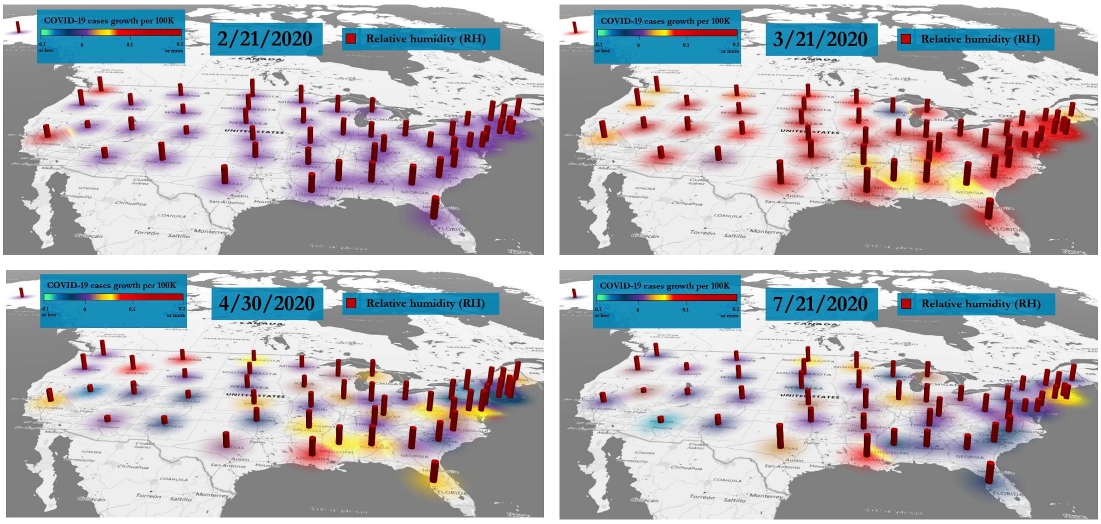

Fig. 4| Humidity and growth rate of COVID-19 cases per 100,000

Notes: The time-lapse version of the figure is available at: **https://youtu.be/1xZQ8MVTPgk**

For all constructed *forward-* and *backward-looking* variables, we define the *effective sample* for each U.S. state as the period signified by the first day with cumulative confirmed cases equal or greater than five, up to the end of the sample. Such treatment leads to a different *effective sample* in terms of time length for each U.S. state (the maximum sample length with 170 observations corresponds to California, while the minimum sample length with 126 observations corresponds to Alaska, Hawaii, North Dakota, and West Virginia). As the fixed-effect panel threshold model necessitates a balanced sample, we use, from the effective sample of each U.S. state, the first 126 observations. Hence, our final *feasible sample* (balanced sample) includes 126 observations for each U.S. state.

Current literature (in the context of Susceptible-Infected-Recovered epidemiological models) assesses the effect of NPIs on COVID-19 infections (or deaths) assuming a homogeneous impact of these interventions irrespectively of their strength (see Hsiang *et al.* 2020; Haug *et al.* 2020; Flaxman *et al.* 2020; Brauner *et al.* 2020). Under this strong assumption, any attempt to evaluate the exact effect of NPIs at their different levels is arguably misspecified. To overcome this limitation, we estimate for the 50 U.S. states a panel fixed-effect threshold specification (Hansen, 1999), which remains robust to time-invariant differences among the states (e.g. population density) and reveals the heterogeneous nature of the relationship between infections and NPIs. Moreover, as ESM are positively correlated with conducted government interventions, non-inclusion of these measures in the specification will lead to biased and inconsistent estimates. To reduce the impact of specification bias, the employed model is augmented with the inclusion of the *ESM* index and two climate variables (temperature and relative humidity). The model specification takes the form:

$$r_{it} = \delta + \vartheta_1 p_{it} \mathrm{I}(p_{it} < k_1) + \vartheta_2 p_{it} \mathrm{I}(k_1 \leq p_{it} < k_2) + \vartheta_3 p_{it} \mathrm{I}(k_2 \leq p_{it}) + \varphi \mathbf{z}_{it} + u_i + e_{it} \quad (1)$$

where, r_{it} is the *forward-looking* growth rate of infections per 100,000, δ and ϑ_j are parameters to be estimated $(j = 1,2,3)$, k_m are the threshold parameters $(m = 1, 2)$, p_{it} is the natural logarithm of the *backward-looking* OxCGRT index (threshold variable), $\mathrm{I}(\bullet)$ is an indicator function which receives the value one if the condition in the parenthesis is true and zero otherwise, $\mathbf{z}_{it}$ is the matrix of the threshold independent variables (the natural logarithm of the *backward-looking* ESM and the two *backward-looking* climate variables), φ is a vector of coefficients, u_i is the state individual effect and e_{it} is the error term.

3. Threshold Testing and Estimation

To identify the number of significant thresholds for the *OxCGRT* index based on our benchmark econometric specification (*eq*. 1), we implement the sequential testing approach proposed by Hansen (1999). Thus, for testing sequentially the null hypotheses of zero, one and two thresholds, we calculate the respective likelihood ratio F_j statistics $(j = 1, 2, 3)$, which follow a non-standard asymptotic distribution. To perform an inferential decision, within a bootstrap framework, we calculate *p*-values based on the empirical sampling distribution, which prove to remain valid asymptotically (Hansen, 1999). The three F_j $(j = 1, 2, 3)$ statistics, along with the associated critical values at the conventional levels of significance and the bootstrapped *p*-values (with 1000 replications), are analytically reported in Table 1.

Table 1. Testing for threshold effects within a panel fixed-effects specification

Threshold	Threshold estimate	Threshold at level	*F*-stat	*p*-value	10% critical	5% critical	1% critical
Single	4.292***	73.149***	73.97	0.002	37.174	43.019	60.213
Double	3.375***	29.230***	59.25	0.003	29.485	34.552	48.841
Triple	3.753	42.657	41.15	0.283	55.603	65.412	91.846

Notes: *** denotes the rejection of the null hypothesis over the alternative at the 0.01 significance level. All trimming values are set equal to 0.1. The reported critical values along with the respective *p*-values are derived by implementing the bootstrap method with 1,000 replications. As the threshold variable is transformed in logarithmic form, each threshold estimate is converted to the level scale by simply calculating the anti-log.

Table 1, implies that the null hypothesis of zero thresholds against one threshold (p=0.002) is rejected. We proceed by examining the null hypothesis of one threshold against two. The respective inference (p=0.002) rejects the second null hypothesis, thus providing support for the presence of two thresholds. Finally, to discriminate between the presence of two or three thresholds, we test the third null hypothesis of two thresholds in favour of three. The resulting evidence (p=0.283) fails to reject the null hypothesis, signalling the existence of two significant thresholds. The point estimates for the two significant thresholds of the *OxCGRT* index are shown in Table 1. The first threshold estimate is 73.1 units (4.292 for the logarithmic transformation) and the second threshold estimate is 29.2 units (3.375 for the logarithmic transformation). Hence, the three resulting regimes range between [0-29.2), [29.2-73.1) and [73.1-100]. For our sample, Fig. 5 shows the two estimated thresholds (the first and second thresholds are signified by the pink and grey surface, respectively) along with the actual OxCGRT index values in a three-dimensional coordinate system.

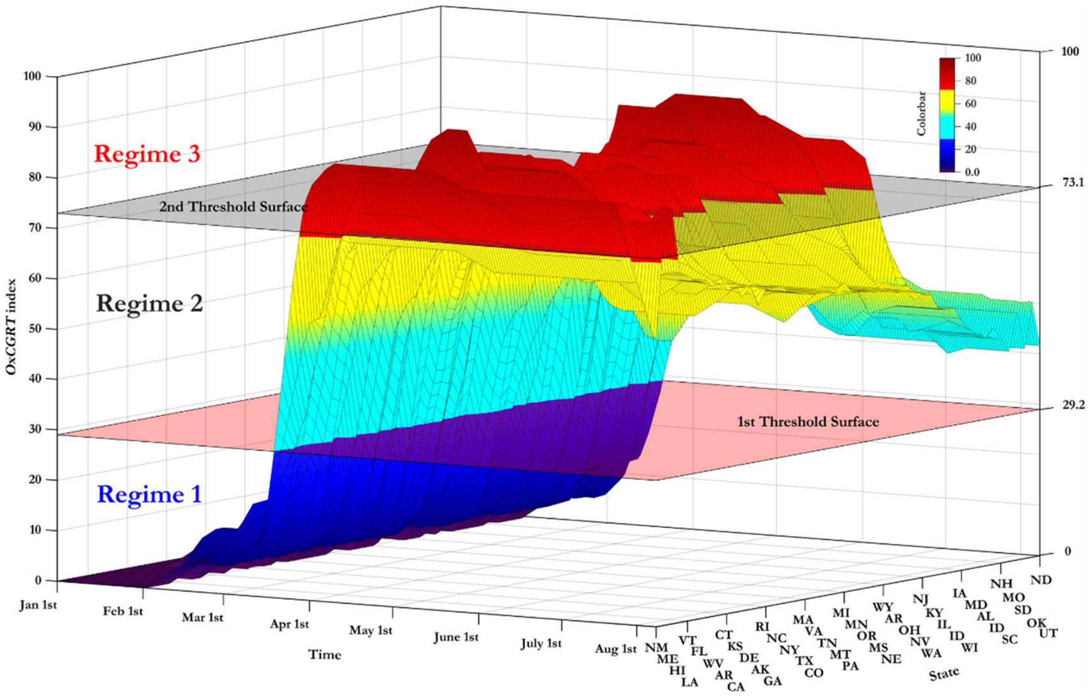

Fig. 5 | *OxCGRT* index and estimated regimes

Notes: (i) The vertical left-axis depicts the stringency of the *OxCGRT* index; the bottom horizontal left-axis displays the date, and the bottom horizontal right-axis depicts the state by using the two-digit code abbreviation.

(ii) The two-digit state abbreviations are: Alabama: AL, Alaska: AK, Arizona: AZ, Arkansas: AR, California: CA, Colorado: CO, Connecticut: CT, Delaware: DE, Florida: FL, Georgia: GA, Hawaii: HI, Idaho: ID, Illinois: IL, Indiana: IN, Iowa: IA, Kansas: KS, Kentucky: KY, Louisiana: LA, Maine: ME, Maryland: MD, Massachusetts: MA, Michigan: MI, Minnesota: MN, Mississippi: MS, Missouri: MO, Montana: MT, Nebraska: NE, Nevada: NV, New Hampshire: NH, New Jersey: NJ, New Mexico: NM, New York: NY, North Carolina: NC, North Dakota: ND, Ohio: OH, Oklahoma: OK, Oregon: OR, Pennsylvania: PA, Rhode Island: RI, South Carolina: SC, South Dakota: SD, Tennessee: TN, Texas: TX, Utah: UT, Vermont: VT, Virginia: VA, Washington: WA, West Virginia: WV, Wisconsin: WI, Wyoming: WY.

(iii) The first and second thresholds of the *OxCGRT* index are signified by the pink and grey surface, respectively.

(iv) The surface for the *OxCGRT* index is colored based on the range of values assigned to each regime.

(v) Out of the 6300 observations for the *OxCGRT* index, across the 50 U.S. states, 8%, 62% and 30% of these are classified into the 'low' regime, 'medium' and 'high' regime, respectively.

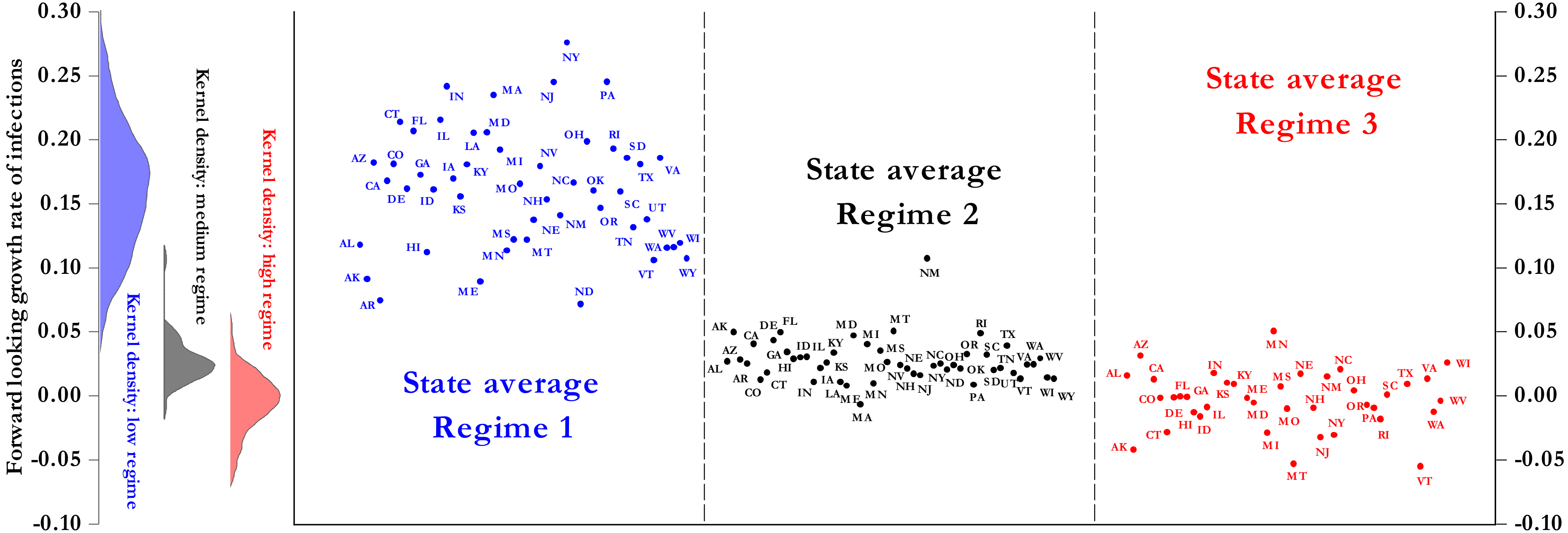

Fig. 6| Regime-dependent average forward-looking growth rate of infections per U.S. state

Notes: (i) Regimes 1, 2 and 3 are defined by the values of the threshold variable (the *backward-looking* OxCGRT index) that belong to [0-29.2), [29.2-73.1) and [73.1-100], respectively.
(ii) Regime 1 and Regime 2 include all 50 U.S. states while Regime 3 includes 39 U.S. states. The states that never have entered into Regime 3 are the following: Arkansas, Iowa, Louisiana, Massachusetts, Nevada, North Dakota, Oklahoma, South Dakota, Tennessee, Utah and Wyoming.
(iii) The kernel densities for the *forward-looking* growth rate of infections belonging in each regime, are presented at the left axis. Kernel density is a non-parametric approach for estimating the probability density function of a variable.
(iv) The two-digit state abbreviations are: Alabama: AL, Alaska: AK, Arizona: AZ, Arkansas: AR, California: CA, Colorado: CO, Connecticut: CT, Delaware: DE, Florida: FL, Georgia: GA, Hawaii: HI, Idaho: ID, Illinois: IL, Indiana: IN, Iowa: IA, Kansas: KS, Kentucky: KY, Louisiana: LA, Maine: ME, Maryland: MD, Massachusetts: MA, Michigan: MI, Minnesota: MN, Mississippi: MS, Missouri: MO, Montana: MT, Nebraska: NE, Nevada: NV, New Hampshire: NH, New Jersey: NJ, New Mexico: NM, New York: NY, North Carolina: NC, North Dakota: ND, Ohio: OH, Oklahoma: OK, Oregon: OR, Pennsylvania: PA, Rhode Island: RI, South Carolina: SC, South Dakota: SD, Tennessee: TN, Texas: TX, Utah: UT, Vermont: VT, Virginia: VA, Washington: WA, West Virginia: WV, Wisconsin: WI, Wyoming: WY.

Given the two estimated thresholds and the resulting three regimes, Fig. 6 shows, how the average *growth of infections* per U.S. state is distributed across each regime. It becomes clear that the average *growth of infections* decreases as the regime level increases, confirming the validity of the estimated thresholds.

For the balanced *feasible sample*, we fit a fixed-effects panel specification with two thresholds by implementing the typical fixed-effects estimator (*eq.* 1). These estimates, along with the associated OLS standard errors, are presented in the first column (FE) of Table 2. The second column of Table 2 illustrates the same estimates but with bootstrapped standard errors this time. Clearly the bootstrapped standard errors appear to be considerably higher. This difference in magnitude reveals the necessity of diagnostic testing.

Table 2. Threshold panel fixed-effects estimation results

Variable	FE (1)	Bootstrapped (2)	Driscoll-Kraay (3)	FGLS (4)	PCSE (5)
Constant	0.3736 ***	0.3736 ***	0.3736 ***	0.3451 ***	0.3307 ***
	(0.0197)	(0.0831)	(0.0559)	(0.0148)	(0.0350)
Hum	- 0.0008 ***	- 0.0008 ***	- 0.0008 ***	- 0.0004 ***	- 0.0008 ***
	(0.0002)	(0.0003)	(0.0002)	(0.0001)	(0.0002)
Tem	- 0.0008 ***	- 0.0008 **	- 0.0008 ***	- 0.0003 **	- 0.0005 **
	(0.0002)	(0.0004)	(0.0002)	(0.0001)	(0.0002)
ESM	- 0.0050 ***	- 0.0050	- 0.0050 *	- 0.0060 ***	- 0.0063 ***
	(0.0015)	(0.0038)	(0.0027)	(0.0010)	(0.0023)
Regime slopes					
$OxCGRT_{R1}$	- 0.0492 ***	- 0.0492 *	- 0.0492 **	- 0.0536 ***	- 0.0367 ***
	(0.0058)	(0.0259)	(0.0192)	(0.0038)	(0.0092)
$OxCGRT_{R2}$	- 0.0635 ***	- 0.0635 ***	- 0.0635 ***	- 0.0639 ***	- 0.0516 ***
	(0.0046)	(0.0204)	(0.0148)	(0.0031)	(0.0075)
$OxCGRT_{R3}$	- 0.0684 ***	- 0.0684 ***	- 0.0684 ***	- 0.0673 ***	- 0.0573 ***
	(0.0044)	(0.0197)	(0.0143)	(0.0030)	(0.0073)
Summary Statistics					
n	6300	6300	6300	6300	6300
R^2-within	0.349	0.349	0.349	-	0.256
F/Wald X^2	0.000	0.000	0.000	0.000	0.000
Diagnostic testing for the FE specification					
Exogeneity (*p*-value)		0.498	Serial correlation (*p*-value)		0.000
Homoskedasticity (*p*-value)		0.000	CSD test (*p*-value)		0.045

Notes: ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 significance level, respectively. The reported values within the (.) are standard errors. *Hum*, *Tem* and *ESM* refer to the three regime-independent variables, that is, relative humidity, temperature and economic support measures, respectively. The subscripts R1, R2 and R3, linked with the *OxCGRT* signify the three regimes formed after the identification of significant thresholds (see Table 1). The columns titled as FE, Bootstrapped, Driscoll-Kraay, FGLS and PCSE refer to the threshold panel fixed-effects estimates (i) with typical standard errors, (ii) with bootstrapped standard errors, (iii) with the Driscoll and Kraay (1998) corrected standard errors (robust to heteroskedastic error as well as to general forms of cross-sectional and temporal dependence), (iv) with the use of the Feasible Generalized Least Squares approach (allowing robust estimation in the presence of serial correlation, heteroskedasticity and cross-sectional dependence) and (v) with the Panel Corrected Standard Errors estimation approach (correcting for serial correlation, heteroskedasticity and cross-sectional dependence, respectively).

Hence, we proceed by testing the benchmark fixed-effects panel specification for: (i) the exogeneity of the *OxCGRT* index, (ii) groupwise homoskedasticity, (iii) serial correlation, and (iv) cross-sectional independence. In more detail, the Durbin-Wu-Hausman statistic (see Durbin, 1954; Wu, 1973; Hausman, 1978), supports (p=0.498) that the *OxCGRT* index is exogenous. Additionally, we test for groupwise homoskedasticity by the modified Wald statistic (see Green, 2000). The respective evidence (p=0.000) implies that the error term violates the assumption of homoskedasticity. On top of the above violation, the error term appears to be serially correlated as the LM statistic (Born and Breitung, 2016) rejects the null hypothesis of uncorrelated residuals of first order. Finally, by implementing a parametric testing procedure for examining the cross-sectional independence of the residuals (Pesaran 2020), we find that these are cross-sectionally dependent (p=0.045) at the 0.05 significance level. Overall, the diagnostic testing reveals that the *OxCGRT* index is exogenous; nevertheless, it shows that the model suffers from heteroskedasticity, serial correlation and cross-sectional dependence.

As the executed diagnostic testing reveals the existence of a non-spherical error term, the initial fixed-effects estimates are expected to be inefficient and their associated standard errors biased, rendering all resulting inferences questionable. Hence, we re-estimate our specification by implementing approaches that are robust to the above-mentioned forms of misspecification. We continue by reporting estimates of the covariance matrix based on the Driscoll and Kraay (1998) approach, which delivers standard errors that remain robust to heteroskedasticity, as well as to general forms of cross-sectional and temporal dependence (column 3). As the mixing conditions to establish asymptotic consistency may not hold for the fixed-effects estimator (Vogelsang, 2012), we also present the Parks (1967) Feasible Generalized Least Squares estimates (FGLS) (column 4). Finally, provided that the FGLS estimator proves to perform poorly in finite samples, we report the Beck and Katz (1995) Panel-Corrected Standard Error (PCSE) estimation results (column 5).

The PCSE estimation results reveal that all explanatory variables are significant at the conventional levels of significance (mainly at the 0.01 level). Most importantly, the *OxCGRT* index, throughout its entire range, remains effective at decreasing the *growth of infections*, albeit with a different impact at each regime. Additionally, *ESM* prove negative and significant, a finding which also holds for the two climatic variables. Given the presence of the thresholds, the model fits the data satisfactorily, as judged by Fig. 7.a and 7.b, which show the raw actual values of the the *growth of infections* per U.S. state and the model's respective fitted values along with the 99% confidence interval.

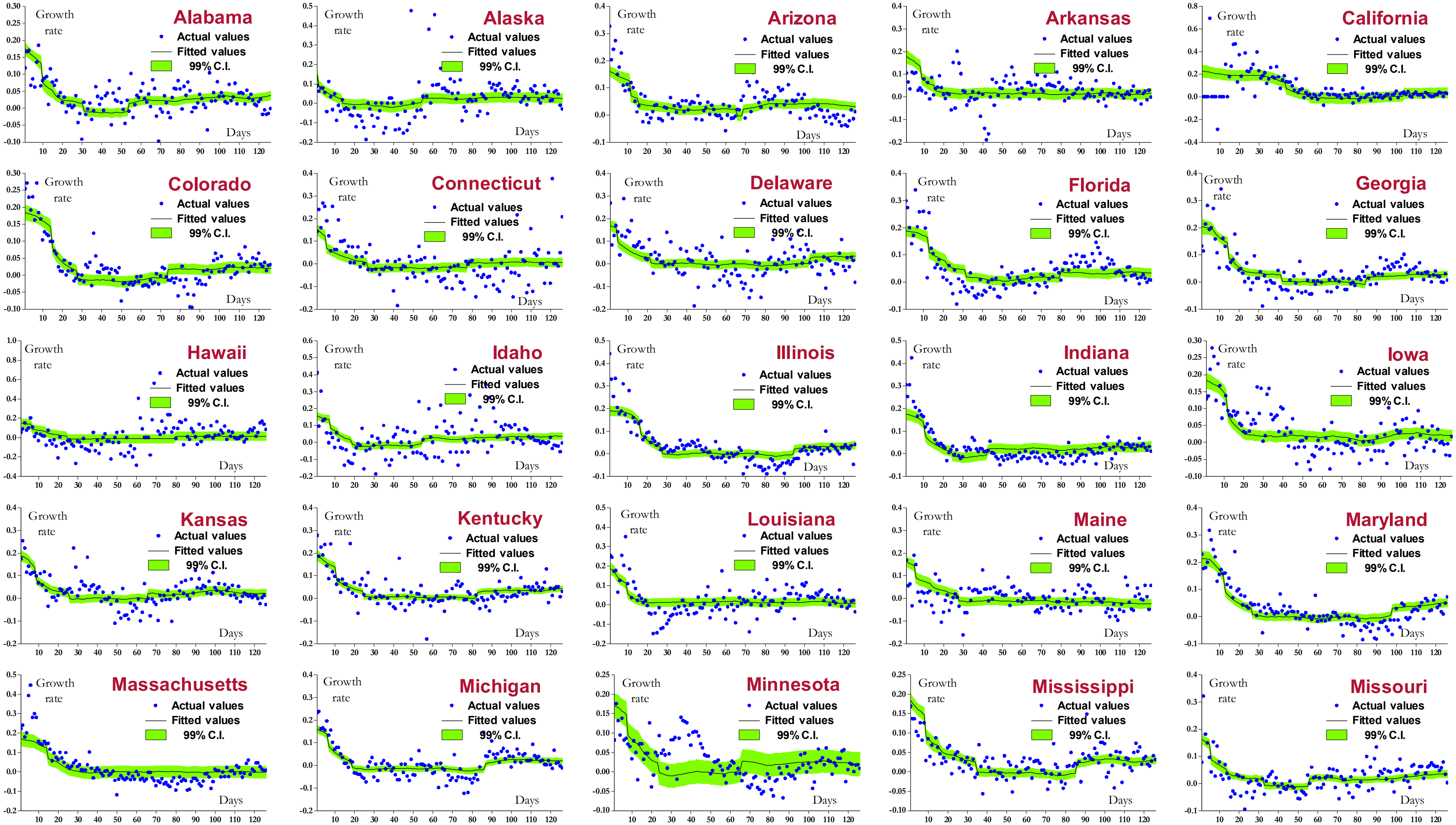

Fig. 7.a | COVID-19 cases growth rate per U.S. state: actual and fitted values along with respective 99% confidence interval

Notes: Estimates are based on the Threshold Panel Fixed-Effects model (*eq.* 1) using the Panel-Corrected Standard Errors (PCSE) estimation approach (see Table 2).

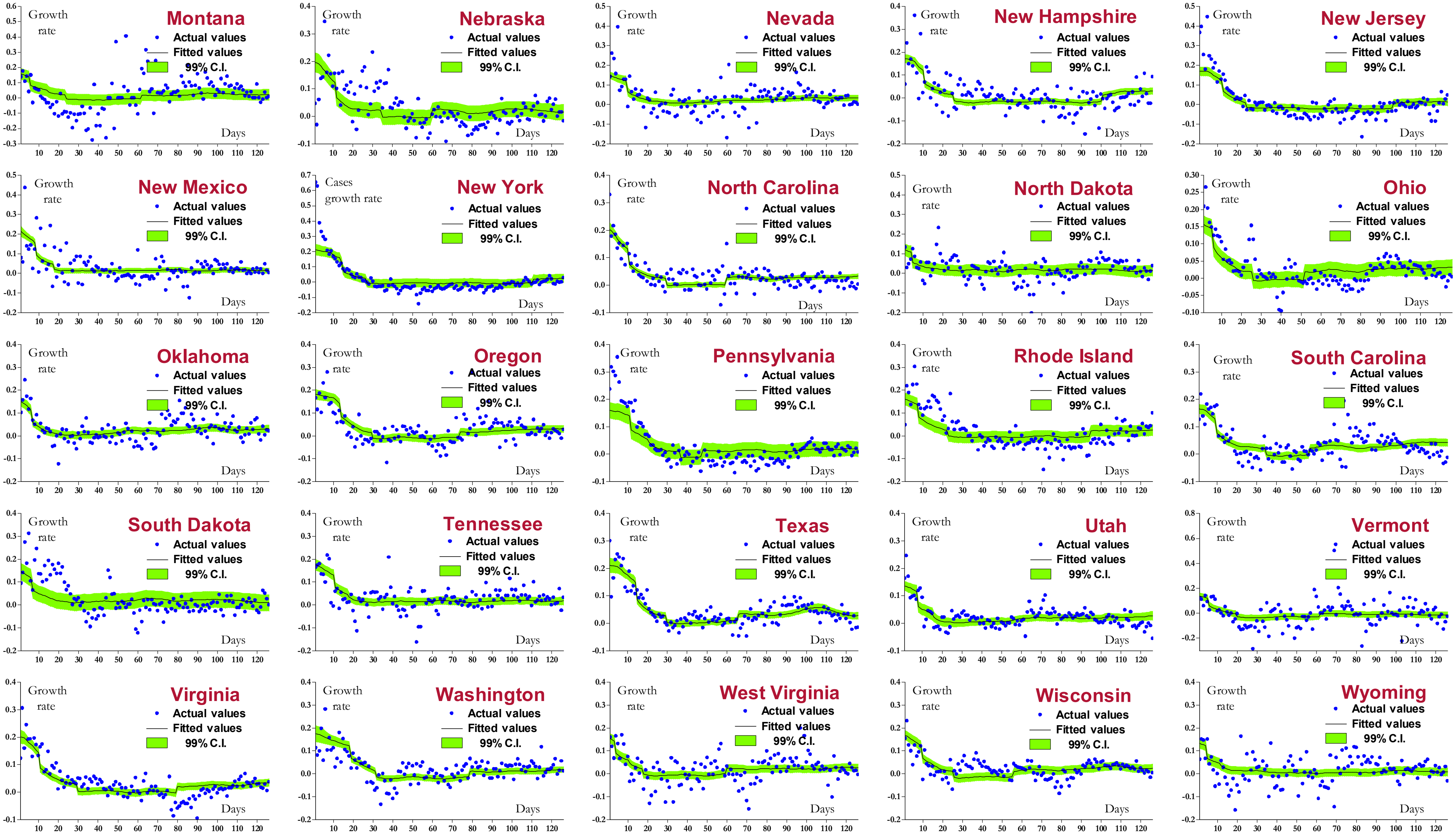

Fig. 7.b | COVID-19 cases growth rate per U.S. state: actual and fitted values along with respective 99% confidence interval

Notes: Estimates are based on the Threshold Panel Fixed-Effects model (*eq.* 1) using the Panel-Corrected Standard Errors (PCSE) estimation approach (see Table 2).

For the PCSE estimation results, we find that the *OxCGRT* index is negatively and significantly (*p-value*<0.01) related to the *growth of infections* at all regimes. More specifically, the regime dependent coefficients with the associated 95% confidence interval are -0.037 [-0.055, -0.019], -0.052 [-0.066, -0.037], and -0.057 [-0.072, -0.043] for the 'low', 'medium' and 'high' regime, respectively. The coefficient for the 'low' regime ('medium' regime), ['high' regime] suggests that a 10% increase in the level of the *OxCGRT* index lowers the daily percentage *growth of infections*, on average, by 0.35%, (0.49%), [0.55%]. Overall, the *OxCGRT* index throughout its entire range remains effective at decreasing the *growth of infections*, albeit with a different impact at each regime.

Moreover, we find a significant (*p-value*<0.01) impact of the *ESM* on the *growth of infections*. *ESM* can be viewed as an important factor, since the population will more likely adhere to government intervention measures when combined with additional economic support. Indeed, *ESM* can partially mitigate the economic losses faced by employees and the whole population, following widespread lockdowns. The magnitude of the coefficient (Table 2) implies that a 10% increase in the *ESM* lowers the daily percentage *growth of infections*, on average, by 0.06%.

Finally, we identify a negative and statistically significant impact of the *backward-looking* temperature (*p-value*<0.05) and the *backward-looking* relative humidity (*p-value*<0.01) on the *growth of infections*. An increase by one degree Celsius in the *backward-looking* temperature lowers, on average, the daily *growth of infections* by 0.05%. The respective impact for a unit increase in the *backward-looking* relative humidity is 0.08%.

4. Counterfactual Analysis

We use the PCSE estimates to run a series of counterfactual scenarios. We hypothesize different levels of the *OxCGRT* index that remain constant across the sample and derive their impact. We start by estimating, per U.S. state, the *growth of infections* assuming no government action. We then estimate the respective *growth of infections* for sequential increase of the *OxCGRT* index by 10 units and up to 100, creating, this way, the response surface illustrated in Fig. 8, which also illustrates the *growth of infections* across all states at the two estimated thresholds.

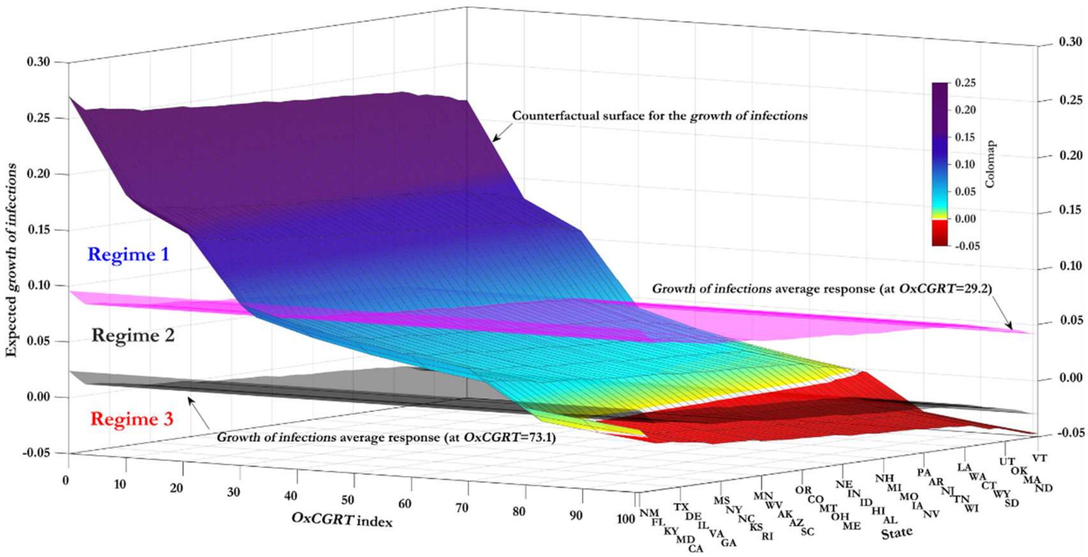

Fig. 8 | Counterfactual analysis for the *OxCGRT* index

Notes: (i) The vertical left-axis depicts the expected growth of infection; the bottom horizontal left-axis displays the stringency of the *OxCGRT* index, and the bottom horizontal right-axis depicts the state by using the two-digit code abbreviation.

(ii) The two-digit state abbreviations are: Alabama: AL, Alaska: AK, Arizona: AZ, Arkansas: AR, California: CA, Colorado: CO, Connecticut: CT, Delaware: DE, Florida: FL, Georgia: GA, Hawaii: HI, Idaho: ID, Illinois: IL, Indiana: IN, Iowa: IA, Kansas: KS, Kentucky: KY, Louisiana: LA, Maine: ME, Maryland: MD, Massachusetts: MA, Michigan: MI, Minnesota: MN, Mississippi: MS, Missouri: MO, Montana: MT, Nebraska: NE, Nevada: NV, New Hampshire: NH, New Jersey: NJ, New Mexico: NM, New York: NY, North Carolina: NC, North Dakota: ND, Ohio: OH, Oklahoma: OK, Oregon: OR, Pennsylvania: PA, Rhode Island: RI, South Carolina: SC, South Dakota: SD, Tennessee: TN, Texas: TX, Utah: UT, Vermont: VT, Virginia: VA, Washington: WA, West Virginia: WV, Wisconsin: WI, Wyoming: WY.

(iii) The expected growth of infections for the first and second thresholds of the *OxCGRT* index are signified by the pink and grey surface, respectively.

(iv) The main response surface for the expected growth of infections is colored based on the magnitude of the responses (e.g. shades of blue, turquoise, and yellow refer to a positive growth of infections, white signifies a zero growth of infections and shades of red imply negative growth of infections).

In the absence of government action, the average daily percentage *growth of infections* for all states is estimated at 24% (Table 3). The analysis suggests that the pursued government intervention policies reduced the average daily percentage *growth of infections* by 21.4 percentage points (Table 3) compared to the case where no action had taken place. This difference is significant (*p-value*<0.01). Considering the other extreme, i.e. government intervention at the highest stringency level, the average daily percentage growth of infections is -2.3% (Table 3). Had therefore government intervention remained at its highest stringency level throughout the sample, the average daily growth rate of infections would have been lower by 4.9 percentage points (Table 3) compared to the impact of the actual government intervention policies. The difference is, again, significant (*p-value*<0.01).

Table 3. COVID-19 cases growth rate per U.S. state: mean fitted values and mean counterfactual responses for different levels of the *OxCGRT* index.

State	Fitted values	Counterfactual response at *OxCGRT* level: 0	29	50	73	100	Difference between column: (2)-(1)	(3)-(1)	(4)-(1)	(5)-(1)	(6)-(1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Alabama	0.030	0.237	0.063	0.035	-0.009	-0.027	**0.207**	**0.032**	0.005	**-0.039**	**-0.057**
Alaska	0.019	0.244	0.069	0.042	-0.002	-0.020	**0.224**	**0.050**	**0.022**	**-0.022**	**-0.039**
Arizona	0.042	0.243	0.069	0.041	-0.003	-0.021	**0.201**	**0.027**	-0.001	**-0.045**	**-0.063**
Arkansas	0.027	0.235	0.060	0.033	-0.011	-0.029	**0.208**	**0.034**	0.006	**-0.038**	**-0.056**
California	0.071	0.255	0.081	0.053	0.009	-0.008	**0.184**	0.010	**-0.018**	**-0.062**	**-0.080**
Colorado	0.026	0.242	0.068	0.040	-0.004	-0.022	**0.216**	**0.042**	**0.014**	**-0.030**	**-0.048**
Connecticut	0.006	0.230	0.056	0.028	-0.016	-0.034	**0.224**	**0.050**	**0.022**	**-0.022**	**-0.040**
Delaware	0.018	0.253	0.079	0.051	0.007	-0.011	**0.235**	**0.061**	**0.033**	**-0.011**	**-0.029**
Florida	0.045	0.263	0.088	0.061	0.017	-0.001	**0.218**	**0.043**	**0.016**	**-0.028**	**-0.046**
Georgia	0.037	0.248	0.074	0.046	0.002	-0.016	**0.211**	**0.037**	0.009	**-0.035**	**-0.053**
Hawaii	0.014	0.238	0.064	0.036	-0.008	-0.026	**0.224**	**0.050**	**0.022**	**-0.022**	**-0.040**
Idaho	0.023	0.238	0.064	0.036	-0.008	-0.025	**0.215**	**0.041**	**0.013**	**-0.031**	**-0.049**
Illinois	0.034	0.252	0.077	0.050	0.006	-0.012	**0.218**	**0.044**	**0.016**	**-0.028**	**-0.046**
Indiana	0.031	0.238	0.064	0.036	-0.008	-0.025	**0.207**	**0.033**	0.005	**-0.039**	**-0.056**
Iowa	0.033	0.236	0.062	0.034	-0.010	-0.028	**0.203**	**0.028**	0.001	**-0.043**	**-0.061**
Kansas	0.028	0.247	0.073	0.045	0.001	-0.017	**0.219**	**0.044**	**0.017**	**-0.027**	**-0.045**
Kentucky	0.033	0.256	0.082	0.054	0.010	-0.008	**0.223**	**0.048**	**0.021**	**-0.023**	**-0.041**
Louisiana	0.024	0.232	0.058	0.031	-0.013	-0.031	**0.208**	**0.034**	0.006	**-0.038**	**-0.056**
Maine	0.003	0.241	0.067	0.039	-0.005	-0.023	**0.238**	**0.064**	**0.036**	**-0.008**	**-0.026**
Maryland	0.032	0.256	0.081	0.054	0.010	-0.008	**0.223**	**0.049**	**0.021**	**-0.023**	**-0.040**
Massachusetts	0.021	0.221	0.047	0.019	-0.025	-0.043	**0.201**	**0.026**	-0.001	**-0.045**	**-0.063**
Michigan	0.009	0.237	0.062	0.035	-0.009	-0.027	**0.227**	**0.053**	**0.025**	**-0.019**	**-0.037**
Minnesota	0.026	0.245	0.071	0.043	-0.001	-0.019	**0.219**	**0.044**	**0.017**	**-0.027**	**-0.045**
Mississippi	0.029	0.248	0.074	0.046	0.002	-0.016	**0.219**	**0.045**	**0.017**	**-0.027**	**-0.045**
Missouri	0.024	0.236	0.062	0.035	-0.009	-0.027	**0.212**	**0.038**	**0.010**	**-0.034**	**-0.052**
Montana	0.021	0.241	0.067	0.039	-0.005	-0.023	**0.220**	**0.046**	**0.018**	**-0.026**	**-0.044**
Nebraska	0.030	0.240	0.065	0.038	-0.006	-0.024	**0.210**	**0.036**	0.008	**-0.036**	**-0.054**
Nevada	0.032	0.236	0.062	0.034	-0.010	-0.028	**0.204**	**0.030**	0.002	**-0.042**	**-0.060**
New Hampshire	0.011	0.237	0.063	0.035	-0.009	-0.027	**0.225**	**0.051**	**0.023**	**-0.021**	**-0.039**
New Jersey	0.010	0.234	0.060	0.032	-0.012	-0.030	**0.224**	**0.050**	**0.022**	**-0.022**	**-0.039**
New Mexico	0.029	0.270	0.096	0.068	0.024	0.006	**0.241**	**0.066**	**0.039**	**-0.005**	**-0.023**
New York	0.023	0.248	0.073	0.046	0.002	-0.016	**0.225**	**0.050**	**0.023**	**-0.021**	**-0.039**
N. Carolina	0.035	0.247	0.073	0.045	0.001	-0.017	**0.213**	**0.038**	**0.011**	**-0.033**	**-0.051**
N. Dakota	0.022	0.217	0.043	0.016	-0.028	-0.046	**0.196**	**0.022**	**-0.006**	**-0.050**	**-0.068**
Ohio	0.027	0.241	0.067	0.039	-0.005	-0.023	**0.215**	**0.040**	**0.013**	**-0.031**	**-0.049**
Oklahoma	0.027	0.223	0.049	0.021	-0.022	-0.040	**0.197**	**0.022**	**-0.005**	**-0.049**	**-0.067**
Oregon	0.030	0.242	0.068	0.040	-0.004	-0.022	**0.213**	**0.039**	**0.011**	**-0.033**	**-0.051**
Pennsylvania	0.029	0.235	0.061	0.033	-0.011	-0.029	**0.206**	**0.032**	0.004	**-0.040**	**-0.058**
Rhode Island	0.017	0.247	0.073	0.045	0.001	-0.017	**0.229**	**0.055**	**0.027**	**-0.017**	**-0.034**
S. Carolina	0.035	0.242	0.068	0.041	-0.003	-0.021	**0.208**	**0.033**	0.006	**-0.038**	**-0.056**
S. Dakota	0.026	0.227	0.053	0.025	-0.019	-0.037	**0.201**	**0.027**	-0.001	**-0.045**	**-0.063**
Tennessee	0.030	0.234	0.060	0.032	-0.012	-0.030	**0.204**	**0.030**	0.002	**-0.042**	**-0.059**
Texas	0.046	0.254	0.079	0.052	0.008	-0.010	**0.208**	**0.034**	0.006	**-0.038**	**-0.056**
Utah	0.023	0.225	0.051	0.023	-0.021	-0.039	**0.201**	**0.027**	-0.001	**-0.044**	**-0.062**
Vermont	-0.007	0.217	0.042	0.015	-0.029	-0.047	**0.223**	**0.049**	**0.021**	**-0.023**	**-0.041**
Virginia	0.032	0.250	0.076	0.048	0.004	-0.013	**0.219**	**0.044**	**0.017**	**-0.027**	**-0.045**
Washington	0.023	0.232	0.058	0.030	-0.013	-0.031	**0.210**	**0.035**	0.008	**-0.036**	**-0.054**
W. Virginia	0.019	0.244	0.070	0.042	-0.002	-0.020	**0.225**	**0.050**	**0.023**	**-0.021**	**-0.039**
Wisconsin	0.023	0.233	0.059	0.031	-0.013	-0.031	**0.209**	**0.035**	0.007	**-0.036**	**-0.054**
Wyoming	0.015	0.227	0.053	0.025	-0.019	-0.037	**0.211**	**0.037**	**0.009**	**-0.034**	**-0.052**
Average	**0.026**	**0.240**	**0.066**	**0.038**	**-0.006**	**-0.023**	**0.214**	**0.040**	**0.012**	**-0.032**	**-0.049**

Notes: (i) Estimates are based on the Threshold Panel Fixed-Effects model (1) in the main text of the paper using the Panel Corrected Standard Errors (PCSE) estimation approach (see Table 2). (ii) For the columns (7), (8), (9), (10) and (11), the significant mean differences, for a significance level 0.01, are signified with bold values.

Since the increasing strength of NPIs harms economic activity, it is essential to identify the minimum level of measures capable of reverting the growth rate of infections to negative values. By setting the government interventions level equal to the second threshold, the average daily percentage *growth of infections* turns negative for the first time and equal to -0.60% (Table 3). This estimate is lower by 3.2 percentage points (*p-value*<0.01) compared to the impact of the actual policies. Overall, the counterfactual analysis suggests that while NPIs are effective in reducing the *growth of infections* at all magnitudes, negative growth rates can be achieved only when government stringency is set to a level being part of the 'high' regime [73.1-100].

What happens if we switch attention to the individual U.S. states? Had the level of government interventions remained at the second threshold, the state of California would have achieved the largest reduction in the *growth of infections* by a daily average of 6.2 percentage points (*p-value*<0.01), followed by North Dakota (reduction of 5 percentage points; *p-value*<0.01) and Oklahoma (reduction of 4.9 percentage points; *p-value*<0.01; Table 3). Our model implies that these U.S. states would have achieved even larger reductions in the average daily *growth of infections* (8, 6.8 and 6.7 percentage points for California, North Dakota, and Oklahoma, respectively and in all cases with a *p-value*<0.01) had government intervention remained at its highest stringency level throughout the sample, compared to the actual implemented policies.

We proceed by running a set of counterfactual scenarios for the *ESM* index. We report in Fig. 9, per U.S. state, the *growth of infections* for a 10-unit sequential increase of the *ESM* index from 0 to 100. In the absence of economic support, the average daily percentage *growth of infections* is estimated at 5% (Table 4). At the opposite extreme, the respective percentage growth is estimated at 2.1% (Table 4). When compared to the actual government economic interventions, both scenarios illustrate statistically significant differences (*p-value*<0.01). Specifically, actual deployed *ESM* reduced the average daily percentage *growth of infections* by 2.4 percentage points compared to no *ESM*. In addition, had *ESM* been implemented at their highest level, the average daily percentage *growth of infections* would have been lower by 0.5 percentage points. Overall, government *ESM* act complementarily to NPIs in significantly reducing further the *growth of infections*.

Table 4. COVID-19 cases growth rate per U.S. state: mean fitted values and mean counterfactual responses for different levels of the ESM index.

State	Fitted values	Counterfactual response at *ESM* level: 0	20	50	80	100	Difference between column: (2)-(1)	(3)-(1)	(4)-(1)	(5)-(1)	(6)-(1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Alabama	0.030	0.053	0.034	0.028	0.025	0.024	**0.022**	**0.004**	**-0.002**	**-0.005**	**-0.007**
Alaska	0.019	0.047	0.028	0.022	0.019	0.017	**0.027**	**0.008**	**0.002**	0.000	**-0.002**
Arizona	0.042	0.064	0.045	0.039	0.037	0.035	**0.023**	**0.004**	**-0.002**	**-0.005**	**-0.006**
Arkansas	0.027	0.050	0.031	0.025	0.022	0.020	**0.023**	**0.004**	**-0.002**	**-0.005**	**-0.006**
California	0.071	0.091	0.072	0.066	0.063	0.062	**0.019**	0.000	**-0.005**	**-0.008**	**-0.010**
Colorado	0.026	0.050	0.031	0.025	0.022	0.021	**0.024**	**0.005**	-0.001	**-0.004**	**-0.005**
Connecticut	0.006	0.032	0.013	0.008	0.005	0.003	**0.026**	**0.007**	0.001	**-0.002**	**-0.003**
Delaware	0.018	0.043	0.024	0.019	0.016	0.014	**0.026**	**0.007**	0.001	**-0.002**	**-0.003**
Florida	0.045	0.068	0.049	0.043	0.040	0.039	**0.023**	**0.004**	-0.002	**-0.005**	**-0.006**
Georgia	0.037	0.060	0.041	0.035	0.032	0.031	**0.024**	**0.005**	-0.001	**-0.004**	**-0.006**
Hawaii	0.014	0.037	0.018	0.012	0.009	0.008	**0.023**	**0.004**	-0.002	**-0.005**	**-0.006**
Idaho	0.023	0.047	0.028	0.022	0.019	0.017	**0.023**	**0.004**	-0.001	**-0.004**	**-0.006**
Illinois	0.034	0.058	0.039	0.033	0.030	0.029	**0.024**	**0.005**	-0.001	**-0.004**	**-0.005**
Indiana	0.031	0.056	0.037	0.031	0.028	0.027	**0.025**	**0.006**	0.000	**-0.003**	**-0.004**
Iowa	0.033	0.058	0.039	0.034	0.031	0.029	**0.025**	**0.006**	0.000	**-0.003**	**-0.004**
Kansas	0.028	0.052	0.033	0.027	0.024	0.023	**0.024**	**0.005**	-0.001	**-0.004**	**-0.005**
Kentucky	0.033	0.060	0.041	0.035	0.033	0.031	**0.027**	**0.008**	**0.002**	**-0.001**	**-0.002**
Louisiana	0.024	0.051	0.032	0.026	0.023	0.022	**0.027**	**0.008**	**0.002**	-0.001	**-0.002**
Maine	0.003	0.026	0.007	0.001	-0.002	-0.003	**0.023**	**0.004**	**-0.002**	**-0.005**	**-0.006**
Maryland	0.032	0.057	0.038	0.032	0.029	0.027	**0.025**	**0.006**	0.000	**-0.003**	**-0.005**
Massachusetts	0.021	0.042	0.023	0.017	0.014	0.013	**0.021**	0.002	**-0.004**	**-0.007**	**-0.008**
Michigan	0.009	0.033	0.014	0.008	0.005	0.004	**0.023**	**0.004**	-0.001	**-0.004**	**-0.006**
Minnesota	0.026	0.049	0.030	0.024	0.021	0.019	**0.023**	**0.004**	**-0.002**	**-0.005**	**-0.007**
Mississippi	0.029	0.051	0.032	0.026	0.023	0.022	**0.022**	**0.004**	**-0.002**	**-0.005**	**-0.007**
Missouri	0.024	0.047	0.028	0.023	0.020	0.018	**0.023**	**0.004**	**-0.002**	**-0.005**	**-0.006**
Montana	0.021	0.044	0.025	0.019	0.016	0.015	**0.023**	**0.004**	**-0.002**	**-0.005**	**-0.006**
Nebraska	0.030	0.054	0.035	0.029	0.026	0.025	**0.024**	**0.005**	-0.001	**-0.004**	**-0.005**
Nevada	0.032	0.056	0.037	0.031	0.028	0.027	**0.024**	**0.005**	-0.001	**-0.004**	**-0.005**
New Hampshire	0.011	0.038	0.019	0.013	0.010	0.009	**0.026**	**0.007**	**0.002**	**-0.001**	**-0.003**
New Jersey	0.010	0.035	0.016	0.011	0.008	0.006	**0.026**	**0.007**	0.001	**-0.002**	**-0.003**
New Mexico	0.029	0.053	0.034	0.028	0.025	0.024	**0.023**	**0.005**	-0.001	**-0.004**	**-0.006**
New York	0.023	0.049	0.030	0.024	0.021	0.020	**0.026**	**0.007**	0.001	**-0.002**	**-0.003**
N. Carolina	0.035	0.059	0.040	0.034	0.031	0.029	**0.024**	**0.005**	-0.001	**-0.004**	**-0.005**
N. Dakota	0.022	0.047	0.028	0.022	0.019	0.017	**0.025**	**0.006**	0.000	**-0.003**	**-0.004**
Ohio	0.027	0.050	0.031	0.026	0.023	0.021	**0.024**	**0.005**	-0.001	**-0.004**	**-0.005**
Oklahoma	0.027	0.051	0.032	0.026	0.023	0.022	**0.024**	**0.005**	-0.001	**-0.004**	**-0.005**
Oregon	0.030	0.054	0.035	0.029	0.026	0.025	**0.024**	**0.006**	0.000	**-0.003**	**-0.005**
Pennsylvania	0.029	0.052	0.033	0.027	0.024	0.023	**0.023**	**0.004**	-0.002	**-0.005**	**-0.006**
Rhode Island	0.017	0.045	0.026	0.021	0.018	0.016	**0.028**	**0.009**	**0.003**	0.000	**-0.001**
S. Carolina	0.035	0.059	0.040	0.034	0.031	0.029	**0.024**	**0.005**	-0.001	**-0.004**	**-0.005**
S. Dakota	0.026	0.050	0.031	0.026	0.023	0.021	**0.024**	**0.005**	-0.001	**-0.004**	**-0.005**
Tennessee	0.030	0.052	0.033	0.027	0.024	0.023	**0.022**	**0.003**	**-0.003**	**-0.006**	**-0.007**
Texas	0.046	0.068	0.049	0.043	0.040	0.039	**0.022**	**0.003**	**-0.002**	**-0.005**	**-0.007**
Utah	0.023	0.046	0.027	0.021	0.018	0.017	**0.022**	**0.004**	**-0.002**	**-0.005**	**-0.007**
Vermont	-0.007	0.018	-0.001	-0.006	-0.009	-0.011	**0.025**	**0.006**	0.000	**-0.003**	**-0.004**
Virginia	0.032	0.056	0.037	0.032	0.029	0.027	**0.025**	**0.006**	0.000	**-0.003**	**-0.005**
Washington	0.023	0.047	0.028	0.022	0.019	0.018	**0.024**	**0.005**	-0.001	**-0.004**	**-0.005**
W. Virginia	0.019	0.043	0.024	0.018	0.015	0.014	**0.024**	**0.005**	-0.001	**-0.004**	**-0.006**
Wisconsin	0.023	0.048	0.029	0.023	0.020	0.019	**0.024**	**0.005**	0.000	**-0.003**	**-0.005**
Wyoming	0.015	0.039	0.020	0.015	0.012	0.010	**0.024**	**0.005**	-0.001	**-0.004**	**-0.005**
Average	**0.026**	**0.050**	**0.031**	**0.025**	**0.022**	**0.021**	**0.024**	**0.005**	**-0.001**	**-0.004**	**-0.005**

Notes: (i) Estimates are based on the Threshold Panel Fixed-Effects model (1) in the main text of the paper using the Panel Corrected Standard Errors (PCSE) estimation approach (see Table 2). (ii) For the columns (7), (8), (9), (10) and (11), the significant mean differences, for a significance level 0.01, are signified with bold values.

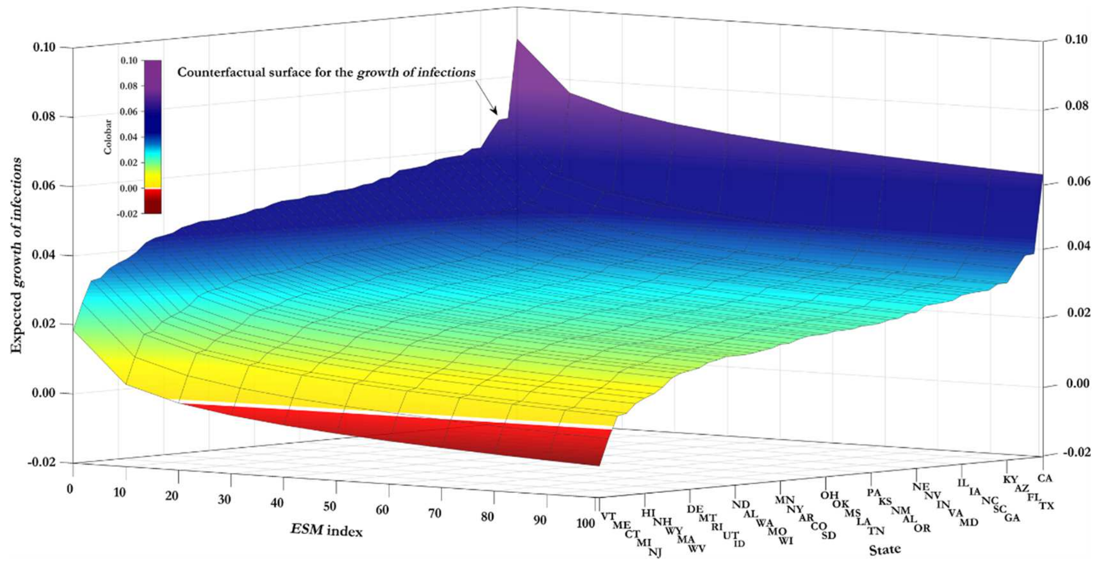

Fig. 9| Counterfactual analysis for the *ESM* index

Notes: (i) The vertical left-axis depicts the expected growth of infection; the bottom horizontal left-axis displays the *ESM* index, and the bottom horizontal right-axis depicts the state by using the two-digit code abbreviation.
(ii) The two-digit state abbreviations are: Alabama: AL, Alaska: AK, Arizona: AZ, Arkansas: AR, California: CA, Colorado: CO, Connecticut: CT, Delaware: DE, Florida: FL, Georgia: GA, Hawaii: HI, Idaho: ID, Illinois: IL, Indiana: IN, Iowa: IA, Kansas: KS, Kentucky: KY, Louisiana: LA, Maine: ME, Maryland: MD, Massachusetts: MA, Michigan: MI, Minnesota: MN, Mississippi: MS, Missouri: MO, Montana: MT, Nebraska: NE, Nevada: NV, New Hampshire: NH, New Jersey: NJ, New Mexico: NM, New York: NY, North Carolina: NC, North Dakota: ND, Ohio: OH, Oklahoma: OK, Oregon: OR, Pennsylvania: PA, Rhode Island: RI, South Carolina: SC, South Dakota: SD, Tennessee: TN, Texas: TX, Utah: UT, Vermont: VT, Virginia: VA, Washington: WA, West Virginia: WV, Wisconsin: WI, Wyoming: WY.
(iii) The response surface for the expected growth of infections is colored based on the magnitude of the responses (responses (e.g. shades of blue, turquoise, and yellow refer to a positive growth, white signifies a zero growth and shades of red imply negative growth).

5. Conclusions

We examine, for the U.S. states, the pairwise relationship between NPIs and the growth of COVID-19 confirmed cases by allowing government interventions to affect infections in a heterogeneous manner based on their varying strength. Using a two-threshold panel fixed-effects specification and conditioning on a set of regime independent variables such as *ESM* and climatic conditions we reach a number of findings. First, we identify three distinct regimes of 'low', 'medium' and 'high' severity interventions; interventions have a stronger impact in reducing infections at the 'high' regime. Second, *ESM* are significant in reducing COVID-19 cases growth down over and above the impact of NPIs. Third, we identify a negative and significant impact of the climatic conditions on the growth of COVID-19 cases. Fourth, counterfactual analysis shows that the actual conducted NPIs significantly reduced the daily average percentage *growth of infections* by 21.4 percentage points compared to the scenario of no government action. At the other extreme, had government NPIs remained at the highest stringency level throughout the sample, the daily average *growth of infections* would have been lower by 4.9 percentage points. Fifth, the implemented ESM reduced the average daily percentage growth of infections by 2.4 percentage points compared to the scenario of no economic support. Finally, we find that only NPIs classified at the high regime can reverse the growth rate of infections to a negative one. Recent epidemiological developments suggest the existence of a mutated Covid-19 variant with higher transmissibility (Kupferschmidt, 2021). It is tempting to argue that stronger government interventions, in excess of the high threshold estimated in this study, might have to be put in place in order to reduce the growth rate of COVID-19 infections not least because such action will arguably restrict the chances of the virus evolving even further.

Our paper contributes to the understanding of the exact pairwise regime-dependent relationship between containment measures and confirmed cases by quantifying in a heterogeneous manner the impact of government interventions on COVID-19 infections. Our findings seek to allow policy-makers to timely plan more effective short-run interventions towards handling infections. Last but not least, our findings seek to inform policy-makers of how to minimize the negative impact of government stringency on economic activity and achieve cost savings in the health sector and efficient allocation of existing (but nonetheless limited) resources.

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