Did Border Closures Slow SARS-CoV-2?

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ABSTRACT

In January 2020, countries began introducing new international border closures to reduce the spread of the severe acute respiratory syndrome coronavirus 2 (SARS-CoV-2). Despite the high economic, social, and humanitarian costs of border closures, more than 1000 new policies were introduced during 2020 by nearly every country in the world. Prior to 2020, the impacts of border closures on disease spread were largely unknown. Using time-series analysis and a matching technique, we combined our novel database of border closures—hand-coded by the COVID Border Accountability Project (COBAP) Team—with the variable of national-level internal movement restrictions (domestic lockdowns) from the Oxford COVID-19 Government Response Tracker (OxCGRT). We found no evidence in favor of international border closures, whereas we found a strong association between national-level lockdowns and reduced spread of SARS-CoV-2 cases. More research must be done to evaluate the short-term health data associated with lockdowns in context with their short and longer-term socioeconomic impacts, as well as compared to other preventative measures introduced at international borders.

Introduction

The objective of this study was to determine whether there is evidence that international border closures introduced in 2020 reduced the spread of SARS-CoV-2. Prior to the declaration from the World Health Organization (WHO) of the COVID-19 pandemic in March 2020, the WHO explicitly advised against travel restrictions as a national response to a pandemic: "there is no reason for measures that unnecessarily interfere with international travel and trade. WHO does not recommend limiting trade and movement."[1,2] Yet, nearly every country in the world introduced a new international border closure in response to the COVID-19 pandemic during 2020. In May 2020, we formed the COVID Border Accountability Project (COBAP) Team to document for the public a systematic database of these closures (`covidborderaccountability.org`). More than 1000 entry restrictions were introduced in 2020, with the peak of the data in the second week of March 2020 at 299 border closures,[3] marking an unprecedented halt to people's access to travel and movement across international borders. Our primary research question is whether and when these policies worked against the spread of the novel 2020-21 coronavirus, SARS-CoV-2.

There is not strong evidence in favor of international border closures from past pandemics as the available data is extremely limited. A review of the epidemiological studies on previous infectious diseases suggested that if measures were introduced early, transmission into the country was delayed only by a few days or weeks.[4] Specifically, maritime quarantines of small islands during the 1918-19 influenza pandemic seem to have delayed arrival of the disease. Another systematic review similarly highlighted that four islands with strict border controls during the 1918-19 pandemic reduced both spread and mortality from influenza compared to islands which introduced no border controls.[5] However, both these reviews reported that the data for the studies was limited and/or of "very low" quality.

Available studies on the impacts of border restrictions on the spread of SARS-CoV-2 are also limited because they implement theoretical models (instead of observational studies) or have drawn from preliminary data limited to specific locations.[6,7] Our study offers this literature the most comprehensive record of global border closures, covering decisions made by 235 country entities—including administrative units of island territories—for the complete timeline of available information for 2020. While the COBAP dataset is regularly updated, this analysis only examines the data from 2020, which had been verified at the time of analysis. To our knowledge, the COBAP database provides the most comprehensive and accurate coverage of border closures introduced in response to the COVID-19 pandemic. Shiraef et. al. (2020) reviewed comparable datasets in more depth in the initial release of the verified 2020 COBAP dataset.[8]

A second limitation of previous studies on the efficacy of border closures introduced in 2020 is that most do not control for domestic responses to the pandemic, thus conflating which measures reduced SARS-CoV-2 spread. One exception to this from past pandemic data examined both international and domestic movement restrictions in response to seasonal influenza, concluding that stringent travel restrictions may delay but do not prevent local transmission of the disease.[9] Two other exceptions to this limitation of available studies include (1) a study which investigated both international and internal travel restrictions by reviewing pre-printed and published studies by 1-Jun 2020 and (2) a subsequent 8-Jun 2020 study which estimated the effect of all non-pharmaceutical interventions but from a limited sample size of six countries with known outbreaks in early 2020 (China, South Korea, Italy, Iran, France and the United States).[7] Our study joins these two contributions by prioritizing national-level lockdowns at the global level as a validity check on our lack of findings that border closures correspond with a reduction in the spread of SARS-CoV-2. We surpass these studies by examining the combined results a much greater length of time and after testing had become more widely available.

Third, our study addresses the problem of bias when analyzing the available panel data on pandemic responses and pandemic outcomes. These studies are inherently prone to significant bias because the outcome of interest – new SARS-CoV-2 cases – also influences the decision to initiate new policies. In other words, pandemic policy research cannot easily disentangle the policy outcomes from the policy responses. Moreover, we expect covariation with case spread and other factors, such as countries' degrees of economic development, political regime type, demographics, and healthcare capacity, all of which reasonably influence both the spread of SARS-CoV-2, as well as our treatment (the decision to introduce new policies). This is a problem for viable causal inference, which necessitates randomized treatment assignment. To address this challenge, we leverage a statistical matching technique across comparable, relevant factors at the country-level – using the PanelMatch package in R. Panel matching generates a synthetic counterfactual to the treated units, allowing for a much improved causal inference strategy.

A fourth limitation of previous studies on border closures is that most implement a binary variable of whether or not a border closure was introduced on a given day or not, measured by international flight reductions. This is insufficient because countries introduced a variety of entry restrictions, with varying durations of time, and targeted at varying routes of access into a given country. Thus, the sub-categorization of the novel COBAP dataset allows our study to compare a broader set of border closures, ranging from complete closures of international borders (with exceptions only for essentials, a broader category which also includes work-related visa exceptions, and/or up to 10 specific country exceptions), to partial closures (including bans targeted at specific country lists based on travel history, citizenship status, or entry route–through air, land, or sea). To our knowledge, our study is the first to include sourced, verified end dates to border policies. As such, the COBAP database allows for robust time-series analysis on health outcomes for this study as well as for a wider variety of socioeconomic outcomes for future studies.

Fifth and finally, an inherent limitation of country-level policy analysis is that many rely on information reported at the country-level for both the independent and dependent variables. But countries vary widely on how and whether they report mortality data and moreover, may have political incentives to under-report the spread of SARS-CoV-2 and its associated deaths following a sweeping, national-level decision. To address this potential bias with mortality data, we recommend the Economist's excess death model, which estimates the "true" death toll from contextual information per country.[11] The outcome variable of this study – SARS-CoV-2 spread data – relies on the John Hopkin's University dashboard,[12] which does draw primarily from country-reported data. However, given that the direction of our hypothesis is that international border closures do not reduce spread, this potential bias entails under-reporting versus over-stating any results. Moreover, our inclusion of health capacity as a covariate limits case reporting bias. Further, our data coverage – in both time and scope of countries whose policies are covered – is as comprehensive as available at this point in time.

Our hypothesis follows the expectations from the literature of past pandemics, predicting that international border closures in response to the COVID-19 pandemic did not delay its spread unless island nations implemented them early in the timeline of global spread. Already we can see from the available data visualized on our project website (https://covidborderaccountability.org/) that international border restrictions were not sufficient, on their own, to contain the spread of SARS-CoV-2.

To test our hypothesis, we implemented panel matching with cross-sectional data to determine, per policy category, whether (and when) the decision to implement a specific policy corresponded with a decline in reported SARS-CoV-2 spread. The covariates we include are political regime dimensions (liberal democracy, freedom of expression), population's demographics (logged population size, percent over 70 years old, median population age), developmental and economic factors (Human Development Index, life expectancy, logged GDP (PPP), land area), number of daily tests per capita as well as controls for the the domestic lockdown policies and border closures not being directly measured.

Methodology

The COBAP dataset

Launched in May 2020, the COBAP database was introduced to track systematically the new international border closures being introduced in response to the coronavirus pandemic. The dataset was hand-coded by research assistants (RAs) we trained and assigned five or more countries to record new border policies, each week, throughout the pandemic – as well as the dates on which they ended. RAs systematized a variable of border closures per country by filling out a 20-question survey per policy. The survey had pre-set categories of border closures that included two discrete meta-categories ("complete closures" versus "partial closures") and eight respective, non-discrete sub-categories. A **complete closure** is when a country restricted entry to all foreigners on a given day. The sub-categories under complete closures include (1) when no exceptions are made except for essential services, such as humanitarian aid and for medical personnel ("essentials only"), (2) when exceptions are made only for citizens ("citizen exception"), (3) when exceptions are made for a broader set of work-related visas ("workers exception"), and (4) when exceptions are made for a specific set of countries, up to 10 ("specific countries exception"). A **partial closure** is when a country has restricted entry to some but not all foreigners and/or through some but not all routes of entry on a given day. The sub-categories of partial closures include: (1) when an entry restriction targets regular air, land and/or sea routes, but not all three ("air", "land" or "sea" border closure) (2) when an entry restriction targets a population based on their recent travel history (travel history ban), (3) when an entry restriction targets a population based on their citizenship status (citizenship ban), (4) or when an entry restriction targets regular visa services (visa ban).

Other public data sources used

For the covariate of separate national-level policies, we relied on the Oxford COVID-19 Government Response Tracker (OxCGRT) for comprehensive national-level restrictions on movement, which we term domestic lockdowns.[13] We restricted the *domestic lockdown* variable to only include OxCGRT variables C1:C7, removing their border-related policies (which were less precise than our database of closures between increased border screenings and actual closures). Moreover, removing their border policies allows for a more precise validity check. The seven variables that compose the lockdown index are thus: school closings, workplace closings, cancellations of public events, restrictions on gatherings, closing public transportation, stay at home requirements, and restrictions on internal movement.

Additional covariates include in our model are accessed from the Economist's Excess Deaths Model dataset, introduced to estimate the "true death toll" per country of the COVID-19 pandemic.[11] These variables are sourced from the Varieties of Democracy (V-Dem) dataset,[14] the World Bank,[15] the United Nations (UN) Development Programme,[16] and the Johns Hopkins Covid-19 Tracker.[12]

After combining the COBAP dataset with these publicly available datasets, we organized panel observations into country-week units. Our first week observation begins on January 19, 2020 and ends on February 25, 2020. This pattern continues through the end of 2020, resulting in 50 weekly periods. SARS-CoV-2 case data and other covariates are not available for the first two weeks of the year, and there are no border closure policies initiated during this time. Due to limited data of our covariate controls, we reduced the original 252 countries and island territories included in the COBAP dataset to 185. This resulted in a maximum possible of dataset of 9,250 observations. However, the available case data per country is smaller than the COBAP dataset, resulting in a panel matching dataset that includes 9,172 country-week observations.

Outcome of interest: Rate of change in new SARS-CoV-2 cases

Our outcome of interest is the rate of change in new SARS-CoV-2 cases, controlling for population size per country. This provides a more accurate outcome of interest than raw case counts. If a policy effectively reduces disease spread, the rate should be lower in subsequent weeks regardless of whether the rate of new cases was rising or falling at the time of the treatment. Our data for this variable was sourced from the COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University.[12] First, cumulative daily case counts were summarized into week-level figures by taking the end-of-week value. The number of new cases is the number of cumulative cases in T_0 minus T_{-1}. The new cases per week are then transformed into per capita figures for each 100,000 persons using each country's population size. Next, we subtract this value in T_{-1} from T_0 to produce the rate in change of new week cases per 100,000 country residence. Finally, in order to neutralize influential outliers, we convert these values to standard deviations.The large differences in the new cases week to week appear driven by either countries with small populations and/or low cases in the prior week. Positive values represent an increase in the number of new SAR-CoV-2 cases per capita, whereas a negative value signals a decline in new cases from the previous week.

The week of treatment is specified as T_0. We select a "lag" of three units, meaning that the control time periods used are T_{-1}:T_{-3}. The resulting matched sets will be composed of control observations that have precisely the same treatment history during the lag period as the treated observation. We select a "lead" of five units, meaning that quantities of interest are calculated for T_0:T_5, or the week of treatment to five weeks after. Estimating from available literature,[17,18] that observes a 5-22 day incubation period for SARS-CoV-2, we track the time range of T_0:T_{+5}, with the expectation that impact of effective policy interventions should be particularly apparent by T_{+2}. We focus on T_{+2} with the expectation that testing data may lag up to a week after an infected person has contracted symptoms and in part, because T_{+2} is when we observe the strongest reduction in spread following domestic lockdowns. For maximum transparency, we report in this study the results of SARS-CoV-2 spread in weeks T_0:T_5.

Global Trends of New Border Policies during 2020

In this section we describe the development of complete and partial border closures on a global level. Figure 1 shows the total number of complete and partial closures, and the growth in reported new SARS-CoV-2 cases[11] from January 31, 2020 until December 31, 2020. Following a rapid increase in the number of complete closures globally in March, and an ensuing peak of 147, we observe that more than 130 out of 200 countries kept their borders completely closed until June (with limited exceptions). From June until the end of the year, the number of complete closures declined non-linearly until reaching a minimum of 50 in December 2020. The number of partial border closure policies implemented also rose sharply in March. Yet, the number of partial closures introduced increased slightly throughout the year and most sharply in the last quarter of the year. Figure 1 also illustrates growth in reported SARS-CoV-2 cases on a global level. The data reflects steady growth in reported global cases throughout 2020, followed by a sharp decline in late December – before returning to previous levels.

Due to variation in the testing and healthcare infrastructures of countries, the raw case data does not allow a causal claim about aggregated complete and partial border closures on the global outcome of case spread. To address this insufficiency in the data, we employ a panel matching method, which allows more precise comparisons between countries with similar underlying factors interacting with the rate of SARS-CoV-2 spread in a given country. With this matching technique, we can better isolate the impact of border closures (versus other policies introduced or interacting factors, such as population density).

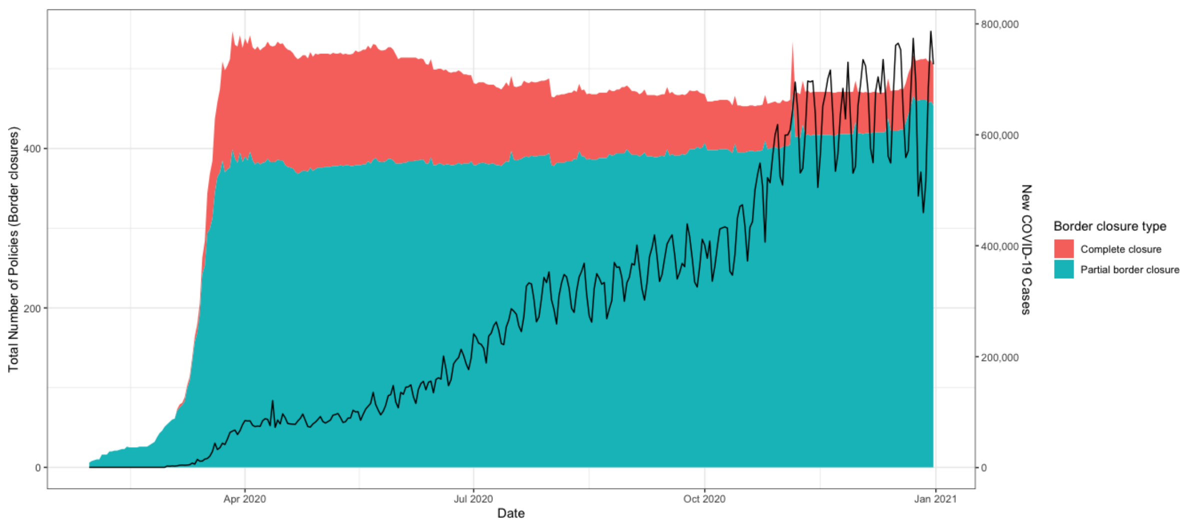

Figure 1. Descriptive data of border closure policies and new Covid-19 cases over 2020

Matching analysis

This section assesses whether border closures were effective in reducing the spread of SARS-CoV-2 cases. We designate from the novel COBAP database the onset of a new border closures introduced during 2020 as our treatment group. Because these border closures were not randomly assigned, we generate a more accurate causal estimate through a matching technique across comparable country-level factors.[19,20] Matching involves a systematic selection of similar cases to approximate a counterfactual to the treated case. In other words, our analysis compares countries with new border closure policy (treatment) to similar countries which did not institute the policy (controls). Since panel data varies over time, matching allows control units to include both the time leading up to the "treatment" for a given country of interest as well as the same period for similar observations that were not treated.

We estimate the effects of different border closure policies included in the COBAP dataset using the *PanelMatch* package in R.[21] The policy effects are quantified through a nonparametric generalization of difference-in-differences. In simulations, the package authors show that panel matching estimation reduces bias compared to the standard two-way fixed effects regression technique commonly used in panel studies.[22] In order to find the best comparable counterfactual control conditions, the *PanelMatch* package both matches and then refines the data to generate matched sets for a limited number of treatment units.

First, we specify the covariates or controls on which matching and refinement occur. These included the political, economic, healthcare, testing, and domestic policies specified above as well as relevant COBAP policy variables. For the country-year covariates, we do not select a time period as they do not vary, while for our country-week covariates, we specify selection on the lag period of interest (T_{-1}:T_{-3}). We calculate and refine our model using two common matching methods – Mahalanobis

Distance and Propensity Score – reporting both in the results section below to gauge how dependent our results are on each matching algorithm.

Once matched sets are designated for the select treatment observations, the quantity of interest – the average treatment effect for treated units (ATT) – can be calculated. This is accomplished by taking the difference between the control sets for each treated unit and generating a weighted average for each of the lead time periods. Finally, standard errors are computed using 1,000 weighted bootstrap samples.

Domestic lockdown validity check

We first conducted a validity check on our model design. We test the effects of a significant increase in domestic policy restrictions as represented by the lockdown variable. Because the index is a continuous variable, we select weekly changes that represent a significant increase in strictness of 0.25 or more to create the *domestic lockdown* treatment variable. This is occurs in 238 (2.8%) country-week observations.

For a policy to be effective, we anticipate a decline in new cases per capita, especially between T_0 and T_{+2}. Figure 2 presents the estimated effects of the Lockdown treatment. The rate of new cases is increasing in both T_0 and one week later in T_{+1}. By T_{+2}, however, new cases per capita are falling at a significant rate. The upper T_{+2} confidence interval is well below the estimators in T_0. Furthermore, for weeks T_{+2}:T_{+5}, the rate of new SARS-CoV-2 cases diagnosed continues to remain negative. We interpret this finding as lockdowns demonstrating a strong and negative effect on new cases.

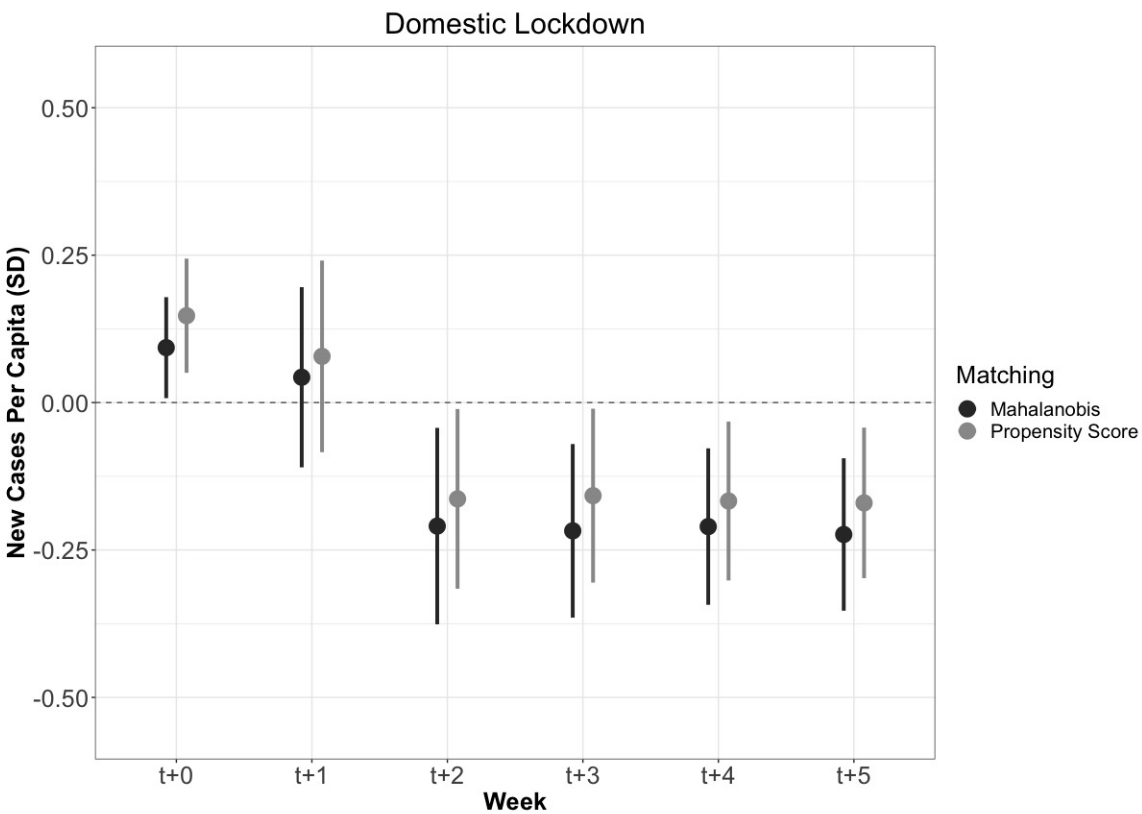

Figure 2. The subsequent effects of a significant increase in the strictness of domestic lockdown policies measured across six weeks. The dependent variable (y-axis) represents a standardized version of the rate of new cases per 100,000 country residents. Estimates are generated using two matching algorithms - Mahalanbis and Propensity Score.

Complete and partial border closures

Next, we test the effectiveness of border closure policies categorized as *complete closures* or *partial closures*, according the COBAP coding scheme. The matching process yields 194 matched sets on 207 treatment observations for complete closures, and 395 sets out of 623 treatment observations for partial closures. Aside from the treatment variable assignment, all other model parameters remain the same as the lockdown model above.

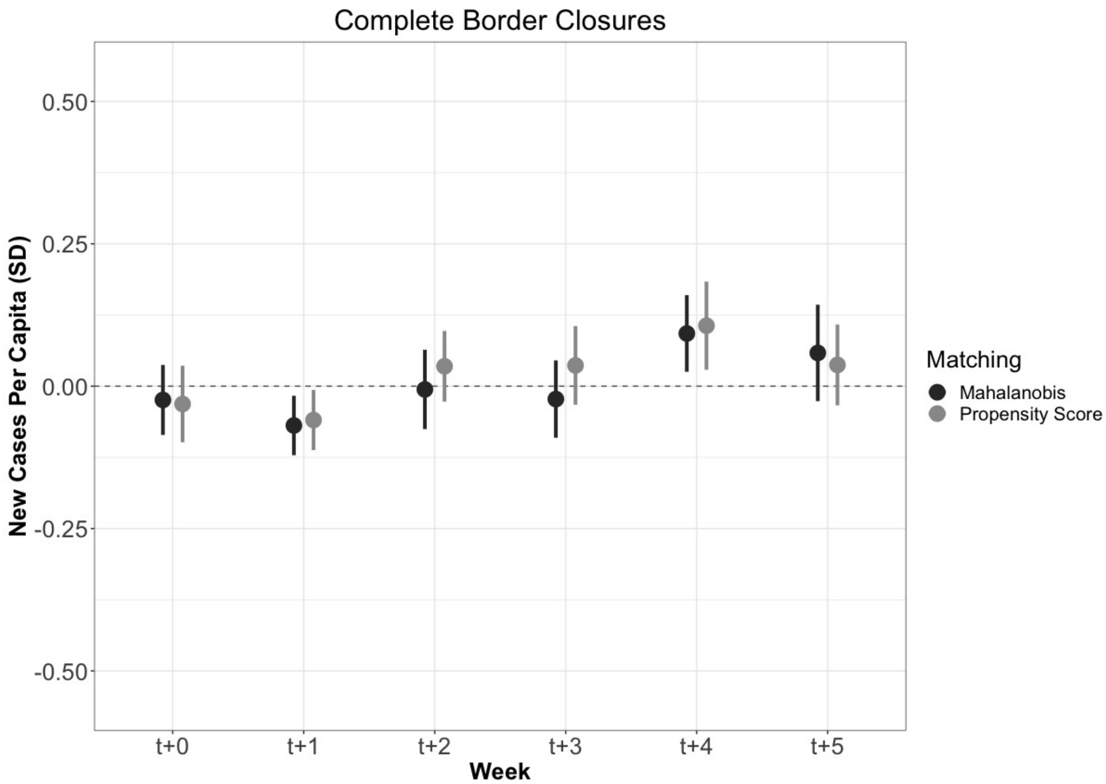

Figure 3. The effects of complete border closures on the rate of new cases over six weeks using two matching algorithm.

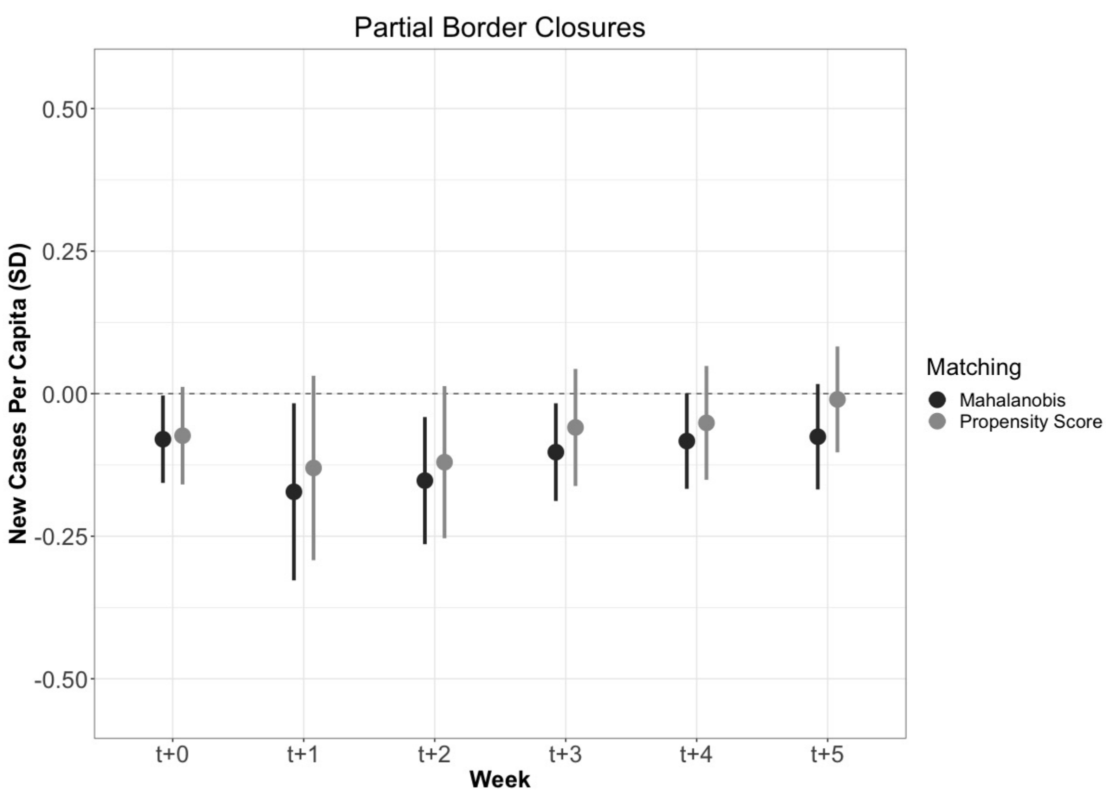

Figure 4. The effects of partial border closures on the rate of new cases over six weeks using two matching algorithm.

Figure 3 presents the estimated effects of complete border closures for the six weeks during and following the beginning of a new policy. In contrast to domestic lockdowns, the complete border closures appear to have no discernible effect on the rate of new cases. There is a very slight decline between T_0 and T_{+1}, but by T_{+2}, there is no statistically significant difference compared to the T_0 reference. If complete closures were generally effective, there would be a meaningful decline over these first three weeks. The result strongly suggests that complete border closures did not have an advantageous effect on reducing SARS-CoV-2 spread.

The results from a second set of policies categorized as partial border closures are presented in Figure 4. The results are similar to those from the complete closures. There is a small decline in T_{+1}, but the rate of new SARS-CoV-2 cases is highly similar in T_{+2} compared to T_0. All of the estimates are negative, which means the rate of new SARS-CoV-2 cases is declining after the policy interventions, but not at a different rate from the start of the policy intervention. Again, we find no substantial evidence that partial border closures in 2020 were effective.

Summary of policy outcomes by sub-type

Table 1 summarizes the effects across the different policy types between T_0 and T_{+2}, where we expect to observe a decline in the rate of new cases. The effect size for domestic lockdowns is -0.30, and the difference between T_0 and T_{+2} is statistically significant at the $p<0.01$ level. The change in the rate of new cases for both complete and partial international border closures are small, in the positive direction, and not statistically significant.

Next, we conduct panel matching models for all eight of the COBAP policy sub-types, the first four of which were categorized (prior to data collection) as "complete closures" and the latter four as "partial closures." These results presented by sub-type are less robust due to the smaller sample sizes, the variation from week to week, and that none are statistically significant at the $p<0.01$ level. We do find one policy sub-type that is statistically significant at the $p<0.05$ level, but in the *positive* instead of negative direction, the complete closures which introduced a citizens exemption. As the most common border policy sub-type of the complete closures, the complete closures that exempted citizens were highly concentrated in late March 2020 as preventative measures. In fact, 61% of these policies were implemented during the two weeks from March 15 to March 28. Between March 15 and April 15, the global total of daily new cases rose steeply from 11,375 to 77,602 as reported cases of the virus increased. Rather than indicative of closures increasing SARS-CoV-2 spread, however, this correlation indicates a need for further analysis on the specific types of closures.

Policy Intervention	**Treatment Units**	**Matched Sets**	$\mathbf{T_0}$	$\mathbf{T_{+2}}$	$\mathbf{T_{+2}}$ **(95% CIs)**	**Difference**
OxGRT Domestic Lockdowns	238	175	0.147	-0.163	-0.300 : 0.000	-0.300**
COBAP Complete Closures	207	194	-0.031	0.035	-0.026 : 0.095	0.066
Specific Country	19	17	-0.116	-0.141	-0.371 : 0.082	-0.025
Work Exception	49	49	0.006	-0.075	-0.194 : 0.050	-0.069
Citizen Exception	120	115	-0.052	0.119	0.015 : 0.221	0.171*
Essentials Only	27	26	-0.036	-0.031	-0.089 : 0.022	-0.005
Islands	50	47	0.068	0.070	-0.026 : 0.158	0.002
COBAP Partial Closures	623	395	-0.074	-0.120	-0.240 : 0.024	-0.046
Visa Ban (Partial)	55	49	-0.001	0.047	-0.069 : 0.142	0.048
Citizenship Ban	75	55	-0.073	-0.165	-0.344 : 0.014	-0.092
Travel History Ban	111	68	-0.188	-0.205	-0.430 : 0.057	-0.017
Border Closures	445	286	-0.062	-0.077	-0.212 : 0.084	-0.015
Islands	121	76	0.018	0.000	-0.074 : 0.072	-0.018
Note: $^{*}p<0.05$; $^{**}p<0.01$						

Table 1. Summary of model outcomes using propensity score matching calculations. Number of treatment units and matched sets produced. The rate of new cases standardized in T_0 and T_{+2}, 95% confidence intervals at T_{+2} and the difference between the T_0 and T_{+2} estimators. The models restricted to islands were computed and displayed above for all complete and partial closures respectively.

It is noteworthy that, on average, the baseline spread at T_0 for all border closure policy sub-types is either zero or negative, whereas for domestic lockdowns at T_0, SARS-CoV-2 cases were, on average, increasing at a significant rate. This may suggest domestic lockdowns were introduced largely in response to the rise in cases whereas border closures were introduced preventatively, though we do not submit further evidence for this pattern. Different types of restrictive policies occurred at different time periods of time, which likely influenced the results even after the matching process. As such, these results are preliminary.

Islands test

We further assess the effectiveness of complete and partial border closure policies by restricting out data to only island countries. Past literature as well as WHO advice indicated that border closures may be more effective for small island countries. The matching process yields 46 matched pairs for 50 treatment observations for complete closures and 76 matched pairs for 121 treatment observations for partial closures. Figure 5 displays the results for complete closures and figure 6 shows the results for partial closures.

We find null results for the data restricted to island countries for both sets of policies. For complete closures, the rate of new cases per capita standardized increases slightly to T_{+3}, and then falls in T_{+4} and T_{+5}. Despite these slight variations, we observe no significant decrease between T_0 and T_{+2} as expected if the closures had reduced spread.

Partial closures for island countries, shown in Figure 6, indicate an increase between T_0 and T_{+2}, though these estimates come with quite large confidence intervals. Taken as a whole, however, the estimates week to week are quite volatile and for T_{+3} and T_{+5}, similar to the first two time periods. While this volatility reduced our confidence in making a clear assessment, we generally find no significant decrease between the rate of new cases and complete and partial closures for island countries. See Appendix for the effects of the lockdown index on new SARS-CoV-2 cases.

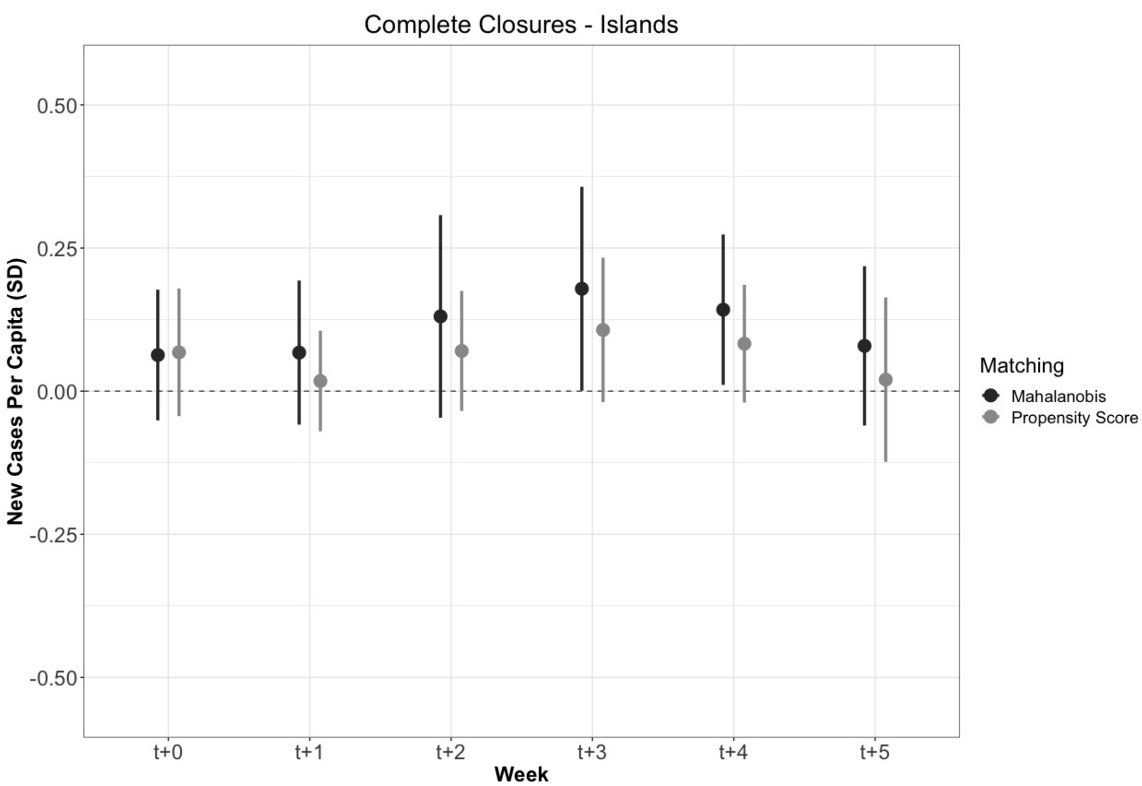

Figure 5. Effects of complete closures policies over six weeks for only small island countries.

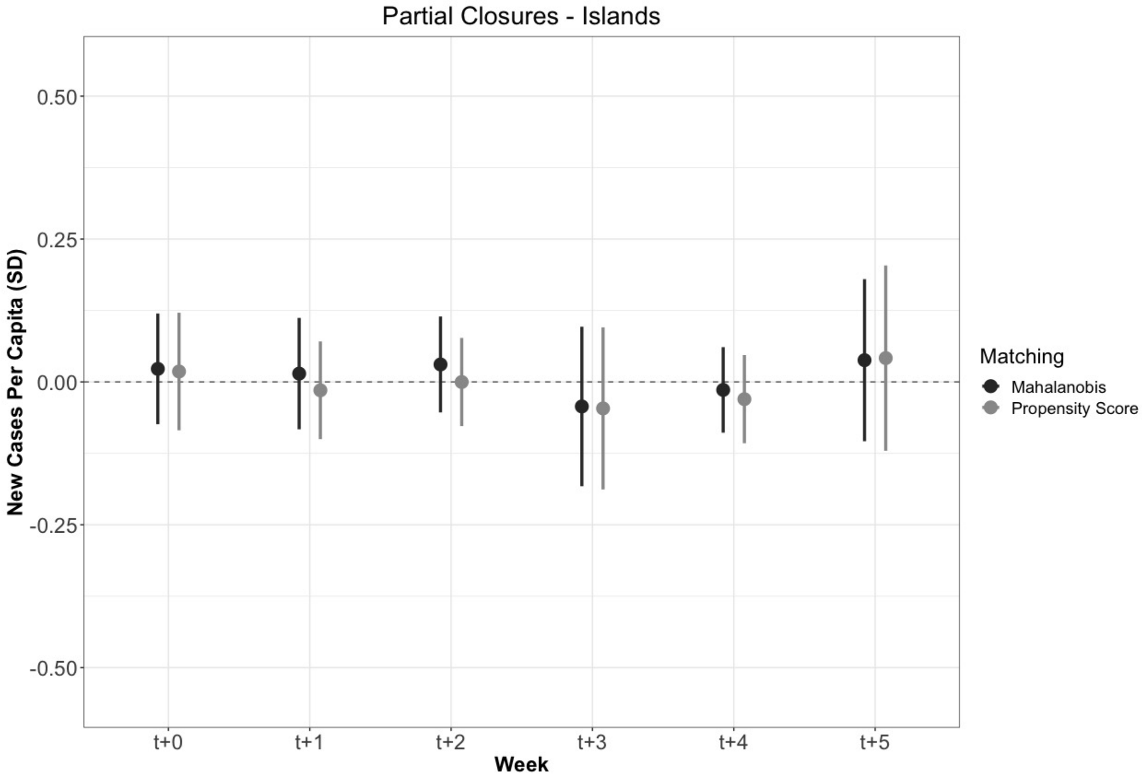

Figure 6. Effects of partial closures policies over six weeks for only small island countries.

Discussion

In summary, we found no evidence that the international border closures recorded in the COBAP database contributed to a reduction in SARS-CoV-2 spread. We found, rather, that domestic lockdowns introduced in 2020 corresponded with a decrease in cases. We also note that, among the sub-types of complete and partial closures: complete closures which exempted citizens preceded a rise in cases, significant at the $p<0.05$ level. This could result from complete closures which exempted citizens being introduced alongside increased testing for SARS-CoV-2 – although further analysis must be done with available testing data to confirm this projection. Another potential explanation is that announcing a border closure which exempted only citizens and citizen residents may have incentivized mass travel by citizens in a given time frame, contributing to SARS-CoV-2 spread. Further analysis and data collection must be done.

Our most surprising finding was that even island countries and territories which introduced complete closures in 2020 did not then see a drop in SARS-CoV-2 spread. We recognize that our panel matching estimation strategy still suffers from inevitable bias, particularly given the significant concentration of new border in time and occasional overlap of different kinds of policies within the same time period. In spite of these limitations, we understand that this estimation strategy provides the most robust assessment on the effects of border closure policies on SARS-CoV-2 spread during 2020 to date.

Our overall results indicate that domestic-level policies introduced to curb human movement within a country may be more effective in response to a global pandemic than closing international borders. Our sub-type results indicate a need for further analysis of specific policies, and our restricted island analysis indicates a need for analysis which takes into consideration specific contexts such as alternative points of entry to a given policy and alternative sites of transmission of SARS-CoV-2. Without justification that international border closures reduced virus spread, future research on these 2020 policies should ask which factors spurred them and why so many are still in place. At time of writing, x international border closures are in effect. Moreover, since policymakers are tasked during a pandemic with weighing potential health benefits against the socioeconomic costs of closures, additional studies should be completed on the human costs of these sweeping border closures. The data from this study serves as a baseline for these future analyses.

Data availability

All data and code required to reproduce the results presented in the manuscript are available at: https://github.com/COBAPteam/border_analysis.

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Author contributions statement

M.A.S. conceived the codebook for the COBAP database and manages data collection; P.F. conceived the design, implemented the matching technique and wrote the code for the analysis; L.F. ran exploratory data analysis, generated the figures, improved code legibility, and managed data on European Union countries. E.B. managed data for the island countries. M.A.S., E.N. and E.B. assisted with RA recruitment. C.H., E.N. and E.B. assisted with RA training. S.H., L.F., D.T., J.F. and S.N. managed data according to their linguistic expertise. All authors reviewed the manuscript.

Additional information

Competing interests

The authors declare no competing interests.

Appendix

Islands Only: Effects of Domestic Lockdowns

We also tested the effect of domestic lockdowns on the standardized new cases per capita variable for only the island countries. The matching process yields 32 matched sets out of a total of 45 treatment observations. The effects as seen in Figure 7 are broadly similar to those observed when testing the full set of countries. The only notable difference is that the rate of new cases continues to significantly decline between T_{+2} and T_{+3} among island nations, whereas for the whole sample the rate of spread decrease reaches it approximate maximum by T_{+2}.

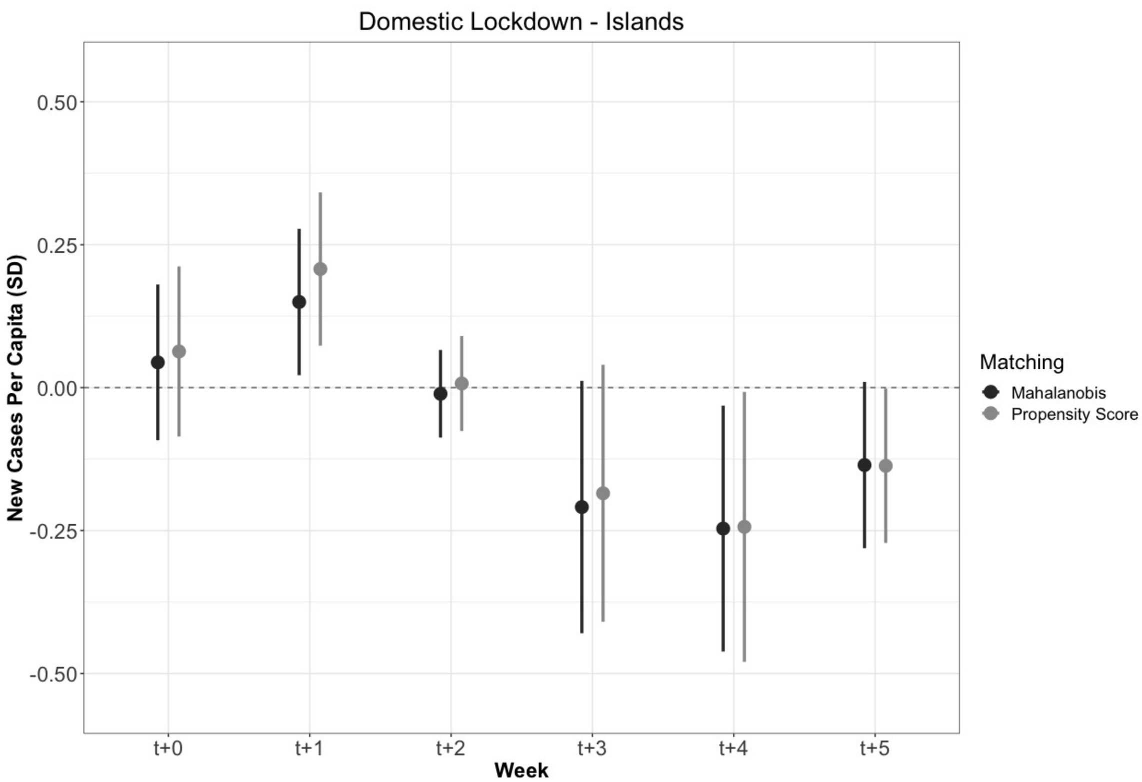

Figure 7. Effects of domestic lockdown over six weeks for only small island countries.