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Stop, in the Name of COVID! Using Social Media Data to Estimate the Effects of COVID-19-Related Travel Restrictions on Migration

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ABSTRACT In the wake of the COVID-19 pandemic, the International Organization for Migration has postulated that international migrant stocks fell short of their pre-pandemic projections by nearly 2 million as a result of travel restrictions. However, this decline is not testable with migration data from traditional sources. Key migration stakeholders have called for using data from alternative sources, including social media, to fill these gaps. Building on previous work using social media data to analyze migration responses to external shocks, we test the hypothesis that COVID-related travel restrictions reduced migrant stock relative to expected migration without such restrictions using estimates of migrants drawn from Facebook's advertising platform and dynamic panel models. We focus on four key origin countries in North and West Africa (Côte d'Ivoire, Algeria, Morocco, and Senegal) and on their 23 key destination countries. Between February and June 2020, we estimate that a destination country implementing a month-long total entry ban on arrivals from Côte d'Ivoire, Algeria, Morocco, or Senegal might have expected a 3.39% reduction in migrant stock from the restricted country compared with the counterfactual in which no travel restrictions were implemented. However, when broader societal disruptions of the pandemic are accounted for, we estimate that countries implementing travel restrictions might paradoxically have expected an increase in migrant stock. In this context, travel restrictions do not appear to have effectively curbed migration and could have resulted in outcomes opposite their intended effects.

KEYWORDS International migration • COVID-19 • Digital and computational demography • Causal inference • Global North–South

Introduction

Accurate and reliable measurement of migration is critical for informing evidence-based policy (IOM Global Migration Data Analysis Centre [IOM GMDAC] 2021a). However, measurements from official statistical systems are often released at overly wide intervals and cannot accurately detect migration discontinuities from external shocks (Alexander et al. 2022). To address the shortcomings of traditional sources, scholars have explored using digital trace data, especially from social media, to

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provide the real-time, fine-grained measurements required for estimating migration responses to external shocks. For example, researchers have used Facebook data to estimate increases in the stocks of migrants from Puerto Rico in the mainland United States in the wake of Hurricane Maria and migrants from Venezuela in other Latin American countries resulting from the Venezuelan economic and political crisis (Alexander et al. 2019; Palotti et al. 2020). These Facebook-derived estimates were consistent with data from other sources.

This previous work has focused on using social media data to estimate positive migration shocks: shocks that increased migration. By contrast, a more recent external shock that has decreased migration is the COVID-19 pandemic and, more specifically, the travel restrictions introduced to control its spread. The effects of these travel restrictions as a negative migration shock have yet to be rigorously estimated using social media data. These restrictions, including suspensions of international transit and border closures, first enacted in March 2020 (often suddenly), created complex and fast-evolving networks of restrictions between migrants' and would-be migrants' countries of origin and destination (International Organization for Migration 2021). These restrictions have particularly impacted migrants from North and West Africa (Schöfberger and Rango 2020). According to survey data, from March until June 2020, a higher proportion of migrants from North and West Africa reported that the pandemic impacted their journeys than did migrants from any other world region (Mixed Migration Center 2020).

In the immediate months following the March 2020 imposition of restrictions, migrant flows within North and West Africa and from them to key Organization for Economic Cooperation and Development (OECD) destination countries, especially in Europe, markedly decreased. Flows registered through key transit points in West and Central Africa decreased between March and May 2020, irregular arrivals to Europe declined dramatically in the first six months of the year, especially along the Western Mediterranean route most commonly used by migrants from North and West Africa (Idemudia and Boehnke 2020:33–49), and regular migrant flows to OECD countries reached historic lows (IOM GMDAC 2021b). In aggregate, the International Organization for Migration (IOM) postulated that travel restrictions lowered migrant stocks in West/North Africa, Europe, and globally by almost 2 million between March and July 2020 relative to their pre-pandemic projections (IOM GMDAC 2021b; Schöfberger and Rango 2020). However, this figure is based on the IOM's assumption of zero growth, which is itself based on assumptions about migrant behavior and not testable with data from traditional sources.

Migration data from traditional sources, often incomplete and of variable quality even before the pandemic, have been especially difficult to collect in a constantly evolving and uncertain global pandemic environment. Many countries' planned 2020 censuses and population surveys, especially in West Africa (Cece et al. 2021), suffered pandemic-related delays, cancellations, interruptions, or otherwise serious compromises of data quality (Black and Sievers 2021), exacerbating their preexisting issues with timeliness and granularity. Thus, using traditional sources to estimate the impact of travel restrictions on the region's migration, which would have been difficult under ordinary circumstances, became all but impossible. Recognizing this challenge, several key stakeholders have called for using data from alternative sources, including social media, to study migration during the COVID-19 pandemic, especially from North and West Africa (Cece et al. 2021; Schöfberger and Rango 2020).

Inspired by this call, we use Facebook data to test the hypothesis that COVID-related travel restrictions reduced migrant stock from North and West Africa in key destination countries relative to what it would have been in the absence of such restrictions during the first half of 2020. We take advantage of the quasi-natural experiment provided by the cross-country imposition of varying levels of travel restrictions at different times, formulating restrictions as a treatment whose effect we attempt to estimate. We also investigate whether any observed effect of travel restrictions on migration could be attributable to the travel restrictions themselves or whether they could be explained by other factors that could also have influenced migration or our estimates thereof during the first few months of the COVID-19 pandemic. We therefore examine the pandemic's health impacts, restrictions not related to international travel, any unobserved impacts of the pandemic's onset (Schöfberger and Rango 2020), and possible algorithmic changes impacting our data. In following a causal inference–inspired approach, our study builds on previous work that took a descriptive approach in using Facebook data to analyze migration trends following an external shock (Alexander et al. 2019; Palotti et al. 2020).

Methods

Data

Facebook

Using Facebook Marketing API (Meta 2024), accessible to anyone with a Facebook account, we collected estimates of monthly active users on the first day of each month from May 2019 to June 2020, except for October 2019, when our automated data collection activities were interrupted. Because Facebook monthly active user estimates pertain to the previous month (Meta 2024), we treat these estimates as pertaining to two weeks prior to the date on which they were collected.[1] These estimates were disaggregated by age (with all Facebook users aged 13 or older), current country of residence, and country previously lived in (if any). For each destination country, we collected measurements of the total number of users who previously lived in a different country and these users disaggregated by their country of origin. Although the algorithm Facebook uses to determine users' previous countries of residence is proprietary, previous work has inferred that users' reported locations of residence and their social networks are key features in making this determination (Zagheni et al. 2017). In this article, we refer to Facebook users who lived in countries other than their current country of residence as "Facebook

[1] We thus have data for the following dates: in 2019, April 17, May 18, June 17, July 18, August 18, October 18, November 17, and December 18; and in 2020, January 18, February 16, March 18, April 17, and May 18.

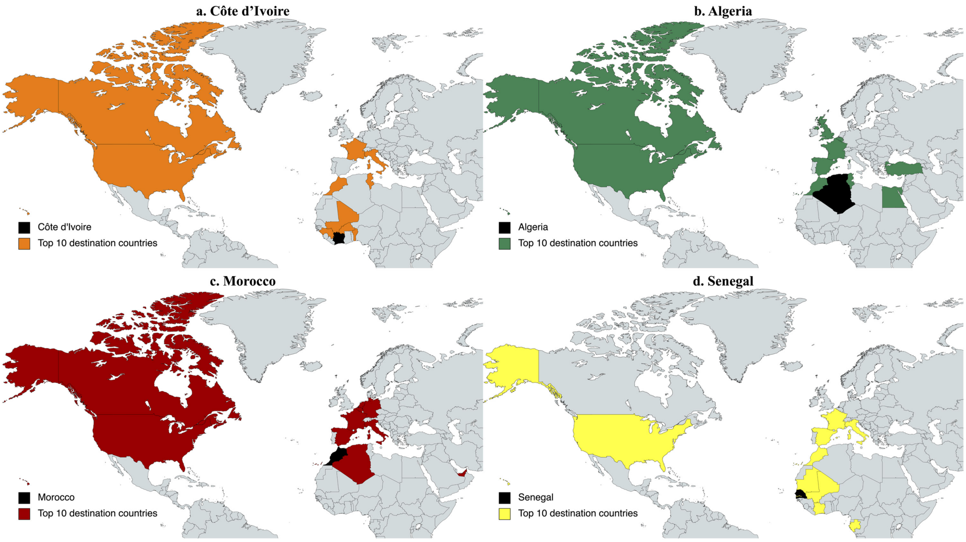

Fig. 1 Top 10 destination countries of Facebook user migrants from (a) Côte d'Ivoire, (b) Algeria, (c) Morocco, and (d) Senegal

user migrants" to denote that our estimates pertain to Facebook users identified as migrants via the algorithm. This definition of migrants differs inherently from those used by official sources, which may identify migrants as foreign-born individuals or individuals with foreign citizenship, depending on the country (United Nations 2020). However, several previous studies have similarly used Facebook user data to estimate migrant stock, including changes in migrant stock over time. These studies have produced estimates that, when adjusted for selection bias, correlate with those from official sources, including the American Community Survey (Alexander et al. 2019, 2022; Spyratos et al. 2019; Zagheni et al. 2017), Eurostat and the OECD (Spyratos et al. 2019), the World Bank (Zagheni et al. 2017), the United Kingdom's Labour Force Survey (Rampazzo et al. 2021), and the United Nations (Palotti et al. 2020; Spyratos et al. 2020; Spyratos et al. 2019).

We identified North and West African countries of origin that satisfied the following criteria: (1) at least 100 irregular border crossings along the Western Mediterranean route were identified by Frontex in 2019 and 2020 (Frontex 2021), and (2) Facebook provides data on the number of users who previously lived in these countries (Meta 2024). Using these criteria, we selected the origin countries: Côte d'Ivoire, Algeria, Morocco, and Senegal (see Figure A1, shown in the online appendix, along with all other figures and tables designated with an "A"). We then identified the 10 countries with the most users from each of these four origin countries in our data (see Figure 1), selecting 23 destination countries in total.[2] We therefore obtained 89 unique origin–destination country pairings.

[2] The destination countries are the United Arab Emirates, Belgium, Burkina Faso, Benin, Canada, *Côte d'Ivoire*, Germany, *Algeria*, Egypt, Spain, France, Gabon, the United Kingdom, Gambia, Guinea, Italy,

Travel Restrictions

Since March 8, 2020, the IOM has been tracking pairwise restrictions between the countries/territories of the world, compiling a travel restriction matrix (International Organization for Migration 2021). We use these data to measure pandemic-related travel restrictions because by contrast to data from other commonly used sources, such as the Oxford Covid-19 Government Response Tracker (OxCGRT), the IOM data provide origin–destination pairwise restrictions; OxCGRT measures travel restrictions only in the destination country without specifying the origin countries from which travel is restricted. The IOM data form a color-coded time series: for a given date, red indicates that destination country *d* barred entry to nationals/passengers from origin country *o*, yellow indicates that country *d* imposed some entry restriction on nationals/passengers from country *o*, and green indicates no restrictions (see Figure 2). We recorded these pairwise color codes for each of our 89 origin–destination country pairings daily from March 8 to May 18, 2020. At the beginning of the period, none of the destination countries had entry restrictions on nationals/passengers from our origin countries of interest. Thus, we coded all origin–destination country pairings as green between February 16 and March 8, 2020. Once destination countries instituted travel restrictions, the restrictions were not reversed.

Covariates

We also collected data on other factors we suspected could confound the relationship between travel restrictions and migration, such as the severity of the COVID-19 pandemic and broader, society-wide activity and mobility disruptions. We used excess mortality to measure the severity of pandemic health impacts. This measure is less sensitive to selection biases in COVID-19 diagnostic testing and reporting than confirmed cases and deaths, especially in lower income countries with lower coverage of diagnostic testing (Karlinsky and Kobak 2021). Excess mortality is well established as a measurement of the "whole system" population health impact of an extreme event, such as a pandemic, that considers both the direct mortality impacts of the disease and indirect mortality impacts of the pandemic (Beaney et al. 2020). We used estimates from the Human Mortality Database (HMD) (Shkolnikov et al. 2021) or the World Mortality Dataset (WMD) (Karlinsky and Kobak 2021), when available. These data sources calculate excess mortality from countries that publish all-cause mortality data. For countries that do not publish all-cause mortality data, we use excess mortality estimates from *The Economist* (Tozer et al. 2022). These estimates, unlike those from the HMD or WMD, are necessarily predicted by a machine learning model and are not based on real-world data. Using these three sources (see Figure A2), we calculated daily

Morocco, Mali, Mauritania, the Netherlands, Tunisia, Turkey, and the United States. Italicized destination countries are excluded from analyses in which they are also the origin country.

Fig. 2 Adoption of travel restrictions (February 16–May 18, 2020): the United Arab Emirates (AE), Belgium (BE), Burkina Faso (BF), Benin (BJ), Canada (CA), Côte d'Ivoire (CI), Germany (DE), Algeria (DZ), Egypt (EG), Spain (ES), France (FR), Gabon (GA), the United Kingdom (GB), Gambia (GM), Guinea (GN), Italy (IT), Morocco (MA), Mali (ML), Mauritania (MR), the Netherlands (NL), Senegal (SN), Tunisia (TN), Turkey (TR), and the United States (US). IOM data form a color-coded time series: for a given date, red indicates that the destination country barred entry to nationals/passengers from the origin country, yellow indicates that it imposed some entry restriction on nationals/passengers from the origin country, and green indicates no restrictions.

excess mortality per 100,000 people for each country in our dataset from February 16 to May 18, 2020.

To measure broader, society-wide activity and mobility disruptions, we used the OxCGRT stringency index (Hale et al. 2021). This index comprises nine subindices: school closures, workplace closures, cancellation of public events, restrictions on gatherings, public transport closures, stay-at-home requirements, restrictions on internal movement, restrictions on international travel, and public information campaigns. To reduce collinearity with international travel restrictions, we excluded the international travel restriction subindex and recalculated OxCGRT's stringency index with the remaining eight subindices following the methodology Hale et al. (2021) described. We created a daily time series of this measurement for February 16–May 18, 2020.

Empirical Strategy

Outcome Variable Construction

We first construct our main outcome variable, Y_{odt}, migrant stock from a specific country of origin o in destination country d at time t, which we calculate using the following formula:

$$Y_{odt} = Facebook\_user\_migrants_{odt} \times \frac{Official\_foreign\_born_{dt \in T}}{Facebook\_user\_migrants_{dt}}, \tag{1}$$

where $Facebook\_user\_migrants_{odt}$ is the raw number of Facebook user migrants from country o living in country d at time t. $Facebook\_user\_migrants_{odt}$ is then adjusted by the inverse of the Facebook penetration rate of migrants aged 13 or older (the minimum age of Facebook users is 13) living in destination country d at time t, $\frac{Official\_foreign\_born_{dt \in T}}{Facebook\_user\_migrants_{dt}}$. $Facebook\_user\_migrants_{dt}$ is the total number of Facebook user migrants from all countries living in country d at time t, and $Official\_foreign\_born_{dt \in T}$ is the total foreign-born population aged 13 or older in country d at time t according to official sources (Eurostat 2021; Ruggles et al. 2021; United Nations 2020). T is the year within which t falls, for which data from official sources are available. This construction of our outcome variable makes it robust to potential algorithmic changes affecting Facebook's classification of users who previously lived in a different country (Rampazzo et al. 2021), assuming that such changes have nondifferential impacts on the identification of these users regardless of origin and destination country. As a robustness check, we also consider an alternative construction of our outcome variable, Facebook user migrants from a given origin country per 100,000 total Facebook user migrants, in the online appendix:

$$Y_{odt} = \frac{Facebook\_user\_migrants_{odt}}{Facebook\_user\_migrants_{dt}} \times 100{,}000. \tag{2}$$

Treatment Variable Construction

We construct a treatment variable $T_{odt \in [0,1]}$, an index of COVID-related travel restrictions, using the following formula:

$$T_{odt} = \frac{red_{odt} + 0.5\ yellow_{odt}}{t_i - t_{i-1}}, \tag{3}$$

where red_{odt} is the number of days in the time interval ending on t_i, starting on t_{i-1}, during which travel from country o to country d was color-coded red, and $yellow_{odt}$ is the number of days in this interval when travel from o to d was color-coded yellow by the IOM travel restriction matrix (International Organization for Migration 2021). We assign yellow-coded days a value of 0.5 because they indicate some unspecified level of entry restrictions on nationals/passengers from country o in country d, falling somewhere between an outright ban indicated by the red color code and no entry restrictions indicated by the green color code. With no additional information about the precise restriction level the yellow color code indicates, this determination is ultimately somewhat arbitrary.

We take advantage of the variability of the treatment in our analyses: the adoption of travel restrictions (our treatment) was staggered, reflected different stringency levels, and occurred in only some destination countries over the observed period (see Figure 2). For example, at opposite extremes, Italy adopted highly stringent travel restrictions very early on, whereas the United States did not adopt any for nationals/passengers from the origin countries of interest over the observed period.

Descriptive Analyses

To understand the descriptive associations between our outcome and treatment, as well as our key covariates, we plot their relationships over time. To descriptively analyze the impact of the pandemic on migration, we first obtain a baseline estimate of expected migrant stock in the absence of the pandemic by constructing autoregressive integrated moving average models to forecast migrant stock from each origin–destination country pairing on May 18, 2020, using observations from April 17, 2019–February 16, 2020. We then subtract the point estimates from these forecasts and their upper and lower bounds from the actual migrant stock estimates in each origin–destination country pair observed on May 18, 2020.

Model Specification

We first present the simplest iteration of our model, without any additional controls, with the following formula:

$$\log(Y_{odt}) = \alpha_{od} + \beta_1 \log(Y_{odt} - 1) + \beta_2 T_{odt} + \varepsilon_{odt}, \tag{4}$$

where α_{od} is a set of origin–destination country pair fixed effects, and ε_{odt} is the error term. The outcome variable is already adjusted for the Facebook penetration rate of migrants in country d (see Eq. (1)). In addition, Eq. (4) further addresses selection bias. Because this model is a panel model with fixed effects, we solve it with the first-difference estimator (Wooldridge 2010). Applying the first-difference estimator and substituting in the formulas for our outcome variable (see Eqs. (1) and (2)) allow us to reexpress our outcome as a difference-in-differences estimator when $Official\_foreign\_born_{dt \in T}$ and $Official\_foreign\_born_{dt-1 \in T}$ are from the same year T (i.e., 2020):

$$\begin{aligned} \log(Y_{odt}) = {} & (\log(Facebook\_user\_migrants_{odt}) - \log(Facebook\_user\_migrants_{dt})) \\ & - (\log(Facebook\_user\_migrants_{odt-1}) - \log(Facebook\_user\_migrants_{dt-1})). \end{aligned} \tag{5}$$

According to previous work by Zagheni et al. (2014) and Zagheni and Weber (2015), our outcome constructed as such is robust to selection bias under the assumptions that this bias is similar in migrants from a specific origin country o in their destination country d to migrants as a whole in country d, and is stable over time.

The coefficient of our treatment effect is β_2. It estimates the natural logarithm of the ratio of the migrant stock from country o in country d at time t_i to the stock at time t_{i-1} if country d completely restricts travel from country o between t_{i-1} and t_i, assuming that yellow-coded days can be considered to have half of the impact of red-coded ones. We can thus convert our estimated treatment effects to percentages by exponentiating them and subtracting 1. We obtain the point estimates and standard errors of the treatment effect sizes in each origin–destination country pairing using the delta method (Cox 2005).

In addition to our simple model, we also fit more complex models that control for potential confounding. The model with our full set of controls is

$$\begin{aligned}\log(Y_{odt}) = \alpha_{od} + \beta_1 \log(Y_{odt} - 1) + \beta_2 T_{odt} + \beta_3 W_t + \beta_4 ExMort_{dt} \\ + \beta_5 ExMort_{ot} + \beta_6 Stringent_{dt} + \beta_7 Stringent_{ot} + \varepsilon_{odt},\end{aligned} \tag{6}$$

where $ExMort_{c \in \{o,d\}t}$ is estimated excess mortality per 100,000 people in country of origin o or destination d in the period ending at time t. $Stringent_{c \in \{o,d\}t}$ is the mean recalculated OxCGRT stringency index (Hale et al. 2021), excluding international travel restrictions, in country o or d over the period ending t.

W_t is a dummy variable equal to 0 if t is before March 2020 and equal to 1 if t is during or after March 2020, when the World Health Organization first declared COVID-19 a global pandemic and COVID-related travel restrictions were first implemented. March 2020 is also when Facebook may have changed its algorithm for classifying users who migrated (Rampazzo et al. 2021). W_t thus controls for both the unobserved effects of the onset of the global pandemic and potential algorithmic changes.

Equations (4) and (6) are dynamic panel models in which we try to estimate the within effect of our treatment in each origin–destination country pair and include a lag of the dependent variable as a predictor. Thus, when we take first differences, we introduce endogeneity into the model because our lagged residuals are correlated with our lagged dependent variable. To address endogeneity, we use the generalized method of moments approach proposed by Arellano and Bond (1991) for estimating dynamic linear panel models. We use $\log(Y_{odt} - 2)$ and more-distant lags of our dependent variable as instruments, taking advantage of the measures of our outcome from $t \in$ {2019-04-17, 2019-05-18, 2019-06-17, 2019-07-18, 2019-08-18, 2019-10-18, 2019-11-17, 2019-12-18, 2020-01-18}, which constitute a pretreatment prepanel before the beginning of the pandemic and the imposition of travel restrictions. We calculate asymptotic two-step corrected standard errors. We evaluate our model specifications using three diagnostic tests: the Arellano–Bond test for second-order serial correlation (Arellano and Bond 1991), Hansen's J test of the validity of overidentifying restrictions (Hansen 1982), and the Wald test for joint significance of the coefficients of our lagged dependent variable and covariates.

In addition to excess mortality and government response stringency, we tried including covariates for confirmed COVID-19 cases and deaths (Appel et al. 2022) and changes in mobility (Google 2022) in our model. However, because models with COVID-19 cases or mobility as covariates do not satisfy the assumptions of the Arellano–Bond method and COVID-19 deaths are highly colinear with excess mortality, we do not include models with these covariates in our results. All modeling and diagnostic tests were performed using R statistical software and the *pdynmc* package (Fritsch et al. 2023; R Core Team 2021).

Results

Descriptive Results

We plot the descriptive associations between estimated migrant stock and travel restrictions (see Figure A3), excess mortality (see Figure A4), and government response stringency (see Figure A5) for each of the 10 destination countries with the largest estimated migrant stocks from each of the origin countries of interest from February 16 to March 18, 2020. With some notable exceptions, countries that implemented stringent travel restrictions early in the pandemic (e.g., Italy and Spain) saw substantial drops in their migrant stocks, indicating that out-migration exceeded in-migration. In countries that did not implement travel restrictions (e.g., the United States, the United Kingdom, and Gabon), migrant stocks dropped less, remained steady, or even slightly increased, indicating that migrant inflows and outflows were comparable. Thus, among the origin countries of interest, some countries that implemented travel restrictions saw greater outflows, lower inflows, or both relative to countries that did not. Similarly, in addition to experiencing large decreases in their migrant stocks, Italy and Spain had some of the highest excess mortality early in the pandemic. However, this relationship between excess mortality and migration could be confounded by travel restrictions; the countries facing the most severe health impacts of the pandemic are likely to have responded by implementing the most stringent travel restrictions. Notably, the United Kingdom, which did not implement travel restrictions, also experienced some of the highest excess mortality early in the pandemic but did not experience as dramatic a drop in its migrant stock. Regarding overall government pandemic response stringency, Italy and Spain again had some of the most stringent government responses coinciding with some of the most substantial drops in migrant stocks. Unlike the previous two explanatory variables, government response stringency shows less variability, since all countries saw large increases in their stringency indices over the observation period. Nevertheless, some countries that also implemented early travel restrictions but had marginally less stringent overall government responses (e.g., Germany and Canada) did not see decreases in migrant stocks as large as those in Italy and Spain. These results suggest that among countries that imposed international travel restrictions, those that implemented fewer restrictions on economic activity and internal mobility saw comparatively greater inflows and/or lower outflows of migrants from the origin countries on which international travel restrictions were imposed. Comparisons of actual estimated migrant stocks on May 18, 2020, with their forecasts based on pre-pandemic trends are shown in the online appendix (see Figure A6).

Table 1 Model results of the effect of travel restrictions on log(migrant stock) with no covariates (Model 1) and with the full set of controls (Model 2)

	Model 1		Model 2	
Log Migrant Stock	0.459***	(0.0603)	0.603***	(0.0657)
Travel Restriction Index	–0.0345**	(0.0122)	0.0533*	(0.0208)
W (time dummy variable)			–0.0316*	(0.0131)
Excess Mortality per 100,000, Destination			0.00105***	(0.000303)
Excess Mortality per 100,000, Origin			–0.0177***	(0.00507)
Stringency Index, Destination			–0.00246***	(0.000706)
Stringency Index, Origin			0.00132*	(0.000567)

Note: Standard errors are shown in parentheses.

*$p<.05$; **$p<.01$; ***$p<.001$

Model Results

We first present the results of our simple model without any additional controls (see Eq. (4) and Table 1, Model 1). We observe that a destination country implementing a total entry ban on nationals/travelers from a given origin country in North or West Africa for a single month between February 16 and May 18, 2020, may expect a statistically significant 3.39% reduction in migrant stock from that origin country compared with the counterfactual in which no travel restrictions were implemented. Over the entire three-month study period, this figure would translate to a 9.83% reduction in overall migrant stock.

Using the Arellano–Bond test, we fail to reject the null hypothesis that the model contains no second-order serial correlation (p value = .768); with Hansen's J test, we fail to reject the null hypothesis that our instruments are valid (p value = .859); and with the Wald test, we reject the null hypothesis that all parameters in our model are jointly 0 (p value of 3.37×10^{-41}). The results of these diagnostic tests support the validity of our observed treatment effect of travel restrictions leading to a reduction in migrant stock from affected origin countries through reducing migrant inflows, increasing migrant outflows, or a combination of the two—at least when other factors that may have influenced migration during the early COVID-19 pandemic are not accounted for. When we translate this treatment effect of the implementation of a month-long complete entry ban to the estimated populations of migrant stocks in each origin–destination country pairing, our expected effect sizes range from 28,536 (95% confidence interval [CI], 9,127–47,945) fewer Algerian migrants in France to 5 (95% CI, 1–8) fewer Senegalese migrants in Tunisia relative to no implementation of travel restrictions (see Figure 3).

When we control for the period (before and after March 2020), a proxy for the unobserved effects of the onset of the global pandemic and potential Facebook user migrant classification algorithm changes, our observed treatment effect of travel restrictions is attenuated toward the null. When controlling for both period and excess mortality in migrants' origin and destination countries, a proxy for the severity of the health impacts of the pandemic, we do not observe further attenuation. However, when we control for the period and stringency of government responses in migrants'

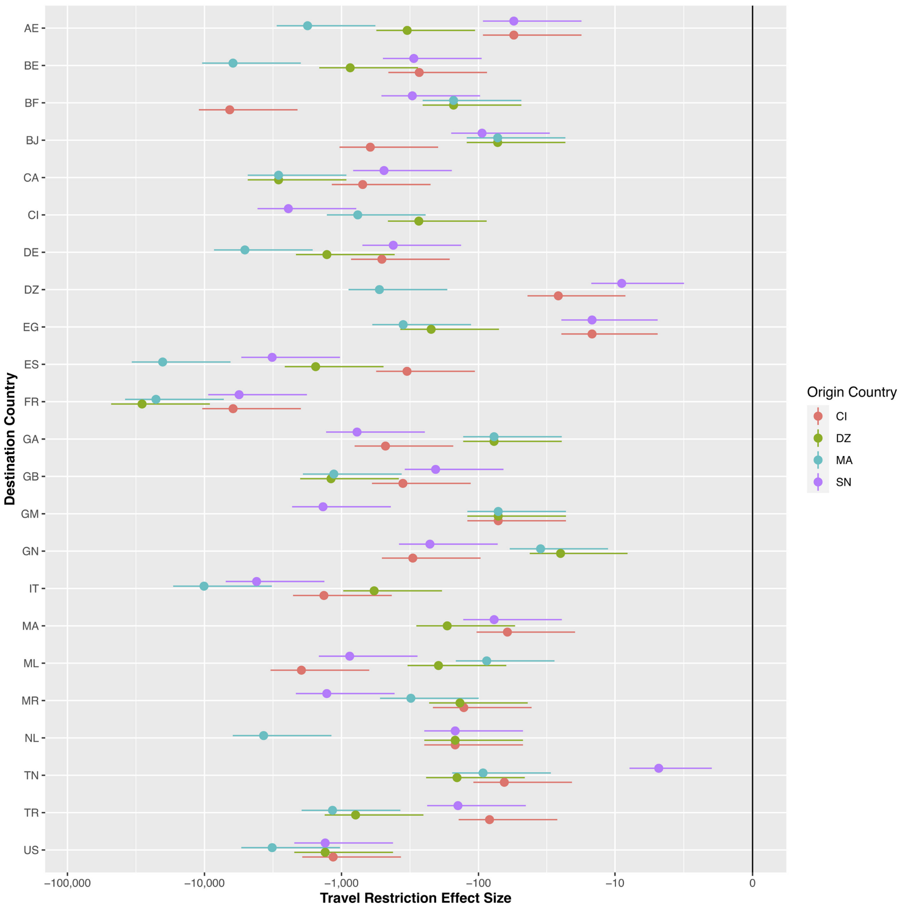

Fig. 3 Estimated effect size of travel restrictions on migrant stock from a model with no covariates in each origin–destination country pairing. Error bars indicate 95% confidence intervals. The United Arab Emirates (AE), Belgium (BE), Burkina Faso (BF), Benin (BJ), Canada (CA), Côte d'Ivoire (CI), Germany (DE), Algeria (DZ), Egypt (EG), Spain (ES), France (FR), Gabon (GA), the United Kingdom (GB), Gambia (GM), Guinea (GN), Italy (IT), Morocco (MA), Mali (ML), Mauritania (MR), the Netherlands (NL), Senegal (SN), Tunisia (TN), Turkey (TR), and the United States (US).

origin and destination countries, a proxy for broad-based societal disruptions of activity and mobility, our treatment effect becomes positive (see Table A1). The model with our full set of controls reveals that countries implementing total entry bans on nationals/travelers from our origin countries of interest could expect a statistically significant 5.47% increase in migrant stock relative to what they could expect if not implementing travel restrictions (see Table 1, Model 2). This figure would translate to a 17.34% increase in migrant stock overall during the three months studied. We also observe a 3.11% reduction in migrant stock for the period after March 2020; a 0.11% increase and 0.25% decrease in migrant stock for each one-point increase in destination countries' excess mortality and stringency index, respectively; and a 1.75% decrease and 0.13% increase in migrant stocks in these destination countries for each one-point increase in excess mortality and stringency index in migrants' countries of origin, respectively. These findings are statistically significant. This model satisfies all the requisite conditions for the Arellano–Bond test.

These results suggest that the reduction in migrant stock observed in countries that implemented travel restrictions may be explained by the broader impacts of the pandemic rather than the travel restrictions themselves. Specifically, the implementation of restrictive policies in a more holistic sense, which include but are not limited to travel restrictions, and the onset of the pandemic itself may explain these reductions. Accordingly, if period, excess mortality, and policy stringency were held constant, we would expect countries that implemented travel restrictions to see an increase in migrant stock, implying that they would see more in-migration, less out-migration, or both relative to countries that did not implement travel restrictions, *ceteris paribus*. We also observe an apparent push–pull effect of policy stringency. Migrants from origin countries with more stringent policies had greater inflows to their main destination countries, lower outflows (including return migration) from them, or both. In contrast, destination countries with more stringent policies had lower migrant inflows, greater migrant outflows (including return migration) from our origin countries of interest, or both. For excess mortality, we unexpectedly observe an inverse of this push–pull effect: more migrants in destination countries with higher excess mortality and fewer migrants in these destination countries from origin countries with higher excess mortality. If we translate the observed treatment effect of travel restrictions in this model to our estimated populations of migrant stocks, they range from 46,086 (95% CI, 9,945–82,226) more Algerian migrants in France to 7 (95% CI, 2–13) more Senegalese migrants in Tunisia than we would expect in the absence of travel restrictions (see Figure 4). We obtain similar results when replicating these models with Facebook user migrants per 100,000 as the outcome variable, demonstrating the robustness of our results to alternative response variable specifications (see Tables A2 and A3).

Discussion

As international travel restrictions were introduced in the early months of the COVID-19 pandemic, migrant inflows (especially to OECD countries) fell (OECD 2021), and outflows (especially return migration) increased (IOM GMDAC 2021b) as migrant stocks from origin countries in North and West Africa in their primary destination countries fell short of their pre-pandemic expectations. However, our findings suggest that these observed effects cannot be attributed to the international travel restrictions but rather to the broader societal disruptions that simultaneously occurred at the beginning of the global pandemic: the onset of the pandemic itself and broader restrictions on activity and mobility, of which international travel restrictions were one component. Accordingly, many destination countries with the most stringent, broad-based activity and mobility restrictions, such as Italy and Spain, were among the first to implement the most stringent restrictions on international travel.

Among migrants from all world regions, those from North and West Africa reported the greatest pandemic impacts on their journeys (Mixed Migration Center 2020; Schöfberger and Rango 2020). During the early months of the pandemic,

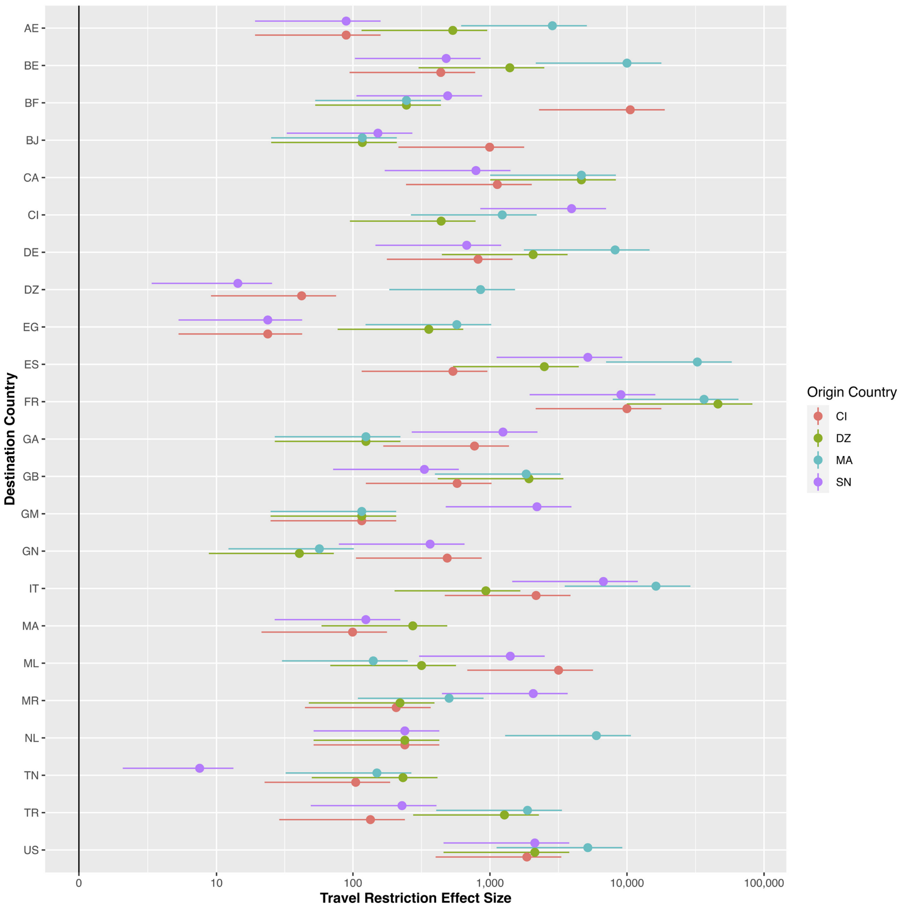

Fig. 4 Estimated effect size of travel restrictions on migrant stock from a model with the full set of controls in each origin–destination country pairing. Error bars indicate 95% confidence intervals. The United Arab Emirates (AE), Belgium (BE), Burkina Faso (BF), Benin (BJ), Canada (CA), Côte d'Ivoire (CI), Germany (DE), Algeria (DZ), Egypt (EG), Spain (ES), France (FR), Gabon (GA), the United Kingdom (GB), Gambia (GM), Guinea (GN), Italy (IT), Morocco (MA), Mali (ML), Mauritania (MR), the Netherlands (NL), Senegal (SN), Tunisia (TN), Turkey (TR), and the United States (US).

they encountered migration disruptions and barriers that extended well beyond border restrictions, including disruption of transportation networks, concerns about contracting and transmitting the virus on their journeys, and especially the loss of income sources. Migrants are overrepresented in sectors most impacted by restrictions on economic activity (e.g., services and retail) in key destination countries, have experienced higher rates of unemployment than nonmigrant workers, and have frequently been excluded from governmental support measures to mitigate pandemic-related economic impacts (IOM GMDAC 2021b). Migrants' losses of income sources in their destination countries decrease inflows to these countries and increase outflows from them in the form of return migration, suggesting that we should see a reduction in migrant stock in countries that impose more restrictions on economic activity than in those that impose fewer. This expectation aligns with our results. We find a push–pull effect: in our destination countries, more stringent policies (including restrictions on economic activity) in migrants' origin countries but less stringent policies in these destination countries are associated with an increase in migrant stock. However, we find an opposite push–pull effect for mortality: increases in migrant stocks from origin countries with lower excess mortality in destination countries with higher excess mortality. This finding is unexpected, especially given that health concerns about COVID-19 itself were chief among migrants' concerns during the early months of the pandemic (Mixed Migration Center 2020; Schöfberger and Rango 2020).

Our findings that when we hold policy stringency, excess mortality, and period constant, travel restrictions lead to an increase rather than a decrease in stocks of migrants from affected countries align with previous literature documenting the ineffectiveness of immigration policies more broadly (Czaika and De Haas 2013). These results could perhaps suggest that travel restrictions have the opposite of their intended effects of reducing migrant inflows. More precisely, though, our evidence suggests that any potential effect of travel restrictions on decreasing migrant inflows has to be more than offset by a corresponding decrease in outflows. This evidence aligns with previous findings that destination countries that implemented travel restrictions, especially in Europe, saw declines in return migration (IOM GMDAC 2022). Migrants who would have otherwise returned to their countries of origin upon losses of income sources in their destination countries following the onset of the pandemic and the introduction of restrictions on economic activity were impeded from doing so by travel restrictions and border closures (IOM GMDAC 2021b). Many migrants became stranded, unable to return to their countries of origin. Migrants in the Middle East and North Africa were disproportionately impacted, comprising up to 40% of the global total of stranded migrants during the first half of 2020, despite representing less than 20% of global migrants overall (United Nations 2020).

We must acknowledge the limitations of our empirical strategy. The IOM's travel restriction matrix, which we use to construct our treatment variable, has only three values: complete travel restrictions, an unspecified degree of partial restrictions, and no restrictions (International Organization for Migration 2021). This empirical strategy assumes, based on a lack of information, that unspecified partial restrictions have half the impact of a complete ban. Further, it fails to fully describe the breadth and complexity of international travel regulations during the COVID-19 pandemic, when almost 1,000 exceptions to restrictions were issued (IOM GMDAC 2021b). These exceptions include carve-outs for labor migrants in essential sectors in which migrants are overrepresented as employees in key destination countries (e.g., agriculture, forestry, and fishing) and family and partner reunification, even in countries whose borders were otherwise closed (Scarpetta and Dumont 2020). Thus, constructing a treatment variable exclusively using the IOM's travel restriction matrix might be subject to unobserved confounding. The subtleties of international travel regulations might be better captured through the broader government response stringency index. Indeed, we observe a negative effect of travel restrictions on migration when holding IOM-defined travel restrictions constant. To fully account for the greater nuances in international travel restrictions during the COVID-19 pandemic, future work should explicitly include these restrictions in models.

In addition, using Facebook user migrants as a proxy for actual migrants suffers from several limitations arising from algorithmic and selection bias. Facebook user migrants differ from migrants as defined by official sources (United Nations 2020) and therefore cannot be directly interpreted as such. However, migration estimates derived from Facebook can be interpreted as a timely but noisy signal from which changes over time can be modeled and estimated when specified assumptions (outlined in the Model Specification section) are met (Zagheni and Weber 2015). Additionally, although the definition of migrants used by Facebook differs from that used in official sources, official sources use inconsistent definitions that vary by country (United Nations 2020). In the absence of a uniform, internationally standardized definition of migrants, any approach trying to estimate international migration dynamics using an alternative data source that is consistent across countries will not necessarily align with data from official sources that are not consistent. In addition, the requirement that Facebook users must be aged 13 or older necessarily means that our analyses exclude migrant children. This exclusion might partially explain our finding of small effect sizes.

Previous work has suggested that Facebook changed its algorithm for classifying users who migrated in March 2020 (Rampazzo et al. 2021). Because this algorithm is proprietary and its change occurred simultaneously with the World Health Organization's declaration of COVID-19 as a global pandemic, we cannot isolate its effect from the effects of the pandemic's onset. Under these conditions, we can only include a variable in our models (W_t) that controls for the effects of both of them but not for either one of them separately.

Finally, the Arellano–Bond method for estimating dynamic linear panel models is limited by the nature of lags of the dependent variable as a weak instrument. We determined that our model specification satisfied the key assumptions of the Arellano–Bond model: no second-degree serial correlation (Arellano and Bond 1991), the validity of overidentifying restrictions (Hansen 1982), and at least one nonzero parameter. However, newer methods have been developed for estimating dynamic treatment effects, such as *difference-in-differences with multiple time periods* (Callaway and Sant'Anna 2021). Although the theoretical grounding of these methods has begun to be extended to continuous treatments (Callaway et al. 2021), this work has yet to be peer-reviewed and implemented in software.

Conclusion

Obtaining accurate and timely migration estimates is a challenge, especially in lower income countries. Pandemic-related disruptions to traditional data collection have exacerbated this challenge. Although subject to measurement error, social media data are potential sources of migration estimates in the absence of data from traditional sources. Using Facebook data, we test the hypothesis proposed by migration stakeholders that international travel restrictions reduced migrant stock from North and West Africa in key destination countries during the first few months of the COVID-19 pandemic relative to the counterfactual in which no travel restrictions were implemented. We observe a reduction in stocks of migrants from Côte d'Ivoire, Algeria, Morocco, and Senegal in destination countries that imposed travel restrictions on them relative to countries that did not. However, this finding is likely not due to the travel restrictions themselves but rather to the far broader societal disruptions wrought by the onset of the COVID-19 pandemic, one small component of which was international travel restrictions. The full complexity of COVID-19 international travel regulations is challenging to capture with a single treatment variable, and further research is needed. In the context of these wider disruptions, though, travel restrictions do not appear to be effective in stopping in-migration from banned countries, and any potential effect they may have is more than offset by stranding migrants and preventing them from returning to their countries of origin. The evidence we present suggests that during a pandemic that has effectively disrupted migration on its own, travel restrictions are ineffective in achieving their stated goals and can elicit deleterious unintended consequences. ■

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