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Original article

The gravity of distance: evidence from a trade embargo

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Abstract

On 5 June 2017, an airspace blockade was imposed on the State of Qatar by Bahrain, Saudi Arabia, United Arab Emirates (neighboring countries), and Egypt. We exploit this exogenous increase in air transportation costs toward non-blockading countries to examine the effect of increased travel distance, due to re-routing, on bilateral trade. Based on a gravity model estimated using a Poisson pseudo-maximum likelihood estimator, we find a distance elasticity of imports between −0.3 and −0.5. Overcoming the limitations of cross-sectional studies and taking advantage of this quasi-natural experiment, our findings are robust and revise downward previous estimates of the distance elasticity.

Keywords: embargo; distance effect, international trade.

JEL classifications: F14, L93.

1. Introduction

Since the industrial revolution, the reduction of transportation costs has been a major driver behind the economic integration of nations. Revolutions in transportation mode, being by rail (Bairoch 1993; Williamson 2011; Donaldson 2018), by sea (Findlay and O'Rourke 2003; Pascali 2017), by road (Baum-Snow 2007; Duranton and Turner 2012; Duranton et al. 2014), or, more recently, by air (Campante and Yanagizawa-Drott 2018; Feyrer 2019, 2021) have often accompanied new waves of globalization. It is therefore not surprising that economists have for long attempted to quantify the elasticity of trade to distance (Frankel and Romer 1999; Disdier and Head 2008; Head and Mayer 2014). The estimated distance elasticity of trade is also a key ingredient in models seeking to assess the global costs of trade shocks. For instance, elasticity estimates from the literature played a key role in quantifying the effect of Brexit on trade and income (see Bisciari 2019, for an overview of these studies).

Our article contributes to the literature on the impact of transportation costs on trade. The seminal finding of an elasticity of −0.85 by Frankel and Romer (1999) is confirmed by the meta-analysis of 159 studies by Head and Mayer (2014) who find an average distance elasticity of −0.93. However, the cross-sectional nature of the correlation between distance and trade is likely to capture not only the importance of physical distance but also other trade-related factors correlated with proximity (e.g. cultural, legal, and migration aspects, among others). Grossman (1998) concludes that something is missing in gravity models because the impact of distance is too large to capture pure transportation costs. To

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address the issue, recent papers have exploited transportation developments or shocks to travel distance to isolate the causal effect of distance on trade (Martincus and Blyde 2013; Pascali 2017; Donaldson 2018; Baniya et al. 2020; Feyrer 2019, 2021). This literature tends to find a much lower elasticity estimate that ranges between −0.15 and −0.7. The only exceptions are Feyrer (2019) and Donaldson (2018): focusing on assessing the impact of trade on economic development, they find a larger trade elasticity to distance with a range of −0.9 and −1.6.

Our article revises such estimates of the causal effect of distance on trade. More specifically, we take advantage of an unexpected change in air travel costs caused by the sudden closure of airspaces surrounding Qatar and only for flights from and to Qatar. In comparison to the existing literature that exploits a shock that affected multiple countries (i.e. the closure of the Suez Canal in Feyrer 2021), steamship introduction (Pascali 2017), or a shock on road transportation (Martincus and Blyde 2013; Donaldson 2018), our setting exploits an exogenous shock on air distance that affected the airspaces surrounding Qatar only. Therefore, we can examine the "pure" effect of air distance on trade without worrying about a spillover effect coming from other countries (not affected except through the change in Qatar's trade). By comparing how trade flows changed with non-blockading countries differently affected by the blockade, our findings point to an elasticity in the range of −0.5 to −0.3, depending on the exact specification. Our article complements Besedes et al. (2024) who exploit the no-fly areas resulting from conflicts to find an elasticity of −0.7 for air freight, which is not far from our estimates. By focusing only on the blockade against Qatar, in this article, we can assess further possible identification threats such as the risk of anticipation, possible substitution between transportation modes, or other time-varying omitted factors. Importantly, our results are robust to a series of robustness checks aimed at controlling for other potential factors. Thus, although identified on a much shorter period and in a different context, the magnitude of our estimates significantly reduces the estimates of this elasticity compared to older literature while echoing recent findings that exploit different time-varying shocks to distance.

The remainder of the article is organized as follows. Section 2 provides some background on the embargo imposed on Qatar. Section 3 discusses the data construction and derives the specification to be estimated, while Section 4 provides the main results followed by a series of robustness checks in Section 5. In Section 6, we discuss the economic significance of our results and the limitations of our analysis.

2. The blockade against Qatar

As a result of political differences, Bahrain, Saudi Arabia, United Arab Emirates (i.e. three neighboring countries), and Egypt unexpectedly severed all diplomatic ties with the State of Qatar on 5 June 2017. The blockading countries justified their action by accusing Qatar of providing financial support to terrorism, maintaining a strong connection with Iran, and meddling in their internal affairs, including through the Al-Jazeera network—allegations that Qatar refuted as baseless (Chughtai 2020). Since then, the blockade has been resolved with a reconciliation agreement in January 2021 (Aljazeera 2021a), which does not affect our analysis as our sample period ends in 2019.

The unexpected nature of the blockade is key for our identification strategy. Two weeks before the embargo, Qatar's state news agency was hacked, and fake statements attributed to the emir were issued (Browning 2017). The statements were aired on different United Arab Emirates and Saudi-owned networks, leading to political tensions between the countries in the area (Chughtai 2020). Despite the fact that relationships between the Gulf States involved were already strained in the 2 weeks preceding the blockade, the embargo seems not to have been anticipated. To explore the lack of anticipation, we make use of the data collected by the Global Data on Events, Location and Tone project (Leetaru and Schrodt 2013), which records daily news items in over 100 languages across the globe.[1] We examine all events categorized as "Impose embargo, boycott or sanctions" (CAMEO Code 163) recorded between January 2015 and April 2020 for which the target actor is Qatar over this period. In total, we find 6,876 such news. As shown in Fig. 1, there is a huge spike of news related to the embargo after its introduction as denoted by the red vertical line (i.e. 1,381 and 1,456 in June and July 2017, respectively), while

[1] GDELT is an open-access dataset monitoring news media in over 100 languages across the globe (https://www.gdeltproject.org/). It contains an average of 8.3 million daily political events for the whole world that are completely geo-coded (Manacorda and Tesei 2020), making it the most comprehensive dataset on human society (https://www.gdeltproject.org/data.html).

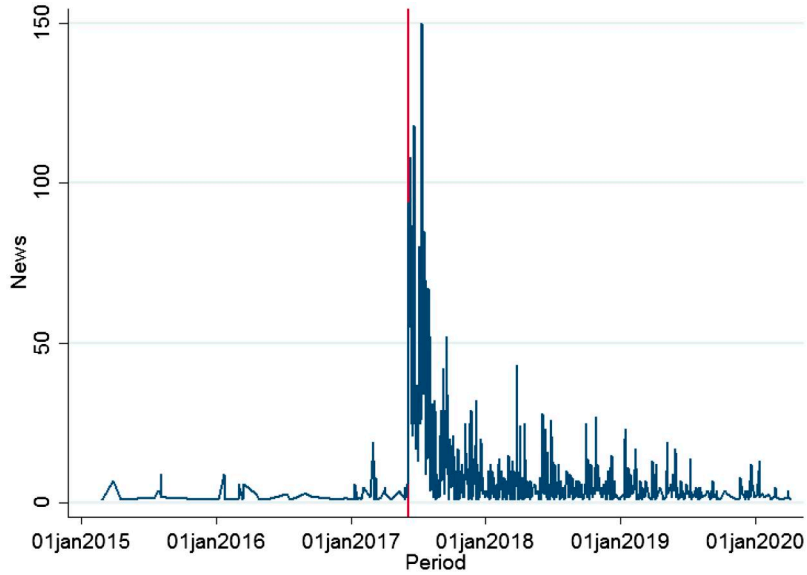

Figure 1. News about the embargo.
Notes: Embargo news about Qatar is released in any country.
Source: Authors' calculations using GDELT Data.

very few in the months before (i.e. 19 in May and overall 108 in the first 5 months of 2017). Most of the news before 5 June 2017, was miscoded. The remaining events refer to other threats of sanctions by the USA, whose motivations were unrelated to the ones behind the 2017 embargo. Such evidence is a clear indication that the embargo was not anticipated and can provide an exogenous shock on travel costs to allow us to identify the causal impact of distance on trade.

The four blockading countries also ended trade ties and suspended flights to and from the country, closed air and land borders, and blocked access to seaports. In addition, Saudi Arabia and the UAE ordered all Qataris to leave their countries within 2 weeks and asked their citizens in Qatar to return back over the same time frame (Aljazeera 2021b). Importantly for this study, the blockade included a ban on the use of their airspaces (an area slightly larger than India) for all flights.[2] This action resulted in longer routes from Hamad International Airport (i.e. Qatar's only international airport at that time) to all airports worldwide. The closure of airspaces surrounding Qatar was a major issue for a country that was importing 32 per cent of its goods by air prior to the embargo (based on June 2016 to May 2017).[3]

Based on the geographical location of countries, certain routes were more affected than others, as shown in Fig. 2 for three non-blockading countries before and after the embargo. The impact on flights to Sudan was more severe compared to those to Turkey and Bangladesh, solely due to the relative locations of these countries in relation to Qatar and the blockading nations. This exogenous variation resulting from countries' geographical positions is crucial for our identification. Since the embargo also involves the closure of land borders and some obstacles in sea transportation, our analysis will additionally consider substitution effects for countries heavily reliant on alternative transportation modes.[4]

This crisis was the first of its kind since the establishment of the International Civil Aviation Organization 70 years ago, and the UN aviation body has not dealt with a similar case before (Macheras 2018). The immediate consequence of the blockade was a collapse in trade with the

[2] The Qatar General Authority of Customs confirmed that all flights, regardless of the airline, carrying goods destined for Qatar were affected.
[3] The equivalent figure for the USA (excluding trade with Mexico and Canada) is 30 per cent (Feyrer 2019).
[4] Based on information from Qatar's General Authority of Customs, blockading countries imposed a ban on Qatari vessels from accessing directly their seaports. The Suez canal remained open for Qatari vessels' passage since its access is governed by the Constantinople Convention of 1988 (Cox 2017). However, the main impact came from the closure of ports in United Arab Emirates (i.e. Fujairah and Jebel Ali ports) although alternative ports were used by Qatar following these measures, and in February 2019, United Arab Emirates eased these restrictions (Aljazeera 2019).

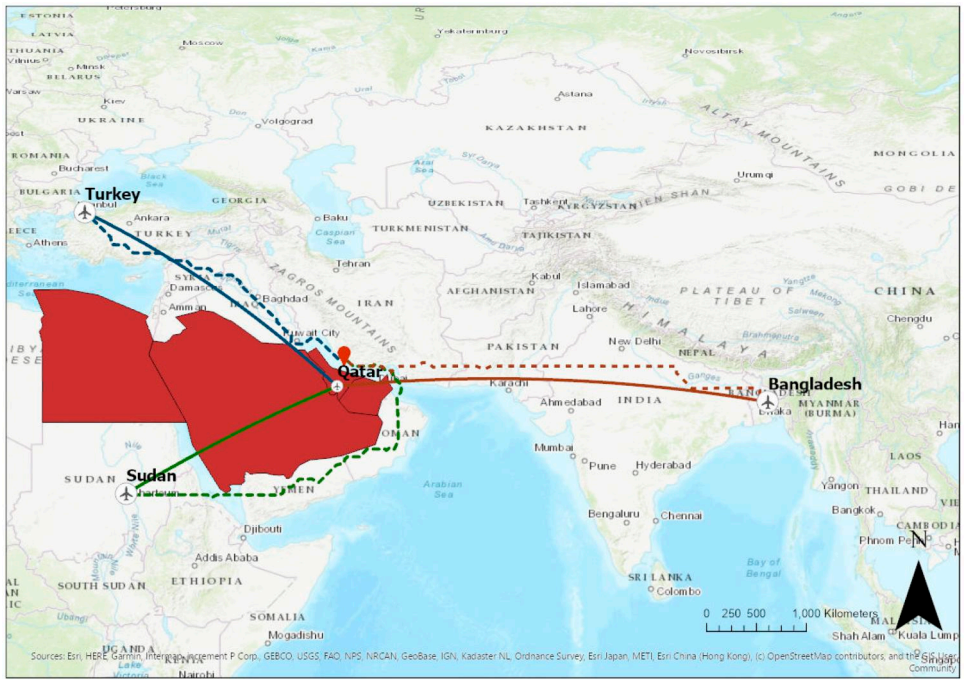

Figure 2. Examples of flights diversion.

Notes: Solid lines indicate pre-embargo routes; dotted lines indicate post-embargo routes.

Source: Authors' calculations using Geocoded Data.

blockading countries. The decline of Qatar's imports from blockading countries was steep and unexpected, as shown in Panel (a) of Fig. 3. Such a sharp fall in imports and overall trade (see Fig. A.1 in the Appendix) confirms the unexpected nature of the embargo, which will be further discussed in Section 4. Interestingly, Panel (b) of Fig. 3 illustrates that exports to the blockading countries were not affected as much because the gas industry, which is the major export industry in Qatar, was exempt from the blockade. In fact, Qatar continued to supply the United Arab Emirates with natural gas via the shared Dolphin pipeline, and their cooperation on the Bunduq offshore oil field remained intact (Dudley 2018).[5]

Faced with such a disruption, we would expect to observe some trade diversion whereby imports from and exports to non-blockading countries may increase. This is what appears to be the case, as shown in Panels (c) and (d) of Fig. 3. Exports, and particularly imports, increased after a short-term reduction upon announcement of the blockade. And such a dramatic fall has been somewhat compensated by increased imports from non-blockading countries, as shown in Panel (c) of Fig. 3.

3. Data and methodology

3.1 Data

To estimate the effect of the embargo on bilateral trade between Qatar and the rest of the world, we obtain monthly trade data at the two-digit Harmonized System (HS) level between Qatar and all available trade partners over the period 2015M1–2019M4 from the Qatar Planning and Statistics Authority (2021). This source of data is very rich since it also allows us to identify the mode of transportation (i.e. air, sea, land, or pipeline), which is crucial as our analysis focuses on trade and imports by air. After dropping the four blockading countries, our sample contains 144 countries (146 units of observations

[5] At the onset of the blockade, Qatar asserted that its exports of liquefied natural gas (LNG) to its biggest buyer in Japan will continue despite the blockade (*Bloomberg News* 2017). Similarly, the head of Dubai-based consultant Qamar Energy stated that Qatar's own waters along with Iran and Oman will allow it to continue its exports of LNG to its main customers in Asia (*Bloomberg News* 2017). As a result, Qatar retained its position as the major LNG exporter in the world in 2017 (Selmi and Bouoiyour 2020).

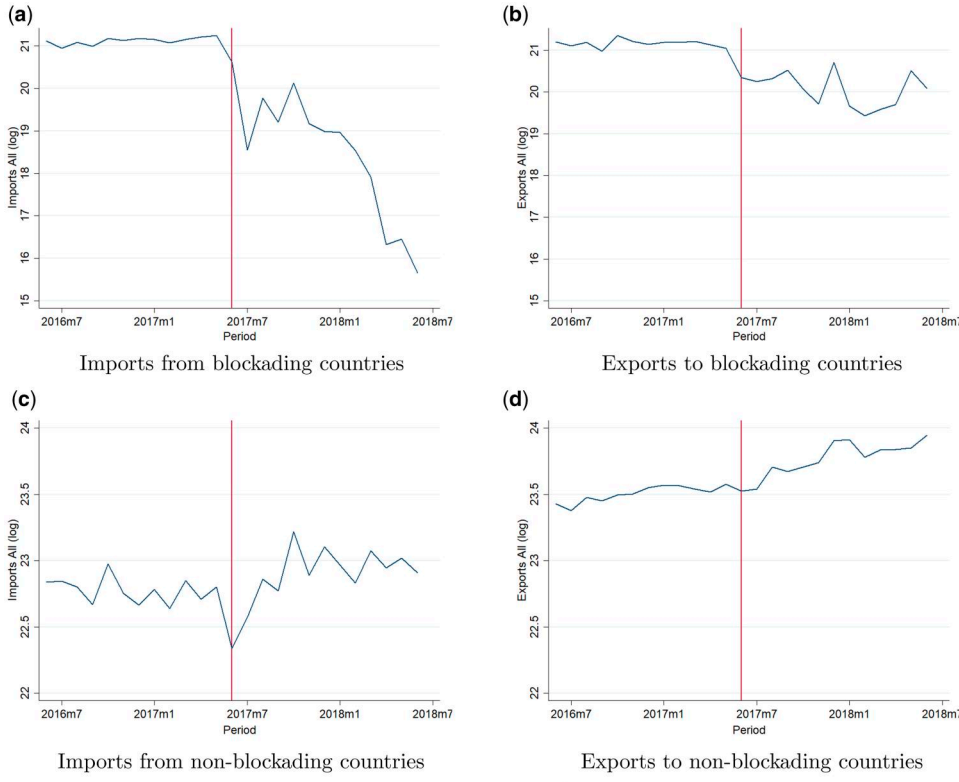

Figure 3. Trade responses to the embargo.

Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

with Canada and the USA split into West and East coasts—see Supplementary Appendix Table A.1 for the complete list of countries) resulting in a total of 292 observations. Notice that we lose a few observations when focusing on imports by air because our dataset does not record any imports by air with seven countries (i.e. 278 observations). In the econometric analysis, data are aggregated over the 97 two-digit HS sectors.[6]

Considering these data and focusing on trade with the blockading countries, Panel (a) of Fig. 4 shows reductions in imports of around 90 per cent for sectors in the top 10th percentile in terms of values (when comparing 1 year before the embargo and 1 year after the embargo). The sector of mineral fuels represents a notable exception, due to the fact that it was not subject to the embargo. The fall of imports from the blockading countries cancels out with increased imports from non-blockading countries, as shown in Panel (b) of Fig. 4. While increased imports did not always make up for the shortfall from blockading countries, they more than compensated for it in some cases. These "unintended" consequences of the blockade are the focus of this article. We exploit the heterogeneous effect of the embargo on air distance across non-blockading trade partners to identify the role of distance on trade. Consistent with what is already mentioned, Qatar's exports are concentrated in the mineral fuels sector (88 per cent of total exports, see Supplementary Appendix Fig. A.2 for a breakdown), which was exempted from the blockade. Qatar's main trade partners (in terms of imports) are the USA and China in both the pre- and post-embargo periods, as shown in Fig. 5. In fact, there has not been any major shift in top trading partners as a result of the embargo. However, countries like Kuwait, Oman, and Turkey increased in ranking following the blockade, which matches with anecdotal evidence on governmental reactions to the embargo (see Section 5 for robustness checks for such political reactions.).

[6] This aggregation does not affect our econometric results, see explanation in Section 3.2 for the details.

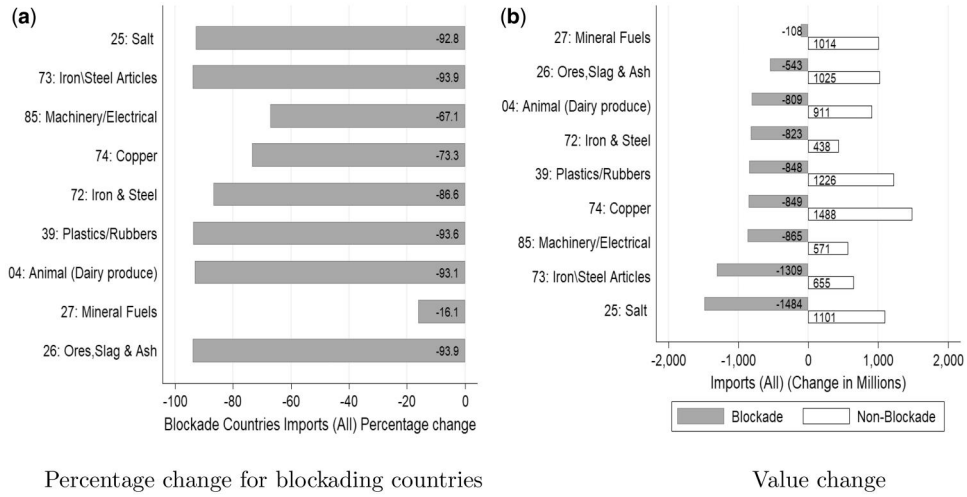

Figure 4. Sectoral import responses to the embargo.

Notes: Sectors chosen for being in the top 10th percentile before the embargo (June 2016 to June 2017) for the value of imports from blockading countries. Changes are calculated over the pre-post embargo period (June 2016 to June 2018).

Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

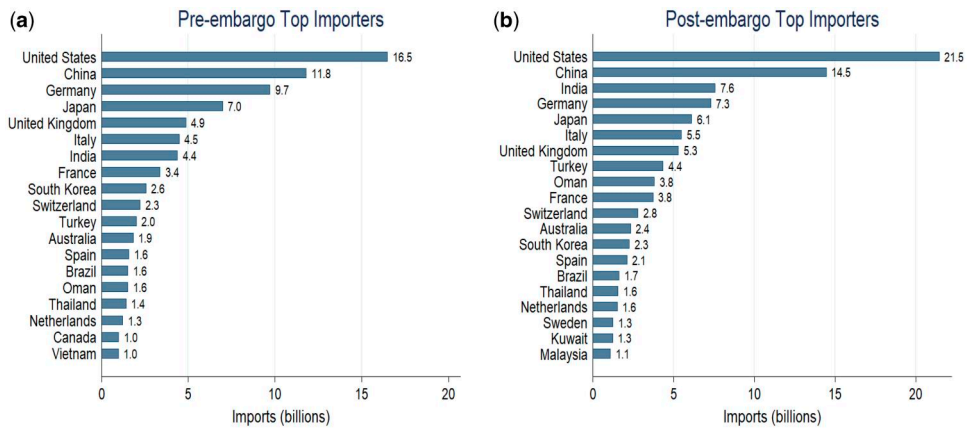

Figure 5. Qatar's top import sources.

Notes: Panel (a) Importers (top 10th percentile) based on average import value for the pre-embargo period: June 2016 to May 2017. Panel (b) Importers (top 10th percentile) based on average import value for the post-embargo period: July 2017 to June 2018.

Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

Air distances between Doha and major airports around the world are the focus of the analysis, and they are needed for the pre- and post-embargo periods. To calculate them, we proceed in two steps (see Supplementary Appendix A.2 for further details). First, we identify one major airport for every country (i.e. the one with the highest number of routes), except for the USA and Canada, where we differentiate between the Eastern and Western coasts, as done by Feyrer (2021).[7] Second, we calculate the distance including or excluding the airspaces of the blockading countries. This step is relatively easy for destinations with direct flights. For those airports that can only be reached through

[7] We follow Feyrer (2021) who uses the population distribution of 1970 to split trade between the two coasts and assigns 80 per cent of trade to the Eastern coast and 20 per cent to the Western coast. We do the same for two reasons. First, population distribution is unlikely to shift dramatically over this period. Second, since our study is focused on trade between Qatar and the rest of the world, this should not matter much. In fact, our results are robust to using either coast for the whole trade volume, as discussed in Section 5.

intermediate stops, we compute the shortest way pre- and post-embargo to connect Doha indirectly to a given destination, assuming only one stop and only using the pre-defined set of main airports. Bilateral air distances were calculated using raw geographic data. The bilateral great circle distance is the most common measure in gravity models (Feyrer 2019). However, following Campante and Yanagizawa-Drott (2018), we compute the geodesic distance, as it is more accurate compared to the great circle distance, as the latter assumes that the Earth is a perfect sphere. Such a proxy for distance overcomes the endogeneity issue due to economic and geopolitical factors present in actual flight distances (Campante and Yanagizawa-Drott 2018). Besedes et al. (2024) rely on information on air highways to approximate the actual flight paths. Compared to them, our proxy is less precise and may magnify measurement errors.

The calculated changes in distance display significant variation. Yemen and some African countries (i.e. Sudan and Ethiopia) are the ones to experience the largest percentage change, but some European countries (e.g. Germany, Sweden, UK) are also in the top 10th percentile of most affected trade partners (see Supplementary Figure A.4). On the other hand, Iran, Canada, and some Central American countries (e.g. Mexico, Costa Rica, Guatemala) are among the countries experiencing the smallest changes in distances. On average, distance increased by around 750 km but with significant heterogeneity across countries, which is the basis of our identification strategy.

In some specifications, we use Gross Domestic Product (GDP) data, taken from the Penn World Tables. In addition, data on the geographical size of countries are obtained from the CEPII GeoDist Database (Mayer and Zignago 2011). Finally, we use data from the UN (2019) for the stock of migrants (by nationality) in Qatar in 2015.

3.2 Methodology

Our empirical analysis rests on the standard gravity model, introduced by Tinbergen (1962) and further formalized by Anderson (1979) and Leamer and Levinsohn (1995). More recently, the derivation by Anderson and Van Wincoop (2003) became the mainstream theoretical underpinning of the gravity model. They derive the following classical gravity model based on identical preferences, profit-maximizing firms, and an iceberg cost:

$$X_{ijt} = \frac{y_{it}y_{jt}}{y_{wt}}\left(\frac{\tau_{ijt}}{P_{it}P_{jt}}\right)^{1-\delta} \tag{1}$$

where X_{ijt} denotes trade (imports) at time t between country i and country j. y_{it}, y_{jt}, and y_{wt} denote the incomes of country i, j, and the world, respectively. τ_{ijt} represents the bilateral resistance term that includes all possible barriers to trade between the two countries i and j at time t. P_{it} and P_{jt} are country-specific multilateral resistance terms (MRTs), which measure the average trade barrier (Anderson and Van Wincoop 2003). MRTs take into account the relative trade costs between i and j, determined by the trade costs between them relative to the average trade costs (Anderson and Van Wincoop 2003).

We follow the recent literature and estimate the gravity equation in its multiplicative form using a Poisson pseudo-maximum-likelihood (PPML) estimator proposed by Silva and Tenreyro (2006) to obtain consistent estimates. The PPML estimator corrects for the likely bias of Ordinary Least Squares estimates in the presence of heteroscedasticity. Another advantage of this estimator is that it provides a natural way to deal with zero trade flows that are common in trade data (Silva and Tenreyro 2006) and accounts for about 9 per cent of our observations. The exponential mean parametrization is standard for Poisson regression models (Cameron and Trivedi 2001), to which we add a stochastic term ν_{jt} and a time dummy for the post-blockade period, $Post_t$. Since our analysis focuses on the bilateral trade relationships only of Qatar (i.e. only the air distances between Qatar and its trade partners are affected by the embargo), we drop i since it is "fixed" to Qatar, while j identifies the various trade partners (except the blockading countries, which are excluded).[8] This leads to the following specification:

[8] Notice that the airspace was closed only to flights from and to Qatar. Hence, considering any other i country would be irrelevant since there is no change in the distance between i and j if i is different from Qatar.

$$X_{jt} = \exp[\beta_1 ln(Air_{jt}) + \pi_j + Post_t] \times \nu_{jt} \quad (2)$$

where Air_{jt} denotes travel distance by air and π_j are country fixed effects.

In order to estimate the specification in Equation (2), we collapse our sample into two periods of equal length (i.e. $t = 0$ and $t = 1$) around the start of the embargo. Specifically, we have 1 year before the embargo (June 2016–May 2017) and 1 year after the embargo (July 2017–June 2018).[9] This approach is similar to Martincus and Blyde (2013) and allows us to rule out seasonality effects. Based on our identifying assumptions, β_1 captures the causal effect of the increased transportation costs, represented by a rise in travel distance by air, on trade. Notice that given the way we constructed our air distance, all routes in our sample are affected. As a result, we are in fact comparing countries with a severe distance shock to those with a less intense one. However, we are not interested in estimating the effect of the embargo on total trade. Instead, we identify how the resulting change in trade is reallocated following the exogenous considerable variation in air distance with non-blockading countries.

It is important to clarify a few simplifying assumptions used to move from Equations (1) to (2). First, a standard practice in the trade literature is to proxy for bilateral trade costs, τ_{ijt}, using different observables such as distance, common language, and other bilateral variables. In this article, however, we focus on travel distance by air (i.e. Air_{jt}), although we will also consider other transportation costs (by land and by sea) in Section 5. Second, Head and Mayer (2014) indicate that modern practice has moved toward including importer (and exporter) fixed effects to control for the structural terms of income Y_{jt} in Equation (1). They point out that in a panel data model, these fixed effects must be time-varying. In our model, we cannot afford to include time-varying fixed effects because they would be collinear with our main variable. Therefore, we follow Feyrer (2021) and assume that the distance shock is orthogonal to changes in income.[10] Still, we include time-invariant country fixed effects (π_j) to take into account MRTs and other unobserved factors such as the distance from big markets, policy factors such as high tariff levels (Bacchetta et al. 2012), the initial population, land area, and other time-invariant pair-specific factors (e.g. common language, common border, etc.). The short nature of the period of investigation and having the data collapsed into two periods alleviate concerns with time-invariant country fixed effects to capture MRTs. The inclusion of such fixed effects allows for identifying the coefficient of interest by exploiting the variation of distance within country pairs. Finally, we use a time dummy, $Post_t$, to account for time-variant unobservables common to all trade partners (e.g. world income) in the post-embargo period. It helps us to rule out any common trend in trade over that period. Our inference is based on standard errors clustered at the country level.

As mentioned when describing the data, we aggregate sectoral data and use total trade (or total imports) in the econometric analysis. Notice that we would obtain the same estimates and standard errors if we used monthly and sectoral data. This is because we have a perfectly balanced dataset and a treatment (i.e. change in distance) that only occurs once and at the same time for all observations and across sectors. Still, in Section 5, we exploit the monthly frequency of the trade data to conduct an event study.

4. Results

Panel A of Table 1 presents our two benchmark specifications, employing total trade by air or imports by air as dependent variables (with the remaining panels reporting the results of several robustness checks, which are discussed in the next section). In the first column, our dependent variable is trade by air. The estimated coefficient of air distance is negative and statistically significant at the 5 per cent level. In particular, the elasticity of air distance with respect to trade shows that a 1 per cent increase in air distance translates to a −0.29 per cent decline in trade by air. As expected, the distance elasticity is much lower than the cross-sectional estimates, since these last estimates tend to capture other determinants, that are time-constant correlates of both trade and distance. The magnitude of our trade elasticity echoes recent findings also exploiting a plausibly exogenous change in distance (Martincus and Blyde 2013; Pascali 2017; Baniya et al. 2020; Feyrer 2019) and in particular, related to air distance (Besedes et al. 2024). The results for imports by air are very similar. This is not surprising,

[9] We exclude June 2017, the blockade month, because it is likely to capture erratic adjustments to the sudden change in trade routes. As discussed in Section 4, results are robust to the inclusion of June 2017.

[10] Among the robustness checks in Section 5, we verify that our results are not affected when we include GDP as a control.

Table 1. Results.

	Trade (Air)	Obs	Imports (Air)	Obs
A. Benchmark results	−0.288**	292	−0.285**	278
	(0.119)		(0.120)	
B. Control for log(Area) × $Post_t$	−0.493***	290	−0.493***	276
	(0.147)		(0.148)	
C. Only direct flights	−0.350***	134	−0.347***	132
	(0.116)		(0.117)	
D. USA/Canada with Eastern coast	−0.281**	288	−0.278**	274
	(0.122)		(0.123)	
E. USA/Canada with Western coast	−0.278**	288	−0.284**	274
	(0.123)		(0.124)	
F. Control for trade/import shares by other modes	−0.502**	292	−0.502**	278
	(0.211)		(0.197)	
	Trade (Non-Air)		Hydrocarbon Trade (All)	
G. Falsification	−0.074	314	0.321	154
	(0.525)		(0.800)	
	Export (All)		Exports (Air)	
H. Exports	0.274	290	−0.981	218
	(0.629)		(2.153)	
I. Control for log(real GDP)	−0.277**	288	−0.273**	274
	(0.116)		(0.117)	
J. Control for pre-embargo log(real GDP) × $Post_t$	−0.339**	274	−0.338**	260
	(0.154)		(0.156)	
K. Control for pre-embargo trade/imports × $Post_t$	−0.388*	292	−0.385*	278
	(0.231)		(0.233)	
L. Control for pre-embargo top importers × $Post_t$	−0.316***	292	−0.313***	278
	(0.108)		(0.110)	
M. Control for pre-embargo migrants × $Post_t$	−0.302***	274	−0.298***	266
	(0.115)		(0.117)	
N. High-GVC sectors	0.183	248	0.181	246
	(0.288)		(0.289)	
Low-GVC sectors	−0.559***	288	−0.559***	274
	(0.109)		(0.109)	
O. Drop the food sector	−0.373***	288	−0.370***	274
	(0.100)		(0.102)	
P. Drop strategic partners	−0.325***	284	−0.321***	270
	(0.105)		(0.107)	
Q. Drop countries that cut ties	−0.288**	280	−0.285**	270
	(0.119)		(0.120)	
R. Drop countries that cut or downgraded ties	−0.302***	274	−0.298**	266
	(0.115)		(0.117)	

Notes: $Post_t$ and country fixed effects included in all specifications; robust standard errors clustered at country level in parentheses; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Detailed results for every panel are reported in the tables in Supplementary Appendix B.

given that most exports are from the hydrocarbon sector, which does not rely on trade by air so that trade by air is almost entirely made up of imports. The difference in sample size between trade and imports is due to the loss of a few countries for which import values are zero throughout the period of investigation.

The full results for these specifications (Supplementary Appendix Table B.1) show an increasing trend of trade post-embargo (i.e. the estimates for $Post_t$ are positive and significant). As pointed out previously, we exploit the allocation of trade to different non-blockading partners given the differences in distance shock. We do not necessarily expect trade overall to fall since our sample comprised only non-blockading countries, which must have been used to compensate for lost trade with blockading partners. Supplementary Appendix Table B.1 also shows that if country fixed effects are not included, the elasticity of distance to trade is statistically significant and positive. Although counter-intuitive, this is due to the fact that Qatar's main trading partners (i.e. the USA and China) are located far away. Finally, Supplementary Appendix Table B.1 also shows that including June 2017, the month of the embargo does not alter our main results with coefficient estimates only slightly lower in magnitude.

These results are based on a 12-month aggregation (i.e. 1 year before and 1 year after the blockade), but it is important to assess how sensitive they are to this modeling choice. To do so, we re-estimate our benchmark specifications by varying the estimation window from 6 to 22 months (i.e. the maximum given by our data availability) before and after the blockade. Supplementary Appendix Figure B.1 provides a graphical illustration of these results and demonstrates that the effect is always negative and statistically significant, and quite persistent even with a window of 22 months. These graphs also illustrate a much larger effect when considering short windows, consistent with major disruptions in the short run.[11] Such big effects immediately following the blockade are also indicative of the absence of significant confounding factors driving our results, as they would take time to materialize. On the other hand, we cannot rule out that seasonality is a factor driving the results in aggregation windows different from 1 year. Supplementary Appendix Figure B.1 shows that the choice of different aggregation periods also affects the number of countries included in the analysis (bottom panel in the figure) because restricting the period leads to more countries never recording any trade or imports by air.[12]

Our identification strategy strongly relies on the unexpected nature of the embargo, together with the absence of pre-existing confounding trends. The evidence based on the news worldwide and the sudden drop in trade with blockading countries discussed in Section 2 already provides a strong indication that the blockade was not anticipated. But we can go further to assess the plausibility of these assumptions, by conducting a placebo test. Specifically, we assume the blockade to have happened in different months prior to the actual date. In Supplementary Appendix Figure B.2, we plot coefficient estimates from a moving window estimation for different placebo blockade dates. We assume the blockade to have happened in each subsequent month starting from June 2016 to April 2018 while we continue to use a symmetric sample of 1 year before and after the placebo blockade month. At the actual blockade date denoted by the vertical red line, Supplementary Appendix Figure B.2 reproduces the point estimate from Panel A of Table 1. Interestingly, the effects are statistically insignificant when assuming that the blockade occurred in any of the previous 10 months, indicating that there were no anticipation effects or other pre-existing confounding trends. It is the case that the effects are significant for placebo shocks earlier in 2016, which are unlikely to be related to the blockade and possibly due to the sharp fall in oil prices that took place between mid-2014 and early 2016. In fact, it was one of the largest drops in modern history (Stocker et al. 2018) and according to a report by the Qatar Planning and Statistics Authority (2018), Qatar's trade between 2014 and 2016 declined substantially before recovering in 2017. Again, the bottom panel of the figure illustrates the different number of countries included in the estimations.

5. Robustness analysis

Beyond the concerns on anticipation and pre-existing trends that we have already addressed, we cannot exclude that other unobserved factors could affect the negative role of air distance on trade. In the following, we summarize a series of robustness checks. The point estimates of the coefficients of interest are included in Table 1, while the full results are reported in Supplementary Appendix B.

5.1 Construction of distance

The procedure we follow to calculate the pre- and post-embargo distances is necessarily based on some assumptions (see Supplementary Appendix A.2). Importantly, they do not seem to drive our results. The use of only one airport per country may be particularly restrictive for large countries. However, controlling for the geographical size of a country with an interaction between $Post_t$ and the log of the area of a country does not affect our qualitative results, as shown in Panel B of Table 1. Restricting the analysis only to countries served with direct flights gives us a similar conclusion as well, as shown in Panel C of the same table. In addition, the results would barely change compared to our main specification if we were to ignore the distinction between the West and East coasts of the USA and Canada, as shown in Panels D and E of Table 1.

[11] Our results contrast with standard reactions to trade policies where adjustments are sluggish. To assess whether short-run disruptions may act as confonding factors, we replicate the main analysis leaving out the first 3 or 6 months after the embargo (results available upon request). The qualitative results are unchanged with corresponding elasticities only slightly larger (in absolute terms).

[12] Composition effects do not explain the results shown in Supplementary Appendix Figure B.1 (and later on in Supplementary Appendix Figure B.2). Similar patterns are observed when restricting the samples to those countries whose data are available for the whole sample period (results available upon request).

5.2 Substitution across transportation modes

Given that the blockade disrupted land and sea routes as well, restrictions on other modes of transportation may affect trade by air. In particular, following the closure of the only land border, 70 per cent of Qatar's imports were channeled through Hamad Port, the country's main port and the biggest in the Middle East (Mohamed 2020). Another concern is that air trade might have increased following the blockade, despite higher transportation costs, due to the closure of the land border. For instance, 80 per cent of Qatar's food imports came from blockading countries: Saudi Arabia and United Arab Emirates in particular (Saul and El Dahan 2017). Around 40 per cent of these food imports were channeled through the land border (Selmi and Bouoiyour 2020). Thus, the closure of the only land border led to the creation of new air and sea routes with countries such as Turkey, Iran, and Pakistan (Castelier and Pouré 2018). To control for these alternative channels, we introduce interaction terms between the share of trade or imports by land and by sea prior to the embargo and the indicator for the post-blockade period. Results are presented in Panel F of Table 1. When controlling for changes associated with land and sea pre-embargo trade shares, the distance elasticity to trade remains qualitatively similar.

5.3 Falsification checks

One legitimate concern may be that we are not only capturing the effect of changing air transportation costs but also other embargo consequences. That would be an issue if these consequences are somehow correlated with our distance shock variable. To assess this possibility, we run two falsification checks. We replicate the main results in Panel G of Table 1, but using non-air trade and the trade flows of hydrocarbon goods (i.e. HS2 item 27, referring to mineral fuels, oils, distillation products, etc.) as dependent variables. As expected, the shock to air distance does not directly affect trade by other modes (sea, land, or pipelines). Likewise, the hydrocarbon sector is also unaffected by the distance shock since oil and natural gas are mainly exported by pipeline or ships. Similarly, we do not find that the embargo-induced change in air distance has affected exports from Qatar, as shown in Panel H of Table 1. This provides further evidence in support of our causal estimates being driven solely by the shock in air distance.

5.4 Confounding factors

We cannot exclude the possibility that other time-varying factors are correlated with our distance shock variable and would then threaten our identification strategy. For instance, we have made the simplifying assumption that the change in air distance induced by the embargo is orthogonal to the change in GDP in non-blockading countries. To alleviate concerns about this, we control for the log of real GDP in Panel I of Table 1 and show that our main coefficient estimates are almost identical. Another concern is that our results would be biased if, for example, those countries highly affected by the distance shock would grow much faster than others, for other reasons. To explore this possibility, in Panel J of Table 1, we control for pre-existing trends based on initial GDP (i.e. we interact the log of pre-embargo real GDP with $Post_t$). The estimates for our main coefficient of interest are qualitatively similar. We could also be concerned that large trade partners prior to the embargo would be on different trends. We therefore augment our specification with interaction terms between the pre-embargo total trade or imports and $Post_t$ in Panel K, and an interaction of being a top importer in the top 10th percentile with $Post_t$ in Panel L. Results in these panels are very similar to our main estimates. Furthermore, the embargo has been reported to have had a detrimental impact on migrants in Qatar, possibly affecting their ability to send remittances or even their incentives to return to their countries of origin. Anecdotal evidence indeed suggests the exit of migrant workers from Qatar following the blockade (Toppa, 2017). To assess the role of such confounding factors, we control for pre-embargo migration stocks (by country) in 2015 in Qatar interacted with our $Post_t$ dummy. Panel M shows that our main coefficient is robust to the inclusion of this interaction variable.[13]

Finally, a concern might be that the existence of localized global value chains (GVCs) acts as an omitted variable where neighboring or close countries would be affected differently by the embargo. One way to examine this possibility is to use the data from the Eora Global Supply Chain dataset to

[13] A more agnostic concern may be that our estimates pick up unobserved changes occurring simultaneously in Africa or Asia. Our estimated elasticities are largely unaltered when adding an interaction between $Post_t$ and a dummy for Africa and/or Asia (results available upon request).

define which of the nine manufacturing sectors included in such data are characterized as high or low in GVC content.[14] Considering data over 2010–2015 (i.e. 2015 is the latest available year), the percentage of GVC-related trade in Qatar's total trade has been around 30%–34% (with the share in 2015 essentially the same as in 2010), although over the same period, there has been a steady decline in the percentage value of GVC-related output and traditionally traded output in Qatar's total output. Based on these data, the sector of "Petroleum, Chemical and Non-Metallic Mineral Products" stands out for its GVC content in 2013–2015, with the addition of also "Metal Products" in 2010–2012.[15] Excluding the former from our sample would not change our qualitative results, while the estimate for our coefficient of interest would not be significant if we only focused on that sector, essentially replicating the results in Panel G of Table 1, where we focus on the hydrocarbon sector. Excluding also "Metal Products" from the low-GVC sample would also not alter the conclusion for the low-GVC sample, while the coefficient remains insignificant for the high-GVC group, as shown in Panel N of Table 1.[16] Another way to examine GVCs and also other possibly omitted variables that are geographically related is to split the countries by quartiles based on the (pre-embargo) distance between Doha and the main airport of partner countries. When estimating our main specifications separately for each quartile (see Supplementary Appendix Table B.7), we always recover a negative estimate for distance, but it is only significant for the countries in the second and fourth quartiles, which include major exporters such as EU countries (second quartile), and Japan and the USA (fourth quartile—see Supplementary Appendix Table A.1 for the list of countries by quartile). In conclusion, these two exercises lead us to conclude that our result is not driven by close countries or GVCs.

5.5 Governmental reactions to the embargo

Anecdotal evidence suggests that the government of Qatar strongly supported the development of the domestic food sector to cope with the embargo and diversify its economy (Oxford Business Group 2019; Ibrahim 2020; Selmi and Bouoiyour 2020). Moreover, the country was able to use strategic food stocks that were previously created in anticipation of future disputes (Kerr 2018).[17] To assess the sensitivity of our results to this possibility, we replicate the main results in Panel O of Table 1 dropping the food-related sectors.[18] Our estimates still provide a similar conclusion.

Other anecdotal evidence points to the government of Qatar's reaction in strengthening its trade relations with specific trade partners, namely Iran and Turkey, after the blockade. Other examples are Oman and Kuwait being reported to have adopted a neutral position toward the embargo, offering credible alternatives for trade (Selmi and Bouoiyour 2020). While we cannot control for the specific reaction of potential trade partners, we assess the importance of such responses by replicating our main results excluding these four countries in Panel P of Table 1. Again, the estimates of our coefficients of interest are very similar to our main results.

5.6 Accounting for other countries that take position

At the onset of the embargo, countries other than the four blockading countries either downgraded diplomatic ties or cut them with Qatar. Countries that cut ties were Yemen, the Eastern government of Libya, Senegal, Maldives, Mauritania, and Comoros. Those that downgraded ties included Jordan, Djibouti, Chad, and Niger (Aljazeera 2021b). We check whether these political choices have any effect on our coefficient estimates by excluding the first set of four countries or all of them from our analysis. The results, shown in Panels Q and R of Table 1, indicate that our conclusions are not driven by the possible actions of these trade partners.[19]

[14] Data accessed at https://wits.worldbank.org/gvc/gvc-output-by-country.html. Unfortunately, Qatar is not included in the two other commonly used datasets analyzing GVCs, namely TiVA and WIOD, which cover more recent years.

[15] In 2015, the average GVC content of Qatar's manufacturing sector was 30.45 per cent reaching 34.69 per cent for "Petroleum, Chemical and Non-Metallic Mineral Products" and 29.85 per cent for "Metal Products". In 2010–2012 also the latter had a GVC content above the average.

[16] If we were to include the third highest GVC sector (i.e. "Electrical and Machinery"), the estimate for the high-GVC sample would remain insignificant. However, defining such sample to include only the second and third highest GVC sector (i.e. excluding "Petroleum, Chemical and Non-Metallic Mineral Products") would lead to a negative, albeit insignificant, coefficient for distance (results available upon request), which is consistent with the interpretation that the petroleum sector is the different one (and not necessarily only because of GVCs).

[17] A less severe diplomatic row with the three blockading Gulf States took place in 2014 (Ramani 2021).

[18] In total, HS2 sectors 1–24 are excluded. They include: live animals; animal products, vegetable products, animal or vegetable fats, and oils and their cleavage products; prepared edible fats; animal or vegetable waxes, prepared foodstuffs; beverages, spirits and vinegar; tobacco and manufactured tobacco substitutes.

[19] The first set of results would be unchanged if we were to keep Senegal in the sample, as their move seems to have been abrupt and Aljazeera (2021b) mentions they later re-established their ties with Qatar.

5.7 Event study

Exploiting the monthly frequency of the trade data, we can conduct an event study by adding lags and leads of the blockade month to our main specifications. The results are represented in Supplementary Appendix Figs B.3 (for total trade by air) and B.4 (for imports by air). Although monthly data are subject to seasonality, the figures show that most point estimates are positive (negative) and significant in the months before (after) the start of the embargo (and the estimates before June 2017 are jointly statistically significant, as well as for those after June 2017). Notwithstanding the caveat of the significant seasonality and adjustments right after the introduction of the embargo, these results provide further corroborating evidence in support of our findings.

Taking stock of these specifications, we can conclude that our benchmark results are qualitatively robust and that the point estimates are in the range of −0.3 and −0.5, which is a much smaller range than the elasticity recovered in previous studies based on cross-country comparisons.

6. Conclusion

Distance plays a major role in determining trade patterns, and understanding such role is paramount for policy choices and assessing the impact of a multitude of events and trade shocks. At the same time, it is not an easy endeavor to be able to provide a clear quantification of the causal effect of distance on trade flows. In this article, we exploit the sudden and unanticipated change in air travel costs experienced by Qatar in June 2017 because of the airspace blockade it faced from its most important trade partners. Based on this quasi-natural experiment, we uncover a distance elasticity of trade between −0.3 and −0.5. These results reduce by about half previous cross-sectional estimates (Frankel and Romer 1999; Disdier and Head 2008; Head and Mayer 2014) and are consistent with other recent estimates (Feyrer 2019, 2021; Martincus and Blyde 2013).

Compared to recent contributions based on other shocks to distance, our results are "cleaner" in that they are focused on one country only, escaping the concerns about cross-country comparisons, and based on a political shock unrelated to technological advances. Notwithstanding the advantages of our framework, it is also important to recognize its limitations. First, we can only quantify the distance elasticity of trade by air, not by other modes. Hummels (2001) documents higher elasticity between distance and freight costs for air freight, relative to ocean freight and land-based shipments. Taking these facts at face value suggests that our estimates are at the upper range of the possible distance elasticities, indicating that earlier estimates of the elasticity may overestimate even more their true size. Second, generalizing our results is a perilous exercise. Although the share of imports by air for Qatar is similar to what has been observed, for example, for the USA, the external validity is always a concern. The geographical position of Qatar makes it an ideal case study for this article (i.e. the blockading countries basically encircle its airspace) but also makes it more of a special case.

Supplementary data

Supplementary data is available at *Journal of Economic Geography* online.

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