THE GRAVITY OF DISTANCE: EVIDENCE FROM A TRADE EMBARGO

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The Gravity of Distance: Evidence from a Trade Embargo*

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Abstract

On June 5^{th}, 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 towards non-blockading countries to examine the effect of increased travel distance, due to re-routing, on bilateral trade. Based on a gravity model estimated with 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 downwards previous estimates of the distance elasticity.

Keywords: Embargo, Distance Effect, International Trade
JEL Classification: F14, L93

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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 (Feyrer, 2019, 2021; Campante and Yanagizawa-Drott, 2018) 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 to quantify the effect of Brexit on trade and income (see Bisciari, 2019, for an overview of these studies).

Our paper contributes to the literature on the impact of transportation costs on trade. Starting with the seminal finding of an elasticity of -0.85 by Frankel and Romer (1999), previous studies have proxied for the role of transportation costs by regressing trade flows on distance. Based on a meta analysis of estimates from 159 studies, Head and Mayer (2014) confirm the seminal result by Frankel and Romer (1999) with 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 of other trade factors correlated with proximity such as cultural factors, legal factors and migration 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 address the issue, recent papers have exploited transportation developments or external shocks on travel distance to isolate the causal effect of distance on trade among other outcomes (Baniya et al., 2019; Donaldson, 2018; Feyrer, 2019, 2021; Martincus and Blyde, 2013; Pascali, 2017). 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) who find a larger trade elasticity to distance with a range of -0.9 and -1.6.

Making use of an alternative identification of the causal effect of distance on trade, our paper revises such estimates. More specifically, we take advantage of an unexpected change in air travel costs due to the sudden closure of the airspaces surrounding Qatar. In comparison to the existing literature that exploits a shock that affected multiple countries (i.e. the closure of 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. Importantly, these 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 an older litearture while echoing recent findings that exploit different time-varying shocks to distance.

The remainder of the paper is organized as follows. Section 2 provides the necessary background to understand how the embargo imposed on Qatar can be used as a natural experiment to assess how unanticipated changes in trade costs affect bilateral trade relationships between Qatar and non-blockading countries. Section 3 summarizes the data construction (Section 3.1) and derives the specification to be estimated from a simplified and standard gravity model (Section 3.2). 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 limits of our analysis.

2 The Blockade Against Qatar

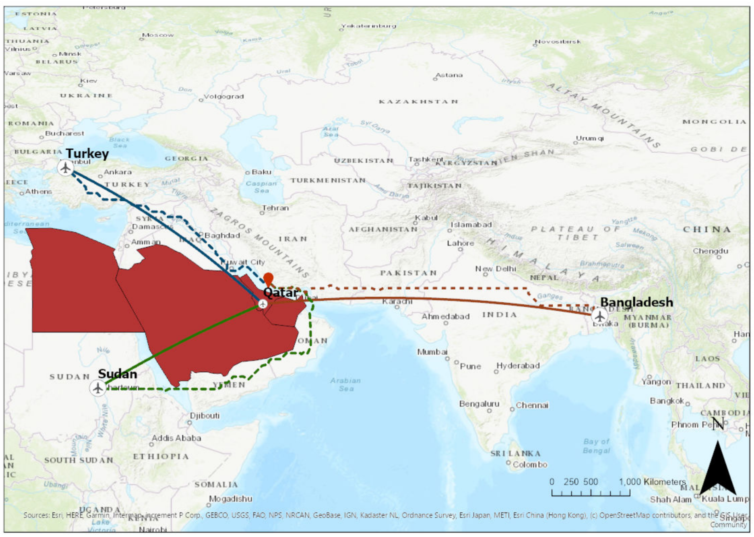
As a result of political differences, Bahrain, Saudi Arabia, United Arab Emirates (i.e. three neighboring member countries of the Gulf Cooperation Council along with Qatar, Kuwait and Oman) and Egypt unexpectedly severed all diplomatic ties with the state of Qatar on June 5^{th}, 2017. The blockading countries motivated their move based on Qatar's alleged financial support for terrorism, maintenance of a close relationship with Iran and interference in their internal matters, including through the Al-Jazeera network – allegations that Qatar refuted as baseless (Chughtai, 2020). Since then, the blockade has been eliminated with the signature of a reconciliation agreement in January 2021 (Aljazeera, 2021a), but its conclusion does not affect our analysis because the sample period of our investigation ends in 2019.

The unexpected nature of the blockade is key for our identification strategy. To support this claim, we use the GDELT (Global Data on Events, Location and Tone) dataset, which records daily news items in over 100 languages across the globe. We examine all the events categorized as "Impose embargo, boycott or sanctions" recorded between January 2015 and April 2020 for which the target actor is Qatar (see Online Appendix A for more details). As shown in Figure A.1 in Online Appendix A, there is a huge spike of news related to the embargo after its introduction (i.e. 1,381 and 1,456 in June and July 2017, respectively) while very few in the months before (i.e. 19 in May and overall 108 in the first 5 months of 2017). While most of the news before June 5^{th}, 2017, were miscoded, the remaining events refer to other threats of sanctions by the United States, whose motivations were unrelated with 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 two 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 (in total an area slightly larger than India) for all flights.[1] As a result, flights had to be diverted, resulting in longer routes from Hamad International Airport (i.e. Qatar's only international airport) to all airports worldwide. The closure of airspaces surrounding Qatar was a major issue for a country which was already importing 32% of goods by air prior to the embargo (as calculated between June 2016 and May 2017).[2]

Figure 1: Examples of flights diversion



Notes: Solid lines indicate pre-embargo routes; dotted lines indicate post-embargo routes.
Source: Authors' calculations using Geocoded Data.

Based on countries' geographical location, some routes were more affected than others. Figure 1 illustrates changes in air distance for a sample of pre- and post-embargo routes to three non-blockading countries. It is clear that flights to Sudan were more severely affected than those to Turkey and Bangladesh, only because of the location of these countries relative to Qatar and the blockading countries. This exogenous variation, only due to countries' geographical position, is key to our identification. Since the embargo also includes the closing

[1]Based on a clarification received from the Qatar General Authority of Customs, all flights were affected in regard of goods traded by air irrespective of whether they were by Qatar Airways or other airlines, as long as their destination was Qatar. Even the flights operated by Qatar's air forces to ship urgent goods had to avoid the blocked airspaces.

[2]As a comparison, Feyrer (2019) reports a share of 30% for the US (excluding trade with Mexico and Canada).

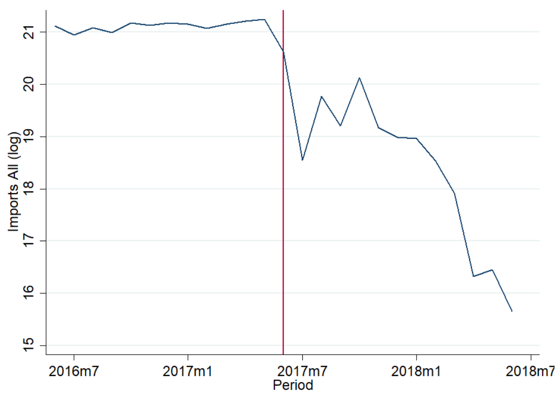
of the land border and some impediments on sea transportation, our analysis will further consider substitution effects for countries highly impacted by other transportation modes.[3]

This crisis was a first of its kind since the establishment of the International Civil Aviation Organization (ICAO) seventy 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 blockading countries. The decline of imports to Qatar from blockading countries was steep and unexpected, as shown in Panel (a) of Figure 2. Since as much as 60% of Qatar's trade originated from the boycotting countries (in particular Saudi Arabia and the UAE), immediately following the announcement of the embargo, imports to Qatar were reported to fall by about 40% (Oxford Business Group, 2019b). Such sharp fall in imports and overall trade (see Figure A.2 of the Online Appendix A) confirms the unexpected nature of the embargo, which will be further discussed in Section 4. Interestingly, Panel (b) of Figure 2 illustrates that exports to the blockading countries were not affected as much, most likely because the gas industry, which is the major export industry in Qatar (see Figure A.3 in Online Appendix A), was exempt from the blockade. In fact, Qatar continued to supply the UAE with natural gas via the shared Dolphin pipeline, and their cooperation on the Bunduq offshore oil field remained intact (Dudley, 2018).

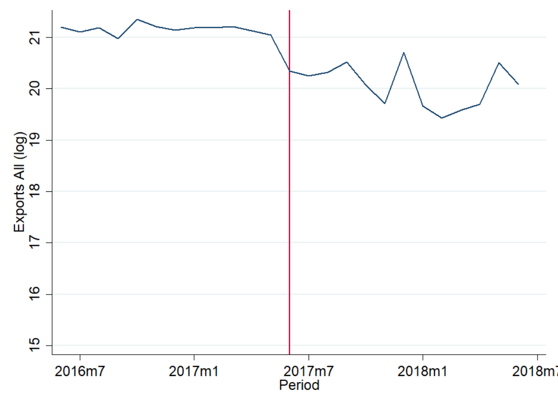
Faced with such a disruption, it is expected to observe some trade diversion whereby imports from and exports to non-blockading countries may increase. This is what appears to be the case, as shown in Panels (c) and (d) of Figure 2. Export and imports, in particular, increased after a short-term reduction upon announcement of the blockade. Moving to a sectoral analysis reveals further heterogeneity. Panel (a) of Figure 3 shows reductions of around 90% for imports from blockading countries in the top 10^{th} percentile of sectors in terms

[3]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 UAE, including the major bunkering site in Fujairah port (Khasawneh and Vukmanovic, 2017) and Jebel Ali port that used to receive big ships and loaded cargos into smaller vessels to be transported to Qatar (BBC, 2017). However, alternative ports were used by Qatar following these measures, and in February 2019, UAE eased these restrictions (Aljazeera, 2019).

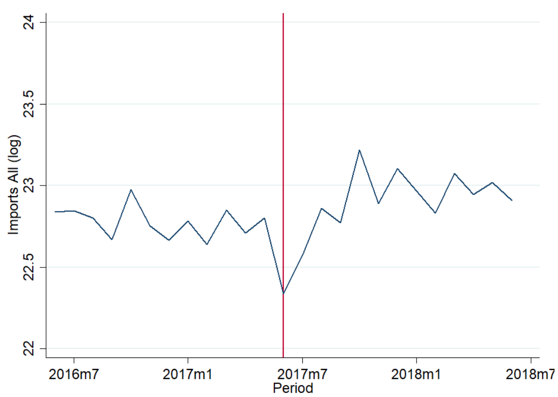
Figure 2: Trade responses to the embargo



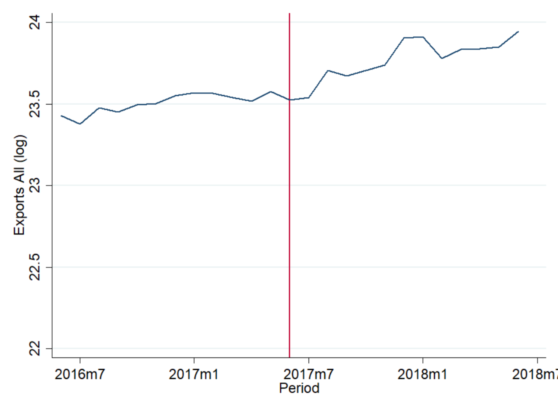
(a) Imports from blockading countries



(b) Exports to blockading countries



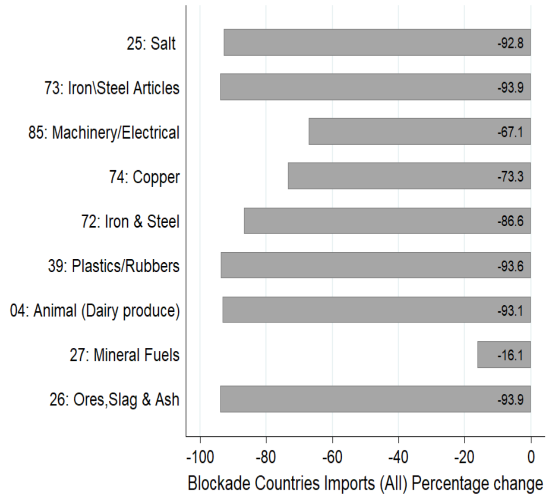
(c) Imports from non-blockading countries



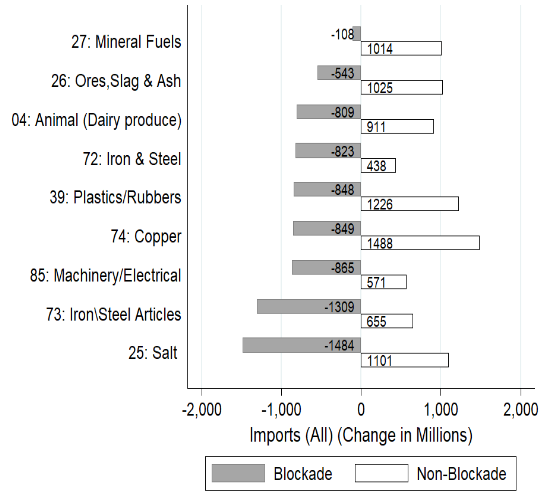
(d) Exports to non-blockading countries

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

Figure 3: Sectoral import responses to the embargo



(a) Percentage change for blockading countries



(b) Value change

Notes: Sectors chosen for being in the top 10^{th} percentile for value of imports from blockading countries; changes calculated over the period June 2018 - June 2016.
Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

of values (when comparing one year before the embargo and one 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. And such a dramatic fall has been somewhat compensated by increased imports from non-blockading countries, as shown in Panel (b) of Figure 3. Increased imports did not always make up for the shortfall from blockading countries while more than compensated it in some cases. As a result of these changes, the import's share by air for non-blockading countries increased from 35% to 40% between June 2016 and June 2018. These "unintended" consequences of the blockade are the focus of this paper. We exploit the heterogenous effect of the embargo on air distance across non-blockading trade partners to identify the role of distance on trade.

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 data on exports and imports at the 2-digit 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.[4]

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 period. In short, they are computed using the geodesic distance by including or excluding the blocked airspaces to obtain the distance before and after the embargo. To this end, we proceed in various steps (see Online Appendix B 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 US and Canada where we differentiate between the Eastern and Western coasts, as done by Feyrer (2021).[5] Second, we calculate the geodesic distance including or excluding the airspaces of the blockading countries using ArcGIS Pro software (and ArcMap for post-blockade distances). This step is relatively easy for direct flights. For indirect flights, we compute the shortest way pre- and post-embargo to connect indirectly Doha to a given destination, assuming only one intermediate stop and only using the pre-defined set of main airports. Importantly, our approximation in terms of change in distance fits pretty well with the change in travel times obtained from the Qatar Civil Aviation Authority for direct flights. In particular, the pairwise

[4] We provide more information on the trade data, including the ranking of top importing countries, in Online Appendix B.

[5] We follow Feyrer (2021) who uses the population distribution of 1970 to split trade between the two coasts and assigns 80% of trade value to the Eastern coast and 20% to the Western coast. We follow the same split 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.

correlation between distance and time shock for direct routes is around 0.50, and it rises to 0.72 for routes in the top 25^{th} percentile of the shock.

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, United Kingdom) are also in the top 10^{th} percentile of most affected trade partners (see Figure B.3 in Online Appendix B). 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 kilometers but with significant heterogeneity across countries, which is the basis of our identification strategy.

After dropping the four blockading countries, our final sample contains 144 countries.[6] The sample decreases to 137 countries when focusing on imports by air because our dataset does not record any import by air with 7 countries (i.e. Comoros, Djibouti, Liberia, Mauritania, Panama, Rwanda and Turkmenistan). In some specifications we use GDP data, taken from the Penn World Tables. In addition, data on the geographical size of countries are obtained from CEPII GeoDist Database (Mayer and Zignago, 2011). Finally, we use data from the UN (2019) for the stock of migrants (by nationality) present 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:

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

[6]See Table B.1 in Online Appendix B for the list of countries and details on excluded countries.

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 heteroskedasticity. Another advantage of this estimator is that it provides a natural way to deal with zero flows that are common in trade data (Silva and Tenreyro, 2006). 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. This leads to the following specification:

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

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

In order to estimate Equation (2), we collapse our sample into two periods of equal length around the embargo ($t = 0$ and $t = 1$). Specifically, we have one year before the embargo (June 2016-May 2017) and one year after the embargo (July 2017-June 2018). We exclude June 2017, the blockade month, because it is likely to capture many erratic adjustments to compensate for the sudden change in trade routes.[7] This approach is similar to Martincus

[7]Nonetheless, results are robust to the inclusion of June 2017, as discussed in Section 4.

and Blyde (2013), and allows us to rule out serial correlation as pointed out by Bertrand et al. (2004) for Difference-in-Difference estimation. Aggregating over two symmetric periods also rules out possible seasonality effects.[8] Based on our identifying assumptions, we estimate the effect of an exogeneous increase in air distance on trade. Our coefficient of interest, 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 Equation (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 paper, 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.[9] 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

[8] We would obtain the same coefficients and standard errors if we used a monthly and sectoral unit of observation. This is due to the fact that we have a balanced panel and the distance shock only varies at the country and time level. Thus, even with such more disaggregated data, we would still measure the average effect across months, sectors and countries.

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

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 the concern with time-invariant country fixed effects to capture MRTs. The inclusion of such fixed effects allows identifying the coefficient of interest by exploiting the variation of distance within the 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.

4 Results

Panel A of Table 1 presents our two benchmark specifications, employing total trade by air or imports by air as dependent variables and including country fixed effects (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% level. In particular, the elasticity of air distance with respect to trade shows that a 1% increase in air distance translates into a decline by -0.29% in trade by air. The results for imports by air are very similar. This is not surprising 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 the sample size between trade and imports is due to the loss of a few countries for which imports values are zero throughout the period of investigation.

The full results for these specifications (in Table C.1 of the Online Appendix C) 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 is comprised only of non-blockading countries, which must have been used to compensate for lost trade with blockading partners. Table C.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 omission of country fixed effects and the fact that Qatar's main trading partners (i.e. the US and China) are located far away. Finally, Table C.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.

Panel A of Table 1 shows our main results based on a 12-month aggregation but it is important to assess how sensitive they are to this modeling choice. To do so, we re-estimate our benchmark specifications varying the estimation window from 6 to 22 months (i.e. the maximum given our data availability) before and after the blockade. Figures C.1 and C.2 in Online Appendix C provide a graphical illustration of these results and demonstrate that the effect is always negative and statistically significant, and quite persistent even with a window of 22 months. These figures also illustrate a much larger effect when considering short windows, consistent with major disruptions in the short run. 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 one year. Figures C.1 and C.2 show that the choice of different aggregation periods also affects the number of countries included in the analysis (bottom panel in the figures) because restricting the period leads to more countries never recording any trade or imports by air.

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 news worldwide and the sudden drop of trade with blockading countries discussed in Section 2 already provides 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 Figures C.3 and C.4 in Online Appendix C, 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 one year before and after the placebo blockade month. At the actual blockade date denoted by the vertical red line, Figures C.3 and C.4 reproduce the point estimates 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 was 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 2014-2016 declined substantially before recovering in 2017. Again, the bottom panel of these figures 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 Online Appendix C.

Construction of distance. The procedure we follow to calculate the pre- and post-embargo distances is necessarily based on some assumptions (see Online Appendix B). 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 and the log of the area of a country does

Table 1: Main Table

	Trade (Air)	Obs	Imports (Air)	Obs
A. Benchmark results[a]	-0.288**	292	-0.285**	278
	(0.119)		(0.120)	
B. Control for log(Area)[b]	-0.493***	290	-0.493***	276
interacted with Post	(0.147)		(0.148)	
C. Only direct flights[c]	-0.350***	134	-0.347***	132
	(0.116)		(0.117)	
D. US/Canada with Eastern coast[d]	-0.281**	288	-0.278**	274
	(0.122)		(0.123)	
E. US/Canada with Western coast[e]	-0.278**	288	-0.284**	274
	(0.123)		(0.124)	
F. Control for trade/import[f]	-0.502**	292	-0.502**	278
shares by other modes	(0.211)		(0.197)	
	Trade (Non-Air)		Hydrocarbon Trade (All)	
G. Falsification[g]	-0.074	314	0.321	154
	(0.525)		(0.800)	
H. Control for log(real GDP)[h]	-0.277**	288	-0.273**	274
	(0.116)		(0.117)	
I. Control for pre-embargo[i]	-0.339**	274	-0.338**	260
GDP (log) interacted with Post	(0.154)		(0.156)	
J. Control for pre-embargo[j]	-0.388*	292	-0.385*	278
trade/imports interacted with Post	(0.231)		(0.233)	
K. Control for pre-embargo top[k]	-0.316***	292	-0.313***	278
importers interacted with Post	(0.108)		(0.110)	
L. Control for pre-embargo[l]	-0.302***	274	-0.298***	266
migrants interacted with Post	(0.115)		(0.117)	
M. Drop the food sector[m]	-0.373***	288	-0.370***	274
	(0.100)		(0.102)	
N. Drop strategic partners[n]	-0.325***	284	-0.321***	270
	(0.105)		(0.107)	
O. Drop countries that cut ties [o]	-0.288**	280	-0.285**	270
	(0.119)		(0.120)	
P. Drop countries that[p]	-0.302***	274	-0.298**	266
cut/downgrade ties	(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 Online Appendix C as follows: [a] in Table C.1; [b], [c], [d] and [e] in Table C.2; [f] and [g] in Table C.3; [h], [i], [j], [k] and [l] in Table C.4; [m], [n], [o] and [p] in Table C.5.

not affect our conclusions, as shown in Panel B of Table 1. Restricting the analysis only to using direct flights does not change our conclusions either, as shown in Panel C of the same table. In both exercises, the coefficients of interest increase in magnitude (in absolute values) and become significant at 1% level. The results would barely change if we were to ignore the distinction between the West and East coasts of the US and Canada, as shown in Panels D and E of Table 1 (full results for this subsection are reported in Table C.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% 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% of Qatar's food imports came from blockading countries: Saudi Arabia and UAE in particular (Saul and El Dahan, 2017). Around 40% of 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). Therefore, if we do not control for land route, we may underestimate the overall effect of the embargo on trade. To correct for these alternative channels, we control for 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 (and Table C.3). When controlling for changes associated with land and sea pre-embargo trade shares, the distance elasticity to trade raises in magnitude to -0.5.

Falsification checks. One legitimate concern is that we are not only capturing the effect of changing air transportation costs but also other embargo consequences. That would be an issue if those 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 (and Table C.3) 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. This provides further evidence in support of our causal estimates being driven solely by the shock in air distance.

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 H 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 I of Table 1 we control for pre-existing trend based on initial GDP (i.e. we interact the log of pre-embargo real GDP with Post). 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/imports and Post or being a top importer in the top 10^{th} percentile prior to the embargo. Results presented in Panels J and K of Table 1 are unaltered. Finally, the embargo is reported to have had a detrimental impact on migrants in Qatar and 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 factor, we control for pre-embargo migration stocks (by country) in 2015 in Qatar interacted with our Post dummy. Panel L shows that our main coefficient is robust to the inclusion of this interaction variable (see Table C.4 for full results of this subsection.)

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 (Ibrahim, 2020; Selmi and Bouoiyour, 2020; Oxford Business Group, 2019a). Moreover, the country was able to use strategic food stocks that were previously created in anticipation of future disputes (Kerr, 2018).[10] To assess the sensitivity of our results to this possibility, we replicate the main results in Panel M of Table 1 dropping the food-related sectors.[11] Our estimates do increase (in absolute value) and become more precisely estimated but they 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 towards 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 N of Table 1. Again, the estimates of our coefficients of interest are very similar to our main results (see Table C.5 for full results of this subsection).

Account 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, Eastern government of Libya, 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

[10] A previous less severe diplomatic row with the three blockading Gulf states took place in 2014 (Ramani, 2021).

[11] 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.

coefficient estimates by excluding the first set of 4 countries or all of them from our analysis. The results, shown in Panels O and P of Table 1, indicate that our conclusions are not driven by the possible actions of these trade patterns (full results reported in Table C.5).[12]

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 paper, 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 based on variations of other quasi-experimental settings (Feyrer, 2021, 2019; 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 and based on a political shock unrelated to technological advances. Notwithstanding the advantages of our framework, it is also important to recognize its limitation. First, we can only quantify the distance elasticity of trade by air, not by other modes. Hummels (2001) documents higher elasticity between

[12] 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.

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 on 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 U.S., the external validity is always a concern. The geographical position of Qatar makes it an ideal case study for this paper (i.e. the blockading countries basically encircle its airspace) but also makes it more of a special case. Furthermore, the ability of the government of Qatar to use its own revenue from exports to cope with the detrimental consequences of the embargo is a case in point. While we acknowledge its potential role, it is nonetheless not obvious to conjecture how such a governmental reaction would be so correlated with the change in distance that it would overturn our main findings.

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Online appendix for:

The Gravity of Distance: Evidence from a Trade Embargo

June 2022

This document contains a set of appendices with supplemental material.

Appendix A Additional Information on the Embargo

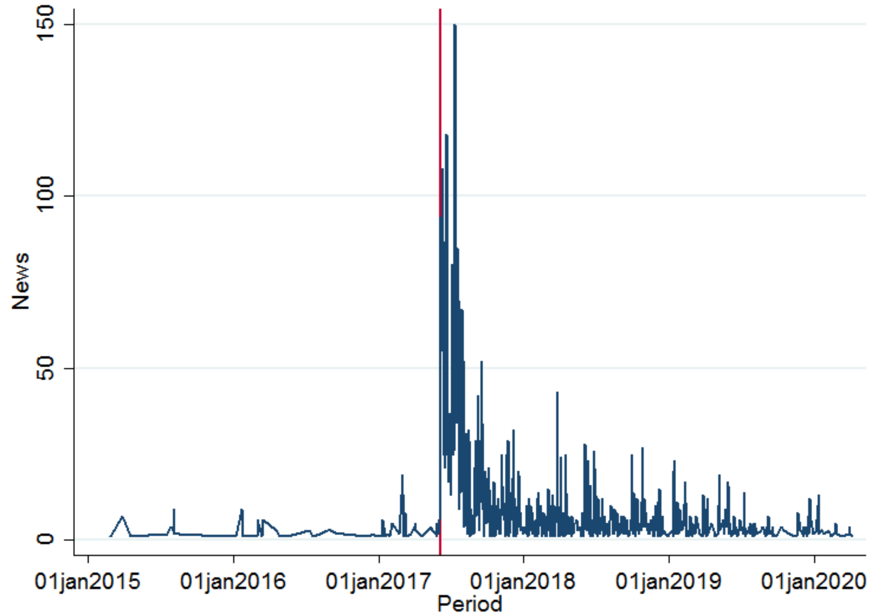
Interestingly, two weeks before the embargo, Qatar's state news agency was hacked and fake statements attributed to the emir were issued (Browning, 2017). The statements were aired on different UAE and Saudi-owned networks, leading to political tensions between the countries in the area (Chughtai, 2020). Despite the fact that relationships between Gulf States involved were already strained in the two 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 GDELT (Global Data on Events, Location and Tone) project (Leetaru and Schrodt, 2013), which 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). The dataset can be used to extract information on a pre-defined set of events or actors. The events and actors are based on the Conflict And Mediation Event Observations (CAMEO) system, which provides a systematic classification to study different types of international interactions (Schrodt, 2012). The CAMEO coding system includes a list of 15,000 events. We focus on events categorized as "Impose embargo, boycott or sanctions" (CAMEO Code 163) and examine all the events in this category between January 2015 and April 2020 for which the target actor is Qatar. In total, we find 6,876 news on embargo where the target country is Qatar over this period. In Figure A.1, we plot the embargo news from January 2015 to April 2020.[1] The figure clearly displays a huge spike of news related to an embargo on Qatar after the date of the embargo denoted by the red vertical line. In the first five months of 2017, only 108 news related to a boycott, a blockade or an embargo with Qatar were reported and a closer look shows that most of these news are miscoded. Some are in fact related to the post-embargo period while a few records refer to a threat of or actual

[1] A similar picture is obtained when we exclude news reported in the blockading countries.

boycott by the US on Qatar or some of its individuals for reasons not related to the 2017 embargo. In contrast, 1,381 news items are recorded for June and a further 1,456 for July. This piece of evidence clearly demonstrates that the embargo was not anticipated.

Figure A.1: News about the embargo

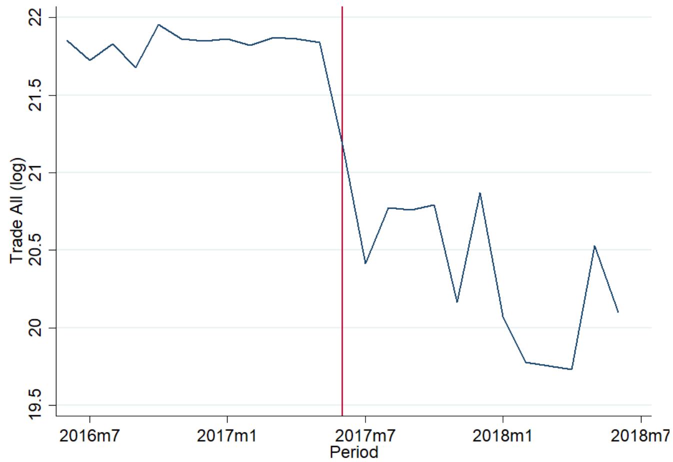


Notes: Embargo news about Qatar released in any country.
Source: Authors' calculations using GDELT Data.

Consistenly with the lack of anticipation of the blockade, trade with blockading countries collapsed immediately as shown in Figure A.2, essentially driven by a drop of imports (Panel (a) of Figure 2), as exports (Panel (b) of Figure 2) of Qatar are concentrated in the mineral fuels sector, as shown in Figure A.3, which was exempted from the blockade and represents 88% of total exports. In fact, at the onset of the blockade, Qatar asserted that its exports of liquefied natural gas (LNG) to its biggest buyer in Japan, Jera Co, will continue despite the blockade (Bloomberg News, 2017). Furthermore, despite banning Qatari ships from some major seaports, 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 based on the International Gas Union (Selmi and

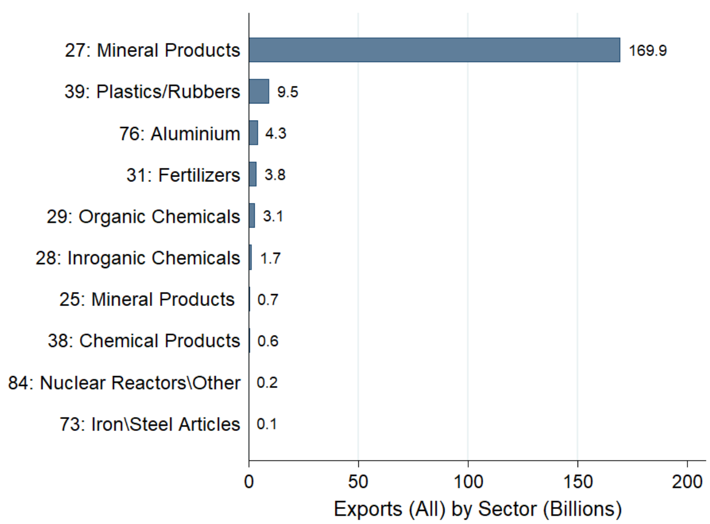
Bouoiyour, 2020).

Figure A.2: Trade between Qatar and blockading countries



Notes: Trade (All) with Qatar from four blockading countries over Jun2016-Jun2018.
Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

Figure A.3: Qatar's top exported sectors



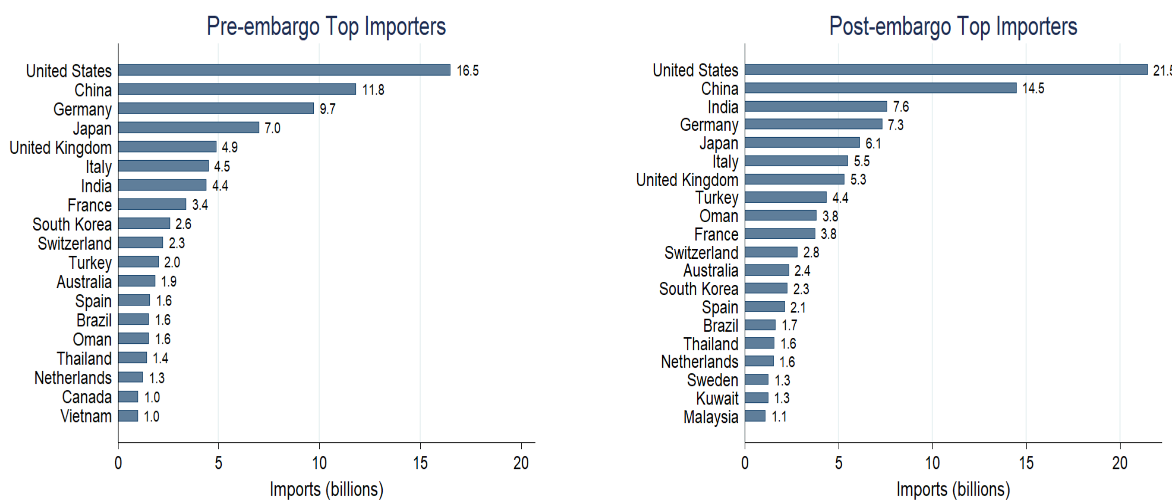
Notes: Exports by Sector in top 10^{th} Percentile over the pre-embargo period Jun2016-May2017.
Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

Appendix B Data

Appendix B.1 Trade Data

Import and export data are recorded following the 2012 version of the Harmonized Commodity Description and Coding Systems (HS). Imports are recorded at their cost, insurance and freight (c.i.f.) values while exports are recorded at their free on board (f.o.b.) values. Exports that we use in the analysis include Re-Exports. All trade values are expressed in Qatari Riyals. The Qatari Riyal is fixed against the US Dollar, so exports in foreign currencies are converted using the official exchange rate of 3.64 QAR for one USD.

Figure B.1: Top importers to Qatar



(a) (b)

Notes: Panel (a) Importers (top 10^{th} percentile) based on average imports value for the pre-embargo period: Jun2016-May2017. Panel (b) Importers (top 10^{th} percentile) based on average imports value for the post-embargo period: Jul2017-Jun2018.
Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

Qatar's main trade partners (in terms of imports) are the US and China in the pre- and post-embargo period, as shown in Figure B.1. Overall, there was no 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. Over the sampled period, imports' share by air for non-blockading countries increased from

35% to 40%, with sectors such as natural and cultured pearls, precious metals, imitation jewelry and coins being traded almost exclusively by air.

Appendix B.2 Distance

Appendix B.2.1 Airspace Background

International transportation by air has seen the establishment of its core principles in the Chicago Convention of 1944, which led to the creation of the International Civil Aviation Organization (ICAO) as a body responsible for overseeing standards and matters related to civil aviation (ICAO, 2018). Qatar and blockading countries are all members of this convention. The first Article of the Chicago convention states that: "The contracting States recognize that every State has complete and exclusive sovereignty over the airspace above its territory." Where territory is defined in Article II as "... the land areas and territorial waters adjacent thereto under the sovereignty, suzerainty, protection or mandate of such State." The adjacent territorial waters are specified to be a maximum of 12 nautical miles from a country's baseline according to Article 3 of the United Nations Convention on the Law of the Sea. The airspace upon which a state is responsible for operational control by ICAO, is referred to as the Flight Information Region (FIR) (Grief, 2009). The FIR of coastal countries includes the airspace above its land and sea territories, in addition to any areas assigned by ICAO (Grief, 2009). The Chicago convention itself only asserts the sovereignty of airspace, as a result; it was followed by the Transit Agreement of 1945 which provides for the freedom of overflight and landing for technical reasons (CAPA Center for Aviation, 2017). All blockading countries are part of this agreement except for Saudi Arabia. Therefore, Saudi Arabia is the only blockading country that can legally impose an airspace ban, but there is no enforcement power of commercial international compacts to impose compliance with the agreement on the other blockading countries (CAPA Center for Aviation, 2017).

The airspace of Qatar is very small, so airlines have mostly relied on Bahrain's airspace (Macheras, 2018). This is due to historical considerations where the Gulf region's FIR has been defined from a military efficiency perspective before their independence from the UK in 1971 and had not been changed afterwards for administrative convenience (Macheras, 2018). As a result, it would have cost Qatar Airways all of its operations had it not been for the two routes to and from Qatar that Bahrain allowed through its FIR (Hamphrey et al., 2017). After the blockade ended in January 2021, ICAO granted Qatar its agreement to establish its own Flight Information Region (FIR) and Doha Search and Rescue Region (SRR) (MOTC, 2021). In March 2022 the DOHA FIR has been officially established (Qatar Civil Aviation Authority, 2022).

Appendix B.2.2 Air distance computation

This section provides details on the main steps that we followed to compute the distance between Doha airport and the world's airports before and after the embargo.

We identify the major airport of a country and its location, by using the one with the highest number of routes, based on the routes and airports datasets from OpenFlights (2021). The United States and Canada are an exception in that we use two airports for each. In line with Feyrer (2019), we differentiate the distance to the Eastern and Western coasts. The two airports used for the US are Hartsfield-Jackson Atlanta International airport and Los Angeles International airport, the first and third busiest airports based on the routes datasets, as the second busiest airport (Chicago O'Hare International Airport) is on the same "coast" as the first one. As for Canada, the first and third busiest airports are Toronto Pearson International Airport and Vancouver International Airport. Similar to the US, the second busiest airport (Montreal Pierre Elliott Trudeau International Airport) is on the same side as the first one.

The routes dataset includes 67,663 routes between 3,321 airports across the globe as of June 2014. It provides data on airports that have routes connecting them and the airline used for that connection. Moreover, the data is directional as it differentiates between flights from A to B versus those from B to A. As a result, we have airports listed by source and

Table B.1: List of Countries

Afghanistan	El Salvador	Madagascar	Slovakia
Albania	Eritrea	Malawi	Slovenia
Algeria	Estonia	Malaysia	Somalia
Argentina	Ethiopia	Maldives	South Africa
Armenia	Fiji	Mali	South Korea
Australia	Finland	Malta	Spain
Austria	France	Mauritania	Sri Lanka
Azerbaijan	Gabon	Mauritius	Sudan
Bangladesh	Georgia	Mexico	Suriname
Belarus	Germany	Moldova	Swaziland
Belgium	Ghana	Morocco	Sweden
Benin	Greece	Mozambique	Switzerland
Bhutan	Guatemala	Myanmar	Syria
Bolivia	Guyana	Namibia	Taiwan
Bosnia and Herzegovina	Haiti	Nauru	Tanzania
Brazil	Honduras	Nepal	Thailand
Bulgaria	Hong Kong	Netherlands	Tunisia
Burundi	Hungary	New Zealand	Turkey
Cambodia	Iceland	Nicaragua	Turkmenistan
Cameroon	India	Nigeria	Uganda
Canada	Indonesia	Norway	Ukraine
Chad	Iran	Oman	United Kingdom
Chile	Iraq	Pakistan	United States
China	Ireland	Panama	Uruguay
Colombia	Italy	Paraguay	Uzbekistan
Comoros	Japan	Peru	Venezuela
Congo (Brazzaville)	Jordan	Philippines	Vietnam
Costa Rica	Kazakhstan	Poland	Yemen
Cote d'Ivoire	Kenya	Portugal	Zambia
Croatia	Kuwait	Puerto Rico	Zimbabwe
Cuba	Laos	Reunion	
Cyprus	Latvia	Romania	
Czech Republic	Lebanon	Russia	
Denmark	Liberia	Rwanda	
Djibouti	Libya	Senegal	
Dominica	Lithuania	Serbia	
Dominican Republic	Luxembourg	Sierra Leone	
Ecuador	Macedonia	Singapore	

Notes: we exclude the following countries: No air distance (Andorra, Curacao, Kosovo, Liechtenstein, Monaco, Pitcairn and San Marino). Furthermore, from the above list the following are excluded when imports (air) is used as a dependent variable since their import values are all zero (Comoros, Djibouti, Liberia, Mauritania, Panama, Rwanda and Turkmenistan).

by destination and check that both dimensions identify the same airport in each country.[1] The route dataset also gives data on the number of stops for a given route which allows us to identify direct routes versus indirect routes (i.e. those with a stop between them). The airports dataset corresponds to January 2017 and provides data on an airport's latitude and longitude. It records the old Doha international airport, coding it using the ICAO code only: OTBD. On the other hand, the routes dataset is referring to the IATA code only for Qatar: DOH that corresponds to the new Hamad international airport. Doha international airport is the old airport that ceased operations for commercial flights in 2014 when Hamad international airport was open. Both airports are very close in terms of location, so we use the latitude and longitude provided by the airport dataset.[2]

In total, we have 144 countries, which are listed in Table B.1. We excluded the following countries for which no routes or airport data are available: Andorra, Curacao, Kosovo, Liechtenstein, Monaco, Pitcairn and San Marino. For each sampled country, we identify one major airport.

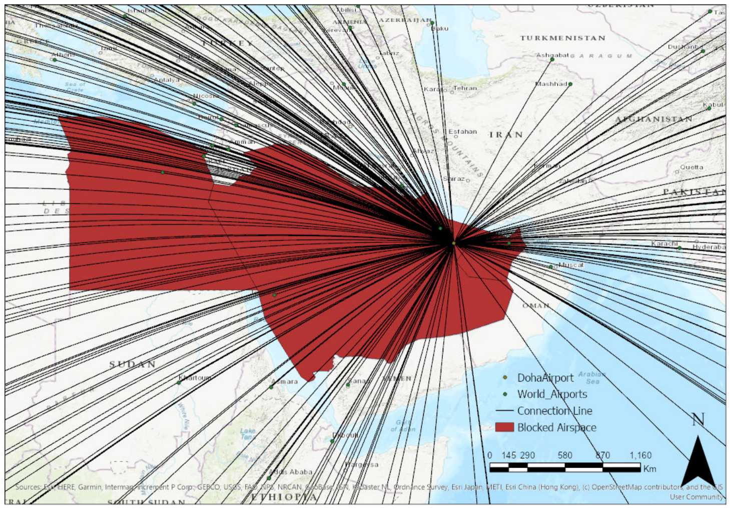
Bilateral air distances were calculated using raw geographic data. The bilateral great circle distance is the most common measure in gravity models (Feyrer, 2019). However, following Campante and Yanagizawa-Drott (2018) we compute the geodesic distance, as it is more accurate compared to the great circle distance, which assumes a perfectly spherical earth (Campante and Yanagizawa-Drott, 2018). Clearly, this is not the actual flight distance, but using it as a proxy for distance overcomes the endogeneity issue due to economic and geopolitical factors present in actual flight distances (Campante and Yanagizawa-Drott, 2018). For the pre-blockade distance we use the *'generate near table tool'* in ArcGIS Pro that gives us the geodesic distance between Doha and all other airports, as shown in Figure B.2.

[1] There are some exceptions: Iran would have two different airports if using the origin or destination, and chose the one with the highest number of routes (Mashhad International Airport); we identify multiple airports for Syria because the maximum number of flights for each is one, and chose the airport in the capital city (Damascus International Airport); Swaziland has different airports by origin and destination with one route each, and chose the airport in the Capital City (King Mswati III International Airport).

[2] Specifically, the latitudes and longitudes for Doha and Hamad international airport are 25.2647° N, 51.5596° E and 25.2609° N 51.6138° E, respectively.

The geodisc distance in GIS is used to define airplanes' path, giving us the shortest distance between two points on a spheroid (ellipsoid) earth's surface (ArcGIS Pro, nd).

Figure B.2: Distance between Doha and airports



Notes: Distance between Doha and main airports pre-embargo using a screen shot of *ArcGIS Pro* software.

However, since we differentiate between direct and indirect routes, we also compute the bilateral distance between all pairs of airports. To differentiate direct and indirect flights, we use the routes dataset (ignoring the directionality dimension). Specifically, we transform all Doha destination as Doha source in the routes dataset and merge it with a file that includes the distance between Doha and all main airports from GIS computation. In our flight sample, we have a total of 176 indirect flights and 59 direct flights. Our distinction between direct and indirect flights match with Qatar Civil Aviation Authority (CAA)[3] data for 159 indirect routes and 54 direct routes. Exceptions consist of 19 routes that we record as indirect while CAA record them as direct. As a result, we argue that the use of *openflight* data to distinguish between direct and indirect flights fits well with the CAA data. The distance is then assigned as follows:

- For direct routes, we take the computed distance directly from the resulting ArcGIS Pro table.

[3]Data were obtained by request from the Air transportation Department in Qatar Civil Aviation Authority.

- For indirect routes, we make the following two assumptions: (a) there is a direct flight from Doha to any intermediary airport, (b) the intermediary airport is the main airport in that country. Then for every Doha and final destination airport with indirect route we include a list of all airports with direct routes as a potential intermediary stop. We then chose the route that gives the minimum total distance.

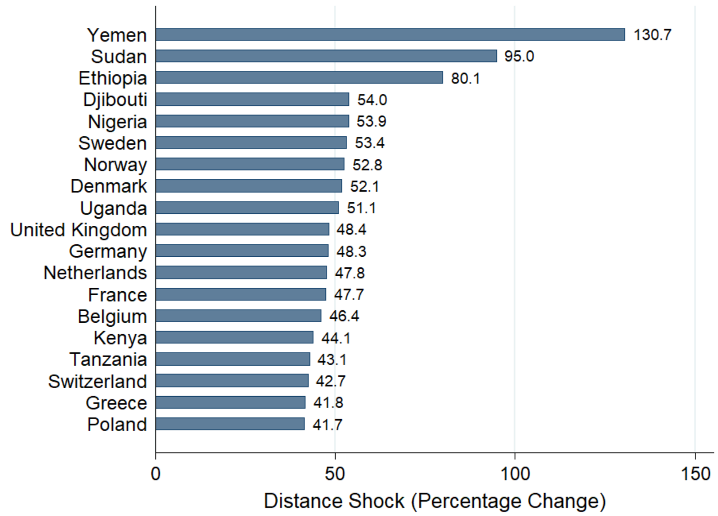
We compute the post-blockade distance using ArcGIS Pro and ArcMap.[4] The main tool for this purpose is the *'least cost tool'*, which allows us to compute the geodesic distance from Doha to all airports by avoiding the blocked airspaces. The FIR of blockading countries were obtained from ICAOGIS (2015).

Once we obtain the distance from Doha to all main airports using GIS, we follow the same logic mentioned in the pre-embargo distance to distinguish direct and indirect routes. However, for indirect routes we use the new post-embargo distance from Doha to main direct-route airports but the pre-embargo bilateral distance connecting intermediate and final airports. This is because the route between intermediate and final airport is not affected as it does not go over the blockade airspaces. Distances are expressed in Kilometers in our final dataset for better representation in descriptive statistics.

Based on this procedure, we obtain a proxy for the changes in transportation shocks for each country, as depicted in Figure B.3 for the top 10^{th} percentile. We argue that the change in distance we compute using GIS correlates pretty well with data we obtained from Qatar Civil Aviation Authority (CAA) on the time change for direct flights between pre- and post-embargo periods. Specifically, the data compare time in minutes in May 2017 and May 2018 for direct flights. In fact, the pairwise correlation between time and distance for CAA direct flights is 0.47, increasing to 0.72 if we limit it to countries with a time shock in the top 25^{th} percentile. Therefore, this strong correlation points to a relatively good quality of our procedure in approximating change in transportation costs.

[4] ArcGIS Pro and Arcmap have similar functionalities, the difference is that Pro is an updated version which is server-based as opposed to Arcmap. Moreover, Arcmap is an older version and has more functionalities that are gradually being moved into ArcGIS Pro.

Figure B.3: Highest distance shock countries



Notes: Countries with distance shock in the top 10^{th} percentile.
Source: Authors' calculations using geocoded data.

Appendix B.3 Summary Statistics

Tables B.2 reports the summary statistics of our main variables over the estimation period. Qatar's trade and imports by air with non-blockading countries increased on average post-blockade. However, the changes have not been homogeneous. Although there was no major shift in top trading partners as a result of the embargo (Figure B.1), there was quite a lot of variation in terms of distance shocks among the top importers (panel (a) of Figure B.4). However, there is no clear match that a rise in distance is associated with imports falling (panel (b) of Figure B.4). This consideration emphasizes the importance of conducting an econometric analysis controlling for country fixed effects to properly identify the role of distance.

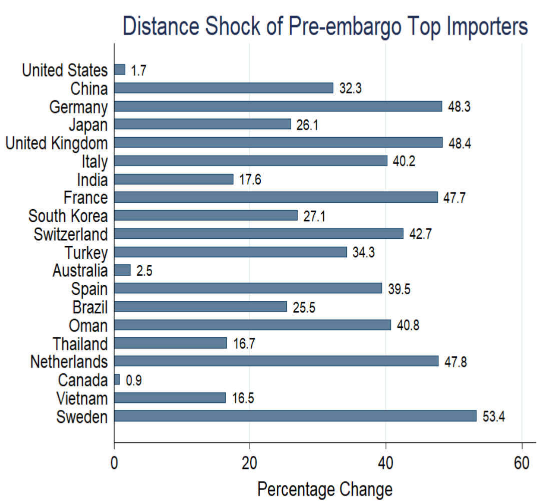
Similarly, correlations over time can be informative. In Figure B.5, we plot the values of imports distinguishing countries in the top and bottom 10^{th} percentile in terms of distance shock. The figure shows a stronger fall in imports for the countries highly affected by the change in distance. Moreover, it highlights the presence of high monthly variations, which is one reason for collapsing data into two periods for the econometric analysis.

Table B.2: Summary statistics

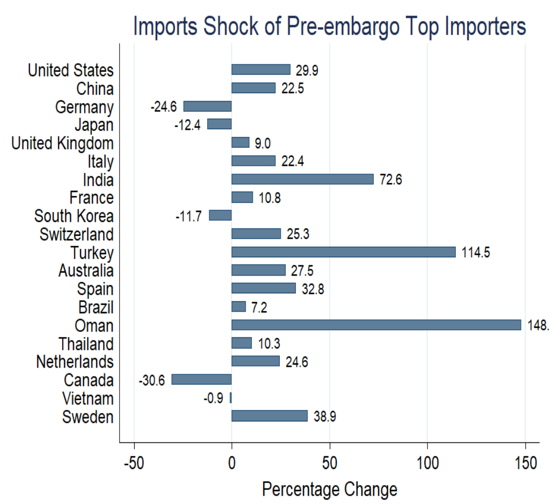
	Summary Statistics				
	Mean	SD	Min.	Max.	Observations
Trade (All)	2.06	6.59	0	45	274
Trade (Air)	0.24	1.17	0	15	274
Trade (Sea)	1.78	6.16	0	45	274
Trade (Land)	1.45	5.54	0	40	274
Imports (Air)	0.24	1.17	0	15	274
Imports (Sea)	0.36	0.99	0	8	274
Imports (Land)	0.04	0.19	0	3	274
Exports (All)	1.42	5.48	0	40	274
Exports (Air)	0.00	0.00	0	0	274
Air Distance	6489.25	3877.62	566	14977	274
Area	0.87	2.23	0	17	274
real GDP	0.07	0.22	0	2	256
Migration Stock	9758.87	59424.28	0	645577	274

Notes: Summary statistics are reported for non-blockading countries sample over the period Jun2016-Jun2018, excluding blockade month June 2017. Trade values in millions of Qatari Riyals, area in million square kilometers, migration stock is number of migrants, GDP at chained PPPs (in tri. 2017US$) and distance values in Kilometers. Minimum trade values are zeros since we are using a balanced panel.

Figure B.4: Top importers distance versus imports shock



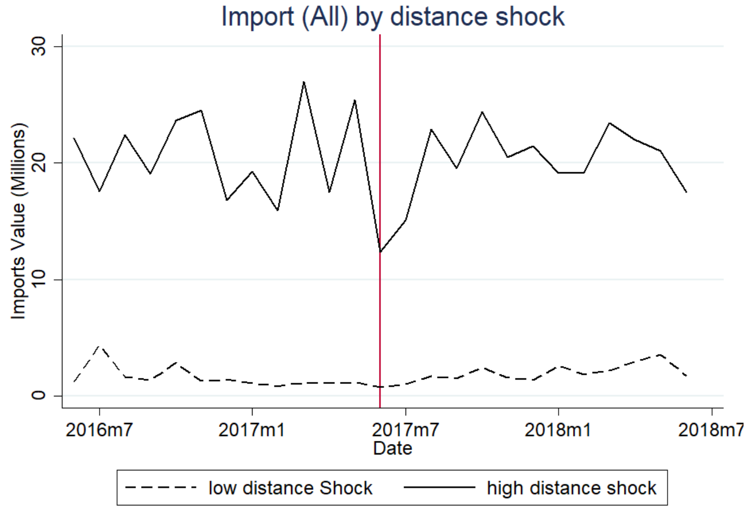
(a)



(b)

Notes: Panel (a) Percentage change of distance for importers (top 10^{th} percentile) based on average imports value of non-blockading countries between pre-embargo period: Jun2016-May2017 and post-embargo period: Jun2017-Jun2018. Panel (b) Percentage change of imports value for importers (top 10^{th} percentile) based on average imports value of non-blockading countries between pre-embargo period: Jun2016-May2017 and post-embargo period: Jun2017-Jun2018.
Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

Figure B.5: Trend of imports by distance shock



Notes: Imports (All) trend split by low and high distance shock based on the lowest and highest 10^{th} percentile over the period Jun2016-Jun2018.
Source: Authors' calculations using Qatar Planning and Statistics Authority Data and geocoded data.

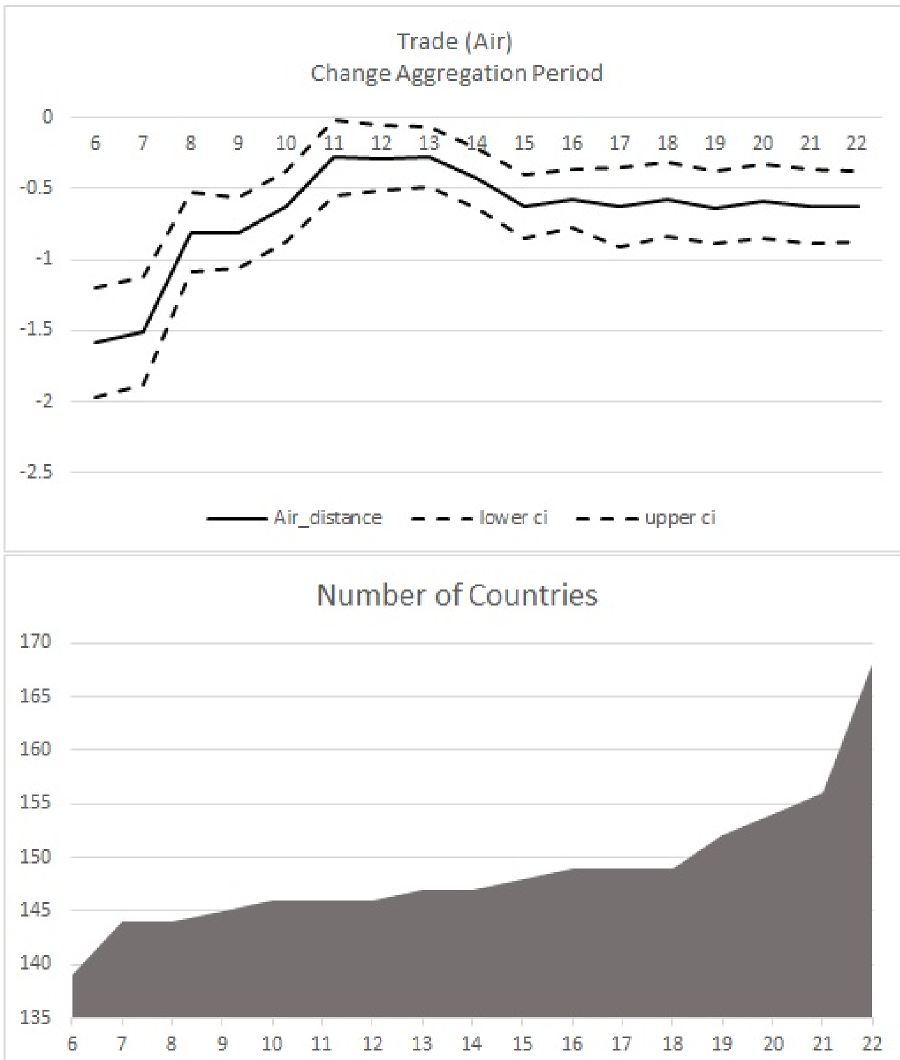
Appendix C Full Set of Results

Table C.1: Main Results

	(1)	(2)	(3)
Panel A.		Trade (Air)	
Air Distance (log)	0.990**	-0.288**	-0.218*
	(0.494)	(0.119)	(0.121)
Post	0.177***	0.341***	0.293***
	(0.041)	(0.028)	(0.028)
Observations	292	292	294
Pseudo R2	0.079	0.997	0.997
Panel B.		Imports (Air)	
Air Distance (log)	0.978**	-0.285**	-0.215*
	(0.495)	(0.120)	(0.123)
Post	0.178***	0.341***	0.293***
	(0.042)	(0.028)	(0.028)
Observations	278	278	280
Pseudo R2	0.078	0.997	0.997
Country FE	No	Yes	Yes
Inc. Jun2017	No	No	Yes

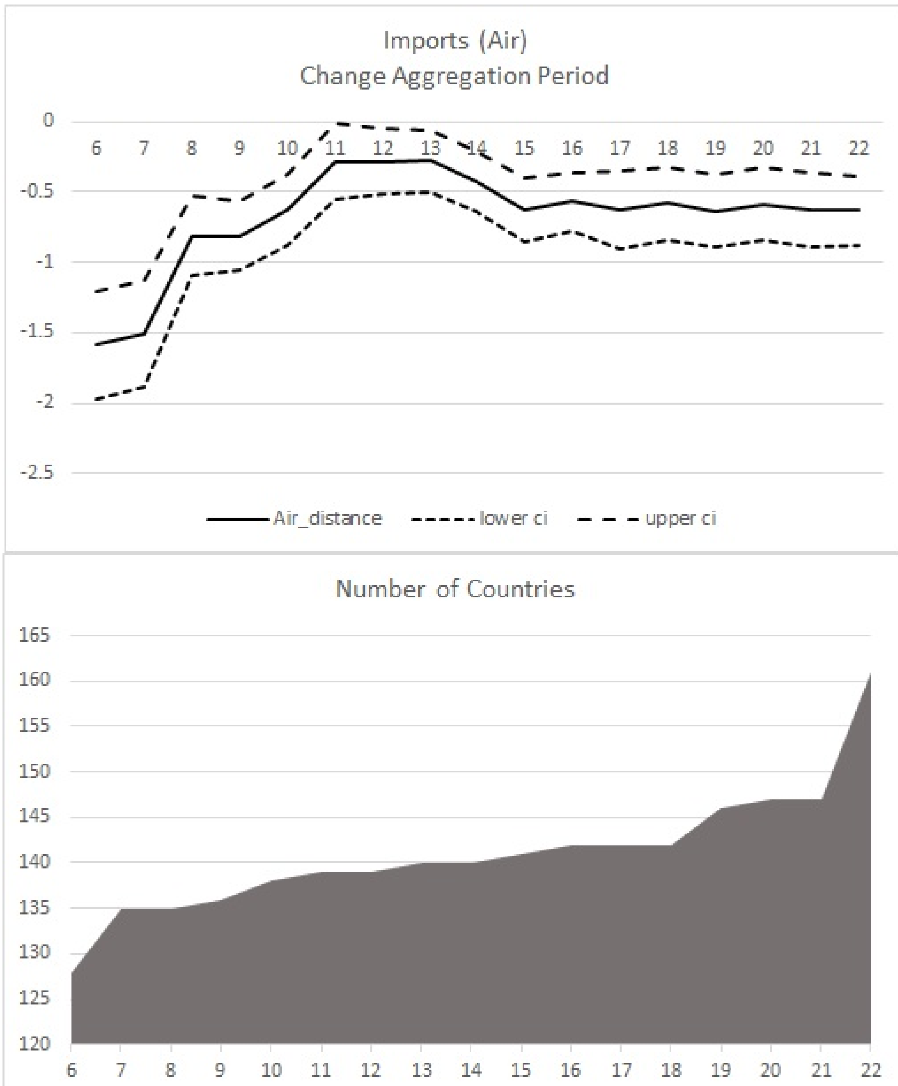
Robust standard errors clustered at country level in parentheses;
***, **, * denote significance at the 1%, 5% and 10% level, respectively.

Figure C.1: Change of aggregation period estimation (trade)



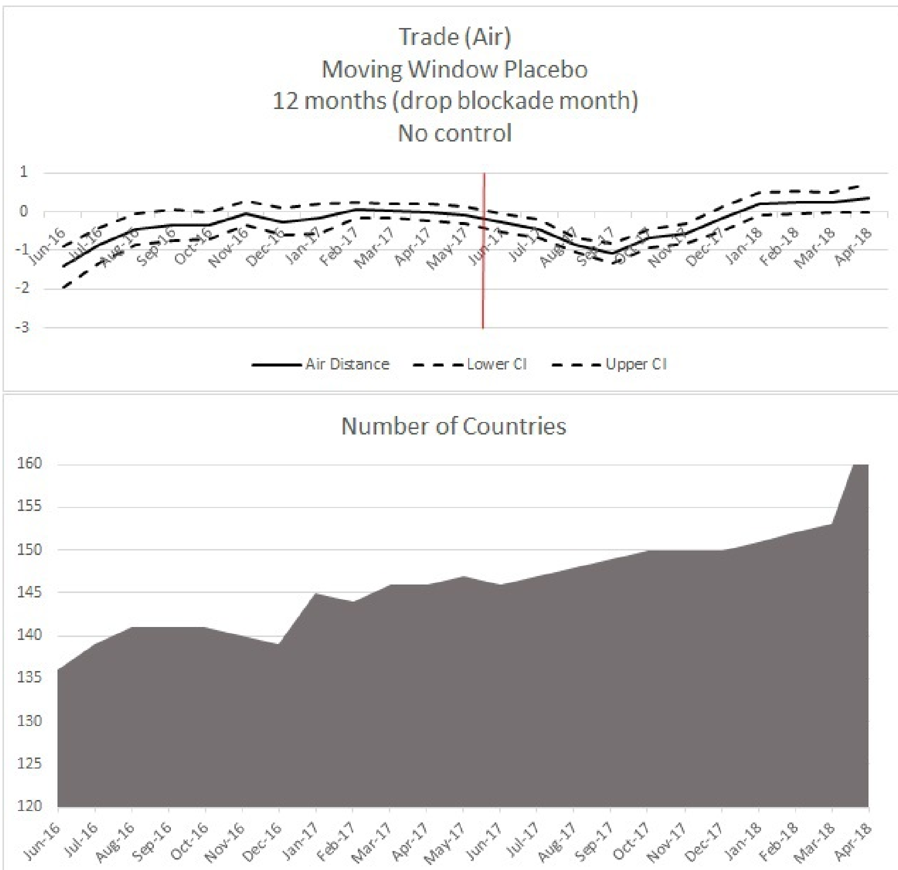
Notes: Confidence intervals defined at the 95^{th} Percentile.
Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

Figure C.2: Change of aggregation period estimation (imports)



Notes: Confidence intervals defined at the 95th Percentile.
Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

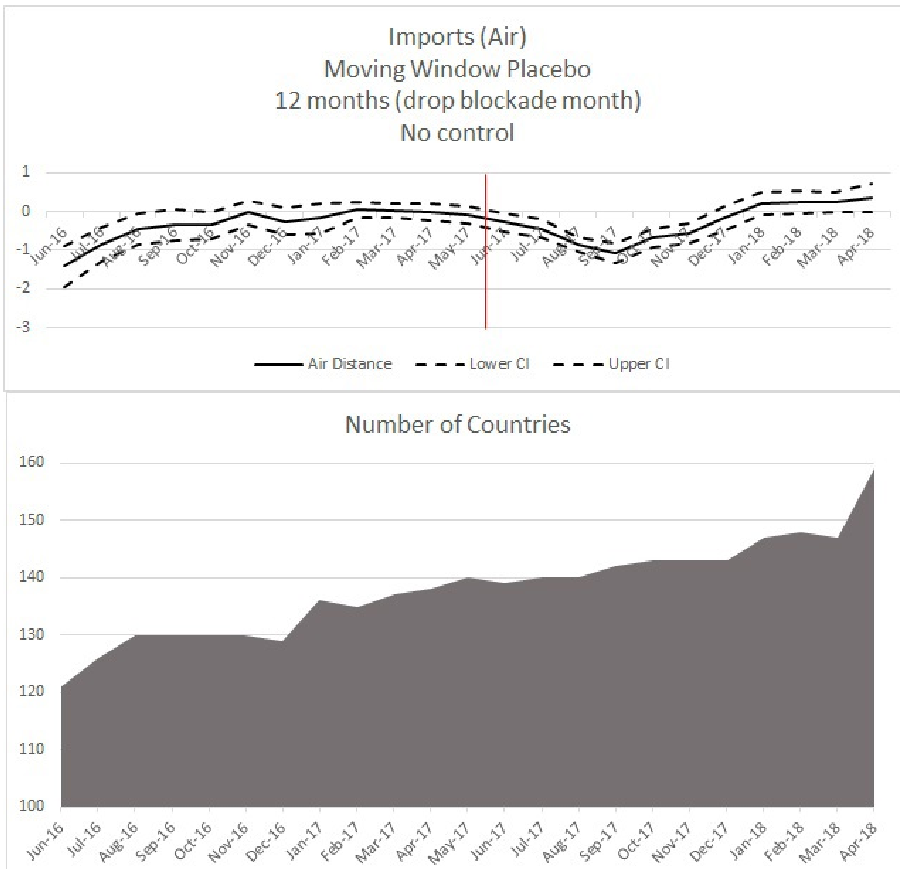
Figure C.3: Placebo estimation (trade)



Notes: Confidence intervals defined at the 95^{th} Percentile. The red vertical line denotes the actual embargo date (Jun2017).

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

Figure C.4: Placebo estimation (imports)



Notes: Confidence intervals defined at the 95th Percentile. The red vertical line denotes the actual embargo date (June2017).
Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

Table C.2: Robustness Table 1

Robustness	(1) Area control	(2) Direct flights	(3) Eastern Coast	(4) Western Coast
Panel A.		Trade (Air)		
Air Distance (log)	-0.493***	-0.350***	-0.281**	-0.287**
	(0.147)	(0.116)	(0.122)	(0.123)
Post	0.784***	0.362***	0.339***	0.341***
	(0.201)	(0.028)	(0.030)	(0.030)
log(Area)*Post	-0.029**			
	(0.013)			
Observations	292	134	288	288
Pseudo R2	0.997	0.997	0.998	0.998
Country FE	Yes	Yes	Yes	Yes
Panel B.		Imports (Air)		
Air Distance (log)	-0.493***	-0.347***	-0.278**	-0.284**
	(0.148)	(0.117)	(0.123)	(0.124)
Post	0.790***	0.362***	0.339***	0.341***
	(0.200)	(0.028)	(0.030)	(0.030)
log(Area)*Post	-0.029**			
	(0.013)			
Observations	278	132	274	274
Pseudo R2	0.997	0.997	0.997	0.997
Country FE	Yes	Yes	Yes	Yes

Robust standard errors clustered at country level in parentheses;

***, **, * denote significance at the 1%, 5% and 10% level, respectively.

Notes: Column (1) augments the main specification with an interaction term between log area and Post. Column (2) replicates the main results only using information on direct flights. Columns (3) and (4) replicates the main results by using full trade/imports value for Eastern and Western Coasts of US and Canada, respectively.

Table C.3: Robustness Table 2

Robustness	(1) Other Mode Share	(2) Non-Air	(3) Hydrocarbon
Panel A.	Trade (Air)		
Air Distance (log)	-0.502**	-0.074	0.321
	(0.211)	(0.525)	(0.800)
Post	0.245***	0.258**	0.224
	(0.060)	(0.114)	(0.173)
Pre-embargo sea share*Post	0.002		
	(0.002)		
Pre-embargo land share*Post	0.001		
	(0.002)		
Observations	292	314	154
Pseudo R2	0.998	0.995	0.992
Country FE	Yes	Yes	Yes
Panel B.	Imports (Air)		
Air Distance (log)	-0.495**	-0.231	-3.696
	(0.216)	(0.651)	(2.267)
Post	0.247***	0.144	1.884***
	(0.061)	(0.146)	(0.669)
Pre-embargo sea share*Post	0.002		
	(0.002)		
Pre-embargo land share*Post	0.001		
	(0.002)		
Observations	278	266	134
Pseudo R2	0.997	0.987	0.944
Country FE	Yes	Yes	Yes

Robust standard errors clustered at country level in parentheses;

***, **, * denote significance at the 1%, 5% and 10% level, respectively.

Notes: Column (1) augments the main specification with an interaction term between pre-embargo land and sea share of trade/imports with Post. Column (2) replicates the main results using non-air trade/imports as a dependent variable. Column (3) replicates the main results using hydrocarbon sector as a dependent variable.

Table C.4: Robustness Table 3

Robustness	(1) GDP	(2) pre-GDP	(3) pre-trade	(4) pre-top partners	(5) pre-migration
Panel A.			Trade (Air)		
Air Distance (log)	-0.277**	-0.339**	-0.388*	-0.316***	-0.275**
	(0.116)	(0.154)	(0.231)	(0.108)	(0.115)
Post	0.262***	0.572*	0.387***	0.532***	0.324***
	(0.101)	(0.315)	(0.098)	(0.072)	(0.026)
Real GDP (log)	2.242				
	(3.484)				
Pre-lGDP*Post		-0.017			
		(0.022)			
Pre-trade*Post			-0.000		
			(0.000)		
Pre top importers*post				-0.207***	
				(0.073)	
Pre-embargo migrants*Post					0.001***
					(0.000)
Observations	274	274	292	292	292
Pseudo R2	0.997	0.997	0.997	0.998	0.998
Country FE	Yes	Yes	Yes	Yes	Yes
Panel B.			Imports (Air)		
Air Distance (log)	-0.273**	-0.338**	-0.385*	-0.313***	-0.272**
	(0.117)	(0.156)	(0.233)	(0.110)	(0.117)
Post	0.264***	0.580*	0.387***	0.533***	0.324***
	(0.101)	(0.316)	(0.099)	(0.072)	(0.026)
Real GDP (log)	2.203				
	(3.494)				
Pre-lGDP*Post		-0.018			
		(0.022)			
Pre-trade*Post			-0.000		
			(0.000)		
Pre top importers*Post				-0.207***	
				(0.073)	
Pre-embargo migrants*Post					0.001***
					(0.000)
Observations	260	260	278	278	278
Pseudo R2	0.997	0.997	0.997	0.997	0.998
Country FE	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered at country level in parentheses;

***, **, * denote significance at the 1%, 5% and 10% level, respectively.

Notes: Column (1) augments the main specification with real GDP (log). Column (2) augments the main specification with an interaction term between pre-embargo real GDP (log) with Post. Column (3) augments the main specification with an interaction term between pre-embargo trade/imports with Post. Column (4) augments the main specification with an interaction term between pre-embargo top importers with Post. Column (5) augments the main specification with an interaction term between pre-embargo migration stock with Post.

Table C.5: Robustness Table 4

	(1)	(2)	(3)	(4)
Robustness	Food sector	Strategic Partners	Cut ties	Cut/downgrade ties
Panel A.		Trade (Air)		
Air Distance (log)	-0.373***	-0.325***	-0.288**	-0.302***
	(0.100)	(0.105)	(0.119)	(0.115)
Post	0.337***	0.337***	0.341***	0.341***
	(0.030)	(0.028)	(0.028)	(0.028)
Observations	288	284	280	274
Pseudo R2	0.998	0.998	0.997	0.997
Country FE	Yes	Yes	Yes	Yes
Panel B.		Imports (Air)		
Air Distance (log)	-0.370***	-0.321***	-0.285**	-0.298**
	(0.102)	(0.107)	(0.120)	(0.117)
Post	0.337***	0.336***	0.341***	0.341***
	(0.030)	(0.028)	(0.028)	(0.028)
Observations	274	270	270	266
Pseudo R2	0.998	0.998	0.997	0.997
Country FE	Yes	Yes	Yes	Yes

Robust standard errors clustered at country level in parentheses;

***, **, * denote significance at the 1%, 5% and 10% level, respectively.

Notes: Column (1) replicates the main results after dropping the food sector. Column (2) replicates the main results after dropping strategic partners post-embargo (Turkey, Iran, Oman and Kuwait). Column (3) replicates the main results after dropping countries that cut ties (Yemen, Libya, Maldives, Mauritania, Comoros and Senegal). Column (4) replicates the main results after dropping countries that cut or downgrade ties (Yemen, Libya, Maldives, Mauritania, Comoros, Senegal, Chad, Djibouti, Jordan and Niger).

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