A Causal Inquiry into the Economics of Migration and Trade

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وَ تَحْسَبُ أَنَّكَ جِرْمٌ صَغِيرْ   وَ فِيكَ ٱنْطَوَىٰ ٱلْعَالَمُ ٱلْأَكْبَرْ [1]

[1] "Do you suppose that you are only a small body, while the macrocosm is placed within you?"

Declaration

I declare that the work presented in this thesis is, to the best of my knowledge and belief, original and my own work. The material has not been submitted, either in whole or in part, for a degree at this, or any other university. This thesis does not exceed the maximum permitted word length of 80,000 words including appendices and footnotes, but excluding the bibliography. A rough estimate of the word count is: 28007

Afnan Al-Malk

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To put gratitude in words is a burden; it is one of those instances where words fail me. Therefore, below is just my very humble attempt.

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Introduction

Causality is central to economics (Hoover, 2006), for it is the means to predict effects of new interventions and calculate policy counterfactuals (Heckman, 2008). However, in a complex world where "everything depends on everything else" (Valavanis, 1959, as quoted in Hoover, 2004), how does one go about identifying cause and effect? In some applied sciences, Randomized Control Trials provide the gold standard for this purpose (Noble Prize Committee, 2021). The use of experiments offers a satisfactory way since it gives evidence that is both controlled and reproducible (Gower, 2012). However, many questions cannot be addressed by an experiment, either due to financial, ethical or practical constraints (Noble Prize Committee, 2021). Many philosophers viewed the use of statistical methods to be a substitute for experiments, because it allows the extraction of a repeated pattern from a large collection of data (Morgan, 1990). More specifically, it gives scientists a way to deal with plurality of causes in a non-experimental context (Morgan, 1990). Since the adoption of statistical techniques in economic modeling, it was evident that randomization is not applicable in a similar manner as is the case of controlled experiments, so regression analysis did not essentially provide a causal interpretation (Wold, 1954). To deal with this, a recent breakthrough has been the adoption of the design-based approach that is "aimed at emulating a randomized experiment to answer a causal question using observational data" (Noble Prize Committee, 2021). With the exception of the first chapter, this thesis applies this methodological tool, in particular; exploits a quasi-experimental variation, to answer causal questions on migration and trade.

The first chapter examines the effect of religiosity on employment among migrants. The economics literature has recently been more active in examining the effect of religion on different economic outcomes. Religion is found to be an important determinant of individuals' preferences. In line with some recent work, we deal with the inherent complexity in disentangling religiosity from culture by adopting the epidemiological approach that focuses on migrants. Although an IV strategy, a design-based approach, has been used in the literature to identify the causal effect of religiosity, we decide against its use due to the limitations of IV in this context. We argue that the epidemiological approach along with a rich set of fixed effects that the survey provides allow us to come as close as possible to identifying a causal effect of religiosity on employment. We use one wave of the European Values Study that gives us a sample of 46 European countries in 2008. Our OLS estimates show a negative effect of religiosity on employment. Robustness checks are carried out that confirm the validity of our estimates. We also look into possible mechanisms that could drive this relationship. However, we do not find evidence that any of the potential variables available in our survey can be the channel through which religiosity drives employment.

The second chapter, co-authored with Jean-François Maystadt and Maurizio Zanardi, moves into international trade and exploits a natural experiment to better understand the effect of transportation costs on trade. The negative effect of distance on trade is well-established in the literature. However, the debate continues on whether the observed effect is exclusively due to transportation costs or other omitted variables. We take advantage of the blockade that was imposed on the State of Qatar in 2017 to rule out the endogeneity problem and examine how the resulting rise in air transportation costs affected trade. We employ a gravity model estimated using a Poisson pseudo-maximum likelihood estimator, and find an air transportation cost elasticity of trade between -0.3 and -0.5. We provide robustness checks to confirm that our results are not driven by potential contaminating factors.

The third chapter, co-authored with Jean-François Maystadt and Maria Navarro Paniagua, attempts to get a better understanding of how an exogenous shock affects remittances received by households in Nepal. The blockade on Qatar in 2017 had its share of negative impact on vulnerable migrants. We exploit three waves of a panel dataset on Nepali households between 2016 and 2018. Following the shock, households who had migrants in Qatar experienced a substantial decline in remittances compared to households whose migrants were in different international destinations. We adopt a difference-in-difference approach and control for pre-embargo characteristics to rule out any confounding factors that could bias our estimates. We also show that the decline in remittances is mainly found amongst the poorest households. This result sheds light on the compounded problem of poverty since it is the poor who seem to suffer most and have least resilience in the face of shocks.

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Chapter 1

Religiosity and Labor Market Participation: Evidence from the European Values Study

1.1 Introduction

The three-century-old secularization hypothesis on the demise of religion (Stark, 1999), and its emphasis that "in due course, the sacred shall disappear altogether except, possibly, in the private realm" (Mills, 2000) remains unconfirmed by the reality of today's world. On a global level, more than eight-in-ten people identify with a religious group (Pew Research Center, 2012). Moreover, not only did religion stand the test of time in the private sphere, but its influential role in the public domain grew significantly in recent decades (Casanova, 2011). This motivated economists to extend the application of economic tools and models to the study of religion (Iyer, 2016). Starting with the seminal work of Iannaccone (1998), different lines of research on the economics of religion have evolved. A major one is concerned with the impact of religion on various economic outcomes (Iyer, 2016). One of the earliest contributions in this realm is the thesis on the role of Protestantism in promoting work ethic and commitment by Weber (1930). Empirical examination of Weber's theory remains inconclusive (Iannaccone, 1998), and an active area of research (Feldmann, 2007; Becker and Woessmann, 2009; Spenkuch, 2017). Some scholars suggest that the notion of work as a calling is universal amongst major world religions (see Praveen Parboteeah et al. (2009) for an overview). In view of the role that commitment to religion might have on people's work outcome, this paper will empirically examine the relationship between individual-level religiosity and employment.

The European labor market represents an ideal context to address this question. Europe witnessed a rise in linguistic, ethnic and cultural diversity in recent decades given the increased flow of migrants (Novotnỳ, 2017). As a result, it provides an appropriate context to study different religious denominations. Moreover, the European labor market is characterized by rigid institutional features (Bertola, 1999) and governed by a set of minimum standards that member states must comply with (European Commission, n.d.a). Yet, significant cross-country differences characterize employment trends (Ward-Warmedinger and Macchiarelli, 2013). Therefore, some studies attempted to explain employment disparities across European countries using cultural factors (Alesina et al., 2015; Algan and Cahuc, 2005; Alesina et al., 2005; Giavazzi et al., 2013; Moriconi and Peri, 2019). Although religion is implicitly accounted for as a subset of culture in some of these studies, this paper focuses on religiosity and distinguishes it from culture.

Religion does not exist in a vacuum; it is engraved in culture that is deeply intertwined with economic and political influences (Moore, 2015). To undertake this empirical investigation of the effect of religiosity on employment, we must separate religiosity from culture. To achieve that we implement the epidemiological approach by Fernández (2011) that exploits migrants who have different cultures but share the economic and institutional set-up in the country of residence. This approach allows separating the effect of country-of-origin culture from country-of-residence economic and institutional influences. We follow Guner and Uysal (2014) and extend the epidemiological approach by including a separate measure for culture of origin and individuals' religiosity to explicitly examine the effect of both. Moreover, we control for a rich set of fixed effects and explore different channels to ensure the observed effect is that of religiosity. For the purpose of our analysis, the European Values Study is the best survey to answer this research question. It contains a wide array of questions on religious belief and practice, in addition to data on migration status and country of origin along with various demographic and personal information. Our main results confirm a negative effect of religiosity on employment.

The remainder of this paper is organized as follows: Section 1.2 positions our contribution with regards to the existing literature. Section 1.3 discusses the identification strategy and presents our model specification. In Section 1.4, the data sources are described along with summary statistics of the main variables. Section 1.5 presents the main results, robustness checks and analysis of channels driving the observed effect. Finally, Section 1.6 concludes.

1.2 Literature Review

Major economic work that addresses the impact of religious affiliation or religiosity on labor market outcomes is scarce. Some exceptions provide empirical evidence on earnings (Steen, 2004; Lipford and Tollison, 2003; Meredith, 2013), productivity (Kaasa et al., 2016), female labor force participation (H'madoun, 2010; Pastore and Tenaglia, 2013), entrepreneurship (Audretsch et al., 2007; Nunziata and Rocco, 2011) and employment (Feldmann, 2007; Spenkuch, 2017). However, the last two cited papers on employment are limited to the impact of Protestantism in an attempt to investigate Weber's hypothesis. The study by Feldmann (2007) differs from the present study as his main interest is on the role of Protestantism in shaping the national culture and behavior. Specifically, he examines how Protestantism affected labor market outcomes in countries where it is the majority religion. Spenkuch (2017) uses micro-level data and focuses on the behavior of individuals who adhere to Protestantism and compares them to Catholics. The novelty in his approach is the use of an instrumental variable strategy by exploiting a historical event in Germany to address the endogeneity of religiosity. His results, however, pose an external validity issue since they are limited to the German case. A related work from management studies by Praveen Parboteeah et al. (2009) provides an empirical investigation on the relationship between different aspects of religion amongst major world religions and work obligation norm. They use data from the World Values Survey, which has the same questionnaire design as the European Values Study (Haerpfer, n.d.) that we use in our analysis. Their results confirm a positive effect of some measured dimensions of religion on work obligation norm. However, they do not use an explicit measure of work. Their outcome variable is individuals' responses to questions about their perceptions towards work norm, which may suffer from social desirability bias (Klein et al., 2017) and not reflect participants' real behavior. Consequently, a measure of individuals' actual employment outcome is deemed more appropriate. Moreover, they examine the role of religion as a social institution by pooling the average of individuals' responses across countries or using country-level indicators of religion. We argue that the observed effect could reflect other rooted aspects of culture in social institutions as opposed to religion. To that end, this paper will employ a direct measure of individual-level religiosity and examine its effect on their actual employment outcome.

A major challenge in the literature linking religiosity with economic outcomes is the identification of a causal effect since religiosity is endogenous (Iyer, 2016). Amongst the different methodological procedures that have been implemented to address this problem, an instrumental variable strategy (IV) has been used by many studies (Iyer, 2016). For instance, Barro and McCleary (2003) use different instruments including the presence of state religion to study the causal effect of religiosity on economic growth. Gruber (2005) uses religious market density as an instrument for religiosity to examine its causal effect on different economic outcomes. Becker and Woessmann (2008) use distance to Wittenberg to instrument for Protestantism and establish its effect on females' educational gap. Fruehwirth et al. (2019) employ peers' religiosity to examine the impact of individuals' religiosity on depression. However, one must be careful with generalizing results from IV estimates since they provide a local average treatment effect. Therefore, they reflect a sub-population (Divenyi, 2015). Moreover, in an IV context, religiosity captures individuals whose religiosity is affected by instruments, which raises a question about the type of individuals being analyzed. Therefore, this paper will instead adopt the epidemiological approach and exploit a rich set of fixed effects to isolate religiosity from culture and other factors.

There is an extensively growing literature on the impact of culture on labor market outcomes (Algan and Cahuc, 2005; Alesina et al., 2005; Alesina et al., 2015; Giavazzi et al., 2009; Giavazzi et al., 2013; Brügger et al., 2009). To separate the effect of culture from economic and institutional factors, an epidemiological approach was recently developed by Fernández (2008). This line of research assumes that religious belief is one facet of culture (Guiso et al., 2006). Two studies that ask a very similar question to our paper and use the epidemiological approach are Mocan and Pogorelova (2015) and Moriconi and Peri (2019). They examine the effect of culture of leisure of migrants living in Europe on employment outcomes. Mocan and Pogorelova (2015) limit their sample to second-generation migrants because they are less likely to suffer from selection bias compared to first-generation migrants. However, the authors point out that second-generation migrants are less likely to reflect the full extent of country-of-origin culture due to cultural assimilation into the host country. Moriconi and Peri (2019), on the other hand, account for both first- and second-generation migrants. They construct a proxy for the value of work preference by combining individuals' responses in each country of origin. This is done to avoid the issues of migrants' selection and reverse causality between culture and economic outcomes. In the present paper, we employ an extension of the epidemiological approach that includes a separate indicator for country-of-origin culture and individuals' religiosity. This is in line with the work of Guner and Uysal (2014) who include a separate proxy for religiosity and culture in their examination of female labor force participation. Our paper therefore contributes to a better understanding of the complex interrelationship between religion and culture since we are also able to better distinguish religiosity from other dimensions of culture.

Research from other disciplines provides insights on the channels through which religiosity may affect employment. Some of these channels highlight a positive relationship. For instance, religious values emphasize hard work and reproach laziness, which has significant implications on economic activity. Specifically, a brief overview by Praveen Parboteeah et al. (2009) illustrates how the teachings of major world religions view work as an individual's obligation. Therefore, religious individuals may have a stronger incentive to secure a job. In addition, studies from different fields, including economics, stress the significant role of religion in fostering social capital (Asquith et al., 2017; Maselko et al., 2011; Kaasa, 2013). A considerable amount of literature has been published on the importance of social capital in supporting job finding (Brook, 2005) and improving labor market outcomes (Asquith et al., 2017). On the other hand, religious values may also prolong individuals' unemployment. One way is by limiting their employment choices. For instance, members from some religious groups might have reservations about some activities in a potential job that they deem inappropriate in light of their respective religious doctrine. Indeed, some studies suggest that religion influences the career choices of some individuals (Hernandez et al., 2011). Higher religiosity may also reduce the likelihood of individuals actively seeking a job compared to the non-religious during periods of unemployment (Clark and Lelkes, 2006). This is because religion insures against adverse life events, making its religious members less negatively affected and more likely to cope compared to the non-religious (Clark and Lelkes, 2006). Moreover, stronger family ties is associated with higher levels of religiosity (Heaton and Goodman, 1985) and lower rates of employment (Algan and Cahuc, 2005).

For some channels, it is not possible a priori to determine the role of religiosity. For instance, a sense of mastery[1] is found to be an important attribute that allows individuals to cope with economic hardships such as job loss (Conger and Conger, 2002). Schieman (2008) proposes two possible hypotheses on the effect of religiosity on the sense of mastery. The relinquished control hypothesis suggests that the belief that one's life is dictated by God's decree could reduce their sense of control. On the other hand, the personal empowerment hypothesis suggests that religiosity would enhance one's sense of mastery through reduced uncertainty and assurance in Divine decree. Which effect holds is likely to vary based on the religiosity measure used (Schieman, 2008) or across different religious denominations. However, the impact of religiosity on individuals' sense of mastery will determine how much efforts they will invest to find a job in case of unemployment.

The aforementioned channels are all concerned with the effect of religiosity on the search process of individuals seeking a job. However, religiosity could affect the demand for labor by functioning as a signal. A relevant model in this context is the distaste for minority framework established by Becker (1957), which is used by Drydakis (2010) to explain discrimination against religious minorities. In short, the model proposes that employers' attitudes towards religion may deter them from accepting job applicants from certain religious backgrounds, irrespective of applicants' productivity and potential. Discrimination against religious minorities is expected to explain the negative effect of religiosity on employment in the European context given the recent cases on the freedom of religious expression in the workplace (Hennette-Vauchez, 2017) and the spread of discrimination in the European labor market (Lamberts et al., 2014).

[1] A sense of mastery refers to individuals' perceived control over their life outcomes (Schieman, 2008).

A report by the European Network Against Racism suggests that religious and ethnic minorities in Europe have a disadvantage in the recruitment process (ENAR, 2017). There is, however, a disparity in the extent of individuals' rights to religious expression in the workplace amongst different European states. This is due to national differences in the scope and implementation of the EU directive 2000/78/EC which provides a general framework against discrimination on the basis of religion amongst other factors (European Commission, n.d.b). Therefore, the extent to which religiosity correlates with discrimination is likely to vary amongst different European countries. Despite the expected impact of religiosity on discrimination, empirical studies that examine labor market discrimination on the ground of religion are scarce (Drydakis, 2010). Some exceptions that tackle labor market discrimination in Europe focus on some religious minorities in a specific country (Drydakis (2010) in Greece; Lindley (2002) and Khattab and Hussein (2018) in the UK; Valfort (2018) in France).

1.3 Methodology

Identification Strategy

Religiosity is multi-dimensional and there is no consensus on which of its aspects should be measured (McAndrew and Voas, 2011). The choice of variables to include in empirical studies is mostly dictated by the available survey questions, which usually results in a discrepancy between the conceptualization of religion and operationalization of the variables in empirical analysis (Basedau et al., 2018). A major concern is how to separate religion from culture. Both concepts are complex and have been defined in different ways. Yet, each has been recently used by economists to examine their effect on economic outcomes. Tylor (1871) defines culture as "that complex whole which includes knowledge, beliefs, arts, morals, law, customs, and any other capabilities and habits acquired by [a human] as a member of society". For the sake of this study, we confine this understanding of culture to the boundary of country of origin. Additionally, although different theories have been postulated with regards to the relationship between the two, we side with the view that religion is a subset of culture (Abd-Allah, 2006; Geertz, 1973; Rees, 2017; Beyers, 2017).[2] Nonetheless, disentangling religion[3] from culture is challenging because of the strong interrelationship between the two. Inglehart and Baker (2000) empirically examine 65 countries covering more than 75% of the world's population and find that religion has become incorporated in their national culture. Consequently, although religion may not have a direct explicit effect in some contexts, it continues to exert influence indirectly through cultural norms and attitudes.

We adopt the epidemiological approach by Fernández (2011) to disentangle the effect of culture from economic and institutional factors. This approach allows separating cultural effects by examining individuals who differ in their culture but share the same economic and institutional environment. In most cases, this is done by examining migrants from different countries of origin who live in the same country of residence (Fernández, 2011). Although migrants are different and some become completely cut off from their home country (Soehl and Waldinger, 2010), the recent advances in travel modes and communication technology increased the forms and intensity of migrants' connections to their home countries (Vertovec, 2001). Scholars suggest that these ties hold for migrants and their descendants (Levitt and Schiller, 2004). Soehl and Waldinger (2012) point out that although aggregate conclusions are risky as they disregard the variation among individuals, the typical migrant maintains some connection with their home country. However, given that our main variable of interest is religiosity, a single residence country context will not be appropriate because, as opposed to culture which is static, religious expression is highly dependent on social and historical contexts (Beyers, 2017). As a result, to disentangle the effect of religion from culture, we take into account different European countries. We argue that

[2] This is not to say that religion is a byproduct of culture, although such view may be true for some religions. What we mean here is that religious practice does have a commonality amongst adherents across different geographical locations but the manifestation of some religious practices may be influenced by local custom.

[3] We leave out giving an explicit definition of religion given the contentious disagreement among scholars (Woodhead, 2011). Although the choice of definition is very important to ensure consistency across research (Hill et al., 2000), this should not affect our research since we are compelled to use the denominations provided by the dataset we use. Compared to defining culture, the definition of religion introduces a unique challenge. One major issue is that scholars are mainly split between those who define religion as a purely human phenomenon (a naturalistic interpretation) and those who define it from the perspective of their own religious belief (a religious interpretation) (Hick, 2004). The chosen side affects the perspective of analysis. However, this discussion is beyond the scope of the paper. Interested readers are referred to: Eliade et al. (1987), Asad (1993), Hill et al. (2000) and Hick (2004).

including explicit measures of culture of origin and religiosity in a multi-residence context will isolate the religious aspect of culture from all others. We use country-of-origin fixed effects as a proxy for culture and assume that it captures factors that affect individuals' unobservable human capital and productivity (Moriconi and Peri, 2019). The literature implementing the epidemiological approach uses different measures to proxy a particular aspect of culture deemed most relevant to the economic outcome of interest. Nonetheless, Mocan and Pogorelova (2015) confirm that using country-of-origin fixed effects leads to very similar results in comparison to the use of specific proxies for different aspects of culture.

Most of the studies on the effect of culture on economic outcomes do not examine the separate effect of religion. One exception is Guner and Uysal (2014) who include separate measures for religiosity and culture. We follow a similar approach and control for religion by including an individual-level religiosity index that accounts for different aspects of religious belief and commitment. We argue that by ruling out cultural factors via country-of-origin fixed effects, our index is more likely to capture the religiosity of individuals as opposed to inherited cultural attitudes. Moreover, we control for religious denominations in our specification to capture aspects that are unique to each religion and may drive the effect of religiosity on employment differently. Furthermore, we control for country-of-residence fixed effects to capture institutions, policies and other factors in country of residence that might affect employment (Moriconi and Peri, 2019).

In our context, it may be the case that some of the taken-for-granted norms in the labor market could be traced back to religion. Thus, people's attitudes may be shaped by religion in spite of them being irreligious (Halman and Luijkx, 2006; Kaasa, 2013). Consequently, failure to separate individuals' religiosity per se from the religious norms inherent in the labor market institutional set-up will overestimate the effect of religiosity. We argue that by working on European countries that have a similar institutional set-up and controlling for country-of-residence fixed effects, we are better able to separate religiosity of individuals from religion's aspects that are inherent in the labor market itself.

Econometric Model

To identify the effect of religiosity on employment, we estimate the following equation:

$$EMP_{iojr} = \beta Relig_{iojr} + \Gamma \boldsymbol{X_{iojr}} + \mu_o + \phi_j + \gamma_r + \epsilon_{iojr} \tag{1.1}$$

The index i denotes individual, o denotes country of origin, j denotes country of residence and r denotes religious denomination. EMP_{iojr} is equal to 1 if the individual is employed and zero otherwise. $Relig_{iojr}$ is the religiosity index, defined in Section 1.4. $\boldsymbol{X_{iojr}}$ is a vector of exogenous controls on observable individual characteristics including gender, migration status, age and its squared form. The literature on employment tends to include more controls such as education, marital status and number of children among others (Moriconi and Peri, 2019; Mocan and Pogorelova, 2015; H'madoun, 2010). However, our model specification is based on a parsimonious set of individual-level covariates. This is done to avoid the bad control problem that results from the inclusion of controls likely to be outcomes of the main regressor (Angrist and Pischke, 2008). The other controls used in the literature are all likely to be determined by religiosity, thus we rule them out from our regression equation. μ_o, ϕ_j and γ_r are country-of-origin, country-of-residence and religious-denomination fixed effects, respectively.

The dependent variable employment is a binary variable. We estimate Equation 1.1 using a linear probability model (LPM). In such case, the coefficient of interest β represents the change in probability of employment when religiosity increases by one unit. A positive coefficient $\beta > 0$ indicates that a rise in religiosity raises the probability of being employed on average, holding other variables constant. Although some criticisms are posed against the use of a LPM in favor of probit or logit models, Bellemare (2013) suggests that a LPM is favorable when you do not have random assignment and your identification is less than clean. Moreover, the heteroskedasticity problem resulting from the use of a LPM can be easily addressed using robust standard errors (Bellemare, 2013). Another issue with the errors in our specification is that the independence assumption is unlikely to hold. This is because we expect migrants to have correlation in some unobservable components that drive the outcome variable (Abadie et al., 2017). Failure to account for the clustering of errors will result in understated standard errors and overestimated statistical significance (Cameron et al., 2010). This issue can be addressed by clustering the errors at a level such that observations are assumed to be dependent within clusters but independent across clusters. We decide to cluster at the country-of-origin level. Evidence suggests that labor market outcomes of migrants in Europe vary across country of origin and country of residence (Ho and Turk-Ariss, 2018). Fernandez and Fogli (2009) suggest that clustering should be done at the level where the main dependent variable is expected to have the highest degree of variation. Since we assumed that religion is a subset of culture, we cluster at the country of origin level. Our sample of 135 countries of origin exceeds the recommended minimum number of clusters for reliable inference; which is between 47 (Angrist and Pischke, 2008) and 50 (Kezdi, 2005).[4]

1.4 Data & Summary Statistics

The primary data source for this study is the European Values Study (EVS). It is a repeated cross-section and multi-national survey which has been conducted in four waves (repeated every 9 years) between 1990-2008. It covers a varying number of European countries in every wave. The dataset provides information on basic human values and covers various aspects of European citizens' opinions on matters related to life, family, work, religion, politics and society. We use the fourth wave, administered in 2008, that includes data on respondents' and their parents' country of origin. This information is crucial to employ our identification strategy because it allows us to identify first- and second-generations migrants. First-generation migrants are defined as those who are born outside the country of residence, whereas second-generation migrants are individuals born in the country of residence but either one or both of their parents are born elsewhere. In the case of parents coming from different countries of origin, we assign second-generation migrants in the sample with mothers' country of origin. Although the convention in the literature is to use fathers' country of origin

[4]The most common way used in the literature to address both heteroskedasticity and clustering of errors is the cluster-robust variance estimator (CRVE) (Cameron and Miller, 2015). However, our clusters are very unbalanced, with cluster's size ranging from 1 to 391. MacKinnon and Webb (2017) present Monte Carlo evidence that CRVE on unbalanced clusters is quite unreliable. On the other hand, they show that using wild cluster bootstrap performs well in case of unbalanced clusters. For wild bootstrap, inference is based on p-value and confidence interval. The reason is that the computation of standard errors relies heavily on asymptotic normality of β, in application of unbalanced clustering this large sample theory may not apply (Roodman et al., 2019). Therefore, we report the wild cluster bootstrap p-value estimate in Table A.5 of Appendix A.2 that confirm our main results.

(Mocan and Pogorelova, 2015; Moriconi and Peri, 2019), we follow the studies that suggest a stronger influence of mothers in the transmission of culture (Rodríguez-Planas and Sanz-de Galdeano, 2016).[5]

We include first- and second-generation migrants to exploit the advantage of both types. Whereas the selection bias is less pronounced in second-generation migrants, they are more likely to have assimilated into the country of residence (Mocan and Pogorelova, 2015). Therefore, we also include first-generation migrants who are more likely to reflect the effect of culture on economic outcomes compared to second-generation migrants (Fernandez and Fogli, 2009). Our sample covers 46 European countries.[6] To implement our identification strategy we exclude natives and migrants with missing information on country of origin. Additionally, we limit the sample to individuals between 16 and 38 years old. The minimum excludes non-working age population. While the maximum ensures we are exploiting the age window where religiosity is likely to matter more. The age effect on attitude and belief formation in general is well-established in the psychology literature, in what is referred to as the impressionable year hypothesis (Dawson and Prewitt, 1968; Czudnowski, 1977; Krosnick and Alwin, 1989). The hypothesis suggests that beliefs and attitudes are shaped in late adolescent and early adult years, after which it is more likely to stabilize. The original sample includes approximately 66,000 individuals with roughly 5200 and 7000 first-and second-generation migrants, respectively. Once we make the aforementioned changes, the final sample includes a total of 3,097 individuals of which 1,933 are first-generation migrants and 1,164 are second-generation migrants.

Table 1.1 shows the summary statistics of the dependent variable and individual control variables used in the empirical analysis, separated for first- and second-generation migrants. Our outcome variable is employment, which is a dichotomous variable that takes the value one if the individual is employed and zero otherwise. It measures labor supply at the extensive margin. Although an intensive margin measure

[5]As a robustness check, we run the same analysis based on fathers' country of origin in Table A.6 in Appendix A.2.

[6]Countries included in wave 4 (2008) are: Albania, Armenia, Austria, Belarus, Belgium, Bosnia and Herzegovina, Bulgaria, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Georgia, Germany, Great Britain, Greece, Hungary, Iceland, Ireland, Italy, Kosovo, Latvia, Lithuania, Luxembourg, Macedonia, Malta, Moldova, Montenegro, Netherlands, Northern Cyprus, Northern Ireland, Norway, Poland, Portugal, Romania, Russian Federation, Serbia, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, Turkey and Ukraine.

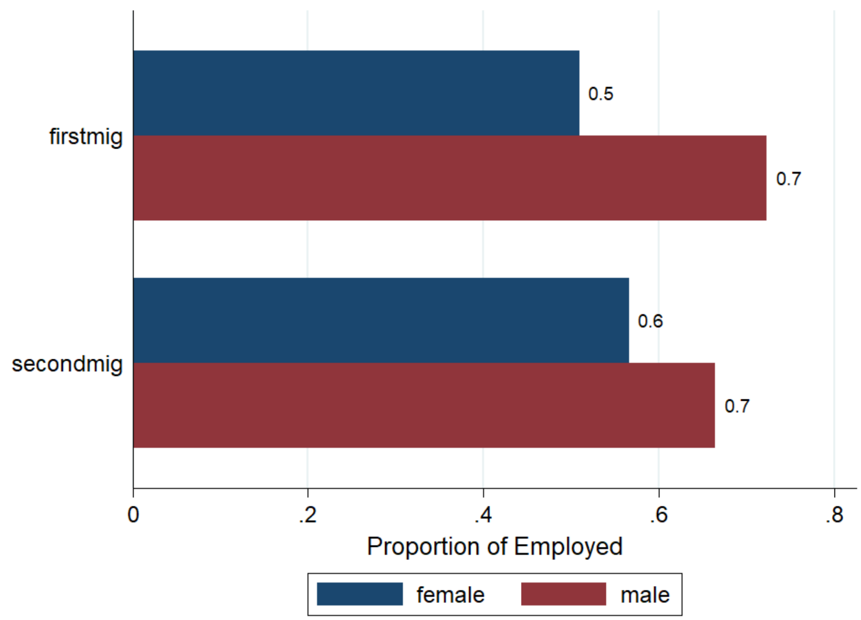
Table 1.1: Summary statistics

	Migrants, 1st					Migrants, 2nd				
Variable	Mean	Sd	Min	Max	Obs.	Mean	Sd	Min	Max	Obs.
Employed	0.61	0.49	0.00	1.00	1917	0.61	0.49	0.00	1.00	1163
Female	0.55	0.50	0.00	1.00	1933	0.54	0.50	0.00	1.00	1164
Age	28.59	5.95	18.00	38.00	1933	27.22	6.01	16.00	38.00	1164
Religiosity	7.18	3.11	1.49	11.61	1449	6.10	3.10	1.49	11.61	885
Jew	0.00	0.04	0.00	1.00	1933	0.00	0.04	0.00	1.00	1164
Christian	0.52	0.50	0.00	1.00	1933	0.53	0.50	0.00	1.00	1164
Muslim	0.18	0.38	0.00	1.00	1933	0.10	0.30	0.00	1.00	1164
Hindu	0.01	0.08	0.00	1.00	1933	0.00	0.04	0.00	1.00	1164
Other	0.03	0.18	0.00	1.00	1933	0.03	0.17	0.00	1.00	1164

of labor supply would provide a more comprehensive analysis (H'madoun, 2010), the EVS does not provide an accurate measure of hours worked. However, it remains important to understand the impact of religiosity at the extensive margin of labor supply. We differentiate between first- and second-generation migrants in our empirical analysis as studies highlight the differences in employment rates due to inherent factors that distinguish between the two. Generally, the employment rate is found to be lower for first-generation migrants compared to second-generation migrants (Eurostat, 2017). One proposed reason is that first-generation migrants are more likely to have issues speaking recipient country language (Fernández, 2011). As a result, they tend to deviate from the norm due to the migration effect (Fernandez and Fogli, 2009) and face higher barriers to employment. However, Table 1.1 shows that 61% of first- and second-generation migrants are employed.

Figure 1.1 shows a breakdown of employment rates by migration status and gender. We find that second-generation migrant females have a slightly higher employment rate compared to first-generation females, whereas the employment rate is the same for sampled males. As shown in Table 1.1, the gender composition of both migrant types is almost identical. This is to our advantage as we have an almost equal representation of men and women. The literature on labor market outcomes provides evidence that gender-specific factors affect employment. The employment rate for women is lower than men across Europe (European Commission, 2017). This is confirmed for our

Figure 1.1: Employment rates by gender and migration status



sample of migrants as shown in Figure 1.1.[7]

The average age of sampled first- and second-generation migrants is 28 and 27, respectively. We control for age and its quadratic form to account for changes in employment due to differentials in wages over the life cycle. Employment is expected to be higher during periods of higher wages (H'madoun, 2010). However, there is mixed evidence on how wages change with age (Myck, 2010; Van Ours and Stoeldraijer, 2011; Cardoso et al., 2011). Therefore, it is not evident a priori what the sign of the coefficients of age and its squared form will be.

As for religiosity, which is our main variable of interest, Table 1.1 shows that first-generation migrants are on average more religious than second-generation migrants in line with (Algan and Aleksynska, 2012). Religiosity measures the degree of religious commitment (Croucher et al., 2017), it is multi-dimensional and incorporates different

[7]Different explanations are suggested in the literature to explain this gap. For instance, Redmond and Mcguinness (2019) point out the importance of differences between men and women in job motives to explain the wage gap in Europe. Specifically, their results show that women are driven towards jobs that provide flexible hours and security whereas men target financial gain. Jobs with flexible hours offer lower pay which reduces women's incentives to participate in the labor market, consequently leading to their lower employment rate compared to men (World Bank, 2011).

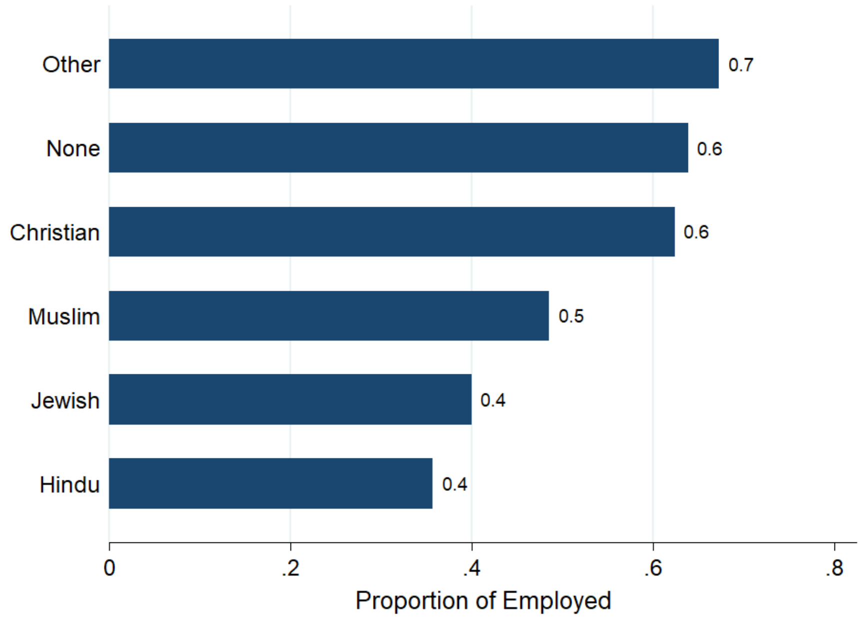
components (Vang et al., 2019). Some studies select a single measure as a proxy for religiosity such as church attendance (Brown and Taylor, 2007; Gruber, 2005), importance of religion to individuals (Caris and Hayo, 2012) and whether individuals believe religion should have more influence in society (Maneschiold and Haraldsonn, 2007). Generally, however, the focus on a single component of religiosity in a given study is likely to generate misleading results. This is because different dimensions of religiosity are highly correlated, and one measure might be reflecting the effect of other excluded measures (Rowatt and Kirkpatrick, 2002). As a result, we construct a religiosity index using Principal Component Analysis (PCA).[8] The religiosity index is based on individuals' responses to the following questions: the degree to which individuals believe in God, an after-life and heaven, and the importance of God in one's life. These questions measure the cognitive aspect of religion that determines an individual's perspective, which enhances their sense of responsibility to meet their work obligation (Praveen Parboteeah et al., 2009). The index also includes frequency of attendance to religious services and number of times individuals pray outside of religious services. These two constitute the normative aspect of religion that ensure individuals' regular exposures to teachings that emphasize the importance of work (Praveen Parboteeah et al., 2009). Finally, it includes the importance of religion in individuals' lives to proxy for intensity of belief. H'madoun (2010) suggests that individuals with a higher intensity of belief are more likely to follow work-related religious doctrines.

Table 1.1 also reports the composition of different religious groups. We find that it is very similar for both migrant types, except for Muslims who are higher amongst first-generation migrants. We further elaborate on the rate of employment between different religious groups in Figure 1.2, which shows that the employment rate of sampled migrants varies across religious denominations. It is difficult to establish whether these are supply-side effects representing differences across individuals or demand-side effects indicating employers' discrimination. In sum, the two migrant groups are quite similar with differences mostly pronounced in religious composition and commitment.

Figure 1.3 shows the employment rate for sampled migrants, displayed by their country of origin. As the map indicates, there is a clear variation in employment

[8] Details on Principal Component Analysis can be found in Appendix A.1.

Figure 1.2: Employment rates by religious denomination



rates by country of origin. Looking at migrants with European origin, they have an employment rate of 62% or more. North America and many South Asian countries have the highest proportion of employed migrants, while lowest employment rates are found amongst African, Middle Eastern and Australian migrants.

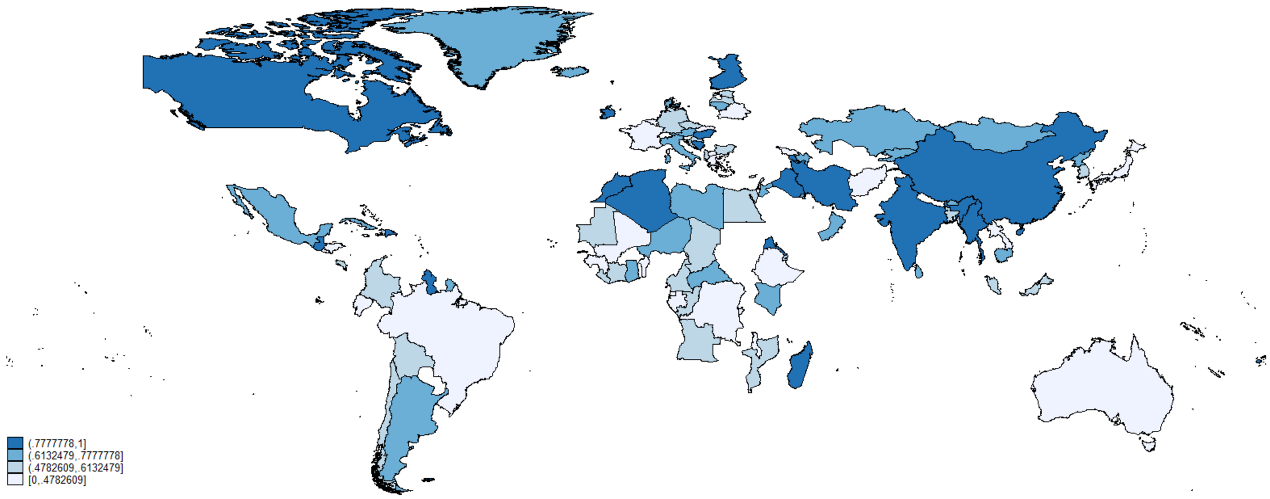
1.5 Results

Main Results

We begin the analysis by reporting the OLS estimates of Equation (1.1) in Table 1.2.[9] In Column (1), we simply regress employment on religiosity without any control variable. The estimated coefficient's magnitude for religiosity is statistically significant at the 1% level. It shows that the probability of employment is lower for individuals with higher religiosity. Specifically, a one standard deviation increase in religiosity leads to a

[9]The number of observation is smaller than our total sample due to missing observations in employment, religiosity index and the use of religious denomination, country of origin and country of residence fixed effects that combine individuals at each level.

Figure 1.3: Employment rates by country of origin



decline in the probability of employment by 1.6 percentage points.[10] As we move from Columns (2) to (5), we separately add individual-level control variables or fixed effects for religious denomination, country of origin or country of residence. In Column (2), the coefficient estimate is identical in magnitude after controlling for individual-level covariates. The coefficient for first-generation migrant is not statistically significant, so there is no evidence of a difference between first- and second-generation migrants when it comes to employment.[11] As expected, females are less likely to be employed compared to their male counterparts. Finally, there is an inverted-U shaped relationship between age and employment, suggesting that employment falls as people get older. In Column (3), we control for religious-denomination fixed effects, so we are only comparing individuals within the same religious denomination in this column. We find a very slight decline in the main coefficient's magnitude, suggesting that differences across denominations is not very strong. Column (4) compares individuals from the same country of origin, so this column separates the effect of culture from religion. The magnitude of religiosity index declines substantially in magnitude. This suggests that failure to account for culture results in an upward bias in the coefficient estimate of religiosity. In Column (5) of Table 1.2, we include country-of-residence fixed effects to control for policies and institutional factors that might drive the employment outcome. In this case, the

[10]The religiosity index is standardized, thus interpretation is based on standard deviation.
[11]This will be further explored in the heterogeneity analysis subsection.

Table 1.2: The effect of religiosity on employment

Dependent var. Employment	(1)	(2)	(3)	(4)	(5)	(6)
Religiosity	-0.016***	-0.016***	-0.015***	-0.012***	-0.011***	-0.011**
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
First-migrant		-0.029				-0.045
		(0.032)				(0.033)
Female		-0.173***				-0.185***
		(0.022)				(0.025)
Age		0.152***				0.165***
		(0.021)				(0.022)
Age squared		-0.002***				-0.003***
		(0.000)				(0.000)
Constant	0.707***	-1.601***	0.697***	0.676***	0.673***	-1.767***
	(0.027)	(0.308)	(0.030)	(0.029)	(0.027)	(0.329)
Observations	2325	2325	2325	2290	2323	2288
Controls:						
Religion FE	No	No	Yes	No	No	Yes
Origin FE	No	No	No	Yes	No	Yes
Residence FE	No	No	No	No	Yes	Yes

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

magnitude of our main coefficient also falls substantially. In the final column (6), we control for all individual-level variables and fixed effects at religious denomination, origin and residence country. Therefore, more variation is lost as we compare individuals from the same country of origin who adhere to the same religion and live in the same country of residence. However, this helps us address the potential omitted variable bias. Compared to the coefficient estimates in Column (1) of Table 1.2, the OLS estimate remains statistically significant but drops in magnitude.

The closest literature to the present study does not offer comparable coefficient estimates. The studies either examine a different measure of culture, use aggregate country-level variables or examine religiosity and different labor-market outcomes (Praveen Parboteeah et al., 2009; Mocan and Pogorelova, 2015; Spenkuch, 2017; Moriconi and Peri, 2019). However, Praveen Parboteeah et al. (2009) who examine the effect of similar dimensions of religiosity on work norm perception find a positive effect. This difference in the direction of the effect compared to the present study is likely driven by empirical considerations. First, they use a separate measure of cognitive and normative components of religiosity as opposed to the present study that combines them into a single index. Second, their dependent variable is individuals' responses to their work norm perceptions as opposed to their actual employment status. Third, their individuals' responses are aggregated to create a country-level measure of religiosity. As a result, they do not account for individual variation. Finally, their sample includes 62,218 individuals from 45 countries across the globe. This latter point raises questions about the generalizability of our results, and hints that they might be specific to the European case. Furthermore, our sample of around 2000 individuals is significantly smaller. Therefore, although our model exploits individual heterogeneity and controls for a rich set of fixed effects, the context of study and small sample size warrant caution in interpreting the study's findings.

Robustness Checks

We provide some robustness analysis to check the sensitivity of our results to changes in the sample's definition or variables' selection. We start by reporting the p-value of wild-bootstrap clustered standard error for the model in Equation (1.1) in Table A.5 in Appendix A.2. The p-value of the model using wild-bootstrap clustered standard error indicates that our coefficient is statistically significant. Therefore, it gives us the same conclusion that religiosity has a statistically significant effect on employment. As a result, we conclude that the unbalanced nature of the clusters are not rendering our estimated results unreliable.

In the main analysis, we defined migrants based on their mothers' country of origin. Since the literature provides mixed evidence on which parent's culture is likely to have more influence on children, in Table A.6 of Appendix A.2 we replicate Table 1.2 but define migrants' culture based on their fathers' country of origin instead of the mother. The estimated coefficients are fairly similar. In Table A.7, we drop individuals who did not specify a religious denomination or did not disclose it. They were included in the main analysis because they answered questions on religiosity despite not being affiliated with a religion or not disclosing it, so we exclude them to check whether they are driving the results. The OLS estimates are still statistically significant and similar in magnitude to our main estimates. In Table A.8, we control for the duration of stay in host country for first-generation migrants. Duration of stay might affect employment as migrants become more accustomed to the country of residence and its labor market. Controlling for this variable changes the sample because it is not defined for second-generation migrants. Therefore, we lose almost half of the observations compared to our main sample. Still, the main coefficient's estimates are similar to our main results.

Overall, the robustness checks confirm our main results. For the remaining analysis, we focus on the sample in our main results in Table 2.1. More specifically, we focus on the specification that controls for all individual-level controls and fixed effects at the religious denomination, origin country and residence country to ensure our religiosity index is not capturing the effect of other variables.

Heterogeneity Analysis

This section provides a heterogeneity analysis to better understand whether the effect of religiosity varies across different components of religiosity or categories of individuals. Although Vang et al. (2019) point out that an index for religiosity is better than separate components, some studies still use the latter measure and examine separate components of religiosity (Brown and Taylor, 2007; Gruber, 2005; Caris and Hayo, 2012; Maneschiold and Haraldsonn, 2007). To situate our results within both strands of literature, we examine the effect of separate components of our religiosity index on employment instead of the combined index. This will also allow us to check if the effect is different across components. The coefficient estimates in Table 1.3 remain negative and statistically significant for two out of seven components, although they vary in magnitude. The variable on importance of religion in one's life has a negative effect on employment and the magnitude is higher compared to previous specifications. This suggests that the more importance individuals attach to religious doctrine, the less likely they are to be employed. This contrasts with the hypothesis that religious doctrines emphasize work and thus increase participation (Praveen Parboteeah et al., 2009). The negative effect of the extent of prayer on employment is in line with theoretical predictions on the trade off between time dedicated to religious practice and work (Basedau et al., 2018).

In Table 1.4, we check how the results change if we split the sample by male and

Table 1.3: The effect of religiosity on employment. Use different religiosity components

Dependent var. Employment	(1) Rel_I	(2) God_B	(3) Afterlife	(4) Heaven	(5) God_I	(6) Attend	(7) Pray
Relig. Component	-0.023**	-0.019	0.000	-0.030	-0.005	-0.009	-0.015***
	(0.009)	(0.023)	(0.016)	(0.023)	(0.004)	(0.007)	(0.004)
First-migrant	-0.037	-0.039	-0.050	-0.046	-0.030	-0.036	-0.035
	(0.031)	(0.030)	(0.031)	(0.031)	(0.030)	(0.030)	(0.029)
Female	-0.177***	-0.184***	-0.186***	-0.184***	-0.175***	-0.180***	-0.171***
	(0.022)	(0.024)	(0.022)	(0.023)	(0.022)	(0.022)	(0.022)
Age	0.147***	0.152***	0.160***	0.153***	0.152***	0.144***	0.148***
	(0.020)	(0.021)	(0.021)	(0.021)	(0.020)	(0.021)	(0.021)
Age squared	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-1.526***	-1.645***	-1.757***	-1.648***	-1.628***	-1.519***	-1.544***
	(0.299)	(0.304)	(0.306)	(0.304)	(0.304)	(0.309)	(0.318)
Observations	3001	2825	2601	2684	2945	3002	2954
Controls:							
Religion FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Residence FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Notes: Rel_I is importance of religion in one's life, God_B is belief in God, Afterlife is belief in life after death, Heaven is belief in heaven, God_I is importance of God in one's life, Attend is attendance at religious services and Pray is extent of prayer outside of religious services.

female or by first- and second-generation migrants. We find that the OLS coefficient estimate is statistically significant for females but not for males. This result is in line with Guiso et al. (2003) who show a more conservative view regarding women's work amongst religious individuals. For first- and second-generation migrants, results are very similar to the main analysis, although the OLS coefficient estimate for second-generation migrants is slightly higher in magnitude. This suggests that religiosity has a stronger impact on employment for second-generation migrants.

Table 1.4: The effect of religiosity on employment. Heterogeneity analysis I

Dependent var. Employment	(1) Male	(2) Female	(3) First-migrant	(4) Second-migrant
Religiosity	-0.006	-0.016***	-0.011**	-0.014**
	(0.007)	(0.005)	(0.005)	(0.007)
First-migrant	0.037	-0.117***	-	-
	(0.057)	(0.035)	-	-
Female	-	-	-0.230***	-0.085**
	-	-	(0.031)	(0.037)
Age	0.217***	0.129***	0.153***	0.192***
	(0.030)	(0.027)	(0.028)	(0.023)
Age squared	-0.003***	-0.002***	-0.002***	-0.003***
	(0.001)	(0.000)	(0.000)	(0.000)
Constant	-2.619***	-1.337***	-1.574***	-2.220***
	(0.413)	(0.387)	(0.390)	(0.320)
Observations	1002	1246	1407	844
Controls:				
Religion FE	Yes	Yes	Yes	Yes
Origin FE	Yes	Yes	Yes	Yes
Residence FE	Yes	Yes	Yes	Yes

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

Table 1.5: The effect of religiosity on employment. Heterogeneity analysis II

Dependent var. Employment	(1) No religion	(2) Muslims	(3) Christians	(4) Other
Religiosity	-0.016	-0.011	-0.007	-0.038
	(0.011)	(0.016)	(0.005)	(0.029)
First-migrant	-0.038	-0.226**	-0.023	0.108
	(0.039)	(0.094)	(0.033)	(0.147)
Female	-0.157***	-0.364***	-0.166***	0.121
	(0.034)	(0.030)	(0.029)	(0.123)
Age	0.148***	0.051	0.208***	-0.177
	(0.032)	(0.039)	(0.029)	(0.123)
Age squared	-0.002***	-0.001	-0.003***	0.003
	(0.001)	(0.001)	(0.000)	(0.002)
Constant	-1.628***	0.032	-2.421***	3.626*
	(0.412)	(0.556)	(0.442)	(1.752)
Observations	602	381	1156	60
Controls:				
Religion FE	Yes	Yes	Yes	Yes
Origin FE	Yes	Yes	Yes	Yes
Residence FE	Yes	Yes	Yes	Yes

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

Channels

In this section, we try to better understand the channels that drive the negative association between religiosity and employment. As discussed in Section 1.2, there are different postulated channels to explain the relationship between religiosity and employment. We focus on potential channels that explain why religiosity might reduce employment; in particular: job reservation, family orientation, sense of mastery and discrimination. We assess the potential plausibility of each of the aforementioned channels, as it is difficult to completely rule out some or fully prove others (Couttenier et al., 2016). To proceed, we estimate our main equation that controls for all individual-level covariates and fixed effects after adding a proxy for these channels, one at a time. This allows us to examine the stability of the coefficient estimate of the religiosity variable, in the case where it changes significantly following the inclusion of the channel proxy we conclude that it is a potential channel (Couttenier et al., 2016). Results are reported in Table 1.6, we include one channel variable in each uneven column, followed by an even column that runs the regression without the channel variable for the same sample to ensure comparability.

We start with a proxy for the possibility that religious people may need more time to find a job or are more likely to turn job offers if it does not conform with the law of their religious doctrine. We use a variable from EVS that asks individuals to select on a scale of 1-10 which of the following statements they agree more with: people who are unemployed should have to take any job available or lose their unemployment benefits versus people who are unemployed should have the right to refuse a job they do not want. An inclination towards the second statement is taken as an indication of job reservation. The results are reported in the first column of Table 1.6, we do not find evidence that job reservation has an effect on employment. Moreover, the coefficient of religiosity is the same as the base regression estimated on the same sample in Column (2). Therefore, there is no evidence that job reservation is the drive behind the observed effects of religiosity on employment. In order to proxy for family orientation, we construct a family index using Principal Component Analysis based on three variables from EVS following Alesina and Giuliano (2014). The coefficient estimate of our main variable remains unchanged, as shown in Column (3) of Table 1.6. Thus, there is no evidence to conclude that the effect of religiosity on employment is driven by strong family ties of

Table 1.6: The effect of religiosity on employment. Channels analysis

Dependent var. Employment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Religiosity	-0.012***	-0.012***	-0.012***	-0.012***	-0.011**	-0.011**	-0.011**	-0.011**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Reservation	-0.004							
	(0.004)							
Family			0.002					
			(0.013)					
Mastery					0.006			
					(0.005)			
Discrim.							0.143**	
							(0.069)	
First-migrant	-0.045	-0.045	-0.045	-0.045	-0.049	-0.049	-0.016	-0.015
	(0.030)	(0.030)	(0.034)	(0.034)	(0.034)	(0.034)	(0.033)	(0.033)
Female	-0.181***	-0.182***	-0.181***	-0.181***	-0.185***	-0.186***	-0.185***	-0.185***
	(0.026)	(0.026)	(0.025)	(0.025)	(0.025)	(0.024)	(0.028)	(0.028)
Age	0.166***	0.165***	0.166***	0.166***	0.163***	0.164***	0.193***	0.194***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.031)	(0.031)
Age squared	-0.003***	-0.003***	-0.003***	-0.003***	-0.002***	-0.002***	-0.003***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Constant	-1.763***	-1.771***	-1.783***	-1.776***	-1.778***	-1.751***	-2.150***	-2.139***
	(0.332)	(0.332)	(0.330)	(0.329)	(0.334)	(0.332)	(0.473)	(0.471)
Observations	2245	2245	2205	2205	2265	2265	1221	1221
Controls:								
Religion FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Residence FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

sampled individuals. A third channel we examine is the sense of mastery. The proxy for it is a question from EVS on how much freedom of choice and control the respondents feel they have over the way their lives turn out. Column (5) of Table 1.6 indicates that the religiosity variable is not significantly affected by the inclusion of this variable. Therefore, there is no evidence that sense of mastery is a potential channel that explains the effect of religiosity on employment.

The final channel we examine is religious discrimination.[12] The EVS questionnaire does not ask individuals to report instances of employment discrimination. Therefore, no direct measure of discrimination is available in our setting. However, an assessment of the extent of labor market discrimination at the country level is available from round 3 of The Religion and State Project (RAS), Minorities Module. It provides a measure of discrimination that captures "instances of societal economic discrimination against minority religions in the workplace" (Fox, 2017). Specifically, the variable has the following values: 2 = this action occurs on a substantial level, 1 = this action occurs on a minor level, 0 = there are no reported incidents of this type of action against the specified minority (Fox, 2017). We merge this variable with our dataset using religious denomination and country of residence. Thus, it captures discrimination faced by individuals from a specific religious group living in a particular country of residence, irrespective of their country of origin. In other words, it does not account for ethnic discrimination. This is to our advantage since we are trying to capture religious discrimination. Also, since we include country of origin fixed effects, we are more likely to rule out the effect of culture. In Column (7) of Table 1.6, we provide OLS estimates of the effect of religiosity after controlling for discrimination. The estimated effects of religiosity does not change. However, we find an odd positive impact of discrimination on employment. This might be due to the combination of a discrimination variable at the country level with individual-level variables. However, due to data limitation, no alternative measure of discrimination is available.

In sum, we do not find evidence that any of the aforementioned channels are a likely connection between religiosity and employment. Therefore, a better proxy for these channels is worth examining if available. Moreover, it may be the case that other channels drive the relationship or it may be the case that religiosity has an inherent

[12]Religious discrimination is defined as "treating a person (an applicant or employee) unfavorably because of his or her religious beliefs" (U.S. Equal Employment Opportunity Commission, n.d.).

effect in and of itself on employment.

1.6 Conclusion

The effect of religiosity has been examined in the economic literature with regards to different economic outcomes. This paper limits its attention to the impact of religiosity on an employment outcome. To that purpose, we examine first- and second-generation migrants in 46 European countries. The use of migrants allows us to exploit recent methodological developments in the economic literature used to understand the role of culture on economic outcomes. Namely, we use the epidemiological approach by Fernández (2008) to separate the effect of culture from economic and institutional factors. We also follow an extension of this approach by Guner and Uysal (2014) and control for an explicit measure of religiosity to disentangle its effect from culture. Furthermore, we control for a rich set of fixed effects at religious denomination, country of origin and country of residence to ensure that we are capturing the effect of individuals' religiosity. The findings suggest a negative effect of religiosity on employment, which holds to a different set of robustness checks. However, we do not find evidence that any of the examined channels are driving this negative effect. Therefore, further exploration using better proxies is encouraged.

Our results should be approached with caution due to some limitations. One key issue is that some of the survey questions on religiosity are oriented towards the Christian religion, which is mainly due to the influence of Catholic-oriented agents in its constellation (Kropp, 2017). This could have affected the responses of individuals from other denominations in unexpected ways. Also, although our religiosity index includes different measures, it is by no means comprehensive. Future research can provide more insight on the relationship between religiosity and employment by taking these issues into consideration. Finally, there are different sources of endogeneity in religiosity with respect to employment. First, religiosity is likely to be a choice variable, so factors that drive religiosity could also affect the outcome of interest (Regnerus and Smith, 2005). Some factors likely to affect both religiosity and employment include ethnicity, race and cultural surroundings (Regnerus and Smith, 2005). Although these variables are observed and can be included directly in the model, there are unobservable factors that affect both religiosity and employment such as personality differences regarding risk-taking behavior (Regnerus and Smith, 2005). Omitted variables bias can result in a misleading conclusion that religiosity affects employment when it is other mediating factors driving this relationship (Regnerus and Smith, 2005). Furthermore, reverse causality is a possible concern. On the one hand, religious individuals may have a stronger incentive to become employed as major world religions view work as an individual's obligation (Praveen Parboteeah et al., 2009). On the other hand, unemployment could make people more religious as they seek refuge in religion to deal with an adverse life event (Clark and Lelkes, 2006; Sinding Bentzen, 2019). Therefore, it is difficult to isolate the impact of religiosity on employment given the bidirectional relationship between the two variables.

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Appendix A

Chapter 1 Appendix

A.1 Principal Component Analysis

Principal Component Analysis (PCA) is a statistical technique that summarizes the variance of a set of correlated variables into a single index by creating a linear combination of them (Bertrand and Schoar, 2006). In particular, it is a data reduction technique that captures a significant portion of the variation between a set of variables into a smaller number of components (Abeyasekera, 2005). Each created component is a weighted linear combination of the original variables and is uncorrelated with other components (Vyas and Kumaranayake, 2006). The first generated component captures the highest level of variability (Abeyasekera 2005), whilst each successive components account for additional but less variability (Vyas and Kumaranayake, 2006). Within each component, variables with higher level of variability are given higher weights, whereas variables with very low variability may be excluded (Abeyasekera, 2005). Since we are trying to combine the different aspects of religiosity into a single index, PCA gives us the tool to achieve that.

Selection of Variables

The first step in PCA is the selection of the variables, which should be based on previous literature (Vyas and Kumaranayake, 2006). However, Crossman (2019) points out the criteria that selected variables must meet. First is face validity, that is;

they must represent the variable we want to measure. Second is unidimensionality in which each variable reflects one dimension of the concept we are trying to measure. Third, variables' selection depends on whether we want to construct a specific or a general measure of the concept of interest. In our case, we are interested in different aspects of religiosity. However, in case we were only interested in the cognitive aspect, for instance, we will limit our variables to those capturing this dimension only. Finally, selected variables should provide enough variation across units (for example individuals), because variables that do not represent any individual or have the exact same value for all individuals will not be useful in index construction. As mentioned earlier, PCA creates components based on the variability between selected variables, if there is no variation then these variables will not be part of the generated components.

For the construction of a general religiosity index, I select a set of eight variables after a review of the literature (Praveen Parboteeah et al., 2009; H'madoun, 2010):

- The degree to which individuals believe in 1) God, 2) an after-life, 3) hell and 4) heaven. These can take values: 0: no, 1: yes.
- The importance of God in one's life. This variable can take values: 1: not at all important, 2, 3, ... 10: very important.
- The frequency of attendance to religious services apart from weddings, funerals and christenings. This variable can take values: 7: more than once a week, 6: once a week, 5: once a month, 4: only on specific holy days, 3: once a year, 2: less often, 1: never, practically never.
- The number of times individuals pray to God outside of religious services. This variable can take values: 7: every day, 6: more than once a week, 5: once a week, 4: at least once a month, 3: several times a year, 2: less often, 1: never.
- The importance of religion in an individual's life. This variable can take values: 4: very important, 3: quite important, 2: not important, 1: not at all important.

The chosen variables are ordinal in nature. PCA can be used for continuous and ordinal data (Abeyasekera, 2005), so we can apply it on the selected variables to create our religiosity index.

Examination of the Correlation Between Selected Variables

We look at the correlation between selected variables to identify the ones we can include in PCA. This is to ensure that selected variables are correlated and are all representative of religiosity (Crossman, 2019). If two variables have a correlation above 0.9, one must be excluded as they are likely measuring the same thing, whereas a very low correlation of of less than 0.1 will load these variables into a single component, so their inclusion is not helpful (UCLA: Statistical Consulting Group, 2019). Given that our variables are ordinal, Pearson correlation cannot be used because it assumes that the data are normally distributed, an assumption that requires truly continuous data to hold (Grace-Martin, n.d.). Therefore, the Polychoric correlation, which is based on maximum likelihood but has the same interpretation as Pearson correlation, will be used (Grace-Martin, n.d.). Since our PCA analysis is conducted on correlations, the variables will be standardized (UCLA: Statistical Consulting Group, 2019). The correlation matrix between the eight variables is given in Table A.1. The estimates of the correlation coefficients show that all variables are highly correlated. However, belief in hell and belief in heaven have a correlation of 0.98 which is very high, so one of them should be removed. I keep the latter because it has a higher level of correlation with other variables compared to the former. The re-estimated correlation matrix after removing belief in hell is given in Table A.2, it shows that all variables are highly correlated with one another. However, none of the correlations are higher or lower than the recommended 0.9 or 0.1, respectively.

Construction of the Religiosity Index

After selecting variables in the previous step, we can run Principal Component Analysis and generate the components. These components represent composite variables that combine several original variables (Crossman, 2019). There are different stopping rules to decide which components will be used to generate the overall religiosity index (Brown, 2009a). One is Kaiser's stopping rule that suggests choosing components with an eigenvalue above 1 (Brown, 2009a). The eigenvalues represent the percentage of total variation in the data that is explained by a given component (Vyas and Kumaranayake,

Table A.1: Correlation between variables 1

Variables	Rel_I	God_B	Afterlife	Hell	Heaven	God_I	attend	Pray
Rel_I	1							
God_B	.812	1						
Afterlife	.480	.660	1					
Hell	.624	.820	.807	1				
Heaven	.627	.828	.801	.980	1			
God_I	.707	.850	.472	.629	.634	1		
Attend	.620	.711	.439	.526	.548	.583	1	
Pray	.727	.874	.541	.646	.672	.770	.670	1

Notes: Rel_I is importance of religion in one's life, God_B is belief in God, Afterlife is belief in life after death, Hell is belief in Hell, Heaven is belief in heaven, God_I is importance of God in one's life, Attend is attendance at religious services and Pray is extent of prayer outside of religious services.

Table A.2: Correlation between variables 2

Variables	Rel_I	God_B	Afterlife	Heaven	God_I	Attend	Pray
Rel_I	1						
God_B	.812	1					
Afterlife	.470	.659	1				
Heaven	.628	.828	.800	1			
God_I	.706	.852	.470	.634	1		
Attend	.620	.709	.437	.547	.580	1	
Pray	.727	.874	.540	.671	.767	.668	1

Notes: Rel_I is importance of religion in one's life, God_B is belief in God, Afterlife is belief in life after death, Hell is belief in Hell, Heaven is belief in heaven, God_I is importance of God in one's life, Attend is attendance at religious services and Pray is extent of prayer outside of religious services.

2006). The rationale of Kaiser's stopping rule is that an eigenvalue greater than one indicates that the component explains more variance than a single variable (Rahn, n.d.). Table A.3 reports the output of PCA components. The first component is the only one that meets Kasier's stopping rule with an eigenvalue of 5.03 (Vyas and Kumaranayake, 2006). Another criteria is a scree plot, which provides a graphical representation of the relationship between eigenvalues and the number of components (Brown, 2009a). The rule is to pick up the number of components at the point where the curve's slope levels off. This is because the amount of explained variance after this point does not rise a lot, so it is not very beneficial to add more components (UCLA: Statistical Consulting Group, 2019). The scree plot in Figure A.1 suggests taking into account component one only. Finally, the cumulative variance accounted for by the inclusion of different components must be taken into consideration (Brown, 2009a). Table A.3 shows that 72% of the total variance is explained by component 1. Brown (2009a) argues that the choice of components to include is an art. Therefore, we choose one component not only because it meets two rules of thumb, but also because the variables in the first component have a relatively similar correlation level with this component. The other components account for some variables more than others as shown in Table A.4.[1] Given our question of interest, we want the religiosity index to reflect the different dimensions of religiosity. Therefore, we construct the religiosity index based on the first component of the PCA. The index value is between 1.5 and 11.6 as shown in Table 1.1.

[1]The PCA output in Table A.4 gives us the component loadings, which is the correlation between each variable and the component. Component loadings allow us to identify which aspect of religiosity is measured by each component (Brown, 2009b). Brown (2009b) suggests that a correlation below 0.3 would be ignored in this analysis. However, choosing what level of correlation is high enough to implicate a given variable is being accounted for by a given component is subjective and must be carefully justified (Brown, 2009b).

Table A.3: PCA component

Component	Eigenvalue	Difference	Proportion	Cumulative
Comp1	5.03695	4.26869	0.7196	0.7196
Comp2	.768266	.318289	0.1098	0.8293
Comp3	.449977	.148172	0.0643	0.8936
Comp4	.301805	.0844863	0.0431	0.9367
Comp5	.217319	.0437668	0.0310	0.9678
Comp6	.173552	.121426	0.0248	0.9926
Comp7	.0521263	.	0.0074	1.0000

Figure A.1: Scree plot after PCA for 7 items of religiosity index

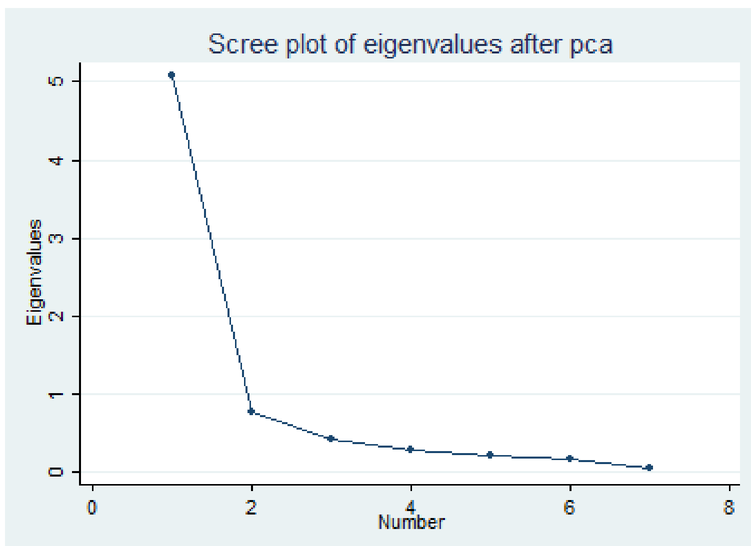


Table A.4: Component loadings

Variable	Comp1	Comp2	Comp3	Comp4	Comp5	Comp6	Comp7	Unexplained
Rel_I	0.3755	-0.2661	-0.1687	0.8514	0.0116	0.1279	0.1354	0
God_B	0.4334	-0.0496	-0.1160	-0.0651	0.0049	-0.1712	-0.8733	0
Afterlife	0.3252	0.7198	0.1174	0.0179	-0.0575	0.5988	-0.0141	0
Heaven	0.3835	0.4515	-0.0285	0.0233	0.1763	-0.7219	0.3092	0
God_I	0.3797	-0.2669	-0.4459	-0.4259	0.5210	0.2706	0.2444	0
Attend	0.3412	-0.3033	0.8579	-0.0950	0.1997	0.0350	0.0739	0
Pray	0.3973	-0.2037	-0.0931	-0.2822	-0.8088	-0.0162	0.2407	0

A.2 Robustness Analysis

Table A.5: Estimate, P values and 95% confidence intervals for religiosity index

	OLS Model	Boottest
Estimate	-0.01055	-
cluster robust S.E	0.00418	-
t-statistic	-2.53	-2.53
P-value	0.012	0.020

Table A.6: The effect of religiosity on employment. Father's country of origin

Dependent var. Employment	(1)	(2)	(3)	(4)	(5)	(6)
Religiosity	-0.016***	-0.014***	-0.015***	-0.011**	-0.012***	-0.008**
	(0.004)	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)
First-migrant		-0.037				-0.037
		(0.032)				(0.037)
Female		-0.175***				-0.184***
		(0.022)				(0.026)
Age		0.142***				0.152***
		(0.020)				(0.021)
Age squared		-0.002***				-0.002***
		(0.000)				(0.000)
Constant	0.707***	-1.467***	0.698***	0.670***	0.678***	-1.598***
	(0.029)	(0.298)	(0.030)	(0.033)	(0.027)	(0.317)
Observations	2298	2298	2298	2266	2297	2265
Controls:						
Religion FE	No	No	Yes	No	No	Yes
Origin FE	No	No	No	Yes	No	Yes
Residence FE	No	No	No	No	Yes	Yes

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

Table A.7: The effect of religiosity on employment. Drop individuals with no denomination or those who do not disclose it

Dependent var. Employment	(1)	(2)	(3)	(4)	(5)	(6)
Religiosity	-0.019***	-0.018***	-0.014***	-0.013**	-0.015***	-0.009*
	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
First-migrant		-0.030				-0.051
		(0.043)				(0.046)
Female		-0.189***				-0.203***
		(0.026)				(0.031)
Age		0.158***				0.174***
		(0.024)				(0.026)
Age squared		-0.002***				-0.003***
		(0.000)				(0.000)
Constant	0.732***	-1.624***	0.695***	0.681***	0.699***	-1.868***
	(0.036)	(0.352)	(0.037)	(0.043)	(0.038)	(0.393)
Observations	1687	1687	1687	1661	1685	1659
Controls:						
Religion FE	No	No	Yes	No	No	Yes
Origin FE	No	No	No	Yes	No	Yes
Residence FE	No	No	No	No	Yes	Yes

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

Table A.8: The effect of religiosity on employment. Control for duration of first-generation migrants

Dependent var. Employment	(1)	(2)	(3)	(4)	(5)	(6)
Religiosity	-0.020***	-0.016***	-0.021***	-0.012**	-0.014***	-0.015***
	(0.005)	(0.003)	(0.005)	(0.006)	(0.005)	(0.005)
firstmig duration	0.002		0.001	0.002	0.003**	-0.000
	(0.002)		(0.002)	(0.002)	(0.001)	(0.002)
First-migrant		-0.029				0.000
		(0.032)				(.)
Female		-0.173***				-0.231***
		(0.022)				(0.035)
Age		0.152***				0.158***
		(0.021)				(0.026)
Age squared		-0.002***				-0.002***
		(0.000)				(0.000)
Constant	0.715***	-1.601***	0.729***	0.659***	0.665***	-1.607***
	(0.041)	(0.308)	(0.047)	(0.042)	(0.041)	(0.365)
Observations	1389	2325	1388	1356	1388	1354
Controls						
Religion FE	No	No	Yes	No	No	Yes
Origin FE	No	No	No	Yes	No	Yes
Residence FE	No	No	No	No	Yes	Yes

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

Chapter 2

The Gravity of Distance: Evidence from a Trade Embargo

2.1 Introduction

Since the industrial revolution, the reduction of transport costs has been a major driver behind the economic integration of nations. Revolutions in transport 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, 2021, 2019; Campante and Yanagizawa-Drott, 2018) have often accompanied new waves of globalization. It is therefore not surprising 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 are used to quantify the effect of Brexit on trade and income (see Bisciari (2019) for an overview of 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 transport costs by regressing trade flows on distance. Based on a meta analysis of estimates from 159 studies, Head and Mayer (2014) confirm Frankel and Romer (1999)'s result 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 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 transport developments or external shocks on travel distance to identify the effect of transportation costs on trade among other outcomes (Feyrer, 2021; Martincus and Blyde, 2013; Pascali, 2017; Donaldson, 2018; Feyrer, 2019; Baniya et al., 2019). 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 between -0.9 and -1.6.

Our paper is revising such estimates by offering an alternative identification of the effect of air transportation costs on trade. More specifically, we take advantage of an unexpected change in air travel costs due to the sudden closure of 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 transportation costs 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 trade embargo, our findings point to an elasticity of -0.3, which increases to about -0.5 when controlling for possible substitution effects with other transportation modes. 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 echoes recent findings that exploit similar time-varying shocks to distance.

The remainder of the paper is organized as follows. Section 2.2 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 2.3 summarizes the data construction and derives the model specification to be estimated from a simplified and standard gravity model. Section 2.4 provides the main results, followed by a series of robustness checks in Section 2.5. In Section 2.6, we discuss the economic significance of our results and the limits of our analysis.

2.2 The Blockade Against Qatar

As a result of political differences, Bahrain, Saudi Arabia, United Arab Emirates (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.

These four 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 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 closing of Qatar's airspace 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]

[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 U.S. (excluding trade with Mexico and Canada).

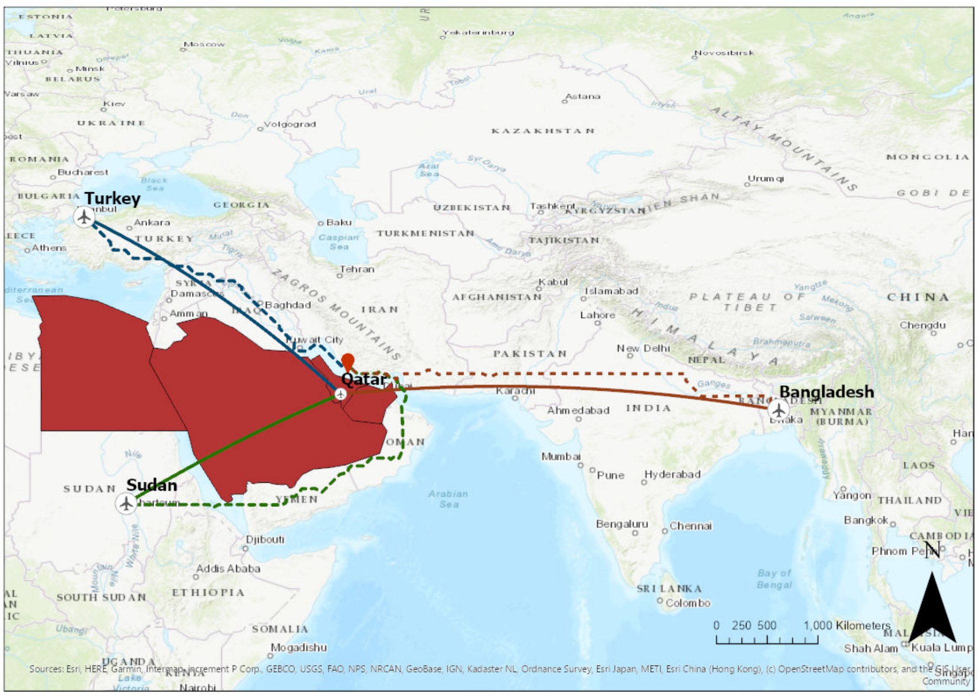
Based on countries' geographical locations, some routes were more affected than others. Figure 2.1 illustrates changes in air distance for a sample of pre- and post-embargo routes to three non-blockading countries. It is clear that flights to Sudan were more severely affected than those to Turkey and Bangladesh, only because of the location of these countries relative to Qatar and the blockading countries. This exogenous variation, only due to countries' geographical positions, is key to our identification. Since the embargo also includes the closing 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]

The immediate consequence of the blockade was a collapse in trade with the blockading countries. 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 decline of imports to Qatar from blockading countries was steep and unexpected, as shown in Panel (a) of Figure 2.2. 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 highlights the unexpected nature of the embargo, which will be further discussed in Section 2.4 and in Appendix B.1.[4] Interestingly, Panel (b) of Figure 2.2 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 B.2 in Appendix B.1), 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).

[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 bunker site in Fujairah port (Khasawneh and Vukmanovic, 2017) and Jebel Ali port that used to receive big ships and loaded cargoes 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).

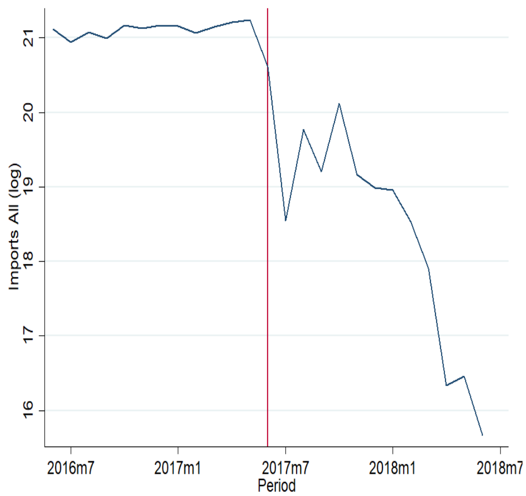
[4] According to Figure B.1 of Appendix B.1, total trade between Qatar and the four blockading countries follow a similar trend to the imports featured in Panel (a) of Figure 2.2.

Figure 2.1: Examples of flights diversion

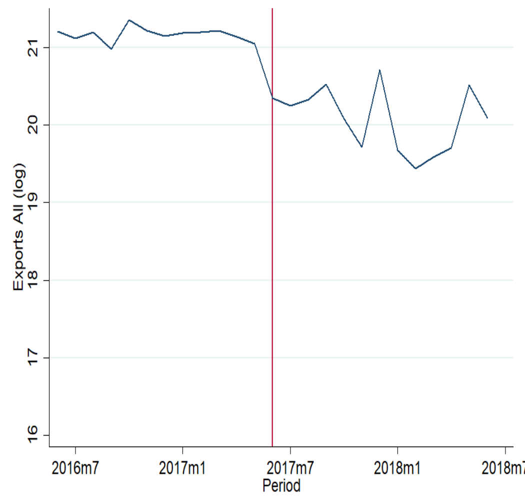


Notes: Solid lines indicate pre-embargo period routes; dotted lines indicate post-embargo period routes.
Source: Authors' calculations using Geocoded Data.

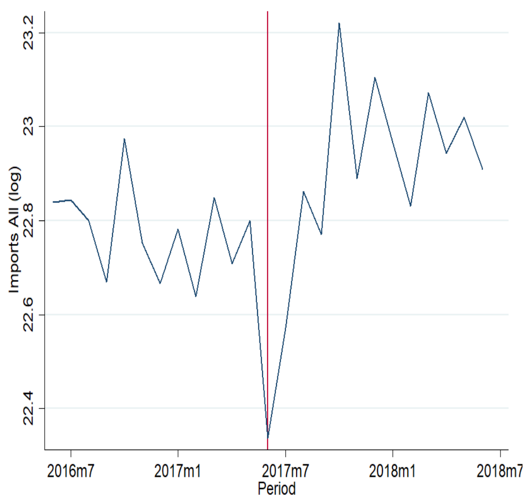
Figure 2.2: Trade responses to the embargo



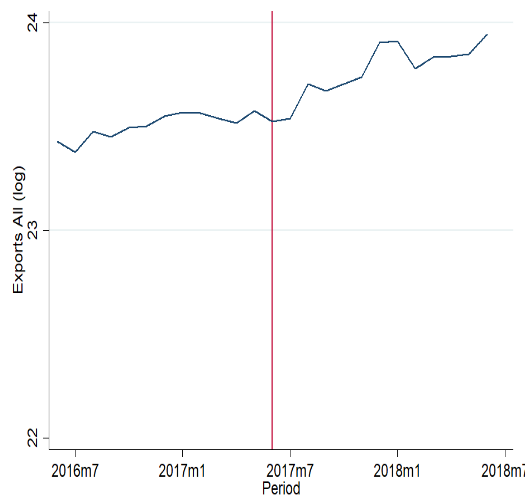
(a) Imports from blockading countries



(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.

Faced with such a disruption, it is expected to observe some trade diversion whereby imports from non-blockading countries may increase. This is what appears to be the case in Panels (c) and (d) of Figure 2.2 where 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 2.3 shows reductions of around 90% for imports from blockading countries in the top 10^{th} percentile of sectors in terms 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 2.3. 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 heterogeneous effect of the embargo on air distance across non-blockading trade partners to identify the role of transportation costs on trade.

2.3 Data & Methodology

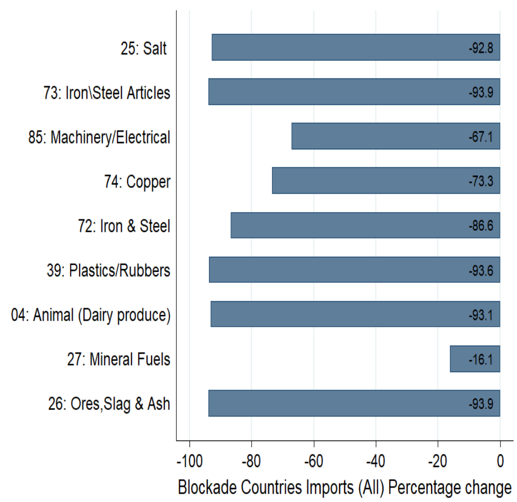
Data

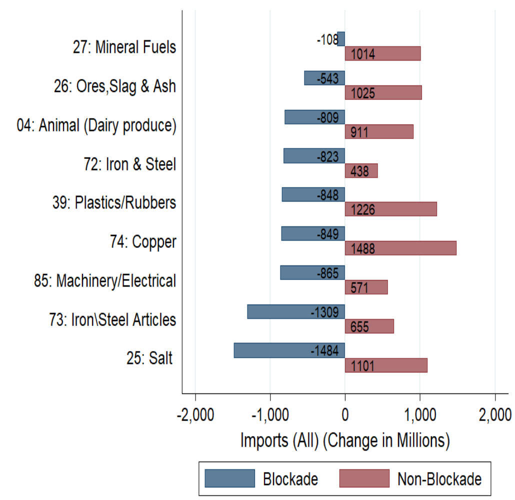
To estimate the effect of the embargo on bilateral trade between Qatar and the rest of the world, we obtain monthly data on exports and imports at the 2-digit HS level between Qatar and all available trade partners over the period 2015M1-2019M4 from Qatar Planning and Statistics Authority (2021). This source of data is very rich in that it also allows us to identify the mode of transportation (i.e. air, sea, land or pipeline), which is useful in that we focus part of our analysis on imports by air.[5]

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

[5]The ranking of top importing countries are presented in Figure B.4 of Appendix B.2. We also give more information on the trade data in Appendix B.2.

Figure 2.3: 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.

airspaces to obtain the distance before and after the embargo. To this end, we proceed in various steps. First, we identify one major airport for every country, except for the U.S. and Canada where we differentiate between the Eastern and Western coasts, as done by Feyrer (2021).[6] The major airport of a country is defined as the one with the highest number of routes, as listed in the routes and airports datasets from OpenFlights (2021), which include 67,663 routes between 3,321 airports across the globe as of June 2014. 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

[6]We follow Feyrer (2021) who uses the population distribution of 1970 (i.e. the mid-year in his sample) 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 shown in Tables B.7 and B.8 in Appendix B.3.

set of main airports. Further details of these procedures are provided in Appendix B.2. 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 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$^{\text{th}}$ 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$^{\text{th}}$ percentile of most affected trade partners (see Figure B.10 in Appendix B.2). 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 (see Table B.5 in Appendix B.2) 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.[7] 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).

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:

[7] See Table B.3 in Appendix B.2 for the list of countries and details on excluded countries.

$$X_{ijt} = \frac{y_{it}y_{jt}}{y_{wt}}(\frac{\tau_{ijt}}{P_{it}P_{jt}})^{1-\delta} \tag{2.1}$$

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 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 parameterization is standard for Poisson regression models (Cameron and Trivedi, 2001), to which we add a stochastic term ν_{jt} and a time dummy, $Post_t$, while we drop i since it is "fixed" to Qatar, as only the distances between Qatar and its trade partners are affected by the embargo. This leads to the following specification:

$$X_{jt} = exp[\beta_1 ln(Air_{jt}) + \pi_j + Post_t] \times \nu_{jt} \tag{2.2}$$

In order to estimate Equation (2.2), we collapse our sample period 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), excluding June 2017, the blockade month, because it is likely to capture many erratic adjustments to compensate for the sudden change in trade routes.[8] This approach is similar to Martincus and Blyde (2013), and allows us to rule out serial correlation as pointed out by Bertrand et al. (2004) for Difference-in-Difference

[8]Nonetheless, results are robust to the inclusion of June 2017. Coefficient estimates are only slightly lower in magnitude as shown in Table B.6 in Appendix B.3.

estimation. Aggregating over two symmetric periods also rules out possible seasonality effects.[9] Based on our identifying assumptions, we estimate the effect of an exogenous increase in air distance on trade. Our coefficient of interest, β_1, captures the effect of the increased transportation costs, represented by a rise in travel distance by air, on trade. Given the method of distance construction, all routes in our sample were 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 (i.e. see Figure B.12 in Appendix B.2) in air distance with non-blockading countries.

It is important to clarify a few simplifying assumptions used to move from Equation (2.1) to (2.2). First, a standard practice in the trade literature is to proxy for the bilateral trade costs term, 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, denoted Air_{jt}, although we will also consider other transport costs (by land and by sea) in Section 2.5. Second, Head and Mayer (2014) mention that modern practice has moved toward including importer (and exporter) fixed effects to control for the structural terms of income Y_{jt} in Equation (2.1). 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, the distance shock, Air_{jt}. Therefore, we follow Feyrer (2021), and assume that the distance shock is orthogonal to changes in income.[10] Country fixed effects would also account for the MRTs, assumed to be time-constant, 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 time variation in our air distance variable does not allow us to control for MRTs by including country-time

[9] We would obtain the same coefficients and standard errors when preserving the monthly and sectoral stratifications. This is because we are working with a balanced panel and the distance shock variable only varies at the country and time level. Thus, if we keep disaggregated data, we still measure the average effect across months, sectors and countries.

[10] Such assumption will be relaxed in the robustness checks in Section 2.5 where we include GDP as a control, which does not change the estimated coefficient of air transportation costs. We do not augment our main specification with GDP since it is likely to be endogenous (Disdier and Head, 2008).

fixed effects, as recommended by Yotov et al. (2016). Nevertheless, the short nature of the period of investigation and having the data collapsed into two periods should make the assumption of time-constant MRTs plausible. 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.

2.4 Results

Table 2.1 presents four specifications, employing total trade by air or imports only by air as dependent variables and including or not country fixed effects. In Column 1, our dependent variable is trade by air and the estimated coefficient of air transportation costs is positive and statistically significant at the 5% level. 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. Moving to Column 2, we include such fixed effects and identify the coefficient of interest by exploiting the variation of distance within the country pairs. As a result, our main coefficient of interest becomes negative and statistically significant at the 5% level. In particular, the elasticity of air transportation costs with respect to trade shows that a 1% increase in air transportation costs translates into a decline by -0.29% in trade by air. Once we control for country fixed effects, we take unobservable country factors other than transportation costs into account, and identify the effect of air transportation costs on trade within each country. The results also show an increasing trend of trade post-embargo. As pointed out previously, we exploit the allocation of trade to different non-blockading partners given the differences in distance shock but 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. The results for imports by air in Columns 3 and 4 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

Table 2.1: Main results (June 2016 to June 2018)

	(1)	(2)	(3)	(4)
Dep. var.	Trade (Air)		Imports (Air)	
Air Distance (log)	0.990**	-0.288**	0.978**	-0.285**
	(0.494)	(0.119)	(0.495)	(0.120)
Post	0.177***	0.341***	0.178***	0.341***
	(0.041)	(0.028)	(0.042)	(0.028)
Country FE	No	Yes	No	Yes
Observations	292	292	278	278
Pseudo-R2	0.0790	0.997	0.0785	0.997

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

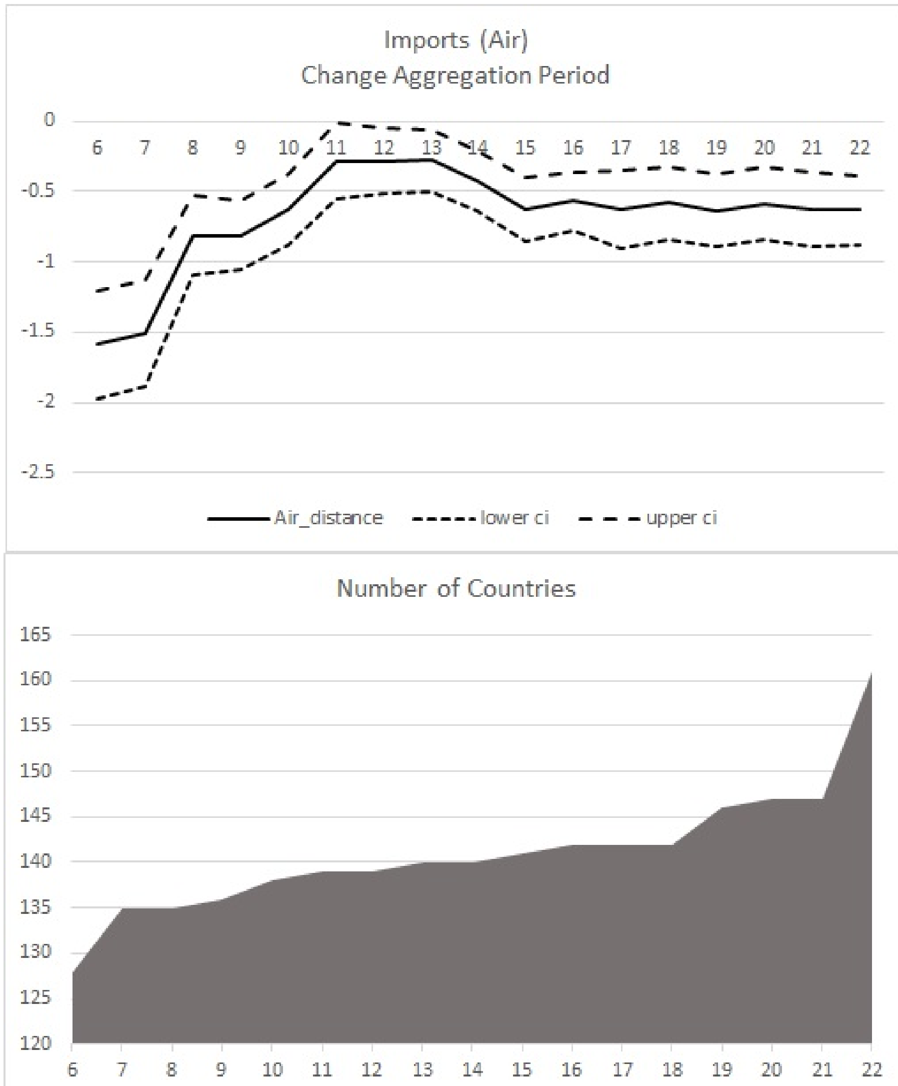
values are zero throughout the period of investigation.

Table 2.1 shows our main results based on a 12-month aggregation but it is important to assess how sensitive they are to this modelling choice. In Figure 2.4, we graphically illustrate the results when the estimation window goes from 6 to 22 months (i.e. the maximum given our data availability). Thus, we keep the blockade date at June 2017 and we re-estimate the specification from the last column of Table 2.1 from the shortest period of Dec 2016-Dec 2017 (6 months before and after) up to August 2015-April 2019 (22 months before and after).[11] We can see that the effect is always negative and statistically significant, and quite persistent even with a window of 22 months. The figure also illustrates a much larger effect when considering short windows, consistent with major disruptions in the short run. Such big effect immediately following the blockade is 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 shorter aggregation windows. The change in the estimation window 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 imports by air.

Our identification strategy strongly relies on the unexpected nature of the embargo, together with the absence of pre-existing confounding trends. To assess the plausibility

[11] A similar figure using trade as a dependent variable is found in Figure B.14 in Appendix B.3.

Figure 2.4: Change of aggregation period estimation (imports)



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

of these assumptions, we conduct a placebo test. Specifically, we assume the blockade to have happened in different periods prior to the actual date. In Figure 2.5, 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, the figure reproduces the coefficient of -0.285 from Table 2.1 for imports by air.[12] Interestingly, the effect is statistically insignificant when assuming that the blockade occurred in any of the previous 10 months, indicating that there was no anticipation effects[13] or other pre-existing confounding trends. It is the case that the effect is significant for placebo shocks earlier in 2016, which is probably due to the sharp fall in oil prices that took place between mid-2014 and early 2016. It was one of the largest in modern history (Stocker et al., 2018) and according to a report by Qatar Planning and Statistics Authority (2018), Qatar's trade between 2014-2016 declined substantially before recovering in 2017. Again, the bottom panel of the figure illustrates the different number of countries included in the estimations.

2.5 Robustness Checks

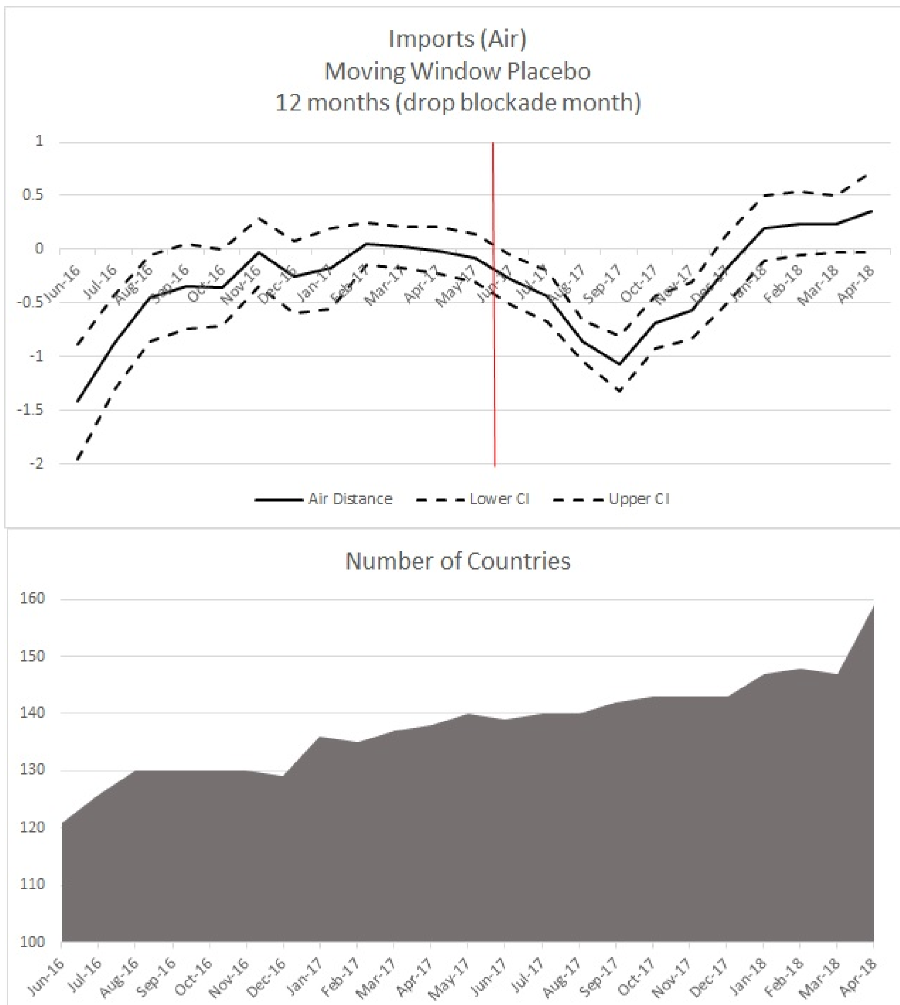
Beyond the concerns on anticipation and pre-existing trends that we have already addressed, we cannot exclude that other unobserved factors could affect the negative role of air transportation costs on trade. In the following, we summarize a series of robustness checks, with the full results reported in Appendix B.3. The point estimates of some of these checks are reported in Table 2.2 where Panel A reproduces the results from Table 2.1 for ease of comparability.

Construction of distance variable. The procedure we follow to calculate the pre- and post-embargo distances is necessarily based on some assumptions. Importantly, they do not seem to drive our results, which are robust to ignoring the distinction between the Eastern and Western coasts of the U.S. and Canada (Tables B.7 and

[12]Similar results are obtained with total trade by air in Figure B.15 in Appendix B.3.

[13]Using the GEDLT-Global Data on Events, Location and Tone- project, we also provide additional evidence of a lack of anticipation in Appendix B.1.

Figure 2.5: Placebo estimation (imports)



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

B.8) or only using the direct flights to proxy for distance (Tables B.9). The use of only one airport per country may be particularly restrictive for large countries. However, controlling for country area with an interaction with Post does not affect our conclusions, as shown in Panel B of Table 2.2: our coefficient of interest increases in magnitude but remains negative and statistically significant.

Substitution across transportation modes. Given that the blockade disrupted land and sea routes as well, restrictions on other modes of transport 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 the 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 C of Table 2.2. When controlling for changes associated with land and sea pre-embargo trade shares, the air transportation costs elasticity to trade raises in magnitude to -0.5%, suggesting important substitution effects.[14]

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 shock variable. To assess this possibility, we run a series of falsification checks. First, we replicate the

[14]Similar to all results presented in Table 2.2, detailed results are presented in Appendix B.3. Given the change in magnitude in the estimated elasticities observed in Panel C of Table 2.2, detailed tables provide results for specifications augmented or not with interaction terms of the share of trade/imports by land and by sea prior to the embargo with post-embargo period indicator.

main results in Panel D of Table 2.2 but using non-air trade as a dependent variable. As expected, the shock to air distance does not directly affect trade by other modes (sea, land or pipeline). Similarly, the hydrocarbon sector should be unaffected by our shock since oil and natural gas are mainly exported by pipeline or ships. In Panel E of Table 2.2, we limit the dependent variable to include trade and exports of HS2 item 27, referring to mineral fuels, oils, distillation products, etc. As expected, the estimated coefficients are statistically insignificant, thus providing further evidence in support of our causal estimates being driven solely by the change in air transportation costs.

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 check for that, we control for real GDP in Panel F of Table 2.2 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 shock would grow much faster than others, for other reasons. To explore this possibility, in Panel G of Table 2.2 we control for pre-existing trend based on initial GDP. 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 post indicator and pre-embargo total imports or being a top importer in the top 10^{th} percentile prior to the embargo. Results presented in Panels H and I of Table 2.2 are unaltered.

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).[15] To assess the sensitivity of our results, we replicate the main results in Panel

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

Table 2.2: Robustness Table

	Trade (Air)	Obs	Import (Air)	Obs
A. Main results	-0.288**	292	-0.285**	278
	(0.119)		(0.120)	
B. Control for country log(Area) interacted with Post	-0.493***	290	-0.493***	276
	(0.147)		(0.148)	
C. Control for trade/imports shares by other modes	-0.502**	292	-0.502**	278
	(0.211)		(0.197)	
	Trade (Non-Air)		Import (Non-Air)	
D. Falsification (Non-Air)	-0.074	314	-0.231	266
	(0.525)		(0.651)	
	Trade (All)		Export (All)	
E. Falsification (Hydrocarbon)	0.321	154	0.393	98
	(0.800)		(0.814)	
F. Control for log(GDP)	-0.277**	288	-0.273**	274
	(0.116)		(0.117)	
G. Control for pre-embargo log(GDP) interacted with Post	-0.339**	274	-0.338**	260
	(0.154)		(0.156)	
H. Control for pre-embargo trade/imports interacted with Post	-0.388*	292	-0.385*	278
	(0.231)		(0.233)	
I. Control for pre-embargo top importers interacted with Post	-0.316***	292	-0.313***	278
	(0.108)		(0.110)	
J. Drop the food sector	-0.373***	288	-0.370***	274
	(0.100)		(0.102)	
K. Drop strategic partners	-0.325***	284	-0.321***	270
	(0.105)		(0.107)	

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 available in Appendix B.3.

J of Table 2.2 dropping the food-related sectors.[16] Our main coefficient estimate only increases slightly in magnitude. 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 K of Table 2.2. Again, the estimates of our coefficients of interest are very similar to our main results.

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 moves have an effect on our coefficient estimates by excluding these countries from our analysis. The results, shown in Tables B.20 to B.22 in Appendix B.3, are not sensitive to excluding these trade partners.[17]

2.6 Conclusion

Transportation costs play 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 transportation costs 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 an air transportation

[16]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.

[17]We report results by including and excluding Senegal since their move seems to have been abrupt because Aljazeera (2021b) mentions they later re-established their ties with Qatar.

costs elasticity of trade of about -0.3. The elasticity amplifies to -0.5 when considering possible substitution effects with other transportation modes. 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).

Notwithstanding the advantages of our framework, it is also important to recognize its limitations. First, we can only quantify the transportation costs 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 suggest our estimations are on the upper range of the possible transportation costs elasticities. 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 special 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 air transportation costs that it would overturn our main findings.

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Appendix B

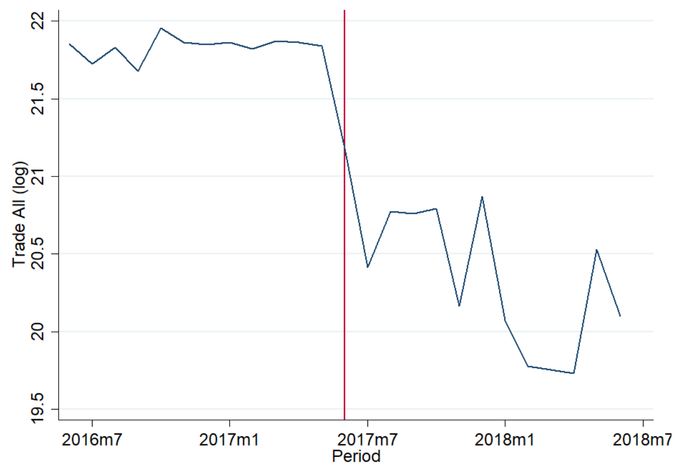
Chapter 2 Appendix

B.1 Additional Information on the Embargo

Two weeks before the embargo, Qatar 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 (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. Trade with blockading countries collapsed immediately (Figure B.1), driven by imports (Panel (a) of Figure 2.2), not exports (Panel (b) of Figure 2.2) which are concentrated in the mineral fuels sector (see Figure B.2) which was exempted from the blockade. 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 liquefied natural gas 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).

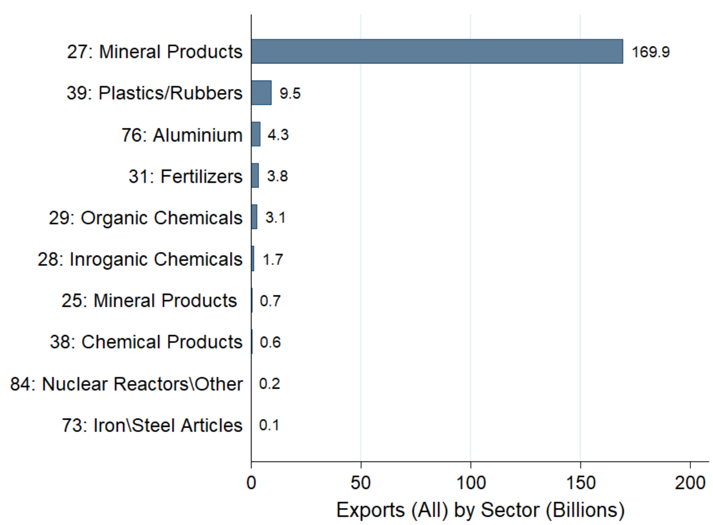
To further explore the lack of anticipation, we use the GDELT-Global Data on

Figure B.1: Trade between Qatar and blockading countries



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

Figure B.2: Qatar's top exported sectors



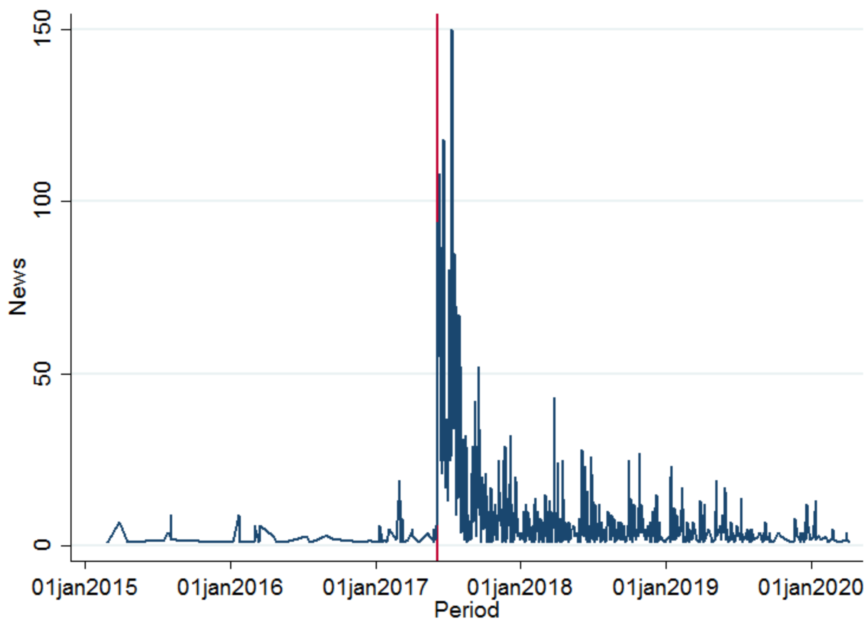
Notes: Exports by sector in top 10th percentile over the pre-embargo period Jun2016-May2017.
Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

Events, Location and Tone- project (Leetaru and Schrodt, 2013).[1] We focus on events categorized as “Impose embargo, boycott or sanctions” (CAMEO Code 163). We examine all the events in this category between January 2015 and April 2020 for which the target actor is Qatar. In total, there are 6,876 news on embargo where the target country is Qatar over the sampled period. In Figure B.3, we plot the embargo news from January 2015 to April 2020.[2] We do observe a huge spike of news related to an embargo on Qatar on and after the date of the embargo denoted by the red 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. They are in fact related to the post-embargo period. Six of the records are related to a threat of or an actual boycott by U.S. on Qatar or some of its individuals for reasons not related to the 2017 embargo. One record is a call by a U.S. association for a boycott of Qatar Airways for claimed human rights violations. The full list of news links in the pre-embargo period in 2017 are provided in Table B.1. Overall, we do not have any indication that the embargo was anticipated. Therefore, it can provide an exogenous shock on travel costs and allow us to identify the impact of air transportation costs on trade.

[1] GEDLT 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), which makes 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 (Manacorda and Tesei, 2020). The events and actors are based on the Conflict And Mediation Event Observations (CAMEO), a coding system that provides a systematic classification to study different types of international interactions (Schrodt, 2012). The CAMEO coding system includes a list of 15,000 events.

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

Figure B.3: News about the embargo



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

Table B.1: Pre-embargo, 2017, GDELT News

20170109	http://www.newsofbahrain.com/viewNews.php?ppId=41024&pid=21&MNU=
20170110	https://tinyurl.com/yb5fu8z9
20170111	http://www.middleeasteye.net/news/security-forces-shut-down-al-jazeera-office-yemens-taiz-466586445
20170126	http://dailynht.com/story/39677
20170126	http://gulfnewsjournal.com/stories/511076556-human-rights-group-calls-for-boycott-of-qatar-airways
20170128	http://axisoflogic.com/artman/publish/Article_78680.shtml
20170130	https://tinyurl.com/ybw8lrr2
20170205	https://www.alaraby.co.uk/english/news/2018/2/4/anti-doha-bloc-plotted-invasion-says-qatar-defence-minister
20170223	http://themoderatevoice.com/kushner-may-member-lucky-sperm-club-gig-just/
20170302	http://www.qatar-tribune.com/news-details/id/114279
20170302	https://tinyurl.com/ycfrhhua
20170302	https://www.newsmax.com/politics/wto-trade-war-trump-tariffs/2018/03/02/id/846448/
20170303	http://www.hellenicshippingnews.com/wto-chief-makes-rare-warning-of-trade-war-over-u-s-tariff-plan/
20170303	https://thefrontierpost.com/saudi-crown-prince-visit-egypt-official-airport-sources/
20170303	https://www.thenational.ae/world/mena/saudi-crown-prince-to-visit-egypt-ahead-of-us-britain-tour-1.709707
20170305	http://www.thedailystar.net/business/wto-chief-makes-rare-warning-trade-war-over-us-tariff-plan-1543195
20170305	https://tinyurl.com/yc497fyg
20170305	http://www.taipeitimes.com/News/world/archives/2018/03/06/2003688787
20170305	http://www.thedailystar.net/business/wto-chief-makes-rare-warning-trade-war-over-us-tariff-plan-1543195
20170309	https://www.middleeastmonitor.com/20180309-uae-intercepted-and-hijacked-qatar-fishing-boat/
20170309	http://www.nj.com/opinion/index.ssf/2018/03/when_your_very_loving_levies_start_a_global_trade.html
20170323	https://newsblaze.com/world/middle-east/bahrain-blasts-tehran-machinations_129100/
20170404	https://www.aljazeera.com/news/2018/04/gulf-summit-postponed-september-180404090905924.html
20170404	https://en.trend.az/world/us/2882306.html
20170405	https://tinyurl.com/ycqfawej
20170513	https://gulfnews.com/news/gulf/qatar/iran-qatar-hold-economic-committee-meeting-1.2220883
20170524	http://www.albawabhnews.com/2541683
20170525	https://www.brookings.edu/blog/markaz/2017/06/01/does-the-road-to-stability-in-libya-pass-through-cairo/
20170527	https://tinyurl.com/ybo2hu2a
20170529	http://www.ecfr.eu/article/commentary_does_the_road_to_stability_in_libya_pass_through_cairo_7294
20170531	http://www.emaratalyoum.com/politics/reports-and-translation/2017-06-01-1.1000251
20170603	http://www.presstv.ir/Detail/2017/07/03/527284/Press-TV-News-Headlines
20170603	http://www.thesundayleader.lk/2017/06/11/did-donald-trump-spark-off-the-me-diplomatic-crisis/
20170604	http://www.arabiansupplychain.com/article-13291-maersk-line-reroutes-qatar-cargo-through-salalah/
20170604	https://www.thenational.ae/world/gcc/qatar-crisis-one-year-on-what-s-changed-1.736873

Notes: All records represent coding error, exceptions are in bold. Broken news links are not included in the list.

B.2 Data

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 U.S. Dollar, so exports in foreign currencies are converted using the official exchange rate of 3.64 QAR for one USD.

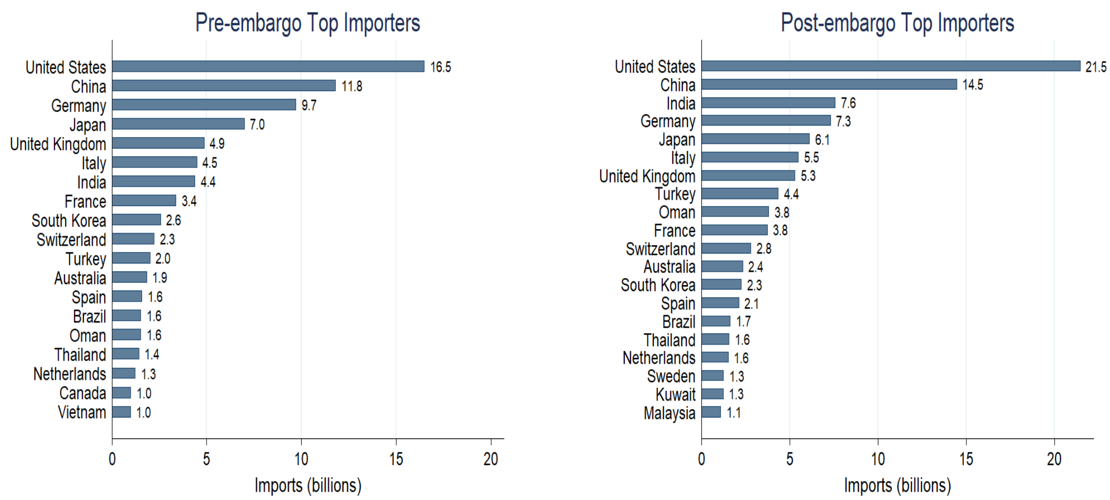
Main Qatar's trade partners (in terms of imports) are the U.S. and China in the pre- and post-embargo period (Figure B.4). Overall, there was no major shift in top trading partners as a result of the embargo. However, countries like Kuwait, Oman and Turkey increased in ranking following the blockade 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 (Table B.2).

Distance

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, n.d.). 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

Figure B.4: Top importers to Qatar



(a) (b)

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

Table B.2: Top 20 HS-2 trade categories by air

HS Code	Description	Air import Value (Millions QAR)	Percent by Air
84	Nuclear reactors, boilers, machinery & mechanical appliances	36906.55	46.27%
88	Aircraft, spacecraft, & parts thereof.	32903.58	96.50%
71	Natural or cultured pearls, precious metals, imitation jewelery, coin.	11028.65	98.41%
90	Optical, photographic, cinematographic, measuring, checking, precision, medical or surgical instruments & accessories	7078.387	75.97%
30	Pharmaceutical products	6801.655	77.50%
62	Articles of apparel, accessories, not knit or crochet	3812.957	59.18%
91	Clocks and watches and parts thereof	3546.108	93.80%
07	Edible vegetables and certain roots and tubers.	1891.492	49.75%
42	Articles of leather, animal gut, harness, travel good.	1601.333	62.33%
01	Live animals	1562.149	46.55%
97	Works of art, collectors' pieces, and antiques	1460.727	87.89%
49	Printed books, newspapers, pictures & other products of the printing industry	704.4123	68.97%
06	Live trees and other plants	368.5307	72.788%
58	Special woven fabrics	81.83209	48.10%
52	Cotton	50.16529	46.51%
50	Silk	38.94104	85.66%
98	Personal Effects & used Appliances	31.47394	58.04%
43	Furskins and artificial fur; manufactures thereof.	26.20672	81.23%
41	Raw hides & skins (other than furskins) & leather.	20.60845	80.75%
51	Wool, fine or coarse animal hair; horsehair yarn & woven fabric.	19.72078	54.81%

Source: Authors' calculations using data from Qatar Planning and Statistics Authority. Imports data cover the period: Jan2015-Apr2019.

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).

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.

To identify the main airport of each country and its location, we used the routes and airports datasets from OpenFlights (2021). The routes dataset includes 67,663 routes between 3,321 airports across the globe corresponding to 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 by destination. It also gives data on the 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 International Air Transport Association (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.[3]

For each country, we define the major airport as the one with the highest number of routes. Since routes are split by source (arrivals) and destination (departures), we check both and find that it gives the same airport in each for most countries. The exceptions are Iran which gives a different airport by source and destination, we choose the one with the highest number of routes (Mashhad International Airport). For Syria, we have multiple airports because maximum flight for each is one, we choose the airport in the capital city (Damascus International Airport). Also, Swaziland has different airports by source and destination with one route each, we choose the airport in the Capital City (King Mswati III International Airport).

In total, we have 144 countries listed in Table B.3. 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. The exception to this are the United States and Canada. In line with Feyrer (2019), we differentiate the distance to the Eastern and Western coasts. The two airports used for the U.S. are Hartsfield-Jackson Atlanta International Airport and Los Angeles International Airport, the first and third busiest airports based on the routes datasets. We do not use the second busiest airport (Chicago O'Hare International Airport) since it is in the same coast as the first. As for Canada, the first and third busiest airports are Lester B. Pearson International Airport and Vancouver International Airport. Similar to the U.S., the second busiest airport (Montreal / Pierre Elliott Trudeau International Airport) is on the same coast as the first.

[3]Specifically, the latitudes and longitudes for Doha and Hamad International Airports are 25.2647° N 51.5596° E and 25.2609° N 51.6138° E, respectively.

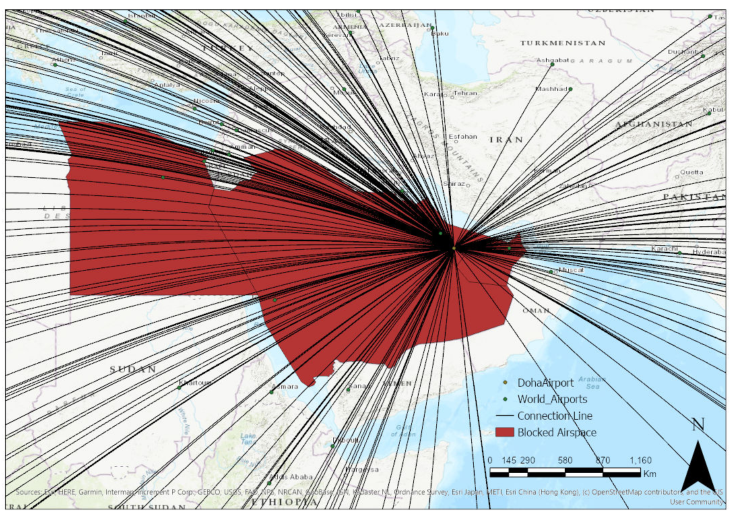
Table B.3: List of countries

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).

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. It is more accurate compared to the great circle distance that assumes a perfectly spherical earth (Campante and Yanagizawa-Drott, 2018). This is not the actual flight distance, but using 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, shown in Figure B.5. The geodesic 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, n.d.).

Figure B.5: 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, but ignoring the directionality dimension. Specifically, we transform all Doha destination as Doha source in the routes dataset and merge it with a created *Stata* file that includes the distance between Doha and all main airports from GIS computation. We assume that all cases for which we have distance but not routes are indirect routes since all cases from the routes dataset were stated to be direct. In our flight sample, we have a total of 176 indirect flights and 59 direct flights. Campante and Yanagizawa-Drott (2018) use a 6000 miles cutoff to differentiate direct from indirect flights. However, they point out that this is unlikely to hold given the stricter measures proposed in 2014 by the U.S. and European regulators regarding flight time limits. Our criteria to distinguish direct and indirect flights match with Qatar Civil Aviation Authority (CAA)[4] data for 159 indirect routes and 54 direct routes. Exceptions consist of 19 routes that we record as indirect while CAA records 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.

- 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. Using *Stata*, we choose the route that gives the minimum total distance. For example, going from Doha to ABJ (main airport in Ivory Coast), the whole list of direct-route airports will be taken as a potential intermediate airport as shown in Figure B.6. The first column denotes the potential intermediate airport. The second column refers to intermediate distance, which is the distance between Doha and the intermediate airport. The third column denotes the final airport we are trying to compute distance for. The final distance in the last column is the distance between the intermediate airport and final airport. Then, we choose the route that gives us the minimum total distance (intermediate distance + final distance) that will be taken as the distance for this indirect route.

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

Figure B.6: Indirect distance demonstration

	interport	interdista~e	finalport	finaldista~e
1	DEL	2558.0591	ABJ	8861.071
2	FAE	5943.6759	ABJ	6308.3375
3	AXA	11467.462	ABJ	6574.3797
4	TUN	4113.554	ABJ	3784.1309
5	GBE	6165.3444	ABJ	4619.3561
6	GUA	13990.458	ABJ	9506.7442
7	CKY	7074.2258	ABJ	1170.6617
8	PRG	4219.5393	ABJ	5254.2351
9	DAC	3923.7266	ABJ	10219.934
10	MBJ	12715.535	ABJ	8144.369
11	DYU	2191.6299	ABJ	8149.5204
12	OUA	5736.9901	ABJ	828.04439
13	ATH	2957.8759	ABJ	4589.54
14	FCO	4040.8224	ABJ	4354.1027
15	OSL	4958.8873	ABJ	6225.5305
16	IOM	5582.5595	ABJ	5413.7661
17	KBP	3301.776	ABJ	5959.1658
18	HKG	6305.5906	ABJ	12611.775
19	KTM	3369.9377	ABJ	9675.3963
20	FNJ	6962.4585	ABJ	12878.145
21	HAH	4168.7844	ABJ	5546.8569
22	YYZ	10897.701	ABJ	8458.7629
23	MTS	6129.6486	ABJ	5176.0916
24	IST	2731.9785	ABJ	5125.9327

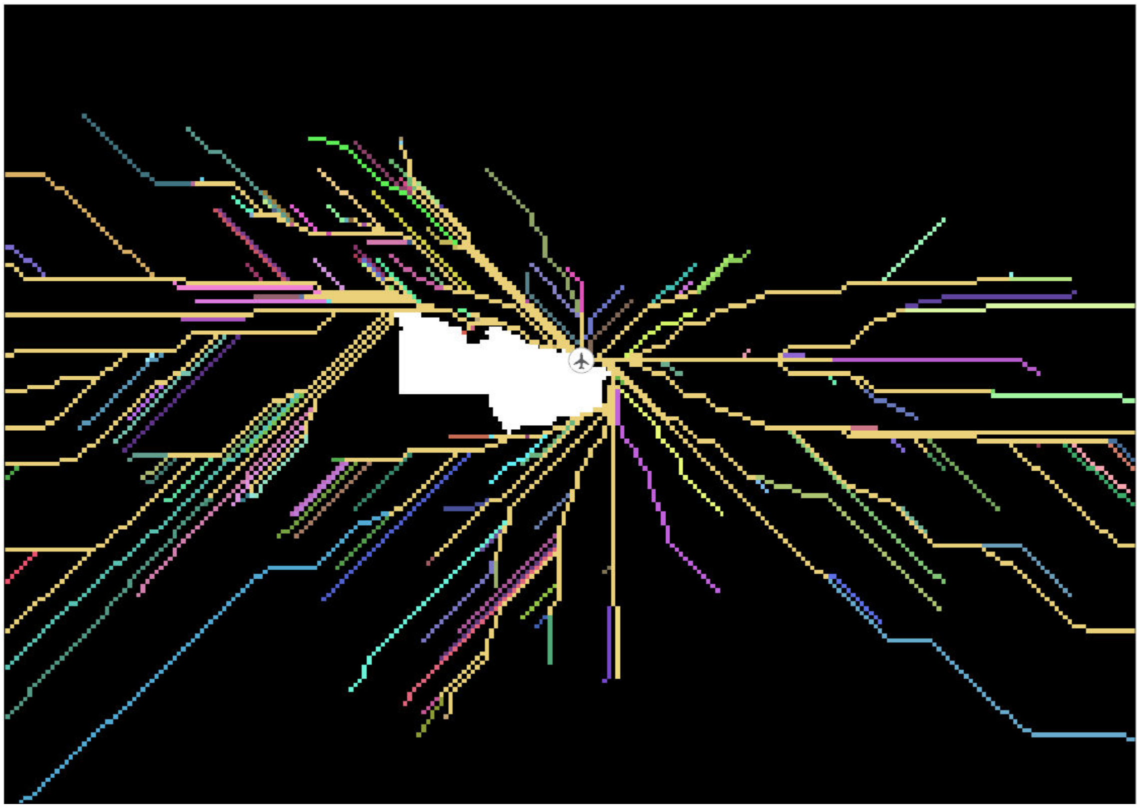
Notes: Demonstration of indirect route distance computation using a screen shot of a *stata* data editor.
Source: Authors' calculations using Qatar Planning and Statistics Authority Data.

We compute the post-blockade distance using *ArcGIS Pro* and *ArcMap*.[5] The main tool for this purpose is the *'least cost tool'*, it allows us to compute the geodesic distance from Doha to all airports by avoiding the blocked airspace. The FIR of blockading countries were obtained from (ICAOGIS, 2015). The *'least cost tool'*, however, can only be applied to raster data where everything is represented by pixels. This is opposed to the pre-blockade distance tool that uses vector data where representation is a combination of points, lines or polygons. As a result, our initial post-embargo distance computation creates pixels connecting Doha to other airports as shown in Figure B.7. The pixel lines are going from an imaginary Doha airport outside the blocked airspace, denoted by an airplane symbol, to all sampled airports around the world without going

[5] *ArcGIS Pro* and *Arcmap* have similar functionalities, the difference is that *ArcGIS 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*.

over the blocked airspace.

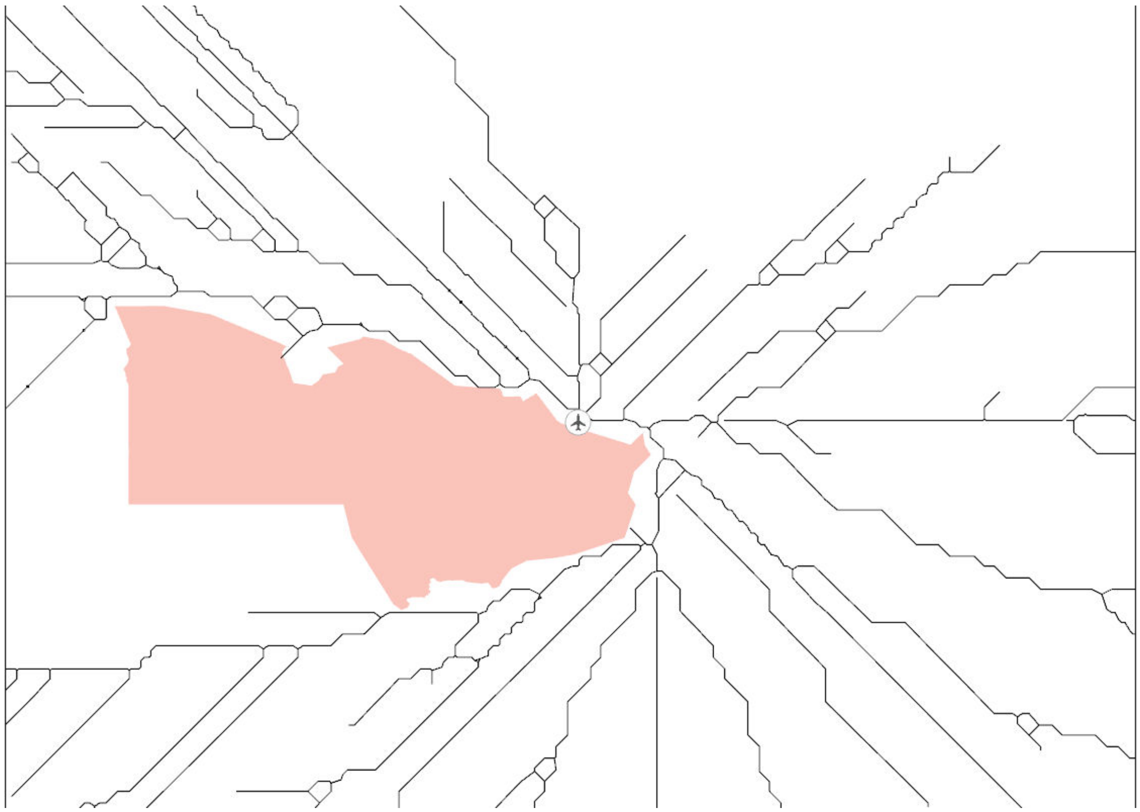
Figure B.7: Post-embargo distance demonstration



Notes: Raster representation of post-embargo distance using a screen shot of *ArcGIS Pro* software.

We then converted this raster format into a vector format to have proper lines that allow us to calculate the distance as shown in Figure B.8.

Figure B.8: Vector representation of post-embargo distance



Notes: Vector representation of post-embargo distance using a screen shot of *ArcGIS Pro* software.

This conversion from one data format to another affects accuracy and introduces error. However, to our knowledge there was no other option. Moreover, using the *'least cost tool'* for the pre-embargo distance computation would not allow us to differentiate direct from indirect routes. This is because the *'least cost tool'* computation requires going over every line one by one to define the distance. This is feasible for 195 lines but not for 11,800 lines of every direct-indirect airport pair. As a result, we had to give up some consistency by using two different tools. Since we had to go over each line in Figure B.8, we chose the closest route when there were multiple possible routes to one destination. For instance, Figure B.9 shows the traced route from Doha to Turkey.

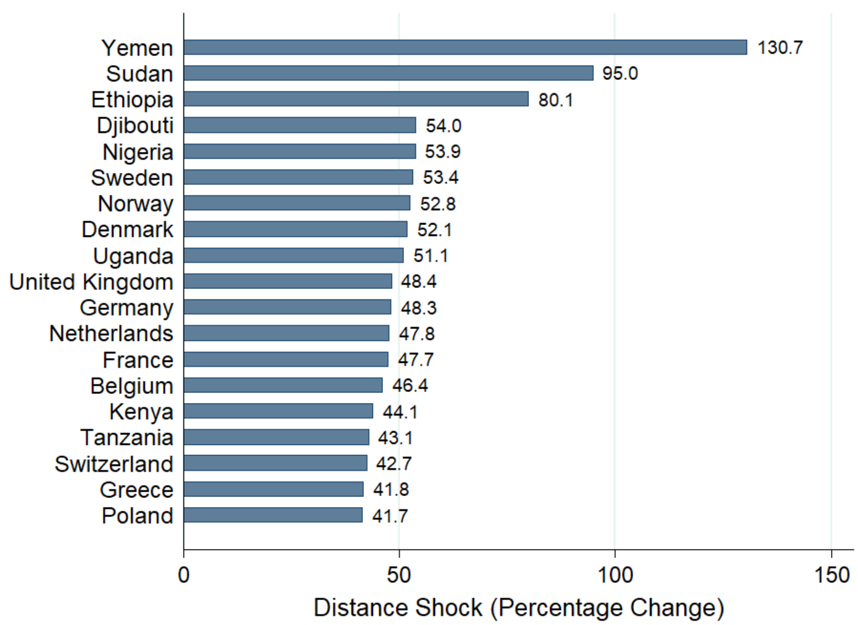
Figure B.9: Sample path post-embargo between Doha and Turkey

Notes: Post-embargo route from Doha to Turkey using a screen shot of *ArcGIS Pro* software.

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 airspace. Both tools generate the distance in Meters, we convert it to Kilometers in our final dataset for better representation in descriptive statistics.

We then obtain a proxy for the changes in transportation shocks for each country, as depicted in Figure B.10 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. Our criteria of differentiating direct versus indirect flights using openflights database matches CAA except for 19 routes that we specify as indirect although CAA reports to be direct as mentioned earlier. The pairwise correlation between time and distance for CAA direct flights is 0.4659. Moreover, if we limit it to countries with a time shock in the top 25$^{\text{th}}$ percentile, the correlation rises to 0.7202. Therefore, this strong correlation points to a relatively good quality of our data in approximating change in transport costs. Figure B.11 displays the list of countries with direct flights with a time shock in the top 25$^{\text{th}}$ percentile.

Figure B.10: Highest distance shock countries

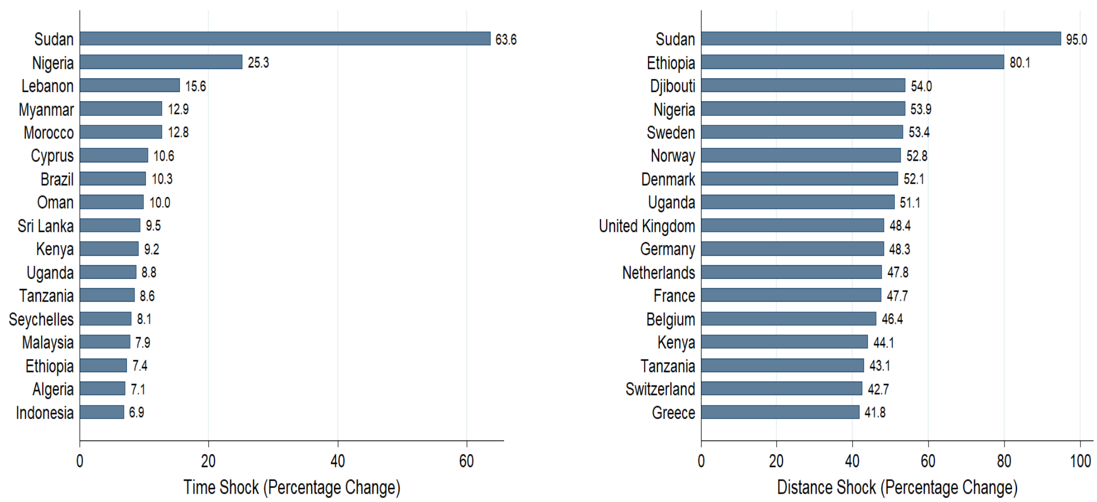


Notes: Countries with distance shock in the top 10$^{\text{th}}$ percentile.
Source: Authors' calculations using Geocoded Data.

Data Exploration

Tables B.4 reports the summary statistics of our main variables over the estimation period. Table B.5 further splits these descriptive statistics by pre-embargo and post-embargo periods. Qatar's trade and imports by air with non-blockading countries

Figure B.11: Time versus distance shock



(a) (b)

Note: Panel (a): Countries with time shock in the top 25^{th} percentile (CAA direct routes only). Panel (b): Countries with distance shock in the top 25^{th} percentile (CAA direct routes only). Source: Authors' calculations using Qatar Civil Aviation Authority Data and Geocoded Data.

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.4), there was quite a lot of variation in terms of distance shocks among the top importers (Panel (a) of Figure B.12). However, there is no clear match that a rise in distance is associated with imports falling (Panel (b) of Figure B.12). This stresses out the importance of controlling for country fixed effects in our econometric model to identify the role of air transportation costs.

Similarly, correlations over time can be informative. In Figure B.13, 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.4: Summary statistics

Summary Statistics					
	Mean	SD	Min.	Max.	Observations
Trade (Air)	0.24	1.17	0	15	274
Imports (Air)	0.24	1.17	0	15	274
Exports (Air)	0.00	0.00	0	0	274
Air Distance	6489.80	3878.34	566	14977	274

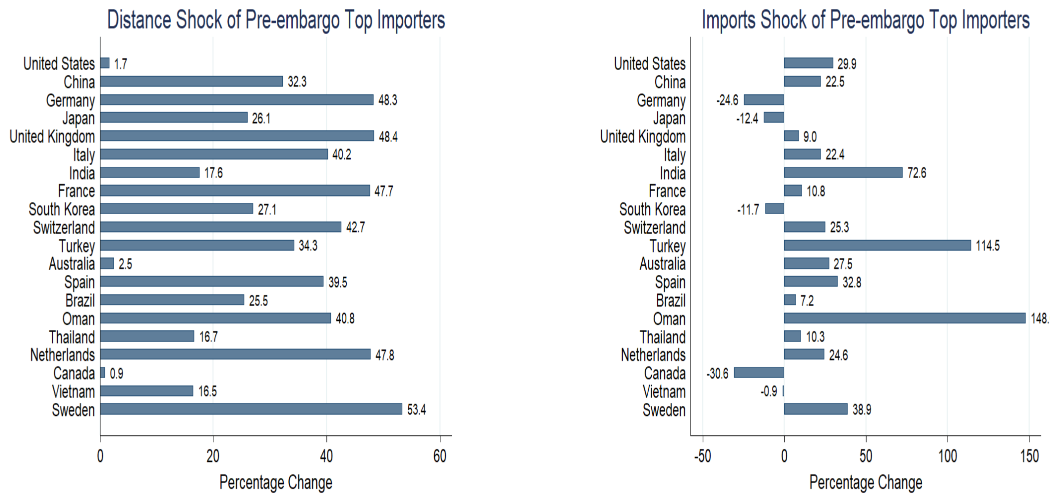
Notes: Summary statistics are reported for non-blockading countries sample over the period Jun2016-Jun2018, excluding blockade month, June 2017. Trade values in millions of Qatari Riyals and distance values in Kilometers. Minimum trade values are zeros since we are using a balanced panel.

Table B.5: Summary statistics, pre- versus post-embargo

Summary statistics										
	Pre Embargo					Post Embargo				
	Mean	SD	Min.	Max.	Obs.	Mean	SD	Min.	Max.	Obs.
Trade (Air)	0.21	0.98	0	11	137	0.28	1.34	0	15	137
Imports (Air)	0.21	0.98	0	11	137	0.28	1.34	0	15	137
Exports (Air)	0.00	0.00	0	0	137	0.00	0.00	0	0	137
Air Distance	6113.93	3897.22	566	14538	137	6865.67	3836.76	715	14977	137

Notes: Summary statistics are reported for non-blockading countries sample over the period Jun2016-Jun2018, excluding the blockade month, June 2017. Trade values in millions of Qatari Riyals and distance values in Kilometers. Minimum trade values are zeros since we are using a balanced panel.

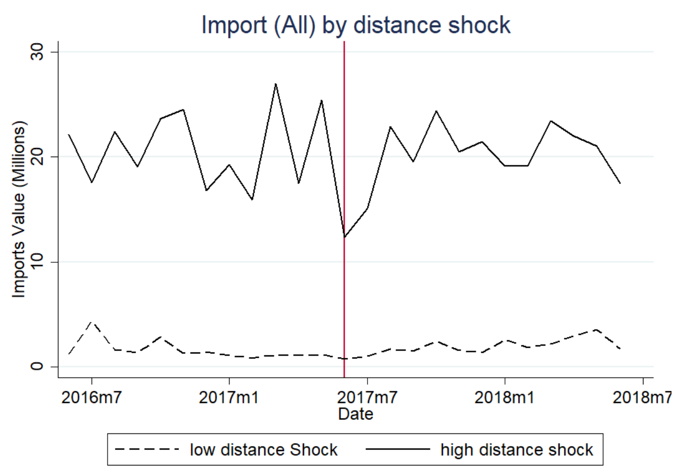
Figure B.12: Top importers distance versus imports shock



(a) (b)

Notes: Panel (a) Percentage change of distance for importers (top 10$^{\text{th}}$ 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$^{\text{th}}$ 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.

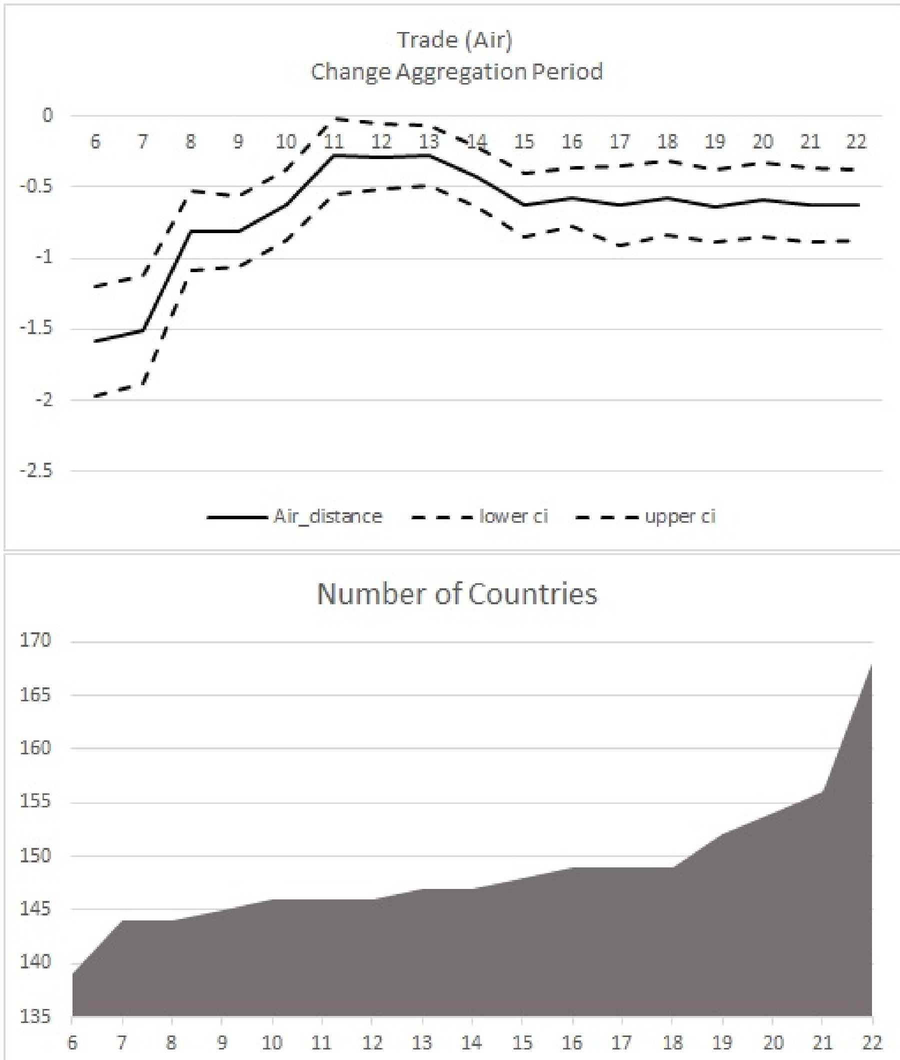
Figure B.13: Trend of imports by distance shock



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.

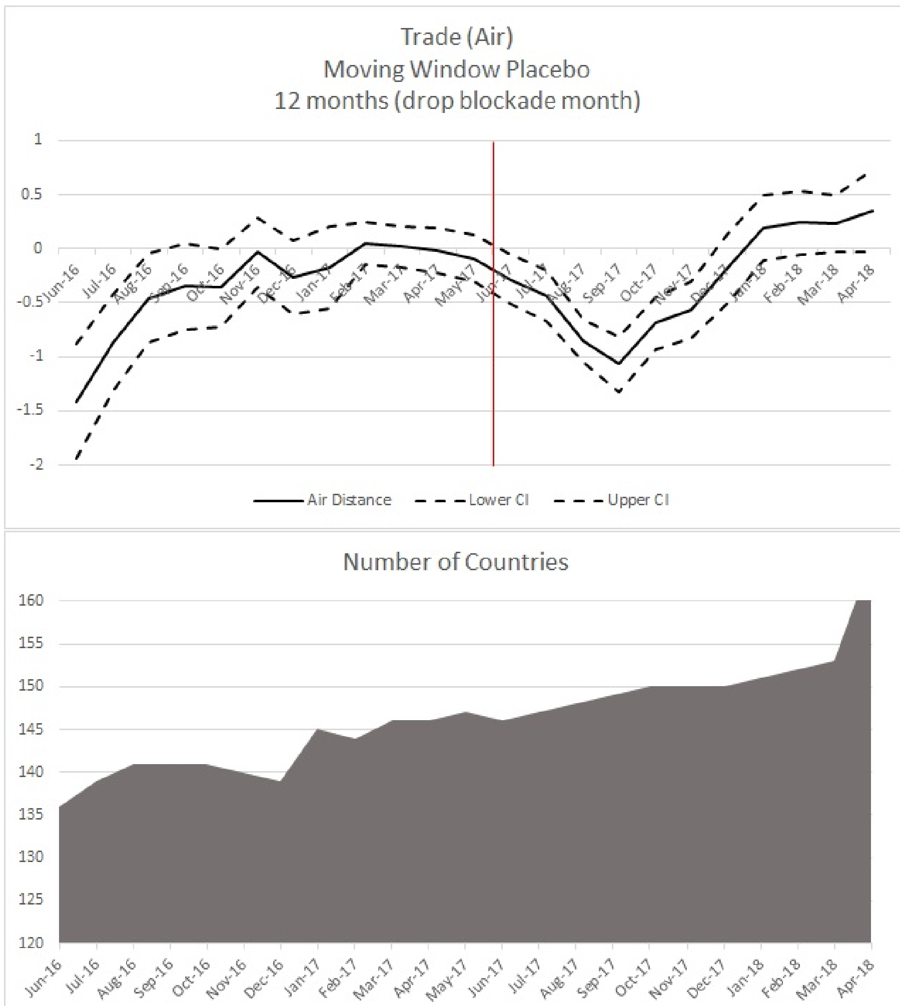
B.3 Extra Robustness

Figure B.14: 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 B.15: 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.

Table B.6: Keep blockade month

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.218*	-0.464**	-0.215*	-0.457**
	(0.121)	(0.207)	(0.123)	(0.192)
Post	0.293***	0.184***	0.293***	0.185***
	(0.028)	(0.058)	(0.028)	(0.052)
Pre-embargo sea share*Post		0.002		0.003*
		(0.002)		(0.002)
Pre-embargo land share*Post		0.001		0.003
		(0.002)		(0.004)
Constant	16.294***	18.465***	16.266***	18.407***
	(1.079)	(1.839)	(1.091)	(1.705)
Observations	294	294	280	280
Pseudo R2	0.997	0.998	0.997	0.997
Country FE	Yes	Yes	Yes	Yes
Observations	294	294	280	280

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

Table B.7: U.S./Canada full trade with Eastern Coast

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.281**	-0.496**	-0.278**	-0.496**
	(0.122)	(0.217)	(0.123)	(0.204)
Post	0.339***	0.242***	0.339***	0.241***
	(0.030)	(0.063)	(0.030)	(0.056)
Pre-embargo sea share*Post		0.002		0.003
		(0.002)		(0.002)
Pre-embargo land share*Post		0.001		0.003
		(0.002)		(0.004)
Constant	17.054***	18.957***	17.026***	18.956***
	(1.080)	(1.918)	(1.092)	(1.806)
Observations	288	288	274	274
Pseudo R2	0.998	0.998	0.997	0.998
Country FE	Yes	Yes	Yes	Yes
Observations	288	288	274	274

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

Table B.8: U.S./Canada full trade with Western Coast

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.287**	-0.501**	-0.284**	-0.502**
	(0.123)	(0.218)	(0.124)	(0.205)
Post	0.341***	0.244***	0.341***	0.244***
	(0.030)	(0.062)	(0.030)	(0.056)
Pre-embargo sea share*Post		0.002		0.003
		(0.002)		(0.002)
Pre-embargo land share*Post		0.001		0.003
		(0.002)		(0.004)
Constant	17.116***	19.022***	17.088***	19.025***
	(1.097)	(1.935)	(1.110)	(1.822)
Observations	288	288	274	274
Pseudo R2	0.998	0.998	0.997	0.998
Country FE	Yes	Yes	Yes	Yes
Observations	288	288	274	274

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

Table B.9: Countries with direct CAA routes only

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.350***	-0.657***	-0.347***	-0.622***
	(0.116)	(0.226)	(0.117)	(0.193)
Post	0.362***	0.239***	0.362***	0.264***
	(0.028)	(0.061)	(0.028)	(0.049)
Pre-embargo sea share*Post		0.004		0.004*
		(0.003)		(0.002)
Pre-embargo land share*Post		-0.000		0.001
		(0.002)		(0.005)
Constant	17.595***	20.311***	17.567***	19.998***
	(1.032)	(2.005)	(1.044)	(1.709)
Observations	134	134	132	132
Pseudo R2	0.997	0.998	0.997	0.998
Country FE	Yes	Yes	Yes	Yes
Observations	134	134	132	132

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

Table B.10: Control for log(Area)*Post

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.493***	-0.753***	-0.493***	-0.763***
	(0.147)	(0.194)	(0.148)	(0.181)
Post	0.784***	0.742***	0.790***	0.762***
	(0.201)	(0.222)	(0.200)	(0.238)
Area (log)*Post	-0.029**	-0.033**	-0.029**	-0.034**
	(0.013)	(0.013)	(0.013)	(0.015)
Pre-embargo sea share*Post		0.002		0.003
		(0.002)		(0.002)
Pre-embargo land share*Post		0.001		0.004
		(0.002)		(0.003)
Constant	18.737***	21.035***	18.734***	21.124***
	(1.303)	(1.711)	(1.313)	(1.596)
Observations	292	292	278	278
Pseudo R2	0.997	0.998	0.997	0.998
Country FE	Yes	Yes	Yes	Yes
Observations	292	292	278	278

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

Table B.11: Control for pre-embargo trade & import share by land and sea (interacted with post)

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.288**	-0.502**	-0.285**	-0.502**
	(0.119)	(0.211)	(0.120)	(0.197)
Post	0.341***	0.245***	0.341***	0.244***
	(0.028)	(0.060)	(0.028)	(0.053)
Pre-embargo sea share*Post		0.002		0.003
		(0.002)		(0.002)
Pre-embargo land share*Post		0.001		0.003
		(0.002)		(0.004)
Constant	16.918***	18.815***	16.890***	18.819***
	(1.055)	(1.868)	(1.067)	(1.742)
Observations	292	292	278	278
Pseudo R2	0.997	0.998	0.997	0.997
Country FE	Yes	Yes	Yes	Yes
Observations	292	292	278	278

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

Table B.12: Falsification (Non-Air)

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Non-Air)		Imports (Non-Air)	
Air Distance (log)	-0.074	0.071	-0.231	-0.330
	(0.525)	(0.440)	(0.651)	(0.539)
Post	0.258**	0.244	0.144	-0.173
	(0.114)	(0.243)	(0.146)	(0.167)
Pre-embargo sea share*Post		-0.003		0.002
		(0.005)		(0.003)
Pre-embargo land share*Post		0.003		1.371**
		(0.004)		(0.556)
Constant	16.893***	15.652***	16.405***	17.243***
	(4.513)	(3.782)	(5.540)	(4.581)
Observations	314	314	266	266
Pseudo R2	0.995	0.995	0.987	0.989
Country FE	Yes	Yes	Yes	Yes
Observations	314	314	266	266

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

Table B.13: Falsification (Hydrocarbon)

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (All)		Exports (All)	
Air Distance (log)	0.321	0.372	0.393	0.399
	(0.800)	(0.815)	(0.814)	(0.817)
Post	0.224	10.714***	0.205	0.204
	(0.173)	(3.682)	(0.177)	(0.177)
Pre-embargo sea share*Post		-0.067*		
		(0.036)		
Pre-embargo land share*Post		-0.038***		-0.586**
		(0.008)		(0.299)
Constant	18.186***	17.704**	17.576**	17.525**
	(6.897)	(7.024)	(7.027)	(7.050)
Observations	154	154	98	98
Pseudo R2	0.992	0.994	0.990	0.990
Country FE	Yes	Yes	Yes	Yes
Observations	154	154	98	98

Notes: Robust standard errors clustered at country level in parentheses;
***, **, * denote significance at the 1%, 5% and 10% level, respectively.
Column (4) does not control for pre-embargo sea share*Post since its inclusion results in missing standard errors.

Table B.14: Control for RGDP(log)

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.277**	-0.500**	-0.273**	-0.469**
	(0.116)	(0.213)	(0.117)	(0.187)
Post	0.262***	0.204**	0.264***	0.208**
	(0.101)	(0.097)	(0.101)	(0.094)
RGDP (log)	2.242	1.065	2.203	1.524
	(3.484)	(3.252)	(3.494)	(3.338)
Pre-embargo sea share*Post		0.002		0.003
		(0.002)		(0.002)
Pre-embargo land share*Post		0.001		0.001
		(0.002)		(0.004)
Constant	-11.839	5.187	-11.367	-0.960
	(44.475)	(41.954)	(44.606)	(43.000)
Observations	274	274	260	260
Pseudo R2	0.997	0.998	0.997	0.997
Country FE	Yes	Yes	Yes	Yes
Observations	274	274	260	260

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

Table B.15: Control for changes linked to pre-embargo RGDP (log)

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.339**	-0.580***	-0.338**	-0.545***
	(0.154)	(0.213)	(0.156)	(0.197)
Post	0.572*	0.496	0.580*	0.469
	(0.315)	(0.375)	(0.316)	(0.364)
Pre-RGDP (log)*Post	-0.017	-0.019	-0.018	-0.016
	(0.022)	(0.026)	(0.022)	(0.025)
Pre-embargo sea share*Post		0.002		0.003
		(0.002)		(0.002)
Pre-embargo land share*Post		0.001		0.002
		(0.002)		(0.004)
Constant	17.383***	19.516***	17.371***	19.203***
	(1.366)	(1.882)	(1.376)	(1.742)
Observations	274	274	260	260
Pseudo R2	0.997	0.998	0.997	0.997
Country FE	Yes	Yes	Yes	Yes
Observations	274	274	260	260

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

Table B.16: Control for changes linked to pre-embargo trade/imports

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.388*	-0.482*	-0.385*	-0.469**
	(0.231)	(0.247)	(0.233)	(0.238)
Post	0.387***	0.223	0.387***	0.212
	(0.098)	(0.150)	(0.099)	(0.143)
Pre-trade*Post	-0.000	0.000		
	(0.000)	(0.000)		
Pre-embargo sea share*Post		0.002		0.003
		(0.003)		(0.002)
Pre-embargo land share*Post		0.001		0.003
		(0.002)		(0.004)
Pre-import*Post			-0.000	0.000
			(0.000)	(0.000)
Constant	17.805***	18.637***	17.779***	18.520***
	(2.044)	(2.184)	(2.057)	(2.107)
Observations	292	292	278	278
Pseudo R2	0.997	0.998	0.997	0.997
Country FE	Yes	Yes	Yes	Yes
Observations	292	292	278	278

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

Table B.17: Control for changes linked to pre-embargo top importers

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.316***	-0.468**	-0.313***	-0.471**
	(0.108)	(0.210)	(0.110)	(0.195)
Post	0.532***	0.426***	0.533***	0.425***
	(0.072)	(0.122)	(0.072)	(0.116)
Pre-top importers*post	-0.207***	-0.169**	-0.207***	-0.166*
	(0.073)	(0.084)	(0.073)	(0.086)
Pre-embargo sea share*Post		0.001		0.002
		(0.002)		(0.002)
Pre-embargo land share*Post		0.001		0.002
		(0.002)		(0.004)
Constant	17.169***	18.515***	17.140***	18.536***
	(0.959)	(1.861)	(0.973)	(1.726)
Observations	292	292	278	278
Pseudo R2	0.998	0.998	0.997	0.998
Country FE	Yes	Yes	Yes	Yes
Observations	292	292	278	278

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

Table B.18: Drop food sector

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.373***	-0.581**	-0.370***	-0.563**
	(0.100)	(0.261)	(0.102)	(0.228)
Post	0.337***	0.254***	0.337***	0.269***
	(0.030)	(0.066)	(0.030)	(0.050)
Pre-embargo sea share*Post		0.001		0.003
		(0.003)		(0.002)
Pre-embargo land share*Post		0.001		0.001
		(0.002)		(0.003)
Constant	18.043***	19.889***	18.015***	19.735***
	(0.889)	(2.313)	(0.903)	(2.020)
Observations	288	288	274	274
Pseudo R2	0.998	0.998	0.998	0.998
Country FE	Yes	Yes	Yes	Yes
Observations	288	288	274	274

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

Table B.19: Drop Turkey, Iran, Kuwait and Oman

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.325***	-0.472**	-0.321***	-0.473**
	(0.105)	(0.206)	(0.107)	(0.189)
Post	0.337***	0.267***	0.336***	0.269***
	(0.028)	(0.058)	(0.028)	(0.048)
Pre-embargo sea share*Post		0.000		0.002
		(0.002)		(0.002)
Pre-embargo land share*Post		0.002		0.002
		(0.002)		(0.004)
Constant	17.300***	18.601***	17.264***	18.612***
	(0.935)	(1.831)	(0.950)	(1.673)
Observations	284	284	270	270
Pseudo R2	0.998	0.998	0.998	0.998
Country FE	Yes	Yes	Yes	Yes
Observations	284	284	270	270

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

Table B.20: Drop countries that cut ties (Yemen, Libya, Maldives, Mauritania and Comoros), except Senegal

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.288**	-0.504**	-0.285**	-0.504**
	(0.119)	(0.211)	(0.120)	(0.197)
Post	0.341***	0.245***	0.341***	0.244***
	(0.028)	(0.060)	(0.028)	(0.053)
Pre-embargo sea share*Post		0.002		0.003
		(0.002)		(0.002)
Pre-embargo land share*Post		0.001		0.003
		(0.002)		(0.004)
Constant	16.924***	18.828***	16.896***	18.828***
	(1.055)	(1.874)	(1.068)	(1.745)
Observations	282	282	272	272
Pseudo R2	0.997	0.997	0.997	0.997
Country FE	Yes	Yes	Yes	Yes
Observations	282	282	272	272

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

Table B.21: Drop all countries that cut ties (Yemen, Libya, Maldives, Mauritania, Comoros and Senegal)

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.288**	-0.504**	-0.285**	-0.504**
	(0.119)	(0.211)	(0.120)	(0.197)
Post	0.341***	0.245***	0.341***	0.244***
	(0.028)	(0.060)	(0.028)	(0.053)
Pre-embargo sea share*Post		0.002		0.003
		(0.002)		(0.002)
Pre-embargo land share*Post		0.001		0.003
		(0.002)		(0.004)
Constant	16.924***	18.828***	16.896***	18.828***
	(1.055)	(1.874)	(1.068)	(1.745)
Observations	280	280	270	270
Pseudo R2	0.997	0.997	0.997	0.997
Country FE	Yes	Yes	Yes	Yes
Observations	280	280	270	270

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

Table B.22: Drop all countries that cut or downgrade ties (Yemen, Libya, Maldives, Mauritania, Comoros, Senegal, Chad, Djibouti, Jordan and Niger)

	(1)	(2)	(3)	(4)
Dep. Var.	Trade (Air)		Imports (Air)	
Air Distance (log)	-0.302***	-0.544**	-0.298**	-0.526***
	(0.115)	(0.217)	(0.117)	(0.189)
Post	0.341***	0.235***	0.341***	0.256***
	(0.028)	(0.062)	(0.028)	(0.049)
Pre-embargo sea share*Post		0.002		0.003*
		(0.002)		(0.002)
Pre-embargo land share*Post		0.001		-0.000
		(0.002)		(0.004)
Constant	17.052***	19.202***	17.025***	19.043***
	(1.027)	(1.924)	(1.039)	(1.678)
Observations	274	274	266	266
Pseudo R2	0.997	0.998	0.997	0.998
Country FE	Yes	Yes	Yes	Yes
Observations	274	274	266	266

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

Chapter 3

International Migration, Remittances and Remaining Households: Evidence from a Trade Embargo

3.1 Introduction

Recent decades have witnessed an overall rise in international remittances,[1] which increased from an estimated $126 billion in 2000 to $689 billion in 2020 (McAuliffe and Khadria, 2019). This rise is attributed to the increase in migration between developed and developing countries, in addition to the fall in migration costs due to technological advancements (Meyer and Shera, 2017). Remittances represent the highest source of received foreign income in many developing countries (Ratha, 2005). They have been argued to improve household welfare and alleviate poverty, despite having a questionable impact on economic development in recipient countries (Adams Jr, 2011). Nonetheless, it has been difficult to identify the impact of remittances on households' welfare given selection into migration and hence, selection into those receiving remittances (Clemens and McKenzie, 2014).

In this paper, we assess the impact of an unexpected shock on the second major destination country among Nepali migrants (Endo and Afram, 2011). Our contribution

[1]McAuliffe and Khadria (2019) mention that "remittances are financial or in-kind transfers made by migrants directly to families or communities in their countries of origin."

in this paper is twofold. First, we contribute to the literature seeking to assess the impact of remittances on economic development. The closest paper to ours is the seminal paper written by Yang (2008). He exploits the effect of the Asian financial crisis on remittances received by Filipino households and examines how this exogenous change in remittances affects households' consumption and investment. One major difference with him is that our remittance shock is completely external to origin households. We also follow Yang (2008) in assessing the stability of our coefficient of interest to the addition of pre-embargo characteristics. Moreover, we are able to further assess the existence of confounding trends between households with migrants in Qatar versus those with migrants in other countries. Second, we shed light on the unintended consequences of a trade embargo. Our results point to a 56% reduction in remittances for households with a migrant in the country exposed to the shock versus households with migrants in other international destinations not affected by the shock.

Nepal is a case in point with remittances representing over a quarter of GDP, making it a significant source of the country's foreign exchange earnings (McAuliffe and Khadria, 2019). In addition, 56% of Nepali households receive remittances as reported in the Nepal Living Standard Survey (2010/11) (Central Bureau of Statistics, 2011, as cited in Bhandari, 2016). These are particularly important in rural Nepal, where remittances constitute the second most important source of household income (Bhandari, 2016). Therefore, shocks to remittances are likely to have detrimental effects on the country's economy and origin households' welfare. A defining feature of Nepal's employment migrants is their reliance on short-term contracts and concentration in Gulf Corporation Council (GCC) countries and Malaysia (McAuliffe and Khadria, 2019). More specifically, 92% of total migrant workers from Nepal were concentrated in Qatar, Kuwait, UAE, Saudi Arabia and Malaysia in 2017/18, which makes them highly vulnerable to shocks in these destinations (McAuliffe and Khadria, 2019).

On June 5th 2017, Egypt, Saudi Arabia, UAE and Barhain cut all diplomatic ties with the State of Qatar. They closed airspaces, land borders and blocked access to seaports. The blockading countries motivated their move as a result of Qatar's alleged financial support for terrorism, maintenance of a close relationship with Iran and interference in their internal matters – allegations that Qatar refuted as baseless (Chughtai, 2020). The blockading countries' measures were generally targeting the entire economy of Qatar without focusing on governmental entities, thus substantial financial losses befell individuals (Javed, 2018) with migrants being particularly vulnerable (Toppa, 2017). We exploit such a sudden shock to assess the impact of the resulting fall in remittances on consumption and expenditure outcomes in origin households. More specifically, we use longitudinal data spanning the years 2016, 2017 and 2018 from the Household Risk and Vulnerability Survey in Nepal.

Given the surge in international remittances since the late 1990s (Yang, 2011), studies on remittances and their impact have increased in parallel (Adams Jr, 2011). Studies either examine remittances at the macro or the micro level (Rapoport and Docquier, 2006). Macro-level data tend to be of poor quality (Rapoport and Docquier, 2006), rendering their results questionable (Gubert, 2017). Although macro data have improved, micro-level household survey data have the advantage of capturing formal and informal channels (Clemens and McKenzie, 2014). By collecting a wide array of disaggregated data, micro-level data allow for a more accurate examination of remittances (Adams Jr, 2011). This does not mean that household surveys do not pose important challenges. For instance, Clemens and McKenzie (2014) point out that self-reported remittances may not necessarily correspond to actual levels. However, the authors argue that since the problem is likely to persist over time, this data would still give a fairly accurate picture of the growth rate.

Given the context of our study, we provide a brief overview of some studies that exploit household surveys to study remittances in developing countries. A methodological challenge is that households decide to send migrants, so there is a self-selection problem with the expectation that more able members are sent abroad (Karki Nepal, 2016). Another issue is that remitting behavior is not random (Karki Nepal, 2016). In other words, migrants decide how much to send back home and this may be correlated with household characteristics or outcomes of interest, limiting the possibility of identifying a causal effect. Adams Jr (2011) mentions different approaches undertaken in the literature to address the different methodological issues while studying remittances. A popular convincing one is the exploitation of a natural experiment. In his seminal paper, Yang (2008) examines the causal effect of remittances sent by Filipino migrants abroad on remaining households' different consumption and investment outcomes. To achieve identification, he exploits the panel structure of his dataset and limits his sample to households with at least one international migrant to deal with self-selection into migration. To ensure that the migration decision was not influenced by the shock, the sample of households are selected in the pre-shock period. Second, to deal with endogeneity in remittance behavior he exploits the 1997 Asian financial crisis and the exogenous change in remittances due to differential changes in exchange rates across Filipino migrants' destination countries. His results confirm that the positive shock in exchange rates leads to an increase in remittances received. Moreover, it has a large positive effect on different household investment variables.

With the exception of Yang (2008), the other papers that exploit different shocks on remittances tend to use an instrumental variable strategy. For instance, Karki Nepal (2016) uses the difference in exchange rates between Nepal and migrants' destination countries during the 2008 financial crisis as an instrument for remittances. In a different context, Cuadros-Meñaca (2020) addresses the endogeneity between remittances and outcome variables by using the interaction of historical migrants' shares in destination countries and post-financial crisis shocks in the labor market experienced by the destination countries. Alcaraz et al. (2012) examine the effect of remittances on child schooling and child labor in Mexico by exploiting the impact of the financial crisis on Mexican migrants' unemployment in the U.S. as an instrument for remittances. McKenzie and Yang (2012) point out the shortcomings of utilizing shocks as instruments. First, it will not be representative of the migrants' populations since the local average treatment effect will be narrowed to those households that change migration post-shock. Second, they argue that it is unlikely that the instrument satisfies the exclusion restriction as there are usually multiple channels through which the shock affects the outcome in the second stage. As a result, the authors argue in favor of running a reduced form in which we regress the outcome of interest directly on the shock, similarly to Yang (2008). This is what we do in this paper. In this case, we do not capture the effect of remittances or migration on outcomes, instead we look at how households' outcomes respond to temporary shocks in migrants' earnings. For instance, if we find that households save more, this is a response to the shock and we are not able to infer a general conclusion that remittances are used for savings (McKenzie and Yang, 2012).

In addition, our paper contributes to the literature trying to understand the consequences of an imposed embargo. A large literature seeks to assess the impact of an embargo on the targeted country (see for instance: Felbermayr et al., 2019; Crozet and Hinz, 2020; Ahn and Ludema, 2020; Draca et al., 2021; Chakravarty et al., 2021 among others), but little is known about the indirect consequences for non-blockading countries. One exception is Al-Malk et al. (2021) who exploit the embargo in Qatar to quantify the trade elasticity to air transportation costs. In this paper, we examine the consequences of the same embargo on Nepali households with migrants in Qatar at the time of the shock. Therefore, we examine whether the effect of the embargo spills over to other non-targeted countries.

The remainder of this paper is organized as follows, Section 3.2 gives an overview of the blockade and trends on migration and remittances. Section 3.3 discusses the identification strategy employed and data used. In Section 3.4, we present the results along with robustness checks. Finally, Section 3.5 concludes.

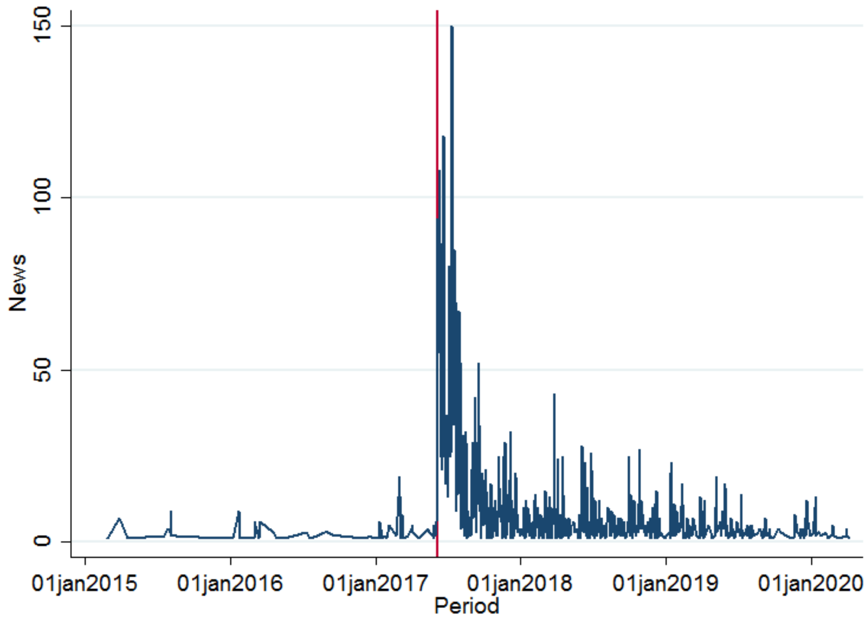
3.2 Background

The blockade imposed on Qatar on June 5$^{\text{th}}$, 2017 by Saudi Arabia, United Arab Emirates, Egypt and Bahrain was not expected as shown by Figure 3.1, which plots the count of the news on embargo targeting Qatar over the period January 2015 to April 2020. There is a spike on the blockade date before which no mention of such an event can be spotted.[2] Also, none of these news recordings come from Nepal. Therefore, we argue that migrants could not anticipate the blockade and change remittance behavior beforehand.

When the embargo was imposed, migrants were affected in different ways. First, some Qataris employ migrants to work in Saudi Arabia. When the blockade was suddenly imposed, those migrants were stranded there with no channel to send them food, water or money (Human Rights Watch, 2017). They could not send remittances back to their countries of origin either. Second, the rise in food prices due to the closure of the land border had a negative effect on migrants residing in Qatar (Human Rights Watch, 2017). Therefore, it is expected that migrants who kept their jobs needed

[2] This figure is taken from Al-Malk et al. (2021) where we elaborate the unexpected nature of the blockade in more detail. The few news' count before the blockade date in Figure 3.1 are either an error or refer to a threat of imposing an embargo by other countries.

Figure 3.1: News about the embargo



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

to spend more money for their consumption and had less money available to send as remittances. Third, some migrants were laid off from sectors that were affected. For instance, the manufacturing sector suffered due to the loss of raw materials, while the transportation sector was negatively affected due to closure of the land border for trade (Javed, 2018). Moreover, since 50% of tourists to Qatar are GCC nationals, tourism declined following the blockade (Yap et al., 2020), which meant that migrants employed in the hospitality industry were heavily affected. In particular, taxi drivers faced substantial losses as their daily earnings fell by 70% (Javed, 2018). To deal with such adverse effects on its tourism sector, Qatar has facilitated its VISA policies and introduced a new visa-free entry for nationals of 80 countries (Javed, 2018). Although such a move contributed to somewhat offsetting the decline in GCC tourists, numbers have not reverted yet to pre-blockade levels (Javed, 2018).

Table 3.1 reports the distribution of Nepali migrants overseas, pre- and post-embargo based on our survey. India is the largest destination, accounting for more than 30% of migrants in both periods. It is followed by Malaysia, Qatar and Saudi Arabia, all

Table 3.1: Nepal's migration and remittances

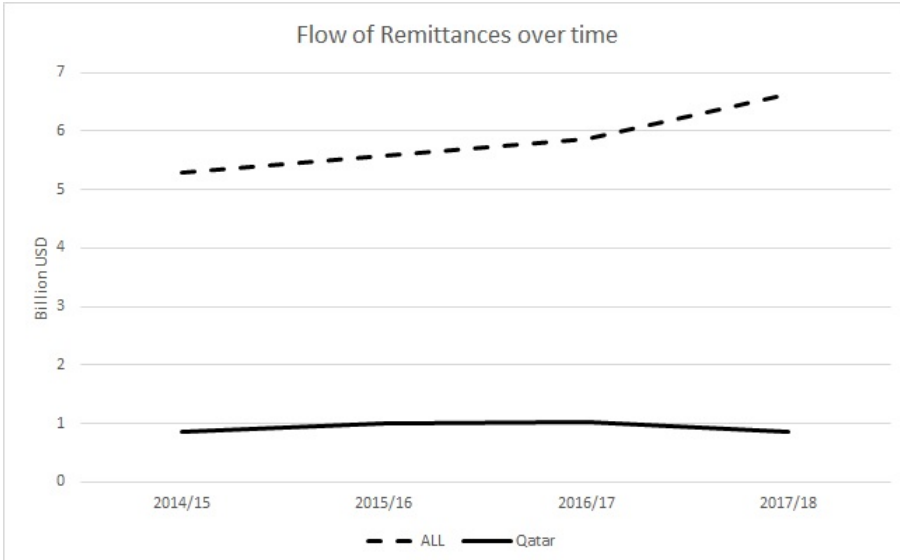
Location of overseas workers pre- and post-embargo					
Location	Number of overseas workers		% of total		% change in remit
	wave 1 (2016)	wave 3 (2018)	wave 1 (2016)	wave 3 (2018)	wave 3-wave 1
India	589	482	33.64	34.28	105.31
Malaysia	359	265	20.50	18.85	3.80
Qatar	244	193	13.93	13.73	28.85
Saudi Arabia	242	187	13.82	13.30	11.25
UAE	100	104	5.71	7.40	12.53
Japan	45	41	2.57	2.92	10.58
South Korea	26	25	1.48	1.78	39.40
United States	18	18	1.03	1.28	36.65
Australia	13	13	0.74	0.92	-32.14
Hong Kong	10	10	0.57	0.71	-64.0
United Kingdom	5	4	0.29	0.28	57.80
Israel	2	1	0.11	0.07	400
Other	98	63	5.60	4.48	-7.05
Total	1,751	1,406	100	100	

Notes: this table shows data for migrants from households with international migrants, excluding mixed households with migrants in Qatar and other international destinations. We report information on overseas migrants pre- and post-embargo.

of which represent more than 10% of the total separately. The remaining migrants are dispersed across other countries, none of which surpasses 8% of the total. The number of migrants to Qatar declined over the period. Generally, migrants' representations of the total did not change much over the two periods, except for Malaysia and UAE where it declined and increased by around two percentage points, respectively. Over the period, remittances sent by households increased from all destinations except for Australia, Hong Kong and the Other destinations category. Remittances from Qatar increased but less than those from other destinations like India, South Korea, the United States or the United Kingdom.

To track the evolution of remittances over time, we use Figure C.2 in Appendix C.2 and take the average remittance value for Qatar on the one hand and for all destinations with the exception of Qatar on the other hand, and plot the two series in Figure 3.2. We can see that remittances from Qatar were steadily increasing over the years, but declined in 2017/18. This trend is not observed for other destinations, pointing to a

Figure 3.2: Flow of remittances to Nepal (Qatar versus Other)



Source: Nepal Labour Migration report 2020, *Government of Nepal*, page 94.

potential detrimental effect of the embargo on remittance inflows from Qatar.

3.3 Empirical Analysis

Identification Strategy

To estimate the effect of the embargo on Nepali households with migrants working in Qatar, we use two waves of panel data at the household level. By focusing on households repeated across the time periods, we are able to deal with different sources of endogeneity biases, selection and omitted variable biases including unobservable characteristics (Adams Jr, 2011). Indeed, a major identification challenge in the literature is to deal with selection into migration. Since an individual's choice to migrate is endogenous, the likelihood to receive remittances is likely to be also driven by factors that potentially differentiate these households from others with no migrants. In short, comparing households with migrants with others without migrants is likely to capture unobserved differences between them. Table 3.2 shows that there is a statistically significant difference between these households for most characteristics. To deal with this key identification challenge, we only focus on households with migrants in the pre-embargo period, i.e. wave 1 of our survey, similar to Yang (2008). If we were to define our sampled households based on the presence of migrants in the post-embargo period, we may capture the effect of the embargo itself such as the return of migrants who were working in Qatar due to job loss. In fact, there was a reduction of 25% in the number of Nepali migrants in Qatar in July following the blockade (Toppa, 2017). We also limit our sample to households observed in both waves to ensure we are dealing with a balanced panel and can deal with unobserved heterogeneity between households. We then compare households with migrants in Qatar, potentially exposed to the embargo imposed on Qatar, with other households with migrants in other international destinations. Specifically, we estimate the following linear and first-difference models:

$$Y_{jt} = \alpha_0 + \beta(QatMig_J * post_t) + \sigma QatMig_J + \gamma post_t + \epsilon_{1jt} \quad (3.1)$$

$$\Delta Y_{jt} = \phi_0 + \beta(QatMig_J * post_t) + \epsilon_{2jt} \quad (3.2)$$

Y_{jt} denotes different outcome variables of household j at time t. Our main outcome variable is the sum of remittances received over the last 12 months. We transform such a variable into logarithm (adding the value one to deal with zero values to ease interpretation).[3] $QatMig_J$ is an indicator variable that equals one if the household has at least one migrant in Qatar before the embargo took place. It controls for unobservable characteristics that differentiate these households from control households without a migrant in Qatar. $Post_t$ is an indicator variable that equals one if the household is surveyed post-embargo and zero otherwise. It accounts for time-varying factors that affect all households in a similar way. The interaction term, $QatMig_J * post_t$, is our main variable that measures how the embargo affects households with at least one migrant in Qatar. Finally, ϵ_{1jt} and ϵ_{2jt} are the error terms, respectively. The difference between Equations (3.1) and (3.2) is that the second accounts for time-constant unobservable

[3]As a robustness check, we report results using the inverse hyperbolic sine transformation in Appendix C.4.

characteristics of households. In both models, we report both robust standard errors and standard errors clustered at the ward (Village Development Committee VDC) level. Cameron and Miller (2015) point out that clustering of standard errors is routinely used at the primary sampling unit (PSU) in complex survey designs.[4] It means we cluster standard errors at the PSU level (371 clusters).

Our identification rests on the parallel-trend assumption, that households with migrants in Qatar prior to the trade embargo would follow a similar trajectory compared to those with migrants in other international destinations. In other words, both would have not had a systematic difference in outcomes had the embargo not taken place. This cannot be tested directly, but following Yang (2008) we include pre-embargo characteristics in our regression equations to check for potential contamination of our main coefficient. Specifically, this allows us to test for the possibility that differences in pre-embargo characteristics between treated and control households are the drive of differential outcomes that we attribute to the embargo. Therefore, we augment our main model in Equations (3.1) and (3.2) with pre-embargo characteristics X_{jt-1}.[5] As a result, our main coefficient β captures the effect of the embargo on outcomes of the households followed overtime, conditional on households' characteristics. Visual inspection of Figure 3.2 is indicative that remittances prior to the trade embargo follow a similar trend for households with migrants in Qatar and households with migrants in other international destinations. In addition to this, Figures 3.1 and 3.3 show that neither the blockade was expected nor was there an estimated anticipation effect of the embargo on remittances.

[4] The suggestions in clustering of standard errors is either at the treatment level (Cameron and Miller, 2015) or the sampling design level (Abadie et al., 2017). Yang (2008) follows the former and clusters at country of destination level, but in his case the treatment varies across different countries of destination, whereas our treatment only takes place in Qatar versus other destinations. Thus, we cluster standard errors following the sampling design approach.

[5] These controls include at household level: religion, number of members and a dummy for the presence of members below the age of 16, at household's head level: being male, age and marital status, at the migrant level: age of migrants, number of months away and a dummy for expectation of moving back to country of origin. All of migrant-level controls are averaged at the household level. Also, all of these controls are reported at pre-embargo level (i.e wave 1).

Data & Sample Construction

We use two waves out of the three waves of panel data from the Household Risk and Vulnerability Survey in Nepal (Walker and Jacoby, 2020).[6] We omit wave 2 because our main outcome variable, remittances, is asked retrospectively (i.e. remittances received over the past 12 months). Therefore, we limit our analysis to waves one and three since we cannot be sure whether wave 2's remittances are pre- or post-embargo. The survey was carried out by the World Bank for three consecutive years from 2016 to 2018. It covers 6,000 households in non-metropolitan areas in Nepal.[7] Stratified sampling was used in this survey. In order to obtain the sample, the country was divided into 11 strata following the NLS-III survey, except that three urban strata were excluded. 50 out of the 70 districts of Nepal were chosen with probability proportional to size in order to increase households' concentration, with the size measured in number of households. Then primary sampling units (PSU) were selected from all administrative wards in those 50 selected districts, for each strata at a time. Finally, 15 households were randomly chosen from the entire list of households at each PSU.[8]

The retention rate of households across all three survey waves is 94%, giving a total of 5,654 households that are available in all waves (Walker et al., 2019).[9] The longitudinal nature of the dataset allows us to track households overtime and effectively deal with potential omitted variable bias (Andreß, 2017). In addition, it gives us data pre- and post-embargo, allowing us to carry a difference-in-difference estimation to identify the causal effect of the embargo on different outcomes. The survey dates are as follows: wave 1 was conducted between June 5th, 2016 to August 21st, 2016; wave 2 between June 12th, 2017 to August 14th, 2017 and wave 3 between June 10th, 2018 to August 22nd, 2018. Noting that the embargo took place on June 5th, 2017, this effectively gives us one pre-embargo period and 2 post-embargo periods.

Migrants in the dataset are defined as "any person who had not lived in the

[6]Disclaimer: The user of the data acknowledges that the original collector of the data, the authorized distributor of the data, and the relevant funding agency bear no responsibility for use of the data or for interpretations or inferences based upon such uses. Copyright: The World Bank Group, 2020.

[7]As a result, conclusions drawn are not necessarily applicable to metropolitan areas (Walker et al., 2019).

[8]A clarifying table and map from Walker et al. (2019) are provided in Appendix C.1.

[9]Despite the full sample attrition being 6%, all households with international migrants prior to the embargo in our sample have been followed in wave 3, featuring no attrition.

household during the previous six months but was still contributing financially to the household" (Walker et al., 2019). Therefore, it includes households with local migrants, international migrants or a mix of both with destination of migration identified. Studies on the impact of remittances on different outcomes vary with regards to the inclusion of local migrants (Askarov and Doucouliagos, 2020). In our case, there is a strong rationale to exclude internal migrants since origin households with international migrants appear to be very different compared to other households. Our research design is comparing households with migrants in Qatar with households with migrants in other countries. Walker et al. (2019) point out several factors that seem to differ between households with local versus international migrants in our survey of Nepali households. For instance, internal migrants' share in the lowest asset quintile was 5.2% whereas it was 6-7% for richest households. Furthermore, the share of international migrants in richest households was 20.2% compared to 10% in the poorest. Therefore, it seems that characteristics of households differ depending on whether its members migrated locally or internationally. Since our study aims to examine the impact of a shock that affected migrants in one international destination, we include households with international migrants only.[10] Therefore, our results are not representative of the entire Nepali population. In total, our sample includes 1,508 households.[11]

Common Trend Assumption

Our key identification strategy to ensure that we obtain causal estimates on the effect of the embargo is that households with migrants in Qatar versus other destinations follow a similar trend in the studied outcomes had the embargo not taken place. We have already shown in Figure 3.2 that prior to the embargo, remittances to households with migrants in Qatar follow a similar trend as remittances to households with migrants

[10]We also drop mixed households to rule out any crowding-out effect from either local migrants or those in other international destinations not affected by the shock. In a similar situation to Mahapatro et al. (2017), households that have both internal and international migrants are very few to allow derivation of meaningful results so we omit them from the analysis. Moreover, we exclude households with both migrants in Qatar and other international destinations, since their exposure to the embargo is not clear. There are 62 households with a mix of local migrants and migrants in Qatar, 106 households with both migrants in Qatar and other international destinations and 384 households with a mix of migrants in other international and local destinations- which we exclude from our sample.

[11]In addition to the definition of migrant types that excludes some households, we also drop 6 households that are unwilling to disclose religion, one main control variable at the household level.

Table 3.2: Summary statistics (households with migrants versus no migrants)

Summary Statistics (wave 1)							
	No Migrants HH			Migrants HH			
	Mean	SD	Obs.	Mean	SD	Obs.	t-test
Remit (millions)	0.00	0.02	3884	0.16	0.20	1508	-0.159*** (-48.03)
Muslim HH	0.02	0.15	3926	0.01	0.11	1508	0.00966* (2.27)
Christian HH	0.02	0.15	3926	0.02	0.13	1508	0.00726 (1.67)
Hindu HH	0.86	0.35	3926	0.87	0.33	1508	-0.0187 (-1.78)
Buddhist HH	0.08	0.27	3926	0.08	0.27	1508	-0.00148 (-0.18)
Kirant HH	0.02	0.14	3926	0.02	0.13	1508	0.00161 (0.39)
HH Size	4.92	1.95	3884	4.79	1.99	1508	0.131* (2.20)
Member<16	0.73	0.44	3884	0.79	0.41	1508	-0.0614*** (-4.67)
Head Male	0.90	0.31	3884	0.61	0.49	1508	0.284*** (25.55)
Head age	48.36	13.83	3884	49.03	13.90	1508	-0.666 (-1.59)
Head Single	0.01	0.08	3926	0.00	0.06	1508	0.00188 (0.85)
Head Married	0.89	0.32	3926	0.87	0.34	1508	0.0177 (1.81)
Head Widow	0.09	0.28	3926	0.12	0.32	1508	-0.0315*** (-3.56)
Head Divorced	0.01	0.11	3926	0.01	0.10	1508	0.00126 (0.40)
Age (mig)	3.68	9.25	3926	29.64	8.75	1508	-25.95*** (-93.99)
Months away (mig)	3.40	15.13	3926	22.79	32.04	1508	-19.39*** (-30.17)
Expect Return (mig)	0.15	0.36	3926	0.97	0.16	1508	-0.822*** (-86.25)

Notes: we only include households with international migrants (mig) observed in waves 1 and 3. Migrants are defined pre-embargo and refer to overseas migrants only. We drop households with mixed-migrant types. All variables are reported for wave 1 (pre-emabrgo). We include household-level and household-head level variables. Migrant-level variables are the average of all migrants within a given household. Remittances are reported in million Nepali Rupees. For the t-test, we report the mean, and t-test of difference in parenthesis.

Table 3.3: Summary statistics (households with migrants in Qatar versus Other)

Summary Statistics (wave 1)							
	Other Mig. HH			Qatar Mig. HH			
	Mean	SD	Obs.	Mean	SD	Obs.	t-test
Remit (millions)	0.16	0.22	1258	0.17	0.13	250	-0.0108 (-0.77)
Muslim HH	0.01	0.10	1258	0.03	0.17	250	-0.0177* (-2.23)
Christian HH	0.02	0.13	1258	0.01	0.11	250	0.00469 (0.54)
Hindu HH	0.87	0.33	1258	0.88	0.33	250	-0.00719 (-0.31)
Buddhist HH	0.08	0.27	1258	0.06	0.25	250	0.0179 (0.96)
Kirant HH	0.02	0.13	1258	0.02	0.13	250	0.00149 (0.16)
HH Size	4.80	2.01	1258	4.72	1.94	250	0.0765 (0.55)
Member<16	0.79	0.41	1258	0.82	0.38	250	-0.0322 (-1.15)
Head Male	0.62	0.49	1258	0.58	0.49	250	0.0329 (0.97)
Head age	49.22	13.79	1258	48.05	14.40	250	1.173 (1.22)
Head Single	0.00	0.05	1258	0.01	0.11	250	-0.00962* (-2.21)
Head Married	0.87	0.34	1258	0.87	0.34	250	0.000839 (0.04)
Head Widow	0.12	0.32	1258	0.11	0.32	250	0.00644 (0.29)
Head Divorced	0.01	0.10	1258	0.01	0.09	250	0.00233 (0.34)
Age (mig)	29.43	8.82	1258	30.66	8.35	250	-1.223* (-2.02)
Months away (mig)	23.54	33.51	1258	19.05	22.97	250	4.494* (2.03)
Expect Return (mig)	0.97	0.18	1258	1.00	0.00	250	-0.0331** (-2.97)

Notes: we only include households with international migrants (mig) observed in waves 1 and 3. Migrants are defined pre-embargo and refer to overseas migrants only. We drop households with mixed-migrant types. All variables are reported for wave 1 (pre-emabrgo). We include household-level and household-head level variables. Migrant-level variables are the average of all migrants within a given household. Remittances are reported in million Nepali Rupees. For the t-test, we report the mean, and t-test of difference in parenthesis.

in other international destinations. Therefore, we are confident in attributing the drop in remittances to households with migrants in Qatar observed in Figure 3.2 to the embargo. At the same time, both Figures 3.1 and 3.3 inform us that neither the embargo could have been forecasted according to the media nor there seems to be an estimated anticipation effect of the embargo on remittances. Apart from this visual inspection of the data we are willing to explore this further since diverging trends are more likely when households are different to start with. Table 3.3 sheds light on potential initial differences comparing key households' characteristics between households with migrants in Qatar versus other international destinations. The variables are chosen following Yang (2008) and all of them are defined at the pre-embargo (wave 1) level.[12] These variables either appear to be fixed over time or are measured before the embargo. They include household's charachteristics: religion, household size and a dummy for the presence of members below the age of 16; household head's characteristics: age, gender and marital status; and migrant characteristics: age, number of months away and a dummy for expectation of moving back to the country of origin, all taken as an average per household in case of multiple migrants. We also report in Table 3.3 the amount of remittances received.[13] As we can observe in the table, there is no difference in the means of the two groups for most variables at the household's and head's level. Exceptions are that households with migrants in Qatar are more likely to be Muslim and have a single head. However, differences are more pronounced if we look at the migrants' characteristics. For instance, migrants to Qatar are slightly older, spend less time away and are more likely to return compared to international migrants sent to other destinations. We will test the sensitivity of our main results controlling for such initial differences in our results section.

[12]We also select all these variables with the exception of remittances with a view to a matching exercise (Caliendo and Kopeinig, 2008).

[13]More controls at the migrant level are also given in Table C.1 in Appendix C.1: months away, earnings, gender, reason for migration and use of remittance fund.

3.4 Results

The Impact of the Embargo on Remittances

Main results. In Table 3.4, we present the results regarding the impact of the embargo on remittances for households with a migrant in Qatar, considering households with at least one international not-in-Qatar migrant as the control group. Our sample is limited to households observed in both waves 1 and 3,[14] and includes households with at least one international migrant (excluding mixed-migrant households). Every cell in Table 3.4 reports the coefficient estimate $\hat{\beta}$ of the $QatMig_J * post_t$ variable from a different specification, with standard errors in parentheses. Panel (A) in Table 3.4 presents the coefficient estimates from Equation (3.1) in Column (1) and Equation (3.2) in Column (4), not augmented with any pre-embargo control variables. We report robust standard errors in Columns (2) and (5), and standard errors clustered at PSU level in Columns (3) and (6). The estimates show that the embargo had a negative and statistically significant effect on remittances received. Specifically, a household with a migrant in Qatar is associated with a 56% reduction in remittances compared with households with international migrants in other destinations.[15] In remaining panels, we augment our specification with controls for household's religion, size and presence of family members below the age of 16 in Panel (B), household's head age, being male and marital status in Panel (C), migrant's age, months away and expectation to return dummy (all averaged at the household's level) in Panel (D) and all of these controls combined in the same specification in Panel (E). The estimates remain negative and statistically significant throughout the different panels, though they slightly differ in magnitude in the first-difference estimator. A first-difference estimator is equivalent to controlling for household fixed effects, so it captures unobservable factors compared to our linear model where the coefficient's magnitude does not change. An increase in the coefficient's magnitude in the first-difference model suggests that pre-embargo controls are associated with a fall in remittances over the study period for households affected by the embargo, and we therefore observe a higher effect of the embargo once we control

[14] As mentioned earlier, since the question on remittances received is asked retrospectively, we omit wave 2 from our analysis.

[15] To interpret the coefficient's estimate, we follow Halvorsen and Palmquist (1980) correction for the case of log-linear form with dummy variables, $(100 * (e^{\beta} - 1))$.

Table 3.4: Main results

	Linear DID			FD		
	(1)	(2)	(3)	(4)	(5)	(6)
	Coeff.	SE	Clustered SE	Coeff.	SE	Clustered SE
Panel A:						
No controls	-0.834*	(0.459)	(0.453)	-0.834*	(0.451)	(0.453)
Panel B:						
Households' Controls	-0.834*	(0.460)	(0.453)	-0.836*	(0.452)	(0.457)
Panel C:						
Heads' Controls	-0.834*	(0.458)	(0.453)	-0.809*	(0.453)	(0.457)
Panel D:						
Migrants' Controls	-0.834*	(0.457)	(0.453)	-0.755*	(0.452)	(0.456)
Panel E:						
All Controls	-0.834*	(0.457)	(0.454)	-0.789*	(0.453)	(0.464)
Obs.	3016	3016	3016	1508	1508	1508

Robust & clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Notes: coefficient estimates reported are of the $QatMig_J * post_t$ variable, an interaction of having a migrant in Qatar present in a household interacted with post-embargo period. The dependent variable for the linear DID model is log(remit), while the dependent variable for the first-difference (FD) estimator is D.log(remit). All of the control variables are fixed at pre-embargo period value (i.e. wave 1). Households' controls: Religion, size & presence of members below 16; Heads' controls: male, age & marital status. Migrants' controls (household average): age, months away & dummy for expectation of return. Clustered standard errors are clustered at VDC (village development committee) level, which is the PSU in the survey. There are 371 VDC in our estimated sample. Detailed results of each panel is reported in Tables C.2 to C.6 in Appendix C.3.

for them.

Control variables. All our control variables are defined pre-embargo.[16] At the household level, we control for religious affiliation. Kelly and Solomon (2009) find that remittance behavior differs by religious affiliation for migrants in the US. In our case, however, although all coefficient estimates of different religious affiliations are negative and statistically significant compared to the reference category of other religion, all are of similar magnitude except for Muslim households who seem to have a smaller effect on remittances. Since our sample is split into migrants to Qatar versus other destinations, the effect of Muslim households might be showing the fact that a higher

[16]Detailed results for coefficient estimates of the control variables are presented in Table C.6. We comment on first-difference estimates with full set of controls, since the linear model is likely suffering from omitted variable bias.

percentage of Muslim households send their migrants to Qatar as shown in Table 3.3. The other household-level variable that we control for is household size, which measures the number of members per household. Mannan and Farhana (2014) cite some studies which suggest that household size does affect migration and remittance behavior positively. Indeed, we find a positive and statistically significant coefficient. Finally, we control for a dummy indicating the presence of members below 16 years of age at the household level. McDonald and Valenzuela (2012) mention that the presence of children increases the amount remitted by women, not men. We find that this variable is negative and statistically significant. Since all of the migrants in our sample are men, this is in line with McDonald and Valenzuela (2012).

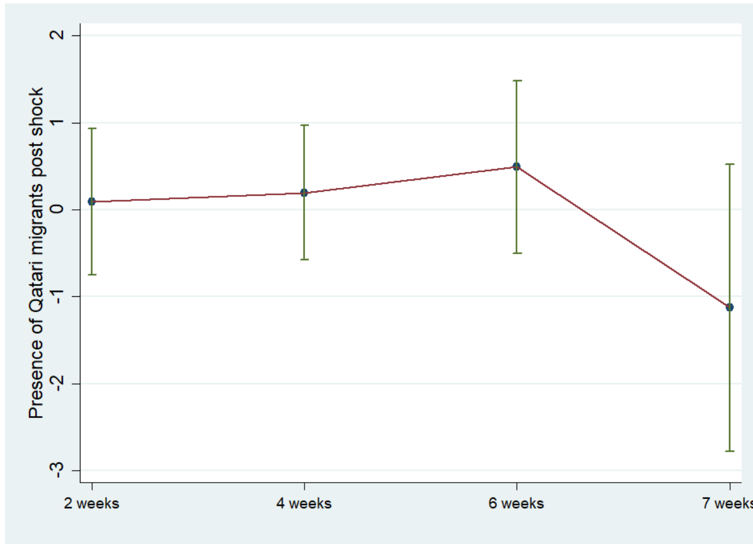
We then turn to controls at the household's head level. Some studies show that the spending of remittances on household expenditure varies if the receiver is a male or a female (Pickbourn, 2016). Therefore, this may affect the migrant's motive to send remittances. Since we do not have data on who receives remittances, we control for the gender of the household's head. It turns out that, our coefficient of household's head being a male is not statistically significant. The head's age also has no significant effect on remittances. Although some studies argue that households with an older head are likely to receive more remittances given an altruism effect from migrants (Hagen-Zanker and Siegel, 2007), our results are in line with McDonald and Valenzuela (2012) who find that the amount of remittances sent by male migrants is not correlated with the head's age. The last household's head control we include is marital status. McDonald and Valenzuela (2012) point out that the relationship between the migrant and households' head is a key determinant of remittance value, with spouses likely to remit more. Although we do not have a variable that measures the relationship between the migrant and the household's head, we control for marital status to proxy for potential spousal relationship. We do not find that a household with a married head in the pre-embargo period is likely to receive more remittances than a household with a single head (base category).

Regarding the migrants' set of controls, we include a migrant's age, months away and whether they expect to move back to their home country. Hagen-Zanker and Siegel (2007) report mixed results for a migrant's age. In our estimation, we find a negative and statistically significant coefficient of migrants' ages on remittances. In our context, this could be explained by the older age of migrants in Qatar versus other international destinations who receive more remittances on average as shown in Table 3.3. Hagen-Zanker and Siegel (2007) also mention that a migrant's length of stay can be used as a measure of risk, because after a longer stay the migrant knows the destination country better. As a result, the effect of migrants' durations on remittances is generally found to be positive (Hagen-Zanker and Siegel, 2007). On the contrary, we find a negative relationship between migrants' durations and remittances. Since most Nepali migrants are on short-term contracts in countries such as South Korea, Malaysia and Gulf countries, this could explain the negative coefficient in our context. All of the migrants in our dataset are male, which is why we do not control for a migrant's gender although it is an important determinant of remittance behavior (Guzmán et al., 2008). We control for a migrant's intention to return back and find that it is associated with a decline in the amount of remittances received. Contrary to our findings, Dustmann and Mestres (2010) find that temporary migration increases the amount remitted.

That being said, controlling for observed initial differences is not a guarantee that households with migrants in Qatar would follow a similar trend compared to households with migrants in other destinations. In absence of multiple household data prior to June 5$^{\text{th}}$, 2017, we cannot directly test for the parallel pre-trend assumption. However, it is likely migrants may not stop or reduce remitting immediately when the embargo is imposed. It may take a few weeks for them to adjust their consumption and asset patterns. Since we have a second wave of data with households interviewed right after the embargo, between June 12$^{\text{th}}$, 2017 to August 14$^{\text{th}}$, 2017, in absence of pre-existing trend, we should expect the negative impact on remittances to materialize only after a few weeks. To check for that empirically, we run an equivalent of our first-difference equation using waves 1 and 2 on our sample of households with international migrants only. The key to this exercise is that our variable of interest, interaction of household with migrant in Qatar and post, gradually incorporates extra 2 weeks of wave 2. The idea being that since the embargo took place on June 5$^{\text{th}}$, the closer the interview to that date, the more unlikely that this household's remittances were affected. However, as we extend the duration, we are including more post-embargo period to see whether an effect starts to show. The results are shown in Figure 3.3.[17] It shows that our

[17] Given the interview starts on June 12$^{\text{th}}$, 2 weeks: is on or before June 27$^{\text{th}}$, 4 weeks: is on or before July 11$^{\text{th}}$, 6 weeks: is on or before July 25$^{\text{th}}$ and 8 weeks: is on or before August 1$^{\text{st}}$.

Figure 3.3: Test pre-embargo trends



Source: Authors' computations using waves 1 and 2 of the Household Risk and Vulnerability Survey in Nepal.

coefficient of the effect of having a migrant in Qatar post-embargo is close to zero and not statistically significant in the first 7 weeks post-embargo (6 weeks after interview). However, although not statistically significant, we see a negative coefficient in week 8 after the embargo (7 weeks after interview). Thus, it gives us preliminary evidence that further into the future, an effect is likely to be present following the embargo.[18]

Robustness Checks

We undertake some robustness checks to confirm our results in Appendix C.4. First, we show that we obtain similar coefficients when sampling weights are introduced as shown in Table C.7. Second, given that our main dependent variable, remittances, includes some zero observations, we use the inverse hyperbolic sine transformation (IHS). We report coefficient estimates where the dependent variable is transformed using IHS in Table C.8. Third, we report coefficient estimates after dropping blockading countries from the sample in Table C.9. We find that the coefficient estimates are higher in magnitude. This suggests a possible contamination effect as migrants in

[18] The analysis is restricted to up to 7 weeks of the second round, because after that period the number of respondents declines substantially as shown in Figure C.3 in Appendix C.2.

blockading countries may also be impacted by the embargo shock. Finally, we confirm our estimated effect of the embargo on remittances by using propensity score matching (PSM) combined with a difference-in-differences (DID) approach. Although we have shown that our main estimated coefficient is robust to the inclusion of pre-embargo characteristics, we relax our assumption that the treatment and control groups are similar conditional on those covariates. Our coefficient estimates are robust to the use of this alternative estimation method.

Further Results

Remittances and shocks in part through remittances may play a role not only reducing poverty and improving well-being of recipient households but are also likely to have an effect on economic development of recipient countries. This would be the case if remittances constitute an alternative mean to finance human capital and entrepreneurial investments in migrants' origin households. Also, if remittances are used to increase savings and participation in the income and health scheme via pension and social security system contributions (Cuadros-Meñaca, 2020).

In Table 3.5, we estimate the main regression of the first-difference model with all set of control variables and look at the different outcomes across columns. Column (1) looks at the effect of the shock on an asset index[19]. In Column (2) we look at food insecurity, which is a measure constructed following Coates et al. (2007) by using the questions on the households' food consumption patterns from the survey. In Column (3), the outcome is total income that is a sum of proceeds from agricultural and non-agricultural sources in addition to proceeds of members in long-term employment. The final outcome in Column (4) is total expenditure, the sum of expenditure on food, frequent non-food items, infrequent non-food items, school and health. The estimates

[19]This is calculated following Walker et al. (2019). In addition to a long list of household characteristics and durables, the asset index also includes ownership of livestock and household head's age and education as a measure of human capital. Specifically, we include: number of storeys of the house, ownership of dwelling, wall material, roof material, type of toilet, foundation of the house, source of drinking water, type of fuel used for cooking, type of stove used for cooking, ownership of facilities: (telephone, mobile phone, cable TV, email/internet), ownership of durable goods: (radio/cassette/CD player, camera (still/movie), bicycle, motorcycle/scooter, motor car, etc, refrigerator or freezer, washing machine, fans, heaters, television/VCR/VCD player, pressure lamps/petromax, telephone sets cordless/mobile, sewing machine, furniture, rugs, clocks, jewelry (including watches), computer/printer, ownership of livestock, head's age and head's education.

Table 3.5: Shock on other outcomes

	Asset Index	Severe Food Insecurity	Total Income (log)	Total Exp. (log)
DID	-0.103	0.003	0.131	0.464
	(0.089)	(0.017)	(0.459)	(0.466)
Obs.	1508	1508	1508	1508

Robust & clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

do not provide us with evidence that the shock had an effect on any of these outcomes. Yang (2008) finds that exchange rate shocks, which manifest themselves in part via changes in remittances, have negligible effects on household consumption but large effects on various types of household investments such as child schooling, child labour and entrepreneurial activity. Karki Nepal (2016) finds an effect on expenditure in child education although this is not translated into improved educational and child labour outcomes. Alcaraz et al. (2012) find that remittance recipient households seem to be credit constrained since they face the negative shock on remittances by sending their children to work.

Given the substantial decline in remittances, it seems odd at first that we do not find evidence that this reduction is passed on to other outcomes. However, given that we are exploring the effect one year after the blockade happened, it may be that the effect is not contemporaneous and may show in the future. If shocks and policies in host countries affecting migrant workers have an influence on households in recipient countries, this is going to be relevant from a policy point of view.

Walker et al. (2019) provide an overview of the dataset we use in this paper along with an analysis of different shocks such as rainfall, blockade by India and others. They find that the effects of shocks are more severe for poorer households who are more exposed to shocks and less likely to cope with them. Therefore, we will check whether the effect of the shock on remittances differed for poorer versus richer households. There are different measures to differentiate the poor from the rich. A simple way to look at relative poverty of households is to divide households into quantiles (Fry et al., 2014). Using a wealth index for the classification is better than income which has some problems; for instance, respondents may not report it accurately and it is a hard-to-measure variable (Fry et al., 2014). In addition, using a single asset variable does not give us enough information to determine a household's status (Fry et al., 2014). A wealth index is particularly useful in the context of low-income countries, since income is likely to come from diverse sources and vary over seasons while expenditure poses difficulties due to both the high price differences over time and across areas and individuals' unwillingness to disclose expenditure levels (Howe et al., 2008). We use Principal Component Analysis (PCA) to construct the wealth index.[20] One limitation of this approach is that weights on individual indicators are not theoretically grounded (Filmer and Pritchett, 2001).[21]

In Table 3.6, we estimate the main regression of the first-difference model with the entire set of control variables. We split households into quartiles of wealth index in columns (2)-(5). The coefficient of the effect of the shock on remittances is much higher in magnitude and statistically significant at the 1% level for poorer households, its magnitude also decreases gradually throughout the quartiles. Therefore, it seems that poorer households were more affected, with their remittances being reduced as a result of the embargo.

Why are Poor Households More Vulnerable?

We explore different channels that could explain why the remittances effect seems to be driven by poor households. We start by controlling for access to credit to see if the effect of the shock on remittances differs whether or not the household has access to credit. We control for access to credit using ownership of bank account pre-embargo in Panel A of Table 3.7. We control for a dummy on whether the household had a bank account in wave 1, and an interaction of this dummy with the shock variable. The

[20]The variables we include following Filmer and Pritchett (2001), when available, are: a household's ownership of consumer durables (clock/watch, bicycle, radio, TV, sewing machine, refrigerator, car, motorcycle), a household's dwelling characteristics (toilet facilities (flush toilet, pit toilet/latrine, none/other), drinking water sources (pump/well, open source, other source), rooms in dwelling (number of rooms, kitchen as a separate room), building materials including roof's and outside wall's material, availability of lightning, type of fuel used for cooking and ownership of greater-than-6-Acres land.

[21]See Filmer and Pritchett (2001) for more details on limitations, technical details and assumptions of constructing the wealth index using PCA.

Table 3.6: Shock on remittances by quartile of wealth index

	Average	Q1	Q2	Q3	Q4
DID	-0.789*	-3.397***	-1.718	-1.255	0.502
	(0.453)	(1.136)	(1.603)	(1.222)	(0.758)
Obs.	1508	315	314	314	314

Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Coping strategies interacted with the shock

	Average	Q1	Q2	Q3	Q4
A. Outcome: Remittances					
DID	-0.959	-4.778***	-2.902	-0.282	1.785
	(0.664)	(1.477)	(2.051)	(1.860)	(1.266)
Bank account	-0.380	-2.168***	0.409	0.968	0.102
	(0.369)	(0.813)	(0.945)	(0.773)	(0.907)
DID*Bank account	0.391	3.918*	3.687	-1.796	-2.116
	(0.899)	(2.294)	(3.118)	(2.460)	(1.587)
Obs.	1508	315	314	314	314
B. Outcome: Land Sale					
DID	0.000	0.001	-0.002	0.026	-0.017
	(0.007)	(0.011)	(0.009)	(0.023)	(0.018)
Obs.	1506	315	314	314	314

Robust & clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

effect is only observed for poorer households, and the ownership of the bank account reduces the severity of the shock (-4.778+3.918). Finally in Panel B, we use sale of land as an outcome variable to examine whether households cope by selling their land. As the coefficient estimate shows, there is no evidence that this is the case. Therefore, we find that poor households who had a bank account pre-embargo suffered less than their counterparts without one. One other possible explanation is that their migrants were more vulnerable compared to those from richer households, this is what we explore next.

In Panel A of Table 3.8, we look at migratory responses after the shock. This allows

Table 3.8: Shock on returned migration and number of migrants

	Average	Q1	Q2	Q3	Q4
A. Share of returned migration					
DID	0.010	0.220***	0.106	0.074	-0.136***
	(0.031)	(0.084)	(0.162)	(0.072)	(0.037)
Obs.	1316	279	268	276	274
B. Δ in number of migrants to Qatar					
DID	-0.362***	-0.570***	-0.585***	-0.450***	-0.122**
	(0.035)	(0.087)	(0.091)	(0.080)	(0.057)
Obs.	1508	315	314	314	314
C. Δ in number of migrants to Other					
DID	0.433***	0.391***	0.526***	0.488***	0.406***
	(0.040)	(0.121)	(0.095)	(0.100)	(0.064)
Obs.	1508	315	314	314	314

Robust & clustered standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

us to understand whether migrant returns differed between poor and rich households. Similar to Yang (2008), we use a measure of migrant return rate as a total of returned migrants per household post-shock divided by number of migrants pre-shock. The results show that returned migration from Qatar increased for poor households but declined for richer households after the shock. This means that poorer households are not only vulnerable given a decline in their remittances, but their migrants are also more vulnerable. In Panels B and C of Table 3.8, we look at the change in the number of migrants to Qatar and other international destinations due to the embargo, respectively. Intuitively, the number of migrants to Qatar decreased and it increased to other international destinations. Although this effect is observed on average and for all quartiles, the magnitudes differ between the rich and the poor. Particularly, the coefficient estimates suggest that although all households exposed to the shock decreased their migrants in Qatar, the magnitude is lower for richer households. Similarly, the increase in migrants to other destinations was higher for richer households. This again points out that the poor are more vulnerable and less likely to cope with the shock.

Table 3.9: Other shocks on remittances (log)

	Average	Q1	Q2	Q3	Q4
DID	-0.837*	-3.559***	-2.172	-1.224	0.430
	(0.455)	(1.156)	(1.636)	(1.230)	(0.794)
Natural disaster w1	-0.372	-0.542	-1.909**	0.765	0.412
	(0.337)	(0.796)	(0.822)	(0.820)	(0.790)
Econ. Shock w1	0.196	-0.318	1.369	-0.683	0.369
	(0.368)	(1.083)	(1.121)	(0.821)	(0.783)
Health Shock w1	-0.519	-0.796	-0.741	0.346	-1.798
	(0.464)	(1.050)	(1.168)	(1.043)	(1.195)
Other Shock w1	0.682	1.110	2.462	1.383	-1.761
	(0.819)	(1.233)	(2.427)	(1.642)	(2.299)
Obs.	1508	315	314	314	314

Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Further Robustness

Based on Walker et al. (2019), different shocks occurred during the years in which the survey was undertaken. We examine how different shocks affect our results, many of them were observed during 2014-2018 and some impacted households' well-being severely (Walker et al., 2019). The more severe and widespread shocks were in 2015 and 2016, whereas fewer and less spread shocks took place in 2017-2018 (Walker et al., 2019). Although it did not happen simultaneously at the time of the embargo, we examine whether its aftermath confounds our main coefficient estimates.[22] Following Walker et al. (2019), we categorize all reported shocks into four categories to deal with the very few observations in some of them: "natural disasters (the earthquake, floods, landslide, drought, fire, hail, lightning); agricultural shocks (pests, post-harvest loss, livestock loss); economic shocks (the blockade, price hikes, personal economic shocks); and health shocks (disease, injury, death)" (Walker et al., 2019). We report our estimated results of the first-difference model with all control variables augmented

[22]Since we run a first-difference model, the variables of other shocks are equivalent to including their pre-shock value interacted with post.

with all shocks in wave one in Table 3.9. Our coefficient estimates are negative and statistically significant both on average and for the poorest quartile. This confirms that our main results are not capturing the effect of other shocks.

3.5 Conclusion

International remittances are transfers of money migrants send to their origin households. They often consist in very small magnitudes for which they pay a large fee and are done at relatively high frequencies. Remittances are likely to help recipient households overcome credit and liquidity constraints and reduce their risk of falling into poverty. They also have the potential to have an effect on economic development. Remittances in Nepal represent over a quarter of GDP and are the highest source of received foreign income. This paper examined the effect of the unexpected embargo on Qatar in 2017 on remittances received by households in Nepal. Qatar is the second largest destination for Nepali migrants, giving us a suitable context to shed light on how shocks to migrants in host countries affect their origin households. Using the Household Risk and Vulnerability Survey in Nepal, we follow Yang (2008) and estimate a first-difference model augmented with pre-shock characteristics. By limiting our sample to households with international migrants before the embargo, we deal with the selection into migration problem. In addition, the shock affected migrants in Qatar exclusively compared to migrants in other international destinations, giving us a clean control group. We argue that these allow us to identify the causal effect of the embargo on remittances. Our estimates show that the shock resulted in a 56% fall in remittances for households with a migrant in Qatar. The estimates are robust to different checks. Moreover, we find that it is mainly the poor households who suffered from the decline in remittances. This result aligns with Walker et al. (2019) who also find that it is the poor households who are more vulnerable to shocks and are less likely to cope. Furthermore, since migrants in Qatar from poorer households were more likely to return, this suggests that the impact of poverty spills over to migrants even if they work in rich destinations. In addition to this, we show that poorer households who had access to bank accounts in the pre-embargo period could somehow mitigate the shock, since the decline in remittances was in part alleviated by this.

Our results are also informative about migration outflows. Nepali migrants seem to shift their location from Qatar, a country which experiences an embargo, to other international destinations. It is also interesting to find out the type of households that are better able to adjust their overseas destinations after the embargo. Poorer households reduce more than richer households the number of migrants they send to Qatar after the embargo but richer households are more likely to send migrants to other international destinations.

In conclusion, this study provides a contemporaneous effect of the embargo on Qatar on remittances in Nepal. If data on future periods become available, it would be worth investigating long-term impacts. This would provide us with a better understanding on how this sudden decline in remittances translated into other household outcomes. If a reduction of remittances was to affect investments in human capital and other assets, we could expect an impact on the country's economic development.

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Appendix C

Chapter 3 Appendix

C.1 Extra Background

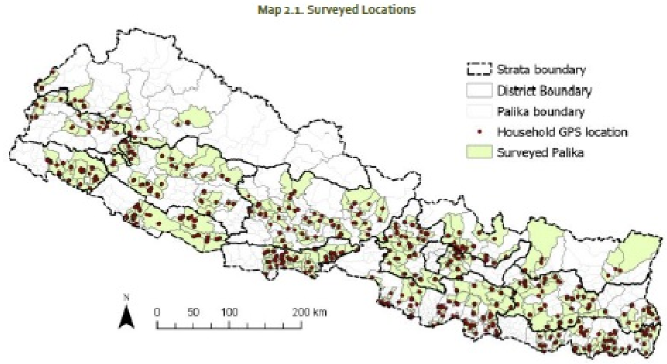
Figure C.1: Nepal surveyed districts

Table 2.1. Division of Districts by Stratum

Stratum	Districts (Bold indicates district was included in sample)	No. of PSUs*
Mountain	**Bajhang**, **Bajura**, **Darchula**, **Dolakha**, Dolpa, Humla, **Jumla**, **Kalikot**, Manang, Mugu, Mustang, Rasuwa, Sankhuwasabha, **Sindhupalchok**, **Solukhumbu**, **Taplejung**	40
Far Western Hill	**Achham**, **Baitadi**, Dadeldhura, **Doti**	18
Mid Western Hill	**Dailekh**, **Jajarkot**, Pyuthan, **Rolpa**, **Rukum**, Salyan, **Surkhet**	34
Western Hill	Arghakhanchi, **Baglung**, **Gorkha**, **Gulmi**, Kaski, **Lamjung**, **Myagdi**, **Palpa**, Parbat, **Syangja**, **Tanahu**	48
Central Hill	**Dhading**, **Kavrepalanchok**, **Makwanpur**, **Nuwakot**, Ramechhap, **Sindhuli**	48
Eastern Hill	**Bhojpur**, **Dhankuta**, **Ilam**, **Khotang**, **Okhaldhunga**, Panchthar, Terhathum, **Udayapur**	39
Far Western Terai	**Kailali**, Kanchanpur	18
Mid Western Terai	**Banke**, Bardiya, **Dang**	24
Western Terai	Kapilbastu, **Nawalparasi**, **Rupandehi**	35
Central Terai	**Bara**, Chitawan, **Dhanusa**, **Mahottari**, **Parsa**, Rautahat, **Sarlahi**	48
Eastern Terai	**Jhapa**, **Morang**, **Saptari**, Siraha, **Sunsari**	48
Total		400

* 15 households were interviewed in each PSU.

Map 2.1. Surveyed Locations

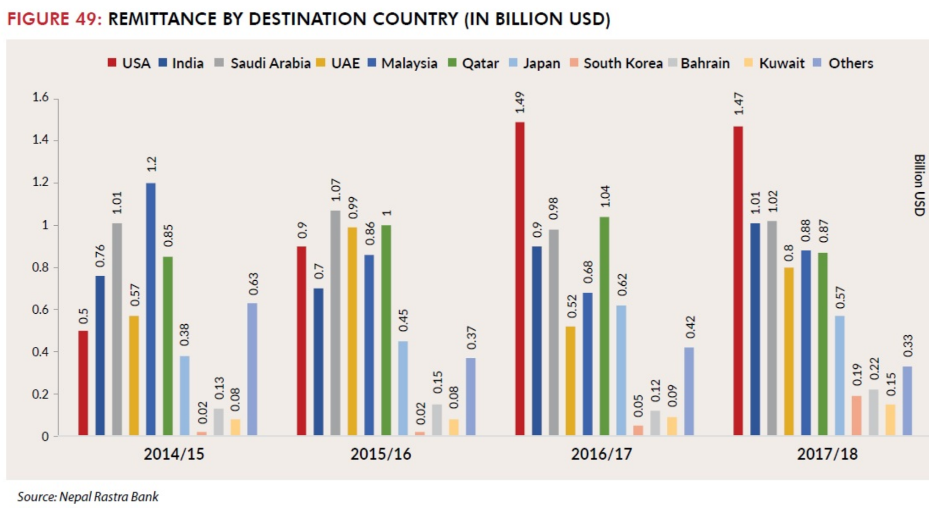


Source: Walker et al. (2019)

C.2 Extra Figures

Figure C.2 from McAuliffe and Khadria (2019) shows the disaggregation of remittance inflows to Nepal from major destination countries over the period 2014/15 to 2017/18.

Figure C.2: Remittances to Nepal by destination



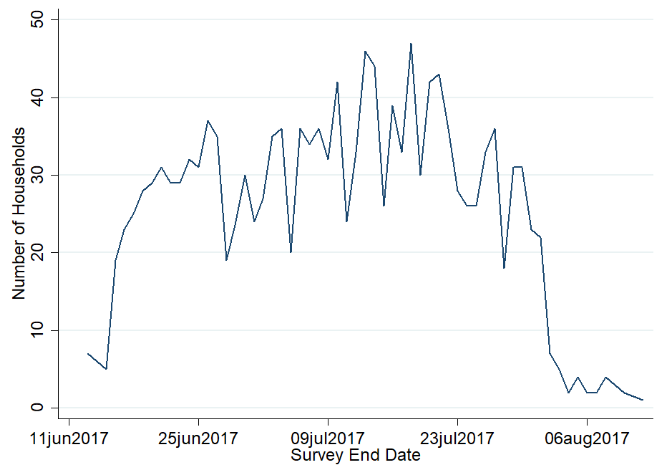
Source: Nepal Labour Migration report 2020, *Government of Nepal*, page 94.

Table C.1: Summary statistics (Qatar versus Other migrants)

Summary Statistics (wave 1)							
	Other Migrants			Qatar Migrants			
	Mean	SD	Obs.	Mean	SD	Obs.	ttest
remit	0.13	0.18	1563	0.17	0.13	260	-0.0358** (-3.06)
Months Away	24.14	38.72	1562	19.04	22.88	260	5.095* (2.06)
mig. earnings	0.02	0.03	1563	0.03	0.04	260	-0.00730** (-3.21)
female	0.07	0.26	1563	0.02	0.14	260	0.0511** (3.15)
male	0.93	0.26	1563	0.98	0.15	260	-0.0479** (-2.93)
move (marriage)	0.00	0.00	1562	0.00	0.00	260	0 (.)
move (follow family)	0.06	0.23	1562	0.00	0.00	260	0.0551*** (3.89)
move (other family reason)	0.01	0.08	1562	0.00	0.00	260	0.00576 (1.23)
move (educ)	0.03	0.17	1562	0.00	0.00	260	0.0307** (2.87)
move (training)	0.00	0.04	1562	0.00	0.00	260	0.00128 (0.58)
move (job search)	0.44	0.50	1562	0.32	0.47	260	0.124*** (3.77)
move (start new business/job)	0.49	0.50	1562	0.72	0.45	260	-0.222*** (-6.73)
move (job transfer)	0.00	0.07	1562	0.00	0.00	260	0.00448 (1.08)
move (family conflict)	0.00	0.04	1562	0.00	0.00	260	0.00192 (0.71)
move (natural disaster)	0.00	0.00	1562	0.00	0.00	260	0 (.)
move (easy lifestyle)	0.00	0.06	1562	0.00	0.00	260	0.00320 (0.91)
move (other)	0.00	0.06	1562	0.00	0.00	260	0.00384 (1.00)
remit (land purchase)	0.01	0.12	1195	0.03	0.18	238	-0.0194* (-2.09)
remit (livestock purchase)	0.02	0.15	1195	0.03	0.17	238	-0.00682 (-0.63)
remit (business farm use)	0.05	0.21	1195	0.05	0.23	238	-0.00692 (-0.45)
remit (dwelling purchase)	0.01	0.09	1195	0.00	0.06	238	0.00333 (0.56)
remit (improve dwelling)	0.09	0.28	1195	0.07	0.26	238	0.0148 (0.75)
remit (marriage/funeral/ceremony)	0.05	0.21	1195	0.03	0.17	238	0.0175 (1.20)
remit (migration)	0.01	0.07	1195	0.02	0.13	238	-0.0118* (-2.00)
remit (repay debt/interest)	0.32	0.47	1195	0.40	0.49	238	-0.0803* (-2.40)
remit (other)	0.01	0.12	1195	0.02	0.13	238	-0.00258 (-0.30)

Notes: this sample is based on individual-level data and corresponds to our sample of main estimation at household level. It follows international migrants (mig) only, looking at their wave 1 pre-embargo characteristics. Other migrants include international migrants in countries other than Qatar. Remittances are reported in million Nepali Rupees. For the t-test, we report the mean, and t-test of difference in parenthesis.

Figure C.3: Number of surveyed households over time (wave 2)



Source: Authors' computations using wave 2 of the Household Risk and Vulnerability Survey in Nepal.

C.3 Extra Results

Table C.2: Main estimation with no control variables

	(1)	(2)	(3)	(4)
Dep. Var	lremit	lremit	D.lremit	D.lremit
DID	-0.834*	-0.834*	-0.834*	-0.834*
	(0.459)	(0.453)	(0.451)	(0.453)
QatMigHH	1.023***	1.023***	-	-
	(0.246)	(0.253)	-	-
post	-1.629***	-1.629***		
	(0.197)	(0.199)		
Observations	3016	3016	1508	1508
Linear DID Model	Yes	Yes	No	No
FD Model	No	No	Yes	Yes

Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

DID is the $QatMig_J * post_t$ variable, an interaction of having a migrant in Qatar present in a household in pre-embargo period interacted with post-embargo period. All of the control variables are fixed at pre-embargo period value (i.e. wave 1). Households' controls: religion, size & presence of members below 16 years of age; heads' controls: male, age & marital status; migrants' controls (household average): age, months away & dummy for expectation of return. Columns (1) and (3) report robust standard errors, while Columns (2) and (4) report standard errors clustered at VDC (village development committee) level, which is the PSU in the survey. There are 371 VDC in our estimated sample.

Table C.3: Main estimation with household control variables

Dep. Var	(1) lremit	(2) lremit	(3) D.lremit	(4) D.lremit
DID	-0.834*	-0.834*	-0.836*	-0.836*
	(0.460)	(0.453)	(0.452)	(0.457)
QatMigHH	1.010***	1.010***	-	-
	(0.247)	(0.253)	-	-
post	-1.629***	-1.629***		
	(0.197)	(0.199)		
pre-Muslim	4.355	4.355***	-9.376***	-9.376***
	(4.637)	(0.714)	(1.068)	(0.941)
pre-Hindu	3.443	3.443***	-13.465***	-13.465***
	(4.578)	(0.167)	(0.234)	(0.254)
pre-Buddhist	4.369	4.369***	-13.956***	-13.956***
	(4.586)	(0.345)	(0.518)	(0.483)
pre-Christian	2.578	2.578***	-13.532***	-13.532***
	(4.639)	(0.822)	(1.471)	(1.129)
Pre-Kirant	2.714	2.714***	-13.457***	-13.457***
	(4.640)	(0.742)	(1.441)	(1.454)
pre-size	-0.038	-0.038	0.276***	0.276***
	(0.046)	(0.052)	(0.091)	(0.090)
pre-below-16-dum	0.080	0.080	-1.476***	-1.476***
	(0.243)	(0.263)	(0.450)	(0.448)
Observations	3016	3016	1508	1508
Linear DID Model	Yes	Yes	No	No
FD Model	No	No	Yes	Yes

Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

DID is the $QatMig_J * post_t$ variable, an interaction of having a migrant in Qatar present in a household in pre-embargo period interacted with post-embargo period. All of the control variables are fixed at pre-embargo period value (i.e. wave 1). Households' controls: religion, size & presence of members below 16 years of age; heads' controls: male, age & marital status; migrants' controls (household average): age, months away & dummy for expectation of return. Columns (1) and (3) report robust standard errors, while Columns (2) and (4) report standard errors clustered at VDC (village development committee) level, which is the PSU in the survey. There are 371 VDC in our estimated sample.

Table C.4: Main estimation with household's head control variables

Dep. Var	(1) lremit	(2) lremit	(3) D.lremit	(4) D.lremit
DID	-0.834*	-0.834*	-0.809*	-0.809*
	(0.458)	(0.453)	(0.453)	(0.457)
QatMigHH	1.028***	1.028***	-	-
	(0.245)	(0.251)	-	-
post	-1.629***	-1.629***		
	(0.196)	(0.199)		
pre-Head-male	-1.006***	-1.006***	0.891**	0.891**
	(0.237)	(0.265)	(0.435)	(0.439)
pre-Head-age	0.034***	0.034***	-0.005	-0.005
	(0.009)	(0.010)	(0.016)	(0.016)
pre-Head-married	0.490	0.490	-0.290	-0.290
	(1.428)	(1.214)	(3.331)	(3.352)
pre-Head-widow	-0.595	-0.595	-0.227	-0.227
	(1.477)	(1.283)	(3.410)	(3.374)
pre-Head-divorce	0.636	0.636	1.409	1.409
	(1.685)	(1.616)	(3.609)	(3.614)
Observations	3016	3016	1508	1508
Linear DID Model	Yes	Yes	No	No
FD Model	No	No	Yes	Yes

Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

DID is the $QatMig_J * post_t$ variable, an interaction of having a migrant in Qatar present in a household in pre-embargo period interacted with post-embargo period. All of the control variables are fixed at pre-embargo period value (i.e. wave 1). Households' controls: religion, size & presence of members below 16 years of age; heads' controls: male, age & marital status; migrants' controls (household average): age, months away & dummy for expectation of return. Columns (1) and (3) report robust standard errors, while Columns (2) and (4) report standard errors clustered at VDC (village development committee) level, which is the PSU in the survey. There are 371 VDC in our estimated sample.

Table C.5: Main estimation with migrant control variables

	(1)	(2)	(3)	(4)
Dep. Var	lremit	lremit	D.lremit	D.lremit
DID	-0.834*	-0.834*	-0.755*	-0.755*
	(0.457)	(0.453)	(0.452)	(0.456)
QatMigHH	0.937***	0.937***	-	-
	(0.242)	(0.246)	-	-
post	-1.629***	-1.629***		
	(0.196)	(0.199)		
pre-mig-age	0.049***	0.049***	-0.063***	-0.063***
	(0.010)	(0.012)	(0.019)	(0.019)
pre-mig-month-away	0.002	0.002	-0.018***	-0.018***
	(0.003)	(0.004)	(0.007)	(0.007)
pre-mig-back-yes	1.047*	1.047	-2.535***	-2.535***
	(0.600)	(0.775)	(0.875)	(0.879)
Observations	3016	3016	1508	1508
Linear DID Model	Yes	Yes	No	No
FD Model	No	No	Yes	Yes

Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

DID is the $QatMig_J * post_t$ variable, an interaction of having a migrant in Qatar present in a household in pre-embargo period interacted with post-embargo period. All of the control variables are fixed at pre-embargo period value (i.e. wave 1). Households' controls: religion, size & presence of members below 16 years of age; heads' controls: male, age & marital status; migrants' controls (household average): age, months away & dummy for expectation of return. Columns (1) and (3) report robust standard errors, while Columns (2) and (4) report standard errors clustered at VDC (village development committee) level, which is the PSU in the survey. There are 371 VDC in our estimated sample.

Table C.6: Main estimation with all control variables

Dep. Var	(1) lremit	(2) lremit	(3) D.lremit	(4) D.lremit
DID	-0.834*	-0.834*	-0.789*	-0.789*
	(0.457)	(0.454)	(0.453)	(0.464)
QatMigHH	0.956***	0.956***	-	-
	(0.244)	(0.245)	-	-
post	-1.629***	-1.629***		
	(0.196)	(0.200)		
pre-Muslim	3.366	3.366***	-8.572***	-8.572***
	(4.647)	(0.756)	(1.106)	(0.998)
pre-Hindu	2.426	2.426***	-12.601***	-12.601***
	(4.589)	(0.315)	(0.447)	(0.451)
pre-Buddhist	3.281	3.281***	-13.024***	-13.024***
	(4.597)	(0.446)	(0.651)	(0.604)
pre-Christian	1.530	1.530*	-12.084***	-12.084***
	(4.651)	(0.924)	(1.491)	(1.232)
Pre-Kirant	1.593	1.593**	-12.720***	-12.720***
	(4.650)	(0.766)	(1.530)	(1.537)
pre-size	-0.024	-0.024	0.234**	0.234**
	(0.052)	(0.056)	(0.100)	(0.098)
pre-below-16-dum	-0.106	-0.106	-1.104**	-1.104**
	(0.257)	(0.278)	(0.472)	(0.471)
pre-Head-male	-0.425	-0.425	-0.503	-0.503
	(0.289)	(0.301)	(0.539)	(0.551)
pre-Head-age	0.026***	0.026**	0.005	0.005
	(0.009)	(0.010)	(0.017)	(0.016)
pre-Head-married	0.903	0.903	-1.112	-1.112
	(1.409)	(1.160)	(3.497)	(3.528)
pre-Head-widow	0.191	0.191	-1.667	-1.667
	(1.468)	(1.239)	(3.592)	(3.590)
pre-Head-divorce	1.427	1.427	0.023	0.023
	(1.674)	(1.576)	(3.778)	(3.800)
pre-mig-age	0.043***	0.043***	-0.062***	-0.062***
	(0.013)	(0.014)	(0.023)	(0.023)
pre-mig-month-away	0.002	0.002	-0.017***	-0.017**
	(0.003)	(0.003)	(0.006)	(0.007)
pre-mig-back-yes	1.097*	1.097	-2.489***	-2.489***
	(0.594)	(0.760)	(0.859)	(0.866)
Observations	3016	3016	1508	1508
Linear DID Model	Yes	Yes	No	No
FD Model	No	No	Yes	Yes

Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

DID is the $QatMig_J * post_t$ variable, an interaction of having a migrant in Qatar present in a household in pre-embargo period interacted with post-embargo period. All of the control variables are fixed at pre-embargo period value (i.e. wave 1). Households' controls: religion, size & presence of members below 16 years of age; heads' controls: male, age & marital status; migrants' controls (household average): age, months away & dummy for expectation of return. Columns (1) and (3) report robust standard errors, while Columns (2) and (4) report standard errors clustered at VDC (village development committee) level, which is the PSU in the survey. There are 371 VDC in our estimated sample.

C.4 Robustness Checks

Sampling Weights

Table C.7: Main results with sampling weights

	Linear DID		FD	
	(1)	(2)	(3)	(4)
	Coefficient	SE	Coefficient	SE
Panel A:				
No controls	-0.643	(0.442)	-0.643	(0.432)
Obs.	3016		1508	
Panel B:				
Household's Controls	-0.643	(0.442)	-0.698	(0.433)
Obs.	3016		1508	
Panel C:				
Head's Controls	-0.643	(0.441)	-0.616	(0.434)
Obs.	3016		1508	
Panel D:				
Migrants' Controls	-0.643	(0.439)	-0.620	(0.432)
Obs.	3016		1508	
Panel E:				
All Controls	-0.643	(0.439)	-0.683	(0.434)
Obs.	3016		1508	

Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Given the survey design, we mentioned that our results are not representative of the entire Nepali population. Therefore, survey weights become irrelevant since our data cannot be representative of the Nepali population in non-metropolitan areas. Furthermore, since the sampling design in the survey involved the use of stratified sampling with the sample of each strata selected using probability proportional to size, it is self-weighted so no weighing is required (Mckenzie, 2009). We do not face any non-response problem ruling out the use of sampling weights to deal with that issue (Mckenzie, 2009). The only reason sampling weights may be used in our context is to test whether the model is misspecified in presence of heterogeneous effects (Solon et al., 2015).

We re-estimate the models after accounting for sampling weights. Replicating the results of our main specifications in Table C.7, we can see that the coefficient estimates and standard errors from weighted regressions are not drastically different even if the coefficient estimate loses its statistical significance. Therefore, we argue that the similarity of our coefficients increases our confidence that there is no model misspecification.[1]

IHS Transformation

The Inverse Hyperbolic Sine transformation is an approximate of the log transformation but has the advantage of retaining zero observations (Bellemare and Wichman, 2020). Specifically, IHS transforms our variable y into:

$$\tilde{y} = arcsinh = ln(y + (\sqrt{(y^2 + 1})$$

The estimated coefficients reported in Table C.8 confirm our main results. Given that we are dealing with a dummy independent variable, the transformation to obtain the elasticity of remittances for a switch of our interaction dummy variable (DID) from 0 to 1 is very similar to our main coefficient estimates (Bellemare and Wichman, 2020). Therefore, there is no substantial difference in coefficient estimates between using the log or IHS transformation.

[1]Lee and Solon (2011) compare coefficient estimates of OLS and WLS (weighted least squares) regressions, their results show contradicting coefficient estimates in terms of sign or ones that are very different in terms of magnitude to support their conclusion that the difference between the two points to model misspecification. Whereas in our case the coefficients are negative in both models and very close in terms of magnitude: -0.83 and -0.64 in OLS and WLS, respectively.

Table C.8: Main Results using IHS instead of log transformation

	(1)	(2)
Dep. Var.	IHS_remit	D.IHS_remit
Panel A: No controls		
DID	-0.882*	-0.834*
	(0.485)	(0.451)
Obs.	3016	1508
Panel B: Household's Controls		
DID	-0.834*	-0.836*
	(0.460)	(0.452)
Obs.	3016	1508
Panel C: Head's Controls		
DID	-0.834*	-0.809*
	(0.458)	(0.453)
Obs.	3016	1508
Panel D: Migrants' Controls		
DID	-0.834*	-0.755*
	(0.457)	(0.452)
Obs.	3016	1508
Panel E: All Controls		
DID	-0.834*	-0.799*
	(0.457)	(0.453)
Obs.	3016	1508
Linear DID Model	Yes	No
FD Model	No	Yes

Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Drop Blockading Countries

Table C.9: Main results, blockading countries dropped

	Linear DID		FD	
	(1)	(2)	(3)	(4)
	Coefficient	SE	Coefficient	SE
Panel A:				
No controls	-1.073**	(0.471)	-1.073**	(0.459)
Observations	2402		1201	
Panel B:				
Household's Controls	-1.073**	(0.472)	-1.090**	(0.457)
Observations	2402		1201	
Panel C:				
Head's Controls	-1.073**	(0.469)	-1.073**	(0.469)
Observations	2402		2402	
Panel D:				
Migrants' Controls	-1.073**	(0.469)	-0.946**	(0.465)
Observations	2402		1201	
Panel E:				
All Controls	-1.073**	(0.469)	-0.989**	(0.463)
Observations	2402		1201	

Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Propensity Score Matching

We apply a matching procedure that matches individuals in the treatment group to those in the control group based on observable covariates to ensure the only difference between the two is whether they have a migrant in Qatar or not (Ferraresi et al., 2018). The advantage of matching before implementing the difference-in-difference estimator (DID) is to have a treatment and a control group that are similar not only in trends but also in levels at the pre-treatment level (Mckenzie, 2021).

There are different matching estimators, but we limit our analysis to non-parametric DID Kernel estimator as it addresses not only differences between treatment and control groups but also potential differences in the distributions within each group (Ferraresi et al., 2018). The Kernel matching estimator matches each individual treated unit to all control units, down-weighting the distant observations (Heckman et al., 1998). We estimate the following equation on the common support:[2]

$$\gamma^{DID} = \sum_{i \in QA} \{[Y_{it1} - Y_{it0}] - \sum_{j \in NQA} W_{ij}[Y_{jt1} - Y_{jt0}]\} w_i \tag{C.1}$$

Where t_0 and t_1 are time periods before and after the embargo, QA are households with migrants in Qatar that will be affected post-embargo and NQA are households with migrants in other international destinations that will not be affected post embargo. W_{ij} is the weight assigned to the counterfactual control unit j for a given treated unit i. Y is the remittances of households and w_i is the reweighting that is used to reconstruct the distribution of remittances in the treated group to match the control group's distribution. We start by estimating a propensity score $p(x)$, which is the probability of an individual being assigned into the treatment group given their observed covariates X (Caliendo and Kopeinig, 2008). The variables used for the calculation of $p(x)$ must influence both the participation decision and outcome but should not be affected by the participation or its anticipation, so they should be either measured before intervention or fixed over time (Caliendo and Kopeinig, 2008). Having identified our variables on this basis in the main regression, we then want to identify how different they are between the treated and control groups. We have already done this in the summary statistics and complemented it with a t-test in Table 3.3. However, one issue is that we do not know the units that these variables are measured in, so we are unable to identify which variables have a major difference between treated and control groups (Lunt, 2014). In addition to that, Lunt (2014) mentions that significance tests are dependent on the sample size and are not indicative of the extent of difference between treated and control groups. To deal with this, he recommends looking at the difference in standard deviation, i.e. standardized differences that are shown in Table C.10. The

[2]Heckman et al. (1998) cited from Ferraresi et al. (2018).

Table C.10: Standardized differences for unmatched sample

Var.	Mean in Treated	Mean in Untreated	Standardized diff.
Muslim HH	0.03	0.01	0.129
Christian HH	0.01	0.02	-0.039
Hindu HH	0.88	0.87	0.022
Buddhist HH	0.06	0.08	-0.069
Kirant HH	0.02	0.02	-0.012
HH Size	4.72	4.80	-0.039
members below 16	0.82	0.79	0.081
Head Male	0.58	0.62	-0.067
Head Age	48.05	49.22	-0.083
Head Married	0.87	0.87	-0.002
Head Widowed	0.11	0.12	-0.020
Head Divorced	0.01	0.01	-0.024
Age (mig)	30.66	29.43	0.143
Months Away (mig)	19.05	23.54	-0.156
Expect Return (mig)	1.00	0.97	0.266

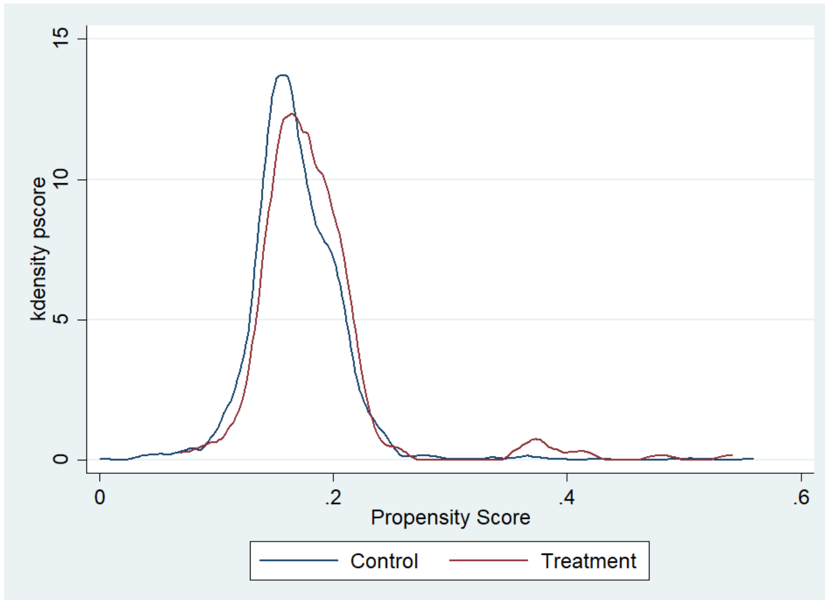
Notes: we only include households with international migrants (mig) observed in waves 1 and 3. Migrants are defined pre-embargo and includes overseas migrants only, we drop households with mixed-migrant types (i.e. Qatar and other international destinations). All variables are reported for wave 1 (pre-emabrgo). We include household level and household's head variables. Migrant variables are the average of all migrants within a given households.

smaller the standardized difference between treated and untreated units for a given variable indicates that this variable is more similar across the two groups compared to other variables. A serious issue of imbalance is indicated by more than 20% difference (Rosenbaum and Rubin, 1985). All of our variables are different by less than 20% except for migrant returns. We check the standardized difference after the matching procedure in Table C.11, in which all variables have a standardized difference below 20%.

We estimate the propensity score using a logit model where we regress the dependent variable that indicates the presence of a migrant in Qatar on the variables specified in Table C.10, for the same sample of overseas, non-mixed migrant households in the main analysis. We look at the distributions of propensity score in the treated and control groups in Figure C.4. This step is to check the overlap assumption of the propensity score (Cameron and Trivedi, 2005):

$$0 < pr(D = 1|X) < 1$$

Figure C.4: Density of propensity score of treated and control groups



It ensures that people with the same X values have a positive probability of being both in treated and control groups (Caliendo and Kopeinig, 2008). If the assumption fails then there is no overlap between treatment and control groups and we have individuals with a given X who are all in the treatment group and individuals with another X who are all in the control group (Cameron and Trivedi, 2005).

Unfortunately, there is no way to check whether included variables are correct or whether some important variables are omitted. However, we can check if the functional form of our regression equation is wrong. To do that we use the Hosmer-Lemeshow test where the null hypothesis is that the model fits the data. We obtain a Hosmer-Lemeshow Ch2(8) of 6.71 with a p-value of 0.569. Therefore, we fail to reject the null hypothesis and conclude that our functional form is not misspecified and the logit model fits the data well. We use the computed propensity score in our non-parametric Kernel weighted DID and find a coefficient estimate of -0.938 (clustered SE: 0.461) that is statistically significant at the 5% significance level. It is very close to estimates found in our main analysis using a simple difference-in-difference technique. This gives us further evidence that the observed negative effect on remittances for households with migrants in Qatar is due to the shock.

Table C.11: Standardized differences for matched sample

Var.	Mean in Treated	Mean in Untreated	Standardized diff.
Muslim HH	0.03	0.01	0.125
Christian HH	0.01	0.01	-0.026
Hindu HH	0.88	0.88	0.014
Buddhist HH	0.06	0.08	-0.070
Kirant HH	0.02	0.02	-0.005
HH Size	4.72	4.81	-0.042
members below 16	0.82	0.79	0.076
Head Male	0.58	0.62	-0.067
Head Age	48.05	49.14	-0.077
Head Married	0.87	0.87	0.001
Head Widowed	0.11	0.12	-0.024
Head Divorced	0.01	0.01	-0.021
Age (mig)	30.66	29.45	0.141
Months Away (mig)	19.05	21.17	-0.088
Expect Return (mig)	1.00	1.00	.

Notes: we only include households with international migrants (mig) observed in waves 1 and 3. Migrants are defined pre-embargo and includes overseas migrants only, we drop households with mixed-migrant types (i.e. Qatar and other international destinations). All variables are reported for wave 1 (pre-emabrgo). We include household level and household's head variables. Migrant variables are the average of all migrants within a given households.