LUND UNIVERSITY

SCHOOL OF ECONOMICS AND MANAGEMENT

Using evidence from the embargo against Qatar to estimate sanctions' trade effects

by

Fredrik Prinz Blix

October 2021

Master in Economics

Supervisor: Maria Persson
Master Thesis II

Abstract

While trade policies have been found to promote peace and international cooperation, they are also used in conflicts to punish or coerce the actions and behavior of one's trading partners. In a globalized world characterized by interconnectivity and complex supply and value chains, their impact and relevance in political conflicts have become even more apparent. Such contemporary developments are reflected in the embargo against Qatar by Bahrain, Saudi Arabia, and United Arab Emirates (UAE) (the Tripartite) in 2017. Halting essentially all trade to/from Qatar in addition to banning Qatari traffic from using Tripartite land-, sea, and airspace, it marks an unprecedented regional event that is seldom observed. As a consequence, it warrants an investigation as to how impactful the embargo actually was on Qatari-Tripartite bilateral trade flows.

In this paper, I quantify and analyze the trade impact brought by the embargo against Qatar over the period 1995 to 2019 using the synthetic control method (SCM) to estimate how trade flows would have developed in the absence of the embargo. Actual trade flows indicates that Qatari trade with the Tripartite-countries dropped to autarky-like levels whereas the constructed synthetic estimates suggests that bilateral trade flows would have been substantially higher had the embargo not been imposed. Moreover, the prevalence of anticipatory and spillover effects indicates that the effect of the embargo was extensive in terms of both time and space. A number of placebo and lag tests shows however that the credibility and accuracy of the synthetic estimates is questionable.

Keywords: Synthetic control method, placebo, lags, embargo, international trade, Qatar, MENA

Acknowledgements

I would like to extent my deepest and greatest gratitude to my supervisor, Maria Persson, for her expertise, guidance, and support during the writing process and completion of this thesis. Although I will never be equal to her level of academic expertise and professionalism, I hope I will be able to emulate it one day.

Table of Contents

1 Introduction 1
2 Economic Sanctions and Embargoes – Overview and the Case of Qatar 4
2.1 The Economic Implications and Efficacy of Sanctions and Embargoes 4
2.2 The Embargo Against Qatar 7
3 Empirical Strategy and Data 13
3.1 Introducing the SCM 13
3.2 The Econometric Setting of the SCM 14
3.3 Application of the SCM to the Case of the Qatari Embargo 17
4 Results and Discussion 21
4.1 The SCM Results 21
4.2 Sensitivity Analysis and Robustness Test 27
4.2.1 In-Time Placebo 28
4.2.2 In-Place Placebo 30
4.2.3 Lag Robustness Test 32
4.3 The SCM Results – Interpretation and Discussion 35
4.4 The Relevance of Ex Ante and Ex Post Factors 38
4.5 The SCM and its (in)abilities 40
5 Conclusion 43
6 Appendix A 58
7 Appendix B 59
8 Appendix C 60

List of Tables

Table 1. List of the countries involved in the embargo. 10
Table 2. Synthetic control group composition for the bilateral trade analysis 61
Table 3. Percentage difference between actual and synthetic trade flows between Qatar and the Tripartite-countries 26

List of Figures

Figure 1: Regional overview of the countries involved in the embargo. Note that the Quartet refers to all the embargoing countries, including Egypt. 10
Figure 2: Qatar Airways air traffic from Doha, Qatar, before (to the left) and after the imposition of the embargo (to the right). Source: FlightRadar24. 11
Figure 3: Two-way average bilateral trade flows between Qatar and the UAE vs their synthetic estimators. 22
Figure 4: Two-way average bilateral between Qatar and Bahrain vs their synthetic estimators. 22
Figure 5: Two-way average bilateral trade flows between Qatar and Saudi Arabia vs their synthetic estimators. 22
Figure 6: Figure 6: Two-way average bilateral trade flows between Qatar and the Tripartite vs their synthetic estimators using only the Gulf-states, Turkey, and Jordan as control units. 24
Figure 7: Two-way average bilateral trade flows between Qatar and the Tripartite and their synthetic estimators using the treated unit's pre-treatment trade average x/- one standard deviation as control units..... 25
Figure 8: Two-way average bilateral trade between Qatar and the UAE and their synthetic counterfactual with false embargo backdated to 2013. 29
Figure 9: Two-way average bilateral trade between Qatar and Bahrain and their synthetic counterfactual with false embargo backdated to 2013. 29
Figure 10: Two-way average bilateral trade between Qatar and Saudi Arabia and their synthetic counterfactual with false embargo backdated to 2013..... 30
Figure 11: Average bilateral two-way trade (billions of US$) between Qatar and its in-place placebo-treated trade units. 32
Figure 12: Different lag structures for the two-way average bilateral trade between Qatar and the UAE..... 33
Figure 13: Different lag structures for the two-way average bilateral trade between Qatar and Bahrain. 34
Figure 14: Different lag structures for the two-way bilateral average trade between Qatar and Saudi Arabia..... 34

1 Introduction

The relations and interactions between states has long been shaped and formed by the use of trade policies. It has been shown that trade agreements can strengthen relations between countries and an extensive literature in political science has studied the role of international trade in promoting cooperation and peace between countries (see e.g., Gartzke et al., 2001; Barbieri, 2002; Li and Reuveny, 2011; Massoud and Magee, 2012). At the same time, the use of trade policies in the case of conflict have grown in both significance and frequency since the end of the Cold War (van Bergeijk, 1995). This manifests itself in countries' use of boycotts, embargoes, and sanctions as instruments to coerce or punish specific actions or behavior among its trading partners. Their prevalence in international disputes stems from the decreasing tolerance for violence and militarized action between states as a mean to resolve conflicts (Pinker, 2011). Parallel to this, the removal of the superpower structure between the U.S. and the Soviet Union and increased levels of cooperation improved the possibility to create large coalitions of countries. The imposition of trade measures is thus made more impactful while making it more difficult for the targeted country to avoid their adverse effects on its trade and economy as a whole (van Bergeijk, 1995).

The proliferated use of trade policies in conflicts has also been due to the globalization process (van Bergeijk, 1995). In its wake, international trade has surged and progressed from a simple exchange in final goods to an intricate system of global value chains, worldwide logistical networks, and shared production among firms (Heilmann, 2016). Trade policies' disruptions or obstruction to these complex systems thus makes them potentially even more harmful for those targeted by them. The interconnection between states through these systems also makes it plausible that the adverse effects of trade measures spreads to non-targeted trading partners. According to a survey by The Economist, senior supply-chain, and executive procurement executives consider geopolitical factors as having the most risk to negatively impact supply chains up to 2025 (The Economist, 2021a). Indeed, the importance of trade policies in (geo)political conflicts at large have been highlighted by the Trump administration's trade war with China in 2018 (Amiti et al, 2019), sanctions and countersanctions between the EU and Russia over the latter's annexation of Crimea in 2014 (Crozet and Hinz, 2020), and Chinese

boycotts of Japanese goods over the disputed Senkaku/Diaoyu islands in 2012 (Heilmann, 2016).

The importance of trade policies in political conflicts is further reflected in the embargo against Qatar by Bahrain, Saudi Arabia, and the United Arab Emirates (UAE) in 2017. Qatar's alleged support to terrorist groups, interference in the domestic affairs of the other countries, and its incompliance with Gulf Cooperation Council (GCC)[1] agreements was cited as the main reasons for its imposition (Sciutto and Herb, 2017). The embargo was implemented with an immediate effect and essentially halted all imports and exports, banned all traffic with Qatar from using their land-, sea- and airspace in addition to other measures. Although diplomatic rifts have occurred in the past amongst GCC-members, the dispute lacked precedence and was the worst since the GCC:s formation in 1981 (Wintour, 2017). In light of the unprecedented conflict and the comprehensiveness of the embargo, it renders the question: what was its impact on Qatari trade flows with Bahrain, Saudi Arabia, and the UAE?

This paper therefore aims to answer this question by quantifying and analyzing the impact of the embargo against Qatar on international trade with Bahrain, Saudi Arabia, and the UAE and thus to learn more about how political conflicts impact trade relationships. To achieve this, I employ the synthetic control method (SCM), applied to the three treated units Qatar-Bahrain, Qatar-Saudi Arabia, and Qatar-United Arab Emirates (UAE) and 203 (potential) comparison units over the period 1995 to 2019. By using the weighted average of the identified control units' outcomes, I construct synthetic counterfactuals to the treated units that display the evolution of Qatari trade flows with Bahrain, Saudi Arabia, and the UAE had the embargo not been imposed. Additionally, different sets of the (potential) control units together with placebo and lag sensitivity tests are used to check the robustness of the results. Notice that although Egypt was also one of the embargoing-countries, it is not an GCC-member as the remaining three countries are. Being an outlier relative to the other treated units in this regard, it will not be included in this analysis. For the remainder of this paper, the term *Tripartite* will be used when referring to Bahrain, Saudi Arabia, and the UAE.

[1] The GCC is a political and economic alliance that was establish between the six Gulf states (Bahrain, Kuwait, Oman, Qatar, Saudi Arabia, and the UAE) in 1981 (Britannica, 2021).

In addition, this paper provides several novel contributions to the existing literature on sanctions, embargoes, and their respective impact on trade flows. To the best of my knowledge, no study has so far tried to quantify the trade effects brought by the embargo against Qatar. Further, the unexpectedness and comprehensiveness of the embargo allows me to study a regional event that is seldom observed. Its ensuing effects on trade may thus provide regional insights as to how embargoes affect trade flows, especially in a world characterized by interconnectivity and well-integrated supply chains. Further, the SCM is a relatively nascent methodological edition to international economic studies and its application has so far been limited in scope. This paper thus provides an opportunity to further test its applicability in international economics and specifically when studying the trade effect of embargoes.

Although the performance and accuracy of the synthetic estimates is arguably questionable, the results nonetheless indicate that Qatari trade flows with Bahrain, Saudi Arabia, and the UAE would have been substantially higher in the absence of the embargo. Actual trade flows relative to their synthetic estimates suggests that bilateral trade flows decreased by 79, 88, and 66 percent respectively across the post-embargo period. Further, the negative effect on trade got amplified over time indicating an inverse relationship between bilateral trade flows and the duration of the embargo. The placebo and sensitivity analyses indicate that both anticipatory and spillover effects are prevalent which points towards that the trade impact of the embargo was more extensive than anticipated.

The remainder of the paper proceeds as followed. The next section introduces the factors that determines the efficacy of an embargo in addition to the background to the embargo against Qatar. Section three presents the econometric setting of the SCM and its application to Qatari embargo whereas the empirical results are presented and discussed in section six. Section seven concludes.

2 Economic Sanctions and Embargoes – Overview and the Case of Qatar

In this section, I provide background information on the economic implications of embargoes, their enforcement mechanisms, and other factors that determines their economic impact. Finally, I outline the events leading up to and occurring during the embargo as well as their immediate consequences.

2.1 The Economic Implications and Efficacy of Sanctions and Embargoes

In the political integration of states, economic sanctions are a recurring and regular feature that is a used as tool in international diplomacy. Most commonly are the deployment of negative sanctions[2], i.e., sanctions with the purpose of inflicting damage on the target country's economy. This comprises measures such as boycotts, embargoes, and sanctions (Caruso, 2003). Since this paper investigates the trade effect of the embargo against Qatar, precedence will be given to this particular economic sanction. An embargo refers to the comprehensive or targeted trade ban with a target[3] country where the former refers to the broad-based restriction and prohibition of trade with an entire country while the latter restricts and prohibits trade with a target country in/with specific sectors, firms and/or goods (Brooks, 2002; Hufbauer and Oegg, 2000; Caruso, 2003). By design, embargoes are deployed to reduce the target country's gains from trade by deteriorating its terms of trade and therefore induce lower welfare. The conventional motivation behind the imposition of embargoes is to inflict welfare losses and coerce the government of the targeted country to seize actions or behavior that caused the sender country to impose the embargo in the first place. The negative impact on the target country's economy is commonly assumed to be positively related to the level of economic integration

[2] Positive sanctions, such as various types of cooperation and aid, are also used as a mean to influence or otherwise change the behavior of states (see e.g. Baldwin, 1971).
[3] A sender country is the country that imposes the embargo, while a target country is the country that is hit by the embargo.

with the embargoing country, i.e. the more economically integrated the countries are, the greater the negative impact will be (Caruso, 2003).

The efficacy of an embargo to inflict economic harm derives from a number of factors. One is the sender countries' ability to enforce the application of the embargo through the prevention of defections by private actors (*sanction-busting*) and nation-states (*backsliding*) (Drezner, 2000; Caruso, 2003). Sanction-busting refers to the risk that private[4] actors in the sender countries may be incentivized to evade the embargo and devise relationships in other third countries to continue trading, although illicitly, with the target country. Firms may achieve this by relocating their operations to countries where commercial exchanges with the target country is still allowed or by using offshore locations. The extent and degree of this illicit trade depends on how reliant various sectors are on trade with the target country, the level of abnormal profits firms can make, and the enforcement capacity of the sender countries (Bapat and Morgan, 2003; Drezner, 2000). Similar to sanction-busting, backsliding is when a country defects from an embargoing coalition of countries as the economic costs or political pressure outweighs the gains of continuing the embargo (see e.g. Naylor, 2001)[5]. In general, the more private actors engage in illicit activities, the larger the pressure will be politically on the embargoing government to legalize such activities and ultimately end the embargo (Drezner, 2000).

Another possibility is that the target country may mitigate or even advert the loss of trade by deepening trade ties with third countries or promote business links with third-country firms that are not subject or affected by the embargo. This is referred to as *trade deflection* or *trade diversion* (Haidar, 2017). Provided that sanction-busting, backsliding and trade deflection is great enough, the reductions in trade and the subsequent welfare loss may be limited on the part of the target country. Given this, the objective of the embargo that is pursued by the sender countries may be undermined and possibly even unachievable (Caruso, 2003; Drezner, 2000; Early, 2009).

One of the key challenges for the sender country is therefore to use its enforcement powers in such a way that domestic actors are induced to follow the imposed embargo laws (Bapat and Morgan, 2003). This is usually achieved through export licenses where only firms and

[4] Private actors are considered non-country entities, notably firms and individuals.
[5] Similarly, the Jeffersonian embargo in 1807-1809 in the U.S. was not removed because of large scale smuggling but rather due to growing domestic opposition to it (Irwin, 2005).

individuals that has been granted a license is exempted from the embargo (Caruso, 2003). In addition, government regulations may require financial institutions and other regulated entities to disclose any transactions by firms and individuals that infringe on an embargo. Other supporting measures, notably the threat of financial penalties and criminal prosecutions, are put in place to ensure compliance and penalize violations (Early and Preble, 2020). For example, through the Iran and Libya Sanctions Act of 1996, American firms and individuals were disincentivized to engage in business with the two targets as they faced prosecution if they assisted in enhancing Libya's and Iran's aviation and petroleum resources (Bapat and Morgan, 2003). Further, the sender country can impose *secondary sanctions* in which sanctions, such as embargoes, are extended to also include foreign-based firms and individuals that conduct commercial and financial transactions with the target country. Failure to adhere to the imposed sanctions leads to the denial or limitation of financial and commercial relations with the sender country as well as potential fines. The purpose is to deter third-country actors from engaging in business with the target country, induce greater multilateral cooperation in the sanction campaign, and thus exert more economic pressure on the target-country by limiting trade deflection and sanction-busting (Han, 2018; Meyer, 2008; Peksen, 2019).

The efficacy of embargoes also depends on the response of the target country. Despite the possibilities of deflecting trade towards new markets, firms and sectors may be unable to systematically circumvent the imposed sanctions for which they incur heavy financial losses, especially for those dependent on imported goods and services from the sender countries. The government of the targeted country may therefore choose to mitigate this by *shielding* sectors and firms that are deemed strategic. This strategy refers to the ex-post transfer of resources, bailouts and acquisitions/nationalizations in which the target government shifts the burdens of the sanctions to itself rather than the targeted firms. Thus, facing a government that has the ability to shield sensitive firms and sectors at will, there are limits to what the sender country's governments hope to achieve with the imposed embargo (Ahn and Ludema, 2020).

Similarly, the regime type of the target country may limit the effect of embargoes. In fact, sanctions imposed on democratic states are more likely to elicit concessions than those imposed on authoritarian states (Allen, 2005, 2008; Brooks 2002; Kaempfer, Lowenberg, and Mertens 2004; Lektzian and Souva, 2007; Peksen 2019). This is because authoritarian leaders can often escape the macroeconomic pressures and costs directed towards themselves and their support base, thus making them more defiant. Specifically, by intervening in the economy and redirect

existing wealth and resources made scarce by the sanctions away from rivals and citizens and towards their support base, authoritarian leaders can evade the adverse effects of the sanctions. In addition, comprehensive sanctions that create broad-sided economic hardships among the target country's population often inspire rally-'round-the-flag-effects[6] and may, counterproductively, strengthen the position of the authoritarian regime (Brooks, 2002; Galtung, 1967). The likelihood of leaders being willing to elicit concessions to external demands is only possible where the target's ruling coalition is harmed by the imposed sanctions (Brooks, 2002). In contrast, democratic leaders are bound to their broad coalition of supporters that include all citizens or a large share of them. The restricted ability of democratic leaders to insulate the "median voter" from the sanctions makes it hard for democratic regimes to provide incentives to retain constituent loyalty. Further, functioning checks-and-balances and strong rule of law makes it unlikely for democratic governments to resort to repression to quell growing opposition brought by economic suffering. Democratic leaders are therefore more likely to concede to demands in order to remain in office (Peksen, 2019).

2.2 The Embargo Against Qatar

The prelude to the embargo can in many ways be traced to diverging political interests in the Middle East between Qatar and the three other member states of the GCC Bahrain, Saudi Arabia, and UAE. The regional neighbors of Qatar have long criticized them for its support of Islamist movements in the region, such as the Muslim Brotherhood and Hamas. Viewing political Islam as a threat to its rule and that of other Gulf monarchies, the UAE has been particularly critical (Kerr, 2017). Similarly, Qatar's support to these movements have been viewed as active attempts to interfere in internal affairs of other countries such as Egypt which ousted the Qatari-backed Muslim Brotherhood government in 2013 after a military coup. Seen as a security threat, many of the Islamist movements are labeled as terrorists by the UAE, Egypt as well as other GCC states. Qatar's alleged meddling in domestic policies has primarily occurred through news reporting on Qatari-funded broadcasters, especially *Al Jazeera*. Al Jazeera has been accused by the Tripartite of providing a platform for opposition groups in the region such as in Bahrain and Egypt (Sciutto and Herb, 2017). Due to this, Bahrain, Saudi

[6] This phenomenon refers to the temporary increase in popular support of a country's political leader or government during periods of war or international crisis, such as when economic sanctions are placed on a country (Allen, 2008; Drezner, 1999; Galtung, 1967).

Arabia, and the UAE has pushed Qatar to tone down its media and its pro-Islamist foreign policy (The Economist, 2021b). Qatar's traditionally less aggressive stance towards Iran has also created further wedges with the other GCC states, especially Saudi Arabia which view Iran as a strategic rival (Wintour, 2017). Moreover, Qatar's alleged support of Iran-backed militias-cum-organizations, such as Hezbollah in Lebanon, has created further tensions with other Gulf states as it is viewed as active attempts to undermine GCC interests and security (Sciutto and Herb, 2017).

In the past, these competing interests has led to the temporary suspension of diplomatic relations, such as in March 2014 when the Tripartite withdrew their ambassadors from Qatar. Qatar's incompliance with the *Riyadh Agreement* in 2013 that laid out non-interreference in the internal affairs of other Gulf states and political or financial support to "deviant" groups were cited as the main reason for the Tripartite's action. Diplomatic relations were restored nine months later as the involved states signed the *Riyadh Supplementary Agreement*. The agreement also out, among other things, provisions in which support to groups or movements that pose a security or stability threat to the GCC states would be barred (Sciutto and Herb, 2017).

While there was a period of détente following the 2014 diplomatic rift, diplomatic tensions deteriorated significantly in April of 2017 after the Qatari government transferred 1 billion USD to release Qatari royals that were held hostage in Iraq. The Tripartite considered the event as a covert operation to transfer funds to Iranian-backed Shia militias and to al-Qaeda affiliated terrorist groups (Solomon, 2017). Relations deteriorated further in May after the Emir of Qatar allegedly expressed support for Iran and Israel along with other Islamist organizations, notably Hamas and Hezbollah, on Qatari news outlets. Later in May and June, the email account of the Emirati ambassador to the U.S. as well as the Bahraini foreign minister's Twitter-account was hacked, a move seen by the UAE and Saudi Arabia as a Qatari attempt to harm their diplomatic relations with the U.S. (Ulrichsen, 2020).

After a month of escalations, the Tripartite announced that they would impose an embargo against Qatar on June 5th 2017. Qatar's alleged support to various terrorist groups and Qatar's continued violation of the Riyad Agreement in 2013 and Riyad Supplementary Agreement in 2014 were cited as the main reasons for their actions. The supposed intention with the embargo in regard to what the Tripartite hoped to achieve, however, was unknown as no official demands

had been communicated prior to the imposition of the embargo. The embargo thus came as a surprise to Qatar (Sciutto and Herb, 2017).

In practical terms, the embargo comprised a concerted land, sea, and air blockade as all Qatari-owned and Qatari-bound vessels, ships and aircrafts were denied access to the Tripartite's landbound, maritime and airspace routes. In addition, all the Tripartite's airline traffic, cross-border trade by land, and shipments via the Tripartite's ports to and from Qatar were suspended. This action subsequently blocked Qatar's trade with third countries via Tripartite territories. In three of the embargoing countries (Saudi Arabia, Bahrain and the UAE), Qatari citizens and visitors were given two weeks to leave, and their own citizens were ordered out of Qatar (Wintour, 2017). With the exception of UAE's continued purchase of natural gas from Qatar, constituting a third of UAE's natural gas consumption (Alkhalisi, 2017), the embargo prohibited all imports, exports, re-imports and re-exports between the Tripartite and Qatar. Later, other countries would provide tepid support to the Tripartite by either issuing statements condemning Qatar, scaling back relations or curtailing them altogether (see Table 1 and Figure 1). However, none participated in the embargo or imposed other economic sanctions. The two other states of the GCC, Kuwait and Oman, declared themselves neutral in the conflict and other countries in the region called for a peaceful resolution through dialog (Ulrichsen, 2020). The different stances taken by the countries reflect the divisiveness within GCC and that the embargo lacked widespread regional support (Bianco and Stansfield, 2018; Koch, 2021). Nonetheless, given the scope and severity of the measures taken by the Tripartite makes the embargo unprecedented in the history of the GCC (Wintour, 2017).

Table 1. List of the countries involved in the embargo.

Embargoed country	Countries that aided Qatar	The Quartet	Countries that either condemned, cut-back or seized diplomatic relations with Qatar
Qatar	Iran	Bahrain	Djibouti
	Turkey	Egypt	Chad
		Saudi Arabia	Comoros
		United Arab Emirates	Eritrea
			Gabon
			Jordan
			Libya
			Maldives
			Mauritania
			Niger
			Senegal

Note: In the case of Libya, the country is claimed to be governed by two competing governments - the Tobruk-goverment and the General National Congress. It was the Tobruk-government that ceased diplomatic ties although they do not have diplomatic relations with Qatar (Ulrichsen, 2020).

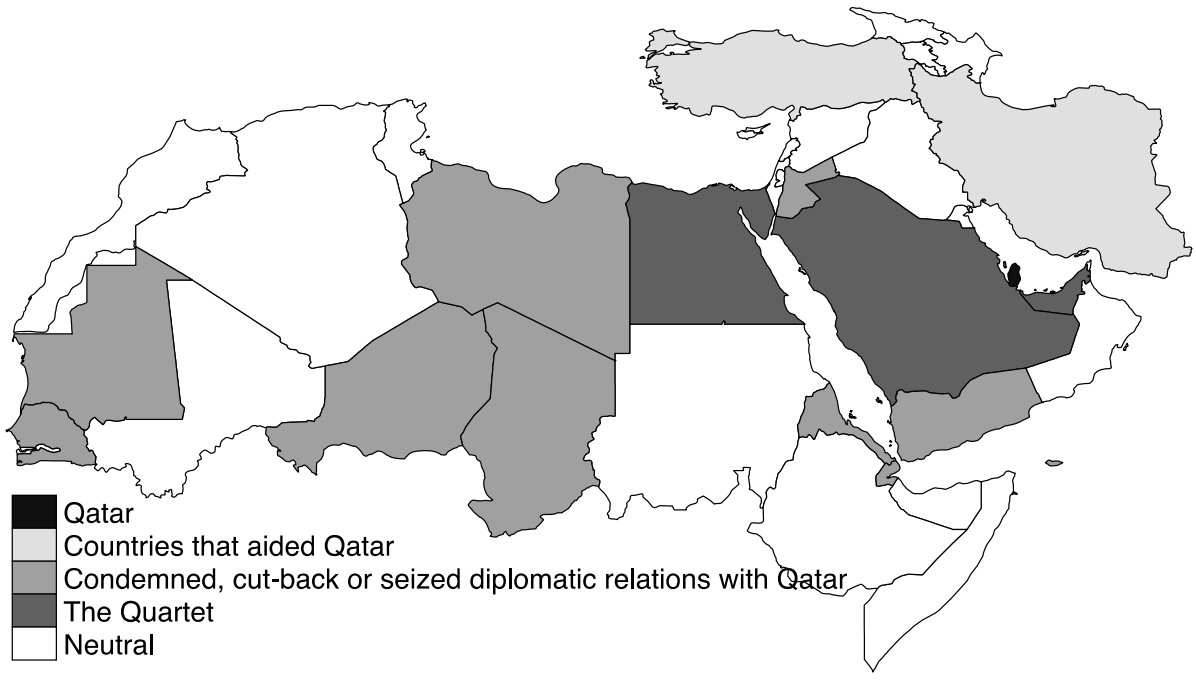

Figure 1: Regional overview of the countries involved in the embargo. Note that the Quartet refers to all the embargoing countries, including Egypt.

The effect of the embargo can be clearly seen in Figure 2 which displays the air traffic of Qatar Airways, Qatar's flagship airline, in the region before and after the embargo. Following the imposition of the embargo, Qatar Airways had to redirect its air traffic away from Tripartite air space and find alternative flight routes, primarily through Iran. This is evident from Figure 2 as no flights can be seen over Saudi or Egyptian air space after the embargo was imposed while flight routes over Iran and Turkey have increased substantially (right hand side).

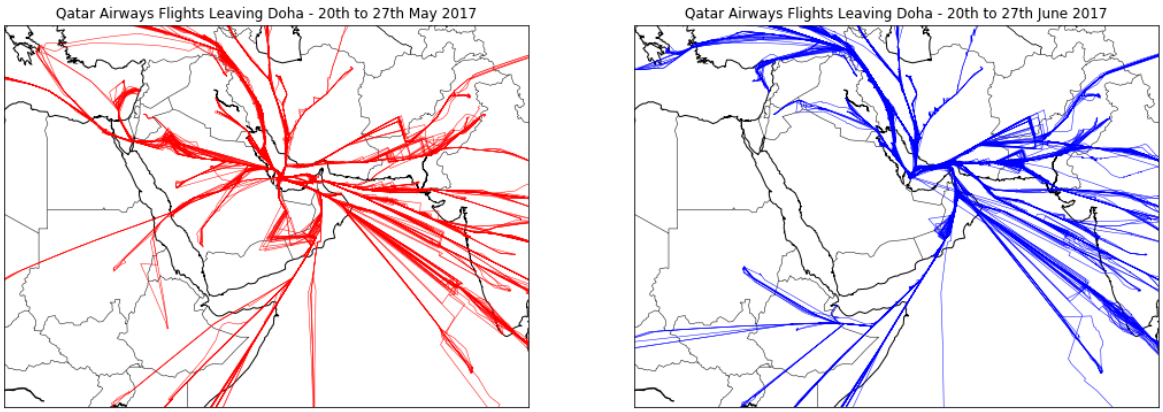

Figure 2: *Qatar Airways air traffic from Doha, Qatar, before (to the left) and after the* imposition of the *embargo (to the right). Source: FlightRadar24.*

The embargo presented Qatar with severe and immediate security threats. Prior to the embargo, Qatar imported over 90 percent of its food supplies and over 40 percent of all their food and medical supplies was imported via the Qatari-Saudi land border (Kaitibie & Rakotoarisoa, 2017; Monroe, 2020; Selmi & Bouoiyour, 2020). In addition, with the inability to service large cargo ships, Qatar relied on the Emirati port of Jebel Ali for feeder services to handle approximately 85 percent of all of its shipborne cargo (The Economist, 2017). To avert an immediate food crisis, Qatar engaged with other countries in the region, in particular Iran and Turkey, to secure alternative supply lines. On June 7th, Turkish support in the form of food and water supplies along with troop deployment to their military base were pledged to Qatar (Finn, 2017a). With the suspension of feeder services from Emirati ports, transshipment points were rerouted to the Omani ports of Salalah and Sohar by the shipping companies Maersk and MSC on the 8th of June to accommodate trade between Qatar and non-embargoing countries (Ulrichsen, 2020). Similar to Turkey, Iran deployed supplies (fruits and vegetables) via four cargo planes with the promise of continuing their supply food on the 11th of June (Reuters, 2017). Two weeks later, on June 22nd, the Tripartite presented a list of 13 demands that Qatar

had to agree to within ten days in order to reach a peaceful resolution and end the embargo (Charfeddine and Al-Refai, 2019). The list included demands such as reducing diplomatic relations with Iran and sever all ties organizations deemed as terrorists by the Tripartite (see Appendix A for the full list of demands). Qatar refused to concede to the demands as they viewed it as a violation of Qatar's sovereignty and independency as a state. Consequently, the embargo continued to be in force until January 2021 when the Qatar and the Tripartite agreed to restore diplomatic ties and end the embargo (BBC, 2021a; BBC, 2021b).

3 Empirical Strategy and Data

In this section, the empirical methodology used in this paper is presented. Subsection 3.1 introduces the intuition behind SCM and its empirical framework, while the application of the SCM to the case of the embargo against Qatar is presented in subsection 3.2.

3.1 Introducing the SCM

To estimate the evolution of Qatari trade following the embargo by the Tripartite, I will employ the synthetic control method (SCM) that was developed by Abadie and Gardeazabal (2003) and further extended in Abadie et al. (2010; 2015). This statistical method incorporates elements from difference-in-differences and matching techniques to more accurately evaluate the effect of a treatment[7] at the aggregate level (e.g., countries, states, regions, age groups etc.). In essence, and similar to difference-in-differences approaches, the SCM constructs a weighted combination of groups that is used as comparison units which the treatment group is then compared with. In turn, this control group is used to estimate the counterfactual outcome had the treatment group not received treatment. Unlike the difference-in-difference method, however, changes in the effects of cofounder over time can be accounted for in the SCM by weighting the comparison group and in return better match the treatment group prior to intervention. That is, by using relatively extended time series of the dependent variable and its corresponding explanatory variables before the treatment, weights are estimated and assigned by the SCM in such a way that the comparison group reflects the movements and trajectories of the treatment group as closely as possible. Thus, by comparing the post-intervention period of the treatment group to that of the synthetic control group, one can observe the treatment effect of the intervention (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015; Abadie, 2019).

[7] Note that the terms "treatment", "intervention", and "event" will be used interchangeably in this paper.

The econometric setting of the SCM makes it follow a more data-driven and formalized procedure in selecting control groups than previous econometric methods. More specifically, it allows researchers to be less reliant on informal statements of affinity and arbitrary selection procedures when selecting a set of comparison units.[8] In addition, rather than relying on just one single comparison unit, which can be difficult and inappropriate in the case of aggregate entities, the SCM allows for a combination of comparison units to better construct a counterfactual outcome. For this reason, the SCM has become increasingly common in comparative case studies and is gaining traction in the field of economics (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015; Abadie, 2020).

3.2 The Econometric Setting of the SCM

To better understand the econometric construct of the SCM, assume there are J + 1 units (e.g., countries, states, regions, age groups etc.) with T observations that consists of J potential control units and one treated unit. The number of pre-treatment periods is denoted as T_0, and $T_0 + 1$ is the first period with treatment. Without loss of generality, the first unit ($i = 1$) can be defined as the treated unit whereas the rest ($i = 2, \ldots, J + 1$) constitute the *donor pool*, i.e. the collection of untreated units that are assumed to be unaffected by the event, from which a potential control group can be formed. Let the outcome variable that would have been observed in the absence of treatment be denoted as Y_{it}^N for $i \in [1, J+1]$ and period $t \in [1, T]$, including the post-intervention period $[T_0 + 1; T]$. Similarly, let Y_{it}^I denote the actual recorded values of the outcome variable for unit i at time t if unit i is exposed to the intervention in periods $t \in [T_0 + 1; T]$. Assuming that the intervention does not have any effect on outcomes prior to its implementation, by construction $Y_{it}^I = Y_{it}^N$ for the pre-treatment periods and units, that is, $t \in [1, T]$ and $i \in [1, J+1]$. In the post-intervention period, the difference $\alpha_{it} = Y_{it}^I - Y_{it}^N$ is the impact of treatment on unit i at time t. Since only unit $i = 1$ is subject to treatment in period $t > T_0$, we have:

[8] According to Abadie (2020), prior comparative case studies, such as Card (1990) and Card and Kruger (1994), have this potential drawback.

$$Y_{it}^{I} = Y_{it}^{N} + \alpha_{it}D_{it}, \text{ where } D_{it} = \begin{cases} 1 & \text{for } i = 1, t > T_0 \\ 0 & \text{otherwise} \end{cases} \quad (1)$$

In this setup, all outcomes for all periods are observed with the exception of Y_{it}^{N} for $t > T_0$ which represents the synthetic outcome for the treated unit in the post-intervention period. D_{it} is a dummy equal to unity if the unit is exposed to the intervention at time t, and value zero otherwise. As Y_{it}^{I} is observed, the estimate of α_{it} is obtained by estimating Y_{it}^{N}. In turn, Y_{it}^{N} is determined by employing the following factor model:

$$Y_{it}^{N} = \delta_t + \theta_t Z_i + \lambda_t \mu_i + \beta_t \text{treatment}_{it} + \epsilon_{it} \quad (2)$$

"where δ_t is an unknown common factor with constant factor loadings across units, Z_i is a ($r \times 1$) vector of observed covariates (not affected by the intervention), θ_t is ($1 \times r$) vector of unknown parameters, λ_t is ($1 \times F$) vector of unobserved common factors, μ_i is an ($F \times 1$) vector of unknown factor loadings" (Abadie et al., 2010, p. 495), and ϵ_{it} is an error term with mean zero that captures transitory shocks at the aggregate entity level.

To construct the synthetic post-treatment outcome, optimal vector of weights $\mathbf{W} = (w_2, ..., w_{J+1})'$, such that $w_j \geq 0$ and $\sum_{j=2}^{J+1} w_j = 1$, needs to be obtain. A potential synthetic control is derived from each particular value of **W** that constitutes a particular weighted average of control units. The idea is to find the optimal values of vector **W**, such that:

$$\begin{aligned} &\sum_{j=2}^{J+1} w_j^{*} Z_j = Z_1 \\ &\sum_{j=2}^{J+1} w_j^{*} Y_{jt} = Y_{1,t}, \forall\, t = 1, \ldots, T_0 \end{aligned} \quad (3)$$

After the estimation of the optimal weight vector, the identification of the treatment effect is:

$$\widehat{\alpha_{1t}} = Y_{jt} - \sum_{j=2}^{J+1} w_j^{*} Y_{jt} \quad \forall\, t > T_0 \quad (4)$$

In practice, however, finding weights in a way that makes equation (3) and (4) hold exactly is not possible. Such is the case if the treated unit's characteristics fall outside of the convex hull of the control group's characteristics by which replication cannot be achieved with the

restriction on w_j. Ideally, the synthetic control group would be comprised of both the factors Z and μ of the treatment unit, yet this approach is infeasible since μ is unobserved. Provided that regularity conditions are mild, however, Abadie et al. (2010) show that μ only matches with that of the synthetic control group given that μ also matches with an extended period of pre-treatment outcome variables Y_{it}. The ambition is thereby to choose weights that minimizes both the deviations in the pre-treatment outcomes Y_{it} as well as known characteristics Z. As is standard in the SCM literature, the optimal weights are given by the minimization of the vector **W**, such that:

$$W = \arg\min_{w} \| X_1 - X_C W \| = \arg\min_{w} \sqrt{(X_1 - X_C W)' V (X_1 - X_C W)} \quad (5)$$

where $\boldsymbol{X_1}$ is a vector of pre-intervention characteristics of the treated unit, and $\boldsymbol{X_C}$ is a matrix of pre-intervention characteristics of the potential control units that are assumed to be unaffected by the treatment. Put it differently, the selection of weights and their distribution is on the basis that synthetic control units' observed characteristics and outcomes replicate or as closely as possible mirror those of the treated units in the pre-treatment period. By using the weighted average of the identified comparison units' outcomes, a synthetic estimate of the treated unit can be constructed to produce counterfactual projections in the post-treatment period.

The importance of each pre-treatment characteristic is captured and measured by the non-negative weights in the term **V**, which is a positive and diagonal definite matrix. For the pre-treatment periods, **V** is selected such that the mean squared prediction error (MSPE) of the outcome variable is minimized. The pre-treatments factors with the largest predictive power are thus given precedence in terms of relative weights. Further, a sufficiently large pre-intervention matching window enables the matching of pre-treatment outcomes to control for unobserved factors in addition to the heterogeneous effects of unobserved and observed factors on the outcome variable (Abadie et al. 2010; Abadie, 2015). According to Abadie et al. (2015, pp. 498), this is because "only units that are alike in both observed and unobserved determinants of the outcome variable as well as in the effect of those determinants on the outcome variable should produce similar trajectories of the outcome variable over extended periods of time". That is, on the condition that pre-intervention trajectories are very similar between the treatment unit and its synthetic counterpart, any discrepancies occurring post-intervention can be viewed

as the treatment effect upon the dependent variable. In contrast, if the control units are insufficiently similar to the treated units, then any differences in post-intervention outcomes may solely reflect divergences and disparities in the characteristics of the two sets of units (Geddes, 2003; George and Bennet, 2005; King, Keohane, and Verba, 1994). Applied to SCM, this means that a poor pre-intervention fit between the synthetic control unit and the actual treated unit is an indication that the estimated treatment effect is erroneous and the SCM results should be interpreted with caution (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015; Abadie, 2020).

Finally, a pre-condition for a credible synthetic estimator is that the pre-treatment window is sufficiently long. This is because data generating processes that follows a factor model, as in equation (2), creates a synthetic control estimator whose biasness is inversely proportional to the length of the matching window prior to the treatment. A small number of pre-treatment periods may thus generate estimated values of the treated unit that are spurious, even in the case of a close or perfect fit. Consequently, the synthetic estimator of the treated units may fail to credibly reproduce the path of the outcomes in the absence of treatment (Abadie et al., 2010).

3.3 Application of the SCM to the Case of the Qatari Embargo

In this section, the methodological application of the SCM used to estimate the short-term trade flows that would have been between Qatar and the Tripartite, if the Tripartite had not imposed an embargo against Qatar is presented. Applying the logic and language of the SCM, a unit is the bilateral trade between two countries (trade unit) and treatment is the embargo. Similar to other papers (see e.g., Saia, 2017) the dependent variable is the average of two-way bilateral trade flows, i.e., the averaged sum of exports and imports, between two countries.[9] In order for the SCM to generate as credible results as possible, it has to be adapted to fit the contextual requirements of this paper. In a broad sense, these requirements consist of 1.) the choice of time horizons and 2.) the choice of comparison groups.

[9] For further examples, see e.g., Baier and Bergstrand, 2009; Disdier and Head, 2008; Micco et al., 2003; Rose, 2000; Tomz et al., 2007).

To mitigate issues related to insufficient time horizons, as specified in section 4.1., this paper uses annual exports and imports data between 1995-2019 from the International Monetary Fund's (IMF) Direction of Trade Statistics (DOTS) database (IMF, 2021). This provides a pre-intervention window of 21 years, in addition to a post-intervention window of two years. The chosen time period was primarily dictated by the availability of data for the explanatory variables of the countries included in this paper. In addition, the Covid-19 pandemic that culminated in the beginning of 2020 naturally restricts the post-intervention period to 2019 as its incorporation would compromise the credibility of the SCM estimator. Due to the constricted post-embargo window, this paper will only study the short-term trade effects of the embargo.

Concerning the selection of relevant covariates to form $\boldsymbol{Z_i}$ in equation (2) and in the optimization procedure of $\boldsymbol{X_1}$ and $\boldsymbol{X_C}$ in equation (5), conventional gravity-model variables provide a natural framework. Specifically, and in line with other studies that use SCM to estimate trade flows (see e.g. Adarov, 2018; Hannan, 2016; Saia, 2017), this paper employs the following variables: the log of the sum of each country's GDP in every bilateral pair at time t and averaged over the period 1995 to 2016, the log weighted distance between two countries, and a dummy variable that takes the value of 1 for common religion, common language and adjacency. Data on GDP is obtained from the World Bank Development Indicators while the other variables are retrieved from the Centre d'Études Prospectives et d'Informations Internationales (CEPII) (CEPII, 2021; World Bank, 2021b). In addition, the outcome variable is also used as an explanatory variable. Although it is common in the SCM literature to lag the outcome variable in the pre-intervention period, there is no consensus concerning the extent of lagging it (Ferman et al, 2020; Dube and Zipperer, 2015; Kaul et al., 2018).[10] In this paper, the outcome variable, i.e. two-way average bilateral trade, is lagged every second year. However, to check the robustness of this lagging-approach, alternative lag structures are employed. A more detailed elaboration is provided in section 4.2.

Another considerable challenge to obtain robust SCM results lies in finding a credible comparison group to the bilateral trade between Qatar and the Tripartite. Abadie and Gardeazabal (2003) and Abadie (2020) suggests that *interpolation biases*, i.e. heterogeneity, can be alleviated if the donor pool is limited to a subset of units that share similar characteristics

[10] For example, Abadie et al. (2010) added three years of lagged smoking consumption (outcome variable), whereas Hannan (2016) lags his outcome variable (exports) for every third period. In contrast, Saia (2017) uses a pre-treatment average of the outcome variable as a predictor.

and closely resemble the structural processes of the treated unit. Besides mitigating potential biases, Abadie (2020) also advices researchers to select control units from the donor pool where both the control and treated units were affected or exposed to the same regional economic shocks. As with using the lagged outcomes as predictors, there is no consensus how to construct a donor pool using the SCM in international economics study. In his study of consumer boycotts of Danish exports by Muslim countries following the publication of the Mohammad caricature in 2006, Heilmann (2016) uses Danish exports to minority-Muslim countries as his control group. Adarov (2018) on the other hand uses a more data driven approach in his donor pool construction in order to study the trade effect brought by the formation of the Eurasian Customs Union (EACU) between Belarus, Kazakhstan, and Russia in 2010. His selection is based on the several criteria, such as i.) Both control countries must belong to the Eurasian and Central Asia region; ii.) Control units where either the exporter or importer is a member of the EACU or the Commonwealth of Independent States Free Trade Area (CISFTA) are excluded. Finally, Saia (2017) studies UK synthetic trade flows had it joined the euro in 1998 and uses trade flows between countries that adopted the euro as his donor pool.

Considering the uniqueness of Qatar's and the Tripartite's economic size and composition, it is difficult to find countries that are "structurally" similar to them and their respective trade patterns. To mitigate this, a similar approach to that of Adarov (2018) is employed in this paper. As such, the donor pool will be restricted to only include trade units that belong to the same geographical region as Qatar and the Tripartite, i.e. the Middle East and North Africa (MENA). The rationale is that control units that are in the geographical vicinity of the treated units should share economic similarities and resembling trade patterns (Adarov, 2018). Contrary to Adarov (2018), I include units where a Tripartite-country is either an exporter or importer as not to make the donor pool too restrictive. In addition, I adopt Saia's (2017) approach and thus include inter-Tripartite trade such as Saudi Arabia-Bahrain. In line with Heilmann's (2016) approach, I also include Qatar's trade with other countries in the MENA-region. These approaches should provide the donor pool with eligible candidates to create the synthetic estimator as the countries would be exposed to similar regional shocks, such as those related to the Arab Spring and hydrocarbon price fluctuations.

These geographical restrictions and donor pool specifications, however, are insufficient to create a suitable comparison group without further adaptation to the contextual elements of the embargo against Qatar. Specifically, treatments may generate *spillover effects* on non-targeted

units. If such spillover effects are large and affect non-targeted units in close geographical proximity of the treated units, the ensuing synthetic estimator may produce a biased counterfactual outcome in the absence of treatment for the units that actually are targeted and affected by the treatment (Abadie, 2020). Considering that shipping routes were redirected via Oman and both Iran and Turkey assisted Qatar with food and medical supplies, it can be argued that the embargo created spillover effects through diverted trade flows, i.e., trade deflection. Including these bilateral trade flows may thus attenuate the synthetic control estimator and, consequently, the post-embargo treatment effect. Following the advice by Abadie et al. (2015) and Abadie (2020), bilateral trade between Qatar, Iran, Oman, and Turkey is removed from the donor pool.

In summary, the SCM is applied to the three treated units QAT-ARE, QAT-BHR and QAT-SAU where their respective synthetic estimators will be constructed from a donor pool consisting of 203 potential comparison units. The list of countries that are in the MENA-region and included in this study can be found in Table 1. The donor pool and all the potential comparison units can be found in Table 2 in Appendix C.

Although this donor pool should suffice, a common practice to check this is to change the donor pool composition. For example, Abadie et al. (2010) excludes states in the U.S. that introduced similar tobacco control programs to the one studied in California or increased cigarette taxes during the period under study. To verify the robustness of their synthetic estimates, they later include the discarded states in the donor pool. Similarly, Abadie et al. (2015) introduces countries they deem as outliers in their donor pool to check if they have an impact on the synthetic outcomes. To follow their examples, this study breaks down the original donor pool into two different subsets: 1.) Only includes two-average bilateral trade with the Gulf-states in addition to Jordan and Turkey, and 2.) Only include control units that has a pre-embargo two-way bilateral trade average that is within one standard deviation to the treated units under study. The latter approach is also employed by Adarov (2018).

4 Results and Discussion

In this section, the results of the SCM will be presented. Section 4.1 deals with the main results of the SCM which is then followed by the introduction of various sensitivity and robustness tests that is performed within the framework of the SCM in section 4.2. Finally, further discussions of the SCM and the embargo will be presented in section 4.4.

4.1 The SCM Results

Figure 1-3 plots the evolution of the bilateral trade of the Qatar-Tripartite and of the synthetic units over the period 1995 to 2019. The solid lines show the actual two-way average bilateral flows between Qatar and the Tripartite-countries, while the dashed lines display the synthetic scenario of bilateral trade flows in which the embargo had not been imposed. The weight composition of the synthetic estimators for each treated unit can be found in Appendix C. Upon inspection, the fit of the pre-embargo synthetic trade flows to that of actual trade flows for all of the three bilateral trade pairs can be questioned given the dynamic and sporadic trade patterns that we observe. Prior to the imposition of the embargo, the synthetic trade flows are volatile and particularly pronounced after the Financial Crisis in 2007. This pattern signals that synthetic trade flows may change as a consequence of other shocks occurring prior to the embargo. Moreover, in all the treated units, actual trade flows follow a negative trajectory before the embargo is imposed. This suggests that *anticipation effects* may be present. Such effects arise if forward-looking agents anticipate or otherwise reacts before the intervention has taken place. Anticipation effects may also arise if other components related to the intervention are imposed ahead of the actual enactment/implementation of the intervention. If such effects are present, it may bias the synthetic control estimates and subsequently its credibility (Abadie, 2020).

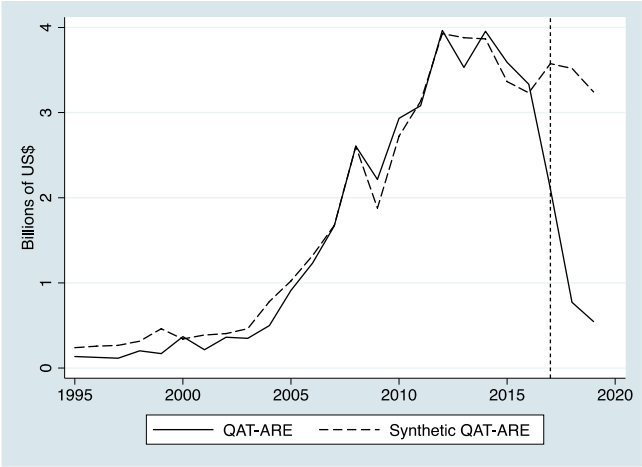

Figure 3: Two-way average bilateral trade flows between Qatar and the UAE vs their synthetic estimators.

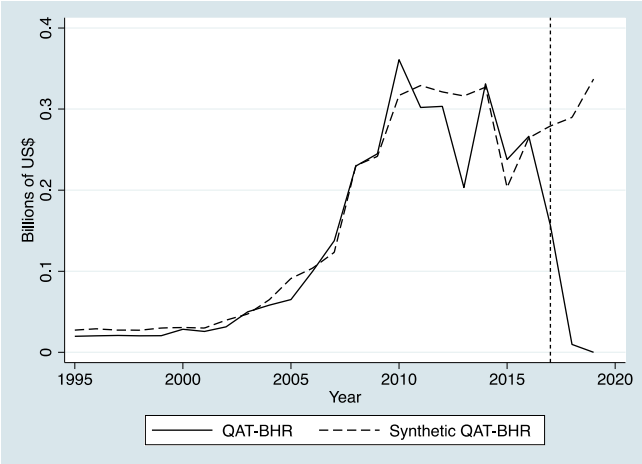

Figure 4: Two-way average bilateral between Qatar and Bahrain vs their synthetic estimators.

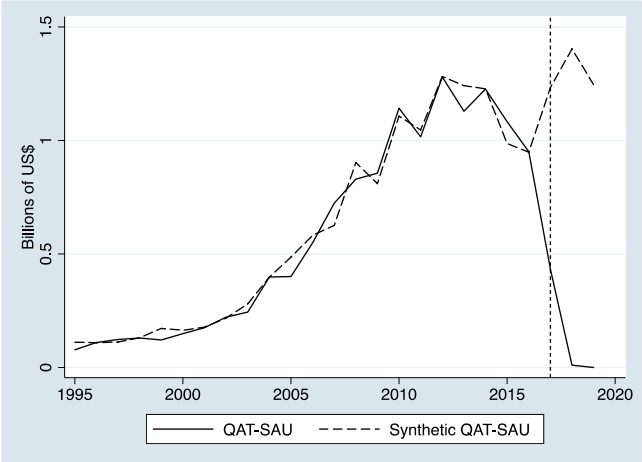

Figure 5: Two-way average bilateral trade flows between Qatar and Saudi Arabia vs their synthetic estimators.

From 2017, the dashed lines reflect trade flows developments if the embargo against Qatar had not been imposed. Synthetic trade flows for Qatar-Bahrain and Qatar-Saudi Arabia diverge substantially from actual trade between 2017-2019 and has a clear upward sloping trajectory. Contrary to the other trade pairs, Qatar-United Arab Emirates has a synthetic counterfactual that is downward sloping. However, it still significantly outstrips that of observed trade flows. This shows that in a counterfactual world without an embargo, Qatar's trade with the United Arab Emirates, Bahrain, and Saudi Arabia would have been larger than it was in the real (embargo) world. Moreover, this suggests that the embargo had the intended negative effect on trade.

Table 2 reports the difference between actual and synthetic two-way average bilateral trade in percentages over different time periods. During the period prior to the imposition of the embargo, i.e. our matching window, Qatar's actual trade the UAE, Bahrain, and Saudi Arabia performs worse relative to that of its synthetic counterpart at -15.85, -10.94, and -3.65 percent respectively. Further, the actual trade in the full post-embargo period (2017-2019) for each bilateral pair is substantially lower than that of the synthetic estimator. In fact, the estimate of the reduction in terms of lost trade from the embargo is -66.46 (Qatar-UAE), -79.45 (Qatar-Bahrain), and -88.05 percent (Qatar-Saudi Arabia) respectively.

To evaluate the disaggregate post-embargo effect, I separate the total post-embargo period into three standalone years: 2017, 2018 and 2019. The estimates suggests that the embargo had an immediate and large effect upon its imposition in 2017 for all the treated units: -41.13 (Qatar-UAE), -41.95 (Qatar-Bahrain), and -64.87 percent (Qatar-Saudi Arabia) respectively. Subsequently, the negative trade impact derived from the embargo grows exponentially in the following two years after its imposition. In 2018, trade decreased substantially: -78.08 (Qatar-UAE), -96.36 (Qatar-Bahrain) and -99.99 % percent (Qatar-Saudi Arabia). The following year, in 2019, trade reductions are even larger: -83.19 (Qatar-UAE), -99.99 (Qatar-Bahrain), and -99.99 (Qatar-Saudi Arabia). The results indicates that the adverse embargo effects increase over time, at least in the short-term. Further, this shows that the intended goal to decrease trade with Qatar on the side of the United Arab Emirates, Bahrain, and Saudi Arabia was to a large extent successful. Indeed, the drop in trade levels can be interpreted as autarky-like in nature, i.e. a world where trade does not exist. Interestingly, and although they are over 200 years apart, this trade outcome is similar to that of the self-imposed Jeffersonian embargo in the U.S. between 1807 to 1809 (see e.g. Irwin, 2005). Although all Tripartite-countries were successful in terms

of trade reductions, Saudi Arabia seemed to be more efficient. This since it both aggregately reduced more trade and did so relatively more expedient. Bahrain performance, however, is not lagging too far behind that of Saudi Arabia. Out of the three, UAE:s trade with Qatar decreases less.

Finally, the application of the SCM to the two subsets can be seen in Figure 6 and 7. In both cases, they do not markedly improve the synthetic results. On the contrary, the synthetic estimator performs worse relative to when the SCM is applied using the full donor pool of 203 potential comparison units. Problems with matching the trajectories of actual vs their synthetic estimates seem to be particularly pronounced when constructing a donor pool solely on a numerical criterion.

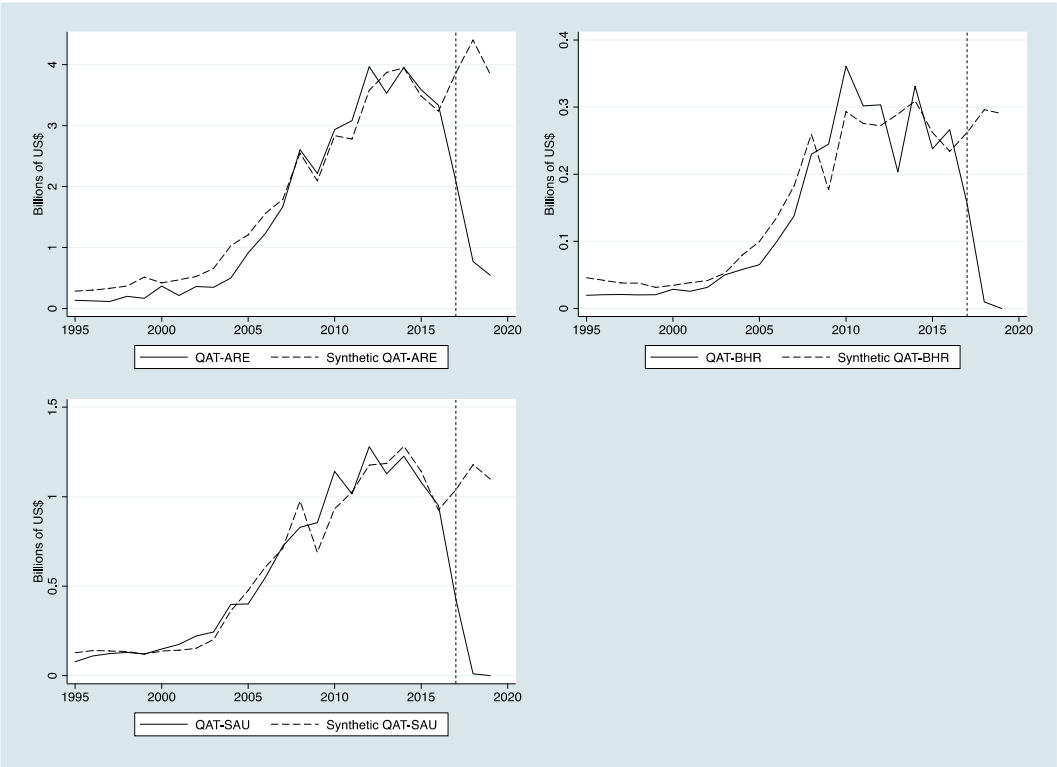

Figure 6: Figure 6: Two-way average bilateral trade flows between Qatar and the Tripartite vs their synthetic estimators using only the Gulf-states, Turkey, and Jordan as control units.

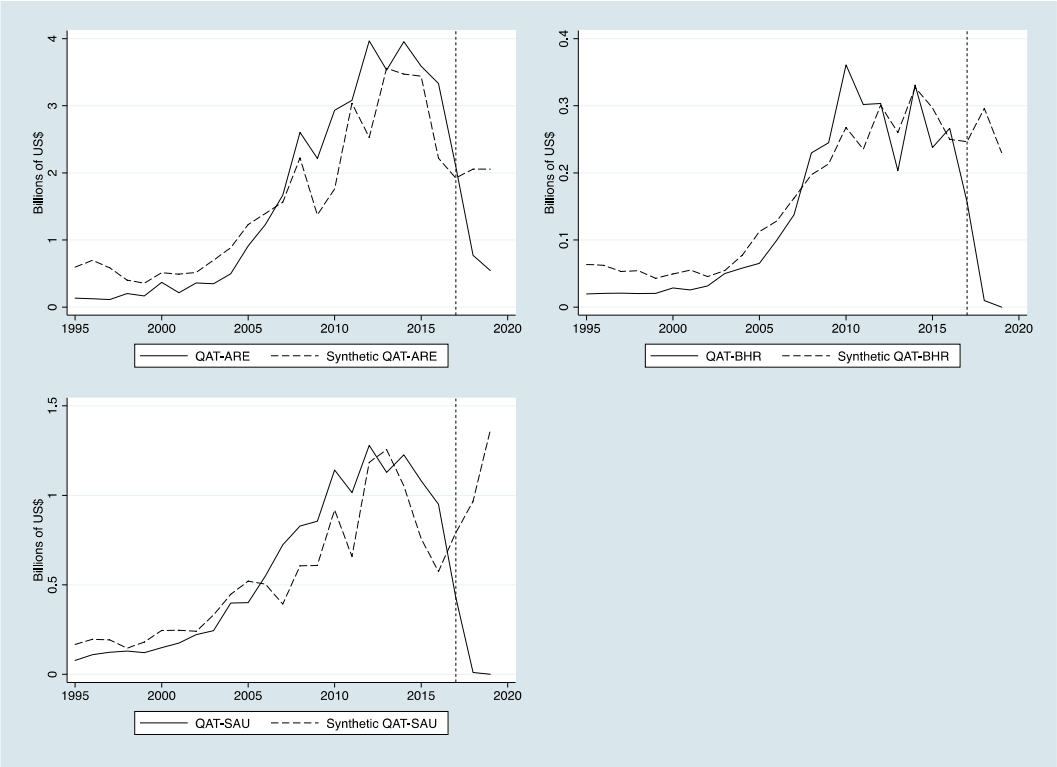

Figure 7: Two-way average bilateral trade flows between Qatar and the Tripartite and their synthetic estimators using the treated unit's pre-treatment trade average x/- one standard deviation as control units.

Table 3. Percentage difference between actual and synthetic trade flows between Qatar and the Tripartite countries

	1995–2016	2017–2019			
		2017–2019	2017	2018	2019
% Difference actual vs. synthetic: QAT-ARE	-15.85	-66.46	-41.13	-78.08	-83.19
% Difference actual vs. synthetic: QAT-BHR	-10.94	-79.44	-41.95	-96.36	-99.99
% Difference actual vs. synthetic: QAT-SAU	-3.65	-88.05	-64.87	-99.28	-99.99

Note: The table shows the percentage difference between actual and synthetic trade flows between Qatar (QAT), the United Arab Emirates (ARE), Bahrain (BHR), and Saudi Arabia (SAU) over different periods. Also notice that the estimates for the periods 1995-2016 and 2017-2019 are averages of the aggregate pre- and post-embargo period.

4.2 Sensitivity Analysis and Robustness Test

In the application of the SCM, the validity of the synthetic estimator is conditioned on its ability and capability to reproduce the outcomes of the treated units had the intervention not taken place. Common approaches in the SCM literature to test this is through the use of *placebo-tests* or *falsification tests*. There are two dimensions in which these placebo-tests can detect wrongful inference and that will be used in this paper: *in-time-placebo,* and *in-space-placebo* tests. The former is employed to analyze the sensitivity of the results to the choice of matching window. The test warrants concern over the validity of the results if a large embargo-effect is obtained in a period in which the embargo did not occur as it reflects that the synthetic control group is merely picking up idiosyncratic effects. That is, the observed impact on trade may be the cause of other unobserved factors or shocks besides that of the embargo (Abadie et al. 2015). In addition, Abadie (2020) recommends the use of in-time placebo tests to verify that there are no anticipation effects that may bias the synthetic control estimates. By backdating the intervention before any anticipation effects are expected to arise, the effect of the intervention can to a greater extent be estimated and evaluated. Similarly, *in-place-placebo* tests are used to construct synthetic controls for all or a subset of the comparison units in the donor pool in order to check whether the comparison units have been subject to treatments themselves. If a large number of the comparison units in the donor pool display similar treatment effects as the original treated units, it would suggest that the embargo did not have an unusual impact on trade between Qatar and the Tripartite (Abadie et al., 2010).

Another area that requires further vetting is the lag structure of the outcome variable that acts as a predictor. In their replication study of Abadie et al. (2010), McClelland and Gault (2017) show that the choice of lags in SCM has a significant impact on the pre-treatment fit of the synthetic counterfactual to that of actual outcomes and, correspondingly, the post-estimate projections. Moreover, Ferman et al. (2020) recommends that different sets of lags are employed in the analysis to check the robustness of the SCM results. More specifically, Kaul et al. (2018) suggests that one lags the last year of the pre-intervention period, i.e. one lag, and/or take the whole pre-treatment average of the outcome variable. Besides verifying that the SCM results are not noticeably affected by the different lag structures, the robustness check is also a tool to safeguard against *cherry picking*. That is, one chooses a certain lag structure for

the SCM simply because it generates the best results (Ferman et al., 2020). To accommodate these recommendations and to further check the robustness of the SCM results, this paper will employ five different lag structures for each treated trade unit: i.) one lag (2016); ii.) three lags (2002, 2009, and 2016); iii.) five lags (1996, 2001, 2006, 2011, 2016); iiii.) all pre-embargo outcome lags.; and v.) the pre-embargo average of the outcome variable.

4.2.1 In-Time Placebo

For the in-time placebo test, I construct a SCM for two-way average bilateral trade between Qatar and the Tripartite-countries by setting the matching window to 1995-2012 with the intervention year backdated to 2013. Although the year 2013 seem arbitrary, it is chosen to avoid other shocks that occurred during this time period that risks affecting the sensitivity of the in-time placebo test. Notably, this relates to the Financial Crisis of 2007-2008 that affected the entire world economy and the Arab Spring in 2010 that caused significant economic effects for many countries in the MENA-region. In addition, and in line with the advice of Abadie (2020), I backdate the placebo-embargo date before the anticipation effects are expected to occur, which seems to coincide with the start of the negative trajectory of synthetic trade flows around 2014 following the diplomatic tension between Qatar and the Tripartite.

In Figure 8-10, the actual bilateral trade between Qatar and the Tripartite-countries (solid line) and their synthetic counterfactuals (dashed line) over the period 1995 to 2019 with the embargo year in 2013 is plotted. As can be seen in all of the figures, the post-embargo trajectories of the synthetic bilateral trade flows of this "false" intervention year show that there is a substantial treatment effect in 2013. As prescribed by the theory behind in-time placebos, these large treatment effects indicate that other idiosyncratic effects interfere or otherwise contaminate the construction of the synthetic estimator. Further, the large treatment effect can also be the result of anticipation effects. As the imposition of the embargo was an unforeseen event, it is unlikely that it could have been anticipated. However, it could be connected to potential events that has affected trade flows or trade relationships between Qatar and the UAE, Bahrain, and Saudi Arabia prior to the embargo, such as the diplomatic tension in 2014. From that perspective, parts of the embargo have already been put in place, meaning that its corresponding embargo effects should be felt earlier than when the actual embargo is imposed. Hence, we observe a large treatment effect in the false embargo-year.

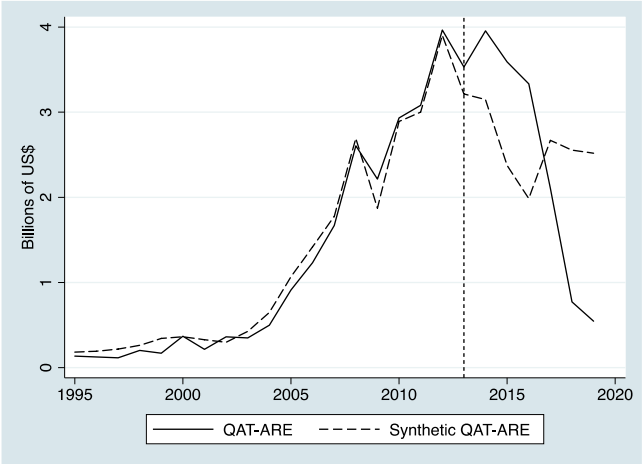

Figure 8: Two-way average bilateral trade between Qatar and the UAE and their synthetic counterfactual with false embargo backdated to 2013.

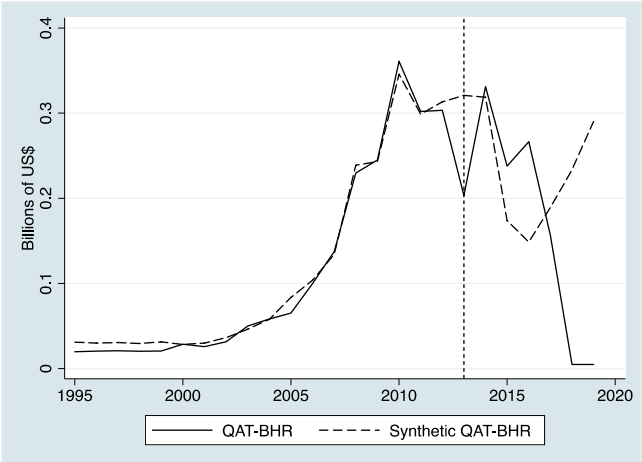

Figure 9: Two-way average bilateral trade between Qatar and Bahrain and their synthetic counterfactual with false embargo backdated to 2013.

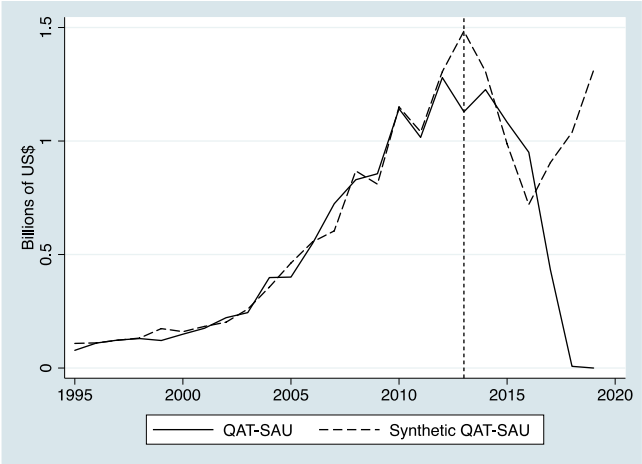

Figure 10: Two-way average bilateral trade between Qatar and Saudi Arabia and their synthetic counterfactual with false embargo backdated to 2013.

4.2.2 In-Place Placebo

To investigate whether the embargo effect is an isolated event between Qatar, the UAE, Bahrain, and Saudi Arabia, I perform the in-place placebo test using Qatar's average two-way bilateral trade with other countries in the control group. These consists of the following nine bilateral pairs: Qatar-Algeria (QAT-DZA), Qatar-Iran (QAT-IRN), Qatar-Jordan (QAT-JOR), Qatar-Kuwait (QAT-KWT), Qatar-Lebanon (QAT-LBN), Qatar-Oman (QAT-OMN), Qatar-Tunisia (QAT-TUN), Qatar-Turkey (QAT-TUR), and Qatar-Yemen (QAT-YEM).[11] The in-placebo approach employed in this paper is similar to the falsification test used by Heilmann (2016).

The results are reported in Figure 11. The concern that Qatar's post-embargo trade with Iran, Oman, and Turkey may have been the consequence of spillover effects (see section 3.3) seems to be confirmed in Figure 9. In all three cases, actual trade flows increase substantially following the imposition of the embargo and are substantially higher to that of their synthetic estimates. This suggests that Qatar deflected or otherwise substituted lost trade flows with the Tripartite to these three countries. Removing them from the donor pool is thus warranted. At the same time, this embargo-effect does not seem to be isolated to these countries alone. Rather,

[11] Note that none of the QAT-IRN, QAT-OMN, and QAT-TUR were included in the donor pool in the main analysis due to the potential risk of spillover effects.

Qatar's actual trade relative to that of their synthetic counterfactual outcomes greatly increases with countries who were neutral in the conflict, i.e., Algeria, Kuwait, and Lebanon. Tunisia is the exception where synthetic trade flows outstrip that of actual. Nonetheless, this suggests that trade deflections between Qatar and other third countries in the region were more far-reaching than anticipated. However, the pre-embargo resemblance between actual vs. synthetic trade flows are poor which give rise to concerns in regard that other idiosyncratic shocks may be influencing the construction of the synthetic estimator. Given the spike in trade between Qatar and its placebo trade partners, their removal from the donor is warranted since their incorporation may have attenuated or otherwise biased the synthetic estimator.

In the case of Qatari trade with Jordan and Yemen, they both follow a negative trending trajectory prior to the embargo. In the case of Yemen, this can relate to the civil war in Yemen that started in 2014. Thus, it can be interpreted that Qatar-Yemen trade evolution is not unusual relative to the embargo effect. However, in the case of Jordan, it suggests that trade flows were negatively impacted even for those countries that cut-back or seized diplomatic relations but did not participate in the embargo. As with the case of the neutral countries, it shows that the effect of the embargo does spread to other countries although they are not themselves targeted or directly involved.

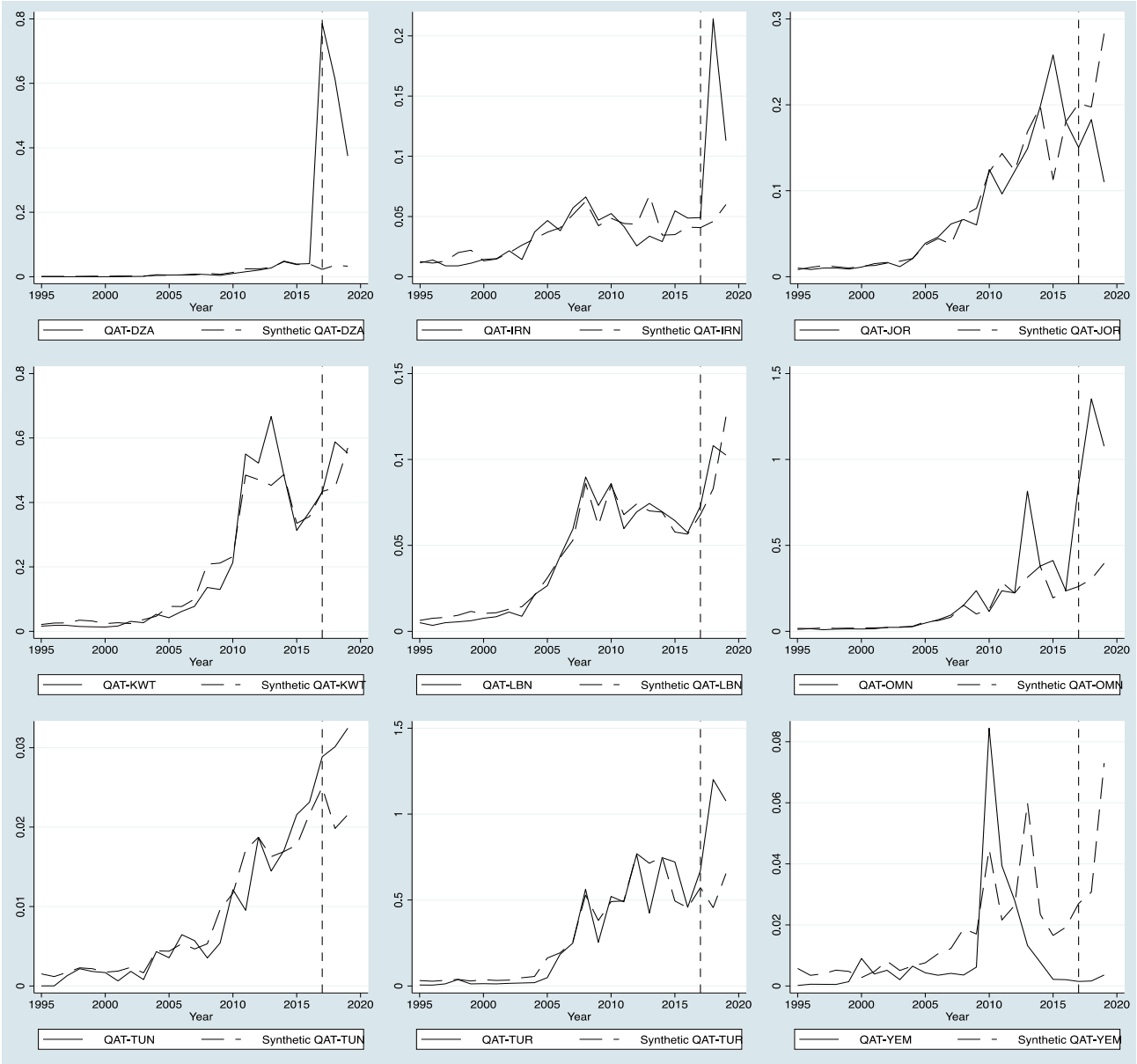

Figure 11: Average bilateral two-way trade (billions of US$) between Qatar and its in-place placebo-treated trade units.

4.2.3 Lag Robustness Test

In line with McClelland and Gault's (2017) findings, the results from the lag robustness test show that employing different lag structures has a significant impact on the synthetic estimator's ability to reproduce bilateral trade outcomes in absence of the embargo. As can be seen in Figure 12-14, the one lag and the average of the outcome variable across the whole pre-embargo period perform the worse for all the treated trade pairs in terms

of pre-embargo fit. In line with Kaul et al. (2018) observations, differences in one lag and pre-intervention average results seldom leads to markedly different result in terms of pre-treatment fit. Although the fit improves with the three- and five-year lags, it is still poor and dissimilar between the two lag structures. In addition, the lag structures for all treated trade pairs differ substantially in terms of post-embargo projections which points towards over- and underestimations of the synthetic outcomes had the embargo not been imposed. The promising exception across all specifications in terms of pre-embargo fit is when all lagged outcomes of the pre-treatment period is used as predictors. They closely resemble the patterns of actual Qatari-Tripartite trade flows and, as prescribed by the SCM-literature, should as a result provide the most accurate synthetic estimate of trade flows had the treatment not occurred. As in the case of the main analysis, the synthetic estimator indicates that actual trade flows would in the absence of the embargo be substantially higher between Qatar, Bahrain, Saudi Arabia, and the UAE. Moreover, the results are similar to those in the main analysis (see Figure 3-5), which indicates that bilateral trade relative to that of their synthetic estimates dropped to what can be interpret as an autarky-like state.

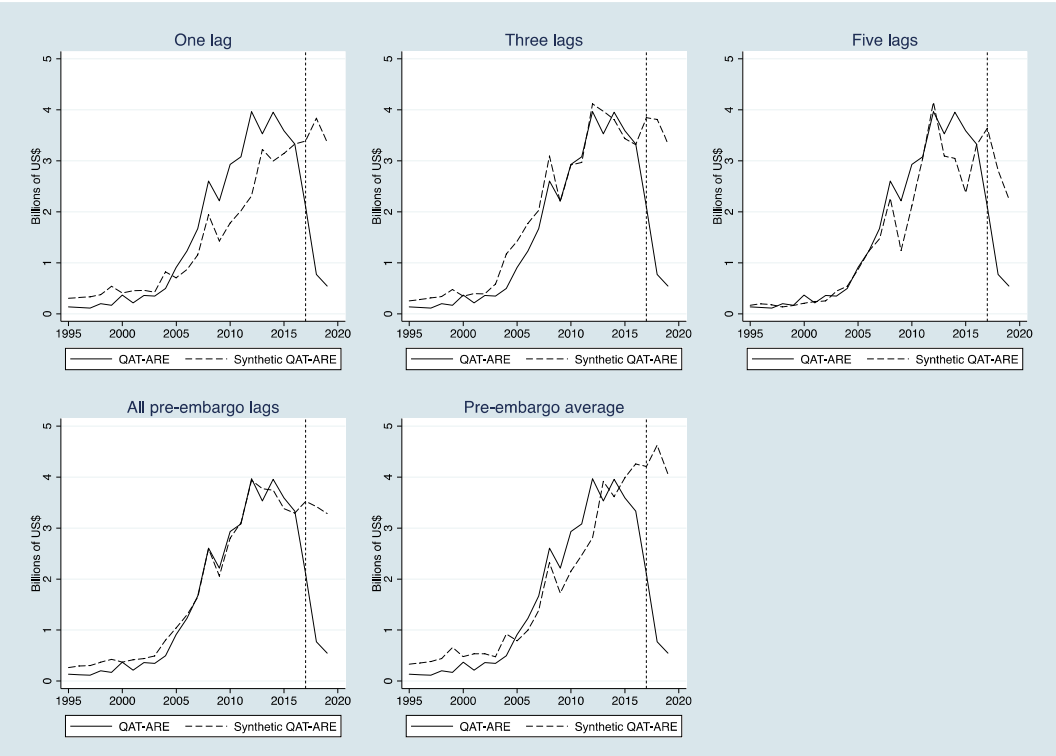

Figure 12: Different lag structures for the two-way average bilateral trade between Qatar and the UAE.

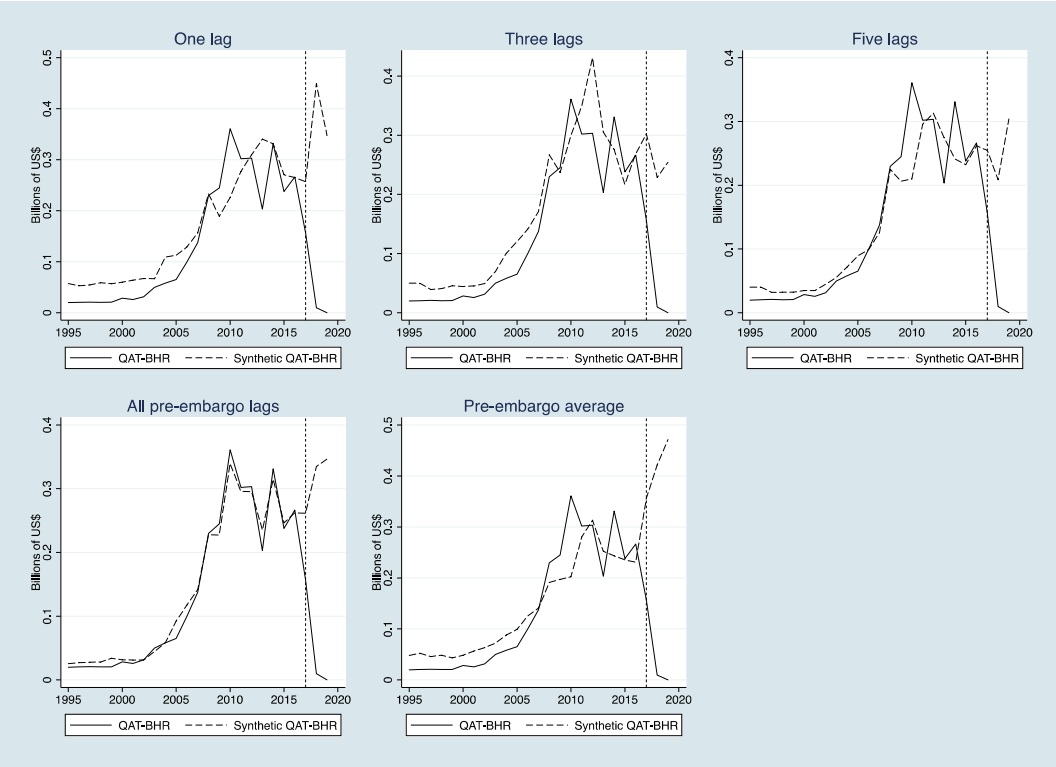

Figure 13: Different lag structures for the two-way average bilateral trade between Qatar and Bahrain.

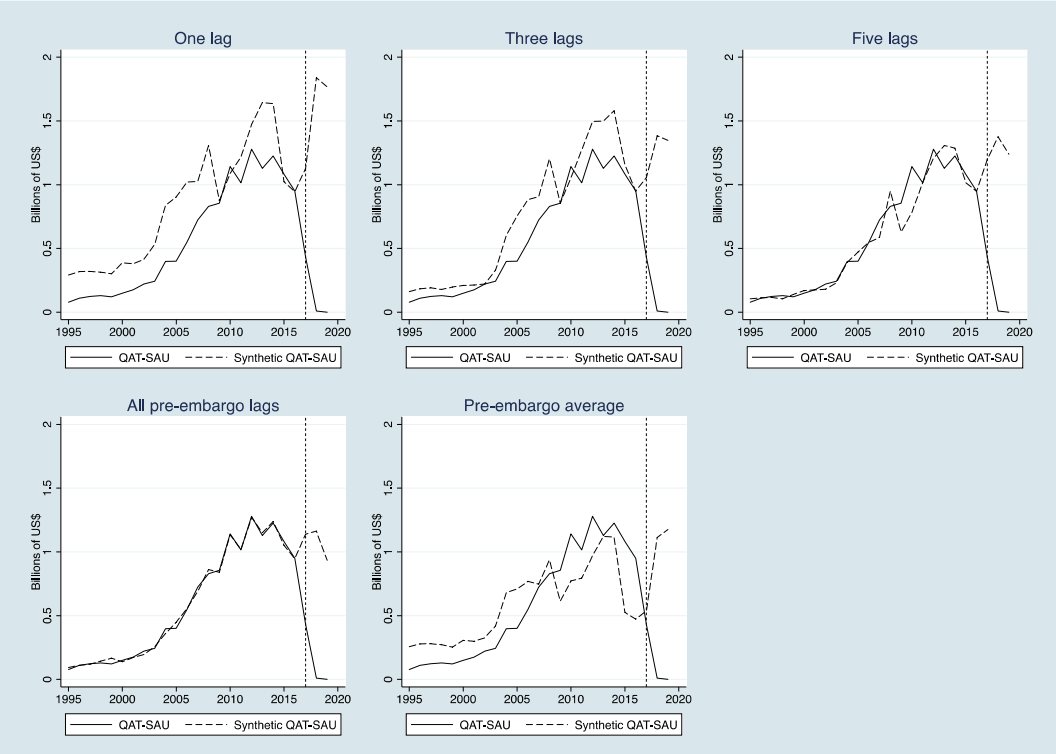

Figure 14: Different lag structures for the two-way bilateral average trade between Qatar and Saudi Arabia.

4.3 The SCM Results – Interpretation and Discussion

Following the SCM-theory as prescribed by Abadie's (2020) and (Abadie et al., 2010; 2015), the in-time and in-place placebo tests show that multiple factors negatively influence the synthetic counterfactual outcomes. This is the case since the results of the former implies that anticipatory effects and idiosyncratic shocks prior to the embargo is at hand whereas the latter suggests that a majority of the selected control units were affected by the embargo via spillover effects. Both of which should have a negligible impact on the synthetic estimates in order to be viewed as credible. In case of the in-time placebo tests, they show that the post-embargo counterfactual estimators are substantially dissimilar to the ones constructed in the main analysis, which should not occur if the synthetic estimators are to be consider credible. Moreover, the difference in synthetic estimator patterns may be the result of disparities across the control units, meaning that the synthetic estimator is simply a reflection of dissimilarities between the control and treated trade units (Abadie et al., 2015; Abadie, 2020). In the case of in-place placebo, the synthetic estimates can still be considered credible if a minority of the control units display a significant treatment effect. Such is the case with Heilmann (2016) which finds that, during the Chinese boycott of Japanese goods in 2012, Japanese exports were substituted towards Thailand and the U.S. but not to other countries in the control group. In this paper, however, a majority of the control units display signs of treatment which stipulates that the embargo effect is not isolated to Qatar and the Tripartite. Significant evidence to its synthetic impact on Qatari-Tripartite trade can thus not be established. Due to the abovementioned erroneous sources, the placebo tests indicate that synthetic estimates, in the absence of treatment, do not reflect the effect upon trade outcomes and lack the necessary predictive power.

In light of the placebo tests, a question arises as to the appropriateness of the donor pool and corresponding (potential) control units of this paper. Adopting a similar donor pool approach to that of Adarov (2018), Heilmann (2016), and Saia (2017) may have not been applicable to the embargo against Qatar. Even when limiting the donor pool to two different subsets, dissimilarities seem to linger, and the performance of the synthetic estimator does not markedly improve. Although numerical criteria have an objective advantage, such approach may prove problematic if it's done to only one variable. Indeed, a treated unit and that of control units may display a similar trade pattern may not be similar in terms of other covariates deemed relevant

to understand the outcome variable at hand. In such as case, the donor pool and its corresponding control units may be unfit to construct the synthetic estimator. However, whether the observed dissimilarities in the main analysis are primarily driven by disparities among the control units in the donor pool or (un)observed idiosyncratic shocks at large is hard to determine or discern between. However, a combination of the two seems to be the most probable explanation.

The empirical results obtained from the lag-robustness analysis seems to, at first glance, concur with that of the placebo tests. Firstly, the inconsistency in both pre- and post-embargo trajectories points toward that the SCM is merely picking up shocks and factors other than the embargo that in turn is reflected in different pre-embargo reproductions and post-embargo projections of the synthetic estimator. According to McClelland and Gault (2017) and Kaul et al. (2018), such inconsistencies can also be due to the change in distribution of weights across the comparison group that derives from the change in lags. The synthetic estimator weight composition for each treated pair lends support to this as weights significantly change across the different lag-specifications. Secondly, Kaul et al. (2018) notes that synthetic estimates may be sensitive to the inclusion of certain pre-treatment lags of the outcome variable. Such sensitivity could allude to the dissimilarity between using three or five lags for the pre-embargo period, i.e. depending on the years lagged the synthetic estimator may capture changes occurring in these specific years and thus illustrate a dissimilar pre-treatment reproduction path. From this perspective, control group reassignment in terms of weights coupled with idiosyncratic shocks seem to be important factors behind the dissatisfactory synthetic counterfactual outcomes in the absence of the embargo. Together with the in-time and in-place placebo results, the synthetic estimates should be interpreted as unfit to follow pre-embargo trade movements and their subsequent post-embargo projections credibly and accurately.

At the same time, there seems to be an association between lagging the outcome variable and the synthetic estimator's performance in reproducing pre-treatment pathways. Indeed, using all pre-treatment outcome lags as predictors generated a pre-embargo trade pattern that closely mirrors that of the treated trade units. This would suggest that previous SCM-specifications were not lagged enough, thus resulting in dissimilar synthetic estimators. Although using all

lags of the outcome variable seems like a preferred approach[12], a notable caveat with such specifications is that it renders other covariates irrelevant. Thus, such SCM-specifications may lead to a potentially biased estimator as the explanatory information carried by the covariates are ignored (Kaul et al., 2018). On the other hand, Kaul et al. (2018) notes that excluding covariates and fitting solely on lagged outcomes may prove beneficial in capturing unobserved confounders. This since unobserved cofounders and ignored observed covariates are not too different from each other and, even in the presence of the former, the SCM is asymptotically[13] unbiased. Capturing unobserved confounders may thus improve the efficiency of the synthetic control as reflected in greater pre-intervention resemblance. Moreover, Kaul et al. (2018) comment that if the included covariates lack relevant explanatory power to the outcome variable of interest, using all the lagged outcomes as predictors may be a more appropriate approach.

From this perspective, the diverging synthetic reproductions of pre-embargo trade flows in this paper may be, in part, because the included covariates' predictive power over that of lagged outcome predictors is limited in terms of explaining bilateral trade flows in the MENA-region. For example, Lee and Gohar (2010) and Youseff and Hassan (2000) allude that trade patterns in the MENA-region can be in part be driven by political factors that may supersede that of conventional indicators for economic integration, such as geographical proximity, linguistic, religious, and cultural similarities. In addition, Kastner (2007) show that competing political interests may impact trade flows in the case of several countries in the Middle East. Non-political factors may also play an important role. As the economies of Qatar and the Tripartite are all heavily dependent on hydrocarbon exports, they are subject to the volatility and fluctuations in hydrocarbon prices. Such mechanism may also allude to the highs and troughs observed in actual Qatari-Tripartite trade flows. Moreover, hydrocarbon prices may act as unobserved confounders and indicators of idiosyncratic shocks that influence the construction of the synthetic counterfactual outcome.[14] As a result, the absence of covariates that could potentially capture these political and market relationships may be reflected in dissatisfactory reproductions of pre-embargo trade patterns. Additionally, the presence of anticipation effects

[12] Several studies in various fields that employ the SCM have used this approach, see e.g. Bilgel and Galle (2015), Billmeier and Nannicini (2013), Bohn, Lofstrom, and Raphael (2014), Cavallo et al. (2013), Hinrichs (2012), Kreif et al. (2016), Liu (2015), Nannicini and Billmeier (2011), and Stearns (2015).

[13] "Asymptotically" refers to when the pre-treatment period grows to infinity (Kaul et al., 2018).

[14] To better control for the impact of oil and natural gas price fluctuations on trade outcomes, I tried to include them as determinants in the main analysis SCM. Further, I assumed that countries in the treated units and potential comparison units in the donor pool face the same hydrocarbon prices on global markets. However, due to the non-existence of variation in these predictors among the donor pool, the Synth-command in Stata could not run the regression.

and spillover/trade deflection effects and their respective impact on trade may be hard to capture using the covariates employed in this paper. Such unobserved confounders may thus warrant the use of all the pre-embargo outcome lags as predictors unless other covariates can be used in the SCM-specification to reflect their impact.

The choice of lag structure and relevant covariates in the SCM is subject for more discussion, especially in the context of sanctions and embargoes where there seems to be many unobserved factors involved. Indeed, Kaul et al. (2018) states that "to what extent ignoring the covariates is harmful depends on the length of the pre-treatment timespan as well as the unobserved confounders' and observed covariates' importance for explaining the outcome" (p. 11). Although this may be a viable and even accurate approach, it must be viewed in relation to the other test results of this paper. As such, the synthetic estimator credibility and accuracy in predicting counterfactual outcomes in the absence of the embargo should still be viewed with caution.

4.4 The Relevance of Ex Ante and Ex Post Factors

As shown by the in-time and in-place placebo tests, sanctions and embargoes may have both anticipatory and spillover effects associated with them. Such ex ante and ex post dimensions may provide further insights into the overall impact of embargoes and sanctions on international trade. This warrants a discussion as to their potential relevance in the context of Qatari-Tripartite trade prior and after the imposition of the embargo.

To begin with, the reduction in trade between Qatar and the Tripartite prior to the embargo seems to follow the traditional hypothesis that sanctions/embargoes and their corresponding anticipation effects can give rise to negative trade outcomes between the target and sender country (see e.g. Morrow et al, 1998; Anderson and Marcouiller, 2002). Indeed, several studies find a negative relationship between political and diplomatic tensions and international trade (see e.g. Fuchs and Klann, 2013; Heilmann, 2016; Morrow et al., 1998; Morrow, 1999). The likelier a political or diplomatic tension erupts, the higher the risk that trade disruptions occurs. To compensate for the increased risk of such trade disruptions, trade must become more profitable (Morrow et al., 1998). One way forward-looking economic agents may limit their ex ante exposure is by anticipating the potential dispute and adopt strategies that minimizes the

economic burden (Morrow, 1999). Such ex ante behavior by producers and consumers can manifest itself through stockpiling (van Bergeijk, 2009), prompt diversification away from sources of supply exposed to or targeted by potential trade measures (Hoekman and Leidy, 1989), or other adjustments in the target's economic structure (van Bergeijk and van Marrewijk, 1995). Applied to the Qatari embargo, trade patterns may have shifted as a result of the diplomatic tension in 2014. As a response firms may have become more wary of future tensions and adjusted its business behavior accordingly. In turn, this is reflected in pre-embargo trade-deterioration between Qatar and the Tripartite.

Trade-deteriorating effects can also be linked to other diplomatic elements between states. Previous literature finds that trade-creating effects are associated with the creation of foreign missions in a country (Rose, 2007), export promoting agencies (Gil et al., 2008), and state visits (Nitsch, 2007). Empirically, Rose (2007) shows that the creation of an embassy or consulate has a positive impact on exports through their export promotion activities. Conversely, and considering that the Tripartite recalled their ambassadors from Qatar in 2014, it can be argued that the temporary or permanent closure of foreign missions have a opposite and deteriorating effect on trade. Although the Qatari-Tripartite diplomatic dispute was settled in 2014, the trade-deteriorating effects associated with the withdrawal of one's diplomatic representation may have lingered prior to the embargo. Such analysis goes in line with that of Fuchs and Kann's (2013) findings. They show that the negative bilateral trade effect between China and countries that received the Dalai Lama did not disappear until the second year after a meeting with the spiritual leader took place. Reductions in trade flows as a consequence of scaling back one's diplomatic representation also seems like a plausible explanation to the in-place placebo findings. That is, the decision of other states in the region to cut-back on diplomatic relations following the imposition of the embargo may explain the post-embargo decline in bilateral trade flows between Qatar and, for example, Jordan.

Parallel to the ex ante anticipatory and diplomatic tension effects on trade, the degree of trade deflection seems to play a significant ex post role in the embargo against Qatar. As shown by the in-place placebo test, Qatari trade increased substantially with sympathetic or neutral countries within their geographical vicinity. This is indicative that Qatar's bilateral trade with countries not included in this paper may have increased as well, thus mitigating the negative trade impact brought by the embargo further. These findings resonate with those of Evenett (2002) and Haidar (2017) which find that significant trade deflections mitigated the impact of

sanctions targeted towards South Africa and Iran. The latter finds that as much as two-thirds of Iranian exports were diverted to other non-sanctioning markets. Furthermore, high levels of diversion in these cases can be attributed to the non-universal application of the implemented sanctions which allows firms in the targeted country to find new markets for their goods (Gullstrand, 2020). Interestingly, and contrary to contemporary findings, the embargo against Qatar illustrates how a comprehensive embargo can still give rise to significant levels of trade diversion. Considering the level of trade diversion with that of the control units, foregone trade with the Tripartite may have to a large extent been replaced. Haidar (2017) argues that a strong level of diversion in core exports may reflect that firms are more competitive and productive and can thus withstand the effects of sanctions more. Conversely, importers may have been equally efficient in substituting lost imports with those from other countries. In addition, relatively more competitive exporters from other countries could have moved in and replaced the trade lost as a result of the embargo. Which, if any, factor applies best to Qatar is discussable, however, it pinpoints that heterogenous effects are at hand and may provide a greater understanding for how trade flows were redirected and reformed as a consequence of the embargo.

4.5 The SCM and its (in)abilities

The SCM provides new econometric tools to evaluate policies, events, and shocks at the aggregate level. However, as the main results and the corresponding placebo and robustness tests can attest to, the SCM is sensitive to shocks, parallel anticipation and spillover effects, and lag structures. This highlights one of the shortcomings of the SCM, that is, its inability or lack thereof to account for unobserved heterogeneity, something that is common across trade partners. That is, there are bilateral trade factors and variations that may impact the outcome of trade patterns that are not captured by the included covariates. Through a panel data approach, the bias generated from heterogeneity across countries can also to a larger extent be mitigated, specifically through fixed effects (Cheng and Wall, 2005). Previous econometric methods, notably the difference-in-difference model, thus function as a good complement or even supplement to the SCM in order to control for dyadic heterogeneity. Such a panel data approach

was used by Heilmann (2016) and may prove valuable when studying the trade effects of the embargo against Qatar or other sanction-related event.

Parallel to panel data approaches, there are methods and techniques within the SCM literature that could also mitigate or otherwise ameliorate the identified issues when applying the SCM to the case of the embargo against Qatar. One way is through further incorporation of structural properties and elements in the SCM. For example, Abadie and l'Hour (2019) proposes that a penalty term is incorporated into the synthetic control estimators to penalize inappropriate matching between the treated units and every individual comparison unit that contributes to the construction of the synthetic estimator. Such mechanism would help to better safeguard against discrepancies found in pairwise matching that in turn give rise to interpolation biases. Further, in settings where there are multiple treated units, another practical challenge arises where the synthetic estimator may be unable to closely reproduce the predictor values for one or several treated units, or close resemblance can only be achieved through the pairwise matching of control units that has large discrepancies in its own predictor values relative to that of the treated units. In such a scenario, there are concerns that the matching of the treated units and their respective synthetic estimates is a result of potential biases that originates from matching discrepancies (Abadie, 2020). To account for these complications, Abaide and l'Hour (2019) and Ben-Michael et al. (2021) suggests that the synthetic estimator undergoes modifications that is similar to the bias-correction methods used by Abadie and Imbens (2011), Quade (1982), and Rubin (1973). That is, the biasness of the synthetic controls is attenuated through regression-adjustments in contexts where the predictor values of the comparison units do not closely resemble or reproduce treated units' predictor values.

Another possibility to improve the fit of synthetic control estimator to that of actual outcomes is to allow for extrapolation within the weight construction and assignment of potential control units. Specifically, Doudchenko and Imbens (2016) suggests that weights must not take a value between zero and one (as is the case in this paper) and may go beyond such restrictions to include negative weights that do not sum to one. This would allow the level of the synthetic estimator to have a constant shift relative to that of linear combinations of the control groups' outcomes. At the same time, allowing for extrapolation opens up for more uncertainty in regard to how weights are assigned as they would no longer follow the standard framework where the sum of weights add up to one. In turn, such a set-up begs the questioned if the ability of the

synthetic estimator to mirror that of actual outcomes is credible or if close resemblance has been erroneously constructed (Abadie, 2020).

In light of this, the SCM is still a nascent econometric toolkit, and it's set of methods is continued to be expanded and enriched (noteworthy mentions include Chernozhukov et al., 2019; Gobillon and Magnac, 2016; Li, 2020; and Xu, 2017). Future research is therefore encouraged to investigate this further in order to enhance the SCM as an econometric method and, in turn, its application in studies relating to sanctions and embargoes.

5 Conclusion

In this paper, I quantify and analyze the trade effects of the embargo against Qatar that was imposed by Bahrain, Saudi Arabia, and the UAE in 2017. The synthetic control method (SCM) is utilized as proposed by Abadie and Gardeazabal (2003) and Abadie et al. (2010; 2015) to construct a synthetic counterfactual outcome that shows how two-way average bilateral trade flows would have been in the absence of the embargo. The empirical analysis is undertaken using a large donor pool containing 203 (potential) comparison units over the period 1995 to 2019.

In line with previous empirical studies (see e.g. Caruso, 2003), I find an immediate and substantial reduction in bilateral trade flows between Qatar and the Tripartite-countries following the imposition of the embargo. Conversely, the synthetic estimates suggests that trade flows would have been substantially higher had the embargo not been imposed. Disaggregated analysis over the post-embargo period further indicates that the adverse trade effect was amplified across the post-embargo period suggesting an inverse relationship between reduced trade flows and the duration of the embargo. There is also evidence that indicates that both considerable anticipation and spillover effects exists. Their presence and extensiveness show that the effect of an embargo is far more reaching than anticipated and affects trade patterns beyond those of the sender and target countries. Such findings also indicate how the impact of trade derived by an embargo works in a world characterized and shaped by extensive supply and value chains and a great degree of interconnectivity between countries.

The findings should, however, be considered in light of the limitations of this paper. In line with the caveats identified by Abadie et al. (2010; 2015) and Abadie (2020), the presence of idiosyncratic, anticipation, and spillover effects implicate the construction of credible synthetic estimates in the absence of the embargo. The estimates' sensitivity to different lag structures, as prescribed by Kaul et al. (2018) and McClelland and Gault (2017), further deteriorates the possibility to generate credible post-embargo projections. Biased estimations of the synthetic counterfactuals are therefore likely and thus warrants a cautious interpretation of the SCM results. In addition, the absence of previous empirical studies on the Qatari embargo makes it

difficult to assess the reliability and accuracy of the embargo's impact on Qatari-Tripartite trade flows. In light of this, the novel attempt to apply a relatively new econometric method to a so far unstudied event is perhaps the main contributions of this paper. Considering the proliferated use of trade policies in conflicts, their relevance in world politics has only grown and will continue to do so. From that perspective, both sender and target countries will seek to develop different mechanisms that maximizes and minimizes their adverse effects respectively. Thus, understanding trade policies' direct as well as indirect impact on international trade is crucial for future policymaking, especially when to navigate in a world increasingly shaped and characterized by geopolitical tensions.

While the empirical results of this study show that the embargo brought a sharp and substantial decline in Qatari-Tripartite trade flows, future research could aim to further analyze ex ante and ex post trade patterns that embargoes and sanctions at large give rise to. Discerning how anticipatory dimensions affect trade flows prior to the imposition of sanctions, in face of political tensions or threat of sanctions, as shown by e,g, Abesorgbor (2019) and Fuchs and Klann (2013), may provide new insights into sanctions and embargoes' trade-deteriorating effects. Similarly, more rigorous evaluation of trade deflections may explain why some sanction- and embargo-campaigns generate more adverse effects than others. Naturally, investigating how these ex ante and ex post factors may fit within a SCM-framework is of interest too. Additionally, this paper only investigates the short-term impact of the embargo on trade outcomes. Although Qatar and the Tripartite-countries re-established diplomatic ties and ended the embargo in 2021, the question is whether bilateral trade will rebound to pre-embargo levels. More importantly, re-forging political and diplomatic trust may take significantly longer time to mend in which trade relations may be inhibited in the long run. Future research is thus encouraged to study this relationship further.

References

Abadie, A. (2020). [Forthcoming]. Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects, *Journal of Economic Literature*. [Online]. [Accessed December 30th, 2020]. Available from: https://www.aeaweb.org/articles?id=10.1257/jel.20191450.

Abadie, A., & Gardeazabal, J. (2003). The economic costs of conflict. A case study of the Basque Country, *American Economic Review*, vol. 93, no. 1, pp. 113–132.

Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic control methods for comparative case studies. Estimating the effect of California's tobacco control program, *Journal of American Statistics Association*, vol. 105, no. 490, pp. 493–505.

Abadie, A., Diamond, A., & Hainmueller, J. (2015). Comparative politics and the synthetic control method, *American Journal of Political Science*, vol. 59, no. 2, pp. 495-510.

Abadie, A. & Imbens, G. W. (2011). Bias-corrected matching estimators for average treatment effects, *Journal of Business & Economic Statistics*, vol. 29 , no. 1, pp. 1-11.

Abadie, A. and J. L'Hour (2019). A penalized synthetic control estimator for disaggregated data, Working paper, Available at: https://sites.google.com/site/jeremylhour/research [Accessed 11 October 2021].

Adarov, A. (2018). Eurasian Economic Integration: Impact Evaluation Using the Gravity Model and the Synthetic Control Methods, wiiw Working Paper, no. 150, The Vienna Institute for International Economic Studies (wiiw).

Afesorgbor, S. K. (2019). The impact of economic sanctions on international trade: How do threatened sanctions compare with imposed sanctions?, *European Journal of Political Economy*, vol. 56, pp. 11-26.

Ahn, D. P., and Ludema, R. D. (2020). The sword and the shield: The economics of targeted sanctions, *European Economic Review*, vol. 130, pp. 1-21.

Alkhalisi, Z. (2017). Qatar keeps gas flowing to UAE despite blockade, CNN Money, June 7, Available at: https://money.cnn.com/2017/06/07/news/qatar-uae-gas-pipeline/index.html [Accessed the 11 of October 2021].

Allen, S. H. (2005). The Determinants of Economic Sanctions Success and Failure, *International Interactions*, vol. 31, no. 2, pp. 117-138.

Allen, S. H. (2008). The Domestic Political Costs of Economic Sanctions, *Journal of Conflict Resolution*, vol. 52, no. 6, pp. 916–944.

Amiti, M., Redding, S. J., & Weinstein, D. E. (2019). The Impact of the 2018 Tariffs on Prices and Welfare, *Journal of Economic Perspectives*, vol. 33, no. 4, pp. 187-210.

Anderton, C. H., & Carter, J. R. (2001). The Impact of War on Trade: An Interrupted Times-Series Study, *Journal of Peace Research*, vol. 38, no.4, pp. 445–457.

Anderson, J. E., & Marcouiller, D. (2002). Insecurity and the pattern of trade: an empirical investigation, *The Review of Economics and Statistics*, vol. 84, no. 2, pp. 342-352.

Baier, S. L., & Bergstrand, J. H., (2009). Estimating the effects of free trade agreements on international trade flows using matching econometrics, *Journal of International Economics*, vol. 77, no. 1, pp. 77-85.

Baldwin, D. A. (1971). The Power of Positive Sanctions, *World Politics*, vol. 24, no. 1, pp. 19-38.

Bapat, N. A., & Morgan, T. C. (2003). Imposing Sanctions: States, Firms and Economic Coercion, *International Studies Review*, vol. 5, no. 4, pp. 65-79.

Barbieri, K. (2002). The Liberal Illusion: Does trade promote peace?, University of Michiang Press, Ann Arbor.

Bayer, R., & Rupert, M. C. (2004). Effects of Civil Wars on International Trade, 1950– 1992, *Journal of Peace Research*, vol. 41, no. 6, pp. 699–713.

Ben-Michael, E., Feller, A., & Rothstein, J. (2021). The augmented synthetic control method, *Journal of the American Statistical Association*, pp. 1-27

Bianco, C., & Stansfield, G. (2018). The intra-GCC crises: mapping GCC fragmentation after 2011, *International Affairs*, vol. 94, no. 3, pp. 613-635.

Billmeier, A., & Nannicini, T. (2013). Assessing Economic Liberalization Episodes: A Synthetic Control Approach, *The Review of Economics and Statistics*, vol. 95, no. 3, pp. 983–1001.

Bilgel, F., & Galle, B. (2015). Financial incentives for kidney donation: A comparative case study using synthetic controls, *Journal of Health Economics*, vol. 43, pp. 103–117.

Bohn, S., Lofstrom, M., & Raphael, S. (2014). Did the 2007 Legal Arizona Workers Act Reduce the State's Unauthorized Immigrant Population?, *The Review of Economics and Statistics*, vol. 96, no. 2, pp. 258–269.

Britannica (2021). Gulf Cooperation Council. Available at: https://www.britannica.com/topic/Gulf-Cooperation-Council. [Accessed 11 October 2021].

British Broadcasting Cooperation (BBC). (2021a). Qatar crisis: Saudi Arabia and allies restore diplomatic ties with emirate, 5 January, Available at: https://www.bbc.com/news/world-middle-east-55538792 [Accessed 12 of April 2021].

British Broadcasting Cooperation (BBC). (2021b). UAE to restore Qatar trade and travel links 'within a week' after row ends, 7 January, Available at: https://www.bbc.com/news/world-middle-east-55579014 [Accessed 12 of April 2021].

Brooks, R. A. (2002). Sanctions and Regime Type: What Works, and When?, *Security Studies*, vol. 11, no. 4, pp. 1-50.

Card, D. (1990). The Impact of the Mariel Boatlift on the Miami Labor Market, *Industrial and Labor Relations Review*, vol. 43, no. 2, pp. 245-257.

Card, D., & Krueger, A. B. (1994). Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania, *The American Economic Review*, vol. 90, no. 5, pp. 1397-1420.

Caruso, R. (2003). The Impact of International Economic Sanctions on Trade: An Empirical Analysis, *Peace Economics, Peace Science, and Public Policy*, De Gruyter, vol. 9, no. 2, pp. 1-36.

Cavallo, E., Galiani, S., Noy, I. & Pantano, J. (2013). Catastrophic Natural Disasters and Economic Growth, *The Review of Economics and Statistics*, vol. 95, no. 5, pp. 1549–1561.

Centre d'Études Prospectives et d'Informations Internationales (CEPII). (2021). Available online: http://www.cepii.fr/cepii/en/bdd_modele/bdd.asp [Accessed 3rd of February 2021].

Charfeddine, L., & Al-Refai, H. (2019). Political tensions, stock market dependence and volatility spillover: Evidence from the recent intra-GCC crises, *North American Journal of Economics and Finance*, vol. 50.

Cheng, H., & Wall, H. J. (2005). Controlling for Heterogeneity in Gravity Models of Trade and Integration, *Review*, vol. 87, no. 1, pp. 49-63.

Chernozhukov, V., Wüuthrich, K. & Zhu, Y. (2019). An Exact and Robust Conformal Inference Method for Counterfactual and Synthetic Controls, Journal of the American Statistical Association, pp. 1-16.

Disdier, A.-C., & Head, K., (2008). The puzzling persistence of the distance effect on bilateral trade, *The Review of Economics and Statistics*, vol. 90, no. 1, pp. 37-48.

Doudchenko, N., & Imbens, G. W. (2016). Balancing regression, difference-in-differences and synthetic control methods: A synthesis, NBER Working Papers, no. 22791, pp. 1-33

Drezner, D. W. (2000). Bargaining, Enforcement, and Multilateral Sanctions: When is Cooperation Counterproductive?, *International Organization*, vol. 54, no. 1, pp. 73–102.

Drezner, D. W. (1999). The Sanctions Paradox: Economic Statecraft and International Relations, New York: Cambridge University Press.

Crozet, M., & Hinz, J. (2020). Friendly fire: the trade impact of the Russian sanctions and counter-sanctions, *Economic Policy*, vol. 35, no. 101, pp. 97-146.

Dube, A., & Zipperer, B. (2015). Pooling multiple case studies using synthetic controls: An application to minimum wage policies. IZA Discussion Papers 8944. Bonn, Germany: Institute of Labor Economics.

Early, B. R. (2009). Sleeping with Your Friends' Enemies: An Explanation of Sanction-Busting Trade, *International Studies Quarterly*, vol. 53, no. 1, pp. 49-71.

Early, B. R., & Preble, K. A. (2020). Going Fishing versus Whale Hunting: Explaining Changes in How the US Enforces Economic Sanctions, *Security Studies*, vol. 29, no. 2, pp. 231-267.

Egger, P., & Tarlea, F. (2015). Multi-Way Clustering Estimation of Standard Errors in Gravity Models, *Economics Letters*, vol. 134, pp. 144–147.

Evenett, S. J. (2002). The Impact of Economic Sanctions on South African Exports. *Scottish Journal of Political Economy*, vol. 49, no. 5, pp. 557–573.

Ferman, B., Pinto, C., Possebom, V., & Barnow, B. (2020). Cherry Picking with Synthetic Controls, *Journal of Policy Analysis & Management*, vol. 39, no. 2, pp. 510-532.

Finn, T. (2017). Qatar vows no surrender in Gulf crisis as U.S., Kuwait seek solution, Reuters, 8 June, Available online: https://www.reuters.com/article/us-gulf-qatar-idUSKBN18Z1C9 [Accessed 11 of October 2021].

FlightRadar24. (2017). Available online: https://www.flightradar24.com/25.27,51.61/10 [Accessed 6 of April, 2021].

Fuchs, A., & Klann, N. H. (2013). Paying a visit to the Dalai Lama: Effect on International Trade, Journal of International Economics, vol. 91, no. 1, pp. 164-177.

Galtung, J. (1967). On the Effects of International Economic Sanctions: With Examples from the Case of Rhodesia, *World Politics*, vol. 19, no. 3, pp. 378–416.

Gary, K., Keohane, R. O., & Verba, Sidney. (1994). Designing Social Inquiry: Scientific Inference in Qualitative Research. Princeton, NJ: Princeton University Press.

Gartzke, E., Li, Q., & Boehmer, C. (2001). Investing in the Peace: Economic Interdependence and International Conflict, *International Organization*, vol. 55, no. 2, pp. 391-438.

Geddes, B. (2003). Paradigms and Sand Castles: Theory Building and Research Design in Comparative Politics, Ann Arbor: University of Michigan Press.

George, A. L., & Bennett, A. (2005). Case Studies and Theory Development in Social Sciences, Cambridge MA: MIT Press.

Gil, S., Llorca, R., & Martínez Serrano, J. A. (2008). Measuring the impact of regional export promotion: The Spanish case, *Papers in Regional Science*, vol. 87, no. 1, pp. 137-146.

Gobillon, L., & Magnac, T. (2016). Regional policy evaluation: Interactive fixed effects and synthetic controls, Review of Economics and Statistics, vol. 98, no. 3, pp. 535-551.

Gullstrand, J. (2020). What goes around comes around: The effects of sanctions on Swedish firms in the wake of the Ukrainian crisis, *World Economy*, vol. 43, no. 9, pp. 2315-2342.

Haidar, J. I. (2017). Sanctions and Export Deflection: Evidence from Iran, *Economic Policy*, vol. 32, no. 90, pp. 319-355.

Han, B. (2018). The role and welfare rationale of secondary sanctions: A theory and a case study of the US sanctions targeting Iran, *Conflict Management and Peace Science*, vol. 35, no. 5, pp. 474-502.

Hannan, S. A. (2016). The Impact of Trade Agreements: New Approach, New Insights, IMF Working Paper, vol. 16, no. 117, International Monetary Fund (IMF).

Heilmann, K. (2016). Does political conflict hurt trade? Evidence from consumer boycotts, *Journal of International Economics*, vol. 99, pp. 179-191.

Hinrichs, P. (2012). The Effects of Affirmative Action Bans on College Enrollment, Educational Attainment, and the Demographic Composition of Universities, *The Review of Economics and Statistics*, vol. 94, no. 3, pp. 712–722.

Hoekman, B. M., & Leidy, M. P. (1989). Dumping, antidumping, and emergency protection, *Journal of World Trade*, vol. 23, no. 5, pp. 27-44.

Hufbauer, G. C., & Oegg, B. (2000). Targeted Sanctions: A Policy Alternative?. *Law and Policy in International Business*, vol. 32, no. 1, pp. 11-20.

Hufbauer, G. C., Schott, J. J., Elliott, K. A., & Oegg, B. (2009). Economic Sanctions Reconsidered, 3rd, Washington, DC: Peterson Institute for International Economics.

International Monetary Fund. (2021). Direction of Trade Statistics (DOTS), Available online: https://data.imf.org/?sk=388DFA60-1D26-4ADE-B505-A05A558D9A42&sId=1479329334655 [Accessed 3 of February 2021].

Irwin,, D. A. (2005). Welfare Effects of Autarky: Evidence from the Jeffersonian Embargo of 1807–1809, NBER Working Papers, p. 1. Available at: https://search-ebscohost-com.ludwig.lub.lu.se/login.aspx?direct=true&db=edb&AN=61423788&site=eds-live&scope=site [Accessed 11 Ocotber 2021].

Kaempfer, W. H. , Lowenberg, A. D., & Mertens, W. (2004). International Economic Sanctions against a Dictator, *Economics and Politics*, vol. 16, no. 1, pp. 29-51.

Kaitibie, S., & Rakotoarisoa, M. A. (2017). Determinants of Intra-GCC Food Trade, *International Trade Journal*, vol. 31, no. 1-5, pp. 272-293.

Kastner, S. L. (2007). When Do Conflicting Relations Affect Trade?, *Journal of Conflict Resolution*, vol. 51, no. 4, pp. 664-688.

Kaul, A., Klößner, S., Pfeifer, G., & Schieler, M. (2018). Synthetic control methods: Never use all pre-intervention outcomes together with covariates. Working Paper. Available online: http://www.oekonometrie.uni-saarland.de/papers/SCM_Predictors.pdf [Accessed 9 of April 2021].

Kerr, S. (2017). What is behind the extraordinary Gulf dispute with Qatar?, The Financial Times, 5 June, Available at: https://www.ft.com/content/3c6b7224-49d0-11e7-a3f4-c742b9791d43 [Accessed 23 September 2021].

Koch, N. (2021). Food as weapon? The geopolitics of food and the Qatar-Gulf rift, *Security Dialogue*, vol. 52, no. 2, pp. 118-134.

Kreif, N., Grieve, R., Hangartner, D., Turner, A. J., Nikolovna, S, & Sutton, M. (2016). Examination of the Synthetic Control Method for Evaluating Health Policies with Multiple Treated Units, *Health Economics*, vol. 25, no. 12, pp. 1514–1528.

Lee, Y.-F. L., & Gohar, Abdelaziz. (2010). Trade and Intra-regional Integration: Is Arab Region a Potential Candidate for Economic Union?, J*ournal of Economic Cooperation and Development*, vol. 31, no. 3, pp. 67-82.

Lektzian, D., & Souva, M. (2007). An Institutional Theory of Sanctions Onset and Success, *Journal of Conflict Resolution*, vol. 51, no. 6, pp. 848-871.

Li, K. T. (2020). Statistical inference for average treatment effects estimated by synthetic control methods, Journal of the American Statistical Association, vol. 115, no. 532, pp. 2068-2083.

Li, Q., & Reuveny, R. (2011). Does trade prevent or promote interstate conflict initiation?, *Journal of Peace Research*, vol. 48, no. 4, pp. 437-453.

Liu, S. (2015). Spillovers from universities: Evidence from the land-grant program, *Journal of Urban Economics*, vol. 87, pp. 25–41.

Long, A. G. (2008). Bilateral Trade in the Shadow of Armed Conflict, *International Studies Quarterly*, vol. 52, no. 1, pp. 81–101.

Magee, C. S. P. (2008). New measures of trade creation and trade diversion, *Journal of International Economics*, vol. 75, no. 2, pp. 349-362.

Major, S. (2012). Timing is Everything: Economic Sanctions, Regime Type, and Domestic Instability, *International Interactions*, vol. 38, no. 1, pp. 79-110.

Massoud, T. G., and Magee, C. S. P. (2012). Trade and political, military, and economic relations, *Peace Economics, Peace Science, and Public Policy*, vol. 18, no. 1, pp. 1-37.

McClelland, R. & Gault, S. (2017). The Synthetic Control Method as a Tool to Understand State Policy, *Urban Institute*, Available online: https://www.urban.org/sites/default/files/publication/89246/the_synthetic_control_method_as_a_tool_1.pdf [Accessed 8 April 2021].

Meyer, J. A. (2008). Second Thought on Secondary Sanctions, *University of Pennsylvania Journal of International Law*, vol. 30, no. 3, pp. 905-968.

Micco, A., Stein, E., & Ordonez, G. (2003). The currency union effect on trade: Early Evidence from EMU, *Economic Policy*, vol. 37, pp. 315-356.

Mohinksy, B. (2017). Gulf states issues 13 demands to Qatar and set a deadline of 10 days to comply, Business Insider, 17 August, Available online: https://www.businessinsider.com/qatar-gulf-13-demands-2017-6?r=US&IR=T [Accessed 10th of September 2021].

Monroe, K. V. (2020). Geopolitics, Food Security, and Imaginings of the State in Qatar's Desert Landscape, *Culture, Agriculture, Food, & Environment*, vol. 42, no. 1, pp. 25-35.

Morrow, J. D. (1999). How Could Trade Affect Conflict?, Journal of Peace Research, vol. 36, no. 4, pp. 481-489.

Morrow, J. D., Siverson, R. M., & Tabares, T. E. (1998). The Political Determinants of International Trade: The Major Powers, 1907-90, *The American Political Science Review*, vol. 93, no. 3, pp. 649-61.

Peksen, D. (2019). When Do Imposed Economic Sanctions Work? A Critical Review of the Sanctions Effectiveness Literature, *Defence and Peace Economics*, vol. 30, no. 6, pp. 635-647.

Quade, D. (1982). Nonparametric analysis of covariance by matching, *Biometrics*, vol. 38, no. 3, pp. 597-611.

Nannicini, T., & Billmeier, A. (2011). Economies in Transition: How Important Is Trade Openness for Growth?, *Oxford Bulletin of Economics and Statistics*, vol. 73, no. 3, pp. 287–314.

Naylor, R. T. (eds). (2001). Economic warefare: Sanctions, embargo busting, and their human cost [e-book], Northeastern University Press, Available at: Google Books: books.google.com [Accessed 10 October 2021].

Nitsch, V. (2007). State visits and international trade, *World Economy*, vol. 30, no. 12, pp. 1797-1816.

Pinker, S. (2011). The better angels of our nature: why violence has declined, New York: Viking. Available online: http://ludwig.lub.lu.se/login?url=https://search.ebscohost.com/login.aspx?direct=true&db=cat07147a&AN=lub.2493634&site=eds-live&scope=site [Accessed January 13th, 2021].

Reuters (2017). Iran flies food to Qatar amid concerns of shortages, 11 June, Available at: https://www.reuters.com/article/us-gulf-qatar-iran-idUSKBN1920EG?il=0 [Accesses 11 October 2021].

Rose, A. (2000). One money, one market: the effect of common currencies on trade, *Economic Policy*, vol. 15, no. 30, pp. 7-46.

Rose, A. K. (2007). The foreign service and foreign trade: embassies as export promotion, *World Economy*, vol. 30, no. 1, pp. 22-38.

Rubin, D. B. (1973). The use of matched sampling and regression adjustments to remove bias in observational studies, *Biometrics*, vol 29, no. 1, pp. 185-203.

Saia, A. (2017). Choosing the open sea: The cost to the UK staying out of the euro, *Journal of International Economics*, vol. 108, pp. 82-98.

Sciutto, J., & Herb, J. (2017). Exclusive: The secret documents that help explain the Qatar crisis, CNN, July 11, Available at: https://edition.cnn.com/2017/07/10/politics/secret-documents-qatar-crisis-gulf-saudi/index.html [Accessed 20 of September 2021].

Selmi, R., & Bouoiyour, J. (2020). Arab geopolitics in turmoil: Implications of Qatar-Gulf crisis for business, *International Economics*, vol. 161, pp. 100-119.

Solomon, E. (2017). The $1bn hostage deal that enraged Qatar's Gulf rivals, The Financial Times, 5 June, Available at: https://www.ft.com/content/dd033082-49e9-11e7-a3f4-c742b9791d43 [Accessed 20 of September 2021].

Stearns, J. (2015). The effects of paid maternity leave: Evidence from Temporary Disability Insurance, *Journal of Health Economics*, vol. 43, pp. 85–102.

The Economist. (2017). The boycott of Qatar is hurting its enforcers, 19 October, Available at: https://www.economist.com/middle-east-and-africa/2017/10/19/the-boycott-of-qatar-is-hurting-its-enforcers [Accessed 12 of April 2021].

The Economist (2021a). A fraying system: In global trade, power increasingly trumps rules, 6 October, Available at: https://www.economist.com/special-report/2021/10/06/a-fraying-system [Accessed 11 of October 2021].

The Economist (2021b). The Arab League has done little for its members in nearly 70 years, 24 August, Available at: https://www.economist.com/special-report/2021/08/24/the-arab-league-has-done-little-for-its-members-in-nearly-70-years [Accessed 20 of September 2021].

Tomz, M., Goldstein, J. L., & Rivers, D. (2007). Do we really know that the WTO increases trade?, The American Economic Review, vol. 97, no. 5, pp. 2005-2018.

Ulrichsen, K. C. (2020). Qatar and the Gulf Crisis. Oxford University Press. Available at: https://oxford-universitypressscholarship-com.ludwig.lub.lu.se/view/10.1093/oso/9780197525593.001.0001/oso-9780197525593-miscMatter-001 (Accessed 12 of October 2021).

UNCTAD & WTO. (2012). A Practical Guide to Trade Policy Analysis, Geneva: United Nations.

van Bergeijk, P. A. G. (1995). The impact of economic sanctions in the 1990s, *World Economy*, vol. 18, no. 3, pp. 443-455.

van Bergeijk, P. A. G. (1998). Success and Failure of Economic Sanctions, Kyklos, vol. 42, no. 3, pp. 385-404.

van Bergeijk, P. A. G. (2009). Economic Diplomacy and the geography of international trade, [e-book] Cheltenham: Edward Elgar Publishing, Available at: Google Scholar: https://scholar.google.com/scholar_lookup?title=Economic%20Diplomacy%20and%20the%20Geography%20of%20International%20Trade&publication_year=2009&author=P.%20A.%20van%20van%20Bergeijk [Accessed 12 of October 2021].

van Bergeijk, P. A. G., & van Marrejik, C. (1995). Why do sanctions need time to work? Adjustment, learning and anticipation, *Economic Modelling*, vol. 12, no. 2, pp. 75-86

Wintour, P. (2017). Gulf plunged into diplomatic crisis as countries cut ties with Qatar, The Guardian, 5 June, Available online: https://www.theguardian.com/world/2017/jun/05/saudi-arabia-and-bahrain-break-diplomatic-ties-with-qatar-over-terrorism [Accessed 11 October 2021].

World Bank. (2021a). Middle East and North Africa: Overview, Available online: https://www.worldbank.org/en/region/mena [Accessed 3 of February 2021].

World Bank. (2021b). World Bank Development Indicators, Available online: https://databank.worldbank.org/source/world-development-indicators [Accessed 3 of February 2021].

Xu, Y. (2017). Generalized synthetic control method: Causal inference with interactive fixed effects models, *Political Analysis*, vol. 25, no. 1, 57-76.

Yousef, T., & Hassan, A. T., (2000). Intra-Arab Trade: Is It Too Little?, *IMF Working Papers*, no. 10, International Monetary Fund (IMF).

6 Appendix A

A. 1: The list of 13 demands given to Qatar by the Tripartite

1. Qatar must reduce diplomatic representation with Iran.

2. Qatar must immidately shut down the Turkish military base that is being established, and halt any military co-operation with Turkey in Qatar.

3. Qatar must announce severance of ties with terrorist, ideological and sectarian orgs, including the Muslim Brotherhood, Islamic State, Al Qaeda and Hizbollah; and designate them as terrorists.

4. Qatar must cease any funding activities to extremist and terrorist individuals, entities and organisations.

5. Qatar must hand over all designated terrorists, wanted by the four countries; freeze their assets; stop hosting others in the future.

6. Qatar must shut down Al Jazeera and all affiliated channels.

7. Qatar must stop interference in other countries' domestic and foreign affairs.

8. Qatar must provide reparations to these countries for any opportunity costs incurred over the past few years because of Qatari policies.

9. Qatar must get in sync with its Gulf and Arab neighbourhood on all levels.

10. Qatar must provide all databases related to oppositionists that it provided support to and clarify what help was provided.

11. Qatar must declare all media outlets backed by it directly or indirectly.

12. These demands must be agreed within 10 days, otherwise they would be invalidated.

13. The agreement will involve clear goals and a reporting mechanism – monthly reports in the first year, every three months and then annually for 10 years.

Source: Mohinsky (2017).

7 Appendix B

A. 2: The list of countries that are in the MENA region and donor pool.

Countries:	Iso3-code:
Algeria	DZA
Bahrain	BHR
Djibouti	DJI
Egypt	EGY
Iran	IRN
Israel	ISR
Jordan	JOR
Kuwait	KWT
Lebanon	LBN
Libya	LBY
Morocco	MAR
Oman	OMN
Qatar	QAT
Saudi Arabia	SAU
Tunisia	TUN
Turkey	TUR
United Arab Emirates	ARE
Yemen	YEM

Note: The definition of the MENA region varies and include a different set of countries. This paper follows the definition provided by the World Bank and includes the countries specified above (World Bank, 2021). The only difference is that Iraq, Syria and West Bank and Gaza has been excluded due to the lack of available data and Turkey is included since it was involved in the embargo. Further, Israel is included since it is in the geographical vicinity of the MENA region although it is not officially apart of it.

8 Appendix C

Table 3: Bilateral trade pairs

Treated trade units	Donor pool trade units			
QAT-ARE	ARE-BHR	EGY-LBN	EGY-LBY	SAU-YEM
QAT-BHR	ARE-DZA	EGY-MAR	LBN-OMN	TUN-ARE
QAT-SAU	ARE-EGY	EGY-OMN	LBN-QAT	TUN-BHR
	ARE-IRN	EGY-SAU	LBN-SAU	TUN-DJI
	ARE-JOR	EGY-TUN	LBN-TUR	TUN-DZA
	ARE-KWT	EGY-TUR	LBN-YEM	TUN-EGY
	ARE-LBN	EGY-YEM	LBY-BHR	TUN-IRN
	ARE-MAR	IRN-ARE	LBY-DZA	TUN-JOR
	ARE-OMN	IRN-TUR	LBY-EGY	TUN-KWT
	ARE-SAU	ISR-EGY	LBY-JOR	TUN-LBN
	ARE-TUN	ISR-TUR	LBY-LBN	TUN-LBY
	ARE-TUR	JOR-ARE	LBY-MAR	TUN-MAR
	ARE-YEM	JOR-BHR	LBY-OMN	TUN-OMN
	BHR-ARE	JOR-DJI	LBY-SAU	TUN-QAT
	BHR-EGY	JOR-DZA	LBY-TUN	TUN-SAU
	BHR-JOR	JOR-EGY	LBY-TUR	TUN-TUR
	BHR-KWT	JOR-IRN	MAR-DZA	TUN-YEM
	BHR-LBN	JOR-KWT	MAR-LBY	TUR-ARE
	BHR-LBY	JOR-LBN	MAR-SAU	TUR-BHR
	BHR-MAR	JOR-LBY	MAR-TUN	TUR-DJI
	BHR-OMN	JOR-MAR	MAR-TUR	TUR-DZA
	BHR-SAU	JOR-OMN	OMN-ARE	TUR-EGY
	BHR-TUN	JOR-QAT	OMN-BHR	TUR-IRN
	BHR-TUR	JOR-SAU	OMN-DJI	TUR-ISR
	BHR-YEM	JOR-TUN	OMN-DZA	TUR-JOR
	DJI-ARE	JOR-TUR	OMN-EGY	TUR-KWT
	DJI-EGY	JOR-YEM	OMN-IRN	TUR-LBN
	DJI-JOR	KWT-ARE	OMN-JOR	TUR-LBY
	DJI-LBN	KWT-BHR	OMN-KWT	TUR-MAR
	DJI-OMN	KWT-DZA	OMN-LBN	TUR-OMN
	DJI-SAU	KWT-EGY	OMN-LBY	TUR-SAU
	DJI-TUN	KWT-IRN	OMN-MAR	TUR-TUN
	DJI-TUR	KWT-JOR	OMN-SAU	TUR-YEM
	DZA-ARE	KWT-LBN	OMN-TUN	YEM-ARE
	DZA-BHR	KWT-MAR	OMN-TUR	YEM-BHR
	DZA-EGY	KWT-OMN	OMN-YEM	YEM-EGY
	DZA-JOR	KWT-QAT	SAU-ARE	YEM-IRN
	DZA-KWT	KWT-SAU	SAU-BHR	YEM-JOR

DZA-LBN	KWT-TUN	SAU-DJI	YEM-KWT
DZA-LBY	KWT-TUR	SAU-DZA	YEM-LBN
DZA-MAR	KWT-YEM	SAU-EGY	YEM-LBY
DZA-OMN	LBN-ARE	SAU-IRN	YEM-MAR
DZA-QAT	LBN-BHR	SAU-JOR	YEM-OMN
DZA-SAU	LBN-DJI	SAU-KWT	YEM-SAU
DZA-TUN	LBN-DZA	SAU-LBN	YEM-TUN
DZA-TUR	LBN-EGY	SAU-LBY	YEM-TUR
EGY-ARE	LBN-IRN	SAU-MAR	QAT-DZA
EGY-BHR	LBN-JOR	SAU-OMN	QAT-JOR
EGY-DZA	LBN-KWT	SAU-TUN	QAT-KWT
EGY-JOR	LBN-LBY	SAU-TUR	QAT-LBN
			QAT-TUN
EGY-KWT	LBN-MAR	SAU-YEM	QAT-YEM

Note: Since the SCM is sensitive to missing values, the donor pool only consists of those trade pairs that have observations for the average two-way bilateral trade flows throughout the period under study (1995-2019). This leaves us with a total of 203 potential comparison units in the donor pool.

Table 2. Synthetic control group composition for the bilateral trade analysis

Qatar ↔ Bahrain		
Weight	Trade Unit	
0.286	Jordan	Bahrain
0.274	Lebanon	Jordan
0.148	Israel	Egypt
0.124	Bahrain	Oman
0.111	Egypt	Libya
0.046	Egypt	United Arab Emirates
0.007	United Arab Emirates	Iran
0.004	Libya	Lebanon

Qatar ↔ Saudi Arabia		
Weight	Trade Unit	
0.278	Israel	Egypt
0.22	Yemen	Saudi Arabia
0.138	Egypt	Algeria
0.093	Algeria	Tunisia
0.081	United Arab Emirates	Saudi Arabia
0.053	Oman	Saudi Arabia
0.05	Egypt	United Arab Emirates
0.046	United Arab Emirates	Iran
0.039	Algeria	Libya
0.002	Turkey	Iran

Qatar ↔ United Arab Emirates		
Weight	Trade Unit	
0.361	Egypt	Kuwait
0.308	United Arab Emirates	Oman
0.109	Egypt	Turkey
0.108	Iran	Turkey
0.058	Turkey	United Arab Emirates
0.05	United Arab Emirates	Iran
0.005	Bahrain	United Arab Emirates
0.001	Egypt	United Arab Emirates

Note: The table shows the composition of the synthetic control group with weights for each treated unit (average two-way bilateral trade between two countries as represented by the symbol ↔) as identified by the SCM in the bilateral trade analysis.