ENFORCING PEACE: MEASURING THE EFFECTS OF THE QIZ AGREEMENT ON THE MARGINS OF EGYPTIAN-ISRAELI TRADE

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ENFORCING PEACE: MEASURING THE EFFECTS OF THE QIZ AGREEMENT ON THE MARGINS OF EGYPTIAN-ISRAELI TRADE

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Abstract

The United States implemented the Qualifying Industrial Zones (QIZ) agreement in 2004 to promote economic normalization between Egypt and Israel. The agreement granted Egypt non-reciprocal duty-free access to US markets for goods that contain 11.7 percent Israeli-manufactured components. Previous literature has found that Egyptian-US trade volumes increased post-QIZ, but effects on Egyptian-Israeli trade have remained unexplored. This paper utilizes Egyptian and Israeli trade data to evaluate the spillover effects of non-tariff trade barrier elimination from the QIZ agreement on marginal trade growth. Using a commodity-level difference-in-differences approach, I find negative effects of the agreement on Egyptian exports to Israel, driven primarily by the extensive margin. Using an aggregate-level synthetic control, I find no effects on Egyptian or Israeli bilateral export volumes after employing log specification and ridge bias correction. Interpretation of the commodity-level results is limited due to violations of parallel trends and control group selection issues. Underlying data issues and pretreatment length may limit aggregate interpretations, though they are markedly more confident. Nonetheless, this paper provides potential novel contributions to the value of bias-corrected synthetic controls in trade agreement literature, the potential social and political impediments to non-tariff trade barrier elimination, and an evaluation of the efficacy of US normalization policy in the Middle East.

KEYWORDS: normalization, trade agreement, trade margins, Qualifying Industrial Zones (QIZ), regional trade integration
JEL CODES: C54, F13, F51

ON MY HONOR, I HAVE NEITHER GIVEN NOR RECEIVED UNAUTHORIZED AID ON THIS THESIS

Signature

Table of Contents

List of Tables

List of Figures

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1. Introduction

The question of how to effectively promote the normalization of Israel has influenced US policy in the Middle East for decades.[1] To achieve this goal, US economic policy in Egypt has sought to promote what Jadallah (2016) describes as a "US-dominated, Israel-normalizing system," where economic development and normalization coincide. However, the "taboo" of transparently dealing with Israeli businesses in Egypt required the US to take a more subtle approach toward economic normalization (Yadav, 2007). This manifested primarily in the American promotion of export-oriented and globally competitive sectors, uplifting business elites amenable to normalization efforts (Jadallah, 2016). Under this economic normalization, the Egyptian textile, clothing, leather, and footwear (TCLF) sector experienced substantial growth in the 1990s, making up 25 percent of all manufacturing employment in Egypt by 2004 (Yadav, 2007).

However, the end of the World Trade Organization's (WTO) ten-year transition period on January 1, 2005, created a crisis of uncertainty for Egyptian producers and an economic normalization opportunity for the US. Had Egypt not signed an agreement with a major importer before 2005, TCLF producers would likely have been subject to duties as high as 35 percent, severely harming global competitiveness and threatening the industry's survival (Yadav, 2007). Despite strong international business connections, TCLF magnates experienced a series of failures to negotiate a trade agreement with a significant importer of TCLF goods, exacerbating an already dubious situation (Jadallah, 2016).

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[1] The term "normalization" refers to efforts to foster amicable relations between Israel and its neighbors, typically initiated by the US or other Western governments and achieved through various economic, political, and military means.

Amidst this uncertainty, the US began negotiating the Qualifying Industrial Zones (QIZ) agreement, ensuring the stability of the TCLF industry through the first overt display of economic normalization. Signed on December 14, 2004, the QIZ agreement created multiple export-processing zones where eligible products could be manufactured and assembled. For these commodities to qualify for non-reciprocal, duty-free access to the US, their total value-added must consist of 11.7 percent Israeli-manufactured components.[2] Though this agreement closely mirrors other Preferential Trade Agreements (PTAs), it notably diverges with the inclusion of the Israeli value-added criterion, requiring Egypt to utilize Israeli components for a commodity to qualify for duty-free entry into the US.[3]

The QIZ agreement has two goals: (1) promote Egyptian exports to the US for its globally competitive industries and (2) foster economic normalization. Existing literature has primarily focused on assessing whether the US has achieved the first goal, which has mostly concluded that it has increased trade volumes (Refaat, 2006; Hutcheson, 2006; Salinger, 2008; Nugent & Abdel-Latif, 2010; Carter et al., 2015). However, the efficacy of the second goal has remained relatively unaddressed outside of qualitative descriptions focusing on political theory and descriptive statistics rather than rigorous empirical evaluations (Yadav, 2007; Jadallah, 2016; Yehudith & Tamar, 2020).

Existing literature on the effects of PTAs on trade volume growth has revealed positive results, especially along the extensive margins, but has done so largely for

[2] This required Israeli-value-added criterion decreased to 10.5 percent in 2005. Egypt, Israel, and the US agreed decrease it further to 8.2 percent in 2023 but have not moved towards implementing these changes to date. See Seif-Eddin and Eddin-Essam (2023).
[3] Though the non-reciprocal nature of the QIZ agreement is also unique, implementing full PTA with the US would likely not significantly change Egyptian importing behavior given past evidence from Jordan. See Busse and Gröning (2012).

agreements that eliminated tariffs (Trefler 2004; Baier & Bergstrand, 2007; Kehoe & Ruhl, 2013; Dutt et al., 2014; Cho & Díaz, 2018; Amarsanaa & Kurokawa, 2021, Cho et al., 2022; Ahmed et al., 2023). However, the Israeli value-added criterion aims to encourage economic normalization solely by promoting bilateral business relations and eliminating non-tariff trade barriers, with tariffs remaining relatively stagnant since the signing of the agreement. For the QIZ agreement, the overwhelming success of PTAs in increasing trade volumes should not be taken as given.

Despite uncertainty about the agreement's effects on increasing Egyptian-Israeli economic relations, calls for a full Egypt-US Free Trade Agreement (FTA) have remained unanswered (Lawrence, 1998; Mabrouk, 2019). Further, recent reports have revealed the Israeli value-added criterion artificially limits Egyptian TCLF exports due to Israel's difficulties with intermediary TCLF good production, such as zippers and shoe soles (Abd Rabo, 2016; Seif Eddim & Eddim Assam, 2023). Additionally, it has increased Egyptian economic inefficiency, as Israeli intermediary goods are often costlier than those from more efficient producers (Nugent & Abdel-Latif, 2010). Therefore, assessing the impact of the QIZ agreement on economic normalization is paramount to ensuring that the continued American belief in the agreement reflects the economic reality of the situation rather than the ideological underpinnings of US foreign policy.

In this paper, I provide a novel analysis of the spillover effects of the QIZ agreement on the growth in margins of trade volumes between Egypt and Israel for commodities not traded under the agreement. Specifically, I determine whether the Israeli value-added criterion, which reduces non-tariff trade barriers (NTTBs) by encouraging Egyptian-Israeli trade of QIZ commodities, led to economic normalization at the commodity and aggregate levels. I hypothesize that overall marginal growth in trade volumes between Egypt and Israel will be more modest than previous analyses of PTAs due to the non-elimination of tariffs. However, I predict that the QIZ's goal of fostering economic normalization has succeeded due to NTTB elimination.

Utilizing Egyptian and Israeli export data, I construct country-pair proportion-driven commodity groups to evaluate the effects of the QIZ agreement on the extensive and intensive margins. I then analyze commodity-level changes by employing a difference-in-differences analysis, which is broadly supported in the literature but also generally sensitive to violations of parallel trends. To address these potential issues, I analyze aggregate-level effects by employing synthetic control methodology, a highly underutilized tool in international trade that substantially relaxes the pretreatment trends assumption and better accounts for time-varying unobservable confounders.

At the commodity level, this study finds that the QIZ agreement negatively impacted Egyptian exports to Israel for all margins and Israeli exports to Egypt for the intensive margin. However, I find no effect on Israeli exports for extensive and combined goods. Difference-in-differences analyses with time-varying implementation dummy variables find significant results 6 years after QIZ implementation, confirming previous conclusions on the phase-in effects of trade agreements. Further analysis employing the Poisson Pseudo-Maximum Likelihood estimator reveals the robustness of Egyptian exports to alternative specifications. However, this analysis is limited due to violations of the parallel trends assumption and coefficient bias indicated by country-specific time trends analysis. Future studies should employ propensity score matching to address poor pretreatment fit and contribute broader commodity-level conclusions to the literature.

At the aggregate level, this study finds that the QIZ agreement did not affect Egyptian or Israeli exports for all marginal commodity groups. I employ synthetic control methodology with log specification of the outcome variable and ridge bias correction to address convex hull considerations and potential issues with small donor pools. However, placebo permutations ranked treated units low across signatory and commodity groups compared to potential donor units who were arbitrarily assigned treatment. Though this estimation may have underlying issues, such as a low number of potential donors, insufficient pretreatment years, and a loss of detail from data aggregation, the specification and bias correction process indicates the results are likely significant.

Given the conclusion that the QIZ agreement had no aggregate spillover effects on Egyptian-Israeli trade, this paper provides a few novel contributions to the literature. Since synthetic control methodology better accounts for time-varying unobservable confounders and constructs synthetic counterfactuals using export determinants, it is highly valuable for accounting for unobserved heterogeneity in difficult-to-approximate treated units. Though the downsides of data reaggregation must be carefully considered, this paper advocates for the broader use of synthetic controls in trade agreement literature, particularly when confronted with concerns about poor pretreatment fit.

Further, this paper advocates for additional research into the political and social impediments to NTTB elimination. Existing research on NTTB elimination operates under the assumption of absolute amicability between trade partners. However, the absence of a causal relationship between the QIZ agreement and spillover effects on Egyptian-Israeli trade of non-QIZ goods indicates the need for further analysis of trade creation in politically and socially contentious environments.

Finally, this paper contributes valuable recommendations for US economic policy towards Egypt. The absence of a causal relationship between the QIZ agreement and aggregate Egyptian-Israeli export volumes highlights the ineffectiveness of current US policy on Egypt to promote economic normalization. If the effects of the QIZ agreement found in this paper are valid, the US must restructure its regional economic policy to avoid imposing adverse costs and limitations on key strategic partners and promote its regional interests using methods founded on maximizing economic growth.

The following section reviews existing assessments of the efficacy of the QIZ agreement, the evolutions of methods for evaluating trade growth, and the advantages of difference-in-differences and synthetic control methodology. Section 3 outlines the methodological approach, including data and variables used and specific considerations for calculating the trade margins. Section 4 considers trade diversion theory. Section 5 provides benchmark results and robustness checks. Finally, Section 6 discusses limitations and my contributions to existing literature and broader policy evaluations.

2. Literature Review

2.1 Egyptian QIZ Implementation

Existing literature on the effects of Egyptian QIZ implementation falls into two categories: (1) a majority evaluating the growth of Egyptian exports to the US and (2) a minority evaluating the agreement's effects on Egyptian-Israeli relations broadly.

Most literature has concluded that the QIZ agreement has increased Egyptian exports to the US with a few key caveats. Immediately after the agreement's implementation, the effects of the QIZ agreement on exports were not noticeable, as the seasonal nature of the TCLF industry led to orders being placed far in advance (Refaat, 2006; Hutchison, 2006). Further, the Israeli domestic manufacturing industry was unprepared for a sudden increase in the demand for TCLF intermediary goods (Salinger, 2008). Though there were concerns about what appeared to be lagging ex-post growth in Egyptian exports to the US (Nugent & Abdel-Latif, 2010), the trade volumes from the QIZ agreement increased to the point that they began mirroring those of broader US FTAs in the Middle East by the mid-2010s (Carter et al., 2015).

However, others have argued that the basis for comparison should not be Egypt’s neighboring countries but those specializing in exporting TCLF goods. In 2016, Egyptian TCLF exports totaled just under 1 billion USD, whereas those of countries such as Vietnam and Bangladesh exceeded 10 billion USD (Abd Rabo, 2016). Experts attribute this discrepancy to the Israeli value-added criterion for two key reasons. First, it creates an artificial limit on Egyptian exports due to the limited growth potential and relative weakness of Israeli TCLF intermediary manufacturers compared to countries like Japan and China (Nugent & Abdel-Latif, 2010). Second, it increases economic inefficiency by forcing Egyptian producers to source more expensive TCLF intermediary goods from Israel rather than cheaper alternatives elsewhere (Abd Rabo, 2016; Seif Eddim & Eddim Assam, 2023). Though the QIZ agreement has led to a well-studied increase in exports to the US, this paper answers the critical question of whether the Israeli value-added criterion has increased economic normalization, therefore meriting Egypt’s discrepancy and limitations compared to other TCLF exporters.

Existing literature on the normalizing effects of the QIZ agreement has drawn mixed conclusions. Nevertheless, it still offers crucial insights into the underlying political processes affecting these relations. In particular, it highlights this agreement's controversial place within broader societal discourse across both nations. Egyptian society has continuously disapproved of the QIZ agreement, believing it represents unnecessary cooperation with Israel for minimal economic benefits (Yadav, 2007; Abd Rabo, 2016). Egyptian business elites saw the agreement primarily as a quick fix to prevent job shedding due to the end of the WTO transitionary period but also as a method of overcoming the “taboo” of conducting business with Israeli firms, a venture they viewed as highly profitable (Yadav, 2007; Jadallah, 2016). Alternatively, Israeli business elites viewed the QIZ agreement as a wholly political endeavor meant to further normalization but with limited potential to aid Israeli economic development (Yehudith & Tamar, 2020). Conclusions about the agreement’s effect on Egyptian-Israeli economic relations have been extremely limited, with only one article offering somewhat negative beliefs about the agreement’s potential for increased cooperation due to bilateral demographic and economic asymmetries (Yehudith & Tamar, 2020).

However, notably absent from the literature is a robust evaluation of the impacts of the QIZ agreement on Egyptian-Israeli trade volumes. Some encountered limitations with measuring Egyptian-Israeli trade as they were unable to isolate non-QIZ commodities (Abdel-Latif & Nugent, 2015), whereas others utilized descriptive methodologies that did not address unobservable confounders and other endogeneity concerns (Jadallah, 2016; Yehudith & Tamar, 2020).

Relative to existing QIZ implementation literature, I produce three main contributions. First, I advocate for a shift in the focus of the literature to assess the spillover effects of the QIZ agreement on Egyptian-Israeli trade relations as a measure of economic normalization. By focusing on the normalizing potential of the QIZ agreement, I provide empirical backing to qualitative observations, address concerns about the economic limitations caused by the Israeli value-added criterion, and evaluate the soundness of the US' continued support for this agreement. Second, I address endogeneity concerns in existing descriptive analyses. By utilizing robust, empirical methodologies, I account for various endogeneity concerns central to measurements of post-PTA trade growth and the unique considerations for Egyptian-Israeli trade. Third, I utilize granular data to isolate ex-post effects on non-QIZ commodities and provide a clear picture of the agreement's impacts. The Egyptian Ministry of Trade has since published data that shows which commodities are traded as part of the QIZ agreement. Utilizing this granular data decreases the endogeneity threats of including QIZ commodities and allows for a more accurate analysis of the agreement's impacts on economic normalization.

2.2 Post-PTA Trade Growth

The proliferation of different forms of PTAs has necessitated the development of methodologies geared towards measuring their impacts. Though gravity models were the first method for analyzing the ex-post effects on bilateral trade flows, using a dummy variable to measure the effects of PTAs has frequently led to contradictory results (Baier & Bergstrand, 2007). Further, gravity models cannot account for a central endogeneity issue, as countries that already engage in high levels of bilateral trade likely choose to sign trade agreements with one another. Therefore, utilizing standard gravity models to measure post-PTA trade growth risks biased coefficients that overstate impacts (Trefler, 2004; Cho et al., 2022) and do not account for reverse-causality bias (Baier & Bergstrand, 2007; Billmeier & Nannicini, 2013).

This paper does not suffer from standard forms of reverse-causality bias, as Egypt's decision to sign the QIZ agreement was influenced primarily by the desire for continued access to US markets for the TCLF industry (Yadav, 2007). The QIZ agreement was also the first attempt by Egyptian business elites to overcome social stigmas against openly conducting business with Israel (Yadav, 2007; Jadallah, 2016). However, employing more robust causal methodologies ensures I avoid overstating the effects of the QIZ agreement on trade growth.

2.2.1 Non-Tariff Trade Barriers. Though PTAs often begin with tariff reduction, recent literature has revealed that they can increase export growth by eliminating NTTBs through changes in bilateral trade policies and improvements in business relations. Particularly when strong economic incentives are present, PTAs can spark substantial change as they offer a base framework that incentivizes increased cooperation and subsequent bilateral barrier reduction (Marković et al., 2021). Specifically, PTAs can encourage the reduction of barriers easily accomplished through increased bilateral cooperation, such as harmonizing business practices, reducing informational frictions, and encouraging improved regulatory coordination, though these changes cause weaker trade creation effects than traditional tariff elimination (Hou, 2023). The effects of the reduction of NTTBs are also prone to spillover onto non-signatories and commodities not included in the PTA, as the energy put into barrier reduction often easily extends past the initial sectors and partners (Hou, 2023).

The idiosyncrasies of the QIZ agreement present a unique adaptation of the implications of recent NTTB literature. An underlying logic of the success of the reduction of NTTBs is the propensity for PTA signatories to willingly collaborate or that there are no social impediments to the reduction in NTTBs (Marković et al., 2021; Hou, 2023). Reports indicate the successful initial collaboration between Egyptian and Israeli firms to develop supply chains and reduce informational frictions for QIZ goods (Salinger, 2008). However, the "taboo" nature of bilateral economic cooperation with Israel in Egyptian society offers a unique opportunity to analyze the social and political dimensions of the spillover of NTTB elimination onto non-QIZ commodities.

Considering the limited Egyptian-Israeli trade mandated by the QIZ agreement combined with the spillover propensity for NTTBs, recent evolution in the literature provides the foundation for my method for analyzing the QIZ agreement. If the QIZ agreement promoted economic normalization where Egypt overcame social limitations of openly conducting business with Israel, then the spillover effects of NTTB elimination would cause a detectible increase in the Egyptian-Israeli trade volumes of non-QIZ goods. However, if the QIZ agreement did not successfully promote economic normalization, then there would be no impact on Egyptian-Israeli trade volumes of non-QIZ goods. This paper builds upon recent advances in NTTB elimination literature by testing the spillover effects of an agreement that saw the creation of trade highly limited by sector to provide conclusions on its broader normalizing potential.

2.2.2 Marginal Increases in Trade. In 2013, Kehoe and Ruhl introduced an improved empirical application for the theory of extensive and intensive margins of trade, which has since revolutionized studies around the effects of PTAs on trade volume. The extensive margin focuses on the growth of goods traded in small volumes or not traded at all pre-PTA implementation. This allows for the measurement of trade growth through the diversification of existing firms' commodity trade, or the creation of new firms incentivized by the profit potential of a PTA. The intensive margin focuses on the growth of goods already heavily traded pre-PTA implementation, allowing for measuring trade growth via the increase in existing trade. By separating post-PTA increases in trade volume into two margins, the marginal trade theory allows for a more nuanced understanding of the potential domestic reorganization of commodity production that occurs post-PTA implementation.

Previous studies utilize arbitrary cutoff values to determine the margin of a good, which created difficulty in cross-country comparisons due to differing trade volumes (Feenstra, 1994; Hummels & Klenow, 2005; Broda & Weinstein, 2006). However, Kehoe and Ruhl (2013) employ a percentage-based system, where the extensive margin represents the bottom 10 percent of goods traded during the pretreatment period and those not traded at all. In contrast, the intensive margin represents the top 90 percent of trade. By employing this percentage methodology, Kehoe and Ruhl (2013) allow for greater cross-country applicability by minimizing the impacts of differing trade volumes on margin calculations.

Kehoe and Ruhl's (2013) margin methodology has been widely adopted across the literature. Most studies find positive post-PTA increases in trade volume across both margins, with more substantial growth along the extensive margin, demonstrating the importance of new commodities and, by proxy, new industries in trade creation (Dutt et al., 2014; Cheong et al., 2017; Cho & Díaz, 2018; Amarsanaa & Kurokawa, 2021, Cho et al., 2022). I build upon this trend in the literature by employing this methodological approach to provide increased nuance into the impact of certain trade margins and engage in a more robust cross-country analysis.

2.2.3 Difference-in-Differences. Pioneered by Card and Krueger (1994), the difference-in-differences (DiD) approach has become a preferred method for measuring the impacts of economic shocks or policy implementations. DiD measures the difference between the treatment effect on the treated group and the treatment effect on the control group. Due to the relative isolation of the effects of implementing various trade policies, or the assumption that trade policies only affect trade volumes after their implementation, DiD has become prevalent in measuring ex-post effects of trade volume growth (Volpe Martincus & Blyde, 2013), particularly when combined with an analysis of trade margins (Cheong et al., 2017; Cho et al., 2022). However, the DiD approach necessitates a strict assumption of parallel trends, that is, the difference between the treated and control group would be constant over time absent treatment, to determine causality.

The utilization of DiD for measuring the ex-post impacts of FTAs has received substantial criticism for its inability to arrive at widely generalizable results (French & Zylkin, 2024). However, I do not seek to generalize the impacts of QIZ agreements due to their unique structure and lack of utilization outside of Egypt and Jordan. Instead, I engage in novel analysis of the ex-post growth of Egyptian Israeli trade volumes to encourage further analysis of US normalization policy in the Middle East.

2.2.4 Synthetic Control Methodology. First proposed by Abadie and Gardeazabal (2003), Synthetic Control Methodology (SCM) has become increasingly popular within the field of comparative case studies, particularly for studying the impact of economic shocks or policy interventions. Compared to DiD, SCM is particularly beneficial in cases of uncertainty and bias concerns within a control group or when creating a less ad hoc control group is desired (Billmeier & Nannicini, 2013; Cho et al., 2022). To overcome these concerns, SCM constructs a synthetic counterfactual – a weighted combination of potential control units from a donor pool – using a combination of covariates and lagged values of the outcome variable during the pretreatment period (Abadie et al., 2010).

SCM is also advantageous when confronted with a violation of the parallel trends assumption while employing DiD (Billmeier & Nannicini, 2013; Cho et al., 2022). Instead of very similar pretreatment trends, SCM requires a strong pretreatment fit corresponding to specific covariates, measured by the root mean square prediction error (RMSPE), an easier condition to satisfy when confronted with high pretreatment variability. The RMSPE measures the gap between the values of the outcome variable for a unit and its synthetic counterfactual. Considering the unique geopolitical positions of Israel and Egypt, the substantial number of PTAs with neighboring countries, and lower levels of commodity diversity, I maximize the benefits of SCM in dealing with these economic concerns and issues surrounding the ad hoc selection of the control group in DiD.

Further, employing SCM more robustly accounts for time-varying unobservable confounders, whereas more standard approaches, such as DiD and fixed-effect models, only account for time-invariant unobservable confounders (Abadie et al., 2010; 2015). Though this is much less of a concern for research on ex-post effects in countries with high levels of stability, time-varying unobservable confounders pose a particular issue when studying Egyptian and Israeli trade. The economic impacts of destabilizing events, such as the deposition of Egyptian president Hosni Mubarak in 2011 and various wars involving Israel over the sample period, the effect of shifting Egyptian and Israeli political positions of business environments, and undocumented or unknown changes in corruption in post-Mubarak Egypt are time-varying unobservable confounders that pose issues for this paper. By employing SCM, I arrive at confident causal conclusions by accounting for these confounders.

However, this is not to say that SCM is without its issues, particularly in its application to testing ex-post effects of trade policies. A benefit of DiD is the ability to use highly disaggregated trade data because it accounts for unobservable industry or product-level attributes that may be correlated with the implementation of PTAs (Cheong et al., 2017; Cho et al., 2022). However, SCM cannot easily account for a third dimension[4] in testing the effects of PTAs due to its difficulty in evaluating broad policy impacts on many units, necessitating highly aggregate data. Additionally, SCM experiences difficulties when confronted with highly volatile outcome variables, as high levels of noise obscure post-treatment effects and create difficulties with pretreatment optimization of the synthetic counterfactual (Abadie & L'Hour, 2021). Therefore, natural logarithmic transformations are occasionally necessary to smooth the volatility of the outcome variables.

This paper builds upon previous literature by maximizing the differences in the advantages of DiD and SCM. By utilizing highly disaggregated data within my DiD, I capture the effects of unobservable commodity and industry-level attributes and engage in a more granular analysis of ex-post effects on trade volume. My employment of SCM, which reduces the dimensions of analysis to only time and country, still captures much of the effects of the margins by carrying over calculations from when the data was more

[4] The three dimensions are time, country, and commodity.

granular, while also addressing doubts surrounding control group difficulties, violation of the parallel trends assumption, and unobservable time-varying confounders.

3. Theory

Trade diversion theory presents crucial implications of economic inefficiencies that arise from FTAs, which adds another level of complexity to the analysis of post-QIZ growth in Egyptian-Israeli trade. As proposed by Viner (1950), the underlying principle of trade diversion theory is that it is a zero-sum game. In essence, FTAs incentivize signatories to trade more heavily with one another due to the elimination of tariffs, which leads to bilateral trade creation. However, trade diversion can have substantial economic detriments, as the cost of imports from less economically efficient signatories becomes lower than those from more economically efficient non-signatories.

The theoretical elements of trade diversion have been well-known throughout economics and were central to arguments favoring increased multilateral trade liberalization under the WTO. However, the continual failures of WTO trade negotiations until their eventual collapse in 2008 have essentially ended any chance for sweeping multilateral trade liberalizations. Despite these failures, Dai et al. (2013) was the first to assess the validity of trade diversion theory utilizing empirical means and a large sample group. They find that FTAs divert trade away from non-signatories but determine that the effects are more significant on the change in imports for signatories than that of exports (Dai et al., 2013).

Further, Nugent and Abdel-Latif (2010) conclude that the QIZ agreement created substantial trade diversion, though the effect on Egyptian-Israeli trade beyond the limited QIZ commodities remains unexplored. Since Egyptian manufacturers can only source intermediary goods from Israel and are markedly more expensive than those produced by countries with comparative advantages, the QIZ agreement diverts intermediary QIZ good trade away from more economically competitive countries to Israel. With the increased cost of intermediary goods, Nugent and Abdel-Latif (2010) argues the QIZ agreement's economic benefits must be substantial enough to outweigh the economic inefficiency of trade diversion for the continued success of the agreement.

Since Egypt gained duty-free access to the US for goods manufactured under the QIZ agreement, its exports may be diverted away from existing trade partners, such as Israel, because tariffs were not eliminated between those two countries under the QIZ agreement. I attempt to control the effects of trade diversion by isolating non-QIZ commodities, but that does not prevent domestic firms from switching production away from previously less-traded goods to those under the QIZ agreement due to increased demand from the US. Though the QIZ agreement led to the elimination of NTTBs between Egypt and Israel, a decrease or negligible change in Egyptian exports to Israel post-QIZ agreement would suggest this elimination was not enough to overcome the presence of trade diversion. The unsuccessfulness of economic normalization would, therefore, not outweigh the costs created by enforced trade diversion of QIZ intermediary goods.

4. Data & Methodology

In this section, I outline my data sources, method for generating the margins of trade following Kehoe and Ruhl (2013), and econometric strategy for utilizing DiD and SCM to measure the spillover effects of the QIZ agreement on the margins of Egyptian-Israeli trade.

4.1 Trade Data & Controls

I utilize disaggregated commodity-level data from the World Bank World Integrated Trade System (WITS), which employs data from the UN Comtrade Database. I work with four-digit level disaggregated data according to the 1996 Harmonized System (HS) nomenclature. The decision to use four-digit level disaggregation differs from the general norm of utilizing aggregate data in the field but does not go as far as using a six-digit level. Considering I measure the exports from two medium-sized countries, the trade volumes are less diverse compared to existing literature, which typically analyzes the growth in marginal exports from a major global exporter to a smaller country (Cheong et al., 2017; Cho et al., 2022). Therefore, I identify four-digit level disaggregation as detailed enough to provide more nuanced insight into export growth but general enough to account for the smaller trade volume within the experimental countries. Further, since the WITS database only reports data in current dollars, I adjust the trade flow to constant dollars utilizing each country's Commodity Terms of Trade Export Price Index, created by Gruss and Kebhaj (2019).[5]

This study covers the period 1998 to 2017, which includes a sizeable pretreatment period to adequately calculate the margins of trade. The substantial post-treatment period addresses the concerns of Baier and Bergstrand (2007) about delays in the effects of PTA implementation on export data. Further, the initial date of the research period adequately accounts for the transition from the original 1988/1990 HS classification system to the

[5] I chose this deflator due to general data availability. Most of the literature calculates real terms by utilizing each country's good's export deflator from the Organization for Economic Cooperation and Development (OECD) National Accounts database. However, Egypt is not a member of the OECD and therefore does not have a deflator in their database.

1996 edition, the latter of which allows for more granular analysis and ensures no missing values in the covariates.

Following Cho et al. (2022), the covariates I employ to approximate the pre-QIZ characteristics of Egypt and Israel are those typically used in gravity models as export determinants. The covariates include *Real GDP per Capita,* annual GDP per capita rates with USD with base year 2015 sourced from the World Bank World Development Indicators (WBWDI); *Population*, annual population metrics sourced from the Centre d'Etudes Prospectives et d'Informations Internationales (CEPII); *Real Effective Exchange Rate* (REER), annual weighted average of a country's currency based on a basket of other major currencies recently expanded upon by Couharde et al. (2018), sourced from CEPII's EQChange database; *Most Favored Nation (MFN) Tariff Rate*, an annual measure of the highest tariff rate that WTO members charge fellow WTO members, sourced from the WBWDI; and *Distance*, the distance between the capital of the QIZ signatory (either Egypt or Israel) and the treated country or control, sourced from CEPII. Tables 1 and 2 present descriptive statistics for the DiD and SCM models.

4.2 Generating the Margins

Utilizing the disaggregated WITS data, I follow Kehoe and Ruhl (2013) to compute the extensive and intensive margins of trade. They argue that growth along the extensive margins can be calculated by analyzing the growth of exports in the commodities they classify as "least traded" (LT). They combine two different categories to define LT goods: (1) commodities representing the lowest 10% of goods exported during the first three years of the pretreatment period and (2) non-traded goods, which are all possible commodities with no trade recorded during the first three years of the pretreatment period. They argue that combining these two categories allows for a more accurate analysis robust to cross-country differences in minimum trade values. Commonly set at 2000 USD, a minimum trade value is the smallest amount at which national trade statistics organizations measure trade activity (Kehoe & Ruhl, 2013). By incorporating these "non-traded goods" into the LT commodity group, I account for goods that do not initially but eventually exceed the arbitrary minimum trade value and better capture the effect of new commodity trade during the post-treatment period.

However, Kehoe and Ruhl's (2013) initial analysis, as well as subsequent applications by Cheong et al. (2017), Cho and Díaz (2018), Amarsanaa and Kurokawa (2021), and Cho et al. (2022), deal with the marginal growth in exports from significant exporters such as the US, EU, South Korea, and Japan, countries with substantially more extensive and diverse exports than Egypt or Israel, which allows for calculating sufficiently diverse margins solely using the first three years of the pretreatment period.

To overcome this issue, I expand the margin calculations to the entire pretreatment period. However, this may lead to extensive growth being underrepresented in the results, compared to existing literature, due to eliminating the buffer between margin calculations and treatment date.

Following my adaptation of Kehoe and Ruhl (2013), I calculate the average exports during the pretreatment period (1998-2004) for each QIZ signatory and their respective treatment and control groups. I then determine the bottom 10 percent of all four-digit level HS commodity trade and add all commodities not traded for each country-pair. The result is the group of LT goods, which is a proxy for the extensive

Table 1: Difference-in-Differences Descriptive Statistics

	Egypt	Israel
VARIABLE	Mean (Std. Dev.)	Mean (Std. Dev.)
Observations (%)		
Treated	8,000 (22.7%)	9,160 (20.6%)
Control	27,320 (77.3%)	35,360 (79.4%)
Real Exports		
Treated	38.12 (294.83)	131.71 (1,782.44)
Control	149.11 (1,856.82)	158.31 (2,479.80)
Log real GDP		
Treated	26.164 (0.213)	7.963 (0.133)
Control	26.800 (1.667)	8.067 (0.831)
Log real GDP per capita		
Treated	10.366 (0.104)	4,555.660 (3,973
Control	8.092 (1.304)	4,555.660 (3,973
Real effective exchange rate		
Treated	98.940 (7.028)	93.276 (16.748)
Control	103.668 (44.692)	96.965 (26.159)
MFN tariff rate		
Treated	7.041 (2.769)	19.371 (3.652)
Control	12.881 (7.646)	12.122 (6.564)

Note: All logged variables utilize natural logarithmic specification. Exports are reported in thousands of 2012 US dollars. GDP measurements are in 2015 US dollars. The real effective exchange rate (REER) is normalized to 100 in 2010.

Table 2: Synthetic Control Descriptive Statistics

	Egypt	Israel
VARIABLE	Mean (Std. Dev.)	Mean (Std. Dev.)
Observations (%)		
Treated	20 (9.1%)	20 (5.6%)
Control	200 (90.9%)	340 (94.4%)
Log Exports		
Treated	9.528 (0.492)	10.760 (0.747)
Control	9.820 (1.481)	10.113 (1.191)
Log real GDP per capita		
Treated	10.366 (0.106)	7.963 (0.136)
Control	8.132 (1.322)	8.020 (1.089)
Log population		
Treated	15.798 (0.114)	18.260 (0.121)
Control	18.166 (2.004)	17.021 (1.097)
Log distance		
Treated	6.003 (0.000)	6.003 (0.000)
Control	8.538 (0.439)	8.860 (0.557)
Real effective exchange rate		
Treated	98.940 (7.210)	93.276 (17.182)
Control	100.829 (33.123)	97.074 (16.818)
MFN tariff rate		
Treated	7.041 (2.841)	19.371 (3.746)
Control	13.860 (8.152)	9.948 (5.018)

Note: All logged variables utilize natural logarithmic specification. Exports are reported in thousands of 2012 US dollars. GDP measurements are in 2015 US dollars. The real effective exchange rate (REER) is normalized to 100 in 2010.

margin of trade. I then classify the highest 90% of goods traded during the pretreatment period as non-LT goods, which is a proxy for the intensive margin of trade.

I then split four-digit level commodity codes between the 10 and 90 percent groups to ensure precise LT and non-LT groups. It is important to note that since this calculation refers to the unique export pattern of each country, they are country-pair specific. In other words, each pair (e.g., the QIZ signatory and treatment, control, or potential donor country) has different commodity makeups for the LT and non-LT groups. This method is essential for LT and non-LT groups to reflect their respective trade margins and maintain the assumption of parallel trends. To determine aggregate[6] effects for each QIZ signatory, I run all regressions for the group "all goods," which utilizes all commodities exported by a given signatory rather than splitting them into marginal groups.

Note that I did not utilize all possible commodities in the sample. First, I exclude the commodities under the classification HS 2 digit-Codes 01-24 and 50-65 according to data from the Egyptian Ministry of Trade, which reports that the two commodity categories exported under the QIZ agreement are TCLF goods and prepared food.[7] This step isolates the spillover effect of the agreement on non-QIZ commodities, a central concern of Nugent and Abdel-Latif (2010) and Carter et al. (2015), and allows for the adaptation of existing lessons from NTTB elimination literature (Marković et al., 2021;

[6] There are two types of aggregation in this paper. This instance refers to when the commodities are not split up by margins. However, the other one refers to whether the commodities are calculated at the 4-digit HS specification level, or just as total trade volumes by year or signatory.

[7] Recently, the Egyptian Ministry of Trade pulled their data on QIZ exports by sector from their website. However, Jadallah (2016) cites the ministry's tables with the specific distribution of QIZ exports, and I was able to access them using the Wayback Machine at https://web.archive.org/web/20200710013722/http://www.qizegypt.gov.eg/Page/ImportandExport

Hou, 2023). Further, I exclude commodities under HS 2-digit code 27, which are fuels and energy sources, as they were one of the foundational considerations of the Egyptian-Israeli peace agreement, therefore making up a large percentage of bilateral trade that would dominate the intensive margins (Rubinovitz & Rettig, 2018).

Since this paper also deals with industry-level effects, I pair each four-digit level HS commodity code with its corresponding International Standard Industrial Classification of All Economic Activities (ISIC) code, a classification of commodities based on production methods rather than material contents. However, since not all four-digit level HS codes possess a corresponding ISIC classification, the pairing process excludes some goods. Tables A1 and A2 display the commodity breakdown between LT and non-LT goods for the DiD and SCM models. By excluding these commodity categories, I capture results related solely to the spillover effect on QIZ commodities, decrease the impact of regional petropolitics, and control for differing industry-level effects.

4.3 Difference-in-Differences

Typical approaches for a DiD analysis of ex-post trade volumes select treatment countries that export a large and diverse number of commodities along with control countries that are geographically close or share prominent underlying characteristics to capture unobservable similarities (Cheong et al., 2017; Cho et al., 2022). However, numerous difficulties arose when selecting control groups due to Egypt and Israel's complex geopolitical and economic realities. Both countries pursued multiple extensive FTAs with their neighbors during the study period and including them would greatly bias the model. Further, Kehoe and Ruhl (2013) require that a country exports at least 10 percent of all possible commodities to the treatment and control countries during the pretreatment period for their methodology to accurately proxy the intensive and extensive margins of trade. Though not a typical difficulty encountered in the literature, the lower levels of Egyptian and Israeli export diversity combined with the politically contentious nature of choosing to trade with Israel means many potential control countries do not meet the Kehoe and Ruhl (2013) 10 percent rule.

To overcome these issues, I average Egyptian and Israeli non-QIZ exports over the pretreatment period (1998-2004) and select the four countries closest to the treated country that also satisfied the Kehoe and Ruhl (2013) 10 percent rule. The resulting treatment and control groups are as follows: for Egyptian exports, Israel is the treated country while India, Japan, Kenya, and South Africa are the control countries; and for Israeli exports, Egypt is the treated country while Chile, Kenya, Nigeria, and Peru are the control countries.

However, this control group is not without issues, as the relatively small number of control countries means that the ability to make more broad, causal conclusions is limited. Further, difficulty in identifying a suitable control group led to a highly ad hoc selection process, which I adjust for later by employing SCM, but should also be heavily considered when interpreting DiD results.

After determining the control countries, I employ the following three-way fixed effects specification:

$$\text{Exports}_{ict} = \beta_0 + \beta_1 QIZ_{ct} + \tau_t + \gamma_c + \lambda_s + \varepsilon_{ict} \quad (1)$$

where Exports_{ict} represents the real exports of the QIZ signatory for good i to country c in year t and is specified in levels, not logs, to preserve the zeros in the dataset[8]; β_0 represents the baseline real exports of commodity i to country c in 1998; QIZ_{ct} is an interactor dummy that takes the value of 1 if the QIZ agreement is in place for country c in year t, and is zero otherwise; τ_t represents year fixed-effects to control for time-based fluctuations; γ_c represents country-based fixed effects that address differences in country-specific levels of trade; λ_s represents industry-level fixed effects where s represents the corresponding four-digit level ISIC code for each good i; and ε_{ict} represents the error term.

However, Equation (1) does not account for the relative dissimilarity between the treated and control groups outside of exports from the QIZ signatory. To account for general economic differences between the treated and control countries, I employ the following three-way fixed effects specification with typical gravity model determinants of exports as controls:

$$\text{Exports}_{ict} = \beta_0 + \beta_1 \text{QIZ}_{ct} + \beta_2 \ln(\text{GDP})_{ct} + \beta_3 \ln(\text{GDP per capita})_{ct} + \beta_4 \text{ Exchange Rate}_{ct} + \beta_5 \text{Tariff Rate}_{ct} + \tau_t + \gamma_c + \lambda_s + \varepsilon_{ict} \quad (2)$$

where $\ln(\text{GDP})_{ct}$ is the natural log of GDP for country c in year t; $\ln(\text{GDP per capita})_{ct}$ is the natural log of GDP per capita for country c in year t; $\text{Exchange Rate}_{ct}$ is the REER for country c in year t; and Tariff Rate_{ct} is the most favored nation tariff rate for country c in year t.

8 I address alternative specifications for the model in 5.1.4 using the Poisson Pseudo-Maximum Likelihood model.

By including the covariates in Equation (2), I control for different economic motivations to export, which clarifies the effect of the QIZ agreement and addresses the inherent differences between the treatment and control countries. Since I am measuring the effects of the QIZ agreement on the margins of trade, I run Equation (2) six times to capture the effects of LT, Non-LT, and all exports for both QIZ signatories. β_1 is my parameter of interest, which captures the effect of the QIZ agreement on the marginal growth in trade. Since panel data observations are potentially correlated within certain groups according to the dimensions of the dataset, I report standard errors clustered at country, year, and two-digit level ISIC sector jointly.

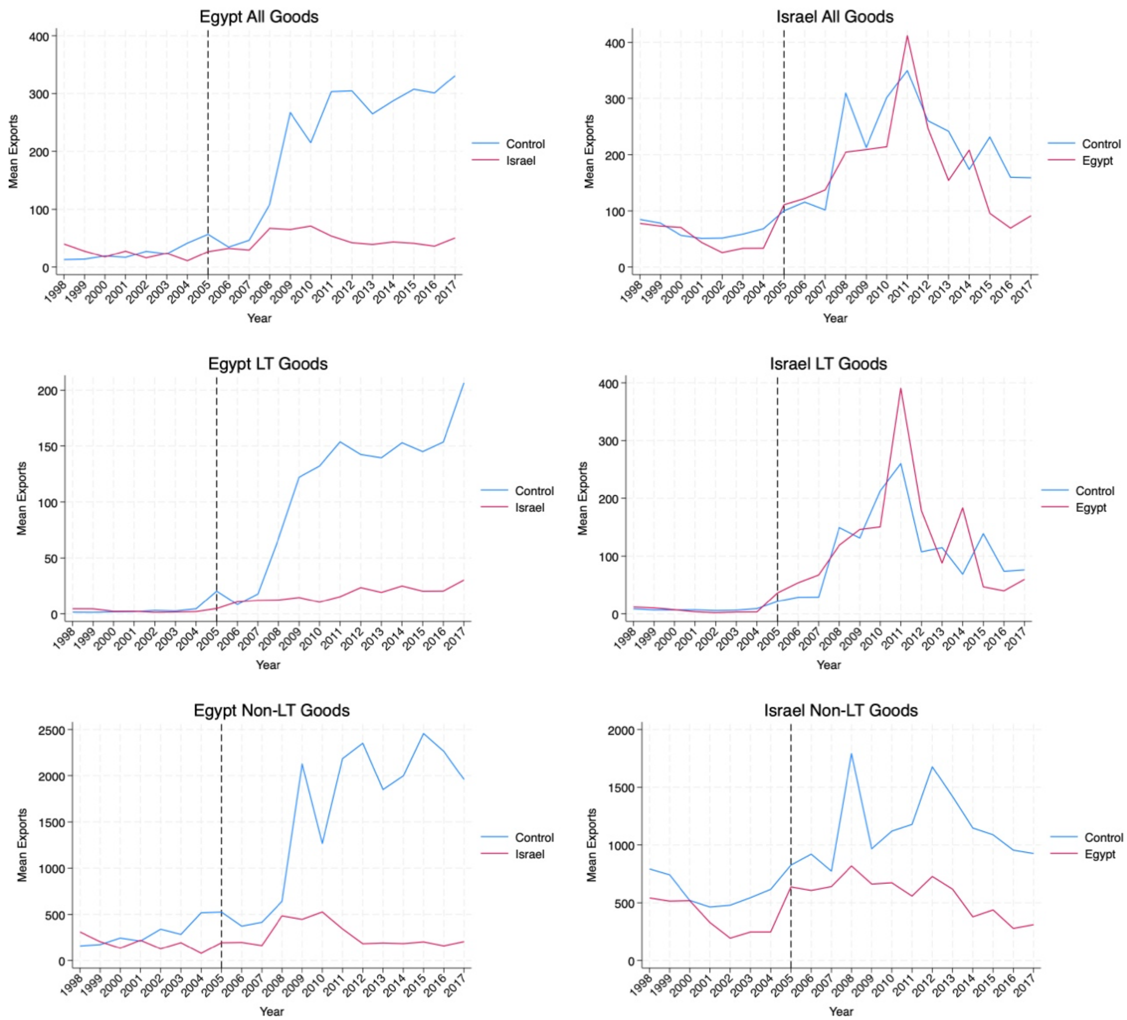
To test parallel trends, I provide a visual representation of the trends in mean exports to treated and control countries for both QIZ signatories in Figure 1. However, the ability of this figure to represent the impact of covariates and the three-dimensionality of the dataset is limited, so I include the p-value for the parallel trends test in my benchmark results in Table 3, where a higher value represents more parallel pretreatment trends. Both the graphical and statistical diagnostics for parallel trends should be considered. However, more weight should be given to the latter for its improved representation of the unique features of the dataset and my employment of covariates.

4.4 Synthetic Control Methodology

To determine a pool of potential donors, I first calculate Egypt and Israel's average non-QIZ commodity exports over the pretreatment period (1998-2004). I rank all global trade partners by value of average exports received. Then, I select the 40 countries that are closest in rank to the treated country during the pretreatment period. From this pool, I drop any countries that signed an FTA with the QIZ signatory during the entire

Figure 1: Observed Means for Egyptian Exports (a) and Israeli Exports (b).

(a) (b)



Note: The horizontal axis is measured in years, with the vertical marker representing the year the QIZ agreement took effect. The vertical axis represents mean exports, measured in thousands of real 2012 dollars. Since the treatment occurred in early 2005, pretreatment trends should be analyzed only from 1998-2004 or from the beginning of the figure to one year before the dashed line.

sample period (1998-2017). I then calculate the total number of commodities traded during the pretreatment period and dropped any that did not trade in at least 10 percent of all possible HS four-digit level non-QIZ commodities within the sample to comply with the Kehoe and Ruhl (2013) 10 percent rule. After eliminating the countries that did not meet these criteria, there are ten potential donor countries for Egyptian exports and seventeen for Israeli exports.[9]

To preserve detail, I conduct margin calculations for each country pair at the four-digit HS code level. To aggregate this granular data, I combine all values at the four-digit HS level to get total exports by year and country.[10] This converts the dataset from three dimensions to two and eliminates all zeros in the outcome variable. It also avoids issues SCM has with accounting for multiple treated units and discontinuous data, which reduces the risk of convex hull violations and overfitting concerns while preserving much of the detail from the disaggregated margins calculations.

However, the dimension reduction potentially introduces unit roots into the dataset, complicating SCM's forecasting capabilities by reducing the ability to differentiate temporary and permanent trend changes. To test for the presence of unit roots, I employ the Im-Pesaran-Shin (2003) test designed for heterogenous panels[11] with time-trend analysis to account for differences in trade pattern dynamics, such as idiosyncratic shocks, the systematic trends created by bilateral economic integration, and technological improvements in transportation costs. The results of these tests are detailed

[9] Note that the smaller donor pool for Egyptian exports is attributed to the large number of FTAs Egypt signed during the sample period and lower levels of export commodity diversity compared to Israel, which eliminated many potential donors.
[10] This is the other instance of reaggregation that I discuss in footnote 6.
[11] Panel signifies the country dimension, or the treated or potential donor countries of the dataset.

in Table A3 and reveal no unit root issues in the dataset, signifying the outcome variable does not require any transformations.

The outcome variable for the SCM is all, LT, and non-LT exports from each signatory to their respective treated and potential donor countries. To approximate the behavior of the QIZ signatories before signing, I utilize identical covariates to those I employ in Section 4.3, as they are export determinants typically employed in gravity models, with a few key differences. I employ a measure of distance, as I do not utilize any fixed effects that would make time-invariant covariates perfectly collinear. I also substitute real GDP with Population, following recommendations from Abadie and L'Hour (2021) to avoid including potentially highly collinear controls.[12] The covariates I employ include real GDP per capita, population, distance from the QIZ signatory, MFN tariff rate, and REER.[13] Following Abadie et al. (2010), I also include lag variables for exports one year and four years before the signing of the QIZ to improve the pretreatment fit of the SCM.

5. Benchmark Results

5.1 Difference-in-Differences

I first present the results of the DiD regressions, beginning with the specification outlined in Equation (2) for each QIZ signatory and margin of trade. Additionally, I present the time-varying effects to test the dynamic implementation of the QIZ, agreement, country-specific time trends to account for differences in the pretreatment

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[12] This decision relies on the inability to include GDP, GDP per capita, and Population as covariates due to perfect multicollinearity concerns. Abadie and L'Hour (2021) advise minimizing collinearity between controls, but not to go as far as to lose predictive power. Considering Population's slower rate of change compared to GDP and inverse proportionality, it is less collinear with GDP per capita than GDP, hence its inclusion in my employment of SCM.

[13] Note that I calculate GDP, GDP per capita, and distance as natural logarithms to address potential convex hull issues.

trends, and a Poisson Pseudo-Maximum Likelihood model to test for robustness to alternative specification.

5.1.1 Average Treatment Effects. The results of the DiD regression specified in Equation (2) are reported in Table 3. To determine the relative strength of each export group, I divide the coefficients by the simple average of the exports from the signatory during the pretreatment period.

For Egyptian exports, all coefficients are negative and significant, indicating that exports, on average, decreased by $167,337 per commodity after QIZ implementation compared to those in the control group. This decrease is primarily led by LT goods, whose average exports decreased by 577.6 percent per commodity compared to 452.7 percent per commodity for non-LT goods. The coefficients on all goods and LT goods are insignificant for Israeli exports, whereas the coefficient on Israeli non-LT goods is significant and indicates the QIZ agreement led to a decrease of $343,639 on average per commodity. However, the p-values on the parallel trends test for all goods and LT goods are concerningly low for Egypt and Israel. Despite their significance, β_1 for all goods and LT goods regressions are likely biased due to the differences between the treatment and control groups, which requires cautious interpretation.

5.1.2 Time-Varying Effects. The literature surrounding the implementation of PTAs has found that the effects of the agreements are often "phased-in," as production chains require time to adjust their behavior in response to changes in trade policy. Baier and Bergstrand (2007) argue that the economic effects of PTAs are often only fully felt after 10 years, with the effects gradually increasing. Similarly, Refaat (2006) and Hutcheson (2006) describe that the effects of the QIZ agreement on Egyptian exports to

Table 3: Average Treatment Effects of QIZ on Exports

	All Goods		LT Goods		Non-LT Goods	
VARIABLE	Egypt	Israel	Egypt	Israel	Egypt	Israel
QIZ	-174.761***	-0.188	-99.115***	33.198	-1,170.515***	-343.639***
	(21.604)	(34.867)	(10.523)	(36.549)	(193.442)	(120.884)
Log real GDP	792.935***	-45.554	433.902***	200.320	2,157.645	-2,582.890***
	(123.683)	(238.444)	(70.983)	(234.102)	(1,381.666)	(757.894)
Log real GDP per capita	-344.147	241.769	-311.621**	-40.349	7,089.444*	3,223.809***
	(256.662)	(152.455)	(125.724)	(130.338)	(3,915.810)	(788.884)
Real effective exchange rate	0.466**	-0.334	0.028	-0.314	7.308***	-0.605
	(0.199)	(0.314)	(0.122)	(0.311)	(2.651)	(1.213)
MFN tariff rate	-1.009	-3.283	-1.396	-4.521*	-34.046	9.502
	(4.386)	(2.620)	(2.144)	(2.533)	(68.080)	-10.465
R-Squared	0.037	0.053	0.026	0.048	0.389	0.149
Observations	34,640	44,140	31,920	39,660	2,800	4,580
Parallel trends test	0.1157	0.0686	0.1333	0.0211	0.4120	0.7211
Export growth (%)	-163.1	-0.1	-577.6	-127.7	-452.7	-74.8

Note: The null hypothesis for the parallel trends test is that the pretreatment lines for the treated and control groups are parallel. Therefore, the higher the p-value, the more parallel the pretreatment lines. Units are in thousands of 2012 US dollars. The last row shows the percentage increase in exports per product, calculated by dividing the estimated coefficients by the pre-FTA average exports to the treated group. All models include country, year, and industry-level fixed effects. Clustered SEs in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

the US were not immediately felt due to delays in the TCLF ordering process. However, the reports of the non-immediate effects of the QIZ agreement came from interviews with individual TCLF manufacturers rather than rigorous empirical analysis. The potential gradual implementation of the QIZ agreement merits an analysis of the time-varying effects of the implementation of the effects on Egyptian-Israeli trade.

To account for the potential for gradual implementation, I add lags to the regression specified in Equation (2). Following a modification of Cho et al. (2022), I incorporate dummy variables for each year up to the fifth year after the QIZ agreement came into effect, or until 2010, and then one that captures the effects of six or more years after the agreement, or 2011 and later. The reason for expanding the number of lags is twofold: (1) the substantial post-treatment period allows for a more detailed analysis of the impacts of the agreement, and (2) the overthrow of Egyptian President Mubarak in 2011, one of the most avid supporters of the QIZ agreement, has the potential to greatly impact the exporting behavior of both signatories, meriting closer analysis. The modified specification of Equation (3) to include the time-varying effects of the QIZ agreement is as follows:

$$\text{Exports}_{ict} = \beta_0 + \sum_{q=0}^{j} \beta_j \text{QIZ}_{c,t-j} + \beta_2 \ln(\text{GDP})_{ct} + \beta_3 \ln(\text{GDP per capita})_{ct} + \beta_4 \text{ Exchange Rate}_{ct} + \beta_5 \text{Tariff Rate}_{ct} + \tau_t + \gamma_c + \lambda_s + \varepsilon_{ict} \quad (3)$$

Table 4 presents the results of the specification in Equation (3). Following Refaat (2006) and Hutchison (2006), Egyptian exports do not experience growth in the initial years after the signing of the QIZ agreement. Further, the significant coefficients in Table

Table 4: Time-Varying Effects of QIZ Implementation

	All Goods		LT Goods		Non-LT Goods	
VARIABLE	Egypt	Israel	Egypt	Israel	Egypt	Israel
QIZ year	-8.178	41.642	-10.691	36.391	230.006	7.524
	(37.381)	(37.673)	(14.476)	(37.773)	(403.411)	(278.864)
1 year after	19.530	35.966	7.860	46.048	396.489	-124.600
	(38.098)	(42.181)	(12.474)	(28.870)	(413.975)	(247.324)
2 years after	0.817	56.053	-2.299	47.797*	248.928	67.467
	(35.177)	(35.417)	(14.892)	(26.911)	(449.868)	169.308)
3 years after	-36.814	-86.042	-52.684***	-20.946	63.719	-788.417*
	(39.767)	(83.920)	(15.713)	(78.371)	(421.066)	(412.626)
4 years after	-206.377**	17.889	-108.424***	27.620	-1,622.313**	-121.772
	(82.700)	(75.349)	(37.215)	(80.916)	(648.693)	(195.819)
5 years after	-142.241***	-61.664	-122.551***	-45.747	-525.868	-255.933
	(54.011)	(110.357)	(28.514)	(127.641)	(368.908)	(192.912)
6 or more years after	-276.501***	1.753	-146.309***	53.941	-2,011.586***	-488.755***
	(28.098)	(58.525)	(13.830)	(62.588)	(292.375)	(145.022)
R-Squared	0.037	0.053	0.027	0.048	0.378	0.151
Observations	34,640	44,140	31,920	39,660	2,800	4,580

Note: Controls following the specification in Equation (3) were included in all models but not in the table for conciseness. All models include country, year, and industry level fixed effects. Clustered SEs in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

3 all become significant at six or more years in Table 4, confirming findings in the literature that the effects of trade agreements are phased in. The negative coefficient contradicts Baier and Bergstrand (2007), but this may be due to the non-elimination of tariffs, instability created by the Arab Spring, or other Egypt-specific idiosyncrasies. However, these results, combined with those in Table 3, indicate that the benefits (insofar as they exist) arising from NTTB elimination were insufficient for increasing bilateral trade.

Additionally, the substantial changes in the magnitude of the coefficients over time are likely due to the volatility of post-treatment trade volumes. The post-treatment period aligns with a particularly volatile time for the global and regional economies, such as differing abilities to weather the 2008 financial crisis and instability caused by the Arab Spring. Therefore, the impacts of time-varying unobservable confounders are likely quite substantial and prevent a more robust analysis of the trade flows using DiD. However, I account for these particularly volatile country-specific idiosyncrasies in my employment of SCM.

5.1.3 Country-Specific Time Trends. A central limitation of this DiD study is the ad hoc selection of control units. I select control countries by computing the simple average of exports from the signatory to the treatment country and all potential control countries and selecting the four closest to the treatment country in value. However, computing simple averages does not account for potential trend differences between countries.

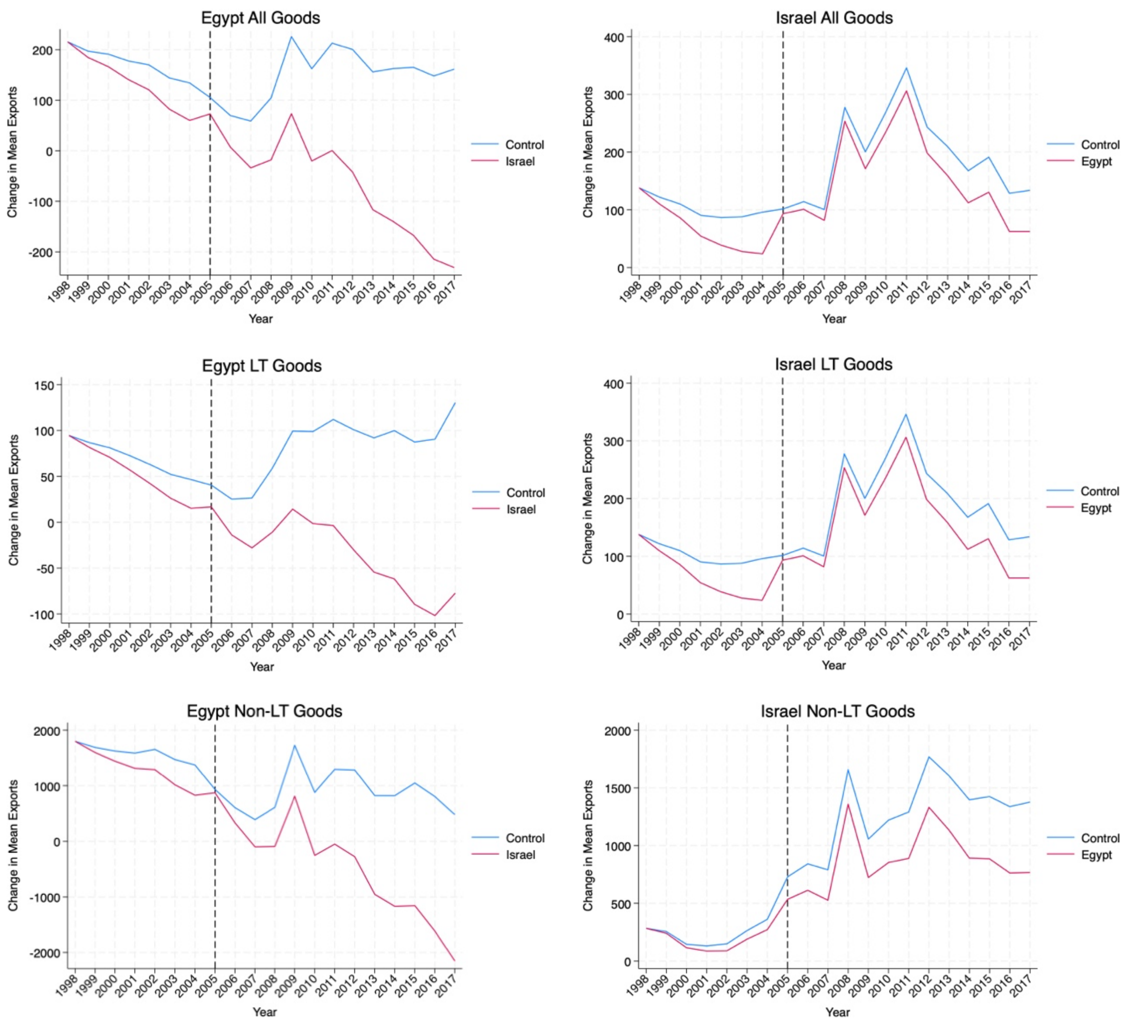
To further explore the potential divergence in pretreatment trends, I construct linear-trends figures for all, LT, and non-LT goods for each QIZ signatory, shown in Figure 2. For each group of goods and signatory, the trends for the change in average exports decrease faster for the treatment group than for the control group. Therefore, the differing pretreatment trends for the treated groups in Table 3 likely bias the coefficient downwards, obscuring the actual treatment effects of the QIZ agreement. The underlying economic logic for why both the treatment and control groups experience decreasing exports from the QIZ signatory is likely due to trade diversion. Since I select countries that did not sign any PTAs with the QIZ signatory during the entire treatment period, these countries are the most susceptible to trade diversion effects due to the extreme quantity of PTAs Egypt and Israel signed.

To account for differences in the pretreatment trends between treated and control groups, I employ country-specific time trends following Ryan et al. (2018) and Cho et al. (2022), which relaxes the parallel trends assumption by accounting for differences in the underlying temporal patterns between countries. In other words, instead of assuming that the treated and control groups would follow identical trends absent treatment, employing country-specific time trends allows country-specific factors to vary over time. Assuming each country follows different pretreatment trends, I employ the following specification where the differences would be captured by $\mu_c t$:

$$\text{Exports}_{ict} = \beta_0 + \beta_1 \text{QIZ}_{ct} + \beta_2 \ln(\text{GDP})_{ct} + \beta_3 \ln(\text{GDP per capita})_{ct} + \beta_4 \text{ Exchange Rate}_{ct} + \beta_5 \text{Tariff Rate}_{ct} + \tau_t + \mu_c t + \gamma_c + \lambda_s + \varepsilon_{ict} \quad (4)$$

Figure 2: Linear Trends for Egyptian Exports (a) and Israeli Exports (b).

(a) (b)



Note: The horizontal axis is measured in years, with the vertical marker representing the year the QIZ agreement took effect. The vertical axis represents the change in mean exports from the previous year for the treatment and control groups, measured in thousands of real 2012 dollars. Since the treatment occurred in early 2005, pretreatment trends should be analyzed only from 1998-2004 or from the beginning of the figure to one year before the dashed line.

$\mu_c t$ is defined utilizing the following specification from Ryan et al. (2018) to represent the results of when country c signs the QIZ agreement in year t:

$$(E[\text{Exports}_{c_1,t}|c_1,t] - E[Exports_{c_1,t-1}|c_1,t]) - (E[\text{Exports}_{c_0,t}|c_0,t] - E[Exports_{c_0,t-1}|c_0,t]) = \beta_1 + (\mu_{c_1} - \mu_{c_0}) \quad (5)$$

Equation (5) reduces to zero if time trends are even across countries, but since I observe that $\mu_{c_1} < \mu_{c_0}$ I infer that the β_1 from Equation (2) is potentially biased downward. I run the specification in Equation (4) for each export group and signatory, the results of which are in Table 5.

Interestingly, all the coefficients are significant, albeit at different levels, except for Israeli non-LT exports. Further, the coefficients have all become positive, confirming issues with differing underlying temporal patterns between countries during the pretreatment period. The results indicate that the QIZ agreement led to an increase of, on average, \$118,196 more per commodity for Egypt and \$109,258 more per commodity for Israel than what would have been expected given each country's pretreatment trends in trade volume.

Unfortunately, these coefficients only point out the bias in the original specification and do not have causal explanatory powers due to extrapolation bias. Since the model extrapolates the negative pretreatment trends into the post-treatment period, the model cannot account for post-treatment changes in the trends of the outcome variable, which is imperative given the potential economic effects of post-treatment political volatility for Egypt and Israel.

Table 5: Country-Specific Linear Time Trends

	All Goods		LT Goods		Non-LT Goods	
VARIABLE	Egypt	Israel	Egypt	Israel	Egypt	Israel
QIZ	118.196***	109.258**	61.747***	97.531**	642.690*	73.385
	(42.960)	(46.220)	(19.253)	(42.054)	(371.427)	(233.114)
R-Squared	0.037	0.054	0.027	0.048	0.394	0.150
Observations	34,640	44,140	31,920	39,660	2,800	4,580
Export growth (%)	110.3	63.3	359.9	375.0	248.6	16.0

Note: Controls following the specification in Equation (4) were included in all models but not in the table for conciseness. Units are in thousands of 2012 US dollars. The last row shows the percentage increase in exports per product, calculated by dividing the estimated coefficients by the pre-FTA average exports to the treated group. All models include country, year, and industry-level fixed effects. Clustered SEs in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

5.1.4 Robustness to Alternative Specifications. An underlying issue with analyzing trade volume growth at the granular commodity level is the large number of zeros in the dataset. I have analyzed trade volumes in levels instead of logs to address this limitation. Typical robustness checks for alternative specifications using zeros is to employ a log-linear specification using log + 1. However, Silva and Tenreyro (2006) show that data with many zeros, particularly disaggregate trade volume data, is highly susceptible to heteroskedasticity. Instead, a more robust alternative specification for high-zero datasets is estimating the model in multiplicative form using the Poisson Pseudo-Maximum Likelihood (PPML) estimator pioneered by Silva and Tenreyro (2006). By employing the PPML estimator, I estimate the coefficient in percentage terms, which directly tests my previous approximations, without incurring heteroskedasticity issues or requiring that the data follows a Poisson distribution. Therefore, I estimate the model in the following form:

$$\text{Exports}_{ict} = \exp[\beta_0 + \beta_1 \text{QIZ}_{c,t} + \beta_2 \ln(\text{GDP})_{ct} + \beta_3 \ln(\text{GDP per capita})_{ct} + \beta_4 \text{ Exchange Rate}_{ct} + \beta_5 \text{Tariff Rate}_{ct} + \tau_t + \gamma_c + \lambda_s] + \varepsilon_{ict} \quad (6)$$

Table 6 demonstrates the results from this estimation. The continued significance of almost all the coefficients from Table 3, except for Israeli non-LT exports, demonstrates the model is robust to alternative specifications. This echoes the findings of Cho et al. (2022), which is meaningful given my divergence from general trends in the literature. This paper focuses on the export behavior between countries with medium-sized economies and less diverse trade rather than following the general trend in the literature, which focuses on the behavior of large exporters to smaller countries.

Table 6: Poisson Pseudo-Maximum Likelihood Specification

	All Goods		LT Goods		Non-LT Goods	
VARIABLE	Egypt	Israel	Egypt	Israel	Egypt	Israel
QIZ	-1.382***	0.183	-1.886***	0.355	-0.914***	-0.105
	(0.249)	(0.219)	(0.240)	(0.421)	(0.243)	(0.158)
Observations	34,640	44,140	31,920	39,660	2,800	4,580
Export growth (%)	-74.87	20.08	-84.83	42.62	-59.91	-9.97

Note: Controls following the specification in Equation (6) were included in all models but not in the table for conciseness. Units are in thousands of 2012 US dollars. The last row depicts the PPML coefficients in percent terms, which is accomplished by utilizing the equation $(\exp(\beta_1) - 1) \times 100\%$. All models include country, year, and industry level fixed effects. Clustered SEs in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Therefore, the number of zeros is much greater than typically encountered, which makes the robustness of this model to alternative specifications especially important.

These results also support conclusions on the varying propensity for overestimation by margin in Cho et al. (2022). The differences in percent change estimations between Table 3 and Table 6 are much larger for LT goods than for non-LT goods. This directly strengthens the conclusions of Cho et al. (2022), who found that the magnitudes of the coefficients on LT goods are likely overestimated due to the significant presence of zeros in the dataset and are much less so for non-LT goods, which typically have fewer zeros. They also support the general conclusion that employing DiD models with datasets containing substantial numbers of zeros leads to overestimating the magnitudes of coefficients (Silva & Tenreyro, 2006; Cho et al., 2022).

Given the weakness of the parallel trends assumptions and the likely negative bias in coefficients, this paper does not arrive at confident conclusions about the effects of the QIZ agreement at the commodity level. However, I address the potential issues with violations of parallel trends by employing aggregate SCM analysis in the following section.

5.2 Synthetic Control Methodology

I begin by presenting the results of a typical SCM estimation and discussing the underlying issues given the data constraints and features. I then propose a solution by employing natural logarithmic specification on the outcome variable and ridge bias correction before detailing robustness checks.

5.2.1 Classic Method. Tables 7 and 8 provide the predictor means after running the synthetic process detailed in Section 4.4 for Egyptian and Israeli exports. Tables 9 and 10 provide the weights of potential donor countries for Egyptian and Israeli exports. Multiple issues arose during the process of generating the synthetic counterfactuals. There is some weakness in the synthetic matching of treated predictor means, which is evident in the substantial difference between the treated units and their synthetic counterparts, which introduces systematic bias into the model.

Further, there are potential issues with the predictor variable values of the treated units lying outside the convex hull. This is partly due to the lower levels of Egyptian and Israeli export diversity, which leads to substantial differences between the pretreatment trade volume for treated and potential donor countries. The proximity of Egypt and Israel creates additional convex hull issues, as the small values of the treated distance covariate combined with the large values for the potential donor countries ensure all possible weights of potential donors cannot adequately approximate the distance between the signatory and the treated country.

In addition to the weakness of fit for some of the predictor means, there are additional concerns with estimating the synthetic counterfactual. First, the small number of donor countries and pretreatment years, potential non-uniqueness of and subsequent correlation between donor countries, and convex hull issues make the generation of the synthetic counterfactual highly susceptible to overfitting. This issue is quite evident in the Egyptian LT goods in Table 9, which weights Canada at over 90% of the total synthetic, a classic example of overfitting caused by focusing too heavily on the pretreatment noise in the outcome variable, which severely limits the prediction capabilities of the SCM.

Table 7: SCM Predictor Means for Egyptian Exports

	All Goods		LT Goods		Non-LT Goods	
Variable	Israel	Synthetic	Israel	Synthetic	Israel	Synthetic
GDP per capita	10.251	9.070	10.251	10.259	10.251	8.849
Distance	6.003	8.801	6.003	9.034	6.003	8.733
Population	15.673	18.097	15.673	17.035	15.673	17.985
REER	101.682	101.737	101.682	75.620	101.682	101.527
MFN tariff rate	9.530	10.364	9.53	7.601	9.530	11.095
Lagged Exports						
QIZ – 1 year	4348.59	7632.22	704.62	872.60	3643.97	7231.93
QIZ – 4 years	11034.13	6361.12	821.61	665.96	10212.52	6060.71
RMSPE		6019.244		393.142		5412.53

Note: Distance, population, and GDP per capita variables are in natural logs. The real effective exchange rate (REER) is normalized to 100 in 2010. Lagged exports are in thousands of dollars. RMSPE is the root mean squared prediction error, which measures the overall pretreatment fit.

Table 8: SCM Predictor Means for Israeli Exports

	All Goods		LT Goods		Non-LT Goods	
Variable	Egypt	Synthetic	Egypt	Synthetic	Egypt	Synthetic
GDP per capita	7.805	6.269	7.805	6.771	7.805	6.239
Distance	6.003	8.120	6.003	8.187	6.003	8.110
Population	18.128	17.611	18.128	18.128	18.128	17.642
REER	96.381	86.758	96.381	88.563	96.381	87.267
MFN tariff rate	22.714	15.918	22.714	22.472	22.714	16.129
Lagged Exports						
QIZ – 1 year	15439.16	22196.01	1296.84	3076.62	14142.32	18666.32
QIZ – 4 years	20195.33	20202.11	1417.02	2012.34	18778.31	17836.39
RMSPE		8116.309		1432.412		7198.670

Note: Distance, population, and GDP per capita variables are in natural logs. The real effective exchange rate (REER) is normalized to 100 in 2010. Lagged exports are in thousands of dollars. RMSPE is the root mean squared prediction error, which measures the overall pretreatment fit.

Table 9: SCM Weights for Egyptian Exports

Country	All Goods	Extensive	Intensive
Canada	0.000	0.918	0.000
China	0.000	0.000	0.000
Djibouti	0.000	0.063	0.000
Ghana	0.000	0.000	0.000
India	0.000	0.000	0.000
Japan	0.569	0.000	0.495
Kenya	0.343	0.020	0.396
Nigeria	0.000	0.000	0.000
Pakistan	0.000	0.000	0.000
South Africa	0.088	0.000	0.108

Table 10: SCM Weights for Israeli Exports

Country	All Goods	Extensive	Intensive
Chile	0.000	0.000	0.000
Costa Rica	0.000	0.000	0.000
Cote d'Ivoire	0.000	0.000	0.000
Dominican Republic	0.000	0.000	0.000
Ecuador	0.000	0.000	0.000
Ethiopia	0.648	0.283	0.663
Guatemala	0.000	0.000	0.000
Indonesia	0.000	0.000	0.000
Kazakhstan	0.000	0.000	0.000
Kenya	0.006	0.267	0.000
New Zealand	0.000	0.000	0.000
Nigeria	0.000	0.450	0.008
Peru	0.000	0.000	0.000
Sri Lanka	0.346	0.000	0.329
Uganda	0.000	0.000	0.000
Vietnam	0.000	0.000	0.000
Zimbabwe	0.000	0.000	0.000

However, all the donor weights in Tables 9 and 10 present relatively unstable synthetic counterfactuals that rely heavily on a few potential donor pool countries, while the majority do not receive any weighting. Therefore, the bias these underlying issues create in the classic SCM circumvents causal conclusions. However, I detail my approach to overcoming these limitations in the next section.

5.2.2 Log Specification and Ridge Bias Correction. To account for the underlying issues in the classic SCM, I employ natural logarithmic specification on the outcome variable and ridge bias correction, proposed by Ben-Michael et al. (2021). By utilizing log specification, I account for the multiplicative nature of export volumes and improve the pretreatment fitting of the synthetic counterfactual by minimizing the impact of extreme values and outliers.[14] Log specification also addresses the issue with the values of the treated unit's outcome variable lying outside the convex hull by reducing the magnitude of the outcome variable, which in turn improves pretreatment fit and reduces systematic bias. Additionally, log specification allows for interpreting the impacts of the QIZ agreement as proportional changes, which provides a more holistic picture of the economic effects.

Following Ben-Michael et al. (2021), I employ ridge bias correction with the SCM, which adds a penalty term λ to the optimization process. This prevents the extreme weighting of donor units and allows weights to be shrunk close to zero but prevents any of them from being set to exactly zero.[15] This process reduces the risk of overfitting to noise, makes the SCM more robust to outliers in the donor pool, and results in a more

[14] I also test for unit roots with log specification, the results of which are in Table A3.
[15] Note that classic employments of Ridge bias correction require the manual selection of λ. However, the allsynth package in Stata automatically cross-validates to optimize the ridge parameter and balance bias-variance tradeoffs. See Wiltshire (2024) for additional information on automated cross-validation in Stata.

stable optimization that is less sensitive to idiosyncratic shocks in individual donor units and small changes in the outcome variable. Ridge bias correction also prevents the arbitrary overweighting of one donor when confronted with highly similar donor countries, a potential issue given the small sample size.

Additionally, ridge bias correction accounts for potential issues with the treated unit lying outside the convex hull by allowing for the negative weighting of potential donor units. In classic SCM, potential donor weights must be non-negative and sum to one, as evident in Tables 9 and 10. However, when a potentially treated unit lies outside the convex hull, no combination of the donor units can perfectly match it. Ridge bias correction addresses these issues by allowing for extrapolation in the optimization process, which may cause the weight of some potential donor units to go negative. This significantly reduces systematic bias that arises from imperfect pretreatment matching.

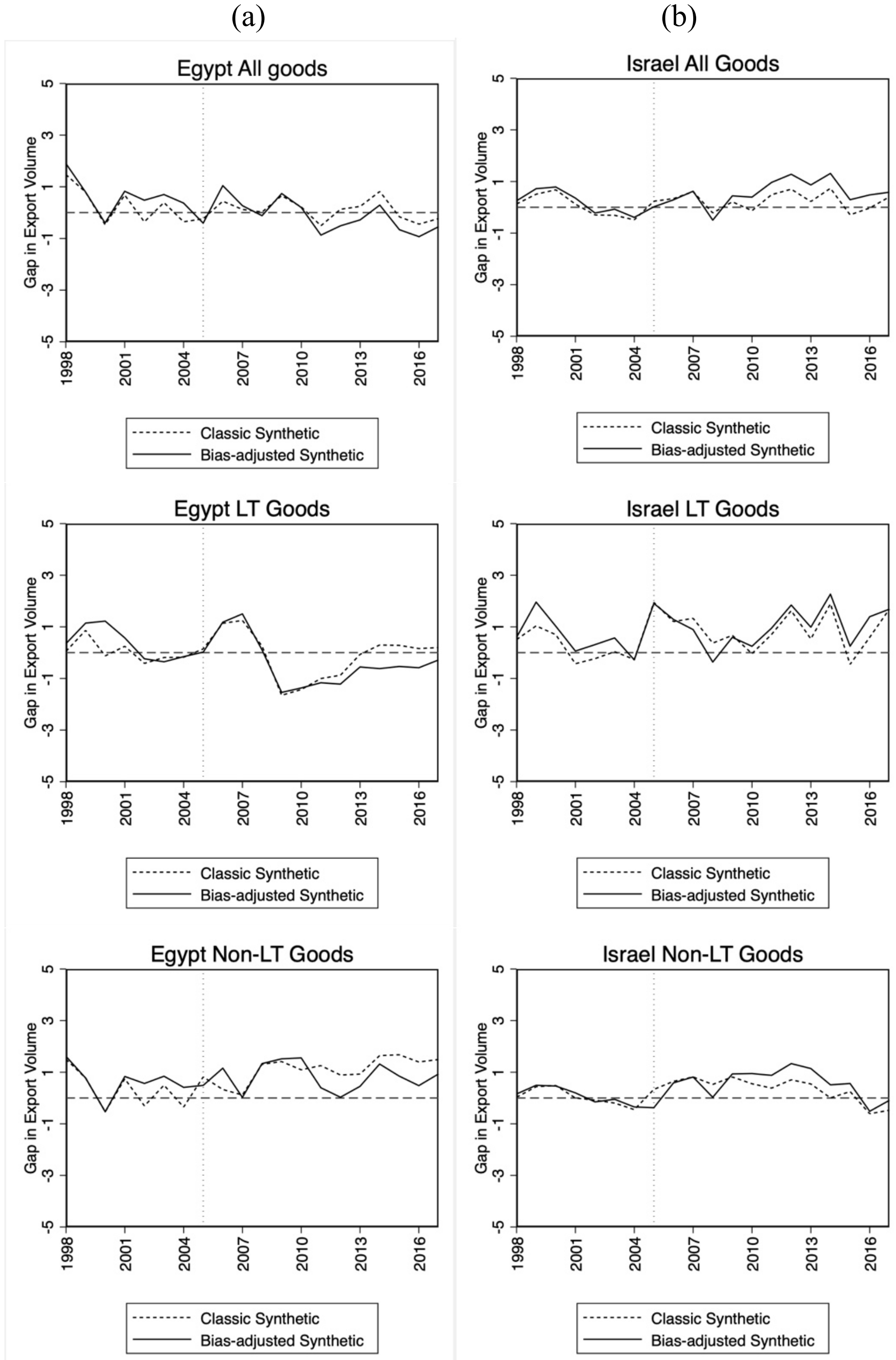
If left unchecked, the negative weighting process would overfit the pretreatment period and introduce substantial extrapolation bias into the model. However, the ridge bias correction pioneered by Ben-Michael et al. (2021) allows for a more tempered utilization of negative weights by balancing improvements in pretreatment fit with the risk of overfitting and overextrapolation.

However, there are additional considerations with the ridge bias correction. By attempting to increase the fit of the synthetic counterfactual to all pretreatment variables, ridge bias correction relaxes the focus on fitting pretreatment values of the outcome variable. As a result, ridge bias correction potentially increases the RMSPE, which would increase the variance of the regression. However, significant diversions between bias-adjusted and non-bias-adjusted synthetic counterfactuals typically signify that the latter is overfitted to the pretreatment period, which suppresses actual treatment effects. Though ridge bias correction is cautious in its utilization of negative weights, there is still the risk of overextrapolation and creating a synthetic counterfactual that does not reflect reality. The negative weighting process also sacrifices the simple proportional interpretation of the classic SCM. To address these potential issues, I conduct two versions of the following test: the first employs a classic SCM with only the natural logarithmic specification of the outcome variable, whereas the second incorporates ridge bias correction.

Given the uniqueness of SCM, the interpretation of the effects differs significantly from that of more common estimation methods. Since there are no traditional coefficients to quantify the effect, I utilize the gap method created by Abadie et al. (2010), which involves calculating the difference between the outcome variable between the treated unit and its synthetic counterpart for each post-treatment year or, in other words, the "gap" between observed and synthetic estimations. This process essentially creates a pseudo-coefficient to model the effects of the QIZ agreement on the margins of trade volumes. I present the results of these gap estimations graphically in Figure 3 for both signatories and empirically in Tables 11 and 12 for Egypt and Israel, respectively.

For ease of interpretation, I focus primarily on the mean percentage change caused by the QIZ agreement, though Tables 11 and 12 report much more detailed results. In almost all instances, the ridge bias-adjusted gaps are larger than the non-bias-adjusted alternatives, which signifies that there are likely overfitting issues in the latter. Therefore, I focus my interpretation on bias-adjusted gaps rather than non-adjusted ones.

Figure 3: SCM Gaps for Egyptian Exports (a) and Israeli Exports (b).



Note: The horizontal axis is measured in years, with the vertical marker representing the year the QIZ agreement took effect. The vertical axis represents the difference between log observed exports and log synthetic exports for the treated country, measured in thousands of real 2012 dollars.

Table 11: Observed-Synthetic Gaps for Egyptian Exports

	All Goods		LT Goods		Non-LT Goods	
YEAR	Classic (%)	Adjusted (%)	Classic (%)	Adjusted (%)	Classic (%)	Adjusted (%)
2005	-0.240 (-21.30)	-0.409 (-33.57)	0.145 (15.60)	0.020 (2.02)	0.815 (125.92)	0.490 (63.23)
2006	0.441 (55.42)	1.045 (184.32)	1.137 (211.74)	1.180 (225.44)	0.330 (39.10)	1.158 (218.36)
2007	0.123 (13.14)	0.268 (30.72)	1.247 (247.99)	1.501 (348.62)	0.117 (12.41)	0.024 (2.43)
2008	0.021 (2.17)	-0.120 (-11.27)	0.259 (29.56)	0.108 (11.40)	1.307 (269.51)	1.334 (279.62)
2009	0.654 (92.36)	0.738 (109.23)	-1.653 (-80.85)	-1.541 (-78.58)	1.413 (310.83)	1.519 (356.77)
2010	0.218 (24.38)	0.195 (21.57)	-1.430 (-76.07)	-1.368 (-74.54)	1.093 (198.32)	1.554 (373.04)
2011	-0.508 (-39.81)	-0.870 (-58.12)	-1.004 (-63.36)	-1.167 (-68.87)	1.257 (251.49)	0.399 (49.03)
2012	0.131 (14.00)	-0.510 (-39.96)	-0.876 (-58.36)	-1.220 (-70.48)	0.891 (143.76)	0.030 (3.05)
2013	0.238 (26.92)	-0.281 (-24.50)	-0.067 (-6.48)	-0.555 (-42.59)	0.928 (152.94)	0.451 (56.99)
2014	0.817 (126.36)	0.292 (33.91)	0.296 (34.45)	-0.619 (-46.15)	1.643 (417.07)	1.318 (273.59)
2015	-0.164 (-15.14)	-0.662 (-48.43)	0.283 (32.71)	-0.535 (-41.43)	1.676 (434.41)	0.849 (133.73)
2016	-0.447 (-36.04)	-0.931 (-60.58)	0.158 (17.12)	-0.584 (-44.23)	1.396 (303.90)	0.479 (61.45)
2017	-0.230 (-20.54)	-0.543 (-41.91)	0.196 (21.65)	-0.285 (-24.80)	1.491 (344.15)	0.930 (153.45)
Mean	0.081 (8.46)	-0.138 (-12.85)	-0.101 (-9.579)	-0.390 (-32.27)	1.104 (201.74)	0.810 (124.88)

Note: The first column displays the gaps for the log-specified synthetic, whereas the second column combines log specification and Ridge bias correction. Percent change in parentheses, calculated using the equation: $(exp(gap) - 1) * 100\%$. The last row shows the simple average of the gaps from 2005-2017.

Table 12: Observed-Synthetic Gaps for Israeli Exports

	All Goods		LT Goods		Non-LT Goods	
YEAR	Classic (%)	Adjusted (%)	Classic (%)	Adjusted (%)	Classic (%)	Adjusted (%)
2005	0.237 (26.74)	0.019 (1.92)	1.949 (602.17)	1.875 (552.08)	0.334 (39.65)	-0.375 (-31.27)
2006	0.336 (39.93)	0.290 (33.64)	1.204 (233.34)	1.296 (265.46)	0.645 (90.60)	0.532 (70.23)
2007	0.606 (83.31)	0.624 (86.64)	1.332 (278.86)	0.896 (144.98)	0.815 (125.92)	0.635 (88.70)
2008	-0.219 (-19.67)	-0.498 (-39.23)	0.371 (44.92)	-0.365 (-30.58)	0.527 (69.38)	0.028 (2.84)
2009	0.213 (23.74)	0.443 (55.74)	0.670 (95.42)	0.592 (80.76)	0.815 (125.92)	0.934 (154.47)
2010	-0.132 (-12.37)	0.394 (48.29)	-0.038 (-3.73)	0.257 (29.30)	0.552 (73.67)	0.948 (158.05)
2011	0.490 (63.23)	0.965 (162.48)	0.706 (102.59)	0.951 (158.83)	0.382 (46.52)	0.879 (140.85)
2012	0.702 (101.78)	1.281 (260.02)	1.626 (408.35)	1.885 (558.64)	0.706 (102.59)	1.335 (280.00)
2013	0.225 (25.23)	0.867 (137.98)	0.532 (70.23)	0.988 (168.59)	0.545 (72.46)	1.143 (213.62)
2014	0.736 (108.76)	1.314 (272.10)	1.894 (564.59)	2.269 (866.97)	0.005 (0.50)	-0.510 (-39.95)
2015	-0.290 (-25.17)	0.293 (34.04)	-0.453 (-36.43)	0.251 (28.53)	0.250 (28.40)	0.565 (75.94)
2016	-0.030 (-2.96)	0.418 (51.89)	0.595 (81.30)	1.393 (302.69)	-0.610 (-45.66)	-0.331 (-28.18)
2017	0.401 (49.33)	0.700 (101.38)	1.684 (438.71)	1.686 (439.78)	-0.473 (-37.69)	-0.130 (-12.19)
Mean	0.252 (28.65)	0.547 (72.79)	0.929 (153.20)	1.075 (193.00)	0.346 (41.29)	0.435 (54.47)

Note: The first column displays the gaps for the log-specified synthetic, whereas the second column combines log specification and Ridge bias correction. Percent change in parentheses, calculated using the equation: $(exp(gap) - 1) * 100\%$. The last row shows the simple average of the gaps from 2005-2017.

Somewhat contrary to the DiD results outlined in Section 5.1, Egyptian exports were, on average, only 12.85 percent less for all goods compared to the synthetic control, which is substantially smaller in magnitude than the results presented in Table 6, which further illustrates the DiD overestimation issues caused by poor pretreatment fit and potential downward bias of the coefficients. The results diverge further when analyzing the differences between LT and non-LT goods. The gaps in LT goods indicate that observed LT exports were 32.27 percent less than the synthetic control, slightly smaller than those estimated in Table 6. However, the aggregate gap measurement on LT exports obscures the shift from positive to negative growth in the late 2010s, as shown in Figure 3, which solidifies previous findings of the ease of change due to the high mobility of LT goods production. Further, non-LT exports were 124.88 percent more than the synthetic control, a substantial divergence from previous estimations that identified a negative effect.

Due to the insignificance of the DiD estimation, the gaps for Israeli coefficients present the first opportunities to interpret potential treatment effects for the country. The results show substantial positive increases for all goods across the margin. Further, this increase appears to be led primarily by LT goods, which are 193 percent larger than the synthetic control compared to 54.47 percent for non-LT goods, directly supporting previous findings in this study and the literature broadly.

However, there are two key limitations to this interpretation. First, the bias-adjusted lines report strong aggregate effects, but they generally fluctuate around zero or, in the case of Egyptian LT goods, experience a substantial change from positive to negative. These trends suggest that the effects of the QIZ agreement may not be strong enough to overcome the idiosyncratic shocks inherent to international trade. Second, a key limitation of SCM is that gap estimates do not contain any measure of their statistical significance. I address this limitation in the following section by discussing the significance of the gap metrics in the context of placebo permutations.

5.2.3 Placebo Permutations. RMSPE differs significantly from traditional error measurements, such as standard error or p-values. The RMSPE measures the lack of fit between the path of the outcome variable for the treated or donor countries and their synthetic counterparts. Due to this unique design, a synthetic counterfactual that accurately mirrors the pretreatment values of the outcome variable for the treated or donor country and demonstrates a significant change will have a low pretreatment RMSPE and a high post-treatment RMSPE. However, the RMSPE scales with the magnitude of the outcome variable, which means that there is no benchmark level for the RMSPE that determines significance.

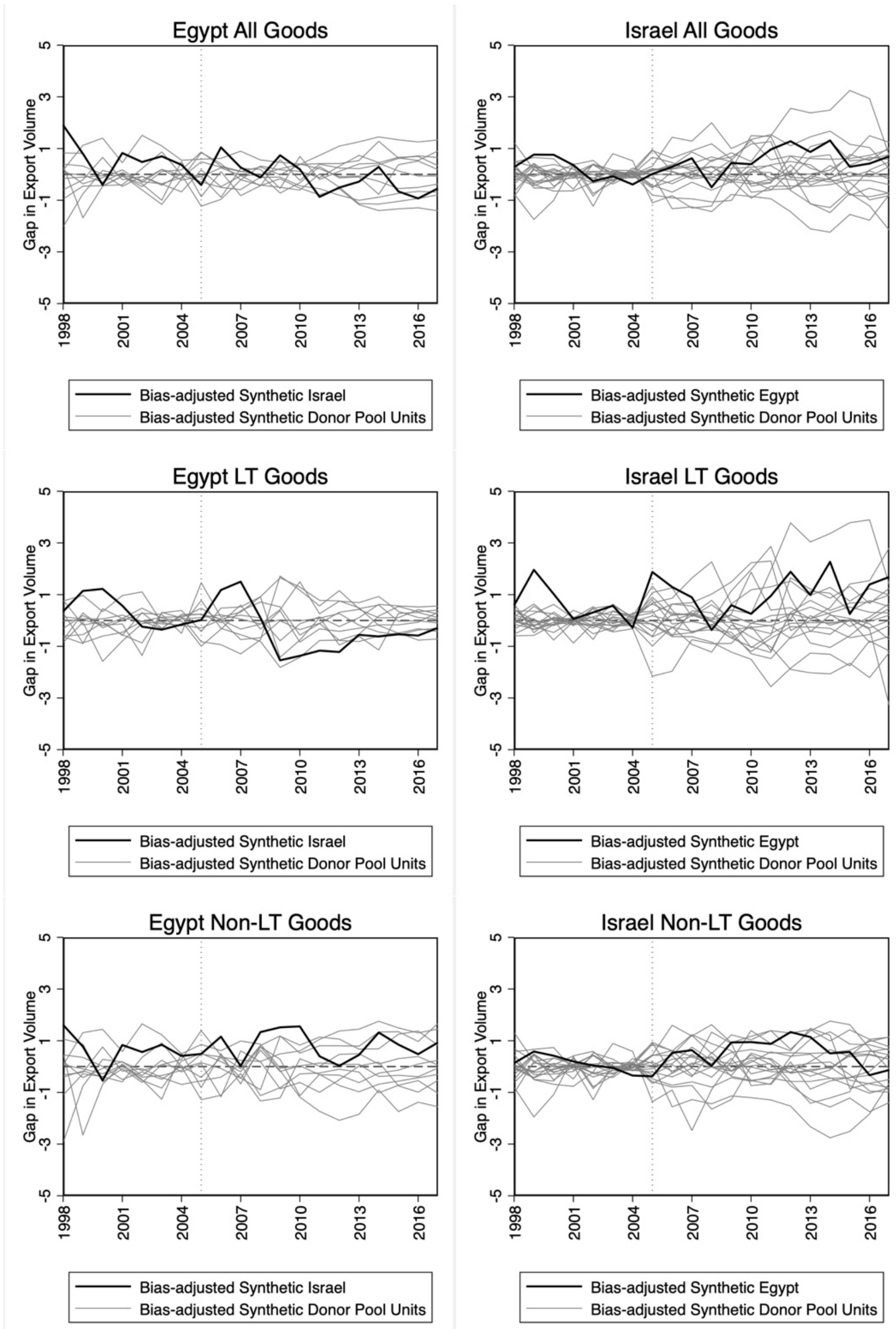
To overcome this limitation, I conduct in-space placebo permutations following Abadie et al. (2010; 2015), which artificially reassigns the treatment to each potential donor country and ranks the treatment country's ratio of post to pretreatment RMSPE compared to the corresponding ratios for all potential donor countries for each post-treatment year. For example, the confidence in highly positive or negative effects due to treatment depends on whether the treatment unit's RMSPE continually ranks high against the placebo permutations during the post-treatment period. Following Abadie et al. (2015), I also construct p-values by estimating in-space placebo effects for each unit and then calculating the fraction of such effects greater than or equal to the effect estimated for the treated unit. I report the ridge bias corrected RMSPE ranks visually in Figure 4 and empirically with corresponding p-values in Table 13.

Though the results of these placebo tests present varying strengths depending on year, QIZ signatory, and margin, the broad interpretation suggests that there are neither aggregate nor trade margin effects of the QIZ agreement on Egyptian or Israeli Exports. For all goods, there are slight variations in the RMSPE rank of the treatment group, but they never break into the top half of ranks for either signatory. For LT goods, Egyptian exports rank slightly higher for the three years after the signing of the agreement before stabilizing slightly below the bottom half of the ranking, whereas Israeli exports rank consistently low. For non-LT goods, the effects for Egyptian exports rank consistently low, whereas those for Israeli exports strengthen in the latter part of the post-treatment period, indicating some potential delayed implementation of the effects of the QIZ agreement.

However, a common characteristic across the results whose ratios rank highly compared to the placebo permutations is that they are not consistently positive or negative, further indicating the likely negligible effects of the QIZ agreement. Though I detail limitations and potential issues with the study in the subsequent section, the inconclusive evidence presented by these placebo permutations merits a serious reanalysis of the benefits of the QIZ agreement.

Figure 4: Cross-Sectional Placebo Permutations for Egyptian (a) and Israeli Exports (b)

(a) (b)



Note: The horizontal axis is measured in years, with the vertical marker representing the year the QIZ agreement took effect. The vertical axis represents the difference between log observed exports and log synthetic exports for treated and potential donor countries, measured in thousands of real 2012 dollars.

Table 13: RMSPE Ranks for Placebo Permutations

	All Goods		LT Goods		Non-LT Goods	
YEAR	Egypt (*p*)	Israel (*p*)	Egypt (*p*)	Israel (*p*)	Egypt (*p*)	Israel (*p*)
2005	7 (0.636)	17 (0.944)	11 (1)	12 (0.667)	9 (0.818)	10 (0.556)
2006	6 (0.545)	16 (0.889)	5 (0.455)	11 (0.611)	7 (0.636)	7 (0.389)
2007	7 (0.636)	14 (0.778)	4 (0.364)	12 (0.667)	7 (0.636)	7 (0.389)
2008	9 (0.818)	15 (0.833)	5 (0.455)	12 (0.667)	8 (0.727)	11 (0.611)
2009	9 (0.818)	15 (0.833)	6 (0.545)	14 (0.778)	7 (0.636)	8 (0.444)
2010	9 (0.818)	16 (0.889)	6 (0.545)	15 (0.833)	8 (0.727)	7 (0.389)
2011	9 (0.818)	14 (0.778)	6 (0.545)	16 (0.889)	8 (0.727)	6 (0.333)
2012	9 (0.818)	9 (0.500)	6 (0.545)	16 (0.889)	8 (0.727)	6 (0.333)
2013	10 (0.909)	10 (0.556)	6 (0.545)	16 (0.889)	8 (0.727)	5 (0.278)
2014	10 (0.909)	10 (0.556)	6 (0.545)	16 (0.889)	8 (0.727)	5 (0.278)
2015	10 (0.909)	11 (0.611)	6 (0.545)	17 (0.944)	8 (0.727)	5 (0.278)
2016	10 (0.909)	11 (0.611)	6 (0.545)	16 (0.889)	8 (0.727)	5 (0.278)
2017	10 (0.909)	12 (0.667)	6 (0.545)	16 (0.889)	9 (0.818)	5 (0.278)
Mean	8.846 (0.804)	13.077 (0.727)	6.077 (0.552)	14.538 (0.808)	7.923 (0.720)	6.692 (0.372)

Note: Each column combines natural logarithmic specification and Ridge bias correction to display the ranked RMSPE ratios. The lower the numerical value of the rank, the higher the treated unit RMSPE ranks against the donor placebo RMSPE permutations. P-value in parentheses, with a high p-value indicating the given treatment unit ranks poorly compared to the donor placebos. The last row shows the simple average of the ranking and p-values from 2005-2017.

6. Conclusion

This paper was the first to empirically assess the spillover effects of the QIZ agreement on the marginal growth in Egyptian-Israeli non-QIZ trade using commodity and aggregate-level export data. Though the agreement was primarily aimed at granting Egyptian industries duty-free access to US markets, this paper focused on the success (or lack thereof) of its secondary goal of fostering economic normalization. Achieved through the Israeli value-added criterion, the agreement led to reductions in NTTBs for Egyptian-Israeli trade in QIZ goods, which are highly prone to spillover effects. With existing QIZ literature focusing mainly on the ex-post growth in Egyptian exports to the US or on more qualitative investigations of the broad effects on Egyptian-Israeli relations, this research shifted the focus towards evaluating the agreement's uniquely normalizing potential utilizing robust empirical methods.

I initially employed a commodity-level difference-in-differences approach and concluded the QIZ agreement negatively affected the marginal growth of Egyptian exports to Israel. However, violations of parallel trends and tests of country-specific time trends indicated substantial issues with poor pretreatment fit and potential coefficient bias.

I addressed these concerns and the effects of time-varying unobservable confounders by employing an aggregate-level synthetic control. I include natural logarithmic specification on the outcome variable and ridge bias correction to preemptively address convex hull violations and concerns surrounding the small number of potential donor units. The results of the synthetic analysis reveal that the QIZ agreement had neither aggregate nor marginal effects on the trade margins of Egyptian or

Israeli exports that endured for the entire post-treatment period. These results reject my initial hypothesis and indicate the absence of spillover effects that would have eliminated NTTBs for non-QIZ commodities, a significant diversion from recent findings, and an indication of continued Egyptian resistance towards economic normalization. Though these results may also be attributed to the failure of the QIZ agreement to counteract the incentives of trade diversion, they nonetheless indicate the weak and potentially negligible nature of the effects of the QIZ agreement on non-QIZ bilateral trade volumes.

Though both the DiD and SCM analyze marginal trade effects, which represent commodity-level effects to some extent, only the DiD captures treatment effect heterogeneity at the commodity level and controls for industry-level effects. Therefore, future research could address underlying issues and arrive at commodity-level conclusions that control for industry-level effects with fewer issues with parallel trend violations by employing DiD with propensity score matching to select and weight control countries.

As there are fewer concerns with the validity of the SCM, the conclusions of aggregate-level effects are markedly more confident. However, there are potential underlying issues with the data analysis that may contribute to the inconclusive results of the SCM. Though most countries maintain a minimum value for which they record and report exports to international trade organizations, analysis of Egyptian exports reveals recorded quantities as small as $10, which increases the sensitivity of Egyptian export data to errors. Though the risk of widespread attenuation bias is relatively low, considering most Egyptian export data to other countries in this study reports a positive relationship over time, export data would have to be cross-validated with corresponding import data to confirm its accuracy. Further, limitations on covariate data availability and identifying appropriate control units prevented the study from having at least ten pretreatment years in the SCM, a standard practice in the literature. Though I attempt to account for this by employing ridge bias correction, this limitation prevented the conduction of in-time placebo tests and may have hindered the synthetic from capturing the treated group's longer-term trends and cyclical trade patterns. Finally, the computational limitations of SCM and the subsequent aggregation process may have contributed to a loss of beneficial commodity-level insights into the relationship between the QIZ agreement and Egyptian-Israeli trade.

While future studies should work to address these potential SCM limitations, as it stands, this paper provides novel contributions to the literature on the post-PTA trade growth and an analysis of US economic policy in the Middle East. Despite its ability to control for time-varying unobservable confounders and address issues with poor pretreatment fit, this is the second paper, to my knowledge, to employ SCM in an analysis of the marginal effects of PTA implementation, after Cho et al. (2022). Though they must be weighed against the costs of aggregation, the unique benefits of SCM critically address underlying issues with poor pretreatment fit and the fundamental differences between units. Additionally, to my knowledge, I am the first to employ SCM with ridge bias corrections to address convex hull violations in an analysis of the marginal effects of PTA implementation. Given the highly unique nature of treated countries, potential non-uniqueness between donor countries, and the manifestation of these issues in broadly available export-determinant covariates, future employments of SCM in trade literature should closely consider recent advances in weighting optimization and the inherent risk of convex hull violations sparked by the idiosyncratic nature of bilateral trade.

Additionally, this paper argues for further consideration of how the benefits of eliminating NTTBs manifest in politically contentious environments. Existing literature finds a high propensity for the elimination of NTTBs to spill over onto new partners and goods, but does so under an assumption of absolute amicability between trade partners (Marković et al., 2021; Hou, 2023). However, the cooperation necessary for NTTB elimination combined with my aggregate results, which indicate that the NTTB elimination in QIZ commodities did not spill over onto non-QIZ commodities, indicates the need for further analysis of trade creation in a contentious environment. In the context of deteriorating relations between long-time trade partners, this paper encourages further analysis of the social and political dimensions of NTTB elimination.

Finally, this paper serves as an initial step in unpacking the inherently complex relationship between trade and normalization in the context of the QIZ agreement. However, its inconclusive results are potentially damning for the efficacy of the QIZ agreement on promoting economic normalization. In the absence of any traceable spillover effects on Egyptian-Israeli trade volumes, the QIZ agreement does not come close to meriting the artificial limit it places on the Egyptian TCLF industry in terms of total production capacity and enforced trade diversion. Additionally, these results would indicate that the US needs to take bolder actions, such as requiring Egypt and Israel to reduce tariffs between one another, to adequately promote economic normalization. Future strategies for economic normalization in the Middle East must consider proven methods for increasing economic relations rather than rely on the spillover effects of

NTTB elimination, which this paper has proven to be ineffective. While investigating alternative methods for economic normalization becomes increasingly urgent, with the QIZ agreement proven ineffective for political goals and a hindrance to Egyptian economic growth, arguments against a full Egyptian-US FTA become nascent at best. With the political situation in the Middle East experiencing one of its most tumultuous but potentially transformative moments in recent history, the US must seriously evaluate the efficacy of its current economic policy in the region and institute solutions that not only promote its regional priorities but also the economic development of key strategic partners.

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Appendix

Table A1: Margin Country Pairs of Non-QIZ Commodities for DiD

	LT Goods		Non-LT Goods	
	Number	% of All Goods	Number	% of All Goods
Egyptian exports to:				
Israel	816.66	94.74	45.34	5.26
India	844.87	98.01	17.13	1.99
Japan	842.47	97.73	19.53	2.27
Kenya	826.23	95.85	35.77	4.15
South Africa	840.11	97.46	21.89	2.54
Average	834.07	96.76	27.93	3.24
Israeli exports to:				
Egypt	806.62	93.58	55.38	6.42
Chile	820.77	95.22	41.23	4.78
Kenya	827.79	96.03	34.21	3.97
Nigeria	796.83	92.44	65.17	7.56
Peru	836.72	97.07	25.28	2.93
Average	820.50	95.19	41.50	4.81

Table A2: Margin Country Pairs of Non-QIZ Commodities for SCM

	LT Goods		Non-LT Goods	
	Number	% of All Goods	Number	% of All Goods
Egyptian exports to:				
Israel	816.66	94.74	45.34	5.26
Canada	855.07	99.20	6.93	0.80
China	857.79	99.51	4.21	0.49
Djibouti	845.61	98.10	16.39	1.90
Ghana	847.56	98.32	14.44	1.68
India	844.87	98.01	17.13	1.99
Japan	842.47	97.73	19.53	2.27
Kenya	826.23	95.85	35.77	4.15
Nigeria	844.63	97.98	17.37	2.02
Pakistan	850.18	98.63	11.82	1.37
South Africa	840.11	97.46	21.89	2.54
Average	842.83	97.78	19.17	2.22
Israeli exports to:				
Egypt	806.62	93.58	55.38	6.42
Chile	820.77	95.22	41.23	4.78
Costa Rica	854.6	99.14	7.40	0.86
Cote d'Ivoire	844.65	97.99	17.35	2.01
Dominican Republic	835.57	96.93	26.43	3.07
Ecuador	842.27	97.71	19.73	2.29
Ethiopia	850.59	98.68	11.41	1.32
Guatemala	838.56	97.28	23.44	2.72
Indonesia	839.99	97.45	22.01	2.55
Kazakhstan	841.46	97.62	20.54	2.38
Kenya	827.79	96.03	34.21	3.97
New Zealand	818.31	94.93	43.69	5.07
Nigeria	796.83	92.44	65.17	7.56
Peru	836.72	97.07	25.28	2.93
Sri Lanka	851.32	98.76	10.68	1.24
Uganda	845.54	98.09	16.46	1.91
Vietnam	845.36	98.07	16.64	1.93
Zimbabwe	849.16	98.51	12.84	1.49
Average	835.90	96.97	26.10	3.03

Table A3: Im-Pesaran-Shin (2003) Unit Root Tests

	All Goods		Extensive		Intensive	
Variable	Egypt	Israel	Egypt	Israel	Egypt	Israel
Export Volume						
P-value	0.0001	0.0000	0.0001	0.0000	0.0000	0.0000
Log Export Volume						
P-value	0.0000	0.0000	0.0164	0.0012	0.0000	0.0000

Note: P-values are adjusted for panels' heterogeneous nature and account for the systematic trends inherent to international trade. The H_0 of this hypothesis is that panels have unit roots, while the H_a of this test is that panels are stationary.