Four Essays in International Trade and Economic Development

By

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Abstract

This dissertation comprises four essays investigating the impact of trade agreements on trade outcomes and the relationship between export diversification and economic growth.

Chapter 1 investigates the effect of the U.S. African Growth and Opportunity Act (AGOA) on Sub-Saharan African (SSA) countries' exports to the U.S. We used synthetic control method and U.S.-AGOA data. The study reveals that AGOA member nations experienced a significant $818.11 million annual increase (42%) in exports compared to levels expected without AGOA. The impact varied across countries and product types, with agricultural, mineral, and textile/apparel exports surging annually by 42%, 15%, and 52%, respectively. This was validated using difference-in-differences and event study approaches, confirming the robustness of the findings.

Chapter 2 focuses on the impact of Brexit on trade flows between the United Kingdom (UK) and the European Union (EU) and the rest of the world (ROW). I used trade data from BACI and CEPII Gravity Database. The study employed a difference-in-difference approach integrated into the gravity model framework and estimated using the Heckman selection model. Brexit led to reduced UK imports and exports values with both the EU and ROW, affecting durable and non-durable goods. In the UK, the import value from the EU decreased by 0.41%, whereas imports from the rest of the world (ROW) dropped by 0.20%. Specifically, imports of durable goods from the EU and ROW declined by 0.39% and 0.24%, respectively. Non-durable goods also saw decreases in imports, with a 0.41% fall from the EU and a 0.18% drop from the ROW.

Conversely, UK exports faced declines as well, with a 0.86% decrease in total export value to the EU and a 0.47% decrease in export value to the ROW. Durable goods exports from the UK

experienced a 0.64% decrease to the EU and a 0.61% decrease to the ROW. Non-durable goods exports followed a similar trend, declining by 0.91% to the EU and 0.43% to the ROW. These results are supported by robustness checks using various methods.

Chapter 3 explores how the North American Free Trade Agreement (NAFTA) unrestricted sugar trade agreement impacted sugar consumption and diabetes prevalence in the United States. We applied methods including synthetic control method, difference-in-difference, and panel event-study to estimate the impact of the policy using sugar consumption and health data for seven countries. Post-agreement, US sugar consumption increased annually by 16% (5240g per capita), corresponding to a 1% annual rise in diabetes prevalence, incurring an estimated $324.37 million yearly. State-level impacts varied, notably affecting areas with specific demographic characteristics such as higher poverty level, greater Black population, lower percentage of the population with a high school degree, and higher percent female population.

Chapter 4 investigates the relationship between export diversification and economic growth in thirty-nine Sub-Saharan African countries. We used macroeconomic data from United Nations Conference on Trade and Development (UNCTAD) and the Arellano-Bond difference generalized method-of-moment estimator, the study finds positive economic growth effects with better corruption control and governance quality, showcasing export diversification beyond the growth-optimized level. This was confirmed through robustness checks using country-fixed effect regression, ensuring the stability of the findings.

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List of Abbreviations

AfCFTA	African Continental Free Trade Area
AGOA	African Growth and Opportunity Act
EU	European Union
LMICs	Low-and Middle – Income Countries
NAFTA	North American Free Trade Agreement
NCDs	Diet-related Noncommunicable Disease
OECD	Organization for Economic Co-operation and Development
PPML	Poisson Pseudo-Maximum Likelihood
SSA	Sub-Saharan Africa
TCA	Trade and Cooperation Agreement
UNCTAD	United Nations Conference on Trade and Development
WHO	World Health Organization

Chapter 1

Trade and Development Implications of U.S. African Growth and Opportunity Act[1]

Introduction

The level of development in Africa has been significantly shaped by its historical economic growth trajectory. Following the period of decolonization between 1960 and 1973, when many African nations gained political independence, there was optimism for improved economic performance (Bosker and Garretsen, 2012; Hodey et al., 2015). However, this initial growth spurt was not sustained in the subsequent decades.

To promote economic development in Sub-Saharan Africa (SSA), the U.S. government and selected African nations signed a non-reciprocal trade agreement under the U.S. African Growth and Opportunity Act (AGOA) in 2000. The AGOA now includes thirty-nine countries and was reauthorized for ten more years in 2015, extending it to 2025. During the early-2000s, SSA experienced somewhat rapid economic growth, known as the "African economic renaissance" (UNECA, 2012)[2]. The rapid growth is associated with several factors, such as an increase in global commodity demand that raised prices and a rise in demand for minerals and petroleum products by Asian countries. The international debt relief program, coupled with conflict resolution in some countries, also boosted foreign investor confidence (Arieff et al., 2010). In this paper, we examine whether the AGOA contributed to the growth in SSA by increasing exports of its member nations.

Views on the efficacy of non-reciprocal trade deals such as the AGOA are mixed. While some studies have shown that the economic benefits of trade agreements in SSA include increased

[1] **Authors:** Derick Taylor Adu, Dr. Wenying Li, Dr. Wendiam Sawadgo

[2] United Nations Economic Commission for Africa.; African Union Commission (2012-03). Economic Report on Africa 2012: Unleashing Africa's Potential as a Pole of Global Growth. Addis Ababa: UNECA. https://hdl.handle.net/10855/21725.

trade flows among SSA and partner countries (Tadesse and Fayissa, 2008) and decreased transaction costs through reduction or elimination of non-tariff barriers (Langyintuo et al., 2005), other studies have criticized such programs, noting that benefits provided to participating countries have not been inclusive (Blackman and Mutume, 1998; Raghavan, 2000). For example, Nouve and Staatz (2003) find that African exports to the U.S are dominated by petroleum products that have relatively low value-added, and existing U.S.-African trade is characterized by exports from a few African countries. However, there is evidence that the AGOA has stimulated exports among beneficiary countries (Edward and Lawrence 2010), as total AGOA exports to the U.S. more than quadrupled from 2001 to 2013, while non-oil exports more than tripled during the same period.

Studies looking at the impact of the AGOA have used various methods and come up with different conclusions. Moyo et al. (2018) use propensity score matching (PSM) and difference-in-differences (DD) methods with data from 2000 to 2014 to estimate the effect of the AGOA trade agreement on the export growth of SSA countries and find that the AGOA has had a negative impact on exports from Africa. However, since the AGOA applies selectivity to both countries and products but not to all countries or products, the regular DD design may suffer from selection bias (Besley and Case, 2000); thus, several studies have used alternative approaches. Frazer and Van Biesebroeck (2010) assess the impact of the AGOA using the triple-difference estimation approach with the data from 1998 to 2006 and find that the AGOA has had a large and positive impact on apparel, agricultural, and manufactured products covered by AGOA exported to the U.S. Tadesse and Fayissa (2008) employ the augmented gravity model to estimate the impact of U.S import from SSA using harmonized system (H.S.) 2–digit disaggregated trade data from 1991 to 2006. The authors decompose the total effect of AGOA into intensive and extensive margin effects. They find a positive but insignificant effect of the AGOA on exports but statistically significant intensive and extensive margins, suggesting that the AGOA has contributed to the initiation of new but insubstantial exports. Studies have also focused on specific product categories, such as apparel (Collier and Venables, 2007) and agricultural products (Di Rubbo and Canali, 2008). One alternative method for studying the impact of the AGOA is the synthetic control method (SCM), which provides a data-driven approach to choosing control groups in comparative case studies. Athey and Imbens (2017, p. 9) consider the SCM to be "perhaps the most significant advancement in the field of policy evaluation research in the last 15 years." The major advantage of SCM is that it can account for the effects of confounders changing over time, which are a concern in this setting. To the best of our knowledge, only Kassa and Coulibaly (2018) use the synthetic control method (SCM) in this setting, but they focus on individual AGOA countries[3] with different eligibility periods.

Our study provides four main contributions to our understanding of the impact of the AGOA. First, using SCM with both multiple and single treated units, we extend Kassa and Coulibaly (2018) by estimating the average impact of AGOA on export trajectories of sixteen member countries with the same treatment period altogether, and individually. While other studies have used the synthetic control method to evaluate the impact of AGOA on exports, the evaluation was previously done only on individual countries being subjected to policy intervention at different time periods. Second, utilizing the SCM with a singular treated unit methodology, we can quantify the overall monetary implications of AGOA on exports to the U.S., an empirical gap previously unexplored in academic literature. Third, we estimate the determinants of trade gains through AGOA to understand why these gains differ across beneficiary countries. Kassa and Coulibaly

[3] South Africa, Angola, Nigeria, Ivory Coast, Kenya, Madagascar, Ghana, Republic of the Congo, Cameroon, Lesotho, Namibia, Ethiopia, Zambia, Niger, Botswana, Tanzania, Mozambique, Uganda, Malawi, Rwanda, Burkina Faso, Benin, and Togo.

(2018) explored AGOA trade gain determinants, but our study further incorporates variables such as financial freedom, government spending, regime durability, and tax burden to enhance understanding of SSA trade gains. Finally, we investigate whether the impact of AGOA on exports to the US differs by HS product, such as agricultural, textiles and apparel, and mineral products.

We find that AGOA member nations had US$818.11 million higher exports than they would have had in the absence of the AGOA, on average annually, a 42% increase. However, there were substantial differences across countries. Congo, Lesotho, Nigeria, and South Africa registered substantial export gains, whereas Malawi and Mauritius have suffered large losses. In addition, we explore what social and economic factors would allow beneficiary countries to benefit from AGOA. We find that trade gains from AGOA decrease with higher agricultural exports and increase with higher petroleum exports. Also, ICT infrastructure, institutional integrity, relaxed labor market regulations, sound macroeconomic variables such as stable and competitive exchange rate, low inflation, government spending, and reduction in government intervention in the financial system mainly drive differences in export gains across AGOA member nations. Finally, we discover that the impact of AGOA differs by product category, with agricultural commodities experiencing a 42% increase, textiles and apparel a 52% increase, and minerals a 15% increase.

Background Information on the AGOA Implementation

On October 1, 2000, the “African Growth and Opportunity Act” (AGOA) came into effect. The program's main objective is to give duty-free access of selected products from SSA countries to the U.S. market. In its early stages, thirty-four SSA countries were granted eligibility. Later, the number of beneficial countries increased to thirty-nine (see Table 1 for details). The program was reauthorized in 2015 and extended to 2025. The underlying principle of the program was to “promote stable and sustainable economic growth and development in SSA” through trade.

The AGOA has two key provisions. The first provision gives eligible economies quota-free and duty-free access to a selected 1800 product groups[4] at harmonized system (H.S.) 8 (HS-8digit) classification. [5] This increased the number of products with preferential access under the pre-existing General System of Preferences program (GSP) from 5,000 to 6,800 product groups[6] at the HS-8digit classification. Additionally, AGOA members are exempt from caps on preferential duty-free imports due to the "competitive need limitations" (CNL) program. The second provision, known as the apparel provision, gives quota-free and duty-free access of selected apparel and textile articles manufactured in eligible SSA countries, subject to a cap. This removes the average most-favored-nation (MFN) tariff (about 11.5%) on textile and apparel imports to the U.S., capturing ineligible products under the GSP or the first provision of AGOA. The articles comprise SSA fabrics and yarns-made apparel, textiles, and textile articles manufactured exclusively in SSA, including cashmere and merino sweaters and eligible hand-loomed, handmade, and printed fabrics, increasing the number of manufacturing products-textile and apparel compared to the GSP. It also provides access to diverse apparel and textile products except for leather products, headgear, glass, and glassware.

From the "Special Rule for Apparel" (SRA) for under-developed eligible economies, twenty-two SSA economies were given additional duty-free and quota-free preferential access for apparel manufactured from fabrics sourced from anywhere in the globe.[7] The "rule of origin" provision has been comparatively liberal to these groups of countries.

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[4] Despite this vast coverage of products, Brenton and Ikezuki (2004) assert that an appreciable number of items have been excluded from the AGOA, such as meat products, dairy products, sugar, chocolate, peanuts, prepared food products, and tobacco, which potentially could be major exports for several SSA countries.

[5] This is an 8-digit product classification code administered by the U.S. International Trade Commission (USITC) on exported commodities to U.S. The code is more of a requirement. It is used to label the particular type of commodity after shipping. This is required for the determination of its import duty and tariffs rates by the receiving country.

[6] This number could change across time depending on changes in legislation and revisions in classification.

[7] Lesser-developed countries are those with a per capita gross national product of less than $1500 a year in 1998 as measured by the World Bank.

For the rest of the eligible SSA economies,[8] the "rule of origin" requires the total cost or value of the materials produced in one or more AGOA beneficiary countries plus the direct cost of processing operations to be greater than 35 percent of the appraised value, for products exported to the U.S. Additionally, preferential treatment for apparel and textiles is conditioned on the eligible countries adopting an effective visa system and related procedures that facilitate compliance with the "rules of origin" requirements. However, the impact of "rules of origin" on exports is unclear. In the face of binding constraints, it could impede export opportunities. In the same vein, countries can benefit from it because it can enhance domestic manufacturing by motivating the sourcing of apparel from domestic production and processing. The reauthorization of AGOA in 2015 calls for higher reciprocity in the removal of restrictions and investment in SSA and allows for increased review of eligibility compliance.[9]

Empirical Methods

In our research, we employ the SCM for both multiple and single treated units to assess AGOA's cumulative and individual country impacts, respectively. Additionally, we utilize a fixed effect regression model to pinpoint key traits explaining the trade gain variations across countries due to AGOA, further examining the program's diverse effects. Finally, we use the DD approach as a robustness check to compare the exports of AGOA countries to the exports of non-AGOA countries.

[8] See the full list of these countries on Table 1.

[9] In July 2017, USTR announced initiation of an out-of-cycle review of the eligibility of Rwanda, Tanzania, and Uganda in response to a petition filed by a trade group that represents secondhand clothing exporters - the Secondary Materials and Recycled Textiles Association (SMART).

Synthetic Control Method with Multiple Treated Units

The SCM, pioneered by Abadie and Gardeazabal (2003) and Abadie et al. (2010), is an alternative method for analyzing the effect of an event or policy intervention. Although DD analysis and randomized control trials are often used in microeconomic research, these techniques are not always useful for studying macroeconomic policies or events. This is because it is difficult to meet some of the assumptions underlining DD, such as the "parallel trend assumption" with macroeconomic policy or event and applying randomized experiments on the macro scale is often unattainable. The SCM offers a bridge between qualitative and quantitative methodologies, as it provides a systematic way to choose comparison units in comparative case studies (Abadie et al., 2015). The main idea behind the SCM is that the outcomes from the control units are weighted to construct the counterfactual unit for the treated unit (called the "donor pool") in the absence of the treatment (Kreif et al., 2016). A synthetic control unit is defined as the time-invariant weighted average of available control units, which have similar pre-intervention characteristics and outcome trajectory to the treated unit prior to the intervention. In contrast to DD, SCM allows the effects of observed and unobserved predictors of the outcome to change over time, if pre-intervention covariates have a linear relationship with outcomes post-treatment (Kreif et al., 2016).

In the context of evaluating the policy impact of AGOA, the SCM addresses the endogeneity challenge associated with omitted variable bias (e.g., a preference for products made in a specific country, efficiency in customs clearance, domestic policies, and macroeconomic conditions) by accounting for the presence of time-varying unobservable confounders (Billmeier and Nannicini, 2013). The SCM builds on DD estimation but uses arguably more attractive comparisons to obtain causal effects (Athey and Imbens, 2017). SCM can also safeguard against the estimation of extreme counterfactuals (King and Zeng, 2006). The scenario described by

Abadie et al. (2010) assumes that only the first country of the $D + 1$ countries is exposed to the policy. In our setting, the treated country is in the AGOA program, while the other countries are not in the program and are part of the donor pool. Outcomes are observed for T periods, and the program starts at $T_0 + 1$. The observed outcome vector of each country d is that.

$$Y^d = \left(Y_1^d \ldots Y_{T_0}^d \ldots Y_T^d\right)' \quad (1)$$

The observed outcome is specified as the sum of a treatment-free potential outcome, Y_t^{dN}, and the effect of the treatment, α_{dt}, such that

$$Y_t^d = Y_t^{dN} + \alpha_t^d G_t^d \quad (2)$$

$$Y_t^{dN} = \delta_t + \lambda_t \mu^d + \theta_t Z^d + \varepsilon_t^d \quad (3)$$

where δ_t is a time fixed effect, Z^d is a vector of time-invariant measured predictors with a time-varying coefficient vector θ_t, μ^d is the vector of time-invariant unobserved predictor variables with time-varying coefficients λ_t, G_t^d is an indicator variable that after T_0 takes the value of 1 for treated units and 0 otherwise, and ε_t^d is the unobserved transitory shocks with zero mean. Assuming a linear correlation between the outcome and the predictors, the SCM generalizes the DD method by allowing the effects λ_t of the unobserved predictors μ^d to vary over time, while the DD constrains these effects to be constant. Before the AGOA, the treatment-free potential outcome Y_t^{dN} is observed, for both the treated and control countries. For periods after T_0, the treatment-free counterfactual for the treated country, Y_{1t}^N is unobserved.

To estimate the treatment effect for post-intervention periods, the SCM estimates the unobserved Y_{1t}^N by generating a "synthetic control unit" weighted combination of potential controls that best approximates the relevant pre-intervention characteristics of the treated country. Weighting vector W is defined as $W = (w_2 \ldots w^{D+1})'$, where w^d is the contribution of each control country to the synthetic control unit, and the weights are constrained such that $w_d \geq 0$ and $w_2 + \cdots + w^{D+1} = 1$. The counterfactual estimator is constructed as the linear combination of the observed outcomes of potential control countries: $\hat{Y}_{1t}^N = \sum_{d=2}^{D+1} w^d \, Y_t^d$. The estimated treatment effect for the treated unit for each year after T_0 can then be obtained as $\hat{\alpha}_{1t} = Y_{1t} - \hat{Y}_{1t}^N$. Under the assumption that outcome is a linear function of both observed and unobserved potential confounders, if the weighted value of the observed covariates and pre-treatment outcomes for the control pool equals those of the treated country, then $\sum_{d=2}^{D+1} w^d \, Z^d = Z_1$ and $\sum_{d=2}^{D+1} w^d \, Y_t^d = Y_{1t}$, $t = 1, \ldots, T_0$, $\hat{\alpha}_{1t}$ is an approximately unbiased estimator of α_{1t} (Abadie et al., 2010). The vector W^*estimated to minimize the differences in the observed and unobserved confounders measured between the treated and synthetic control countries before the intervention. The difference is measured by the distance metric.

$$\sqrt{(X_1 - X_0 W)' V (X_1 - X_0 W)} \tag{4}$$

where X_1 is a $k \times 1$ vector including k covariates and pre-treatment outcomes for the treated country, X_0 is the corresponding $k \times D$ matrix of the control countries, and V is a $k \times k$ positive definite and diagonal matrix, which assigns weights according to the relative importance of the covariates and the pre-intervention outcomes. Matrices V and W are jointly chosen to minimize the root mean squared prediction error of the pre-intervention outcomes (Abadie et al., 2010). To account for the fact that there is more than one treated unit, we aggregate our treated observations into a single treated unit (Abadie et al., 2010).

In our setting, this means aggregating the outcome and covariates of the 16 AGOA countries to construct one treated country. However, this would result in insufficient power to detect whether there was a statistically significant treatment effect (Kreif et al., 2016), so we slightly alter the procedure described by Abadie et al. (2010) and follow Kreif et al. (2016) to construct the synthetic control country by directly averaging the 16 AGOA countries into a single treated unit. Let t be the year and i the country identifier such that $i = 1 \; to \; D_1$ countries are treated, while the remaining $D_1 + 1$ to $D_1 + D_2$ countries are controls. As the SCM setup, the observed outcome of a country can be written as $Y_{it} = Y_{1t}^N + \alpha_{it} G_{it}$. The aggregate outcome for the treated country can be defined as

$$\overline{Y_t} = \overline{Y_t}^N + \overline{\alpha_t} G_t \tag{5}$$

where $\overline{Y_t} = \frac{\sum_{i=1}^{D_1} Y_{it}}{n}$, $\overline{Y_t}^N = \frac{\sum_{i=1}^{D_1} Y_{it}{}^N}{n}$, $\overline{\alpha_t} = \frac{\sum_{i=1}^{D_1} \alpha_{it}}{n}$, G_t represents the treatment indicator in year t, and *n* is the number of countries in the AGOA program. The SCM with multiple treated units identifies the average treatment effect on the treated (ATT) (Xu, 2015). We calculate the ATT by averaging the estimated treatment effects $\overline{\alpha_t}$ over the post-treatment period, weighted by the time-fixed number of countries in the AGOA program. We ensure that our outcome and covariate variables are additive.

Statistical Significance of AGOA Estimated Effects

Suppose we seek to conduct inference regarding the positive AGOA effect on beneficiary countries' exports to the US for each of the sixteen post-AGOA years following the approach of Abadie et al. (2010) and Cavallo et al. (2013). In that case, we can determine the year-specific significance level (*p-value*) for the estimated trade agreement effect using the following approach:

$$p - value_t = P_r\left(\hat{\alpha}_{1,t}^{PL} < \hat{\alpha}_{1,t}\right) = \frac{\sum_{d=2}^{D+1} 1\left(\hat{\alpha}_{1,t}^{PL_d} < \hat{\alpha}_{1t}\right)}{\# \; of \; Controls} \tag{6}$$

where $\hat{\alpha}_{1,t}^{PL_d}$ represents the specific effect of AGOA for a given year, under the condition that a placebo AGOA is simultaneously assigned to control country d and treated country 1. In this scenario, the synthetic treatment effect is computed using the similar algorithm specified for $\hat{\alpha}_{1t}$. The process is repeated for each country d within the donor pool, aiming to construct the distribution of the synthetic experiment, and evaluate the placement of the estimate $\hat{\alpha}_{1,t}$ within that distribution. Ultimately, as our objective is to perform reliable inference on $\bar{\alpha}$, we calculate the year-specific *p-value* for the average effect at year t as,

$$p - value_t = P_r\left(D^{-1}\sum\nolimits_{d=1}^{D} \hat{\alpha}_{d,t}^{PL} < \bar{\alpha}_{1,t}\right) = P_r(\bar{\alpha}_t^{PL} < \bar{\alpha}_t)^{10} \tag{7}$$

Validity Tests: Synthetic Control Method

The study employed two validity tests for the SCM. Firstly, it utilized a measure developed by Adhikari and Alm (2016), the pre-treatment fit index, to assess if the donor pool resembled AGOA countries concerning their exports to the U.S. in the years before the agreement. Abadie et al. (2010) used the Root Mean Squared Prediction Error (RMSPE) of the outcome variable to conduct a similar assessment. This metric's flexibility in standardizing the RMSPE facilitated comparisons of fit quality across different outcome variables and countries (Adhikari and Alm, 2016). Given potential significant variations in exports to the U.S. among studied countries, this measure simplified evaluating the fit quality. Equation (8) was employed to calculate a pre-treatment fit, while a zero-fitted model determined the RMSPE in equation (9). Finally, equation (10) computed the pre-AGOA fit as the ratio between equations (8) and (9), allowing for an evaluation of how well the donor pool represented AGOA countries' export patterns before the agreement.

$$\mathrm{R}MSPE = \sqrt{\frac{1}{T_0}\sum_{1}^{T_0}\left(Y_{1t}\sum_{d=2}^{D+1} w_d^* Y_{dt}\right)^2} \tag{8}$$

[10] Additional information about this calculation method can be found in the work of Cavallo et al. (2013).

$$Benchmark\ RMSPE = \sqrt{\frac{1}{T_0}\sum_{1}^{T_0}(Y_{1t})^2} \tag{9}$$

$$Fit\ Index = \frac{RMSPE}{bench\ RMSPE} \tag{10}$$

RMSPE nearing zero implies perfect fit (fit index = 0). Fit index of 1 mirrors benchmark RMSPE[11]. Index > 1 implies significant divergence from counterfactual[12] in U.S. exports. Second, we conduct in-space placebo testing to establish if our findings can be attributed to a surge in exports to the U.S. due to AGOA. This testing, as described in Barlow et al. (2017), involves calculating RMSPE for both pre- and post-AGOA years. The RMSPE after AGOA is divided by the RMSPE before AGOA to determine an RMSPE ratio. This ratio for AGOA is then compared to that of the donor countries.

$$RMSPE\ Ratio = \frac{Post\ RMSPE}{Pre\ RMSPE} \tag{11}$$

Heterogeneity Analyses by AGOA Country

To examine heterogeneity in the effects of AGOA on export gains among beneficiary countries, we undertake two primary estimations. Firstly, we employ SCM to determine the impact of AGOA on each beneficiary country separately. Secondly, we use the fixed effect regression model to estimate the factors that determine export gains via AGOA.

[11] In this sense, the calculated value of [1 - Fit Index] provides similar information to the information provided by the R^2 statistic in regression analysis.

[12] We consider the pre-AGOA fit as appropriate if this index is smaller than or equivalent to “0.10”

Synthetic Control with a Single Treated Unit

We apply the SCM with a single treated unit approach to examine how the effect of AGOA on exports to the US differs by country.

Theoretical Model

Suppose we have data for $(D+1) \in \mathbb{N}$ countries during $T \in \mathbb{N}$ years and a treatment that affects only country 1 from year $T_0 + 1$ to year $\boldsymbol{T}$ with any intervention (see Ferman et al., 2020; Anderson, 2023). Let $Y_{d,t}^{0}$ be the probable outcome that would be observed for the country d in year t if there were no treatment for $d \in 1, \ldots, D+1$ and $t \in 1, \ldots, T$. Let $Y_{d,t}^{1}$ be the potential outcome under treatment. Define $\alpha_{d,t} = Y_{d,t}^{1} - Y_{d,t}^{0}$ to be the treatment effect and let $Y_{d,t}$ be the observed outcome. The aim of the SCM is to retrieve $\left(\alpha_1, T_0 + 1, \ldots, \alpha_{1,} T\right)$. Since $Y_{d,t}^{1}$ is observable for all $t > T_0$, the SCM needs to estimate of $Y_{d,t}^{0}$ to retrieve $\alpha_{1,t}$. Let $(T_0 \times 1)$ be a vector of observed outcomes for country $d \in (1, \ldots, D+1)$ before treatment specified as $\boldsymbol{Y}_d \equiv \left[Y_{d,1} \ldots Y_{d,T_0}\right]'$ and let $\boldsymbol{X}_d$ be the $(F \times 1)$ vector of predictors of $\boldsymbol{Y}_d$. Covariates that explain the outcome and linear combinations of the variables in $\boldsymbol{Y}_d$ can serve as predictors. Finally, let $\boldsymbol{Y}_0 = [\boldsymbol{Y}_0 \ldots \boldsymbol{Y}_{D+1}]$ be a $(T_0 \times D)$ matrix and $\boldsymbol{X}_0 = [\boldsymbol{X}_2 \ldots \boldsymbol{X}_{D+1}]$ a $(F \times D)$ matrix. Considering the matrices available predictors now in $\boldsymbol{X}_d$, a weighted average of the control countries is used to create the counterfactual for the treated country in the SCM, $\hat{Y}_{1,t}^{0} \equiv \sum_{d=2}^{D+1} \widehat{w}_d \, Y_{d,t}$. The weights $\widehat{\boldsymbol{W}} = [\widehat{w}_2 \ldots \widehat{w}_{D+1}]' \equiv \widehat{\boldsymbol{W}}\left(\widehat{\boldsymbol{V}}\right) \in \mathbb{R}^J$ are identified by resolving the nested minimization problem:

$$\widehat{\boldsymbol{W}}(\boldsymbol{V}) \equiv arg \underset{\boldsymbol{W} \in \mathcal{W}}{min} (\boldsymbol{X}_1 - \boldsymbol{X}_0 \boldsymbol{W})' \boldsymbol{V} (\boldsymbol{X}_1 - \boldsymbol{X}_0 \boldsymbol{W}), \tag{12}$$

$$\widehat{\boldsymbol{V}} \equiv arg \min_{\boldsymbol{V}} \left(\boldsymbol{Y}_1 - \boldsymbol{Y}_0 \widehat{\boldsymbol{W}}(\boldsymbol{V})\right)' \boldsymbol{V} \left(\boldsymbol{Y}_1 - \boldsymbol{Y}_0 \widehat{\boldsymbol{W}}(\boldsymbol{V})\right) \tag{13}$$

where $\mathcal{W} \equiv \{\boldsymbol{W} = [w_2 \ldots w_{D+1}]' \in \mathbb{R}^D : w_d \geq 0 \; for \; each \; d \in \{2, \ldots, D+1\} \; and \; \sum_{d=2}^{D+1} w_d = 1\}$ and $\boldsymbol{V}$ is a diagonal (positive semidefinite) $(F \times F)$ matrix. Loosely, $\widehat{\boldsymbol{W}}$ Is a weight vector that quantifies the relative importance of each country within the synthetic control for country 1, while $\widehat{\boldsymbol{V}}$ measures the relative importance of each of the F predictors. The synthetic control estimator of $\alpha_{1,t}$ is then obtained as $\hat{\alpha}_{1,t} = Y_{1,t}^1 - \hat{Y}_{1,t}^0$ for each $t \in (T_0 + 1, \ldots, T)$, where $\hat{Y}_{1,t}^0$ is constructed using the weights $\widehat{\boldsymbol{W}}(\boldsymbol{V})$.[13,14]

Empirical Model

We take a sample of $D+1$ countries, that is indexed by d, and where $d = 1,2, \ldots, D+1$. We assume a single treated country (i.e., an AGOA beneficiary country) expressed as $d = 1$ and a "donor pool" expressed as $d = 2, \ldots, D+1$. Abadie et al. (2015) indicates donor pool should comprise countries with analogous "structural processes" as AGOA beneficiary countries, unaffected by "structural shocks' during the years considered. Also, balanced panel data is used where all the countries are observed at the same time periods $t = 1,2, \ldots, T$[15], and pre-AGOA periods are expressed as $1, \ldots, T_0$. Furthermore, post-AGOA periods T_1 are represented as $T_0 + 1, \ldots, T$. Country $d = 1$ is subject to AGOA at years $T_0 + 1, \ldots, T$, and pre-AGOA years $1, \ldots, T_0$.

For every country d, an outcome of interest Y_{dt} can be observed as well as a set of predictors p with the outcomes, $X_{1d}, \ldots X_{pd}$, which are not affected by the AGOA and may include pre-AGOA values of Y_{dt}. Let X_1 be an $p \times 1$ vector that contain variables of the characteristics of the beneficiary country, and X_0 be an $p \times D$ matrix consisting of the same variables for the donor pool. For every country d and year t, in the absence of AGOA, a possible outcome is given as Y_{dt}^N.

[13] Note as $w_j \geq 0$ and $\sum_{j=2}^{J+1} w_j = 1$, no country receives a negative weight. This means that extrapolation is ruled out.
[14] We solved the nested minimization problem in Equations 12 and 13 using the s*ynth* by Abadie (2011) in Stata.
[15] T is a total of 23 years (periods) from 1993 – 2015 for our study.

The possible outcome in the presence of AGOA, $t > T_0$ for country $d = 1$ (i.e., and AGOA exposed country) is expressed as Y_{1t}^{I}. The effect of AGOA for the beneficiary country in year $t > T_0$ is denoted as:

$$\alpha_{it} = Y_{1t}^{I} - Y_{1t}^{N} \tag{14}$$

From (14), estimating the impact of AGOA is tantamount to the difficulty in estimating Y_{1t}^{N} for $t > T_0$, which shows how the AGOA beneficiary country would have been in the absence of the AGOA. Equation (10) gives room for a change in time regarding AGOA effect since the implementation of the policy may not have instantaneous impact. Synthetic control is expressed as a weighted average of the "donor pool" countries. It can be expressed by a $D \times 1$ vectors of weights $W = (w_2, .., w_{D+1})'$, where $0 \leq w_d \leq 1$ for $d = 2, \ldots, D + 1$ and $w_2 + \ldots + w_{D+1} = 1$. Each value of the vector W reflects a potential synthetic control. Abadie et al. (2015) show that the value of W can be chosen such that the characteristic of the synthetic control best resembles those of the treated AGOA country. For a set of weights W, the synthetic control estimator of Y_{1t}^{N} is expressed as:

$$\hat{Y}_{1t}^{N} = \sum_{d=2}^{D+1} w_d \, Y_{dt} \tag{15}$$

and the synthetic control estimator of α_{it} is expressed as:

$$\hat{\alpha}_{1t} = Y_{1t} - \hat{Y}_{1t}^{N} \tag{16}$$

All weights constrained to be nonnegative to avoid "extrapolation", and not above one, so equation (14) can be written as:

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{d=2}^{D+1} w_d \, Y_{dt} \tag{17}$$

If only one country, c in the donor pool is employed as a synthetic control, then $w_c = 1, w_d = 0$ $\in_d \neq c$, and

$$\hat{\alpha}_{1t} = Y_{1t} - Y_{ct} \tag{18}$$

For "adjacent neighbor" estimators, c is the index value that minimizes $\|X_1 - X_d\|$ over d for some norm $\|.\|$. The pre-AGOA characteristics in X_1 and X_0 may include pre-AGOA values of the outcome variable. The variation between the pre-AGOA characteristics of the beneficiary country and the synthetic control is given by the vector $X_1 - X_0W$. The value of W is chosen such that it lessens the disparity between the pre-AGOA characteristics of the AGOA beneficiary country and the synthetic control. According to Abadie et al. (2010), and Abadie and Gardeazabal (2003), $W^* = (w_2^*, .., w_{D+1}^*)'$ is selected as the value of W that minimalizes:

$$\|X_1 - X_0W\| = \left[\sum_{k=1}^{p} q_k\,(X_{1k} - X_{0k}W)^2\right]^{1/2} \tag{19}$$

where q_k denotes weight reflecting the relative importance assigned to the k^{th} variable when $X_1 - X_0W$ is measured. The matching variables in X_1 and X_0 are meant to predict the post-AGOA outcomes. Large weights q_k be assigned to the values of the variables for the synthetic controls to closely replicate variable values for the AGOA beneficiary country. To measure the effect of AGOA, let Y_{it} be the outcome of country d at time t, Y_1 be a $T_1 \times 1$ vector collecting the post-AGOA values of the outcome for the AGOA beneficial country. The synthetic control estimator of the effect of AGOA is given by the comparison between the outcome for the beneficiary country and the outcome for the synthetic control at that period:

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{d=2}^{J+1} w_d^*\, Y_{dt} \tag{20}$$

The challenge is how to choose q_k. A simple selector of q_k is the inverse of the variance of X_{1k} and X_{0k}, which rescales all rows of $[X_1 : X_0]$ to have unit variance. We selected q_k that minimizes the RMSPE of the synthetic control with respect to Y_{1t}^{N} following closely Abadie et al. (2010), and Abadie and Gardeazabal (2003).

$$\sum_{t\in T_0}\left(Y_{1t-}w_2(q_1)Y_{2t}+\cdots+w_{d+1}(q_p)Y_{D+1t}\right)^2 \tag{21}$$

for some set $T_0 \subseteq \{1,2,\dots,T_0\}$ of years before AGOA.

Estimating the Determinants of Trade Gains through AGOA

Once we have estimated the effect of AGOA on the export gains of member countries, we employ fixed effect regression to identify the country-specific characteristics that account for differences in export gains due to AGOA. The variables we examine are drawn from prior research and encompass institutional quality, infrastructure, macroeconomic factors, and banking efficiency. Equation (22) employs the fixed effects approach to estimate the trade gain between countries (Tadesse and Fayissa, 2008). The data was restricted between 2001 to 2015 to capture indicators after AGOA implementation. The dependent variable, $\hat{\alpha}_{1,t}$, is the estimated change in exports to the U.S. due to the AGOA, which we estimated using the SCM with a single treated unit method from the previous section.

$$\hat{\alpha}_{1,t}=Y_{1,t}^{1}-\sum_{d=2}^{D+1}\widehat{w}_d\,Y_{d,t}=\beta_0+\sum_{k=20}^{K}\beta^k\,lnZ_t^{dk}+v_t+\eta_d+\xi_t^d \tag{22}$$

In our model, Z_t^{dk} is a vector of k explanatory variables for country d at time t, v_t represents the time fixed effect, η_d represents the country-level fixed effect, and ξ_t^d is the random error term.

The explanatory variables in this model were carefully selected through an empirical literature review. Several studies have looked at trade determinants among developing countries, and some argue that external trade barriers are the main determinants of trade flows in such economies. This implies that exports from SSA to the U.S. can be constrained by internal factors even in the presence of a PTA such as AGOA. Low trade performance among SSA countries has been attributed to poor infrastructure, especially transport, customs, and regulatory environments (Portugal-Perez and Wilson, 2012; Limao and Venables, 2001; Wilson et al. 2005). Physical infrastructure, soundness of the macroeconomic framework, quality of institutions, and property rights are major determinants of export performance (UNCTAD, 2005; Krenz, 2016; Tsukanova, 2019).

Other work has shown that the impact of physical infrastructure on trade is much higher than physical skill labor, capital, and contract enforcement (Anderson and Marcouiller, 2002; Nunn 2007; Francois and Manchin 2013). Among SSA economies, issues of national security and vulnerability because of conflict are known to significantly impact trade performance, because insecurity acts as a hidden tax on trade (Anderson and Marcouiller 2002). A strong macroeconomic environment coupled with competitive and stable exchange rates, stable prices, and low levels of debt boost investment and export capacity. According to Rodrik (2008), when poorly managed exchange rates hinder investment and export potential. Aidt and Tzannatos (2008) also show that how a country packages its formal and informal labor market and wage bargaining institutions can attract investment and boost exports. Caution must be exercised in considering estimation as a robust causal mechanism because most of the factors have some level of correlation and are likely endogenous. However, because of the similarities among AGOA countries, any significant difference in the factors could help understand the heterogeneity in using the export opportunities AGOA and other programs provide (for example, Kassa and Coulibaly, 2018).

Robustness check: Difference-in-Differences

To assess robustness of our result, the DD method was applied. The DD is a quasi-experimental research design that is used to evaluate the impact of a program or policy. A DD estimate is the difference in outcomes before and after a treatment (difference one) in a treatment group versus a control group (difference two). We adopted a DD model following Wooldridge (2021):

$$\boldsymbol{y}_{st}^{d} = \eta_s + \gamma_t + \boldsymbol{Z}_{st}^{d}\beta + Q_{st}\delta + \varepsilon_{st}^{d} \tag{23}$$

where $\boldsymbol{y}_{st}^{d}$ be the outcome of country d, in country group s, at time t , $d = 1, \dots, N$, $t = 1, \dots., T$, and $s = 1, \dots, S$. η_s are country group fixed effects, γ_t are time fixed effects, $\boldsymbol{Z}_{st}^{d}$ are covariates, Q_{st} is the time-varying treatment indicator (1 if country i in s is treated and 0 otherwise), and ε_{st}^{d} is the residual errors, δ is the coefficient on the treatment indicator, i.e. the ATET. Let us rewrite (eq. 23) as

$$\boldsymbol{y}_{st}^{d} = \eta_s + \gamma_t + \boldsymbol{Z}_{st}^{d}\beta + Q_{st}\delta + \varepsilon_{st}^{d} = \boldsymbol{y}_{st}^{d} = DD_{dst} + \varepsilon_{st}^{d} \tag{24}$$

The Linear-Trends Model

The linear-trend model augments the eq.24 with two more terms following Shahid et al. (2022):

$$\boldsymbol{y}_{st}^{d} = DD_{dst} + w_d\kappa_{t,0}t\psi_1 + w_d\kappa_{t,1}t\psi_2 + \epsilon_{st}^{d} \tag{25}$$

The add-on terms are made up of two three-way interactions between $\kappa_{t,0}$, w_d, and t, and $\kappa_{t,1}$, w_d, and t . $\kappa_{t,0} = 1(\kappa_t = 0)$ is a variable that represents before treatment time periods, $\kappa_{t,1} = 1(d_t = 1)$ indicate posttreatment time periods, and w_d is variable that is 1 if a country is ever treated, and 0 if never treated. ψ_1 captures the differences in slopes between treatment and control groups before the treatment is applied, and ψ_2 captures post-treatment slope differences. If $\psi_1 = 0$, the outcome shows parallel linear trends before the treatment is administered. We used a Wald test of ψ_1 compared to zero to evaluate if linear trends are parallel before treatment. We produce two parallel trend plots: first shows observed group means over time; second, based on model (25) used for parallel trend test, centers continuous time variable around its minimum.

$$\boldsymbol{y}_{st}^{d} = DD_{dst} + \omega_d \kappa_{t,0}\{t - \min(t)\}\psi_1 + \omega_d \kappa_{t,1}\{t - \min(t)\}\psi_2 + \mu_{st}^{d} \tag{26}$$

Using the minimum time value establishes a shared starting point, aiding detection of deviations from parallelism. The graph then portrays predicted values from this model, evaluated at all time points for each treatment group and average covariate values.

The Granger Model

We used the Granger-type tests to check if treatment effects appear pre-treatment following studies such as Shahid et al. (2022). It enhances DD model with counterfactual treatment-time indicators, known as leads in DD literature. If assume that the treatment took place at time $t = \nu$, then Q_{st} is expressed as $Q_{st} = 1(t \geq \nu)w_d$. If the treatment occurred at time $\nu - 1$, we could construct a new treatment as $1(t_{d\nu} \geq \nu - 1)w_d$. With sufficient time points, we could construct another counterfactual treatment as $1(t_{d\nu} \geq \nu - 2)w_d$ and so on. We augment that DD model with leads (leaving out one for identification purposes). Let V index the time at which the treatment occurs.

$$\boldsymbol{y}_{st}^{d} = DD_{dst} + \sum_{\nu=2}^{V-1} \mathbf{1}(t_{d\nu} \geq d)w_d\,\lambda_\nu + \varphi_{st}^{d} \tag{27}$$

The outcome of the test is acquired through a combined Wald test conducted on the coefficients.λ_ν (H_0: no anticipatory effect).

Event-Study Analysis

In addition to the DD analysis described above, we conducted a panel-event study, with the “event” being the date of implementation of the AGOA following De Giorgi et al. (2022). We estimated the following equation:

$$Y_{dt} = \alpha_1 + \sum_{j=2}^{J} \delta_j (Lag\ j)_{dt} + \sum_{k=2}^{K} \lambda_j (Lead\ k)_{dt} + \mu_t + \psi_d + \varepsilon_{dt} \tag{28}$$

where ψ_d and μ_t are binary variables for country and year, and ε_{dt} is unobserved error term. Further, Lag_j and $Lead_k$ are two binary variables indicating the number of years until implementation of the AGOA in country d. Formally, we defined Lag_j and $Lead_k$ according to Equations (29) – (32)

$$(Lag\ j)_{dt} = 1[t \leq Event_d - J], \tag{29}$$

$$(Lag\ j)_{dt} = 1[t = Event_d - j]\ for\ j \in \{1, \dots, J-1\}, \tag{30}$$

$$(Lead\ k)_{dt} = 1[t = Event_d + k]\ for\ k \in \{1, \dots, K-1\}, \tag{31}$$

$$(Lead\ k)_{dt} = 1[t \geq Event_d + K]. \tag{32}$$

where, $Event_d$ is a variable indicating the year t in which the AGOA was implemented in country d. The first Lag was omitted to capture the baseline difference between treated and control countries.

Data

The AGOA trade agreement comprises the SSA countries, presented in Table 1.1. However, some of the countries have not been in the program continuously or joined on different dates. We restrict our analysis to sixteen AGOA countries that joined the program at the end of the year 2000 and have since remained in the program; hence, they have the same treatment period. North African, Latin American, Caribbean, and Asian countries are used as the "donor pool" due to their similarities with the AGOA countries in terms of income, level of development, trade potential, and population dynamics, and similar pre-treatment exports to the U.S.

Our study uses U.S-AGOA data from 1993 to 2015, which captures seven years pre-treatment and fifteen years post-treatment. The trade data come from the U.S. International Trade

Commission. The dependent variable is the annual total product export (in millions of dollars) from each of the sixteen AGOA countries to the U.S. We also obtained 6-digit Harmonized System (HS) classifications from BACI and then grouped the HS6 categories into broader HS2 categories for HS level heterogeneity analysis. Our particular focus was on agricultural, textiles and apparel, and mineral exports. The data used for the estimation was limited to the period from 1995 to 2015. Macroeconomic data come from World Bank's World Development Indicator (WDI), political corruption data from Transparency International, economic freedom data from The Heritage Foundation's Center, and institutional governance data from World Governance Indicators (WGI). We obtain country eligibility data from the U.S. Government Accountability Office and the International Trade Administration within the U.S. Department of Commerce. Figure 1.1 presents the average annual exports (in million US$) to the U.S. by AGOA countries from 1993 to 2015. The Congo Republic, Nigeria, and South Africa have had the highest exports to the U.S. during this period, and Mauritius has the lowest. We observe that AGOA countries in our sample have experienced growth in their exports to the U.S. since the intervention.

Table 1.2 defines the variables used in the analysis and provides summary statistics. Exports to the U.S. average US$5.25 billion per country annually, with substantial variation across countries. Figure 1.2 shows the export and import trajectories from AGOA SSA countries to the U.S., which vary among the beneficiary countries. Each AGOA country has seen an increase in exports to the U.S.

We follow three fundamental principles when using SCM. First, we utilize balanced panel data. Second, we ensure that each country has data on the outcome variable for all the years under consideration. Third, we make sure that every variable used in the study has at least one observable observation during the pre-AGOA years. These criteria guide our selection of the outcome variable, control nations, and the number of years for the study. For the research, we chose the year 1993 as the first pre-AGOA year, followed by sixteen post-AGOA years. This allows us to analyze the impact of the AGOA on beneficiary exports from United States. With a data span from 1993 to 2015, we have a minimum of seven pre-AGOA years to calculate the pre-intervention effect. Inclusion in our donor pool requires that countries have at least one pre-AGOA observation for all the explanatory variables analyzed. Any country not meeting this condition is excluded. Additionally, we ensure that no donor country has a program like the AGOA.

This step is necessary to match the trajectory of the outcome variable between the treated unit and the donor pool. Including donor countries with similar interventions would hinder our ability to compare outcomes accurately. To evaluate the average treatment effect of AGOA, we carefully select countries with similar trade and economic characteristics as the AGOA countries and a strong fit in the pre-treatment period. This approach avoids biases that might arise from comparing countries with vastly different features and allows us to capture important but unobservable economic and development factors. As part of the evaluation process, we compute placebo treatment effects for the donor countries and compare them to the actual treatment effect in the AGOA countries. This methodology is based on the work of Abadie et al. (2010).

Results and Discussion

This section begins by presenting the findings of the SCM technique that utilizes multiple treated units to gain insights into the overall impact of AGOA. Subsequently, we employ SCM with a single treated unit to explore the heterogeneous impact of the program and investigate the factors that influence the export of AGOA members to the U.S. using the fixed effects model.

SCM with Multiple Treated Units

Figure 1.3 shows the cumulative export trajectories of the sixteen AGOA countries and their synthetic counterfactual between 1993 and 2015. The solid line represents the average exports from AGOA countries to the U.S. The dashed line shows export trajectories of the synthetic AGOA country, depicting the estimated average exports the countries would have experienced in the absence of the AGOA program. There was little difference in exports to the U.S. between the synthetic AGOA and the AGOA countries before the implementation of the program. Thus, synthetic countries closely replicate the export trajectories of actual exports to the U.S. prior to the intervention. Although the SCM method does not require a parallel trend assumption, the similar trajectory between the two lines provides the rationale for using the SCM method in this study. Our estimate of the treatment effect shows that exports to the U.S. for the sixteen AGOA countries are higher than they would have been without the AGOA. The deep decline in export trajectories between 2008 to 2009 is somewhat attributed to the financial crisis as well as the decline in commodity prices. Table 1.3 also presents the estimated synthetic weight from the SCM analysis. Table 1.3 shows that Algeria, Haiti, Jamaica, and Papua New Guinea were the four countries with positive weights among the "donor pool".

In Table 1.4 and Figure 1.4, we present a comparison of predictor means between actual AGOA and synthetic AGOA and population-weighted averages of the fourteen countries in the donor pool. From Table 1.4, we observe that the weighted average of predictors of the fourteen countries that did not participate in AGOA from 2000 to 2015 does not seem to match the weighted averages of the actual AGOA countries. Thus, they would not be a suitable control group for the AGOA countries. Particularly, Figure 1.4 shows that exports to the U.S. prior to the AGOA implementation were higher on average for the fourteen untreated countries than for AGOA participants and the synthetic AGOA. We can see that the synthetic AGOA perfectly matches the actual AGOA in the pre-treatment years. Further, Table 1.4 shows that the weighted averages of actual and synthetic AGOA variables are similar. The closer the weighted average of each variable is, the more appropriate the variable is for constructing the counterfactual. Therefore, we could be certain that the synthetic AGOA would closely match the actual AGOA. The synthetic AGOA is thus a better counterfactual than the fourteen untreated units (i.e., donor pool).

Table 1.5 also compares exports of the actual AGOA and synthetic AGOA. We observe that the AGOA program increases exports of the treated group by about 42% compared to their untreated counterparts, all else equal. This implies that AGOA nations' exports were 42% higher than they would have been in the absence of participating in the program, which represents an export value of US$818 million.[16]

Figure 1.5 shows the probability values of the average treatment effect occurring by chance. We observe that other than the last two years of the intervention, all years after the intervention have *p-values* that are statistically significant. We therefore conclude that our estimation of the effect of the AGOA on exports to the U.S. did not happen by chance and is due to the policy.

Synthetic Control Method: Validity Test Results

Our study produced a fit index of 0.01 for export value, which means that the difference between the treated and synthetic units in each pre-AGOA period for exports was only one percent (as demonstrated in Table 1.6). This finding suggests that the donor pool group we selected matches the treated unit extremely well during the pre-AGOA period, providing a solid basis for comparing the two groups. We also conducted an "in-space" placebo test to determine whether our findings

[16] This value represents the average export difference between AGOA beneficiaries and the counterfactual across post-treatment years.

are attributable to the impact of AGOA. To compute the RMSPE ratio, we divided the post-RMSPE by the pre-RMSPE, as shown in Figure 1.6. We began with an “in-space” placebo study by comparing the estimated effect of AGOA to a placebo effect obtained by repeatedly assigning AGOA to countries that did not actually implement it and then estimating the model for each country. Based on Figure 1.6, we see that AGOA has the second highest RMSPE ratio, indicating a clear distinction between exports to the U.S. by AGOA countries before and after the implementation of AGOA.

Heterogeneous AGOA Effects by Country

This section focuses on the heterogeneity estimations, where we present two sets of results. Firstly, we will discuss the outcomes of the SCM technique with a single treated unit, and secondly, we will present the findings from the fixed effect model.

SCM with Single Treated Unit Results

We use the SCM to estimate the effect of the AGOA on the individual beneficiary countries' exports to the U.S. to understand how the impact differs across AGOA members. Our results suggest that AGOA has had varying impact on the sixteen studied countries (Table 1.7 and Figure 1.7). Figure 1.7 shows export trends for each of the sixteen countries that are eligible for AGOA and have the same treatment year (2000) and their synthetic counterfactual for the period 1993 to 2015 as estimated using SCM. The solid red line represents the observed trends of AGOA beneficiaries export to the U.S., while the dashed blue line represents export trends of the synthetic countries, or the estimated aggregate value of exports a country would have attained had it not joined the AGOA. The vertical broken line indicates the year of eligibility for AGOA. In most cases, the synthetic country closely reproduces the export trend of actual exports before the intervention.

We group the sixteen countries into three classes based on how much they gained or lost through AGOA as a percentage of average post-AGOA GDP: (1) countries that gained more than one percent of their average post-AGOA GDP, (2) countries that gained or lost less than one percent of their average post-AGOA GDP, and (3) countries that lost more than one percent of their average post-AGOA GDP. Four countries (Congo, Lesotho, Nigeria, and South Africa) registered significant trade gains through the AGOA. Nigeria led the way with an increase in the value of its exports totaling US$9.55 billion annually, corresponding to 3.31% of its GDP. Congo led the way in export gains as a percentage of GDP, with a 9.94% increase or US$856 million annually. Most of the gains for Nigeria and Congo DR are due to increases in exports of petroleum and minerals.

Six countries (Tanzania, Mozambique, Namibia, Cameroon, Botswana, Kenya) experienced relatively minor gains in exports to the U.S. from participating in the AGOA. These countries saw increases ranging from US$5.46 million to US$188.07 million, reflecting 0.02% to 0.56% of their GDP. Four countries (Rwanda, Zambia, Ghana, Uganda) saw moderate decreases following the implementation of the AGOA, ranging from US$26.87 million to US$117.56 annually. These losses correspond to an average annual GDP reduction of 0.16% to 0.62%. The majority of these SSA countries export similar commodities.

Two countries (Malawi and Mauritius) experienced significant losses due to the AGOA. Malawi and Mauritius have lost US$140.45 million and US$224.52 million in exports to the U.S. annually, corresponding to 2.95% and 2.62% of GDP, respectively. The variations in the impact can be attributed to the individual beneficiary country's trade and other economic characteristics. For example, Mauritius experienced rapid growth in garment production in the early 1980s, but garment exports have declined since the early 2000s due to a labor shortage and wage increases in the small island economy, which hampered the expansion of domestic garment production (Mattoo et al.,2003). Garment companies had a significant incentive to locate labor-intensive garment production in a low-wage country like Madagascar, lowering domestic garment production. Thus, the negative impact of AGOA may be related in part to the fact that the Mauritian economy was transitioning from labor-intensive manufacturing to service industries during the 2000s.

Determinants of Export Changes Due to AGOA

Table 1.8 presents the estimation results of equation (20), providing the macroeconomic indicators, institutional quality variables, and infrastructure indicators that explain the differences across countries in exports to the U.S. through the AGOA program. The role of sound macroeconomic indicators captured as income (GDP), consumer prices (inflation), exchange rate, external debt, government spending, and bank efficiency have statistically significant influences on export performance related to AGOA across different model specifications agreeing with UNCTAD (2005).

Our findings show that SSA countries with higher levels of physical infrastructure (captured as mobile subscriptions and access to telecommunication) had higher exports to the U.S. This result agrees with what was found in previous studies (Wilson et al., 2005; Portugal-Perez and Wilson, 2012). Countries with higher labor freedom had higher exports to the U.S., confirming Aidt and Tzannatos (2008) and Kassa and Coulibaly (2018). This implies that having stricter labor market regulations imposes a cost by reducing opportunities to expand export capacity. Sub-Saharan African countries with more financial freedom had higher exports to the U.S., which suggests that less government control in the financial sector is essential in expanding export capacity, likely because it allows countries to take advantage of the preferential access granted to them through the AGOA. Like Kassa and Coulibaly (2018), we find that countries that better define property rights had higher exports to the U.S. The result contradicts Yang and Woo (2006) who suggest that strengthening intellectual property rights does not encourage more agricultural trade. SSA countries with strong regulatory institutions and effective governance had higher exports to the U.S., agreeing with Krenz (2016) and Tsukanova (2019). Political corruption and political stability had negative impacts on export gains to the U.S. due to the AGOA, supporting findings of Krenz (2016) and Tsukanova (2019). We did not observe a significant relationship between regime durability and exports.

Several macroeconomic measures also correlate with trade gains through the AGOA. As hypothesized, countries with a higher GDP have greater exports to the U.S. (Michelis and Zestos, 2014). The SSA countries with weaker currencies against the U.S. dollar had lower exports to the U.S., supporting findings of Auboin and Ruta (2011). Government spending also negatively impacted exports, suggesting that high government expenditures can burden export opportunities. Lastly, SSA countries with higher inflation and higher external debt had lower exports to the U.S., agreeing with Stockman (1985).

Because most of the export gains through AGOA has been attributed to petroleum and agricultural commodities (see Kassa and Coulibaly 2018), we include share of exports that are agricultural and share of exports that are oil as covariates. We find that export gains due to the AGOA decrease when agricultural exports form a larger share of the country's exports, while export gains increase as oil exports make up a larger share of their exports to the U.S. This result supports our finding in Table 1.7 that three high-petroleum exporting countries – Nigeria, South Africa, and Congo DR – are the largest beneficiaries from the AGOA.

Heterogeneity by HS Product

In this section, we examine whether the impact of AGOA differs across different HS2 product levels. The HS2 product levels were divided into agricultural, textiles and apparel, and mineral products. We focused on these commodities since they have been shown to dominate the products exported to the U.S. under the AGOA.

Figure 1.8 shows the cumulative agricultural, textiles and apparel, and mineral exports trajectories of the sixteen AGOA countries and their synthetic counterfactual between 1995 and 2015. The solid red line represents the average exports from AGOA countries to the U.S. The dashed blue line shows export trajectories of the synthetic AGOA country, depicting the estimated average exports the countries would have experienced in the absence of the AGOA program. We observe that the AGOA program increases agricultural exports of AGOA countries to the U.S. by about 42% (US$20.14 million) compared to their untreated counterparts, all else equal. We find mineral exports to the U.S. by the AGOA countries increased on average by 15% (US$540.2 million) through AGOA while the textiles and apparel exports increased by 52% (US$32.9 million). The findings are comparable with those of Fernandes et al. (2023), who found that AGOA had a favorable influence on African countries' apparel exports.

Robustness Checks: Difference-in-Differences Result

As a robustness check, we use DD to compare the exports from the untreated countries (Haiti, Jamaica, and Papua New Guinea) that contributed most to the weights in the donor pool of our SCM analysis with the countries in the AGOA program. Using the major countries in the donor pool addresses the criticisms of the regular DD approach in that an analysis that compares treated and control countries that are not similar may impose bias (Moyo et al., 2018). An additional advantage of this approach is its ability to account for unobserved heterogeneity (Khandker et al., 2010).

We performed two DD estimations. We estimated equation (21) without and with covariates. Figures 1.9 and 1.10 show parallel trends as well as Granger-type causality tests. Figure 9 depicts the average export trajectory of the sixteen AGOA-eligible countries and their control counterparts from 1993 to 2015. The red solid line depicts the average export trends of AGOA-eligible country's exports to the U.S., while the blue solid line depicts the export trajectories of the control countries, which indicate the counterfactual trajectory for AGOA countries had they not participated in the AGOA program. It demonstrates that prior to the introduction of the AGOA, exports to the US from the counterfactual and AGOA countries are similar, confirming that the "parallel trend assumption" is satisfied. In the absence of the AGOA, the assumption requires that the difference between the "treatment" and "control" groups remain stable over time.

Figure 1.9 is also consistent with Table 1.10, which reveals that the p-value of the F-static is not statistically significant, indicating a refusal to reject the null hypothesis of a parallel linear trend. The ATET of the DD without covariate is US$1252.67 million, which is greater than our finding from the SCM with multiple treated units (US$ 818.11 million) and the average of the individual beneficiary SCM estimates (US$ 845.36 million), according to Table 1.9. As a result, the DD without covariate might overestimate the impact of AGOA on beneficiaries' exports to the US. Using the Granger-type causality test, Figure 1.10 and Table 1.10 reveal that there is no effect in anticipation of Treatment.

We also estimated the DD with covariates. Figures 1.11 and 1.12 depict the parallel trend and the Granger-type causality tests, respectively. In Figure 1.11, The red solid line depicts the average export trends of AGOA-eligible country's exports to the U.S., while the blue solid line depicts export trajectories of control countries, which indicate the counterfactual trajectory for AGOA countries had they not participated in the AGOA program. It demonstrates that prior to the implementation of the AGOA, exports to the US from the counterfactual and AGOA countries are identical, indicating that the "parallel trend assumption" is satisfied. The assumption requires that the difference between the treatment and control group be constant over time, in the absence of the AGOA. Table 10 reveals that the p-value for the parallel trend test is not statistically significant, indicating a refusal to reject the null hypothesis. Figure 1.12 and Table 1.10 also show that there is no effect in anticipation of Treatment using the Granger-type causality test. The ATET of the DD with covariates is US$852.61 million, which is consistent with our findings from the SCM with multiple treated units (US$818.11 million) and the average of the individual beneficiary SCM estimates (US$ 845.36 million). As a result, the impact of AGOA on the beneficiary countries to the US is estimated to be around US$800 million per year. Figure 1.13 shows the findings of the event-study approach as a supplement to the DD technique. We found that the AGOA had a significant effect on the beneficiary countries' exports to the US.

Conclusion

In this study, we evaluate the effect of the AGOA on exports from member nations to the U.S. using the SCM. The results show that, on average, the AGOA has had a positive impact on export trajectories from SSA to the U.S., with AGOA member nations exporting products worth US$818.11 million more in value than we estimate that they would have in the absence of AGOA membership. This represents an export increase of 42% above what they would have exported to the U.S. without the program.

We find a substantial variation of AGOA impacts among beneficiary countries. While some countries gained from AGOA, others lost. A significant gain in exports was registered by four countries (Nigeria, South Africa, Congo DR, Lesotho) with two countries (Mauritius and Malawi) experiencing substantial losses. While our empirical results suggest that preferential trade agreements such as AGOA have the potential to increase exports in aggregate, which could positively impact economic growth, there is heterogeneity across countries. The differences in the total exports among AGOA members are largely explained by macroeconomic indicators such as exchange rate, inflation, government spending, income, ICT infrastructure, institutional quality, and labor market regulations. Our results inform future trade policy by showing how certain trade agreements can benefit or harm participating nations. This research also offers policy directions in analyzing which countries are likely to benefit from the AGOA and similar preferential trade agreements. We also find that the AGOA program increases agricultural exports of AGOA countries to the U.S. by about 42% (US$20.14 million) compared to their untreated counterparts, all else equal. We find mineral exports to the U.S. by the AGOA countries to be increasing on average by 15% (US$540.2 million) through AGOA while the textiles and apparel commodity export is increasing by 52% (US$32.9 million).

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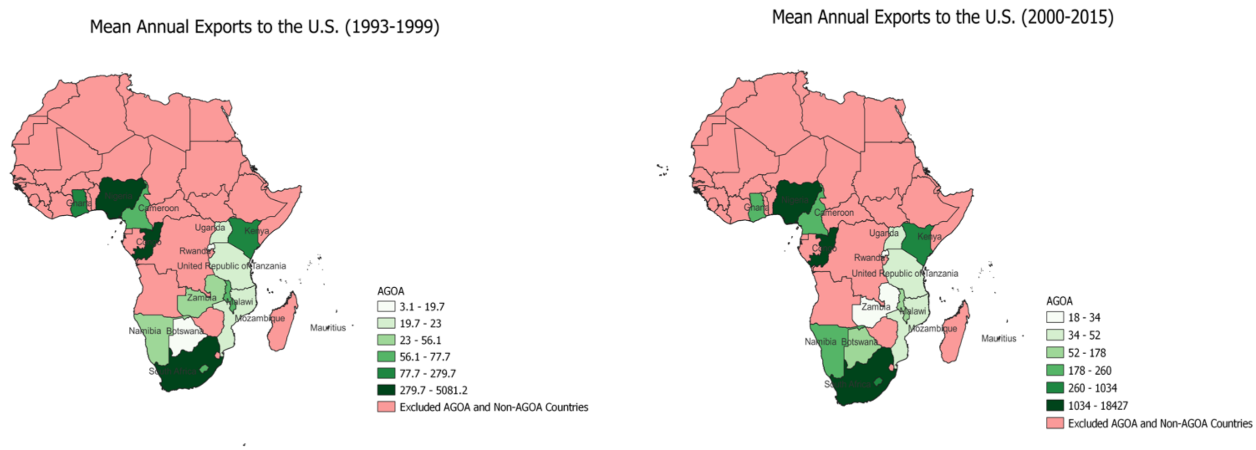
Tables and Figures

Figure 1.1: Exports from AGOA Countries to the U.S. (1993─2015)



Note: Average annual export value in millions of $ (million at constant 2000) to the United States before and after AGOA implementation. Note that these countries were eligible towards the end of 2000.

Figure 1.2: Export and Import Trends, SSA (1993-2015)

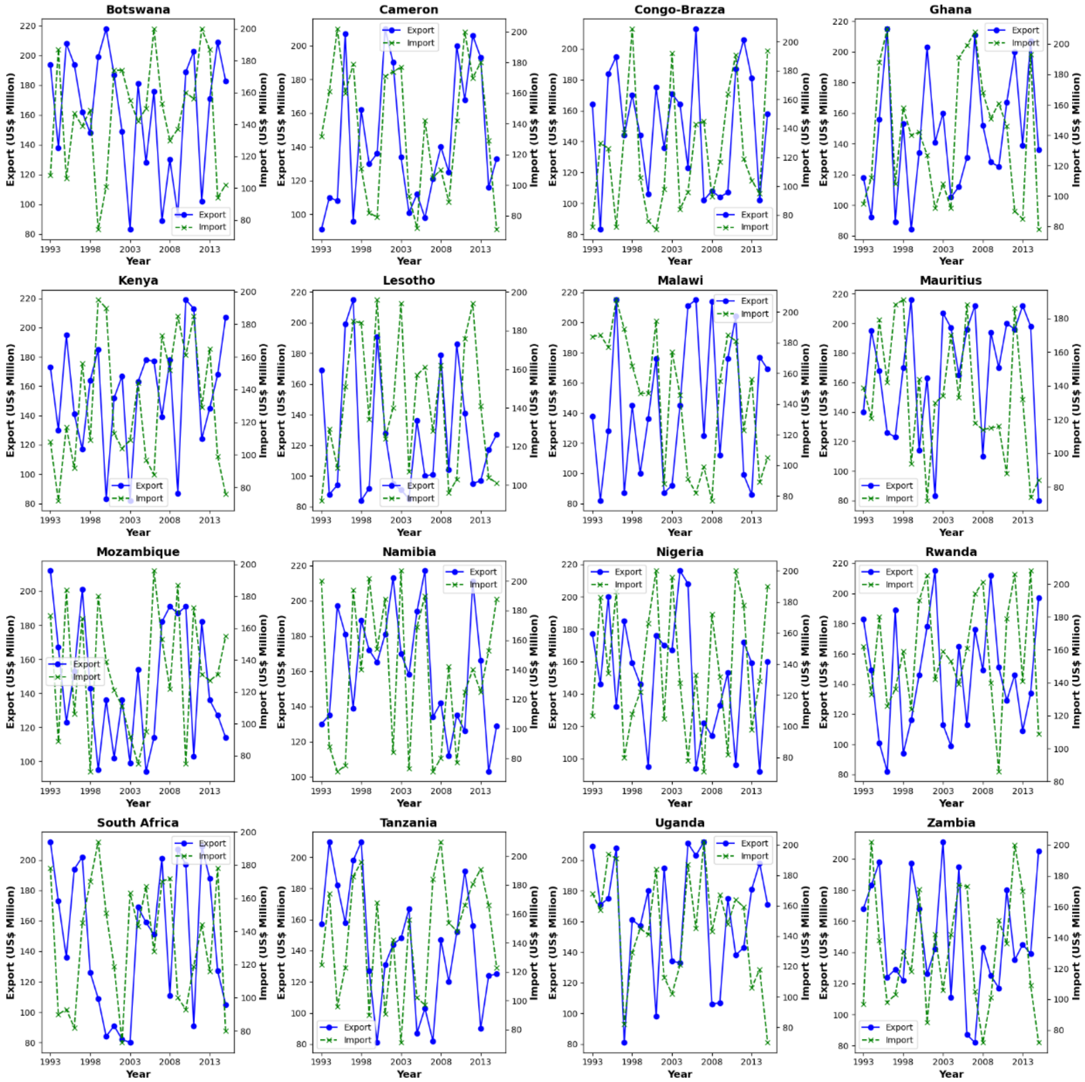
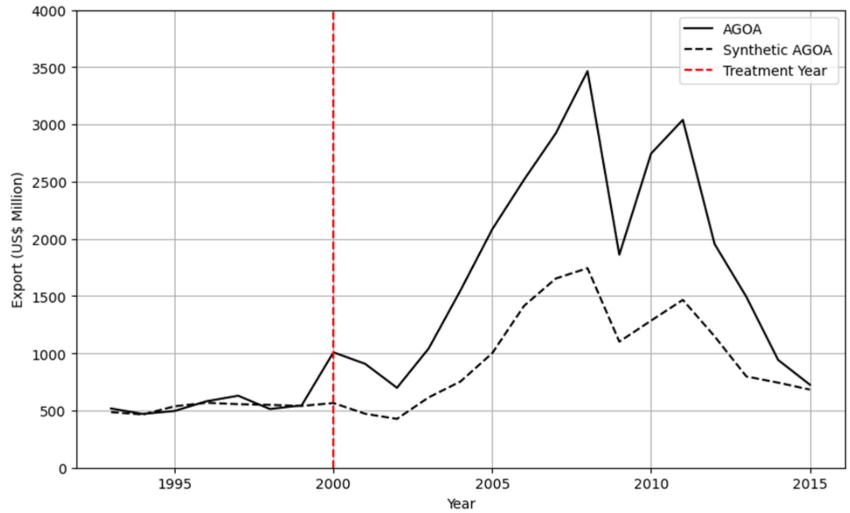


Figure 1.3: Export to the U.S. for AGOA Countries vs. Synthetic Control, 1993-2015



Note: This Figure shows the export trend of AGOA and the synthetic counterparts to the US.

Figure 1.4: Export by AGOA, Synthetic AGOA, and Donor Pool before the Treatment Period

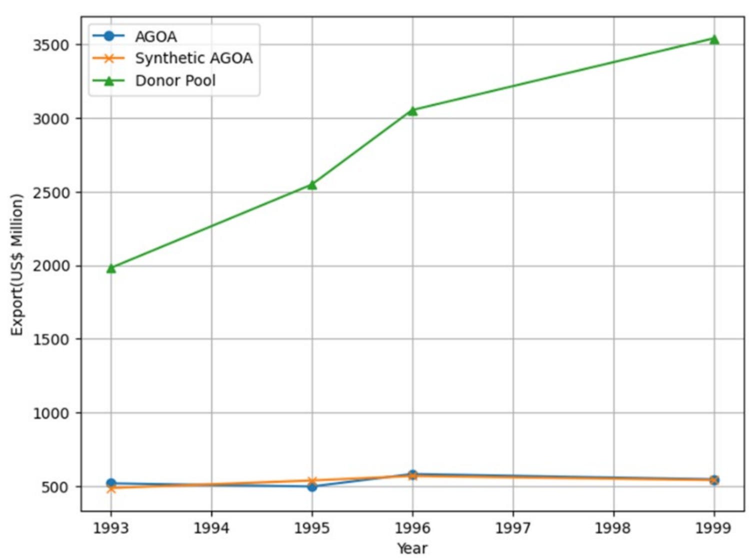
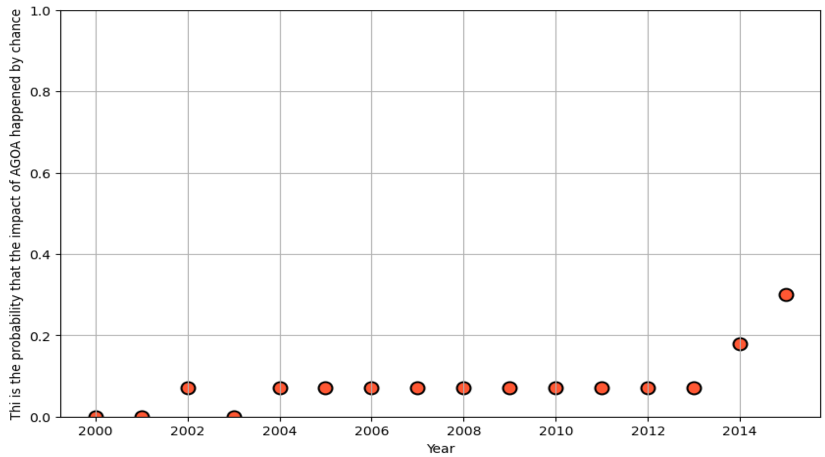
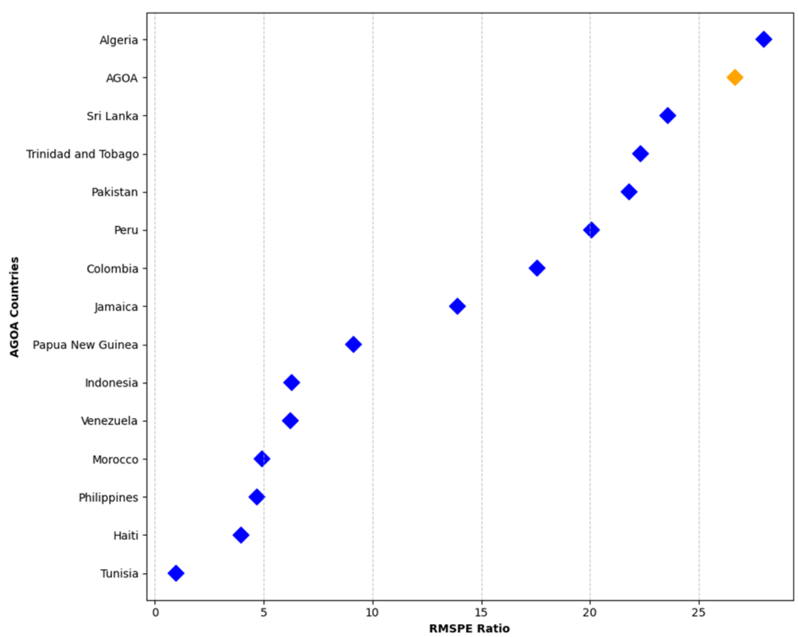


Figure 1.5: Probability Values of Average Treatment Effect



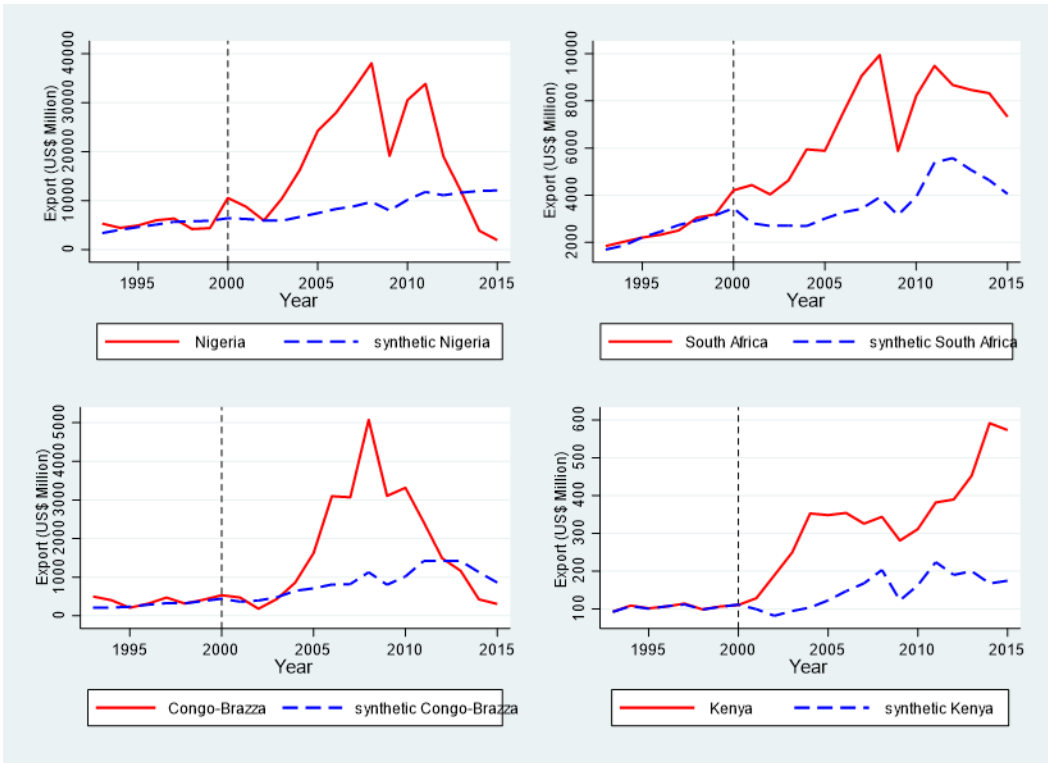
Note: This Figure shows the probability values of the average treatment effect occurring by chance

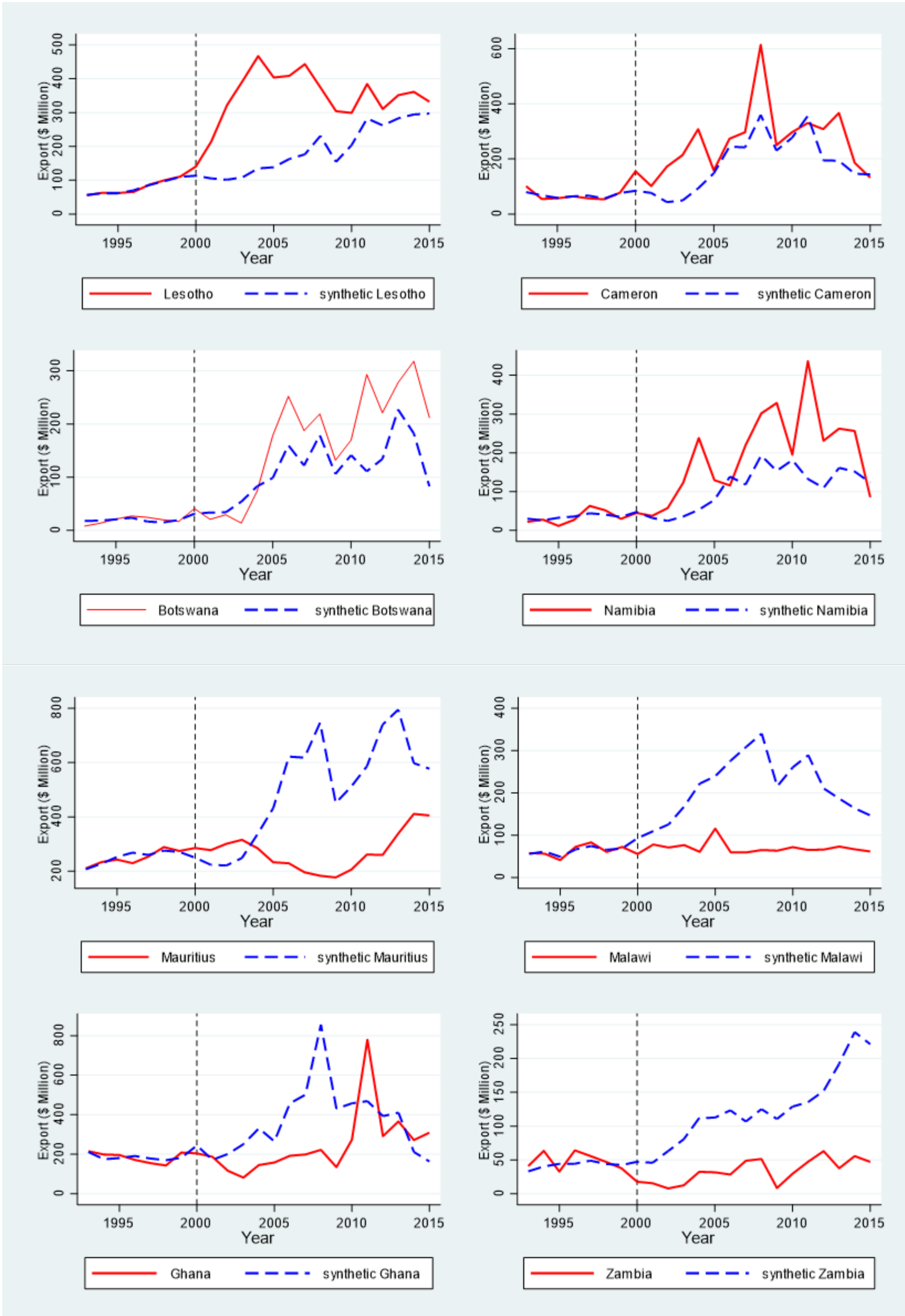
Figure 1.6: In-Space Placebo Result

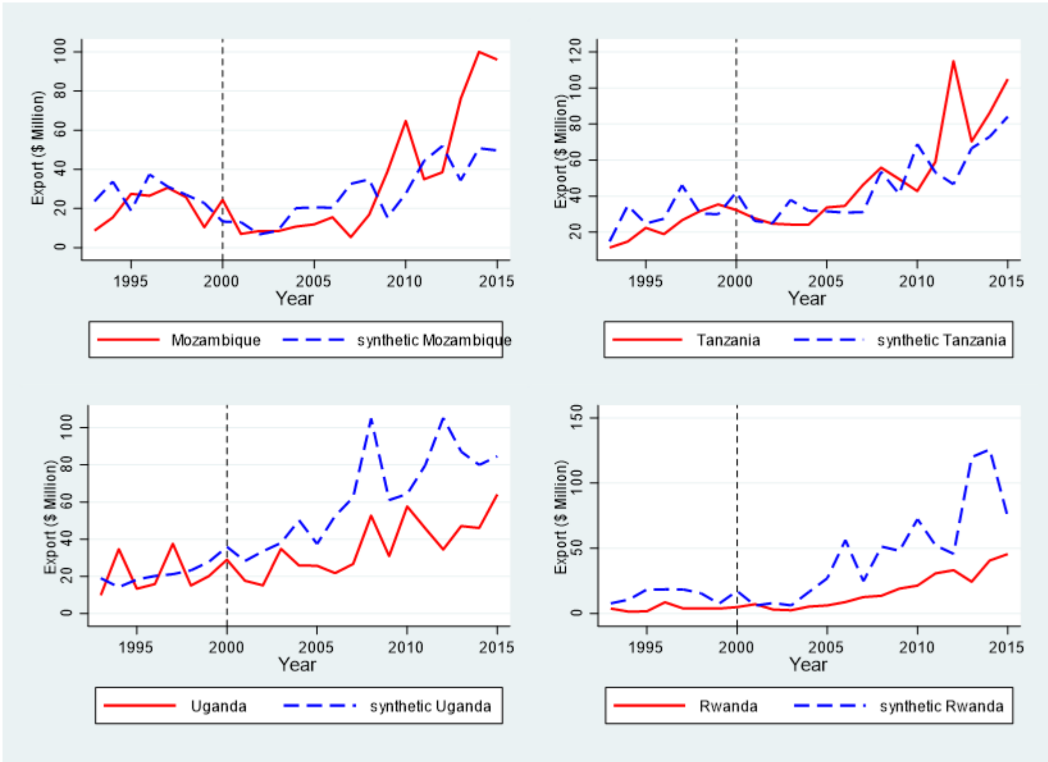


Note: This Figure shows the results of the "in-space placebo "study that shows how the preintervention and postintervention RMSPE compares with AGOA countries and their synthetic counterparts.

Figure 1.7: Export Trends and Synthetic Controls, SSA (1993 – 2015)




Export ($ Million)
Year
Lesotho
synthetic Lesotho
Cameron
synthetic Cameron
Botswana
synthetic Botswana
Namibia
synthetic Namibia
Mauritius
synthetic Mauritius
Malawi
synthetic Malawi
Ghana
synthetic Ghana
Zambia
synthetic Zambia



Note: These Figures show export trajectories for each AGOA beneficiary country and their synthetic counterparts. The dark-blue dashed line indicates synthetic country, and the solid red line shows the actual country.

Figure 1.8: Heterogeneity by HS Products

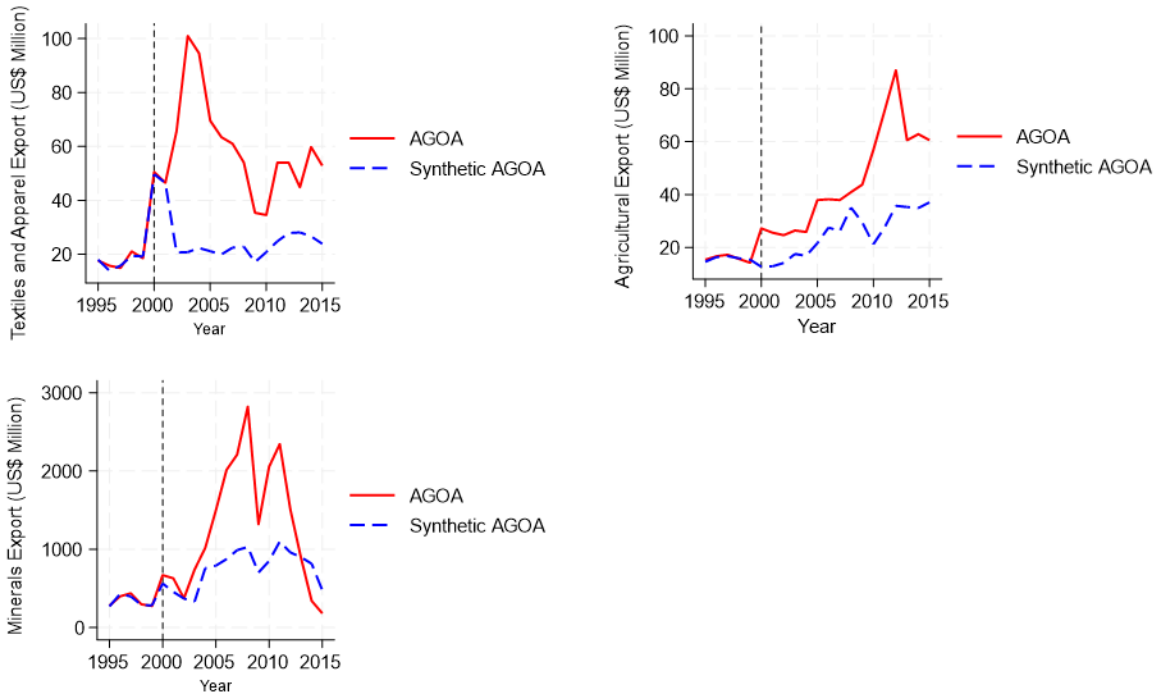


Figure 1.9: Parallel Trends for DD without Covariates

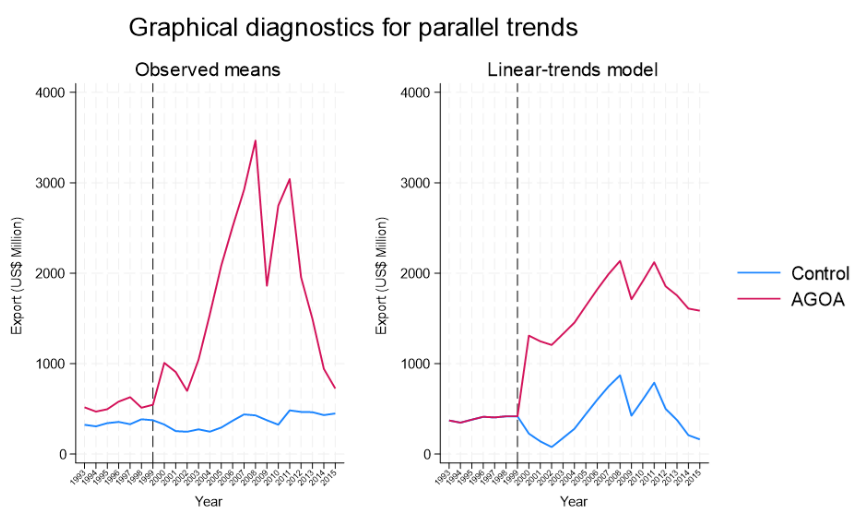


Figure 1.10: Granger Plots for DD without Covariates

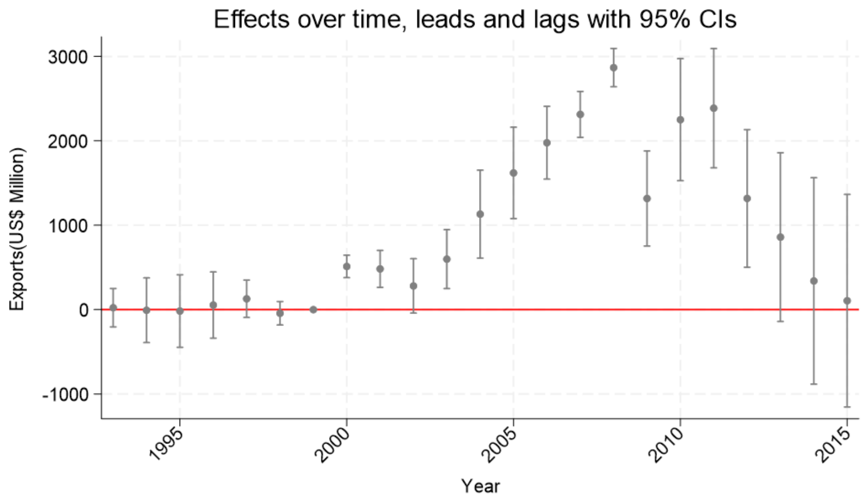


Figure 1.11: Parallel Trends for DD with Covariates

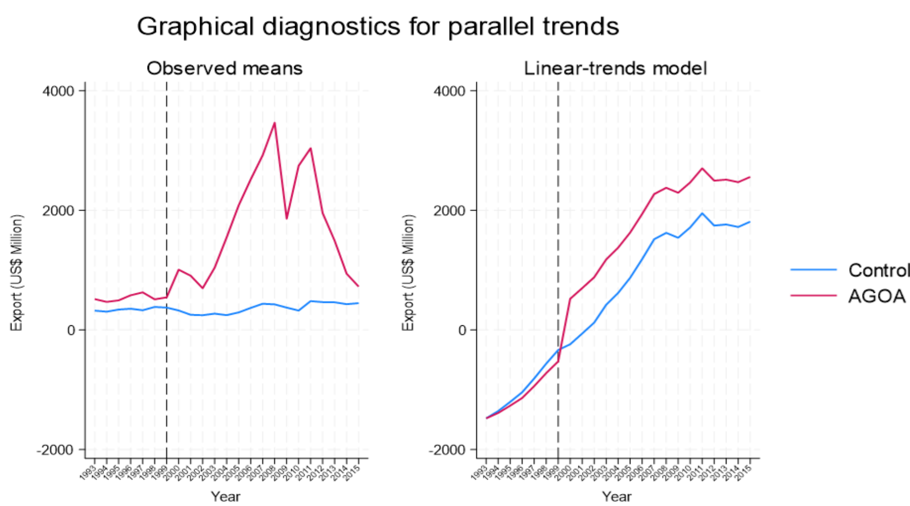


Figure 1.12: Granger Plots for DD with Covariates

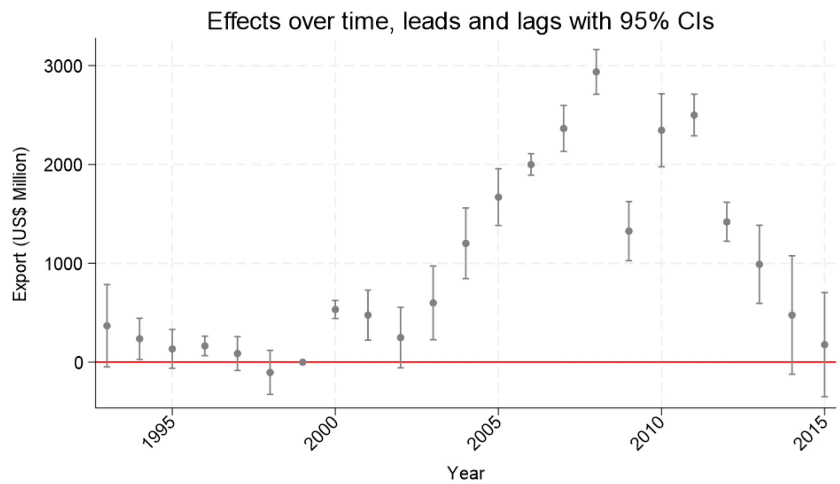


Figure 1.13: Event-Study Results

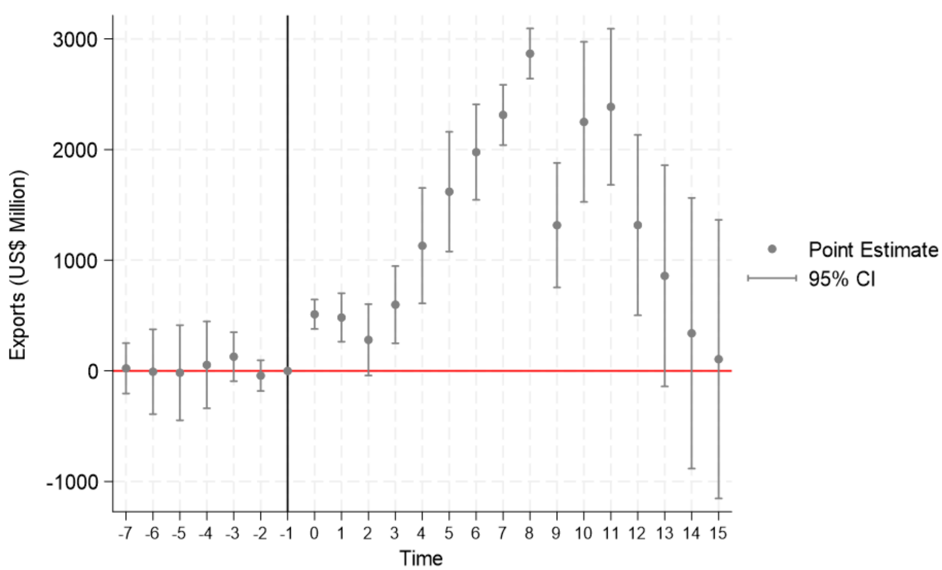


Table 1.1: AGOA Eligibility by Country

Country	AGOA Eligible Beginning	Apparel Provision Eligible Beginning	Special Rule for Apparel	Other eligibility Information	Included in Study
Angola	12/2003				
Benin	11/2000	01/2004	Yes		
Botswana	10/2000	08/2001	Yes		×
Burkina Faso	12/2004	08/2006	Yes		
Burundi	01/2006				
Cameroon	10/2000	03/2002	Yes		×
Cape Verde	10/2000	08/2002	Yes		
Chad	10/2000	04/2006	Yes		
Cote d'Ivoire	Restored			Eligible 05/2002; Ineligible 01/2005; Regained 10/2011	
Comoros	06/2008				
Congo, Rep. of	10/2000				×
Congo, DRC	Ineligible 01/2011			AGOA trade preferences granted in 10/2003	
Djibouti	10/2000				
Ethiopia	10/2000	08/2001	Yes		
Gabon	10/2000		No		
The Gambia	12/2002	04/2008	Yes		
Ghana	10/2000	03/2002	Yes		×
Guinea	Restored			Eligible 10/2000; Ineligible 01/2010; Regained 10/2011	
Guinea-Bissau	Ineligible 01/2013			Eligible 10/2000; Ineligible 01/2013; Restored 12/2014	
Kenya	10/2000	01/2001	Yes		×
Lesotho	10/2000	04/2001	Yes		×
Liberia	12/2006	01/2011			
Malawi	10/2000	08/2001	Yes		×
Madagascar	06/2014			Eligible 10/2000; Ineligible 01/2010; Restored 06/2014	

Mali	Restored 12/2013			Eligible 10/2000; Ineligible 01/2013; Restored 12/2013	
Mauritania	10/2000			Eligible 10/2000; Ineligible 01/2006; Restored 06/2007; Ineligible 01/2009; Restored 12/2009	
Mauritius	10/2000	01/2001	Yes		×
Mozambique	10/2000	02/2002	Yes		×
Namibia	10/2000	12/2001	Yes		×
Niger	Restored			Eligible 10/2000; Ineligible 01/2010; Restored 10/2011	
Nigeria	10/2000	07/2004	Yes		×
Rwanda	10/2000	03/2003	Yes		×
Sao Tome and Principe	10/2000				
Senegal	10/2000	04/2002	Yes		
Seychelles	10/2000		No		
Sierra Leone	10/2002	04/2004	Yes		
South Africa	10/2000	03/2001	No		×
South Sudan	Ineligible 2015			Eligible 12/2012; Ineligible 01/2015	
Tanzania	10/2000	02/2002	Yes		×
Togo	04/2008				
Uganda	10/2000	10/2001	Yes		×
Zambia	10/2000	12/2001	Yes		×

Source: United States Government Accountability Office (2015)[17]

Note: Date is formatted in MM/YYY

[17] United States Government Accountability Office (2015). African growth and opportunity act eligibility process and economic development in sub-Saharan Africa.

Table 1.2: Summary Statistics

Variable	Mean	SD	Min.	Max.
Export (US$ Million)	5,245.36	7,610.37	34.5	51,423.63
GDP (Current US$)	98.1	141	1.88	918
Inflation (Annual %)	8.88	10.31	0.19	121.74
Exchange rate	711.35	2,200.34	0.09	13,389.41
External debt (% of GNI)	48.06	23.46	2.56	168.20
Mobile Subscriptions (100 people)	41.23	44.86	0.00	154.95
Access to Telecom	24.77	24.19	0.30	87.36
Rule of Law	-0.54	0.49	-2.03	0.56
Property Right Index	41.73	17.27	0	90
Political corruption	3.08	0.94	0	5.3
Political Stability	-0.85	0.71	-2.81	0.41
Labor Freedom Index	58.92	13.49	21.7	83.3
Regime Durability	23.22	18.51	0	61
Government Effectiveness	-0.33	0.49	-2.04	0.64
Tax Burden Index	74.10	6.79	48.6	85.5
Government spending Index	78.07	11.35	38.7	99.3
Trade Freedom Index	63.69	13.41	27.2	87.2
Financial Freedom Index	48.644	15.2	10	70
Regulatory quality	-0.27	0.53	-1.89	0.74
Oil Export Share	14.36	26.41	0.00	99.66
Agricultural Export Share	4.73	5.16	0.01	31.56

Table 1.3: Estimated Synthetic AGOA Weight

Country	Unit Weight
Algeria	0.065
Colombia	0
Haiti	0.331
Indonesia	0
Jamaica	0.439
Morocco	0
Pakistan	0
Papua New Guinea	0.165
Peru	0
Philippines	0
Sri Lanka	0
Trinidad and Tobago	0
Tunisia	0
Venezuela	0

Table 1.4: AGOA Predictor Balance

Variables	AGOA	Synthetic AGOA	Donor Pool
log GDP per capita (Current US$)	6.97	7.03	7.68
Property Rights (Index)	48.34	43.73	41.64
log Mobile Subscription (100 people)	-0.87	-1.04	2.07
Political Stability	-0.35	-0.44	-0.87
log Exchange rate	5.28	2.78	3.33
Financial Freedom (Index)	47.96	44.19	48.58
log electricity access (Percentage)	3.24	3.69	4.26
Export (1993)	517.49	485.94	1981.16
Export (1995)	496.23	536.79	2547.39
Export (1996)	580.67	567.76	3054.05
Export (1999)	545.29	539.69	3541.3

Note: In this Table, we are showing the averages of covariates used in the SCM model for the AGOA, synthetic AGOA, and the donor pool. The difference between the AGOA and synthetic AGOA averages shows the predictive power of each covariate.

Table 1.5: Differences Between Actual and Synthetic AGOA

Year	AGOA (Y_{Real})	Synthetic AGOA ($Y_{Synthetic}$)	Gap ($Y_{Real} - Y_{Synthetic}$)	%Gap
1993	517.49	485.94	31.55	6%
1994	470.01	465.44	4.57	1%
1995	496.23	536.79	-40.56	-8%
1996	580.67	567.76	12.91	2%
1997	629.32	555.56	73.76	12%
1998	513.09	549.24	-36.15	-7%
1999	545.29	539.69	5.60	1%
Pre-Treatment Average			7.38	1%
2000	1008.75	565.56	443.19	44%
2001	907.79	471.39	436.40	48%
2002	698.64	426.79	271.85	39%
2003	1042.87	615.14	427.73	41%
2004	1550.13	753.64	796.49	51%
2005	2084.35	1001.57	1082.78	52%
2006	2517.28	1414.51	1102.77	44%
2007	2922.90	1653.68	1269.22	43%
2008	3466.59	1744.35	1722.24	50%
2009	1861.68	1101.65	760.03	41%
2010	2746.14	1285.82	1460.32	53%
2011	3040.61	1467.16	1573.45	52%

2012	1954.86	1148.14	806.72	41%
2013	1493.22	797.64	695.58	47%
2014	941.94	743.44	198.50	21%
2015	725.47	682.95	42.52	6%
Post-Treatment Average			818.11[18]	42%

Table 1.6: RSMPE and Fit Index

Year	Export
RSMPE	37.008
Fit Index	0.01

Notes: This Figure shows the pre-intervention fit results.

Table 1.7: Estimated Annual Change in Exports to U.S. due to the AGOA by Country

Country	Value (US$ millions)	% of GDP
Greater than 1% increase in exports (as % of GDP)		
Congo-Brazzaville	856.19	9.94
Lesotho	153.74	8.59
Nigeria	9545.31	3.31
South Africa	3261.56	1.18
Between a -1% to 1% change in exports (as % of GDP)		
Kenya	188.07	0.56
Botswana	53.64	0.50
Cameroon	80.03	0.36
Namibia	25.45	0.30
Mozambique	7.07	0.06
Tanzania	5.46	0.02
Rwanda	-29.52	-0.63
Zambia	-91.25	-0.62
Ghana	-117.56	-0.45
Uganda	-26.87	-0.16
Greater than -1% decrease in exports (as % of GDP)		

[18] The ATE value is subject to randomness and is influenced by the donor pool weighting and counterfactual predictor selection due to the stochastic nature of estimating the average treatment effect (ATE) within the SCM. As a result, when comparing aggregate analysis to analysis at the individual nation level, the possibility of getting identical ATE values is smaller. Nonetheless, just as MSE is minimized in linear regression to bring the sample parameter closer to the population parameter, donor weights and variables that result in closely matching ATE estimations between individual and aggregate analyzes are preferable. Our results show aggregate ATE values of $818.11 million and individual nation averages of $845.36 million.

Malawi	-140.45	-2.95
Mauritius	-224.52	-2.62
Average	**845.36**[19]	**1.09**

Note: GDP is measured as the average of post-GDP years for each country. This Table shows the average export gains by the individual AGOA countries from 2000 – 2015. *P-values* for individual country ATE are significant in the post-treatment periods.

Table 1.8: Determinants of Trade Gain Through AGOA Regression Results

Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Log GDP (Current US$)	0.192***	0.133***	0.228***	0.075*	0.043*	0.051**
	(0.008)	(0.027)	(0.032)	(0.040)	(0.023)	(0.020)
Log Exchange Rate	0.004	-0.095***	0.043	-0.166***	-0.164***	-0.193***
	(0.022)	(0.034)	(0.030)	(0.034)	(0.049)	(0.030)
Regulatory Quality	0.082	-0.013	0.084**	-0.055	-0.003	
	(0.064)	(0.125)	(0.041)	(0.148)	(0.082)	
Political Stability			0.065	-0.139***	-0.006	-0.111**
			(0.096)	(0.039)	(0.116)	(0.050)
Inflation (Annual %)		-0.003	-0.001	-0.002*		
		(0.002)	(0.001)	(0.003)		
Log External Debt (% of GNI)		0.120***	0.115***			
		(0.025)	(0.030)			
Mobile Subscription			0.000		0.004***	
			(0.001)		(0.001)	
Government Effectiveness			-0.002	-0.124		
			(0.078)	(0.163)		
Rule of Law			0.114			
			(0.121)			
Labor Freedom			0.010***	-0.008*		
			(0.004)	(0.005)		
Property Rights Index		0.004	0.001		0.006**	0.007**
		(0.003)	(0.003)		(0.002)	(0.004)
Government Spending	-0.001	-0.005**				
	(0.002)	(0.002)				
Trade Freedom		0.004				
		(0.004)				
Access to Telecom				0.004***		0.008***
				(0.001)		(0.002)
Tax Burden				-0.002	-0.001	-0.004
				(0.003)	(0.004)	(0.003)
Political Corruption					-0.036	-0.075***
					(0.028)	(0.027)

[19] Refer to the notes on footnote (17)

Regime Durability					-0.012**	
					(0.005)	
Financial Freedom					0.008***	0.006
					(0.003)	(0.003)
Oil Export Share	0.003**	0.006***	0.004**	0.006*	0.009***	0.007***
	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)
Agricultural Export Share	-0.008*	0.004	0.001	-0.004	0.009	0.007
	(0.005)	(0.003)	(0.005)	(0.007)	(0.008)	(0.007)
Observations	83	78	78	83	78	80
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is the log transformed treatment effect ($\boldsymbol{Gap = Y_{Real} - Y_{Synthetic}}$) or the change in exports to the U.S. due to the AGOA (in million U.S. $); Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1. All specifications include both country and year fixed effects, and constant terms. Data from 2001-2015 capturing only the AGOA-eligible countries employed for the estimations.

Table 1.9: DD Average Treatment Effects Estimates

Export (US$ Million)	Coefficient	Robust (SE)	P-value	[95% Conf. Interval]	
DD Without Covariates					
ATET	1252.664	208.588	0.009	588.8436	1916.484
DD With Covariates					
ATET	852.61	246.17	0.041	69.18065	1636.042

Note: ATET coefficients are in US$ million

Table 1.10: Parallel Trend and Granger-Type Causality Test

Test	Prob > F
DD Without Covariates	
Parallel Trends	0.9894
Granger-Type Causality	0.4115
DD with Covariates	
Parallel Trends	0.6122
Granger-Type Causality	0.6359

Note:
H0 (Parallel Trend): Linear Trend is Parallel.
H0 (Granger Causality): No Effect in anticipation of Treatment

Appendix

Table 1.A1: Variable Description

Variable	Definition
Export	Value of exports to the U.S. (in millions of US$)
GDP (Current US$)	Gross domestic product (Current billion US $)
Inflation (Annual %)	Inflation, consumer prices (annual %)
Exchange rate	Exchange rate (local currency to US $)
External debt (% of GNI)	External debt (% of GNI)
Mobile Subscriptions (100 people)	Mobile cellular subscription (per 100 people)
Access to Telecom	Average index based on mobile cellular subscriptions & fixed telephone subscriptions (per 100 people)
Rule of Law	Composite index of rule of law in a country (-2.5 = weak rule of law; 2.5 = strong rule of law).
Property Right Index	Extent to which a country's legal framework allows individuals to acquire, hold, and utilize private property (bounded between 0 and 100).
Political corruption	The abuse of 'entrusted power for private gain'. (0 = highly corrupt country), and (10 = a very clean country).
Political Stability	A measurement of perceptions of the propensity that the government will be destabilized or overthrown by unconstitutional or violent means, including politically motivated violence and terrorism (-2.5 = weak; 2.5 = strong).
Labor Freedom Index	Index of ease of regulations concerning minimum wages, laws on layoffs, severance requirements, and overall labor regulations. (0 = worst rigidity, 100 = most flexible).
Regime Durability	A variable capturing end of transition period defined by the lack of stable political institutions (Ranges between -2.5 – weak; 2.5 - strong).
Government Effectiveness	A variable capturing "perceptions of the quality of public services, civil service, policy formulation and implementation (-2.5 indicates weak governance; 2.5 indicates strong).
Tax Burden Index	A composite measurement that reflects marginal tax rates on both personal and corporate income and the overall level of taxation (including direct and indirect taxes imposed by all levels of government) as a percentage of gross domestic product (GDP). (Ranges from 0 to 100)

Government spending Index	A variable capturing the burden imposed by government expenditures, which includes consumption by the state and all transfer payments related to various entitlement programs (Ranges from 0 to 100).
Trade Freedom Index	A composite measurement of the extent of tariff and nontariff barriers (NTB) that affect imports and exports of goods and services. (0 = NTB/Tariffs are not used to limit international trade; 100 = NTB/ Tariff are extensively used).
Financial Freedom Index	An indicator of banking efficiency as well as a measure of independence from government control and interference in the financial sector (0 = Repressive, 100 = No government interference),
Regulatory quality	A variable reflecting perception of the ability of the government to formulate and implement sound policies and regulations that permit and promote private sector development (-2.5 = weak regulatory quality; 2.5 = strong regulatory quality).
Oil Export Share	Percentage of oil in total export (%)
Agricultural Export Share	Percentage of agricultural commodities in total export (%)

Chapter 2

Is Exiting Burdensome? Examining the Impact of Brexit on UK Trade Outcomes[20]

Introduction

In June of 2016, the United Kingdom (UK) voted to leave the European Union (EU), termed "Brexit". Brexit occurred on January 31, 2020. The UK remained a member of EU's single market and customs union until January 1, 2021, after which the Trade and Cooperation Agreement (TCA) governing future UK-EU relations entered into force (Freeman et al., 2022). The primary objective of Brexit was to grant the UK more freedom to renegotiate its global trade relationships without EU constraints (Freeman et al., 2022). In May 2021, the UK introduced the UK Global Tariff (UKGT), aimed at simplifying tariffs and reducing them on many goods entering the UK. However, this new tariff structure resulted in higher tariffs on certain goods that were previously traded duty-free between the UK and the EU[21]. Consequently, this has increased costs for UK importers and raised administrative expenses to comply with new rules and classification codes. This shift in trade dynamics has been documented in several studies, including Keogh (2018), Byrne and Rice (2018), Oberhofer and Pfaffermayr (2021), Kren and Lawless (2022), Freeman et al. (2022), Pawlak et al. (2022), and Douch and Edwards (2022). Post-Brexit, multinational corporations have faced challenges such as increased manufacturing costs and stricter regulations, altering their once seamless cross-border operations (McGrattan & Waddle, 2020).

In this paper, I explore whether Brexit's impact on UK trade differs for durable and non-durable commodities with the EU and the rest of the world. The focus is on understanding how the

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[21] https://tax.thomsonreuters.com/blog/post-brexit-companies-face-new-uk-tariff-regime-and-ongoing-trade-compliance-concerns/

process of disintegration influences value of trade flows, specifically examining the effects of Brexit on trade in goods between the UK, the EU, and global partners while distinguishing between value of durable and non-durable trade flows. I analyze two distinct periods: pre-2021, marked by uncertainty about Brexit's form, and post-January 2021, after the implementation of the TCA, which introduced new bilateral trade barriers between the UK and the EU. By doing this, I assess the trade effects of both anticipated increases in trade costs and the actual establishment of higher trade barriers under the TCA.

My emphasis on durable and non-durable trade stems from existing research that demonstrates how economic conditions and policies impact the demand and supply of these commodities differently (e.g., Mallick & Mohsin, 2016; Constantinescu et al., 2020; Liu et al., 2022). In relation to Brexit, an International Monetary Fund (IMF, 2018) analysis predicts a trade deficit differential between durable and non-durable products, implying that the UK's trade deficit with EU is mostly driven by machinery and transportation equipment. Fernandes and Winters (2021) use the Brexit referendum as a quasi-natural experiment to study the effect of exchange rate and uncertainty shocks on export volumes, discovering that the effects of the shock are much greater for durable commodities than for non-durable items. These findings are consistent with the literature on the dynamics of durable goods spending over the business cycle (e.g., Engel and Wang, 2011; Leahy and Zeira, 2005).

Consumption of durable goods has been found to be highly volatile and procyclical, as households can postpone the decision to trash and update durable goods such as vehicles, refrigerators, and so on (see Fernandes and Winters, 2021). According to Goldberg and Pavcnik (2016), businesses and consumers cannot change their behavior prior to the implementation of trade policy if it is unexpected. On the other hand, companies and customers may change their behavior beforehand if it is anticipated. For example, businesses may decide to accelerate or postpone investments in anticipation of trade liberalization. In a similar vein, buyers may wait to purchase durable goods until after the trade policy is implemented.

Numerous studies (Byrne and Rice, 2018; Keogh, 2018; Lawless and Morgenroth, 2019; Campos and Timini, 2019; Knecht, 2019; Oberhofer and Pfaffermayr, 2021; Douch and Edwards, 2022; Pawlak et al., 2022; Freeman et al., 2022) have investigated the impact of Brexit on UK trade, either with the EU or the rest of the world. Pawlak et al. (2022) focus on agricultural products from Visegrad countries to the UK. Douch and Edwards (2022) estimate the impact of Brexit on trade between the UK and 14 EU and 14 non-EU trading partners using a synthetic control method and show that UK exports to both groups of countries decreased compared to a hypothetical scenario where the Brexit vote did not occur. Oberhofer and Pfaffermayr (2021) use a panel data structural gravity model to assess the trade and welfare effects of Brexit on manufacturing goods trade. The research explores different post-Brexit scenarios and reveals that the UK's goods exports to the EU are likely to decrease by 7.2% to 45.7% within six years after Brexit. Knecht (2019) examines the effect of the Brexit referendum on UK services trade using the Synthetic Control Method (SCM), finding that total services trade is 4.96% lower than what it would have been if Britain had not voted to leave the European Union. Campos and Timini (2019) employ a gravity model to simulate scenarios with and without a bilateral free trade agreement between the UK and the EU and show that Brexit is likely to have significant negative effects on trade flow between the UK and the EU. Keogh (2018) use the Gravity Model with Poisson Pseudo Maximum Likelihood (PPML) estimation to study the effect of Brexit on Irish merchandise goods exports and show that a 1% increase in trade costs results in a fall of 0.73% in goods export value. A soft Brexit where the UK remains in the EU Customs Union has no effect on the value of trade. Byrne and Rice (2018) analyze the potential reduction in trade between Ireland and the UK due to increased non-tariff barriers post-Brexit and show that indicates a 1.4% decrease in total Irish exports and a 3.1% decrease in total Irish imports. Flynn et al. (2021) examine early post-Brexit trade flows in goods between Ireland and the UK and show a direct impact of Brexit as asymmetric, with a significant reduction in imports from the UK to Ireland but no statistically significant impact on exports, while Lawless and Morgenroth (2019) investigate the impacts of WTO tariffs on merchandise trade between the UK and the EU and show a reductions in trade to the UK falling by 5% (Finland) to 43% (Bulgaria).

This study expands upon this existing research. While the closest comparable study by Freeman et al. (2022) used a panel event study approach with quarterly data from 2013 to 2021 to evaluate Brexit's impact on some selected trade goods between UK and EU and some selected countries in the world, this study relies on comprehensive annual data spanning from 2000 to 2021. When a trade policy is unexpected, the quasi-experimental study design adequately captures its effects since enterprises and consumers are unable to alter their behavior prior to its adoption. However, if a trade policy is anticipated, enterprises and consumers may change their behavior ahead of time. The aspect of anticipation is a key concern, particularly in investigations using higher frequency data. Depending on the conditions, this anticipation factor could lead to either an overestimation or an underestimating of the policy consequences. Nonetheless, by adding data from a sufficiently long period before the announcement and adoption of a given policy, researchers can directly examine whether the policy changed behavior prior to its implementation, allowing for changes in estimating its effects accordingly (see Goldberg and Pavcnik, 2016). I used a difference-in-difference approach within a gravity model framework to evaluate the causal impact of both the transitional period of Brexit (2016 to 2020) and the actual event in 2021, considering pre-transitional and pre-TCA Brexit data from 2000 to 2015. To ensure the reliability of my findings, I integrated the Heckman selection model and PPML with the synthetic control method for robustness checks.

This study makes three key contributions to the existing literature: First, I used extensive yearly data from 2000 to 2021, to analyze pre-Brexit, transition, and post-Brexit periods. This data examines the effect of Brexit on UK trade values within the EU and countries outside of it. Second, in contrast to previous research, I included all HS2 product (96 product lines) classifications and categorized as durable and non-durable goods for analysis. Third, I used an innovative approach, combining difference-in-difference with the gravity model estimated in Heckman selection model. This approach helps me to estimate the causal impact of Brexit on UK trade with the EU and the rest of the world while considering variables that are main drivers of global trade. I used the PPML and synthetic control methods to ensure robust findings.

The findings show that Brexit has reduced values of UK imports and exports to and from the EU, as well as with other countries around the world in both the transition and TCA periods. UK imports value from ROW decreased by 0.20% (0.24% for durable goods and 0.18% for non-durable goods) and from the EU by 0.41% (0.39% for durable goods and 0.41% for non-durable goods). The overall UK exports to ROW decreased by 0.47%. Durable goods saw a decline of 0.61%, while non-durable goods dropped by 0.43%. Additionally, exports to the EU decreased by 0.86%, with durable goods declining by 0.64% and non-durable goods by 0.91%. This discrepancy suggests that factors such as tariff increases, border delays, and customs clearance issues, which have been evident in Brexit, might be impacting non-durable goods more severely. These goods, often perishable or quickly consumed, are particularly sensitive to disruptions in supply chains or delays in transit, leading to a more pronounced decline compared to durable goods.

Methodology

Gravity Model and Difference-in-Differences

The gravity model is commonly utilized for evaluating the effect of trade policies on bilateral trade volumes, taking into consideration either tariffs, non-tariff measures (NTMs), or a combination of both. The use of gravity model in this context has the potential to contribute to the differences in outcomes (see Chen et al., 2018).

I explore recent literature to address three significant issues related to applying the gravity model in the context of Brexit. To begin, the fundamental theory of the gravity model necessitates the inclusion of controls for the barriers a country encounters in comparison to all other nations. Neglecting to incorporate multilateral resistance terms (MRTs) in the analysis can introduce bias into the estimates of the gravity model, as noted by Anderson and van Wincoop (2003) and Xiong and Beghin (2011). When examining the overall effect of predictors on the outcome variable, using fixed effects, represented by country and time dummy variables, reduces the impact of time-invariant characteristics. Second, according to Santos Silva and Tenreyro (2006), when trade values are zero, the log-linearized OLS model exhibits bias in the presence of heteroskedasticity due to Jensen's inequality. These authors recommend using a Poisson pseudo-maximum likelihood (PPML) model instead, as it yields robust results when dealing with various forms of heteroskedasticity. Thirdly, given the non-naturally occurring nature of the data, the treatment effect of Brexit may be prone to selection bias.

In such situations, the Difference-in-Differences (DID) strategy is deemed suitable for assessing the impact of policy implementation on the target group, as commonly seen in economics literature (see for example, Chen et al., 2018). The identification is based on a DID that estimates the differential effect of the referendum on the value of UK exports and imports to the EU, as well as the value of UK imports and exports to non-UK countries in the rest of the world (ROW) compared to the value of other non-UK countries exports and imports to the EU and ROW. The sudden, substantial, and unforeseen shock, involving a significant trading relationship, is well-suited for analysis in the context of the value of UK exports and imports to and from the EU and ROW. This shock is considered unexpected and external from the perspective of UK exporters, creating a unique experimental scenario. Additionally, the economic conditions in the UK showed relatively little change in the 12 months following the Brexit vote (see Fernandes and Winters, 2021), thus reducing potential confounding variables. During this period, the UK maintained its membership in the EU and the single market, only officially notifying the EU of its intention to withdraw in March 2017. My approach is designed to estimate the combined impact of various shocks, such as fluctuations in trade policies and uncertainties in the aftermath of the Brexit vote. This approach offers the advantage of obtaining a clear and direct identification of these effects, eliminating the need for proxies to represent each of these shocks, some of which can be challenging to disentangle.

I operated under the assumption that the counterfactual, such as the parallel trend, was fulfilled. The assumption must hold for the parameter estimates to be unbiased. I empirically examine the parallel trend assumption following the approach of Li et al. (2021). I estimated equations (1) and (2), where the value of UK and other non-UK countries imports and exports are regressed on the treatment dummy variable (Treated), a pre-treatment linear time trend variable, $Year$ (1,2,3…16), and the interaction term between two variables ($Treated \times Year$), and ϵ_{it} and ϵ_{jt} are error terms. The parallel Trend assumption is violated if the coefficient on the interaction term ($Treated \times Year$) is statistically significant (see Li et al., 2021).

$$ln\left(Import_{(i)(t)}\right) = \beta_0 + \beta_1 Treated_i + \beta_2 Year_t + \beta_3 (Treated_i \times Year_t) + \epsilon_{it} \tag{1}$$

$$ln\left(Export_{(j)(t)}\right) = \beta_0 + \beta_1 Treated_j + \beta_2 Year_t + \beta_3\left(Treated_j \times Year_t\right) + \epsilon_{jt} \quad (2)$$

Scholars use the DID approach in fields other than trade to compare the outcomes of the treatment group to those of the control group before and after treatments, as demonstrated by the work of Card and Krueger (1994). Several international trade issues have been studied using the DID approach within the setting of the gravity model (See for example, Gunther, 2012; Tello, 2015; Chen et al.,2018). The specific objectives are to (1) determine the effect of Brexit implementation using DID based on gravity specifications, and (2) estimate the change in UK's export and imports values after Brexit.

I used Synthetic Control method (SCM) (Abadie et al., 2010) as a robustness check to separate the effects of Brexit from other influences. Applying DID analysis to macroeconomic events or policy may be challenging, despite their widespread use in microeconomic research. It might be difficult to meet underlying assumptions in macroeconomic situations, such as the "parallel trend assumption". The SCM bridges the gap between qualitative and quantitative methodologies by offering a systematic approach to select comparison units in comparative case studies (Abadie et al., 2015). I used the SCM to address the parallel trend assumption that might be violated within the DID.

Data

I used a panel dataset bilateral trade from 2000 to 2021 capturing periods: pre-Brexit transition period (2000 – 2015), the transition period (2016 – 2020), and the TCA Brexit (2021). The dataset consists of 9 million observations at the 2-digit harmonized system (HS2) product and exporter and importer country, and year level. I focused on zero and non-zero trade flows from 16 importing countries and 207 exporting countries (if UK is an importer) and 17 exporting countries and 206 importing countries (if UK is an exporter) at HS2 classification recategorized from 6-digit harmonized system (HS6) classification with more than 5000 product levels. The countries are listed in Table 2.A1 in the Appendix. The HS classification by section is presented in Table 2.1. The HS2 digit products were then reclassified to durable and non-durable following Liu et al., (2022). See Table 2.A2 in the Appendix for the details.

I obtained bilateral trade flows from BACI, and trade friction data from the CEPII Gravity Database, which includes distance between two nations as well as binary variables indicating whether two countries share a shared official language, a colonial relationship, or a continuous border. Specifically, these are, $(DIST)_{(i)(j)}$ is the population-weighted average distance between the most populated cities of each country, harmonic mean, in km. $(CONT)_{(i)(j)}$ is a dummy taking a value of one if country i and j share a contiguous border and zero otherwise. $(LANG)_{(i)(j)}$ is a dummy taking a value of one if i and j share common official or primary language. $(COL)_{(i)(j)}$ is a dummy taking a value of one if i and j are or were in colonial relationship post 1945, $(FTA)_{(i)(j)(t)}$ is a dummy taking a value of one if i and j are engaged in a regional trade agreement. COM_{ij} is a dummy taking a value of one if i and j share a common colonizer post 1945. Table 2.2 defines the variables used in the analysis. The estimated coefficient of the dummy variables is interpreted as $(exp(\beta_i) - 1) \times 100)$. Table 2.3 provides summary statistics. Pre-2016 $Import_{(i)(j)(k)(t)}$ for UK averaged \$21,174 (in thousands current USD) and \$23,938 (in thousands current USD), compared to \$6499 (pre-2016) and \$9957 (post-2016) for non-UK countries. $Export_{(i)(j)(k)(t)}$ average \$15,205 and \$15,257 (in thousands current USD) pre-2016 and post-2016, respectively for UK compared to \$6,149 and \$10167 for pre-2016 and post-2016 respectively for non-UK countries.

Empirical Framework

Building on previous research (Chen et al., 2018; Tello, 2015; Gunther, 2012), I used DID approach embedded in the gravity model framework to investigate Brexit impact on the import and export values of UK durable and non-durable products between EU and ROW. I categorized all UK imports and exports value as the "treatment group," while those of the United States, Canada, Mexico, China, Switzerland, and EU countries contributing to less than 0.5% of the UK's imports and exports (e.g., Lithuania, Luxembourg, Croatia, Cyprus, Slovenia, Greece, Latvia, Malta, Estonia, and Bulgaria) constitute the "control group." This demarcation is crucial for identifying the causal impact of Brexit. In establishing causality, I place a critical emphasis on selecting countries similar in characteristic to the UK. On the other hand, identifying countries minimally affected by Brexit.

These countries were selected as the control for two main reasons. Firstly, except for China, all non-EU nations (United States, Canada, Mexico, China, and Switzerland) are members of the Organization for Economic Co-operation and Development (OECD). Even China, a non-member economy, has established collaboration with the OECD through a dialogue and cooperation program initiated in October 1995. Secondly, each of the EU countries contributes less than 0.5% to UK's imports and exports values and they are less likely to be directly impacted by the decision. Their trade characteristics are considered somewhat analogous to those of the UK.

By including these countries in the control group, I aim to achieve a higher level of precision in assessing the impact of Brexit while minimizing potential spillover effects. I ran a regression analysis using pre-TCA Brexit data, following the methodology described by Li et al. (2021), to establish the adherence to the parallel trend assumption. This is to ensure that in the absence of Brexit, the average outcomes of both the treatment and control groups would have followed similar trajectories over time. Additionally, I used the SCM as another means to confirm the validity of this assumption.

Difference-in-Differences Approach Within Gravity Model Framework

I applied the DID approach within the empirical specification of a typical gravity model following Tello (2015) and Chen et al., (2018) and estimate,

$$\begin{aligned} ln\left(Import_{(i)(j)(k)(t)}\right) &= (\alpha_i) + \left(\alpha_j\right) + (\alpha_t) + (\alpha_k) + \beta_1 D(Brexit)_t * D(Treated)_i \\ &+ \sum_{d=1}^{6} \gamma_d X_{(i)(j)(d)(t)} + \varepsilon_{(i)(j)(k)(t)} \end{aligned} \tag{3}$$

where $Import_{(i)(j)(k)(t)}$ is UK imports value of HS2 commodities (k) from exporting country (j) in year (t). The policy dummy $D(Brexit)_t$ is equal zero for UK imports value between 2000 to 2015 (pre-transition and pre-TCA Brexit periods) and one for 2016 to 2020 (for transition period) or 2021 (for the Brexit period). The treatment $D(Treated)_i$ is equal one for UK imports value and zero for imports value for the control countries.

The $X_{(i)(j)(d)(t)}$ includes gravity model variables defined in Table 1. Specifically, $ln(DIST)_{(i)(j)}$, $D(CONT)_{(i)(j)}$, $D(LANG)_{(i)(j)}$, $D(COL)_{(i)(j)}$, $D(FTA)_{(i)(j)(t)}$, and $COM_{(i)(j)}$. (α_i) is the importer fixed effect, $\left(\alpha_j\right)$ is the exporter fixed effect, (α_t) is time fixed effects, (α_k) is product fixed effect, and $\varepsilon_{(i)(j)(k)(t)}$ is error term.

The estimated coefficient of the dummy variables is interpreted as $(exp(\gamma_d) - 1) \times 100)$. The pure treatment effect of interest, (β_1), is the coefficient of the interaction $D(Brexit)_t * D(Treated)_t$. I test the null hypothesis that β_1 is zero, and The failure to reject the null hypothesis suggests that Brexit did not affect the value of UK imports value. A similar model is being developed for UK exports value in (4).

$$ln(Export_{(i)(j)(k)(t)}) \\ = (\alpha_i) + (\alpha_j) + (\alpha_t) + (\alpha_k) + \beta_1 D(Brexit)_t * D(Treated)_i \\ + \sum_{d=1}^{6} \gamma_d X_{(i)(j)(d)(t)} + \varepsilon_{(i)(j)(k)(t)} \tag{4}$$

where $Export_{(i)(j)(k)(t)}$ denotes country i exports value of HS2 commodities (k) to importing country (j) in year (t).

Difference-in-Difference-in-Differences (DIDID) in Gravity Model

I also estimated a triple differences design in (5)

$$ln(Import_{(i)(j)(k)(t)}) \\ = (\alpha_i) + (\alpha_j) + (\alpha_t) + (\alpha_k) + \beta_1 D(Brexit)_t * D(Treated)_i \\ + \beta_2 D(Brexit)_t * D(Treated)_i * D(Commodity)_i \\ + \sum_{d=1}^{6} \gamma_d X_{(i)(j)(d)(t)} + \varepsilon_{(i)(j)(k)(t)} \tag{5}$$

where $X_{(i)(j)(d)(t)}$ has the same variables as in (1). However, the variable of interest becomes $\beta_2 D(Brexit)_t * D(Treated)_i * D(Commodity)_i$. A significant coefficient indicates that Brexit had an impact on UK imports value, accounting for two potentially confounding trends: changes in commodity imports value between countries and changes in UK imports sector-level commodities between pre- and post-Brexit. Similar model is specified for export in (6)

$$ln(Export_{(i)(j)(k)(t)}) \\ = (\alpha_i) + (\alpha_j) + (\alpha_t) + (\alpha_k) + \beta_1 D(Brexit)_t * D(Treated)_i \\ + \beta_2 D(Brexit)_t * D(Treated)_i * D(Commodity)_i \\ + \sum_{d=1}^{6} \gamma_d X_{(i)(j)(d)(t)} + \varepsilon_{(i)(j)(k)(t)} \tag{6}$$

Difference-in-Differences in Heckman Selection Model

Scholars have raised issues about the traditional gravity model specification due to its failure to account for the intrinsic self-selection process in trade. This self-selection is evident when countries could not trade (or choose not to trade). This results in zero trade activity (see Chen et al., 2018). Because the dependent variable in the gravity specification is transformed into logarithmic it becomes undefined when dealing with zero values. As a result, the traditional gravity specification is unable to make use of data with zero trade. These instances are typically ignored in traditional gravity models. In response, researchers used either the PPML or the Heckman sample selection model to address this issue. As the DID method relies on a linear model, it cannot accommodate a non-linear Poisson specification (see Blundell & Dias, 2009). However, the Heckman selection model first presented by Heckman (1976) offers a solution for using the DID strategy. Firstly, it maintains linearity in the second stage, making it straightforward to interpret the interactive dummies in DID analysis. Secondly, in the first stage (selection model), the interactive dummy is only meaningful in signs, because the probit model maintains strict monotonicity (see Puhani, 2012). Because the outcome equation (second stage) is linear, the DID performs well, and the coefficient in that stage represents the pure influence of Brexit at the extensive margin (see for example, Chen et al., 2018).

The Heckman selection addresses sample selection bias by first estimating a probit model focusing on the choice to trade. An inverse Mills ratio (IMR_{ijt}) variable is derived and integrated into the second-stage outcome equation, with the trade value as the dependent variable. This technique has been used in several research (Grant and Boys, 2012; Disdier et al., 2008; Tran et al., 2012; Xiong and Beghin, 2011; Helpman et al., 2008; Chen et al., 2018). If correlation coefficient (rho) and sigma are statistically significant, it implies that omitting zero values will bias the estimates (see Haq et al., 2010). The framework I used comprised of the Heckman selection model, the DID technique, and the inclusion of four-way fixed effects.

$$\begin{aligned} Pr\left(Import_{(i)(j)(k)(t)} > 0\right) \\ &= (\alpha_i) + \left(\alpha_j\right) + (\alpha_t) + (\alpha_k) + \beta_1 D(Brexit)_t * D(Treated)_i \\ &+ \gamma_{15} D(LANG)_{ij} \sum_{d=1}^{6} \gamma_d X_{(i)(j)(d)(t)} + \varepsilon_{(i)(j)(k)(t)} \end{aligned} \tag{7a}$$

$$\begin{aligned} ln\left(Import_{(i)(j)(k)(t)}\right) \\ &= (\alpha_i) + \left(\alpha_j\right) + (\alpha_t) + (\alpha_k) + \beta_1 D(Brexit)_t * D(Treated)_i \\ &+ \sum_{d=1}^{6} \gamma_d X_{(i)(j)(d)(t)} + \varphi IMR_{(i)(j)(k)(t)} + \varepsilon_{(i)(j)(k)(t)} \end{aligned} \tag{7b}$$

where $X_{(i)(j)(d)(t)}$ includes the variables in eq. (3) without $D(LANG)_{(i)(j)}$. Common language is used for exclusive restriction (i.e., exogenous identifying variable) in the first stage (i.e., selection equation). Because it influences the likelihood of trading rather than the value of trade (see for example, Helpman et al., 2008; Disdier & Marette, 2010; Tran et al., 2012; Grant & Boys, 2012).

I rely on the estimated coefficient (β_1) in the selection equation to evaluate the treatment effect on the extensive margins. This is possible because it represents a non-linear, strictly monotonic transformation function, as Puhani (2012), Karaca-Mandic et al. (2012), and Chen et al., (2018) pointed out. Although the sign of this coefficient is meaningful, its exact numerical value does not carry specific significance. The coefficient of interest is (β_1) in the outcome equation indicates the unaltered influence of Brexit decision on the extensive margin. A similar model is specified for exports.

$$\begin{aligned} Pr\left(Export_{(i)(j)(k)(t)} > 0\right) \\ &= (\alpha_i) + \left(\alpha_j\right) + (\alpha_t) + (\alpha_k) + \beta_1 D(Brexit)_t * D(Treated)_i \\ &+ \gamma_{15} D(LANG)_{ij} \sum_{d=1}^{6} \gamma_d X_{(i)(j)(d)(t)} + \varepsilon_{(i)(j)(k)(t)} \end{aligned} \tag{8a}$$

$$ln\left(Export_{(i)(j)(k)(t)}\right) = (\alpha_{UK}) + (\alpha_j) + (\alpha_t) + (\alpha_k) + \beta_1 D(Brexit)_t * D(Treated)_i + \sum_{d=1}^{6} \gamma_d X_{(i)(j)(d)(t)} + \varphi IMR_{(i)(j)(k)(t)} + \varepsilon_{(i)(j)(k)(t)} \quad (8b)$$

Robustness checks

Difference-in-Differences in Poisson Pseudo-Maximum Likelihood Estimator

For robustness check, I estimated the DID model through the PPML following Melstrom et al. (2018). PPML addresses cases where trade values contain zeros. In OLS zero values are omitted due to the undefined outcomes resulting from logarithmic transformations. Disregarding zero observations in the OLS manner introduces the potential for sample selection bias. The PPML accommodates zero observations without altering the model. Additionally, interpreting coefficients within the PPML is straightforward and aligns with the structure seen in OLS. Even though the dependent variable is defined as absolute values rather than logarithms in this model, coefficients of independent variables that enter the model logarithmically are still interpreted as elasticities. Similarly, coefficients of variables entered as absolute values correspond to semi-elasticities, akin to their interpretation in OLS. The PPML involves three distinctive sets of high-dimensional fixed effects to be computed (exporter FE, importer FE, year FE, and product FE). This is essential for ensuring unbiased estimations and avoiding incorrect conclusions. The variables in this model are like that of eq. (3).

$$Import_{(i)(j)(k)(t)} = exp\left[(\alpha_i) + (\alpha_j) + (\alpha_t) + (\alpha_k) + \beta_1 D(Brexit)_t * D(Treated)_i + \sum_{d=1}^{6} \gamma_d X_{(i)(j)(d)(t)}\right] \times \varepsilon_{(i)(j)(k)(t)} \quad (9)$$

The export model is also specified as

$$Export_{(i)(j)(k)(t)} = exp\left[(\alpha_i) + (\alpha_j) + (\alpha_t) + (\alpha_k) + \beta_1 D(Brexit)_t * D(Treated)_i + \sum_{d=1}^{6} \gamma_d X_{(i)(j)(d)(t)}\right] \times \varepsilon_{(i)(j)(k)(t)} \quad (10)$$

I also estimated the triple difference (DIDID) through the PPML as follows. The variables in this model are like that of eq. (5).

$$Import_{(i)(j)(k)(t)} = exp\left[(\alpha_i) + (\alpha_j) + (\alpha_t) + (\alpha_k) + \beta_1 D(Brexit)_t * D(Treated)_i + \beta_2 D(Brexit)_t * D(Treated)_i * D(Commodity)_i + \sum_{d=1}^{6} \gamma_d X_{(i)(j)(d)(t)}\right] \times \varepsilon_{(i)(j)(k)(t)} \quad (11)$$

The export model is also specified as

$$Export_{(i)(j)(k)(t)} = exp\left[(\alpha_i) + (\alpha_j) + (\alpha_t) + (\alpha_k) + \beta_1 D(Brexit)_t * D(Treated)_i + \beta_3 D(Brexit)_t * D(Treated)_i * D(Commodity)_i + \sum_{d=1}^{6} \gamma_d X_{(i)(j)(d)(t)}\right] \times \varepsilon_{(i)(j)(k)(t)} \quad (12)$$

Synthetic Control Method

I utilized the SCM method (Abadie et al., 2010) as a robustness check to separate the effects of Brexit from other influences. This approach effectively addresses the potential parallel trend assumption that might be violated within the DID framework. Suppose there are $N + 1$ cross-sectional units indexed by $i = 1, \dots, N + 1$ and observed over periods $t = 1, \dots, T_0$ (preintervention) and $t = T_0 + 1, \dots, T$ (postintervention). To simplify notation, assume the first unit with $i = 1$ to be the treated unit (exposed to the intervention), while the other units with $i = 2, \ldots, N+1$ are control units (not exposed to the intervention) that form the "donor pool". Let y_{it}^{1} and y_{it}^{0} be the outcomes of unit i in period t with and without intervention, respectively; the observed outcome y_{it} can then be expressed as

$$\begin{aligned} y_{it} &= y_{it}^{1} D_{it} + y_{it}^{0}(1 - D_{it}) \\ &= y_{it}^{0} + \Delta_{it} D_{it} \end{aligned}$$

where D_{it} is a treatment indicator such that $D_{it} = 1$ if unit i is treated in period t and $D_{it} = 0$ otherwise. $\Delta_{it} = y_{it}^{1} - y_{it}^{0}$ denotes the treatment effect for unit i in period t. The goal is to estimate $(\Delta_{1T_0+1}, \ldots, \Delta_{1T})$, which is equivalent to estimating $(y_{iT_0+1}^{0}, \ldots, y_{1T}^{0})$, because $(y_{iT_0+1}^{1}, \ldots, y_{1T}^{1})$ are observed. Suppose that y_{it}^{0} is generated by a factor model.

$$y_{it}^{0} = \delta_t + \boldsymbol{\theta}_t' z_i + \boldsymbol{\lambda}_t' \boldsymbol{\mu}_i + \varepsilon_{it}$$

where δ_t is a time fixed effect (that is, an unknown common factor with constant factor loadings across units), z_i is a $(K \times 1)$ vector of observed covariates, $\boldsymbol{\theta}_t$ is a $(K \times 1)$ vector of unknown coefficients, $\boldsymbol{\lambda}_t$ is a vector of unobserved common factors, $\boldsymbol{\mu}_i$ is a vector of unknown factor loadings, and ε_{it} is an idiosyncratic shock with a zero mean. The SCM seeks to approximate the unknown $y_{it}^{0} (t = T_0 + 1, \ldots, T)$ by a weighted average of donor units, and the treatment effects are estimated accordingly by

$$\widehat{\Delta}_{1t} = y_{it} - \hat{y}_{1t}^{0} = y_{it} - \sum_{i=2}^{N+1} w_i \, y_{it} \quad (t = T_0 + 1, \ldots, T) \tag{13}$$

Let $\boldsymbol{w} = (w_2, \ldots, w_{N+1})'$ be a $(N \times 1)$ vector of weights (a potential synthetic control) such that $0 \leq w_i \leq 1$ for $i = 2, \ldots, N+1$ and $\sum_{i=2}^{N+1} w_i = 1$. The SCM selects the optimal $\boldsymbol{W}$ so that the pretreatment characteristics of the synthetic control are most like those of the treated unit. Let $\boldsymbol{x}_1$

be the $(K \times 1)$ vector containing the pretreatment covariates (predictors) of the treated unit, which may include pretreatment values of outcome, and let $\boldsymbol{X}_0$ be the $(K \times N)$ matrix containing the pretreatment covariates of the N control units. Moreover, let $\boldsymbol{V}$ be a $(K \times K)$ diagonal matrix with nonnegative elements on its diagonal that contains covariate weights measuring the importance of each covariate in predicting the outcome. I used the notation $\|\mathbf{X}\|\boldsymbol{V} \equiv \sqrt{\mathbf{X}'\boldsymbol{V}\mathbf{X}}$ as a distance measure indexed by $\boldsymbol{V}$. If $\boldsymbol{V}$ is the identity matrix, then it reduces to the usual Euclidean norm $\|\mathbf{X}\| \equiv \sqrt{\mathbf{X}'\mathbf{X}}$**.** The optimal synthetic control $\boldsymbol{W}^*(\boldsymbol{V})$ is obtained by solving the following minimization problem:

$$\boldsymbol{W}^*(\boldsymbol{V}) = \underset{\mathrm{W}}{\operatorname{argmin}}\|\mathbf{X}_{\mathbf{1}} - \boldsymbol{X}_{\mathbf{0}}\boldsymbol{W}\|V$$

Let $\boldsymbol{Z}_1$ be the $(T_0 \times 1)$ vector of pretreatment outcomes for the treated unit and let $\boldsymbol{Z}_0$ be the $(T_0 \times N)$ matrix of pretreatment outcomes for the N control units. Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) present a data driven procedure to choose the optimal $\boldsymbol{V}^*$ that minimizes the MSPE of the outcome variable for the pretreatment period:

$$\boldsymbol{V}^* = \underset{\mathrm{V}}{\operatorname{argmin}}\|\mathbf{Z}_{\mathbf{1}} - \boldsymbol{Z}_0\boldsymbol{W}^*(\mathbf{V})\|$$

Given the $\boldsymbol{V}^*$ containing optimal covariate weights, the optimal unit weights $\boldsymbol{W}^* = \boldsymbol{W}^*(\boldsymbol{V}^*)$ can be computed. Thus, we can use the optimal unit weights $\boldsymbol{W}^*$ to estimate the counterfactual outcome $\hat{y}_{1t}^0$ and the treatment effect $\widehat{\Delta}_{1t} = y_{it} - \hat{y}_{1t}^0$ over the posttreatment period according to (1). I used leave-one-out (LOO), in-time placebo, and in-space placebo tests to assess reliability.

Results and Discussion

This section presents the findings of the study estimated through the Heckman selection model and the PPML, and synthetic control approach for robustness check.

Difference-in-Difference in Heckman Selection Model Results

From the gravity model under Heckman selection model, the main results of the impact of Brexit on UK durable and non-durable imports value from EU and ROW are in Table 2.4. The second column in Table 2.4 contains the results of estimation with the UK imports value from the ROW (both durable and non-durable), the third and fourth columns contain ROW durable and non-durable estimates, respectively. The fifth, sixth, and seventh columns contain estimates on UK total imports value from EU, UK durable imports value from EU, and UK non-durable imports value from EU, respectively. Individual fixed effects compensate for heterogeneity in all models, i.e., importer fixed effects, exporter fixed effects, commodity fixed effects, and year fixed effects. The standard errors (SEs) in all cases are clustered at the paired reporter-partner level (distance between exporter and importer).

The regression diagnostics shows the evidence of self-selection bias in the sample and therefore warrants the use of the Heckman Selection Model as our main model (see Chen et al., 2018). This has been seen in all the other specifications. All the models are statistically significant at the 1% level of significance, according to the Wald test. The Rho and sigma (estimates of selection hazard) are statistically significant in all the models indicating that ignoring zero trade flows would bias the estimates. Hence, in this case, OLS estimates (due to excluding zero trade flows) would be biased. I conclude that the Heckman technique is the best model (see for example, Haq et al., 2010). I also find that the exclusive restriction (instrument), common language, is statistically significant in all the models and has a positive effect on UK imports from the selection model (not reported).

We can see that the estimated coefficients that capture the impact of Brexit: $D(Brexit)_t * D(Treated)_i$ is statistically significant and negative in all the models indicating that Brexit implementation has decreased UK imports value from both the EU and the ROW. The value of UK total imports value from the ROW fell by 0.20%, 0.24% for durable commodities and 0.18% for non-durable commodities. The value of UK total imports value from the EU decreased by 0.41%, 0.39% for durable commodities and 0.41% for non-durable commodities. The result is intriguing as it contradicts the anticipated trade diversion for the UK. There was an expectation that the UK would increase its trade with ROW at the expense of the EU, as indicated in studies like Douch et al. (2020)[22]. The outcome aligns with Dhingra and Sampson's (2022) study, examining the actual economic impact of Brexit. Their findings indicate that, at an aggregate level, there was minimal or no redirection of trade away from the EU. This suggests that several projected long-term repercussions of Brexit did not manifest before the initiation of the new UK-EU trade relationship in 2021. However, there is variation in the impact. For example, durable commodities were impacted slightly higher compared to non-durable commodities in the ROW while the reverse holds for UK imports value from EU. Overall, Brexit has a slightly bigger effect on EU imports value than on ROW imports value.

All the estimated gravity variable coefficients are consistent with theory. The coefficient estimate of distance implies that a 1% increase in distance will lead to a 1.40% or 1.80% decrease in bilateral trade flows (see column 2 to 7). Countries with contiguous borders experience a 58.09% to 104.21% increase in bilateral trade which confirms the insight that significant cross-border trade occurs among trade partners. Countries with a colonial relationship trade 253.96% (see column 2) more than countries without. For comparison, the gravity estimates from Rose (2005) and Fratianni and Kang (2006) show that trade increases between 58.4% and 107.5% for contiguous countries

[22] https://ukandeu.ac.uk/trade-diversion-and-brexit-uncertainties/

and between 191.5% and 568.6% for countries with a colonial relationship. Countries with common colonizer experience 241.78% more than countries without (see column 2).

I also checked whether the transition period affected trade outcomes between UK and the EU and ROW. I present the transition period estimates in Table 2.5. All the models have negatively and statistically significant for $D(Brexit)_t * D(Transit)_i$. Also suggesting that Brexit decrease UK imports value from EU and ROW during the transition period. The value of all goods imported by the UK from the ROW decreased by 0.35%, and 0.29%, and 0.38%, respectively, for durable and non-durable goods. The value of all goods imported by the UK from the EU decreased by 0.26%, and 0.24%, and 0.27%, respectively, for durable and non-durable goods. However, compared to the TCA Brexit, the negative impact is higher for the ROW compared to the EU. Also, the impact of higher for non-durable than the durable in the ROW whiles like the TCA Brexit, the impact is higher in the non-durable compared to the durable in the EU. All the estimated gravity variable coefficients are consistent with theory. The Heckman Selection Model is used to further estimate the triple differences model to obtain the coefficient for $D(Brexit)_t * D(Treated)_i * D(Commodity)_i$. Table 2.6 presents this result. Columns 2–3 represent the estimate for TCA Brexit, whereas columns 4–5 show the estimate for transit Brexit. All the coefficients are statistically significant and positive, as can be seen.

In addition, I carried out analysis with the UK as an exporter. This is illustrated in Table 2.7. Columns 2–4 contain the results of estimation using the U.K. total exports value to the ROW, as well as the durable and non-durable estimates. Columns 4–6 contain estimates for total EU exports value, durable imports, and non-durable exports value to the UK. We can see that all the coefficients, $D(Brexit)_t * D(Treated)_i$ are statistically significant and negative with a larger magnitude when compared to estimates with the UK as an importer. Columns 2 – 4 show that the value of all goods exported by the UK to the ROW decreased by 0.47%, 0.61%, and 0.43%, respectively, for durable and non-durable goods. Columns 5 – 7 shows that the value of all goods exported by the UK to the ROW decreased by 0.86%, 0.64%, and 0.91%, respectively, for durable and non-durable goods.

All the estimated gravity variable coefficients are consistent with theory. Table 2.8 also displays the estimated transition period. All the coefficients, $D(Brexit)_t * D(Transit)$ are negative statistically significant. This shows that UK exports value decreased during the Brexit transition period. What is notable is that exports to the ROW fell more during the transition phase than throughout the TCA era. This is the opposite for the EU. Table 2.9 presents the triple-difference results. The first four columns show the TCA estimates and the last four, the transition estimates. We can see that all the coefficients $D(Brexit)_t * D(Treated)_j * D(Commodity)_i$, $D(Brexit)_t * D(Transit)_i * D(Commodity)_i$ are statistically significant and positive except EU non-durable and EU durable which are not statistically significant.

Difference-in-Differences in Poisson Pseudo-Maximum Likelihood Estimator Results

I also utilized PPML for analysis. Table 2.10 shows the TCA and transition period estimates for both durable and non-durable commodities. TCA results for the EU and the ROW are shown in columns 2–7, whereas transition period results for the EU and the ROW are shown in columns 8–13. $D(Brexit)_t * D(Treated)_i$, and $D(Brexit)_t * D(Transit)_i$ are statistically significant and negative, as were the findings from the Heckman selection Model. This suggests that the TCA and the transition period both reduced UK imports value from the EU and ROW. However, the PPML could not provide statistically significant estimates for the triple-differences coefficient, $D(Brexit)_t * D(Treated)_i * D(Durable)_i$ (see Table 2.11).

In addition, I carried out analysis with the UK as an exporter. Table 2.12 shows these results. $D(Brexit)_t * D(Treated)_i$, and $D(Brexit)_t * D(Transit)_i$ are statistically significant and negative, as were the findings from the Heckman selection Model. This shows that both the TCA and the transition period adversely affected UK exports to the EU and ROW. The findings reveal that Brexit has primarily reduced UK imports and exports values to and from the EU, and ROW in both the transition and TCA periods. During the transition period, UK imports to the Rest of the World (ROW) across all HS2 commodities decreased by an average of 0.27% (see Table 2.10). In the TCA Brexit period, this decline deepened to 0.39%. Meanwhile, within the EU, UK imports value decreased by 0.26% during the transition period and by 0.41% in the TCA Brexit period. Brexit had no statistically significant impact on UK durable goods imports value to ROW, both during the transition period and after the TCA. Non-durable goods imports value from the UK to the ROW fell by 0.46% and 0.47% during the transition and TCA Brexit periods, respectively. In the transition and TCA Brexit periods, UK durable goods imports value to the EU fell by 0.25% and 0.39%, respectively. In the transition and TCA Brexit periods, UK non-durable goods imports value to the EU fell by 0.28% and 0.43%, respectively. The triple-differences results are shown in Table 2.11. Except for the EU durable in the TCA, all the coefficients for the durable in the TCA and transition are statistically significant. The ROW durable in both TCA and transit are positive, however EU transit is negative. Similar results are obtained for the non-durable estimates.

In accordance with Table 2.12, during the transition period, UK exports to ROW dropped by an average of 0.35% across all HS2 goods. During the TCA Brexit period, this reduction reached 0.50%. However, the value of UK exports to the EU fell by 0.45% during the transition phase and by 0.58% over the TCA Brexit period. Durable goods exports value from the UK to the

ROW declined by 0.32% and 0.48%, respectively, during the transition and TCA Brexit periods. During the transition and TCA Brexit periods, the value of non-durable goods exports from the UK to the ROW declined by 0.42% and 0.51%, respectively. During the transition and TCA Brexit periods, the value of UK durable goods exports to the EU fell by 0.47% and 0.54%, respectively. In the transition and TCA Brexit periods, the value of non-durable goods exports from the UK to the EU fell by 0.44% and 0.61%, respectively.

Parallel Trend Test Results

Additionally, I conducted an empirical analysis to assess the validity of the "parallel trend assumption" (as outlined in equation 1). The focal point of this equation is the interaction term between the treatment dummy variable and the linear time trend (Treated × Year). Notably, the analysis revealed that the impact of the interaction term (Treated × Year) on the values of UK and other non-UK countries' imports and exports did not display statistical significance. This suggests that the "parallel trend assumption" holds, as depicted in Table 2.14.

Synthetic Control Method Results

To identify the effects of Brexit from other influences, I employ the synthetic control method (SCM) (Abadie et al., 2010). I first evaluate the impact of Brexit on UK imports value from the EU. Figure 2.1 displays import patterns in the UK from EU and its synthetic counterpart from 2000 to 2021. The solid line reflects the trend in UK import value. The dashed line depicts the estimated UK imports value in the event of a no-deal Brexit. There was a small gap in imports between the synthetic UK and the UK prior to Brexit. As a result, imports value in the synthetic UK closely mirrors imports value in the actual UK prior to the intervention (i.e., Brexit). The treatment effect estimated shows that actual UK imports are lower than they would have been without Brexit.

Figure 2.2 compares the covariate balance of the synthetic and average controls, with the gray vertical line representing the treated unit. This shows that the synthetic control outperforms the donor pool in terms of tracking the treated unit. Table 2.A3 also shows a minor bias between actual UK imports value and the synthetic control compared to the donor pool.

Figure 2.2 also shows the optimal unit weights indicating the UK imports is well predicted by the combination of donor countries, including USA, Canada, and Greece. Figure 2.3 indicates that the average treatment effect is statistically significant and that the decrease in UK imports value from the EU following Brexit was not coincidental but can be attributable to Brexit.

I performed three validation tests: leave-one-out (LOO), in-time placebo, and in-space placebo. Figure 2.4 presents the leave-one-out, and in-time placebo tests. With the leave-one-out, one of the control units with a nonzero weight is left out in turn. Figure 2.4 presents the actual outcomes, predicted outcomes, and LOO predicted outcomes. We can see that the results are qualitatively similar, no matter which control unit with a nonzero weight is excluded. This shows that the results are robust in that the estimated treatment effects are not driven by any control unit.

The in-time placebo tests specify 2011 as the fake treatment time, which is six years earlier than the actual treatment time of 2016. Figure 2.4 presents the gap graph with actual and predicted outcomes, pretending that the treatment starts from 2011. There appear to be some noticeable placebo effects during 2011–2015, when there was in fact no treatment. The in-space placebo test is shown in Figure 2.5. In this test, I use all fake treatment units but exclude those units with pretreatment MSPEs two times larger than that of the treated unit. The results show that UK has the largest post/pre MSPE ratio among all 16 countries, yielding an overall p-value of $1/16 = 0.063$, which is significant at the 10% level. Figure 2.6 depicts the import patterns of the UK from ROW and its synthetic counterpart from 2000 to 2021. As shown in Figure 2.1, UK imports from the ROW fell after Brexit. Figure 2.7 depicts the leave-one-out (LOO), in-time placebo tests, which indicate that whether a control unit is excluded, or the treatment period is backdated, the results are consistent. Figure 2.8 shows the export patterns of the UK to EU and its synthetic counterpart from 2000 to 2021. Figure 2.9 shows that, like Figure 1, UK exports to the EU fell after Brexit. Figure 4 depicts similar results for leave-one-out (LOO) and in-time placebo tests. Figure 10 also shows the export patterns of the UK to ROW and its synthetic counterpart from 2000 to 2021. Figure 2.10 shows that, like Figure 2.9, UK exports to the ROW fell after Brexit. The findings of the leave-one-out (LOO) and in-time placebo testing are shown in Figure 2.11.

Conclusion

In June of 2016, the United Kingdom (UK) voted to leave the European Union (EU), termed "Brexit". Brexit officially occurred on January 31, 2020. In this paper, I examined the impact of actual Brexit on the value of trade flow between the U.K. and the EU, as well as the UK and the rest of the world (ROW). The analysis employed the difference-in-difference method, a quasi-experimental approach integrated into the gravity model framework and estimated using the Heckman Selection Model. Additionally, the Poisson Pseudo-maximum likelihood estimator was utilized for a robustness check. Furthermore, I utilized the synthetic control method to check potential violations of counterfactual assumptions within the difference-in-differences framework, such as the presence of parallel trends.

The result shows that Brexit has reduced the value of U.K. imports and exports to and from the EU, and surprisingly with other countries around the world during the transitional and actual Brexit. Additionally, the value of imports and exports of both durable and non-durable goods from the UK to the EU and the rest of the world have decreased. In the UK, the import value from the EU decreased by 0.41%, whereas imports from the rest of the world dropped by 0.20%.

Specifically, imports of durable goods from the EU and ROW declined by 0.39% and 0.24%, respectively. Non-durable goods also saw decreases in imports, with a 0.41% fall from the EU and a 0.18% drop from the ROW. Conversely, UK exports faced declines as well, with a 0.86% decrease in total export value to the EU and a 0.47% decrease in export value to the ROW. Durable goods exports from the UK experienced a 0.64% decrease to the EU and a 0.61% decrease to the ROW. Non-durable goods exports followed a similar trend, declining by 0.91% to the EU and 0.43% to the ROW. These results are supported by robustness checks using various methods.

The average decrease in trade value between the UK and the EU as well as the UK is slightly higher for non-durable goods compared to durable goods. This suggests that challenges like tariff changes, border delays, and customs issues might be impacting perishable or quickly consumed goods more significantly. To ensure a strong trade environment after Brexit, it is crucial for policies to acknowledge the unique hurdles confronting non-durable goods. By tailoring support to these challenges, policymakers can minimize disruptions, assist affected industries, and uphold a competitive and adaptable trade landscape.

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Tables and Figures

Table 2.1: HS2 Product Classification

HS Code	Product Description
01 – 05	Animal and Animal Products
06 – 15	Vegetable Products
16 – 24	Foodstuffs
25 – 27	Mineral Products
28 – 38	Chemical and Allied Industries
39 – 40	Plastics and Rubbers
41 – 43	Raw Hides, Skins, Leather, and Furs
44 – 49	Wood and Wood Products
50 – 63	Textiles
64 – 67	Footwear and Headgear
68 – 71	Stone and Glass
72 – 83	Metals
84 – 85	Machinery and Electrical
86 – 89	Transportation
90 – 97	Miscellaneous

Table 2.2. Definitions of Variables

Dependent Variables	
$Import_{(i)(j)(k)(t)}$	Imports of product k at the HS2 level by i from country j in year t (in thousands current USD)
$Export_{(i)(j)(k)(t)}$	Exports of product k at the HS2 level by i from country j in year t (in thousands current USD)
Independent Variables	
$DIST_{ij}$	Weighted distance between i and exporting country j (km)
$CONT_{ij}$	1 for country pair share a border;0 otherwise
$LANG_{ij}$	1 for country pair share a language; 0 otherwise
COL_{ij}	1 for pairs in colonial relationship post 1945; 0 otherwise
COM_{ij}	1 for pair colonized by the same country; 0 otherwise
FTA_{ijt}	1 for pair in regional trade agreement in force; 0 otherwise

Table 2.3: Descriptive Statistics

	UK (pre-2016)					Non-UK (pre-2016)				
Variables	**Obs.**	**Mean**	**Std. dev.**	**Min**	**Max**	**Obs.**	**Mean**	**Std. dev.**	**Min**	**Max**
$Import_{(i)(j)(k)(t)}$	317,952	21174.01	308032.5	0	4.14e+07	4,769,280	6499.37	234910.10	0	6.97e+07
$DIST_{ij}$	313,728	7139.77	4093.2	333	18428	4,705,920	7627.45	4359.52	95	19027
$LANG_{ij}$	317,952	0.34	0.47	0	1	4,769,280	0.11	0.32	0	1
$CONT_{ij}$	317,952	0.005	0.07	0	1	4,769,280	0.01	0.12	0	1
COL_{ij}	317,952	0.32	0.46	0	1	4,769,280	0.003	0.06	0	1
COM_{ij}	317,952	0	0	0	0	4,769,280	0.05	0.22	0	1
	UK (post-2016)					**Non-UK (post-2016)**				
$Import_{(i)(j)(k)(t)}$	119,232	23938.14	317010.60	0	2.97e+07	1,788,480	9957.38	319304.10	0	9.96e+07
$DIST_{ij}$	118,656	7127.98	4085.41	336	18413	1,779,840	7621.12	4355.97	95	19031
$LANG_{ij}$	119,232	0.34	0.47	0	1	1,788,480	0.11	0.32	0	1
$CONT_{ij}$	119,232	0.004	0.07	0	1	1,788,480	0.01	0.12	0	1
COL_{ij}	119,232	0.32	0.47	0	1	1,788,480	0.003	0.06	0	1
COM_{ij}	119,232	0	0	0	0	1,788,480	0.05	0.22	0	1
	UK (pre-2016)					**UK (pre-2016)**				
$Export_{(i)(j)(k)(t)}$	316,416	15205.74	168419.90	0	1.66e+07	5,062,656	6149.30	220852.60	0	1.59e+08
$DIST_{ij}$	312,192	7167.943	4083.51	333	18428	4,995,072	7604.38	4353.86	55	19027
$LANG_{ij}$	316,416	.3446602	0.48	0	1	5,062,656	0.11	0.31	0	1
$CONT_{ij}$	316,416	.0048544	0.07	0	1	5,062,656	0.02	0.12	0	1
COL_{ij}	316,416	.317051	0.47	0	1	5,062,656	0.003	0.054	0	1
COM_{ij}	316,416	0	0	0	0	5,062,656	0.05	0.22	0	1
	UK (post-2016)					**UK (post-2016)**				
$Export_{(i)(j)(k)(t)}$	118,656	15257.41	154936.6	0	1.45e+07	1,898,496	10167.84	350247.40	0	1.55e+08
$DIST_{ij}$	118,080	7155.87	4075.75	336	18413	1,889,280	7597.52	4350.14	55	19031
$LANG_{ij}$	118,656	0.34	0.48	0	1	1,898,496	0.106	0.31	0	1
$CONT_{ij}$	118,656	0.005	0.070	0	1	1,898,496	0.02	0.12	0	1
COL_{ij}	118,656	0.32	0.47	0	1	1,898,496	0.003	0.05	0	1
COM_{ij}	118,656	0	0	0	0	1,898,496	0.05	0.22	0	1

Table 2.4: Heckman Selection Model (TCA) Results

Variables	ROW All	ROW Durable	ROW Non-Durable	EU All	EU Durable	EU Non-Durable
$log(Dist_{ij})$	-1.444***	-1.428***	-1.438***	-1.702***	-1.393***	-1.789***
	(0.036)	(0.039)	(0.036)	(0.052)	(0.047)	(0.055)
$D(Brexit)_t * D(Treated)_i$	-0.196**	-0.238**	-0.177*	-0.406***	-0.390***	-0.409**
	(0.085)	(0.097)	(0.092)	(0.151)	(0.136)	(0.161)
$Contiguity_{ij}$	0.714***	0.670***	0.686***	0.596***	0.458***	0.629***
	(0.196)	(0.210)	(0.193)	(0.113)	(0.101)	(0.118)
$Colony_{ij}$	1.264***	1.262***	1.251***			
	(0.085)	(0.092)	(0.088)			
$ComCo_{ij}$	1.229***	1.209***	1.234***			
	(0.101)	(0.114)	(0.103)			
Observations	6,343,680	1,321,600	5,022,080	574,464	119,680	454,784
Importer FE	Yes	Yes	Yes	Yes	Yes	Yes
Exporter FE	Yes	Yes	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rho	0.510***	0.574***	0.480***	0.020***	-0.054***	0.031***
	(0.007)	(0.008)	(0.007)	(0.009)	(0.015)	(0.009)
In(sigma)	1.158***	1.050***	1.176***	0.714***	0.625***	0.726***
	(0.003)	(0.004)	(0.003)	(0.009)	(0.009)	(0.009)
Log pseudo-likelihood Wald test	5172.44***	4163.43***	3757.52***	4.86***	13.13***	11.55***

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

Table 2.5: Heckman Selection Model (Transit) Results

Variables	ROW All	ROW Durable	ROW Non-Durable	EU All	EU Durable	EU Non-Durable
$log(Dist_{ij})$	-1.444***	-1.431***	-1.438***	-1.705***	-1.396***	-1.791***
	(0.036)	(0.040)	(0.036)	(0.053)	(0.048)	(0.056)
$D(Transit)_t * D(Treated)_i$	-0.354***	-0.291***	-0.376***	-0.260**	-0.237**	-0.266**
	(0.069)	(0.073)	(0.076)	(0.123)	(0.109)	(0.131)
$Contiguity_{ij}$	0.714***	0.681***	0.684***	0.600***	0.462***	0.633***
	(0.197)	(0.211)	(0.195)	(0.114)	(0.103)	(0.119)
$Colony_{ij}$	1.263***	1.265***	1.247***			
	(0.086)	(0.094)	(0.089)			
$ComCo_{ij}$	1.226***	1.203***	1.232***			
	(0.104)	(0.117)	(0.106)			
Observations	6,053,376	1,261,120	4,792,256	548,352	114,240	434,112
Importer FE	Yes	Yes	Yes	Yes	Yes	Yes
Exporter FE	Yes	Yes	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rho	0.511***	0.574***	0.482***	0.022**	-0.053***	0.034***
	(0.007)	(0.009)	(0.008)	(0.009)	(0.015)	(0.009)
In(sigma)	1.155***	1.047***	1.173***	0.713***	0.623***	0.725***
	(0.003)	(0.004)	(0.004)	(0.009)	(0.008)	(0.009)
Log pseudo-likelihood Wald test	4986.57***	4036.71***	3603.09***	5.86***	12.67***	12.94***

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

Table 2.6: Heckman Selection Model (Triple Differences) Results

Variables	TCA Brexit		Transit Brexit	
	ROW	**EU**	**ROW**	**EU**
$log(Dist_{ij})$	-1.444***	-1.702***	-1.444***	-1.705***
	(0.036)	(0.052)	(0.036)	(0.053)
$D(Brexit)_t * D(Treated)_i$	-0.301***	-0.457***		
	(0.092)	(0.152)		
$D(Brexit)_t * D(Treated)_i * D(Durable)_i$	0.397***	0.247*	0.562***	0.235**
	(0.100)	(0.134)	(0.087)	(0.112)
$Contiguity_{ij}$	0.714***	0.596***	0.714***	0.600***
	(0.196)	(0.113)	(0.197)	(0.114)
$Colony_{ij}$	1.264***		1.262***	
	(0.085)		(0.086)	
$ComCo_{ij}$	1.229***		1.226***	
	(0.101)		(0.104)	
$D(Transit)_t * D(Treated)_i$			-0.503***	-0.309**
			(0.077)	(0.124)
Observations	6,343,680	574,464	6,053,376	548,352
Importer FE	Yes	Yes	Yes	Yes
Exporter FE	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Rho	0.510***	0.020**	0.511***	0.029**
	(0.007)	(0.009)	(0.007)	(0.009)
In(sigma)	1.158***	0.714***	1.155***	0.713***
	(0.003)	(0.008)	(0.003)	(0.003)
Log pseudo-likelihood Wald test	5172.92***	4.85***	4985.47***	5.82***

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

UK is an Exporter

Table 2.7: Heckman Selection (TCA) Results

Variables	**ROW All**	**ROW Durable**	**ROW Non-Durable**	**EU All**	**EU Durable**	**EU Non-Durable**
$log\left(Dist_{ij}\right)$	-1.617***	-1.486***	-1.641***	-1.503***	-1.433***	-1.522***
	(0.034)	(0.032)	(0.0359)	(0.109)	(0.105)	(0.110)
$D(Brexit)_t$						
$* D(Treated)_i$	-0.474***	-0.605***	-0.430***	-0.858***	-0.642***	-0.913***
	(0.071)	(0.079)	(0.072)	(0.133)	(0.150)	(0.130)
$Contiguity_{ij}$	0.413*	0.276	0.439**	0.665***	0.293**	0.765***
	(0.214)	(0.221)	(0.210)	(0.141)	(0.148)	(0.141)
$Colony_{ij}$	1.515***	1.574***	1.487***			
	(0.074)	(0.077)	(0.076)			
$ComCo_{ij}$	1.498***	1.507***	1.460***			
	(0.095)	(0.099)	(0.101)			
Observations	6,740,160	1,404,200	5,335,960	574,464	119,680	454,784
Importer FE	Yes	Yes	Yes	Yes	Yes	Yes
Exporter FE	Yes	Yes	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rho	0.313***	0.306***		0.070***	0.070***	
	(0.007)	(0.007)		(0.107)	(0.108)	
In(sigma)	0.957***	0.896***		0.833***	0.852***	
	(0.002)	(0.003)		(0.007)	(0.007)	
Log pseudo-likelihood						
Wald test	1972.62***	2137.51***		42.53***	41.87***	

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

Table 2.8: Heckman Selection Model (Transition) Results

Variables	ROW All	ROW Durable	ROW Non-Durable	EU All	EU Durable	EU Non-Durable
$log(Dist_{ij})$	-1.619***	-1.493***	-1.642***	-1.507***	-1.441***	-1.524***
	(0.0350)	(0.033)	(0.0364)	(0.110)	(0.107)	(0.112)
$D(Transit)_t * D(Treated)_j$	-0.504***	-0.552***	-0.486***	-0.445***	-0.429***	-0.449***
	(0.059)	(0.063)	(0.060)	(0.134)	(0.147)	(0.132)
$Contiguity_{ij}$	0.417*	0.279	0.442**	0.655***	0.284*	0.755***
	(0.216)	(0.224)	(0.212)	(0.143)	(0.150)	(0.143)
$Colony_{ij}$	1.516***	1.578***	1.486***			
	(0.075)	(0.077)	(0.076)			
$ComCo_{ij}$	1.493***	1.505***	1.454***			
	(0.097)	(0.100)	(0.102)			
Observations	6,431,712	1,339,940	5,091,772	548,352	114,240	434,112
Importer FE	Yes	Yes	Yes	Yes	Yes	Yes
Exporter FE	Yes	Yes	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rho	0.314***	0.308***	0.302***	0.072***	0.048***	0.071***
	(0.007)	(0.007)	(0.008)	(0.010)	(0.010)	(0.010)
In(sigma)	0.956***	0.894***	0.966***	0.831***	0.730***	0.849***
	(0.003)	(0.004)	(0.003)	(0.007)	(0.008)	(0.007)
Log pseudo-likelihood Wald test	1927.96***	2102.23***	1415.77***	44.29***	8.17***	43.55***

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

Table 2.9: Heckman Selection Model (Triple-difference) Results

Variables	TCA Brexit		Transition	
	ROW	**EU**	**ROW**	**EU**
$log(Dist_{ij})$	-1.617***	-1.503***	-1.619***	-1.507***
	(0.034)	(0.109)	(0.035)	(0.110)
$D(Brexit)_t * D(Treated)_i$	-0.553***	-0.924***		
	(0.071)	(0.135)		
$D(Brexit)_t * D(Treated)_i * D(Durable)_i$	0.332***	0.318***	0.501***	-0.0647
	(0.051)	(0.073)	(0.037)	(0.055)
$Contiguity_{ij}$	0.413*	0.665***	0.417*	0.655***
	(0.214)	(0.141)	(0.216)	(0.143)
$Colony_{ij}$	1.515***		1.516***	
	(0.0744)		(0.0746)	
$ComCo_{ij}$	1.497***		1.493***	
	(0.095)		(0.097)	
$D(Transit)_t * D(Treated)_i$			-0.622***	-0.431***
			(0.059)	(0.135)
Observations	6,740,160	574,464	6,431,712	548,352
Importer FE	Yes	Yes	Yes	Yes
Exporter FE	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Rho	0.313***	0.070***	0.313***	0.070***
	(0.007)	(0.011)	(0.007)	(0.011)
In(sigma)	0.957***	0.833***	0.957***	0.833***
	(0.003)	(0.007)	(0.003)	(0.007)
Log pseudo-likelihood Wald test	1972.38***	42.49***	1972.38***	42.49***

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

Table 2.10: Poisson Pseudo-Maximum Likelihood (TCA) Results

	ROW (TCA)			EU (TCA)			Transition (ROW)			Transition (EU)		
Variables	**ALL**	**Durable**	**Non-Durable**	**ALL**	**EU Durable**	**Non-Durable**	**ALL**	**Durable**	**Non-Durable**	**EU**	**EU Durable**	**EU Non-Durable**
$log\left(Dist_{ij}\right)$	-0.478***	-0.899***	-0.664***	-0.746***	-0.665***	-0.815***	-0.501***	-0.901***	-0.687***	-0.749***	-0.673***	-0.815***
	(0.090)	(0.100)	(0.118)	(0.101)	(0.087)	(0.073)	(0.088)	(0.099)	(0.115)	(0.100)	(0.126)	(0.103)
$D(Brexit)_t * D(Treated)_i$	-0.385***	-0.271	-0.465***	-0.406***	-0.394**	-0.426***						
	(0.106)	(0.181)	(0.108)	(0.052)	(0.153)	(0.114)						
$Contiguity_{ij}$	-0.115	-0.267	-0.503*	-0.140	0.012	-0.219	-0.0770	-0.224	-0.472*	-0.139	0.0148	-0.222
	(0.253)	(0.255)	(0.284)	(0.326)	(0.140)	(0.223)	(0.248)	(0.254)	(0.282)	(0.322)	(0.202)	(0.410)
$Colony_{ij}$	0.602**	0.015	0.938***				0.600**	0.016	0.938***			
	(0.244)	(0.249)	(0.306)				(0.240)	(0.250)	(0.302)			
$ComCo_{ij}$	0.615	-0.321	0.777*				0.572	-0.328	0.728*			
	(0.416)	(0.510)	(0.426)				(0.413)	(0.509)	(0.428)			
$Common\ Language_{ij}$	0.012	0.138	0.050	0.438	0.283**	0.549**	0.019	0.144	0.060	0.432	0.274	0.550
	(0.230)	(0.178)	(0.277)	(0.284)	(0.115)	(0.258)	(0.226)	(0.178)	(0.274)	(0.284)	(0.174)	(0.379)
$D(Transit)_t * D(Treated)_i$							-0.266**	-0.007	-0.462***	-0.264***	-0.249***	-0.283***
							(0.105)	(0.175)	(0.104)	(0.0569)	(0.0706)	(0.0550)
Observations	6,343,680	1,321,600	5,022,080	574,464	119,680	454,784	6,053,376	1,261,120	4,792,256	548,352	114,240	434,112
Importer FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exporter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R2	0.739	0.848	0.725	0.828	0.875	0.771	0.741	0.849	0.728	0.830	0.876	0.773
Likelihood	- 5.760e+07	- 1.370e+07	- 3.460e+07	- 1.240e+07	- 4.489e+06	- 6.979e+06	- 5.260e+07	- 1.270e+07	- 3.130e+07	- 1.150e+07	- 4.187e+06	- 6.430e+06

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

Table 2.11: Poisson Pseudo-Maximum Likelihood (Triple Difference) Results

Variables	TCA		Transition	
	ROW	**EU**	**ROW**	**EU**
$log\left(Dist_{ij}\right)$	-0.478***	-0.746***	-0.501***	-0.749***
	(0.090)	(0.101)	(0.088)	(0.100)
$D(Brexit)_t * D(Treated)_i$	-0.352*	-0.303***		
	(0.213)	(0.106)		
$D(Brexit)_t * D(Treated)_i * D(Durable)_i$	-0.079	-0.201	0.257	-0.0931
	(0.380)	(0.167)	(0.352)	(0.183)
$Contiguity_{ij}$	-0.115	-0.140	-0.077	-0.139
	(0.253)	(0.326)	(0.248)	(0.322)
$Colony_{ij}$	0.602**		0.600**	
	(0.244)		(0.240)	
$ComCo_{ij}$	0.615		0.572	
	(0.416)		(0.413)	
$Common\ Language_{ij}$	0.0118	0.438	0.019	0.432
	(0.230)	(0.284)	(0.226)	(0.284)
$D(Transit)_t * D(Treated)_i$			-0.384**	-0.214**
			(0.167)	(0.094)
Observations	6,343,680	574,464	6,053,376	548,352
Importer FE	Yes	Yes	Yes	Yes
Exporter FE	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Pseudo R2	0.739	0.828	0.741	0.830
Likelihood	-5.760e+07	-1.240e+07	-5.260e+07	-1.150e+07

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

UK as an Exporter

Table 2.12: Poisson Pseudo-Maximum Likelihood (TCA) Results

Variables	**TCA (ROW)**			**TCA (EU)**			**Transition (ROW)**			**Transition (EU)**		
	ALL	**Durable**	**Non-Durable**	**ALL**	**Durable**	**Non-Durable**	**ALL**	**Non-Durable**	**Non-Durable**	**ALL**	**Durable**	**Non-Durable**
$log(Dist_{ij})$	-0.499***	-0.458***	-0.558***	-0.623***	-0.457***	-0.787***	-0.507***	-0.469***	-0.564***	-0.614***	-0.449***	-0.778***
	(0.047)	(0.054)	(0.060)	(0.129)	(0.112)	(0.093)	(0.046)	(0.053)	(0.058)	(0.127)	(0.164)	(0.123)
$D(Brexit)_t * D(Treated)_i$	-0.496***	-0.479***	-0.513***	-0.575***	-0.541***	-0.610***						
	(0.066)	(0.079)	(0.079)	(0.075)	(0.120)	(0.118)						
$Contiguity_{ij}$	0.459**	0.312	0.752***	0.606***	0.391**	0.865***	0.459***	0.314	0.751***	0.621***	0.419	0.863***
	(0.183)	(0.220)	(0.193)	(0.207)	(0.164)	(0.142)	(0.178)	(0.216)	(0.187)	(0.204)	(0.261)	(0.234)
$Colony_{ij}$	0.587***	0.554***	0.495***				0.597***	0.564***	0.508***			
	(0.129)	(0.142)	(0.158)				(0.126)	(0.143)	(0.151)			
$ComCo_{ij}$	1.435***	1.704***	1.210***				1.422***	1.721***	1.181***			
	(0.252)	(0.331)	(0.257)				(0.252)	(0.334)	(0.249)			
$Common\ Language_{ij}$	0.0398	0.0475	0.0706	0.459**	0.653***	0.235	0.0451	0.0507	0.0746	0.457**	0.613***	0.274
	(0.107)	(0.134)	(0.112)	(0.210)	(0.140)	(0.185)	(0.108)	(0.137)	(0.108)	(0.205)	(0.225)	(0.319)
$D(Transit)_t * D(Treated)_i$							-0.354***	-0.321***	-0.418***	-0.454***	-0.473***	-0.439***
							(0.0605)	(0.0744)	(0.0672)	(0.0430)	(0.0604)	(0.0534)
Observations	6,740,160	1,404,200	5,335,960	574,464	119,680	454,784	6,431,712	1,339,940	5,091,772	548,352	114,240	434,112
Importer FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exporter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R2	0.848	0.883	0.793	0.805	0.837	0.753	0.849	0.884	0.793	0.808	0.838	0.756
Likelihood	-3.000e+07	-1.300e+07	-1.530e+07	-1.260e+07	-5.149e+06	-6.982e+06	-2.760e+07	-1.200e+07	-1.410e+07	-1.160e+07	-4.747e+06	-6.412e+06

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

Table 2.13: Poisson Pseudo-Maximum Likelihood (Triple -Differences) Results

Variables	TCA		Transition	
	ROW	EU	ROW	EU
$log(Dist_{ij})$	-0.499***	-0.623***	-0.507***	-0.614***
	(0.047)	(0.129)	(0.046)	(0.127)
$D(Brexit)_t * D(Treated)_i$	-0.665***	-0.460***		
	(0.121)	(0.146)		
$D(Brexit)_t * D(Treated)_i * D(Durable)_i$	0.277*	-0.227	0.377**	-0.306**
	(0.157)	(0.207)	(0.152)	(0.148)
$Contiguity_{ij}$	0.459**	0.606***	0.459***	0.621***
	(0.183)	(0.207)	(0.178)	(0.204)
$Colony_{ij}$	0.587***		0.597***	
	(0.129)		(0.126)	
$ComCo_{ij}$	1.435***		1.422***	
	(0.252)		(0.252)	
$Common\ Language_{ij}$	0.0398	0.459**	0.0451	0.457**
	(0.107)	(0.210)	(0.108)	(0.205)
$D(Transit)_t * D(Treated)_i$			-0.588***	-0.301***
			(0.0978)	(0.0875)
Observations	6,740,160	574,464	6,431,712	548,352
Importer FE	Yes	Yes	Yes	Yes
Exporter FE	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Pseudo R2	0.848	0.805	0.849	0.808
Likelihood	-3.000e+07	-1.260e+07	-2.760e+07	-1.160e+07

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

Table 2.14: Parallel Trend Test Results

Variables	Log Imports Value	Log Exports Value
Treated	7.2*	135.07
	(43.3)	(181.6)
Treated × Year	-0.035	-0.066
	(0.022)	(0.090)
Year (Coded as 1 to 16)	0.075***	0.097***
	(0.021)	(0.022)
Constant	-134.10	-178.47
	(41.42)	(44.05)

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

Synthetic Control Method Results

UK as Importer Estimates

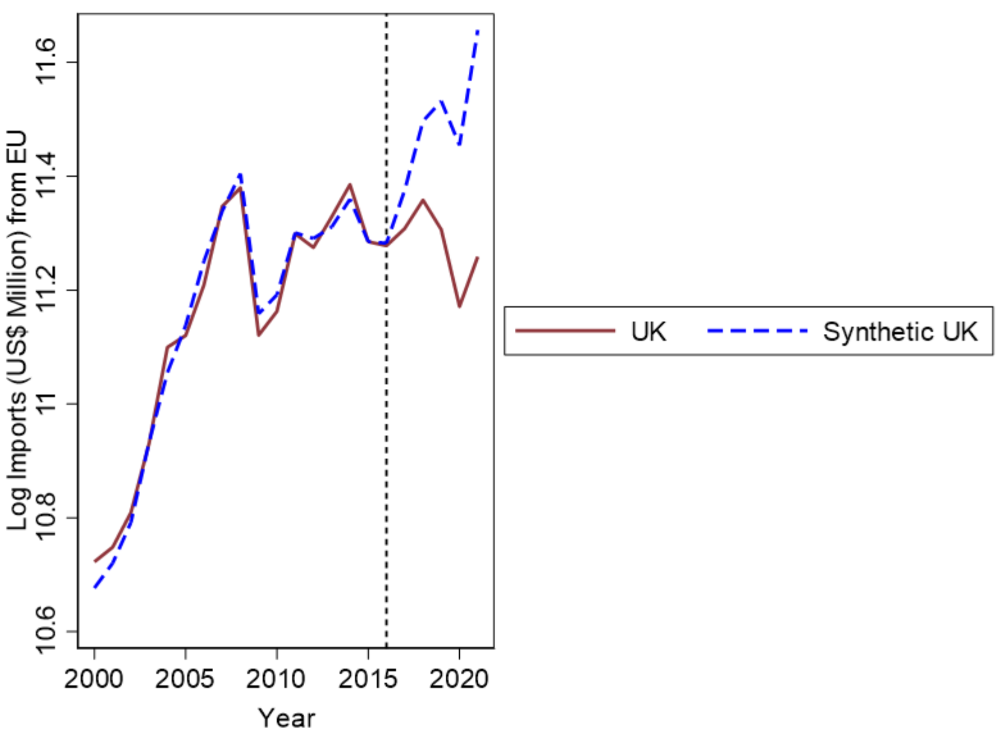

Figure 2.1: Actual and Predicted Paths (Imports from EU)

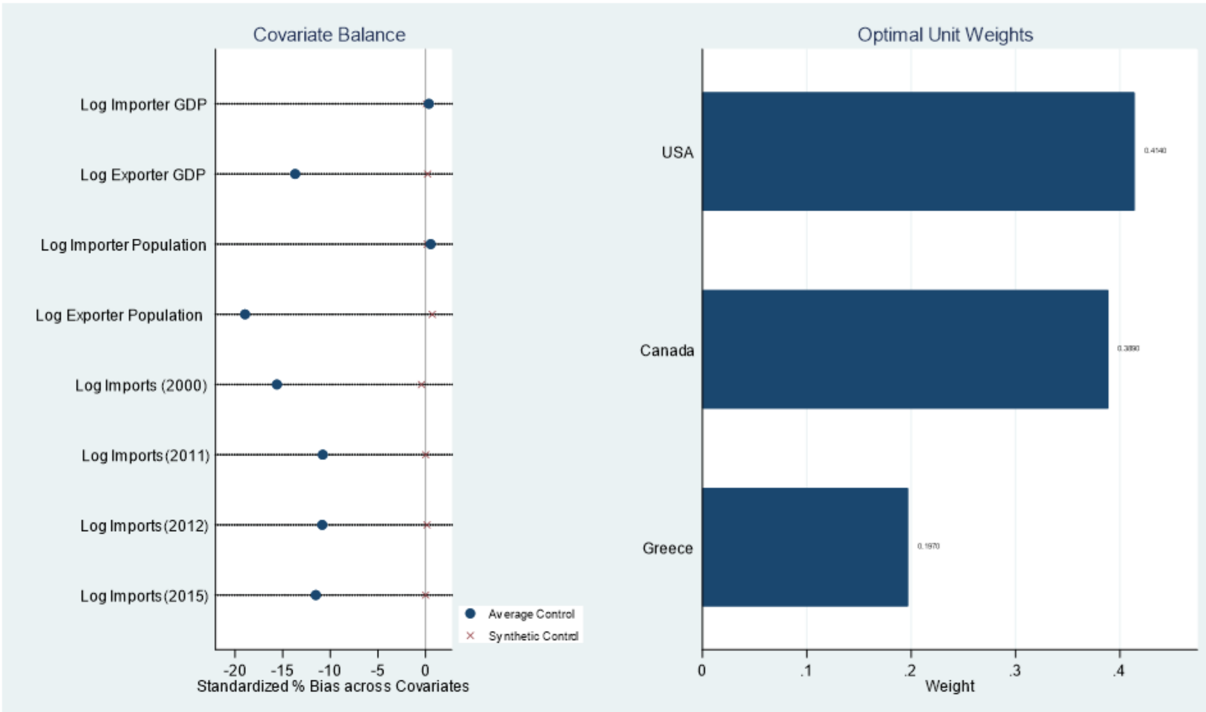

Figure 2.2: Covariate Balance and Optimal Unit Weight (Imports from EU)

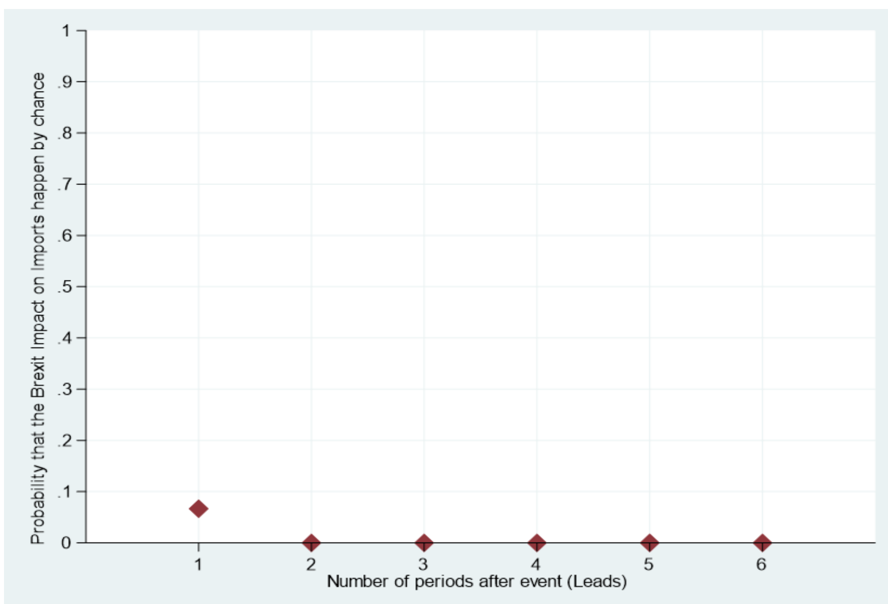

Figure 2.3: Probability Values of Average Treatment Effect (Imports from EU)

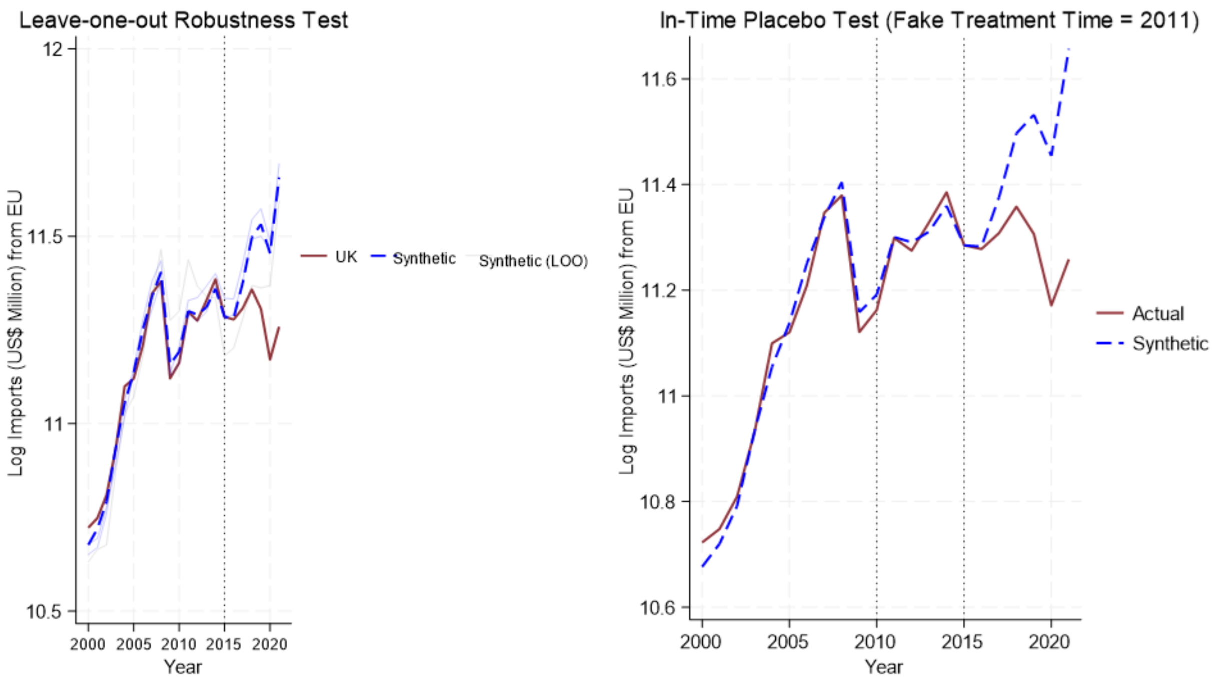

Figure 2.4: Actual and Predicted Path (Imports from EU)

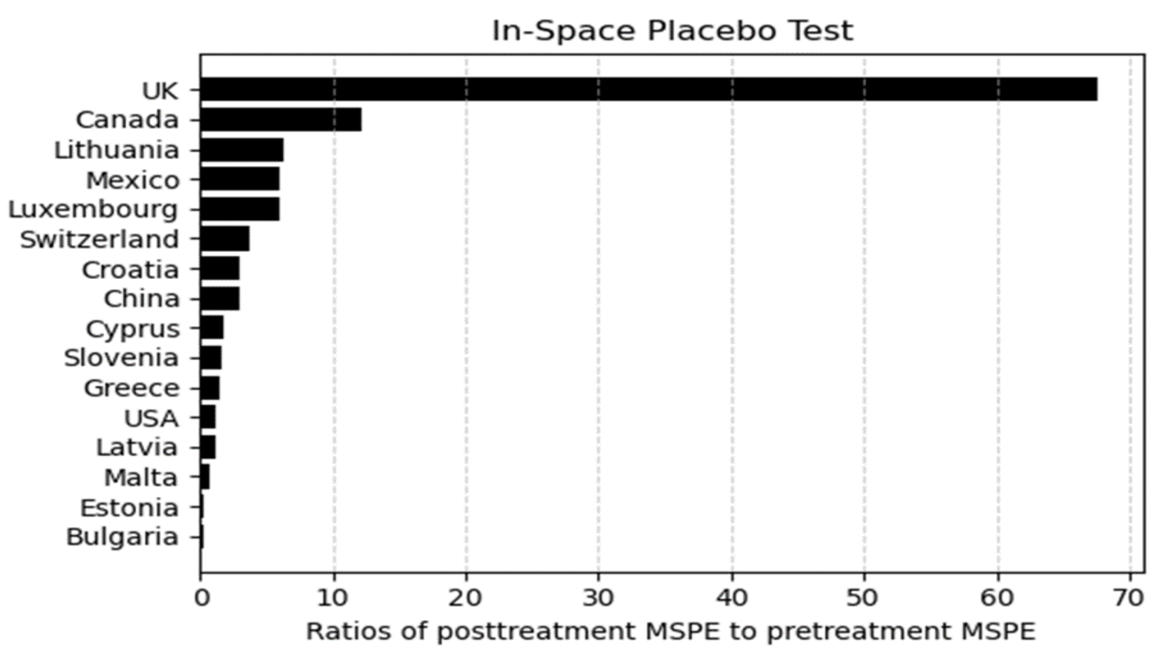

Figure 2.5: In-Space Placebo Test

Note: Using all control units, the probability of obtaining a post/pretreatment MSPE ratio as large as UK's is 0.0625.

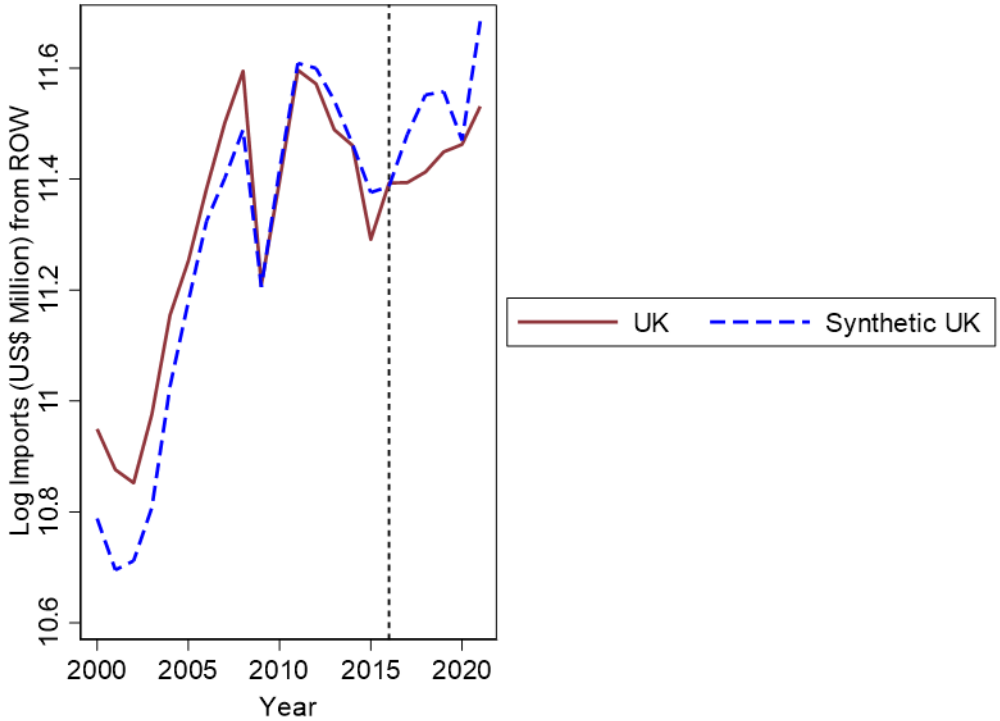

Figure 2.6: Actual and Predicted Paths (Imports from ROW)

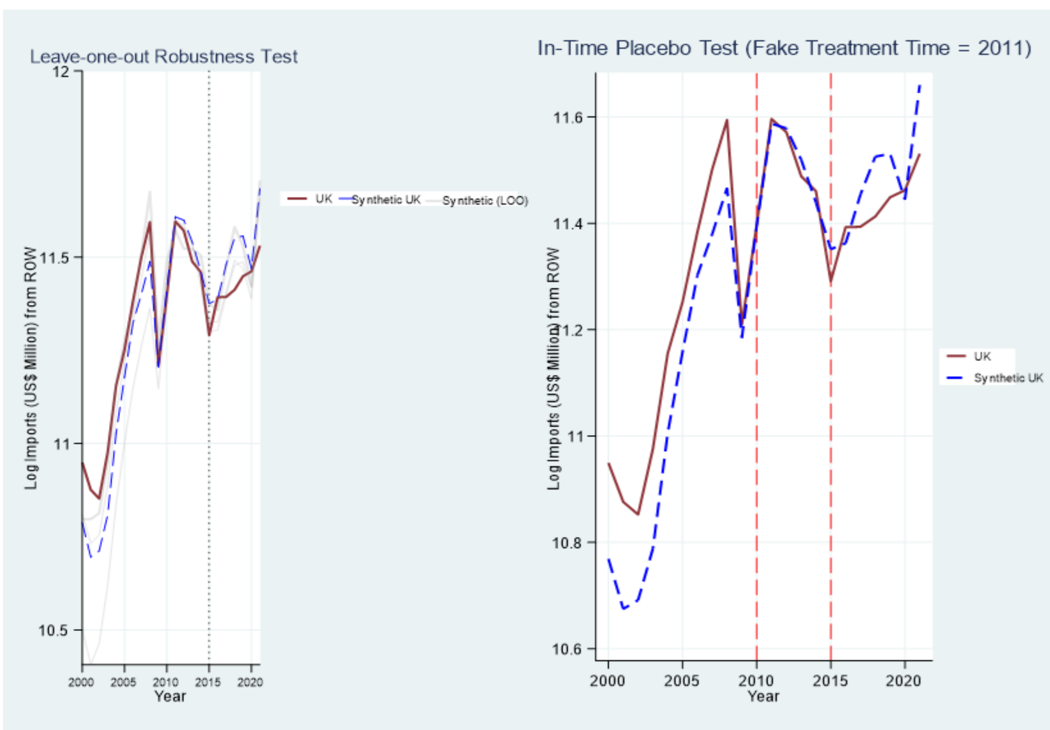

Figure 2.7: Actual and Predicted Path (Imports from ROW)

UK as an Exporter

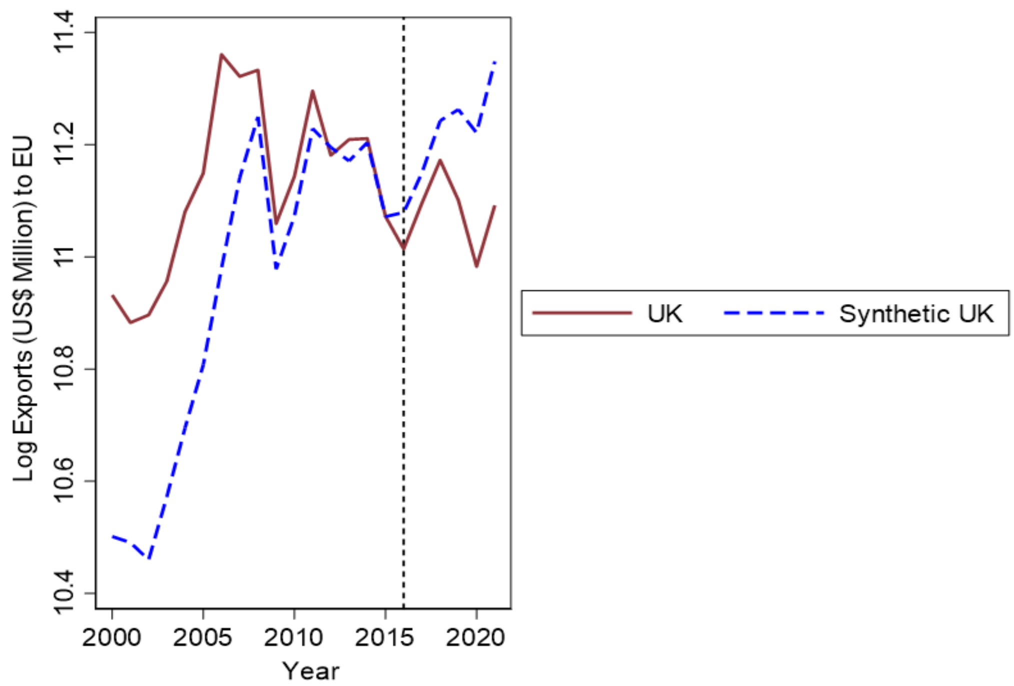

Figure 2.8: Actual and Predicted Paths (Exports to EU)

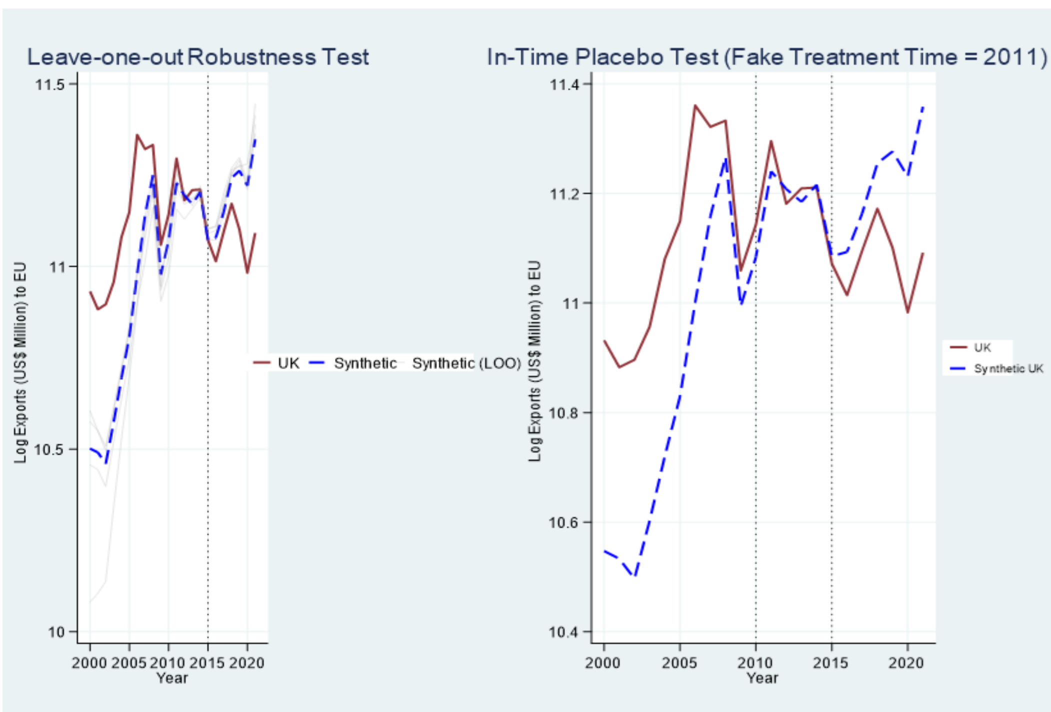

Figure 2.9: Actual and Predicted Path (Exports to EU)

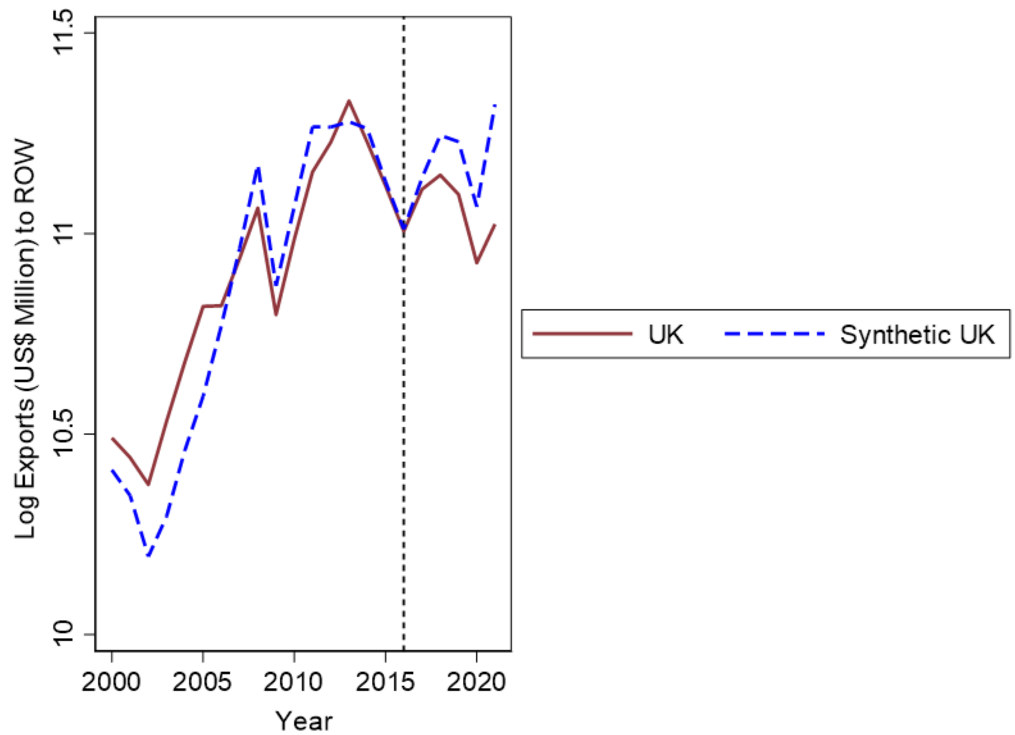

Figure 2.10: Actual and Predicted Paths (Exports to ROW)

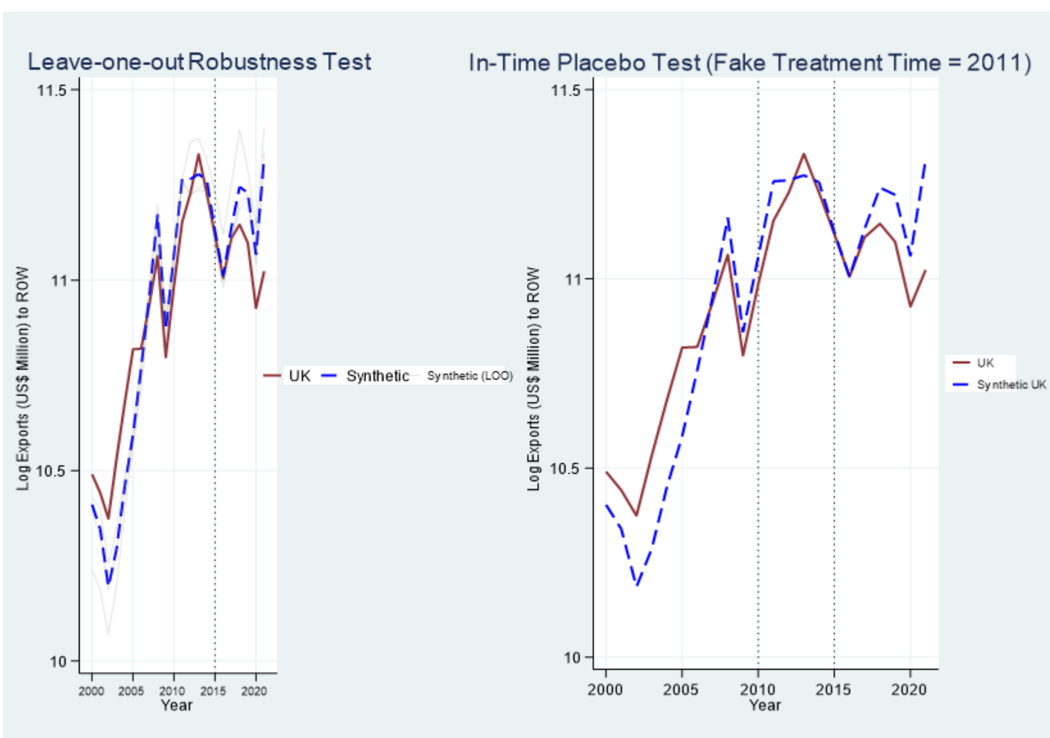

Figure 2.11: Actual and Predicted Path (Exports to ROW)

Appendix

Table 2.A1: Country List

UK as an Importer (Country *j*)					UK as an Importer (Country *i*)				
Reporter	**Partner**	**Partner**	**Partner**	**Partner**	**Partner**	**Reporter**	**Reporter**	**Reporter**	**Reporter**
Bulgaria	Afghanistan	Dominica	Morocco	Spain	Bulgaria	Afghanistan	Ethiopia	Netherlands	Thailand
Canada	Albania	Dominican	Mozambique	South Sudan	Canada	Albania	Eritrea	Curacao	Togo
China	Algeria	Republic	Oman	Suriname	China	Algeria	Falkland	Aruba	Tokelau
Croatia	American	Ecuador	Namibia	Eswatini	Croatia	American	Islands	Sint Marteen	Tonga
Cyprus	Samoa	El Salvador	Nauru	Sweden	Cyprus	Samoa	Fiji	Bonaire, Sint	Trinidad and
Estonia	Andorra	Equatorial	Nepal	Syria	Estonia	Andorra	Finland	Eustatius and	Tobago
Greece	Angola	Guinea	Netherlands	Tajikistan	Greece	Angola	France	Saba	United Arab
Latvia	Antigua and	Ethiopia	Curacao	Thailand	Latvia	Antigua and	French	New Caledonia	Emirates
Lithuania	Barbuda	Eritrea	Aruba	Togo	Lithuania	Barbuda	Polynesia	Vanuatu	Tunisia
Luxembourg	Azerbaijan	Falkland	Sint Marteen	Tokelau	Luxembourg	Azerbaijan	Djibouti	New Zealand	Turkey
Malta	Argentina	Islands	Bonaire, Sint	Tonga	Malta	Argentina	Gabon	Nicaragua	Turkmenistan
Mexico	Australia	Fiji	Eustatius and	Trinidad and	Mexico	Australia	Georgia	Niger	Turks and
Slovenia	Austria	Finland	Saba	Tobago	Slovakia	Austria	Gambia	Nigeria	Caicos Islands
Switzerland	Bahamas	France	New	United Arab	Slovenia	Bahamas	Palestine	Niue	Tuvalu
UK	Bahrain	French	Caledonia	Emirates	Switzerland	Bahrain	Germany	Norfolk Island	Uganda
USA	Bangladesh	Polynesia	Vanuatu	Tunisia	UK	Bangladesh	Ghana	Norway	Ukraine
	Armenia	Djibouti	New Zealand	Turkey	USA	Armenia	Gibraltar	Northern	North
	Barbados	Gabon	Nicaragua	Turkmenistan		Barbados	Kiribati	Mariana Islands	Macedonia
	Belgium	Georgia	Niger	Turks and		Belgium	Greenland	Micronesia	Egypt
	Bermuda	Gambia	Nigeria	Caicos Islands		Bermuda	Grenada	Marshall	Tanzania
	Bhutan	Palestine	Niue	Tuvalu		Bhutan	Guam	Islands	Burkina Faso
	Bolivia	Germany	Norfolk	Uganda		Bolivia	Guatemala	Palau	Uruguay
	Bosnia and	Ghana	Island	Ukraine		Bosnia and	Guinea	Pakistan	Uzbekistan
	Herzegovina	Gibraltar	Norway	North		Herzegovina	Guyana	Panama	Venezuela
	Botswana	Kiribati	Northern	Macedonia		Botswana	Haiti	Papua New	Wallis and
	Brazil	Greenland	Mariana	Egypt		Brazil	Honduras	Guinea	Futuna
	Belize	Grenada	Islands	Tanzania		Belize	Hong Kong	Paraguay	Samoa
	British Indian	Guam	Micronesia	Burkina Faso		British	Hungary	Peru	Yemen
	Ocean	Guatemala	Marshall	Uruguay		Indian	Iceland	Philippines	Serbia and
	Territory	Guinea	Islands	Uzbekistan		Ocean	India	Pitcairn Islands	Montenegro
	Solomon	Guyana	Palau	Venezuela		Territory	Iran	Poland	Zambia
	Islands	Haiti	Pakistan	Wallis and		Solomon	Iraq	Portugal	
	British Virgin	Honduras	Panama	Futuna		Islands	Ireland	Guinea-Bissau	
	Islands	Hong Kong		Samoa			Israel	Timor-Leste	

Brunei
Myanmar
Burundi
Belarus
Cambodia
Cameroon
Cape Verde
Cayman
Islands
Central
African
Republic
Sri Lanka
Chad
Chile
Christmas
Island
Cocos
(Keeling)
Islands
Colombia
Comoros
Mayotte
Congo, Rep.
of the
Congo,
Democratic
Rep. of the
Cook Islands
Costa Rica
Cuba
Czech
Republic
Benin
Denmark
Hungary
Iceland
India
Iran
Iraq
Ireland
Israel
Italy
Cote d'Ivoire
Jamaica
Japan
Kazakhstan
Jordan
Kenya
North Korea
South Korea
Kuwait
Kyrgyzstan
Laos
Lebanon
Lesotho
Liberia
Libya
Macao
Madagascar
Malawi
Malaysia
Maldives
Mali
Mauritania
Mauritius
Mongolia
Moldova
Montenegro
Montserrat
Papua New
Guinea
Paraguay
Peru
Philippines
Pitcairn
Islands
Poland
Portugal
Guinea-
Bissau
Timor-Leste
Qatar
Romania
Russia
Rwanda
Saint Helena
Saint Kitts
and Nevis
Anguilla
Saint Lucia
Saint Pierre
and Miquelon
Saint Vincent
and the
Grenadines
San Marino
Sao Tome and
Principe
Saudi Arabia
Senegal
Serbia
Seychelles
Sierra Leone
Singapore
Slovakia
South
Vietnam
Somalia
South Africa
Yemen
Serbia and
Montenegro
Zambia

British
Virgin
Islands
Brunei
Myanmar
Burundi
Belarus
Cambodia
Cameroon
Cape Verde
Cayman
Islands
Central
African
Republic
Sri Lanka
Chad
Chile
Christmas
Island
Cocos
(Keeling)
Islands
Colombia
Comoros
Mayotte
Congo, Rep.
Congo, DR.
Cook
Islands
Costa Rica
Cuba
Czech
Republic
Benin
Denmark
Dominica
Dominican
Republic
Ecuador
Italy
Cote d'Ivoire
Jamaica
Japan
Kazakhstan
Jordan
Kenya
North Korea
South Korea
Kuwait
Kyrgyzstan
Laos
Lebanon
Lesotho
Liberia
Libya
Macao
Madagascar
Malawi
Malaysia
Maldives
Mali
Mauritania
Mauritius
Mongolia
Moldova
Montenegro
Montserrat
Morocco
Mozambique
Oman
Namibia
Nauru
Nepal
Qatar
Romania
Russia
Rwanda
Saint Helena
Saint Kitts and
Nevis
Anguilla
Saint Lucia
Saint Pierre and
Miquelon
Saint Vincent
and the
Grenadines
San Marino
Sao Tome and
Principe
Saudi Arabia
Senegal
Serbia
Seychelles
Sierra Leone
Singapore
South Vietnam
Somalia
South Africa
Zimbabwe
Spain
South Sudan
Suriname
Eswatini
Sweden
Syria
Tajikistan

	Zimbabwe	El Salvador Equatorial Guinea

Table 2.A2: Crosswalk from HS2 Codes to Broad Product Categories

HS2 – digit Code	Description	Broad Category	HS2 – digit Code	Description	Broad Category
01	Live animals	Non – Durable	51	Wool, fine, animal hair	Non - Durable
02	Meat	Non – Durable	52	Cotton	Non-Durable
03	Fish	Non – Durable	53	Other textile fibers	Non – Durable
04	Dairy	Non-durable	54	Man-made filaments	Non – Durable
05	Product of animal origin	Non-durable	55	Man-made staple fibers	Non – Durable
06	Trees and plants	Durable	56	Wadding	Non – Durable
07	Edible vegetables	Non-durable	57	Carpets	Durable
08	Edible fruit	Non-durable	58	Special woven fabrics	Non – durable
09	Coffee, tea	Non-durable	59	Laminated textile fabrics	Non – Durable
10	Cereals	Non-durable	60	Knitted or crocheted fabrics	Non – durable
11	Products of milling industry	Non – Durable	61	Apparel knitted	Non – Durable
12	Oil seeds	Non-durable	62	Apparel not knitted	Non – Durable
13	Vegetable extracts	Non-durable	63	Other textile fabrics	Non – Durable
14	Other vegetable products	Non-durable	64	Footwear	Non – Durable
15	Animal or vegetable fats and oils	Non-durable	65	Headgear	Non - Durable
16	Preparations of meat	Non-durable	66	Umbrellas	Non-Durable
17	Sugar	Non-durable	67	Articles of feathers	Non-Durable
18	Cocoa	Non-durable	68	Articles of stone, cement	Non-Durable
19	Pastrycook's products	Non-durable	69	Ceramic	Durable
20	Preparations of vegetable, fruit	Non-durable	70	Glass, glassware	Durable
21	Miscellaneous edibles	Non-durable	71	Precious metals	Durable
22	Beverages	Non-durable	72	Iron, steel	Durable
23	Residues and waste from food industry	Non – Durable	73	Articles of iron, steel	Non – Durable
24	Tobacco	Non-durable	74	Copper and articles thereof	Non-Durable
25	Salt, sulphur	Non-durable	75	Nickel and articles thereof	Non – Durable
26	Ores, slag and ash	Non – Durable	76	Aluminum and articles thereof	Non – Durable
27	Mineral fuels, oils	Non – Durable	77	Lead and articles thereof	Non – Durable
28	Inorganic chemicals	Non – Durable	78	Zinc and articles thereof	Non – Durable
29	Organic chemicals	Non – Durable	79	Tin and articles thereof	Non – Durable
30	Pharmaceutical products	Non – Durable	80	Other base metals and articles thereof	Non – Durable

31	Fertilizers	Non – Durable	81	Tools, cutlery	Durable
32	Tanning, dyeing extracts	Non – Durable	82	Miscellaneous articles of base metal	Non – Durable
33	Essential oils and resinoids	Non – Durable	83	Machinery and appliances	Durable
34	Soap, organic surface-active agents	Non – Durable	84	Electrical machinery	Durable
35	Albuminoidal substances	Non – Durable	85	Railway or tramway locomotives	Durable
36	Explosives	Non – Durable	86	Vehicles other than railway	Durable
37	Photographic goods	Non – Durable	87	Aircraft, spacecraft and parts thereof	
38	Miscellaneous chemical products	Non – Durable	88	Ships, boats	Durable
39	Plastics	Non – Durable	89	Optical measuring medical instruments	Durable
40	Rubber	Non – Durable	90	Clocks, watches	Durable
41	Raw hides and skins	Non – Durable	91	Musical instruments	Durable
42	Articles of leather	Non – Durable	92	Arms, ammunition	Durable
43	Furskins and artificial fur	Non – Durable	93	Furniture	Durable
44	Wood	Non – Durable	94	Toys, games, sports requisites	Non – Durable
45	Cork	Non – Durable	95	Miscellaneous manufacturing	Non – Durable
46	Plaiting material	Non – Durable	96	Works of art	Durable
47	Pulp of wood	Non – Durable			
48	Paper and paperboard	Non – Durable			
49	Books, newspapers	Durable			
50	Silk	Non - Durable			

Source: Liu et al., (2022).

Table 2.A3: Covariate Balance in the Pretreatment Periods

Covariate	V.weight	Treated	Synthetic Control		Average Control	
			Value	Bias	Value	Bias
Log Importer Population	0.0001	10.0933	10.1091	0.16%	10.1477	0.54%
Log Importer GDP	0.0015	20.3889	20.4101	0.10%	20.46	0.35%
Log Exporter Population	0.0002	11.0296	11.107	0.70%	8.9386	-18.96%
Log Exporter GDP	0.0004	21.572	21.6186	0.22%	18.6182	-13.69%
Log Imports (2000)	0.0193	10.7226	10.6767	-0.43%	9.0486	-15.61%
Log Imports (2011)	0.037	11.299	11.3006	0.01%	10.0793	-10.79%
Log Imports (2012)	0.0713	11.2748	11.2907	0.14%	10.0508	-10.86%
Log Imports (2015)	0.87	11.2851	11.285	0.00%	9.9843	-11.53%

Note: "V.weight" is the optimal covariate weight in the diagonal of V matrix; "Synthetic Control" is the weighted average of donor units with optimal weights; "Average Control" is the simple average of all control units with equal weights; Root Mean Squared Error: 0.0267; R-squared = 0.986

Chapter 3

US Trade Policy and Public Health: Aggregated and Heterogeneous Effects from the North American Free Trade Agreement[23,24]

Introduction

In 1994, North American Free Trade Agreement (NAFTA from here forth) was implemented to encourage trilateral trade among Canada, Mexico, and the United States by removing tariffs and restrictions. Before 1994, agricultural exports between Mexico and the United States had permit requirements that were changed to tariffs and non-tariff quotas after NAFTA (Zahniser & Link, 2002). In 2008, the United States signed an unrestricted sugar trade agreement with Mexico as part of NAFTA. Before this sugar agreement in 2008, the United States imported a small amount of sugar from Mexico.

Between 2008 and 2014, sugar imports from Mexico got unimpeded entrance to the United States' consumption market. In 2013, sugar imports from Mexico were 2 million short tons, raw value, which formed about 66 % of total United States sugar imports. The 2008 NAFTA sugar trade agreement led to a substantial decrease in price of sugar for United States. This led to an average annual increase in consumer excess of about \$1.67 billion (Sinclair and Countryman 2019). Studies such as Rao and McLaughlin, (2021), and Uppal et al. (2022) show that diabetes rate in the United States has been on the rise immediately after 2008. The Health Care Cost Institute (2013) found the crude diabetes prevalence (both type 1 and 2) in the United States rose from 7.3% to 10.1% between 2008 and 2012.

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Although there has been much discussion about the possibility of NAFTA contributing to the upsurge in diabetes rate in the United States, hitherto, no empirical work has verified this hypothesis. In recent years, global public health has emerged as a primary concern for policy makers and governments worldwide. Within the field of public health, the impact of sugar consumption on health outcomes, such as type 2 diabetes and obesity, has garnered substantial attention from health economists and analysts. Researchers such as Hu and Malik (2010) and Malik et al. (2010) have demonstrated that the consumption of sugar-sweetened beverages is linked to various diseases, including type 2 diabetes, dental caries, and obesity. De Vogli et al. (2014) evaluated the influence of fast-food consumption on the mean population BMI and found that each unit increase in annual fast-food transactions per capita was related with a 0.033 kg/m^2 rise in age-standardized BMI.

In response to this concern, the World Health Organization (WHO, 2013) has recommended a reduction in added sugar consumption to improve global public health. As a result, governments worldwide have implemented policies aimed at reducing the amount of sugar in food and beverages, as noted by Stanner and Spiro (2020). For example, policymakers in the United Kingdom have developed strategies to minimize sugar consumption and lower childhood obesity (H.M. Government, 2016). Previous studies have demonstrated that sugar taxes can significantly reduce the prevalence of obesity, type 2 diabetes, and other diseases associated with the consumption of sugar-sweetened beverages (Cawley & Frisvold, 2015; Fernandez & Raine, 2019; Jou & Techakehakij, 2012; Miljkovic et al., 2008; Nakhimovsky et al., 2016). For example, in a systematic review and meta-analysis of the impact of SSB taxes on population health and nutrition, Andreyeva et al. (2022) discovered that such taxes were linked to increased prices and decreased sales of taxed beverages. Another meta-analysis found that a 10% SSB tax resulted in a 10.0%

decrease in beverage purchases and dietary intake (Teng et al., 2019). Furthermore, the two-tier soft drink industry levy (SDIL) was linked to a decline in obesity prevalence in six-year-old girls (Rogers et al., 2023). Colchero et al. (2016) examines the impact of Mexico's sugar-sweetened beverage tax on purchases from stores and highlights a reduction in sales of taxed beverages while also reporting an increase in sales of untaxed beverages. Silver (et al., 2017) investigates the changes in prices, sales, consumer spending, and beverage consumption one year after the implementation of a tax on sugar-sweetened beverages in Berkeley, California, suggesting that such taxes can successfully reduce consumption of unhealthy beverages.

However, little is known about the potential impact of decreasing sugar prices resulting from international trade agreements. In international trade, reduced trade barriers result in increased imports and decreased commodity prices in the importer country. Trade liberalization can encourage competition leading to increased productivity and a reduction in prices and markups (Chen et al., 2009; Gonzalez-Garcia & Yang, 2022). A systematic review on the relationship between trade liberalization and health identified four key contexts through which liberalization may have an impact on health: increased flows of goods and people, trade in agricultural products, structural adjustment policies, and labor markets (McNamara, 2017). For example, prior studies have found that trade openness could potentially increase availability and consumption of products that can harm public health, such as tobacco (Immurana et al., 2021), alcohol (Milsom et al., 2021), and ultra-processed foods (Baker et al., 2014), thus highlighting the importance of proper planning to manage risks regarding unintended health outcomes (Walls et al., 2015). For instance, An et al. (2019) evaluated the longitudinal relationship between trade openness and obesity rate across 175 countries between 1975 and 2016 and found that a 10% increase in the openness index was linked to a 0.8% increase in the obesity rate. As a result, a bilateral trade agreement that removes tariff and non-tariff barriers on sugar can act as a sugar subsidy and imperil public health as sugar consumption rises (Cernat et al., 2021).

The current study investigates the potential causal impact of the North American Free Trade Agreement's (NAFTA) unrestricted sugar trade agreement on public health, specifically with regards to diabetes prevalence in the United States. Notably, to the best of our knowledge, this is the first study to evaluate the effect of NAFTA on public health in the United States. Although unrestricted trade agreements have well-known benefits, such as boosting economic development, lessening government expenditure, and facilitating technology transfer, their unintended consequences on public health are frequently disregarded. A rare exception is the study by Baggio and Chong (2020), which examines the relationship between engaging in free trade agreements with the United States and the prevalence of obesity among adults. In contrast, our study focuses specifically on the potential causal impact of the North American Free Trade Agreement's (NAFTA) unrestricted sugar trade agreement on public health, particularly diabetes prevalence in the United States. Therefore, our study is more limited in scope and specifically targets the effects of NAFTA, whereas the study by Baggio and Chong (2020) provides a more general analysis of the impact of free trade agreements.

In this study, we utilize the synthetic control method (SCM) - a quasi-experimental technique that allows us to make reliable causal inferences about the aggregate impact of NAFTA on sugar consumption and crude diabetes prevalence in the United States. Traditional comparative case study methods are limited in their ability to accurately quantify the impact of trade agreements on public health, as the selection of control groups can be ad-hoc, leading to uncertainties in the validity of the counterfactual. In contrast, the SCM enables us to construct suitable counterfactuals systematically and transparently through a "weighted average" of similar but untreated comparison units.

To assess heterogeneity in the impact of NAFTA unrestricted sugar trade agreement across US states, we applied a difference-in-difference (DD) research design – another quasi-experimental method commonly used to evaluate the effects of policies or programs. The DD estimate allows us to compare the differences in outcomes before and after the treatment (difference one) between a group exposed to the treatment and a control group (difference two). Additionally, we conducted a panel-event study, where the "event" was the date of implementation of the NAFTA sugar agreement.

Our research reinforces the enduring critique that the NAFTA-driven unrestricted sugar trade agreement poses a significant threat to public health. Our findings affirm the concerns surrounding the potential risks to public health brought about by the unregulated sugar trade agreement within NAFTA. We find that sugar consumption in the United States increased by an average of 16% per year after the agreement was signed, corresponding to 5240 g per capita. Crude prevalence of diabetes increased by an average of 1% per year for the United States population after the agreement was signed, with an increase of about 1% and 2% for men and women, respectively. This unintended consequence of NAFTA has had an estimated economic cost of $324.37 million per year.

We observed an increase in diabetes prevalence ranging from 0.54% to 2.3% across various states within the United States following the implementation of the trade agreement. We find that states with a higher percentage of their population below the poverty level, a greater percentage of Black population, and a lower percentage of high school graduates were associated with greater increases in diabetes prevalence because of the NAFTA sugar trade agreement.

Pathways of Trade Liberalization and Public Health

Trade policies have a substantial impact on power dynamics, wealth distribution, and resource allocation, which influence working conditions, health choices, and overall well-being (Labonté and Schrecker ,2009). When trade liberalization is well-executed, it can boost economic growth by expanding export and investment options. In theory, this can help alleviate poverty and promote human health by improving economic stability, labor standards, access to affordable healthcare, and nutrition (Stevens et al., 2013). Poorly implemented trade policies and agreements, on the other hand, have been proven to heighten power, money, and resource distribution inequality between and within nations, having a negative impact on health and health equality (Friel et al., 2013).

Increased trade and investment in health-harming goods like tobacco, alcohol, sugar, sugar-sweetened beverages, and highly processed foods have occurred concurrently with the rise of Free Trade Agreements, which demand changes in domestic policies and regulatory frameworks (Labonte, 2014). This has resulted in the spread of unhealthy lifestyles throughout the world. The prevalence of diabetes, obesity, and diet-related noncommunicable diseases (NCDs) has significantly increased over the past couple of decades, particularly in low- and middle-income countries (LMICs). Rates of obesity and NCDs in LMICs are now equal to or higher than those in high-income countries (Baker and Friel, 2014).

The transformation of global food systems can be linked to the opening of domestic markets for international food trade, the increased involvement of transnational food corporations, the rise in foreign direct investment in the food industry, and the extensive global marketing and promotion of food products (Labonté et al., 2011). Food trade patterns have shifted, increasing trade volumes for hazardous foods while lowering trade volumes for conventional cereals and starchy root crops. Following NAFTA, countries such as Mexico experienced significant US agribusiness investment, reshaping domestic agriculture into export-oriented cash crop production and that affect availability of food, quality of nutrition, price, and desirability (Khoury et al., 2014). Similarly, in Central America and Asia, decreased investment barriers contributed to the rise of highly processed food markets and lower regulatory standards in the food business (Hawkes and Thow, 2008). Furthermore, attempts to create a health-based labeling system for snack products in Thailand faced criticisms from the US and other countries, impacting the final decision on policy (Hawkes, 2005). Transnational corporations owned by Americans brought majority these food products to Thailand (Friel et al., 2015).

Empirical Methods

To distinguish the effects of the sugar trade agreement from any other influences, we employ the SCM following (Abadie et al., 2010). This approach generates a counterfactual similar to Difference-in-Differences (DiD), but the SCM has merits over the standard panel data regression and DiD. First, it uses precise model-based criteria to assign weight to untreated units in the estimation of treatment effects whereas in a panel data regression, control units are assigned equal weights. Thus, in the SCM, units that differ too much from the treated unit are overlooked. Second, it does not depend on outcomes in the post-treatment periods. This helps us to design choices with no idea of how they affect the final outcome (Abadie et al., 2010). Third, it relaxes the "parallel trend assumption" required in the standard DiD (Billmeier & Nannicini, 2013). Hence, we used SCM to evaluate the counterfactual outcome United States would have attained without NAFTA.

We assume there is one treated unit (United States) and J organizations for Economic Co-operation and Development (OECD) countries serving as potential controls (also called the "donor pool"). Let $i = 1, \ldots, J + 1$ represent all countries sampled, where $i = 1$ corresponds to United

States and $i = 2, \ldots, J+1$ corresponds to each of the J donor OECD countries. We define T_0 as years before the intervention with $1 \leq T_0 < T$. Let Y_{it}^{I} represent diabetes prevalence/sugar consumption that would be observed in country i at time t if that country is subjected to the NAFTA sugar agreement in years $t = T_0 + 1, \ldots, T$, while Y_{it}^{N} represents diabetes prevalence/sugar consumption that would be observed in country i at time t without NAFTA for countries $i = 1, \ldots, J+1$ and time periods $t = 1, \ldots, T$. The objective is to estimate the effect of the introduction of NAFTA sugar agreement on diabetes prevalence/sugar consumption, $\alpha_{1T_{0+1}}, \ldots, \alpha_{1T}$, where $\alpha_{1T} = Y_{it}^{I} - Y_{it}^{N}$ is the effect of the introduction of NAFTA in United States at time $t = T_0 + 1, \ldots, T$. Y_{it}^{I} is the observed outcome in the United States, whereas Y_{it}^{N} is the unobserved counterfactual that is required to identify α_{1t}. The counterfactual, Y_{it}^{N} is estimated through a linear factor model:

$$Y_{it}^{N} = \delta_t + \theta_t Z_i + \lambda_t \mu_i + \varepsilon_{it} \tag{1}$$

where δ_t is an unknown set of time effects that are constant across countries; Z_i is a vector of relevant observed pre-treatment covariates that can be time invariant, and θ_t is the vector of unknown parameters; μ_i captures countries specific unobserved determinants of diabetes prevalence/sugar consumption, and λ_t is an unknown common factor; and ε_{it} captures random shocks with mean zero. Define $W = (w_2, \ldots, w_{J1})'$ as a $(J \times 1)$ vector of weights such that $w_i \geq 0$ and $\sum_2^{J+1} w_j = 1$, where each possible choice of W corresponds to a particular weighted average of donor countries. As shown by Abadie et al. (2010), the outcome variable Y_{it}^{N} for the treated unit is approximated by the synthetic control unit, $\sum_2^{J+1} w_j^{*} Y_{jt}$, which is a weighted average of the donor unit outcomes. The study's estimate of the effect of the introduction of NAFTA is therefore:

$$\hat{\alpha}_{it} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt} \tag{2}$$

The principal objective of the SCM is to construct a synthetic United States that tracks actual diabetes prevalence/sugar consumption in United States before the NAFTA was implemented. In this study, optimal weight vector w^* is selected to minimize the root mean squared prediction error (RMSPE from here forth) of the outcome variable in the period before NAFTA. Ideally, the pre-treatment gap would equal to zero each year before 2008, which would suggest that synthetic United States is a perfect fit for actual United States in terms of diabetes prevalence/sugar consumption. In practice, it is difficult to find a perfect fit due to limitation on the number of potential donor countries and the fact that diabetes prevalence/sugar consumption fluctuates year to year based on the economic and health indicators.

Moreover, a criterion is selected that minimizes the pre-treatment gap between the actual and synthetic United States. One major concern with the synthetic control analysis is the likelihood that control countries also have policies that influence diabetes prevalence/sugar consumption which coincide with NAFTA (see Abadie et al., 2010). Policies introduced to increase diabetes prevalence/sugar consumption after 2008 would bias the estimate of the impact of NAFTA downwards. Countries that have well developed sugar trade agreement policies were excluded from the control group in an "effort to minimize potential attenuation bias in the synthetic control estimation" (see badie et al., 2010). We conducted several placebo tests to ascertain the validity of the SCM and the estimates.

Synthetic Control Method: Validity and Placebo tests

Several validity and robustness tests were carried out to ensure the accuracy of our findings.

Statistical Significance of NAFTA Estimated Effects

The following method can be used to ascertain the year-specific significance level (p-value) for the predicted NAFTA effect.

$$p - value_t = P_r\left(\hat{\alpha}_{i,t}^{PL} < \hat{\alpha}_{i,t}\right) = \frac{\sum_{j=2}^{J+1} 1\left(\hat{\alpha}_{i,t}^{PL_j} < \hat{\alpha}_{it}\right)}{Number\ of\ Control\ Countries} \tag{3}$$

The symbol $\hat{\alpha}_{i,t}^{PL_j}$ signifies the impact of NAFTA during a given year, under circumstances where a placebo NAFTA is simultaneously applied to control country j and treated country 1. In this context, the calculated synthetic treatment effect follows a similar algorithm as that specified for $\hat{\alpha}_{it}$. This procedure is iterated for each country j within the donor pool, aiming to create a distribution reflecting the synthetic experiment and assess the position of the estimate $\hat{\alpha}_{i,t}$ within this distribution. Ultimately, our goal is to establish dependable conclusions about $\bar{\alpha}$, and thus, we compute the year-specific p-value for the average effect at year t as follows.

$$p - value_t = P_r\left(J^{-1} \sum_{j=1}^{J} \hat{\alpha}_{j,t}^{PL} < \bar{\alpha}_{1,t}\right) = P_r\left(\bar{\alpha}_t^{PL} < \bar{\alpha}_t\right)^{25} \tag{4}$$

In-Time, and In-Space Placebo Tests

We conducted two main placebo tests following closely Abadie et al. (2015), Barlow et al. (2017), and Abadie (2021). First, in-time placebo test (or preprogram test). The treatment year is pushed to a year before NAFTA to show that the SCM result is not random. This test examines whether NAFTA significantly affected diabetes prevalence/sugar consumption in the United States before the NAFTA sugar agreement was introduced in 2008. We selected 2006 as the preprogram date to see if the impact of NAFTA on diabetes prevalence happened two years before it was implemented.

[25] Additional information about this calculation method can be found in the work of Cavallo et al. (2013).

Second, in-space placebo test. We check whether the results can be attributed to an effect of an increase in diabetes prevalence/sugar consumption in NAFTA. We generated RMSPE for years before and after NAFTA. Post-NAFTA RMSPE is then divided by the pre-NAFTA RMSPE (see 5). We then compare United States' RMSPE ratio to RMSPE ratios of countries in the donor pool.

$$RMSPE\ Ratio = \frac{Post\ RMSPE}{Pre\ RMSPE} \tag{5}$$

Pre-Treatment Fit: Synthetic Control Method

To see if the control countries ("donor pool") are like the United States in terms of diabetes prevalence/sugar consumption, we adopt a measure that is developed and used by Adhikari & Alm (2016). With this measure, we see how well the synthetic control countries mirrors United States in the years before NAFTA. To the contrary, Abadie et al.(2010) use the RMSPE of the outcome variable to examine a "fit or lack of fit" between the outcome of the treated country and the synthetic group. One important merit of the "pre-treatment fit index" is its ability to normalize the RMSPE making it feasible to compare the fit among the SCM across diverse outcome variables and countries (Adhikari & Alm, 2016). For instance, diabetes prevalence and sugar consumption may vary quite significantly across the sampled OECD countries. This measure yields an index that renders examination of the fit quality intuitive. We rely on (6) to compute a pre-treatment fit.

$$RMSPE = \sqrt{\frac{1}{T_0}\sum_{1}^{T_0}\left(Y_{1t}\sum_{j=2}^{J+1} w_j^*\, Y_{jt}\right)^2} \tag{6}$$

We generate a benchmark RMSPE from a zero-fitted model as

$$Benchmark\ RMSPE = \sqrt{\frac{1}{T_0}\sum_{1}^{T_0}(Y_{1t})^2} \tag{7}$$

Finally, the pre-NAFTA fit is computed as the ratio of RMSPE from a fitted model and benchmark RMSPE.

$$Fit\ Index = \frac{RMSPE}{Benchmark\ RMSPE} \tag{8}$$

We conclude on perfect fit if RMSPE approaches zero making the fit index zero. A fit of one suggests that RMSPE is at par with the benchmark RMSPE[26]. An index above one indicates that diabetes prevalence/sugar consumption in United States is huger (or lesser) in value of "two or more" ("or half or less") compared to its counterfactual[27].

Heterogeneity Estimations

To identify the causal effect of NAFTA sugar agreement on diabetes prevalence at state-level, we employ a Difference-in-Differences model (DD) with fixed effects and a panel event study following the specifications of De Giorgi et al. (2022). In our research design, a treated State adopted the NAFTA sugar agreement in the United States while a control state (i.e., the control country include six Organization for Economic Co-operation and Development, OECD, countries including Australia, China, Norway, Japan, Switzerland, and the United Kingdom) adopted no such trade policy. Intuitively, the idea is to compare the difference in average diabetes prevalence between treated and controls before and after the treatment.

[26] In this sense, the calculated value of [1 - Fit Index] provides similar information to the information provided by the R^2 statistic in regression analysis.
[27] We consider the pre-NAFTA fit as appropriate if this index is smaller than or equivalent to "0.10".

The Difference-in-Differences Analysis

We estimate the following equation following De Giorgi et al. (2022):

$$Y_{st} = \alpha_0 + \beta_0 Treat_s + \beta_1 Post_t + \beta_2 DD_{st} + \gamma_s + \tau_t + \varepsilon_{st} \tag{9}$$

in which Y_{st} is a continuous variable indicating crude diabetes prevalence in state s at year t, $Treat$ is a binary variable indicating whether the state (or country) had adopted NAFTA; $Post$ is a binary variable taking a value of 1 in the post-NAFTA period (and 0 otherwise); and DD is the interaction between $Treat$ and $Post$. Our parameter of interest is β_2. Finally, γ_s , τ_t are, respectively, state and year fixed effects. The key identifying assumption of DD analyses is that of common trends between treated states and the control countries in the absence of the treatment.

Event-Study Analysis

In addition to the DD analysis described above, we conducted a panel-event study, with the "event" being the date of implementation of the NAFTA sugar trade agreement in a particular state. We estimated the following equation as adopted in De Giorgi et al. (2022) and others.

$$Y_{st} = \alpha_1 + \sum_{j=2}^{J} \delta_j (Lag\ j)_{st} + \sum_{k=2}^{K} \lambda_j (Lead\ k)_{st} + \mu_t + \psi_s + \varepsilon_{st} \tag{10}$$

where ψ_s and μ_t are binary variables for state and year, and ε_{st} is unobserved error term. Further, Lag_j and $Lead_k$ are two binary variables indicating the number of years until implementation of the NAFTA sugar agreement in state s. Formally, we defined Lag_j and $Lead_k$ according to Equations (11) – (14)

$$(Lag\ j)_{st} = 1[t \le Event_s - J], \tag{11}$$

$$(Lag\ j)_{st} = 1[t = Event_s - j]\ for\ j \in \{1, \ldots, J-1\}, \tag{12}$$

$$(Lead\ k)_{st} = 1[t = Event_s + k]\ for\ k \in \{1, \ldots, K-1\}, \tag{13}$$

$$(Lead\ k)_{st} = 1[t \geq Event_s + K]. \tag{14}$$

where, $Event_s$ is a variable indicating the year t in which the NAFTA sugar agreement was implemented in state s. The first Lag was omitted to capture the baseline difference between treated and control state.

Effect Heterogeneity Over Time

To assess how the effects of the NAFTA sugar agreement vary with time, we follow (De Giorgi et al., 2022) to estimate a dynamic model where β can vary across years:

$$\begin{aligned} Y_{st} = \alpha_2 + \beta_0 Treat_s + \sum_{t=9}^{17} \beta_{1t} \left[year_{st(t=2008)} - year(T=1)_s\right] \\ + \sum_{t=9}^{17} \beta_{2t}\, Treat_s \left[year_{st(t=2008)} - year(T=1)_s\right] + \gamma_s + \tau_t + \varepsilon_{st} \end{aligned} \tag{15}$$

$Treat_s$ is a binary variable equal to 1 if State s is ever treated (i.e., part of the NAFTA sugar agreement). Then, $[year - year(T=1)]_s$ is the difference between the observation year and the first year of implementation of the NAFTA sugar trade agreement in State s. The parameters of interest are the β_{2t}, which represent the mean difference in the crude diabetes prevalence in a specific year t. We also control for State (γ_s), and year (τ_t) fixed effects.

Data

Data used for this study is taken from several sources. Variables such as the crude prevalence of diabetes, hypertension prevalence, raised blood pressure, and mean total cholesterol are obtained from the "Center for Disease Control and Prevention" (CDC) and NCD Risk Factor Collaboration (NCD-RisC). In this study, when we refer to diabetes prevalence, we are considering the combined prevalence of both type 1 and type 2 diabetes. A more robust approach would be to focus solely on type 2 diabetes, as it is influenced by dietary changes, unlike type 1 diabetes. However, we lack data specific to type 2 diabetes, particularly for control countries. Given the fact that the global prevalence of diabetes has primarily increased due to the rise in type 2 diabetes (Zimmet et al., 2014; Cho et al., 2018). We believe that utilizing the combined prevalence of type 1 and type 2 diabetes will not affect the overall argument of this study. We also include data on the percentage of the population 65 years of age and older and GDP per capita from the World Bank's World Development Indicator (WDI). Data on "average total years of schooling for individuals aged 18 and above" are acquired from (Barro & Lee, 2018) "dataset on education attainment 1870 – 2017".

Data on producer prices of sugar, sugar imports, sugar consumption (kg per capita), and Sugar Food Supply (kcal per capita per day) are obtained from the "Food and Agriculture Organization of the United Nations Statistics Office" (FAOSTAT). Data on the "prevalence of insufficient physical activity among adults aged 18" and above were sourced from the World Health Organization. Information on the consumption of alcoholic beverages (Kcal per capita per day) is obtained from FAO. Data on high-fructose corn syrup (HFCS) consumption was obtained from OECD-FAO Agricultural Outlook 2021 – 2030. Our analysis focuses on seven OECD countries: Australia, China, Japan, Norway, Switzerland, the United States, and the United Kingdom. The United States serves as the treated unit, and the other six countries form the donor pool due to their similarities with the United States in terms of income, level of development, trade potential, and population dynamics and how well they fit the United States in the pre-treatment period.

Appendix Table 3.A1 describes and provides summary statistics of the variables included in the analysis. Sugar consumption averaged 31.4 kg per capita annually across the seven countries. Diabetes prevalence averaged 14.79 % with a standard deviation of 2.73%. In Figure 3.2, we display the averages of diabetes prevalence and sugar consumption both for the donor countries and the United States. We can see that the averages for the two variables are greater in the United States. Sugar imports have an average of 1299.30 metric tons. The mean total cholesterol in our sample is 9.96mmol/L with a standard deviation of 0.53mmol/L. Alcoholic beverage consumption has a mean of 139.99Kcal per capita per day, with significant variation across the countries. The mean calories from HFCS and sugar average 12395.05 kg per capita, with significant variation across the countries.

We used different sets of data sources for the state heterogeneity analysis. The dataset contains 1,054 observations, covering the 50 US states and six countries from The Organization for Economic Co-operation and Development (OECD) each year between 2000 and 2016. Data on crude diabetes prevalence which comprises type 1 and type 2 diabetes were obtained from the US Diabetes Surveillance website maintained by the Center for Disease Control and Prevention (CDC). We obtained data on percentage of poverty (all ages) from 2009 U.S. Census Bureau, Small Area Estimates Branch 2009. The percentage of black population was obtained from the 2009 Census Bureau. Gender-related data was acquired from KFF estimates, which relied on the 2008-2021 American Community Survey's 1-Year Estimates. Additionally, data on educational attainment (high school diploma or more) was sourced from the U.S. Census Bureau's 2009 American Community Survey. Although a stronger strategy would concentrate solely on type 2 diabetes, which is affected by dietary modifications, we faced a lack of precise data regarding type 2 diabetes, particularly for the control countries. Considering the worldwide escalation in diabetes occurrence primarily linked to type 2 diabetes (Zimmet et al., 2014), we hold the viewpoint that incorporating the combined prevalence of type 1 and type 2 diabetes will not impact the fundamental argument of this research.

We use SCM in accordance with the three fundamental rules. To begin, we used balanced panel data. Second, for all the years under consideration, each country had data on the outcome variable. Third, we ensured that each variable used in the study had at least one obtainable observation during the pre-NAFTA years. We can select the outcome variable, the control nations, and the number of years for our study based on this criterion. We selected the year 2000 as the first pre-NAFTA year, followed by ten post-NAFTA years. This enables us to examine the influence of the NAFTA sugar trade agreement on the consumption of sugar and diabetes prevalence in the United States. We have at least nine pre-NAFTA years to compute the pre-intervention effect with our data span (2000-2016). We only accept countries into our donor pool if they have at least one observation pre-NAFTA for all explanatory variables we analyze. We exclude any country that does not meet this condition. Again, we made sure that no donor pool has a program that resembles the NAFTA sugar agreement. This is due to our aim to match the outcome variable trajectory between the United States and the control OECD countries. If we include OECD countries that underwent similar intervention, it will be impossible to compare their outcomes to those of the United States. To evaluate the average treatment effect of NAFTA, we chose nations with similar trade and economic characteristics to the United States and with strong pre-treatment fit. By doing so, we avoid biases that can arise from “interpolation” among nations with wide differences in features. It also helps us in capturing “unobservable” economic and development features. We compute “placebo” treatment effects for the donor countries and then compare them to the United States treatment effect (Abadie et al., 2010).

Results and Discussion

This section presents the findings obtained through the utilization of three distinct analytical methodologies: the synthetic control method, difference-in-differences analysis, and panel event study approaches, along with their respective discussions.

Results: Synthetic Control Method

Impact of NAFTA on Sugar Consumption

We first investigate if the NAFTA sugar agreement resulted in more sugar entering the United States. Figure 3.3 depicts the sugar consumption patterns in the United States and its synthetic counterpart between 2000 and 2016. The solid line reflects the trend in sugar consumption in the United States. The dashed line depicts the estimated sugar consumption in the United States in the absence of the NAFTA sugar agreement. We can see a marginal difference in sugar consumption between the synthetic United States and the United States before the implementation of the program. Thus, sugar consumption in the synthetic United States closely tracks the sugar consumption of the actual United States before the intervention. The treatment effect estimated shows that sugar consumption in the actual United States is higher than it would have been without NAFTA sugar agreement. The consumption of sugar in the actual United States and the synthetic United States is compared in Table 3.1. We find that sugar consumption in the United States increased by an average of 16% per year after the implementation of NAFTA, corresponding to 5240g per capita.

The estimated synthetic weights are also shown in Appendix Table 3.A2 column 2. The United States' sugar consumption before NAFTA is best produced by the combination of the United Kingdom, Japan, Norway, and Switzerland. Using the covariates shown in Appendix Table 3.A3, we chose a weight matrix that reduces the RMSPE of the response variable. The synthetic

United States is created based on pre-NAFTA sugar consumption and pre-NAFTA predictors consisting of producer price of sugar, GDP per capita, percentage of population 65 and above, sugar import, and average total years of school aged above 18. The pre-NAFTA predictor means for the United States, the synthetic United States, and the mean of the six donor countries are also shown in Appendix Table 3.A3. Following Abadie et al. (2010) closely, we selected the best fit based on the RMSPE with smaller value implying better fit. The RMSPE expresses the mean of squared deviations between sugar consumption in the United States and the synthetic counterpart from 2000 to 2007. The baseline specification generates an RMSPE of 0.72.

Impact of NAFTA Prevalence of Diabetes

Second, we estimated the impact of the NAFTA sugar trade agreement on the crude prevalence of diabetes. Appendix Table 3.A2 column 3 presents the estimated weight that is allocated to each of the countries in the "donor pool". It shows that United States' pre-treatment crude prevalence of diabetes is best produced by combination of Australia, China, Japan, and United Kingdom. The synthetic United States is created using pre-NAFTA diabetes prevalence and pre-treatment predictors consisting of % of the population aged 65 and above, prevalence of insufficient physical activity among adults aged 18 and above years, average total years of schooling for individuals aged 18 and above, producer price of sugar, hypertension prevalence, the prevalence of raised blood pressure, mean total cholesterol (mmol/L), sugar food supply (kcal per capita per day, and alcoholic beverage consumption (kcal per capita per day)(see Appendix Table 3.A4). Appendix Table 3.A4 displays the pre-NAFTA predictor averages for the United States, the synthetic United States, and the average of the six "potential donor" countries.

Figure 3.4 depicts the trends in diabetes prevalence in the United States and its synthetic counterpart between 2000 and 2016. The solid line represents the diabetes prevalence trend for the

United States. The dashed line also shows diabetes trends of the synthetic United States, depicting the estimated diabetes prevalence the United States would have experienced in the absence of NAFTA. We can see that there was a small difference in diabetes prevalence between the synthetic United States and the United States before the policy. Thus, diabetes prevalence in the synthetic United States closely matches the diabetes prevalence of the actual United States prior to the intervention. Our estimate of the treatment effect shows that diabetes prevalence in the actual United States is higher than it would have been without NAFTA. Table 3.2 compares diabetes prevalence in the actual United States and the synthetic United States. We can see that diabetes prevalence in the United States increased by an average of 1% per year after the sugar agreement.

Impact of NAFTA on Prevalence of Diabetes (Gender-Based)

We further estimated the gender-based diabetes prevalence impact of the sugar trade agreement. Figure 3.5 displays diabetes prevalence trends among women in the United States and its synthetic counterfactual between 2000 and 2016. We can see that there was a marginal difference in diabetes prevalence among women in the synthetic United States and the United States before the program was implemented. Thus, diabetes prevalence among women in the synthetic United States closely tracks women's that in the actual United States before the policy. Table 3.3 compares diabetes prevalence among women in the actual United States and the synthetic United States. Our finding shows that, on average, diabetes prevalence among women in the United States has been increasing by 2% annually.

In Figure 3.6, we display diabetes prevalence trends among men in the United States and its synthetic counterfactual between 2000 and 2016. As reported in Table 3.4, diabetes prevalence among men in the United States increased by an average of 1% per year after NAFTA was implemented, as predicted by the combination of donor countries, including China, Japan, and

Switzerland (see Table 3.A2 column 5). We can observe a significant difference in the trade agreement's impact on diabetes prevalence between women and men. Our finding shows that, on average, diabetes prevalence among men in the United States has been increasing by 1% annually. Appendix Table 3.A6 displays the pre-treatment predictor means for diabetes prevalence among men for the United States, the synthetic United States, and the average of the six potential donor countries.

Synthetic Control Method: Validity and Placebo Tests

Several validity tests were performed to ensure that our results were reliable. First, we computed probability values for the average treatment effect for our outcome variables (sugar consumption and diabetes prevalence). By doing so, we demonstrate that the average treatment effects obtained following the sugar agreement were not coincidental but were caused by the policy change. We also examine whether the difference between the actual and counterfactual outcomes is significant for all post-NAFTA outcomes. Figure 3.7 shows the probability values for sugar consumption, diabetes prevalence, diabetes prevalence (women), and diabetes prevalence (men) for the number of periods after the event occurred. With reference to sugar consumption, we observe that part from the p-value for the intervention year (2008) which is greater than 5% levels, all the other 8 periods (2009 to 2016) have p-values less than 1% ($p<0.01$). The same can be seen with diabetes prevalence.

This implies that though the sugar agreement has had impact on sugar consumption and diabetes in the United State as shown in our previous estimates, the impact is substantial after 2008. Similar result was obtained for gender with women been more responsive immediately after the event. The p-values of the number of periods after event for women are all below 1% ($p<0.01$). We can therefore conclude that our estimations on both sugar consumption and diabetes prevalence did not happen by chance, and there is significant difference between the actual U.S. and the counterfactual after the policy.

Second, we performed an in-space placebo test to validate our findings and the SCM (see Figure 3.8) by assigning the treatment to the donor countries. We can see that none of the donor nations display this policy impact when hypothetically exposed to the policy, which further proves that NAFTA plays a pivotal role in the alterations of sugar consumption and diabetes prevalence within the U.S. We closely follow Bohn et al. (2014) and Barlow et al. (2017) to estimate the causal impact of NAFTA on diabetes prevalence for the donor countries (i.e., Australia, China, Japan, Norway, Switzerland, and the United Kingdom). We excluded the United States in this analysis. Removing the United States, we compute the treatment effects that are associated with the placebo tests. The placebo test gives a distribution of the estimated gaps for countries where no NAFTA was implemented. We can observe from Figure 3.8 that the counterfactuals do not track the actual countries in the pre-NAFTA periods. This suggests that the pre- and post-NAFTA effects in Figure 3.4 did not happen by chance but because of the NAFTA sugar trade agreement between the United States and Mexico.

Third, following in the footsteps of Abadie et al. (2015) and Abadie (2021), we did an in-time or pre-program placebo test by pushing back the treatment period to 2006. Figure 3.9 shows the result of computing the impact of NAFTA with the intervention moved to 2006 for diabetes prevalence. We find that the synthetic control estimator tracks diabetes for United States from 2007– 2008, before the actual intervention. The fact that we do not see any impact before NAFTA gives some surety about the reliability of the synthetic control estimator. We can see that that synthetic control makes a replica of the trajectory of outcome variable for the United States before NAFTA (see also Abadie, 2021). Second, the space between diabetes between the United States and the synthetic control appeared after the sugar side agreement of NAFTA. This is happening even when we have pushed the intervention period two years back in our data and the process utilizes no data on the actual intervention year. So far as the estimated effect of NAFTA is seen immediately after 2008, even as the NAFTA policy is intentionally pushed two years back in our data, gives assurance on the reliability of the synthetic control estimator of the NAFTA (see Abadie, 2021).

Furthermore, we performed an "in-space" placebo test to see if our findings can be attributed to a relationship between sugar consumption and diabetes prevalence in the NAFTA sugar agreement (Barlow et al., 2017). This test divides the RMSPE before NAFTA by the RMSPE after NAFTA to give the RMSPE ratio (see Figure 3.10). We interpret a higher ratio as a significant divergence between the United States and its counterfactual after NAFTA. We begin by compare the estimated effect for the United States to a "placebo" effect through reassigning NAFTA to OECD countries that has not in reality implemented the policy. We subsequently estimated the model for every country in our sample. We observe from Figure 3.10 that United States has the highest RMSPE ratio. This suggests that there is a significant difference between sugar consumption and diabetes prevalence in the United States before and after NAFTA sugar agreement.

Testing for Substitution between Sugar and High-fructose Corn Syrup consumption

Furthermore, to test for potential substitution between sugar consumption and high-fructose corn syrup (HFCS) consumption, we use the combined calories from sugar and HFCS as both outcome variable and covariates to determine diabetes prevalence. Figure 3.11 displays the trends of annual calories consumed from sugar and high-fructose corn syrup in the United States and their synthetic counterpart from 2000 to 2016. The solid line represents the calorie trend in the United States. The dashed line represents the estimated sugar and HFCS calories in the United States if the NAFTA sugar deal did not exist. Sugar and HFCS calorie consumption in the synthetic United States closely mirror that of the actual United States, prior to intervention. The estimated treatment effect demonstrates that calories from sugar and HFCS in the United States are greater than they would have been without the NAFTA sugar agreement. This increase in total sweetener calorie consumption suggests that sugar consumption increases following NAFTA were not simply a substitute for HFCS consumption.

Figures 3.12–3.14 depict diabetes prevalence trends in the United States and its synthetic counterfactual between 2000 and 2016, using combined calories from HFCS and sugar consumption as covariate. As further validity check, we attempted to incorporate additional countries with available data, such as Chile, Colombia, Israel, New Zealand, South Korea, and Turkey, and the results remained consistent.

Discussion: Synthetic Control Method

Global public health has emerged as a top priority for policymakers and governments around the world in recent years. The impact of sugar consumption on health outcomes such as diabetes and obesity has piqued the interest of health economists and analysts in the field of public health. Sugar-sweetened beverages have been related to several diseases, including diabetes, dental caries, and obesity, according to research. In response to this concern, the World Health Organization (WHO, 2013) has suggested that added sugar consumption be reduced to promote global public health. As a result, governments around the world have established legislation targeted at limiting the amount of sugar in food and beverages (Stanner and Spiro, 2020). Previous research has shown that sugar taxes can reduce the prevalence of obesity, diabetes, and other disorders connected with the intake of sugar-sweetened beverages significantly. However, little is known about the potential consequences of decreasing sugar prices because of international trade agreements. Reduced trade barriers in international trade result in higher imports and lower commodity prices in the importer country. Trade liberalization can boost competitiveness, resulting in higher productivity and lower prices and markups. As a result, removing tariff and non-tariff barriers to sugar trade can operate as a sugar subsidy, endangering public health as sugar consumption rises (Cernat et al., 2021).

We evaluated the potential causal impact of the NAFTA ‘s unrestricted sugar trade agreement on diabetes prevalence in the United States. Although the benefits of unrestricted trade agreements are well known, previous research focused on different outcome indicators. A rare exception is the study by Baggio and Chong (2020), which examines the relationship between engaging in free trade agreements with the United States and the prevalence of obesity among adults. Our study, on the other hand, focuses on the potential causal influence of NAFTA’s unrestricted sugar trade agreement on diabetes prevalence in the United States. As a result, our study is more limited in scope and focuses exclusively on the consequences of NAFTA, whereas Baggio and Chong’s (2020) study give a more comprehensive analysis of the impact of free trade agreements.

We find that sugar consumption in the United States increased by an average of 16% per year after the implementation of NAFTA, corresponding to 5240g per capita. We also find that diabetes prevalence in the United States increased by an average of 1% per year after the sugar agreement. This result corresponds to the Health Care Cost Institute (HCCI) report, which noted that diabetes prevalence among the United States population has been on the rise after 2008. Our finding suggests that the NAFTA sugar trade agreement has contributed to diabetes prevalence among men and women in the United States. Our estimates show that men have higher rates of diabetes than women, which is consistent with the literature (Danaei et al., 2009; Hackett, 2011;

Nordström et al., 2016). This difference in diabetes rate is often attributed to gender disparities in diet and lifestyle. Moreover, men consume more sugar daily than women, on average, (McNaughton et al., 2020), a finding that is supported by the Centers for Disease Control and Prevention's (CDC) (2021) and the Australian Bureau of Statistics' (ABS) National Health Survey (2017-18).

Our results show that diabetes prevalence increased by an average of 1% and 2% for men and women, respectively. This implies that the responsiveness of women to sugar consumption due to price decrease because of increase in sugar import from the sugar agreement is lower than that of men. This suggests that the own-price elasticity for sugar is lower in women compared to their men counterpart. This result contradicts with prior studies such as Muhammad et al. (2019) which examined price elasticities of sugar-sweetened beverage in the presence of tax policy and found that "own-price elasticity" was higher in men (-1.91) compared to women (-0.70). However, it is consistent with Nelson (2014) which showed that in the presence on alcoholic beverage tax, men have less elastic demand juxtapose with their female counterparts.

Finally, through a series of rigorous validity tests, we establish that the observed effects of NAFTA on sugar consumption and diabetes prevalence are not coincidental. In an attempt to identify any potential substitution between sugar consumption and high-fructose corn syrup (HFCS) consumption, we conducted a test using combined calories from sugar and HFCS as outcome variables and covariates to determine diabetes prevalence. Our findings indicate that there is marginal substitution between HFCS and sugar consumption in the United States following the NAFTA sugar agreement.

Diabetes and its related challenges present significant economic loss to not only affected individuals but their households, the health service, and the economy via direct medical expenditures as well as work and wages loss, even though, we all can attest to the fact that major cost is incurred on outpatient and hospital care. This cost is principally attributed to the upsurge in insulins cost since their prescription has been going up although medical practitioners and health scientists have not been able to prove that its health benefits supersede human insulins which are relatively cheaper.

Unintended Economic Cost of the Unrestricted Sugar Trade Agreement

In this section, we compute the economic cost of the NAFTA trade sugar agreement from our estimation of the increase in crude prevalence of diabetes (1%) after the policy. According to the National Diabetes Statistics from the Center for Disease Control and Prevention, about 37.3 million (11.3%) of the United States population have diabetes. Also, according to the American Diabetes Association (2018), the amount required to treat a diagnosed diabetes increased from $245 billion in 2012 to $327 billion in 2017 (i.e., a 26% increment) for a span of five years. This implies that on average the total cost of diagnosed diabetes has been increasing by 6.5% annually from 2012 to 2017.

From our estimate, diabetes has been increasing by an annual average of 1% since the implementation of the sugar agreement from NAFTA. Calculating 1% out of 37.3 million gives us approximately $0.37 million. Each diabetes patient incurs an average of $876.68 per year on a per-capita basis. The product of $876.68 and 0.37 million leads us to roughly $324.37 million, holding inflation and other economic indicators that could increase the cost of treatment beyond the considered years constant. We therefore conclude that the sugar trade agreement cost the United States economy an additional $324.37 million for treating diabetes patients.

Heterogeneity Results

In our study, we investigate the causal impact of NAFTA's sugar trade agreement on diabetes prevalence across the US 50 states. Overall, we find that the NAFTA sugar trade agreement has significant positive impacts on diabetes prevalence in most states (see Figure 3.15 and Table 3.5). Figure 3.15 displays the results of the event-study analysis, while Table 3.5 shows the estimated average treatment effects (ATE) from the DD analysis. Across most states, both the DD and event analysis methods revealed significant effects of the NAFTA sugar trade agreement on diabetes prevalence. Because the NAFTA sugar trade agreement was a national policy, one would expect the states to experience similar effects. However, the impact on the 50 states varies. The estimated effect varies in statistical significance and magnitude from 2.3% ($p<0.001$) in Alabama to 0.54% ($p<0.1$) in Iowa (Table 3.5).

We classified the 50 states by the magnitude and significance of the estimated impact of the NAFTA sugar agreement on diabetes prevalence. Our estimates suggest that two states (Alabama and Arkansas) saw crude diabetes prevalence increase by greater than two percentage points because of the NAFTA sugar agreement. Twenty-one states had an impact greater than 1% but less than 2% (Arizona, Florida, Georgia, Indiana, Kansas, Kentucky, Louisiana, Maryland, Mississippi, Missouri, Nevada, New Mexico, North Carolina, Ohio, Oklahoma, Oregon, South Carolina, Tennessee, Texas, Virginia, and West Virginia). Nineteen states saw less than a 1% impact (Alaska, California, Connecticut, Delaware, Illinois, Maine, Massachusetts, Michigan, Minnesota, Nebraska, New Hampshire, New York, North Dakota, Pennsylvania, Rhode Island, Washington, Wisconsin, Wyoming, and Iowa). Lastly, eight states (Colorado, Hawaii, Idaho, Montana, New Jersey, South Dakota, Utah, and Vermont) had no effect on diabetes prevalence from the NAFTA sugar trade agreement. This illustrates that, while most states saw a rise in diabetes prevalence because of the NAFTA sugar trade agreement, some did not.

To better understand the rationale behind the disparities in trade policy impact across states, we explore the relationship between selected covariates that have been shown in the health literature to be major factors influencing diabetes prevalence (see Figures 3.16). We use 2008 values of the covariates for the analyses, as this represents the midpoint of our dataset. We concentrated on poverty, educational attainment, percentage of the population that is Black, and percentage of the population that is female. All these variables have a statistically significant association (p-value < 0.001) with the estimated diabetes prevalence (see Figures 3.16). Higher poverty level, for example, is associated with a higher diabetes prevalence. Having a greater Black population, having a lower percentage of the population with a high school degree, and having a higher percent female population are also associated with a higher diabetes prevalence.

These findings are consistent with previous health research. For example, research has demonstrated that individuals with greater levels of education are more likely to practice preventative healthcare behaviors, such as eating healthier meals, exercising more, and preventing type 2 diabetes and obesity (Pampel et al., 2010; Montez and Zajacova, 2013). Also, studies have investigated the effect of race on type 2 diabetes prevalence. The difference in type 2 diabetes prevalence across race and ethnic groups include the prevalence of certain risk factors such as obesity and limited access to healthy foods. For example (Divers et al., 2020) found that type 2 diabetes prevalence is higher in the Black population (5.97%) compared to whites (0.77%) in the US from 2014 to 2015. It has also been shown that diabetes incidence has a greater link with poverty level (Gaskin et al., 2014). Several studies that have found that diabetes prevalence varies between male and females (Biswas et al., 2016). These findings point to the fact that the state-level differences in crude diabetes prevalence as shown in the current study can be attributed to sociodemographic characteristics such as poverty, race, gender, and educational attainment.

Figures 3.17, 3.18 and 3.19 as well as Table 3.6 presents the results of equation (15). Table 3.6 shows the effect of the NAFTA sugar agreement for the 50 States across the post-treatment years. We observe that although the ATET is positive in 2008 (the treatment year) with its 95% confidence interval (CI) ranging from -0.09 to 0.23, it lacks statistical significance. This is not unexpected because just like any trade policy, the year of the agreement does not present a substantial impact. This is also reflected in Figures 3.17, 3.18 and 3.19**,** showing marginal effect for 2008. We obtained statistically significant ATET (0.23%) in 2009 with 95% CI ranging from 0.02 to 0.45. The ATET for 2010 is 0.25% with 95% CI ranging from 0.07 to 0.43. The ATET then increased from 0.99% in 2012 [95% CI from 0.76 to 1.21] to 1.43% in 2016 [95% CI from 1.18 to 1.68].

Conclusion

Increased consumption of sugar is associated with several chronic diseases, including obesity and diabetes, due to their high sugar and calorie content. To address this issue, institutions such as the World Health Organization have recently advocated for the use of sugar taxes, urging policymakers and governments to use pricing mechanisms to discourage excessive consumption of these drinks. Some countries, including Hungary, have already implemented some form of tax on SSBs. However, the sugar trade agreement under NAFTA had the opposite effect of a sugar tax, as it resulted in decreased sugar prices in the United States.

This study investigates the causal impact of this unrestricted sugar trade agreement on sugar consumption and diabetes prevalence in the United States, using the SCM. We find that sugar consumption in the United States increased by an average of 16% per year after the sugar trade agreement went into effect, corresponding to a 5240g increase per capita. Our estimates show that diabetes prevalence has been increasing on average by 1% annually since the agreement, with significant variation between men and women. This unintended consequence of NAFTA has had an estimated economic cost of $324.37 million per year. To better understand NAFTA's effect at state-level, we use the difference-in-differences and event-study approaches to estimate the causal impact of the NAFTA sugar trade agreement on diabetes prevalence in the individual 50 States. The results revealed that the NAFTA sugar trade agreement has led to an increase in diabetes prevalence in most states, with rates ranging from 0.54% in Iowa to 2.3% in Alabama. Additionally, we examine the impact of the NAFTA sugar trade agreement across all 50 States over time. Notably, the policy's significant impact began in 2009. With a 95% CI, the Average Treatment Effect on the Treated (ATET) ranged from 0.23% in 2009 to 1.43% in 2016.

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Tables and Figures

Figure 3.1: Trend of U.S. Sugar Import from Mexico

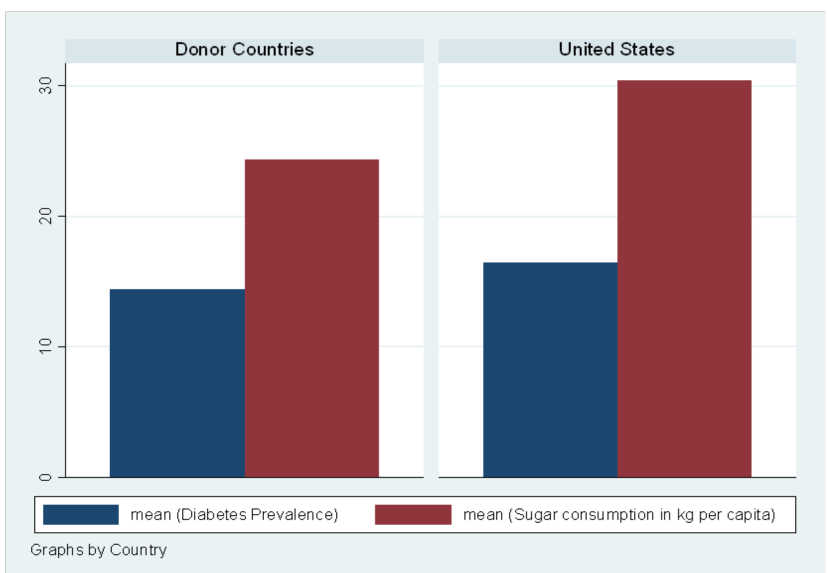


Figure 3.2: Average Diabetes and Sugar Consumption for the U.S. vs. Donor Pool Countries

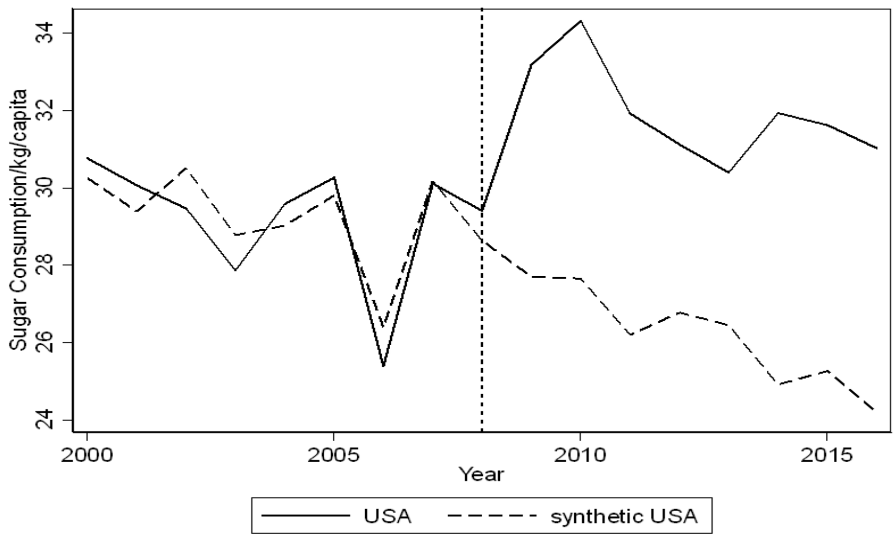


Figure 3.3: Trend of Sugar consumption

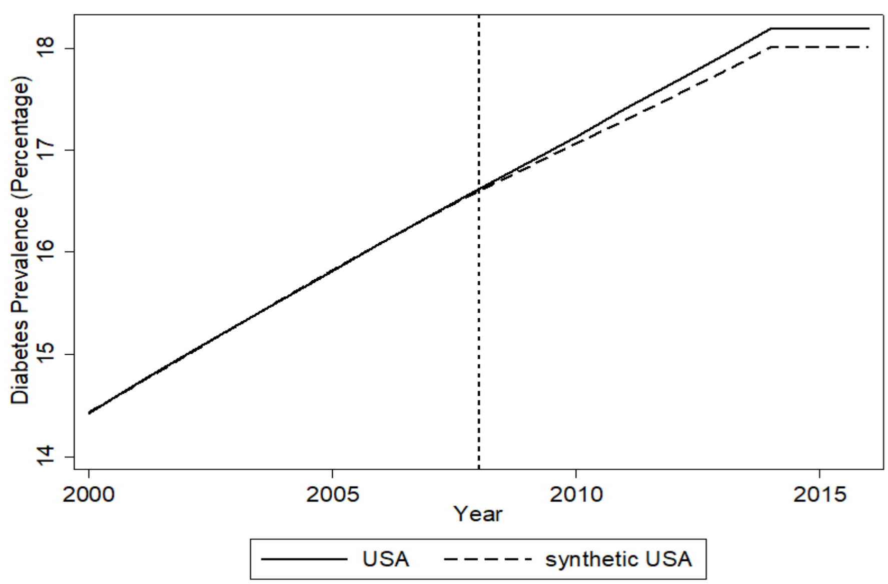


Figure 3.4: Trend in the Crude Prevalence of Diabetes

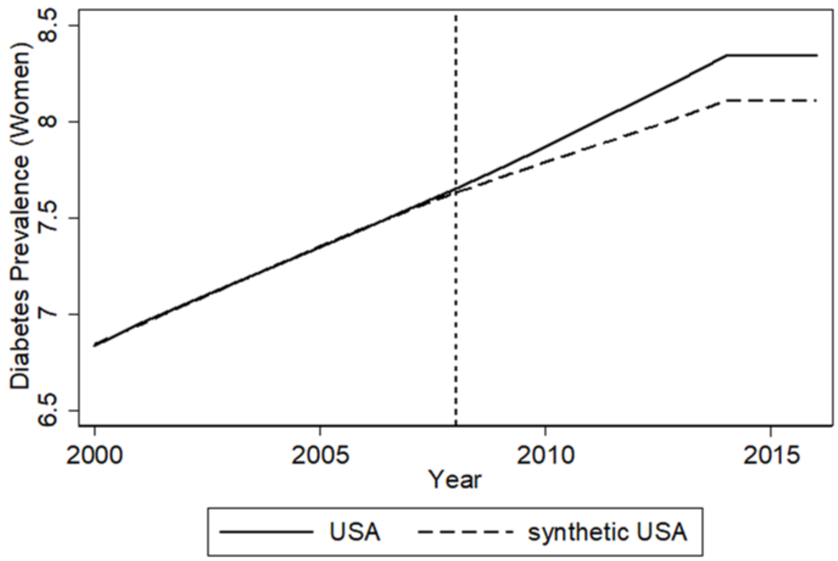


Figure 3.5: Trend in Diabetes Prevalence (Women)

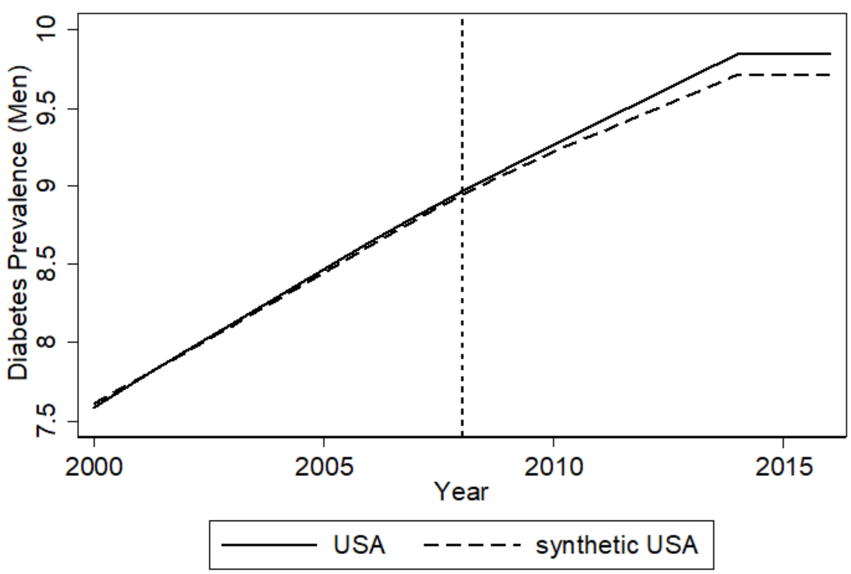


Figure 3.6: Trend in Diabetes Prevalence (Men)

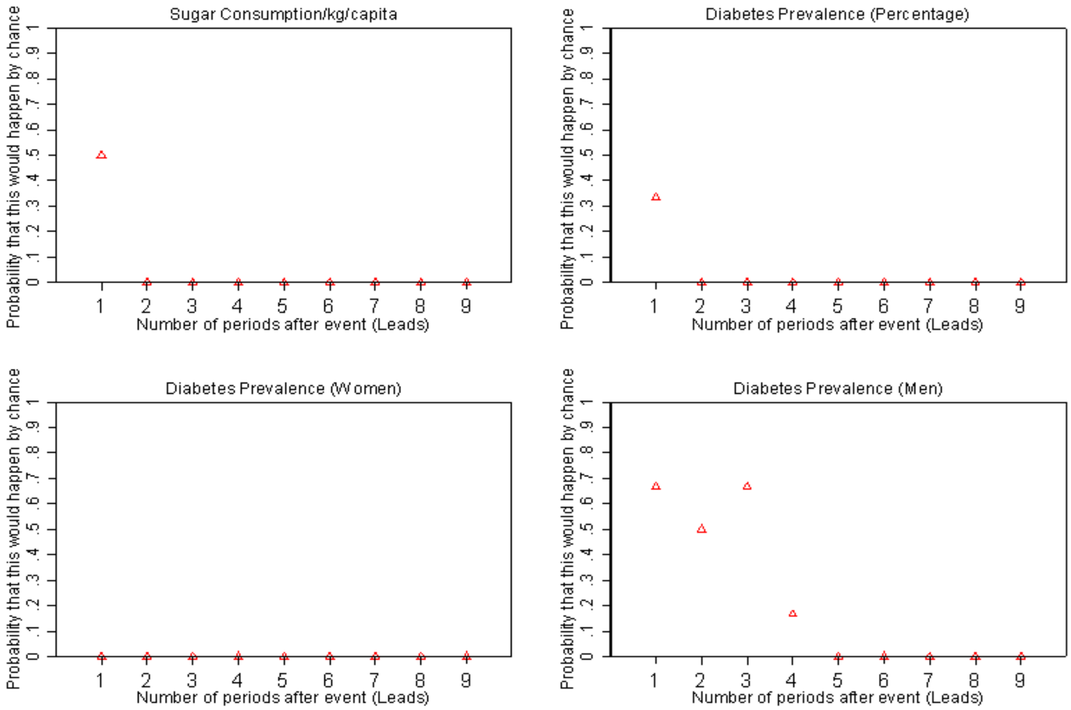


Figure 3.7: Probability Values of Average Treatment Effect

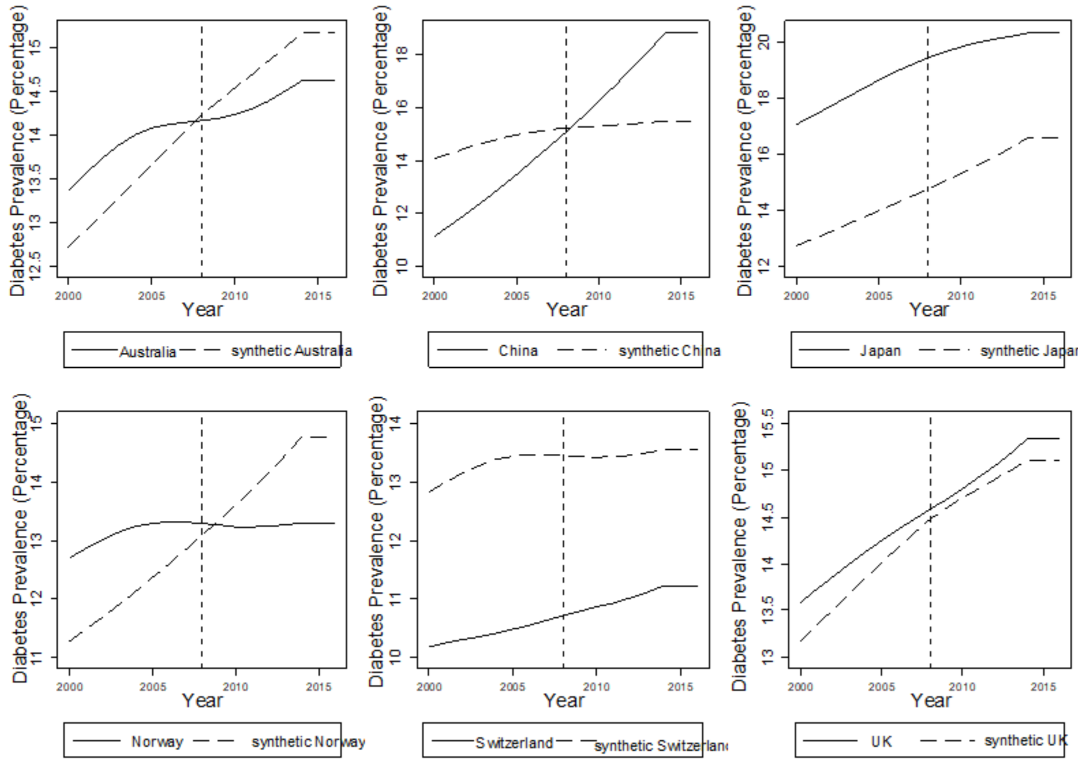


Figure 3.8: Trends in Diabetes Prevalence for Donor Countries

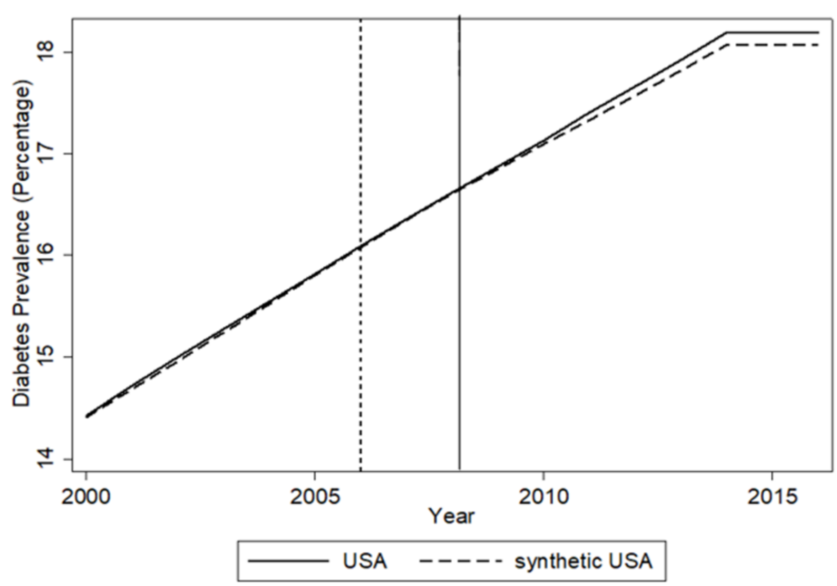


Figure 3.9: Trend in Diabetes Prevalence (Backdated to 2006)

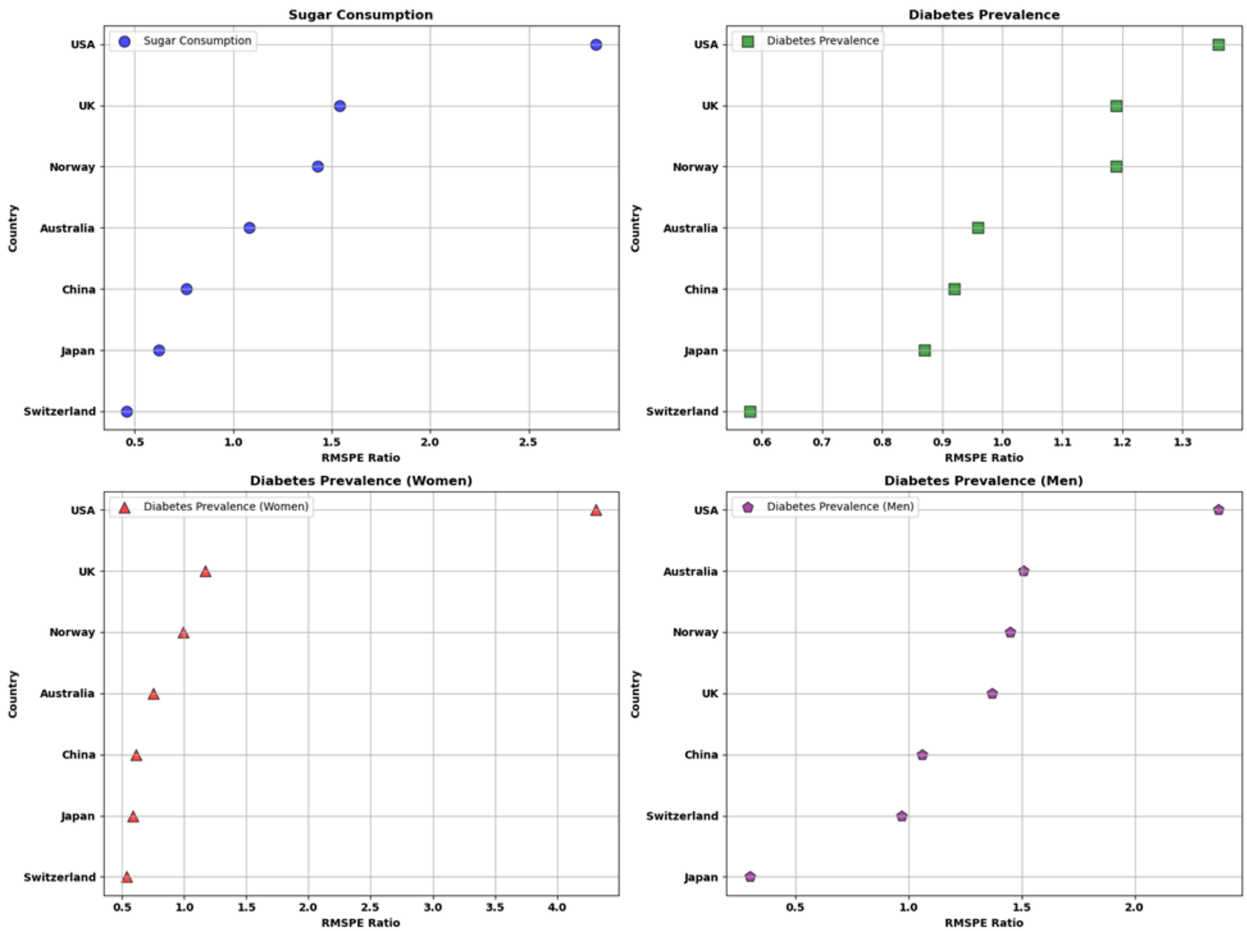


Figure 3.10: In-space Placebo Analysis (post-NAFTA RMSPE divided pre-NAFTA RMSPE)

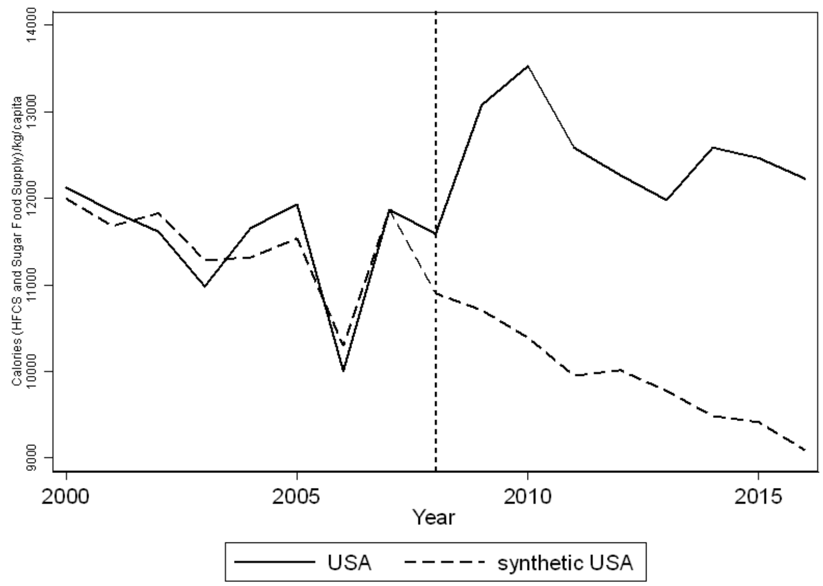


Figure 3.11: Trend of Calories (HFCS and Sugar Food Supply)

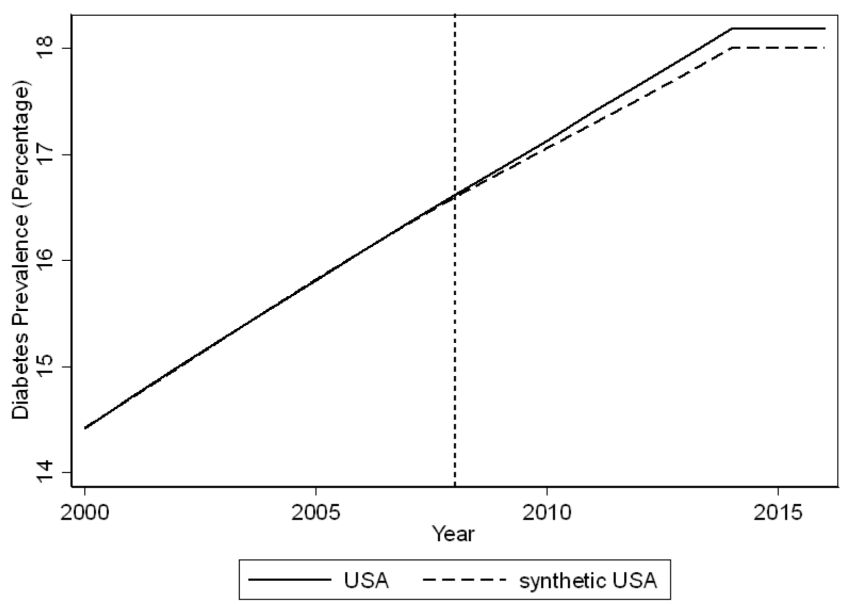


Figure 3.12: Trend in Diabetes Prevalence

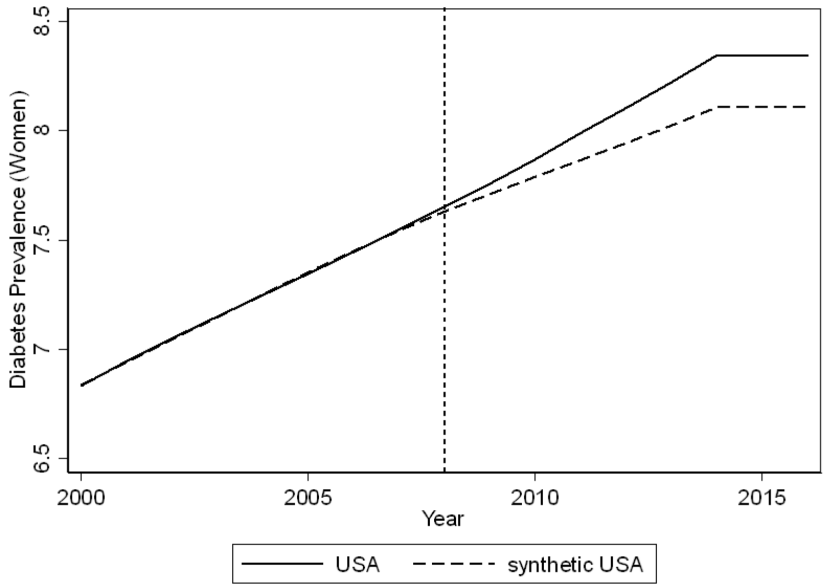


Figure 3.13: Trend in Diabetes Prevalence (Women)

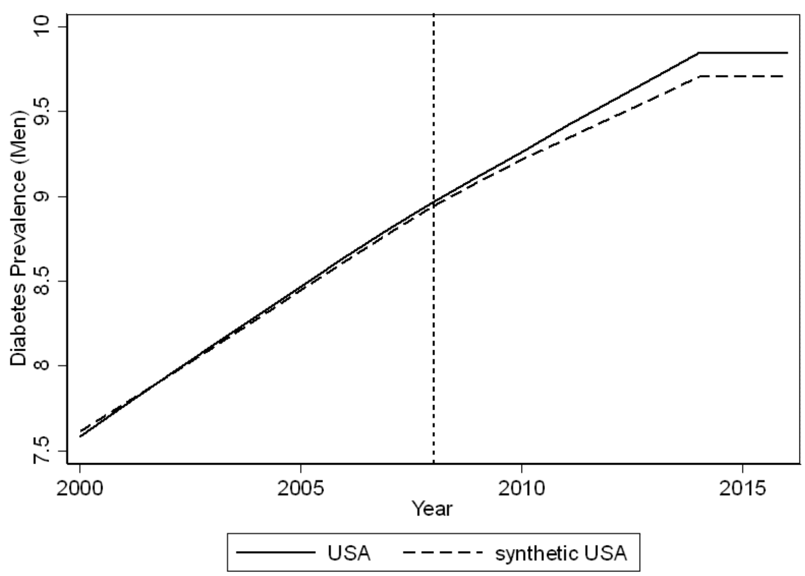


Figure 3.14: Trend in Diabetes Prevalence (Men)

Table 3.1: Average Treatment Effect for Sugar Consumption

Year	United States (Y_{Real})	Synthetic United States ($Y_{Synthetic}$)	Gap ($Y_{Real} - Y_{Synthetic}$)	% Gap
2000	30.77	30.25	0.51	2%
2001	30.07	29.39	0.67	2%
2002	29.47	30.51	-1.04	-4%
2003	27.87	28.78	-0.91	-3%
2004	29.58	29.03	0.56	2%
2005	30.27	29.80	0.46	2%
2006	25.39	26.39	-1.01	-4%
2007	30.12	30.18	-0.06	0%
Pre-Treatment Average			-0.10	0%
2008	29.41	28.64	0.77	3%
2009	33.19	27.70	5.48	17%
2010	34.33	27.66	6.67	19%
2011	31.93	26.20	5.73	18%
2012	31.13	26.78	4.35	14%
2013	30.40	26.45	3.95	13%
2014	31.94	24.91	7.02	22%
2015	31.63	25.27	6.36	20%
2016	31.03	24.18	6.85	22%
Post-Treatment Average			5.24	16%

Table 3.2: Average Treatment Effect for Diabetes Prevalence

Year	United States (Y_{Real})	Synthetic United States ($Y_{Synthetic}$)	Gap ($Y_{Real} - Y_{Synthetic}$)	% Gap
2000	14.42	14.42	0.00	0%
2001	14.71	14.70	0.01	0%
2002	15.00	14.98	0.01	0%
2003	15.27	15.27	0.00	0%
2004	15.55	15.55	-0.01	0%
2005	15.82	15.83	-0.01	0%
2006	16.09	16.09	0.00	0%
2007	16.36	16.35	0.01	0%
Pre-Treatment Average			-0.10	0.00
2008	16.62	16.59	0.03	0%

2009	16.88	16.82	0.06	0%
2010	17.13	17.05	0.08	0%
2011	17.41	17.28	0.13	1%
2012	17.66	17.51	0.16	1%
2013	17.93	17.75	0.18	1%
2014	18.19	17.99	0.20	1%
2015	18.19	17.99	0.20	1%
2016	18.19	17.99	0.20	1%
Post-Treatment Average			0.14	1%

Table 3.3: Average Treatment Effect for Diabetes Prevalence (Women)

Year	United States (Y_{Real})	Synthetic United States ($Y_{Synthetic}$)	Gap ($Y_{Real} - Y_{Synthetic}$)	% Gap
2000	6.83	6.84	-0.01	0%
2001	6.95	6.94	0.00	0%
2002	7.05	7.05	0.01	0%
2003	7.15	7.15	0.00	0%
2004	7.25	7.25	0.00	0%
2005	7.35	7.35	-0.01	0%
2006	7.45	7.45	0.00	0%
2007	7.55	7.54	0.01	0%
Pre-Treatment Average			0.00	0%
2008	7.65	7.63	0.02	0%
2009	7.76	7.71	0.05	1%
2010	7.87	7.79	0.08	1%
2011	7.99	7.87	0.12	2%
2012	8.11	7.95	0.16	2%
2013	8.22	8.03	0.20	2%
2014	8.35	8.11	0.24	3%
2015	8.35	8.11	0.24	3%
2016	8.35	8.11	0.24	3%
Post-Treatment Average			0.15	2%

Table 3.4: Average Treatment Effect for Diabetes Prevalence (Men)

Year	United States (Y_{Real})	Synthetic United States ($Y_{Synthetic}$)	Gap ($Y_{Real} - Y_{Synthetic}$)	% Gap
2000	7.58	7.61	-0.03	0%
2001	7.77	7.77	-0.01	0%
2002	7.95	7.94	0.01	0%
2003	8.12	8.11	0.02	0%
2004	8.30	8.28	0.02	0%
2005	8.47	8.45	0.02	0%
2006	8.64	8.62	0.03	0%
2007	8.81	8.79	0.02	0%
Pre-Treatment Average			0.01	0%
2008	8.97	8.95	0.02	0%
2009	9.12	9.09	0.03	0%
2010	9.26	9.22	0.04	0%
2011	9.42	9.35	0.07	1%
2012	9.56	9.47	0.09	1%
2013	9.70	9.59	0.11	1%
2014	9.85	9.72	0.13	1%
2015	9.85	9.72	0.13	1%
2016	9.85	9.72	0.13	1%
Post-Treatment Average			0.08	1%

Heterogeneity Estimation Tables and Figures

Table 3.5: Difference-in-differences Estimated Results by State

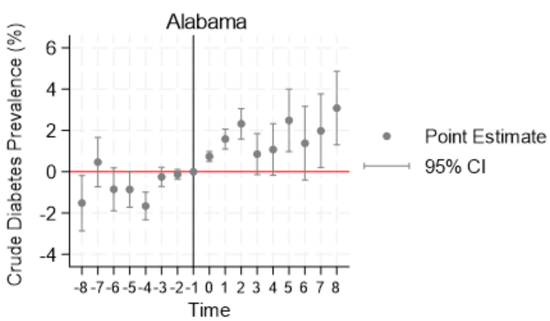
States	Estimates	States	Estimates
Alabama	2.321***	Montana	0.33
Alaska	0.858**	Nebraska	0.613**
Arizona	1.216***	Nevada	1.130***
Arkansas	2.119***	New Hampshire	0.776**
California	0.809***	New Jersey	0.404
Colorado	0.302	New Mexico	1.939***
Connecticut	0.721**	New York	0.849***
Delaware	0.841**	North Carolina	1.054***
Florida	1.286***	North Dakota	0.779***
Georgia	1.155***	Ohio	1.479***
Hawaii	0.299	Oklahoma	1.722***
Idaho	0.413	Oregon	1.049***
Illinois	0.710**	Pennsylvania	0.749***
Indiana	1.487***	Rhode Island	0.541*
Iowa	0.542*	South Carolina	1.265***
Kansas	1.258***	South Dakota	0.29
Kentucky	1.824***	Tennessee	1.166***
Louisiana	1.709***	Texas	1.216***
Maine	0.777***	Utah	0.138
Maryland	1.090***	Vermont	0.273
Massachusetts	0.766***	Virginia	1.206***
Michigan	0.774***	Washington	0.591**
Minnesota	0.544**	West Virginia	1.526***
Mississippi	1.512***	Wisconsin	0.911***
Missouri	1.444***	Wyoming	0.754***

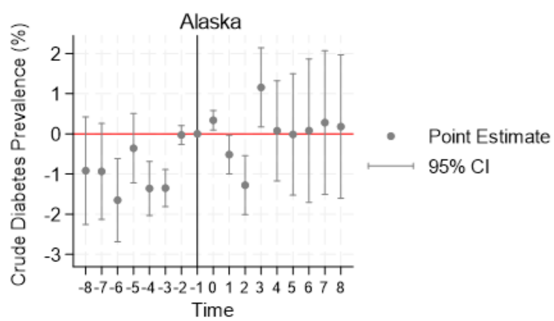
Note: *** p<0.01, ** p<0.05, * p<0.1.

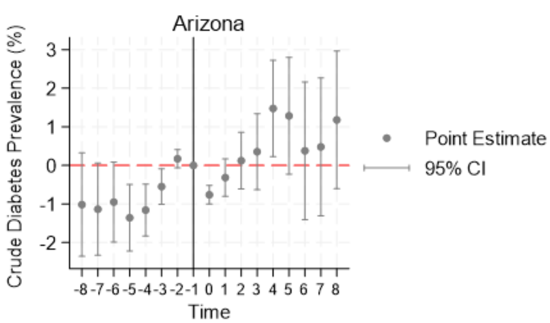
Table 3.6: Post-Treatment - Average Treatment Effect on the Treated (ATET)

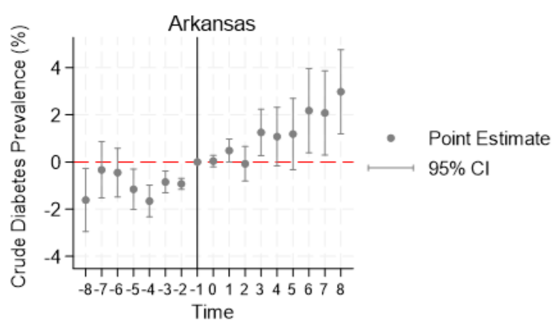
Year	ATET	Robust SD	P-value	[95% CI]
2008	0.07	0.08	0.38	[-0.09; 0.23]
2009	0.23	0.11	0.03	[0.02; 0.45]
2010	0.25	0.09	0.00	[0.07; 0.43]
2011	0.92	0.10	0.00	[0.72; 1.13]
2012	0.99	0.12	0.00	[0.76; 1.21]
2013	0.96	0.11	0.00	[0.75; 1.18]
2014	1.13	0.10	0.00	[0.92; 1.33]
2015	1.11	0.13	0.00	[0.86; 1.36]
2016	1.43	0.13	0.00	[1.18; 1.68]

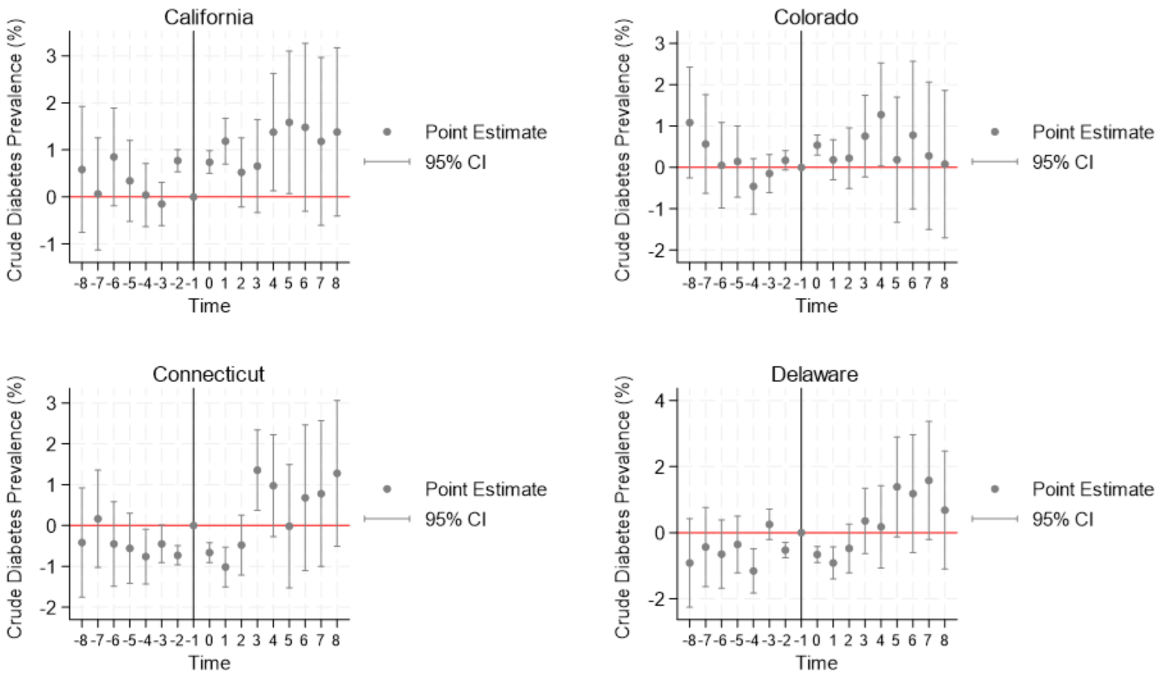
Number of Observations: 952

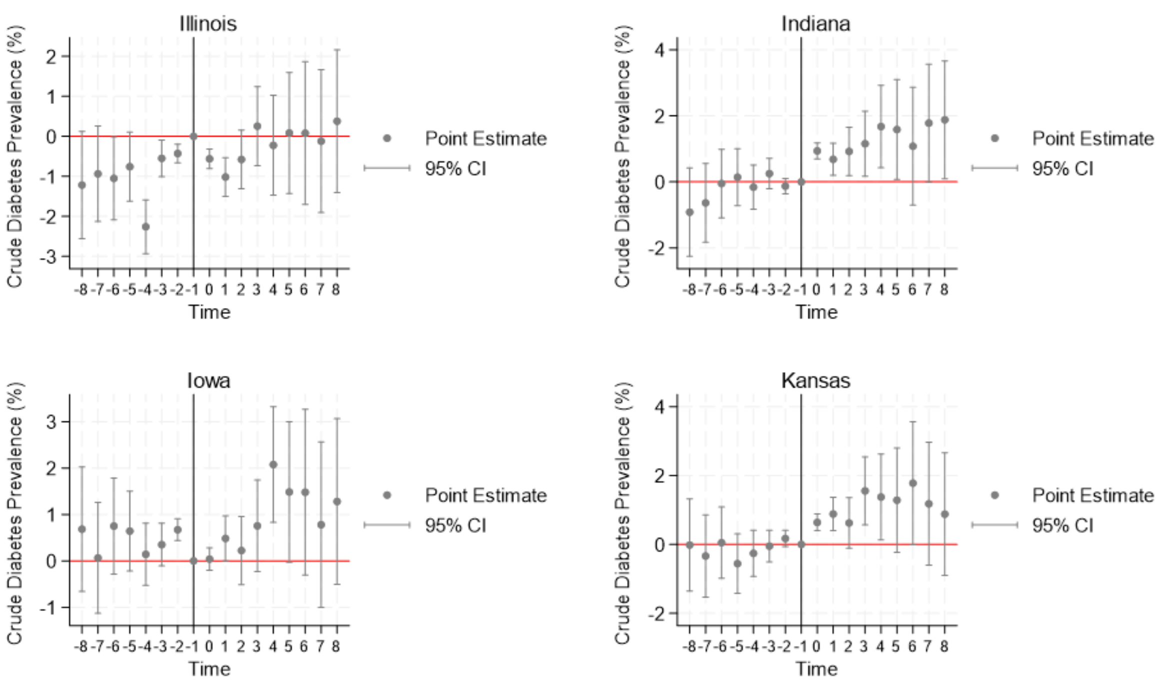


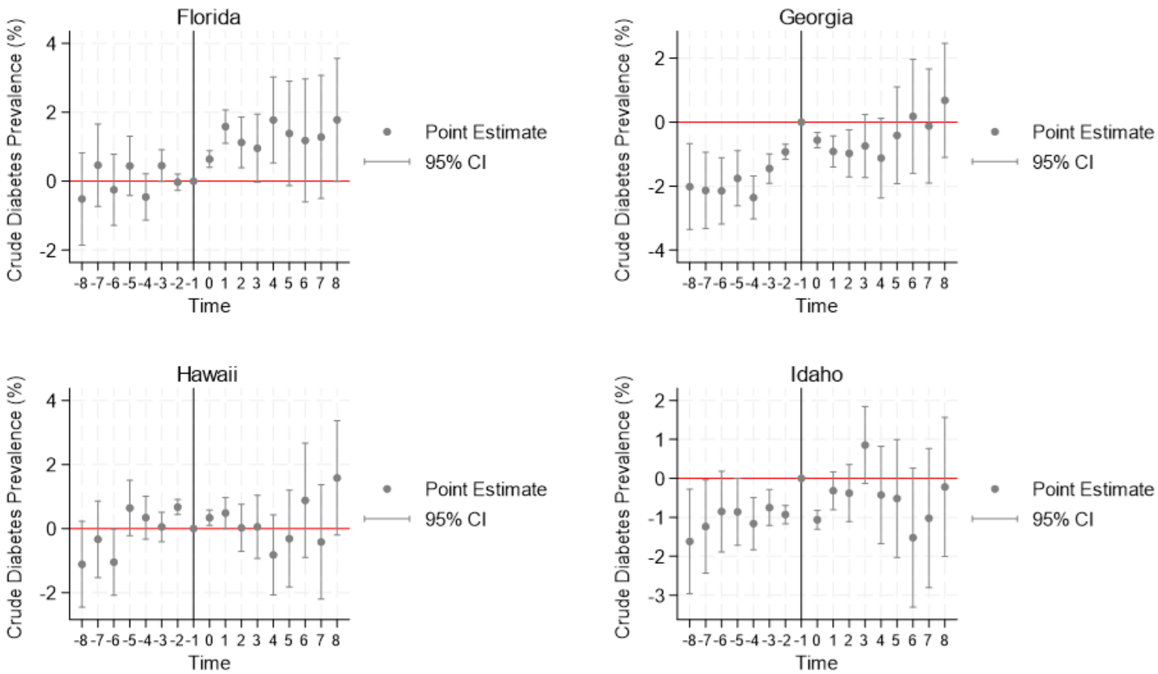


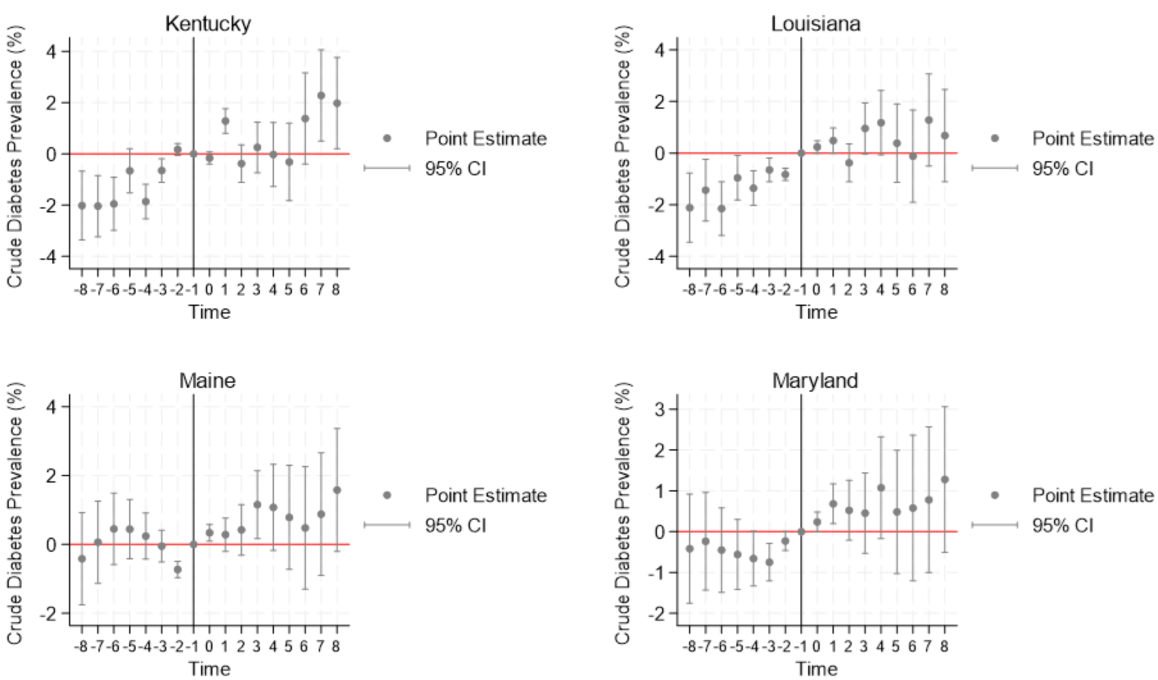


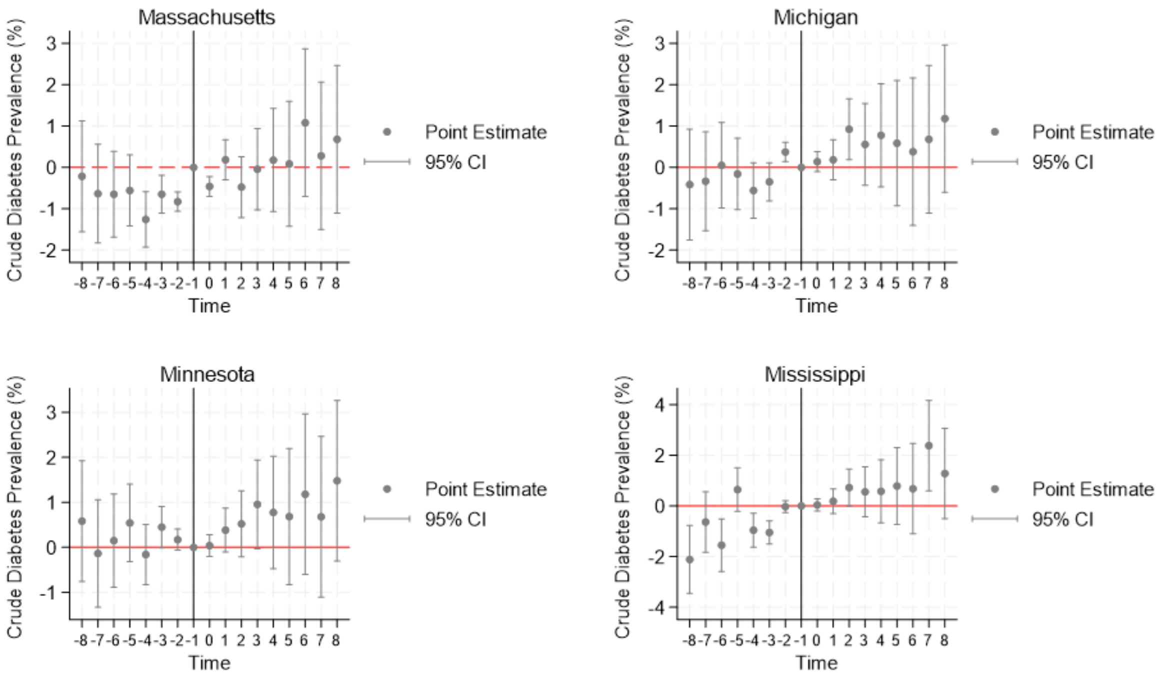


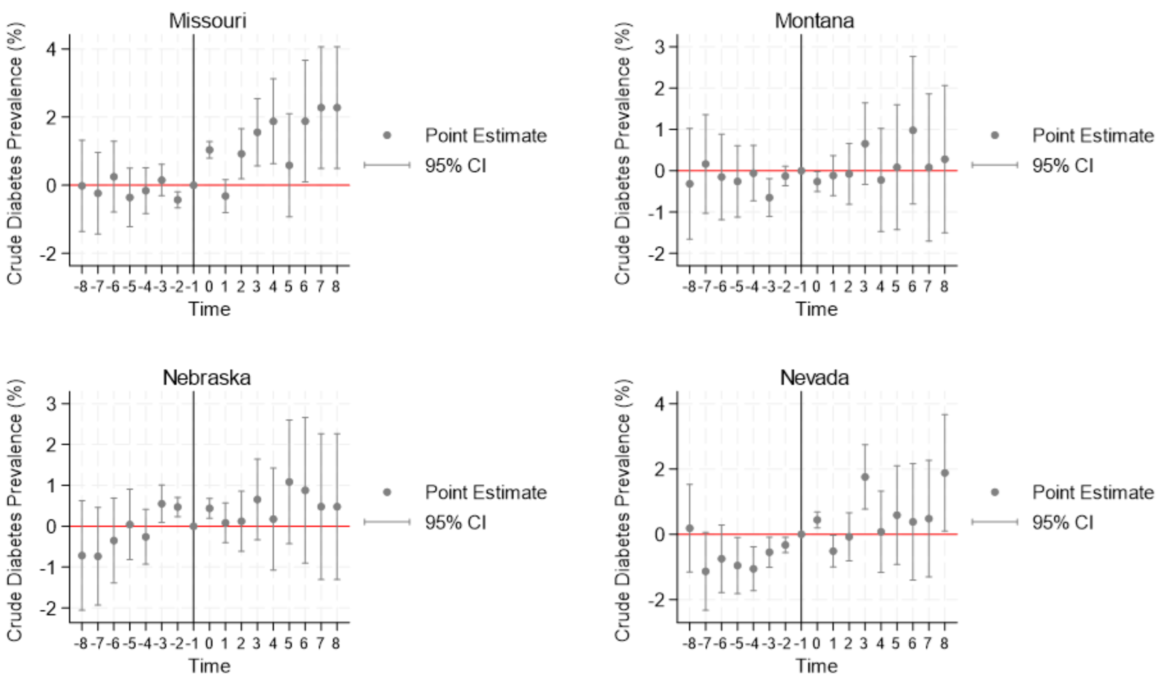

California
Colorado
Connecticut
Delaware
Crude Diabetes Prevalence (%)
Time
Point Estimate
95% CI

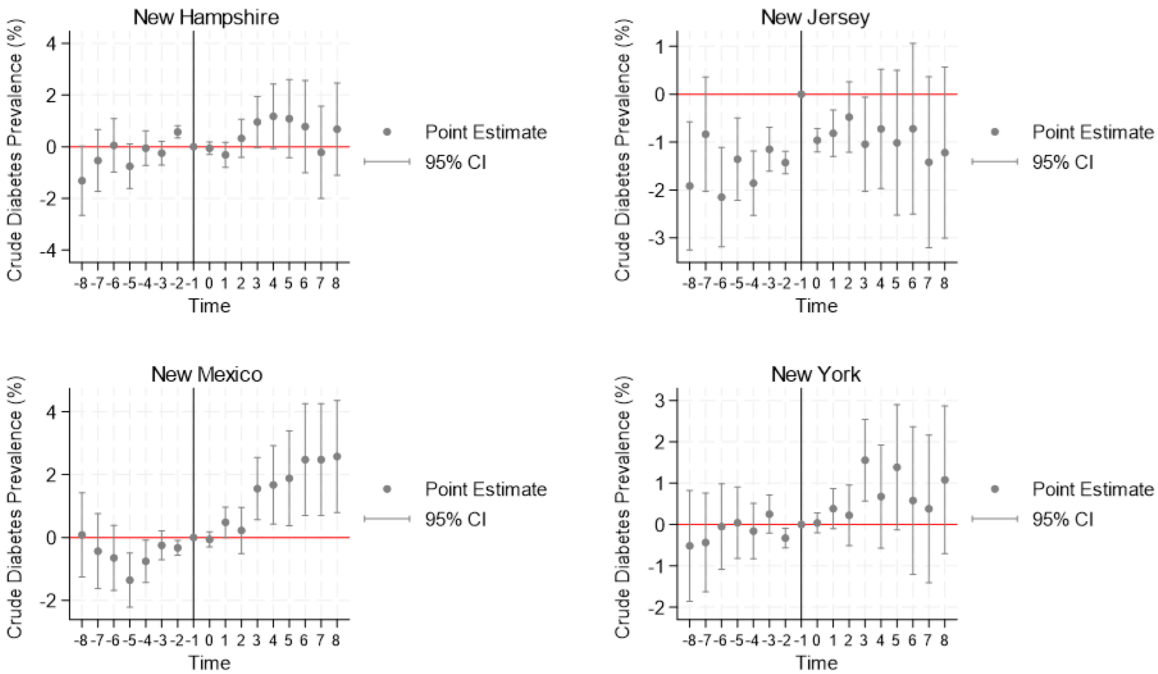

Illinois
Indiana
Iowa
Kansas
Crude Diabetes Prevalence (%)
Time
Point Estimate
95% CI

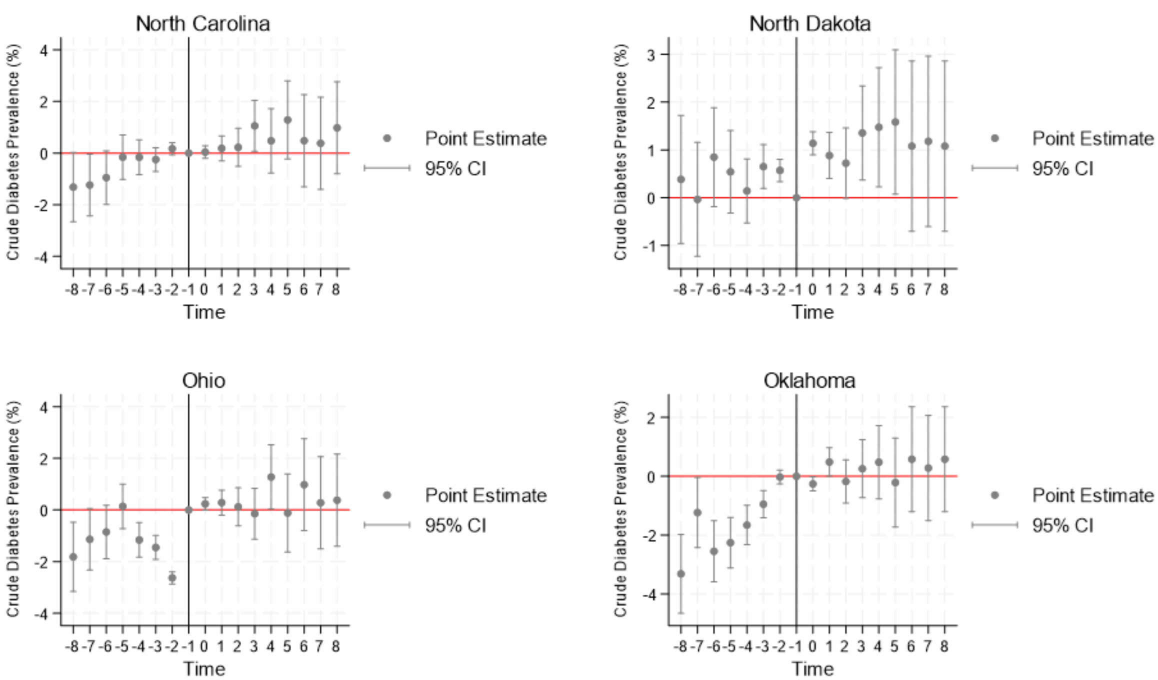

Florida
Georgia
Hawaii
Idaho
Crude Diabetes Prevalence (%)
Time
-8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8
Point Estimate
95% CI

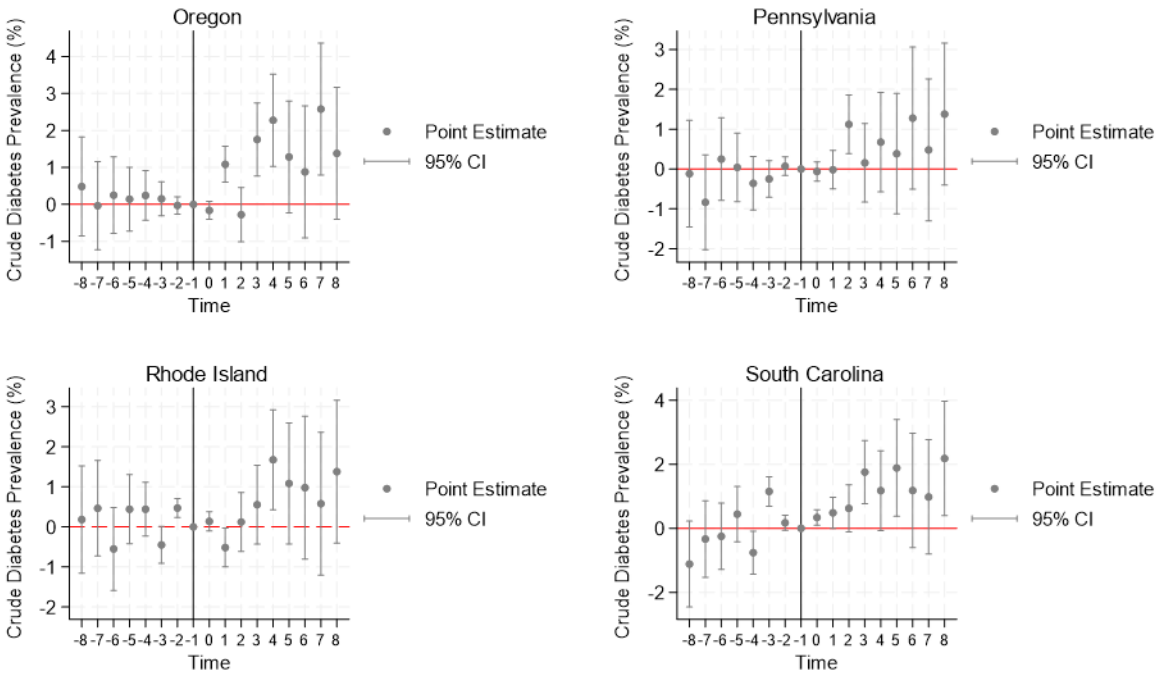

Kentucky
Louisiana
Maine
Maryland
Crude Diabetes Prevalence (%)
Time
-8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8
Point Estimate
95% CI

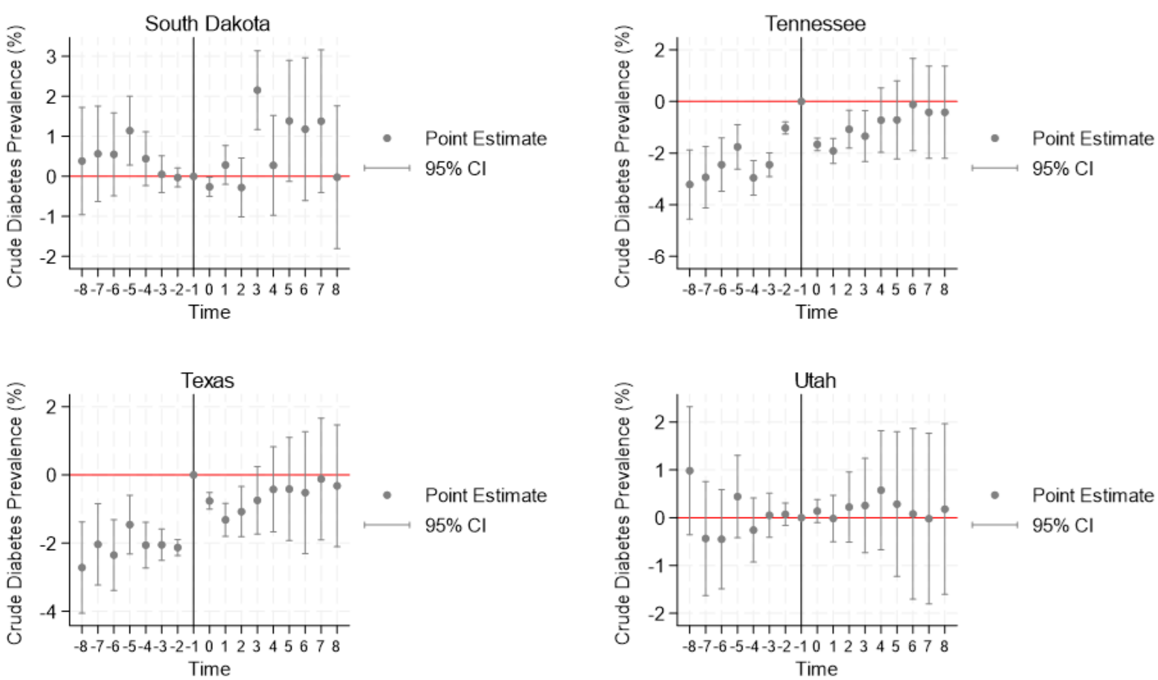

Massachusetts
Michigan
Minnesota
Mississippi
Crude Diabetes Prevalence (%)
Time
Point Estimate
95% CI


Missouri
Montana
Nebraska
Nevada
Crude Diabetes Prevalence (%)
Time
Point Estimate
95% CI


New Hampshire
New Jersey
New Mexico
New York
Crude Diabetes Prevalence (%)
Time
Point Estimate
95% CI


North Carolina
North Dakota
Ohio
Oklahoma
Crude Diabetes Prevalence (%)
Time
Point Estimate
95% CI


Oregon
Pennsylvania
Rhode Island
South Carolina
Crude Diabetes Prevalence (%)
Time
Point Estimate
95% CI


South Dakota
Tennessee
Texas
Utah
Crude Diabetes Prevalence (%)
Time
Point Estimate
95% CI

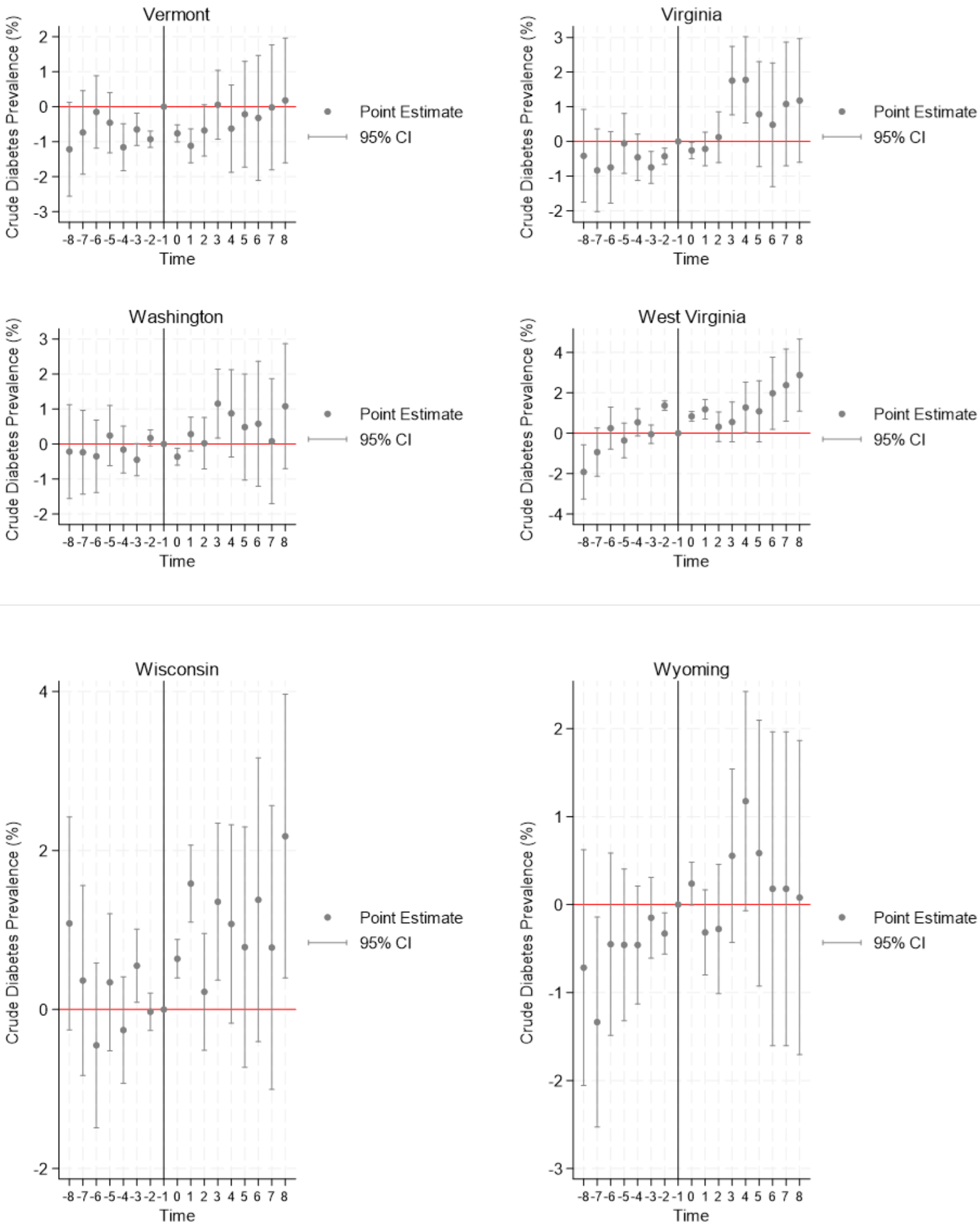
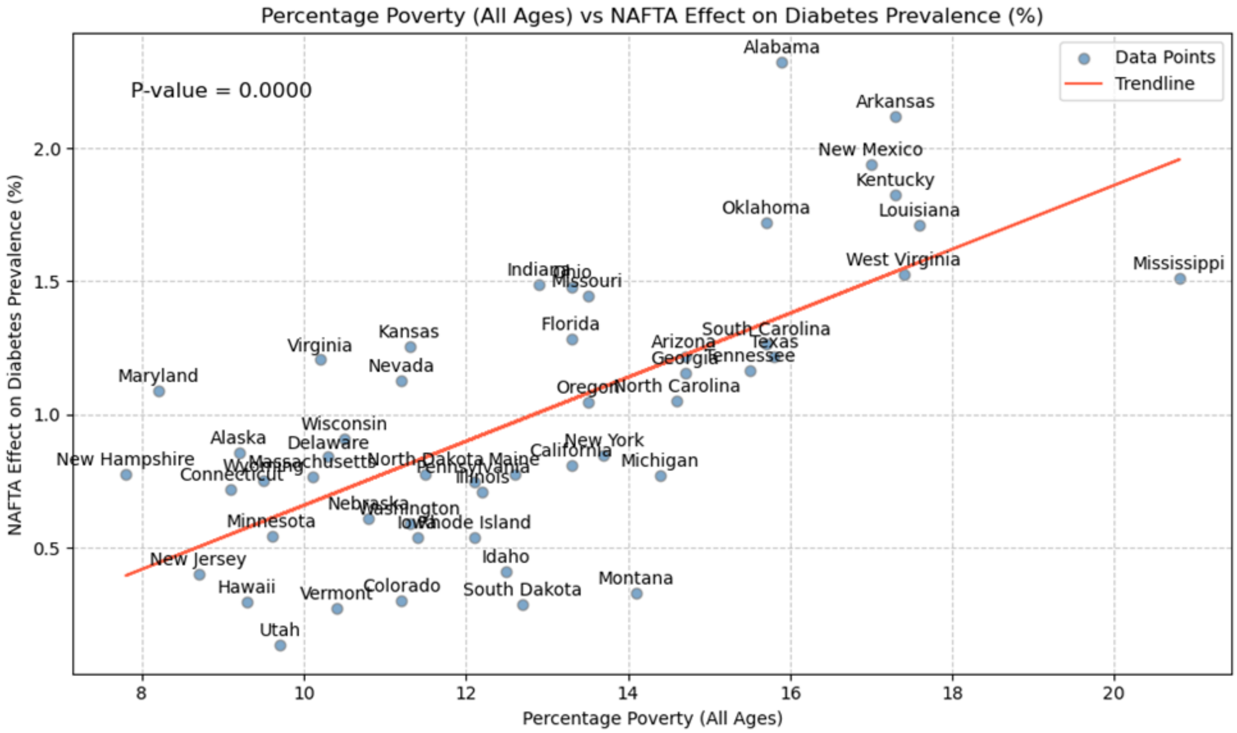
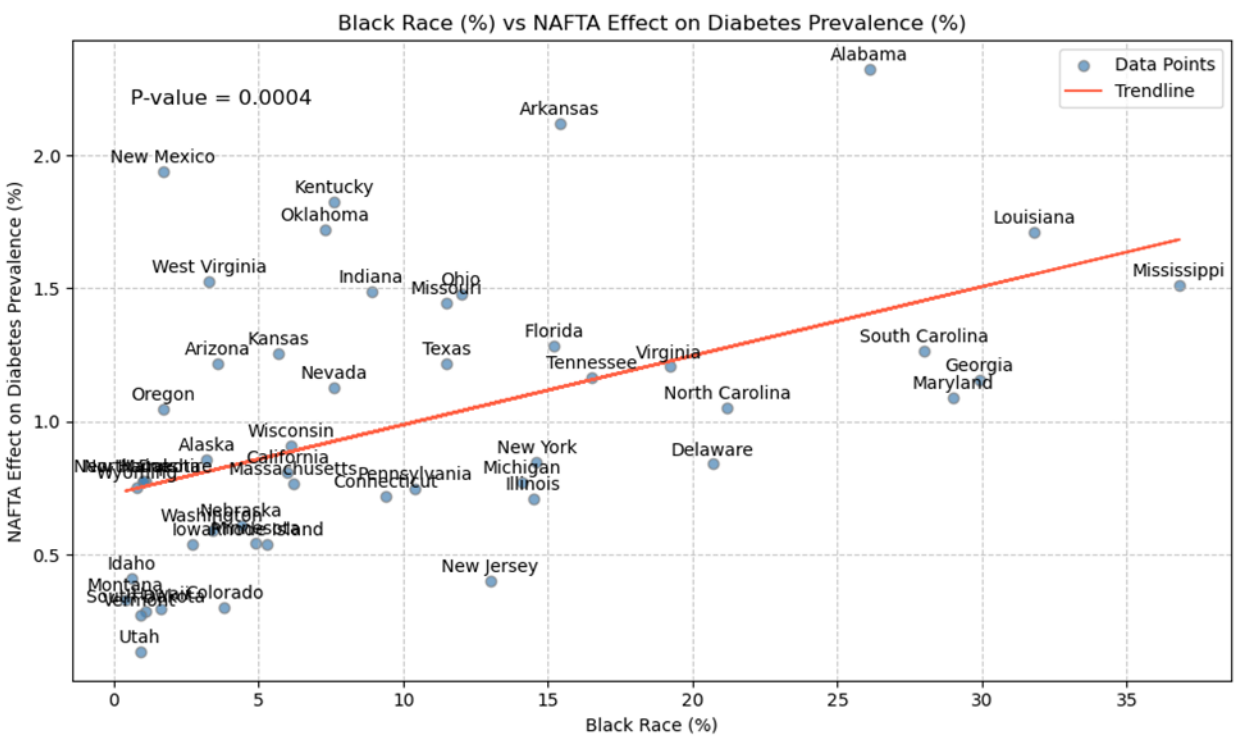


Figure 3.15: Panel Event-Study Results


Percentage Poverty (All Ages) vs NAFTA Effect on Diabetes Prevalence (%)
P-value = 0.0000
Data Points
Trendline
NAFTA Effect on Diabetes Prevalence (%)
Percentage Poverty (All Ages)
Alabama
Arkansas
New Mexico
Kentucky
Oklahoma
Louisiana
West Virginia
Mississippi
Missouri
Florida
Kansas
Virginia
Nevada
Maryland
South Carolina
Arizona
Tennessee
North Carolina
Wisconsin
Alaska
Delaware
New Hampshire
New York
Michigan
Minnesota
New Jersey
Idaho
Hawaii
Colorado
South Dakota
Montana
Utah


Black Race (%) vs NAFTA Effect on Diabetes Prevalence (%)
P-value = 0.0004
Data Points
Trendline
NAFTA Effect on Diabetes Prevalence (%)
Black Race (%)
Alabama
Arkansas
New Mexico
Kentucky
Oklahoma
Louisiana
West Virginia
Indiana
Mississippi
Kansas
Arizona
Texas
Florida
Nevada
Tennessee
Virginia
South Carolina
Georgia
Maryland
Oregon
North Carolina
Wisconsin
Alaska
New York
Delaware
Michigan
Illinois
New Jersey
Idaho
Colorado
Utah

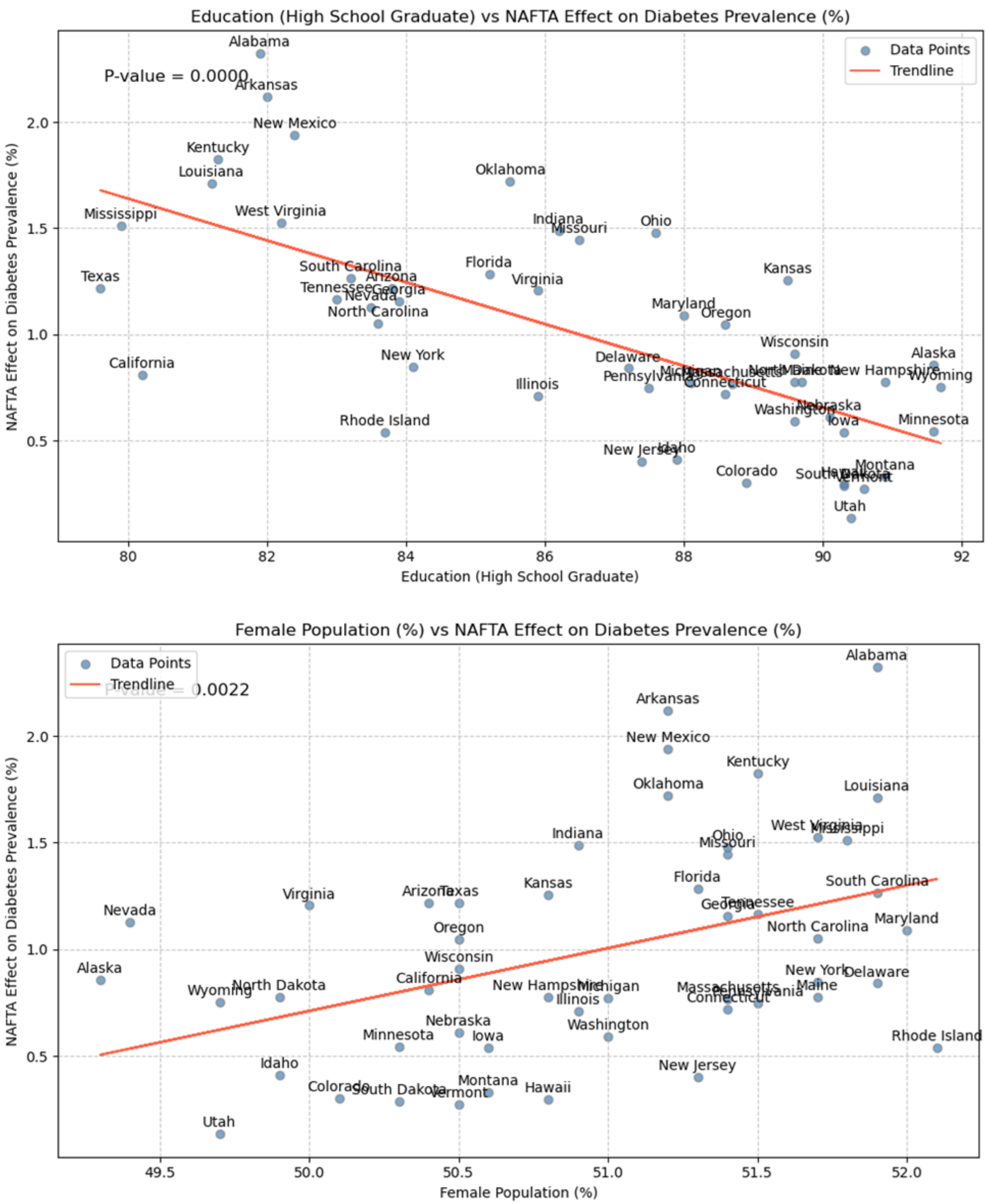


Figure 3.16: Selected Covariates and the NAFTA Effect on Diabetes Prevalence

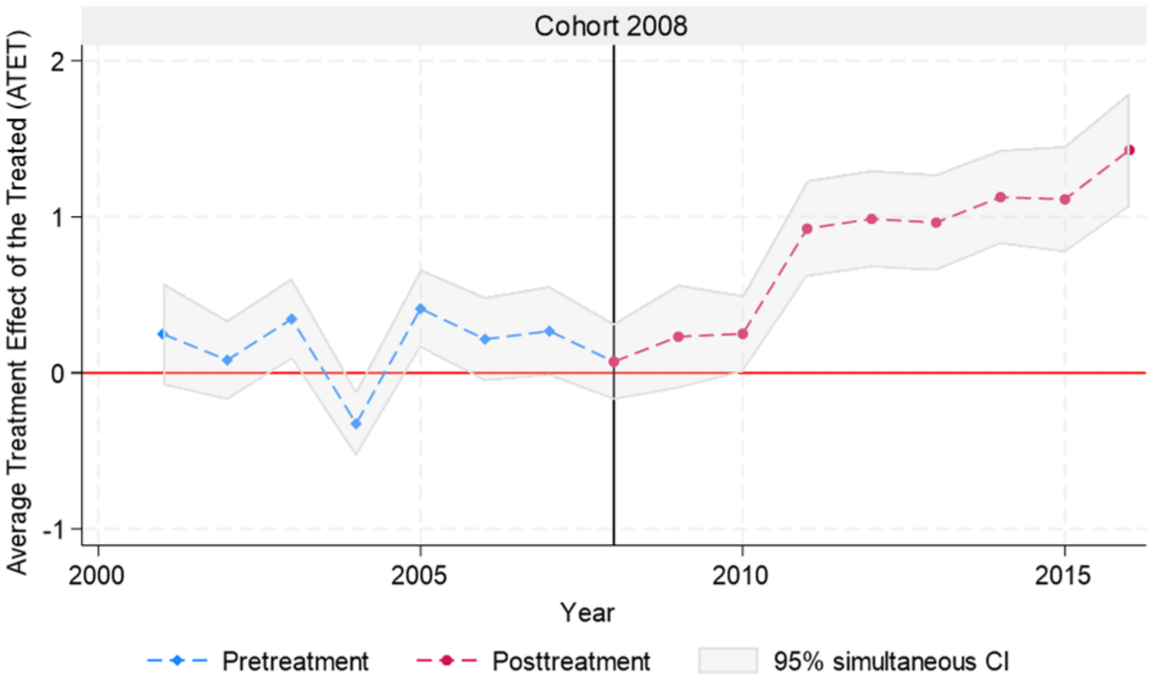


Figure 3.17: ATET in Pretreatment and Posttreatment Years

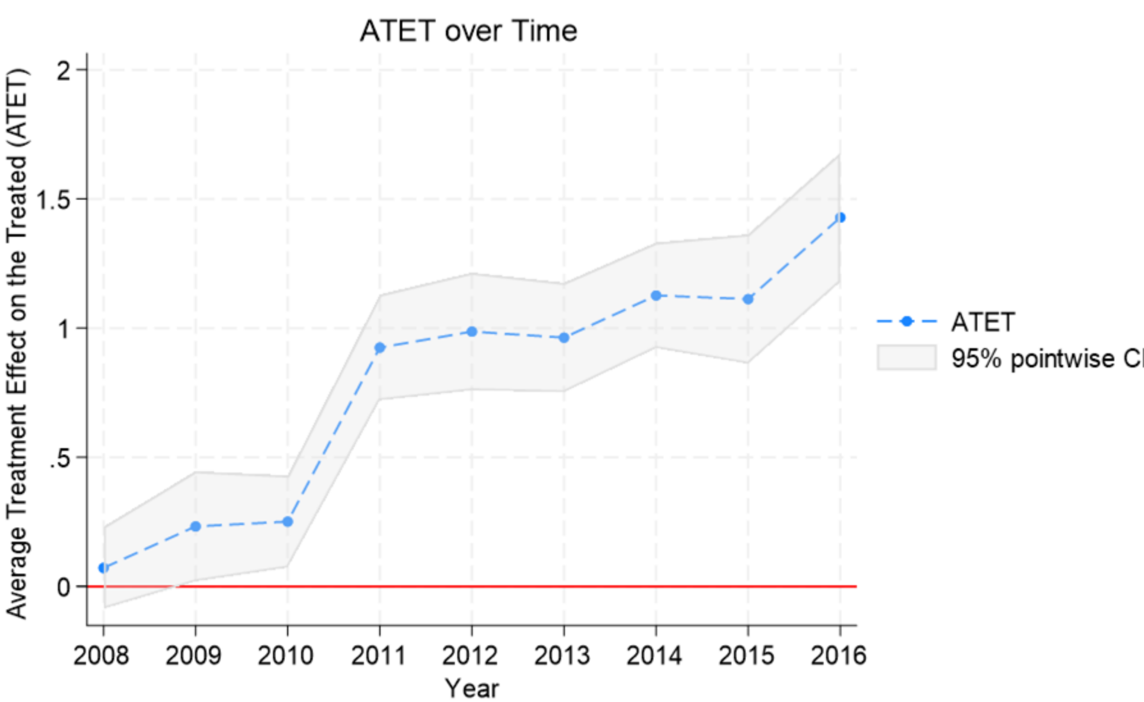


Figure 3.18: ATET in Posttreatment Years

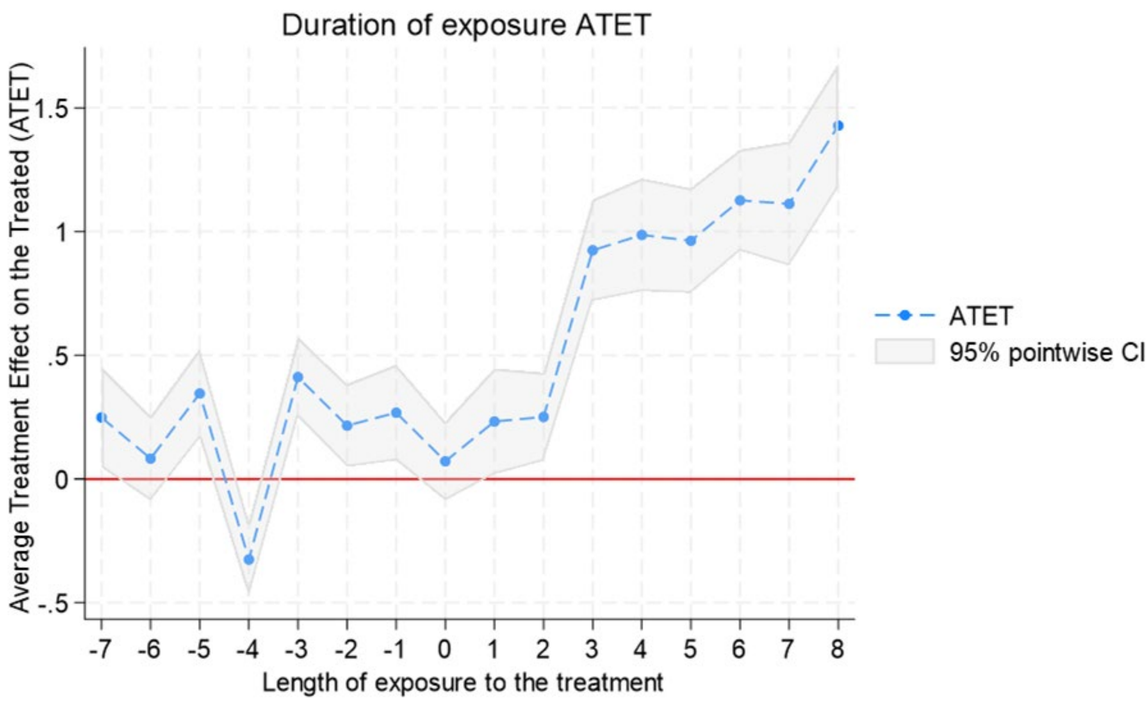
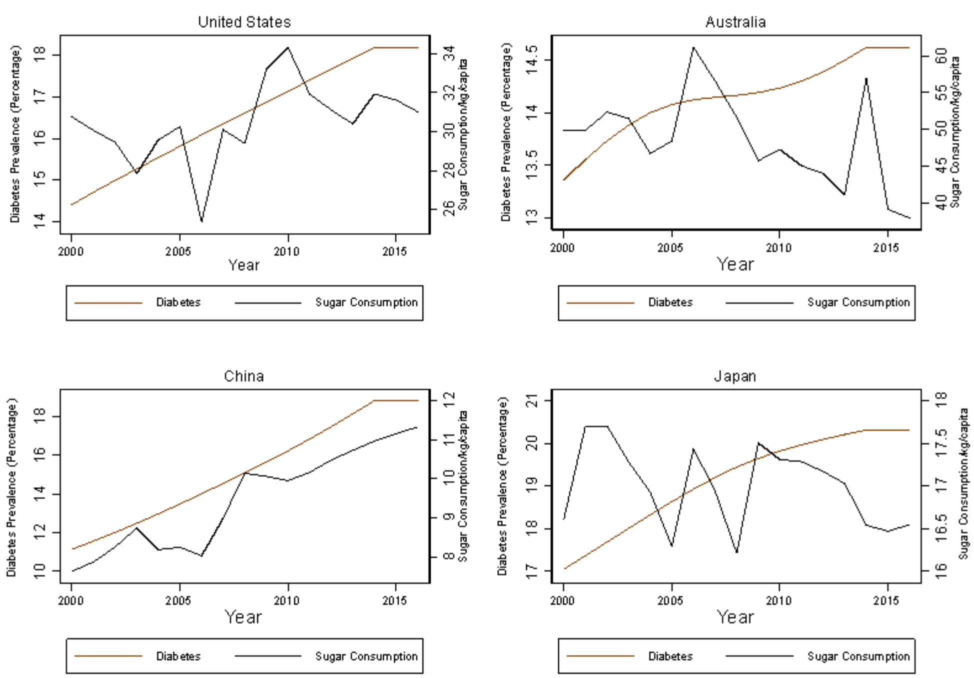


Figure 3.19: ATETs over Different Lengths of Exposure to Treatment

Appendix



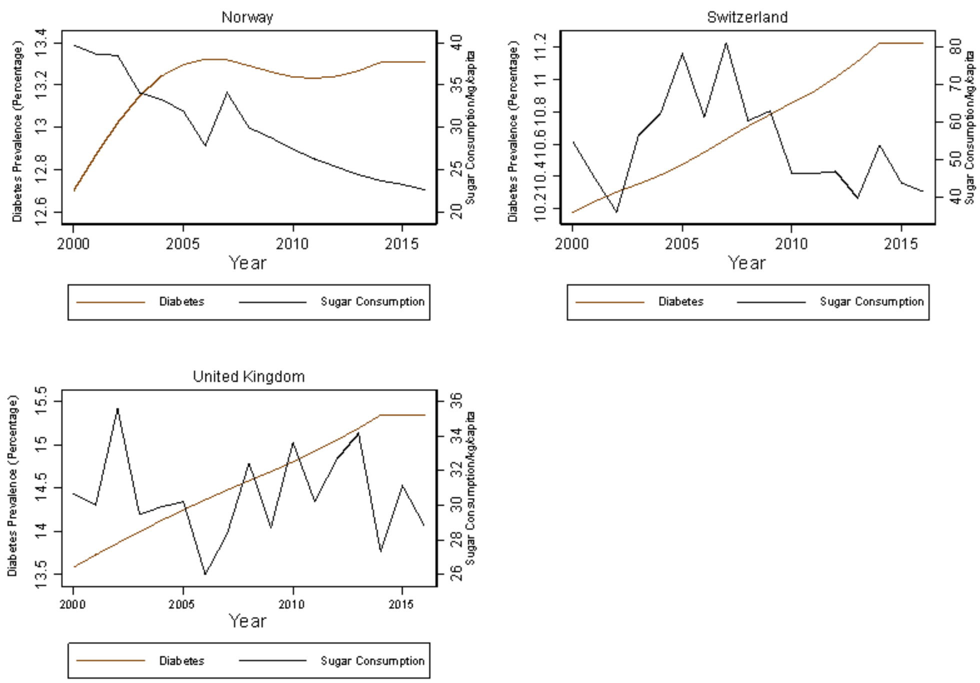


Figure 3.A1: Trends of Sugar Consumption, and Diabetes prevalence

Table 3.A1: Descriptive Statistics (All Countries)

Variables	Mean	Std. Dev.	Min.	Max.
Crude Prevalence of Diabetes (%)	14.79	2.73	10.18	20.32
Sugar consumption (kg per Capita)	31.44	15.80	7.63	81.26
Trade openness	46.90	29.10	14.25	131.80
GDP per capita (Current US$)	45101.35	4950.12	959.37	102913.50
Raw Sugar Import (metric tons)	1299.30	1290.19	24.00	5833.00
Population aged above 65 (%)	14.90	4.09	6.81	26.59
Insufficient Physical activity age above 18 (%)	30.19	8.13	8.13	40.02
Average Total Years of School aged above 18	11.65	1.97	6.50	13.40
Producer Price of Sugar (US$)	1297.83	1572.79	120.70	7113.00
Hypertension Prevalence (%)	0.69	0.08	0.49	0.81
Crude Raised Blood Pressure (%)	0.47	0.08	0.33	0.63
Mean Total Cholesterol (mmol/L)	9.96	0.53	8.85	10.95
Sugar Food Supply (Kcalcapita-1day-1)	307.82	137.97	56	542

Alcoholic Beverage (Kcalcapita-1day-1)	139.89	31.29	58	192
Total Calories Consumption (kg per capita)	12395.05	6225.258	3005.345	32025.44

Table 3.A2: Weight Assigned to Donor Pool

Country	Weight			
	Sugar Consumption	Diabetes Prevalence	Diabetes-Women	Diabetes-Men
Australia	0	0.177	0	0
China	0	0.219	0.336	0.024
Japan	0.33	0.405	0.462	0.557
Norway	0.259	0	0	0
Switzerland	0.077	0	0	0.419
United Kingdom	0.335	0.199	0.202	0
RMSPE	0.720	0.009	0.005	0.02

Note: with synthetic control method extrapolation is not allowed so all weights are between $0 \leq wj \leq 1$ and $\Sigma wj = 1$:

Table 3.A3: Sugar Consumption Predictor Mean

Variables	Treated	Synthetic	Donor pool
log Producer Price of Sugar (US$)	6.15	6.14	6.59
log GDP per capita (Current US$)	10.63	10.63	10.33
Population aged above 65 (%)	12.33	16.66	15.21
log Raw Sugar Import (metric tons)	7.58	6.64	6.19
Average Total Years of School Aged above 18	2.55	2.47	2.41
Sugar consumption ($Kg^{-1}Capita^{-1}$) (2002)	29.47	30.51	31.41
Sugar consumption ($Kg^{-1}Capita^{-1}$) (2003)	27.87	28.78	32.96
Sugar consumption ($Kg^{-1}Capita^{-1}$) (2007)	30.12	30.18	37.71
Sugar consumption ($Kg^{-1}Capita^{-1}$) (2005)	31.63	25.27	27.50

Table 3.A4: Diabetes Prevalence Predictor Mean

Variables	Treated	Synthetic	Donor Pool
log Population aged above 65 (%)	2.51	2.62	2.68
Insufficient Physical activity age above 18 (%)	40.02	29.98	28.55
Average Total Years of School aged above 18	12.78	10.45	11.42
log Producer Price of Sugar (US$)	6.15	5.96	6.60
Hypertension Prevalence (%)	0.67	0.69	0.69
Raised Blood Pressure (%)	0.34	0.50	0.49
Mean Total Cholesterol (mmol/L)	9.82	9.94	10.01
Sugar Food Supply ($\text{Kcalcapita}^{-1}\text{day}^{-1}$)	322.50	230.05	306.42
Alcoholic Beverage ($\text{Kcalcapita}^{-1}\text{day}^{-1}$)	165.88	128.30	135.81
Diabetes Prevalence (2003)	15.27	15.27	15.40
Diabetes Prevalence (2007)	16.09	16.09	14.39
Diabetes Prevalence (2005)	16.88	16.82	14.70

Table 3.A5: Diabetes Prevalence (women) Predictor Mean

Variables	**Treated**	**Synthetic**	**Donor pool**
Population aged above 65 (%)	12.33	14.38	15.21
Insufficient Physical activity age above 18 (%)	40.02	28.38	28.55
Average Total Years of School aged above 18	12.78	9.80	11.42
log Producer Price of Sugar (US$)	6.15	6.20	6.60
Hypertension Prevalence (%)	0.67	0.69	0.69
Raised Blood Pressure (%)	0.34	0.50	0.49
Mean Total Cholesterol (mmol/L)	9.82	9.77	10.01
Sugar Food Supply ($\text{Kcalcapita}^{-1}\text{day}^{-1}$)	322.50	178.45	306.42
Alcoholic Beverage ($\text{Kcalcapita}^{-1}\text{day}^{-1}$)	165.88	120.07	135.81
Diabetes Prevalence (women) (2007)	7.76	7.71	6.39
Diabetes Prevalence (women) (2006)	7.87	7.79	6.46
Diabetes Prevalence (women) (2005)	7.55	7.54	6.50

Table 3.A6: Diabetes Prevalence (men) Predictor Mean

Variables	Treated	Synthetic	Donor Pool
Population aged above 65 (%)	12.33	17.22	15.21
Average Total Years of School aged above 18	12.78	11.29	11.42
Hypertension Prevalence (%)	0.67	0.73	0.69
Raised Blood Pressure (%)	0.34	0.54	0.49
Mean Total Cholesterol (mmol/L)	9.82	10.24	10.01
Sugar Food Supply (Kcalcapita^{-1}day^{-1})	322.50	318.28	306.42
Alcoholic Beverage (Kcalcapita^{-1}day^{-1})	165.88	152.07	135.81
Diabetes Prevalence (men) (2001)	7.77	7.77	7.13
Diabetes Prevalence (men) (2007)	8.81	8.79	7.99
Diabetes Prevalence (men) (2005)	9.26	9.22	8.37

Chapter 4

Revisiting the Impact of Export Diversification on Economic Growth in Sub-Saharan Africa[28]

Introduction

In the 2000s, many African countries experienced relatively significant economic growth, termed the "African economic renaissance". Both export diversification and specialization were used to boost exports of goods and services and to promote economic growth, social development, and poverty reduction (Coulibaly and Akia 2019). Exports provide markets for goods and services and bring in foreign currency. Exports remain one of the few ways for developing countries to increase economic growth in the face of low domestic demand (Hesse 2008). The current study investigates if export diversification contributed to the uneven development in Sub-Saharan Africa (SSA) since 2000.[29]

There are two schools of thought regarding economic growth and the composition of export structure. Neoclassical theories of international trade, such the Ricardian and Heckscher-Ohlin-Samuelson models, advocate that nations should specialize in the manufacture and export of commodities for which they have a comparative advantage to ensure efficiency (see Golub and Hsieh 2000; Plümper and Graff 2001; Bernhofen and Brown 2005; Lee 2011). In contrast, other trade theory suggests that specialization is less effective at promoting growth in the presence of instability or uncertainty (Turnovsky 1974; Ruffin 1974; Osakwe 2007; Gervais 2018; De Sousa et al., 2020). Prebisch (1962) and Singer (1950) have argued that the neoclassical trade theory

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[28] **Authors:** Derick Taylor Adu, Dr. Valentina Hartarska, Dr. Wendiam Sawadgo, Dr. Henry Thompson

[29] Export diversification is the opposite of specialization, that is not restricting the export portfolio to a limited number of export goods (Hirsch and Lev 1971; Love 1983).

based on specialization may not be the best framework for understanding the challenges in Africa. Since the relative prices of primary goods fall, specialization may not be helpful to SSA countries who are primarily commodity exporters.

Governments and policymakers in the region have introduced a number of policy efforts to encourage economic growth through export diversification. A plan for export diversification was proposed by the African Union Summit in 2012 to encourage intra-African industrial trade. Specifically, to considerably increase intra-African trade, particularly in value-added production and trade across all sectors of the African economy, African governments and policymakers signed the African Continental Free Trade Area (AfCFTA) in March 2018. The United Nations Economic Commission for Africa (UNECA) predicts that this agreement will have a significant impact, with intra-regional trade in the African sub-region increasing by 15-25%, or $50 to $70 billion, by 2040. According to the International Monetary Fund (IMF), the agreement could significantly boost the region's overall rating on the Global Competitiveness Index, which could improve competitiveness, resource allocation efficiency, and lead to economies of scale. Yet, for SSA nations to be successful in export diversification, their exports must be globally competitive to gain access to global markets.

A number of empirical studies have looked at how export diversification efforts affect economic growth and have reached divergent conclusions. Part of the differences may be due to variations in the estimation methods, which include ordinary least squares (Ee 2016; Tesfay 2016), generalized method of moment (GMM) (Aditya and Acharyya 2013; Hesse 2008; Hodey et al., 2015; Fu et al., 2017; Lectard & Rougier 2018; Maina & Rieber 2019; Jongwanich 2020), auto-regressive distributed lag (Hinlo & Arranguez, 2017; Francis et al., 2007; Duru & Ehidiamhen 2018), time series error-correction model (Akter 2020; Herzer & Nowak-Lehnmann 2006; Forgha et al., 2014; Lotfi & Karim 2017), fixed effect model (Gurgul and Lach 2013), trend analysis (Karahan 2017), and simulation (Teignier 2018). Previous work in general aims to address the simultaneity bias, in which export diversification has been shown to boost growth, but the opposite is also possible - growth can spur more export growth by encouraging technology adoption and increasing imports consumed as inputs to produce export-oriented commodities (termed as "reverse causality"[30]). Unlike previous studies, we use the Arellano-Bond difference generalized method-of-moments (GMM) estimator (hence referred to as the AB estimator) which can address simultaneity bias between export diversification and economic growth through the use of lags of the dependent variable as explanatory variables, (Ullah et al., 2018). Moreover, our analysis uses recent data for the period 2000-2018.

While trade diversification is likely to affect economic growth, the quality of institutional governance and corruption are also found to influence growth. However, prior trade studies do not explicitly control for these factors despite the evidence that they are crucial to the economies of SSA nations in all areas, particularly the export sector (see for example, Anderson and Marcouiller 2002; Gyimah-Brempong 2002; Bates et al., 2013; Álvarez et al., 2018; Bilgin et al., 2018; Opeyemi et al. 2019; Abreo et al., 2021; Agyei & Idan 2022). Due to the lack of strong institutions, corruption is more common in developing nations, especially those in Sub-Saharan Africa (SSA), and it has a significant negative impact on every area of their economies. For instance, with an average score of 32, the SSA was identified as the region with the highest level of corruption in 2022 by Transparency International.[31] According to the African Development Bank, corruption

[30] Some authors use the term "reverse causality" exclusively for situations in which Y affects X but X does not affect Y, while referring to situations where X and Y affect one another as "reciprocal causality" instead. Arguing that the arrow from Y to X is key, we use the term "reverse causality" to denote any situation in which Y affects X, X also affects Y (Leszczensky & Wolbring, 2022).

[31] https://www.transparency.org/en/news/cpi-2022-sub-saharan-africa-corruption-compounding-multiple-crises. (Accessed on March 12, 2023).

costs the African continent $300 billion yearly, or 25% of GDP, more than aid and donor inflows (Lumumba 2014). Corruption has a negative impact on investments in Africa, which leads to an uneven distribution of infrastructure and resources, erodes democracy and governance, and reduces both competitiveness and income (Evans, 2004). Furthermore, Pomfret and Sourdin (2010), and Shirazi (2012) find that corruption raises the cost of trade. The effects of institutions on economic growth have been studied by Hall and Jones (1999), Alam et al. (2017), and Liu et al. (2018). Since the evidence points to important channels that can support or not support trade, controlling for the governance quality and corruption seem essential when assessing economic growth in SSA countries and helpful to policymakers in the sub-region.

We make three contributions to literature. First, unlike prior work that evaluates the impact of export diversification on economic growth, measuring growth by the growth of GDP per capita, we utilize the growth of GDP per worker, which aligns better with theory (Mankiw et al., 1992; Solow, 1956) and has been successfully utilized in several empirical studies (Well 2007; Waqar 2015). Second, we investigate the possibility of an inverted U-shaped relationship between export diversification and economic growth, since previous work found a non-linear relationship (Di Salvo and Pelkmans-Balaoing, 2015). Testing for non-linear relationships is helpful to determine the optimal export diversification for SSA countries and thus helps policymakers to encourage more or less diversification. Third, our study takes into account institutional governance and corruption to determine how these crucial factors affect the relationship between export diversification and economic growth.

We find that in SSA, there is an inverted-U shaped relationship between export diversification and economic growth, indicating that the average SSA country's export diversification is higher than the growth-optimizing level. Corruption control, and governance quality, as expected, has a positive effect on economic growth. Thus, the findings highlight the importance of corruption and good governance and support policies aimed at reducing corruption and improving institutional governance.

Theoretical Framework

Export Diversification – Economic Growth Nexus

Export diversification can affect the rate of economic growth in several ways, according to trade theory. Herzer and Nowak-Lehnmann (2006) suggest that export diversification improves economic growth by reducing countries' reliance on primary commodities. This is more obvious in developing countries that rely heavily on agricultural and primary commodity exports. Export diversification helps improve poor trading ties between rising economies, according to Prebisch (1962) and Singer (1950). Syrquin (1988) suggests that emerging nations that want to achieve economic progress through export must switch from exporting primary commodities to exporting industrial goods.

Export diversification affects economic growth primarily through two mechanisms. First, the portfolio effect prevents export earnings instability, which occurs when developing nations that export primary commodities experience price volatility. The earnings volatility of exporters, coupled with a rise in the unpredictability of some macroeconomic variables, has a negative impact on economic growth. According to Agosin (2007), economies in nations with more volatility of export earnings (from primary commodities) grow at a slower rate. Syrquin (1988) argues that more export diversification should result in more stable export revenues and boost the purchasing power of the exporting nation, and increased purchasing power encourages investment to improve economic growth (Syrquin, 1988). Compared to nations that export a vast variety of commodities, countries that are largely dependent on fewer export items (specialized) have greater exchange rate fluctuations (Coulibaly and Akia, 2019). These fluctuations can constrain investment in tradable goods and services (Bleaney and Greenaway 2001; Ghosh and Ostry, 1994).

Second, there are dynamic advantages of export diversification. Export diversification offers various dynamic advantages that can contribute to a country's economic growth and stability. Firstly, it reduces the country's vulnerability to external shocks such as changes in demand, price volatility, and market disruptions (Coulibaly and Akia 2019). By diversifying exports, a country spreads its risks across multiple products and markets, minimizing the negative impact on the overall economy when a specific sector or market experiences a downturn. Secondly, export diversification enhances a country′s resilience to global economic cycles. By tapping into different industries and geographic markets, countries can reduce their exposure to the cyclical nature of specific sectors or regions. This helps cushion the impact of economic downturns and maintains a more stable growth trajectory. Moreover, export diversification drives competitiveness and productivity. It encourages industries to upgrade their capabilities, innovate, and improve efficiency to compete in new markets. Expanding into different markets and offering a broader range of goods and services stimulates productivity, fosters innovation, and drives overall economic growth.

Additionally, export diversification contributes to the development of a robust and dynamic private sector. It creates job opportunities, promotes entrepreneurship, fosters a competitive business environment, attracts investment, and supports the growth of small and medium-sized enterprises (SMEs). This, in turn, leads to increased employment and economic development. Furthermore, diversification enables economies of scale, especially in sectors where larger production volumes lead to cost reductions (Coulibaly and Akia 2019). By diversifying exports, countries can leverage their comparative advantages in various sectors and tap into larger markets.

Moreover, entering new markets exposes domestic companies to diverse business practices, customer preferences, and technological advancements, providing valuable learning experiences that can be applied domestically. Importantly, export diversification fosters structural transformation and economic development by shifting the country's reliance away from primary commodities and low-value-added products towards higher value-added activities. This transition increases productivity, creates high-skilled jobs, and supports sustainable economic development in the long term. Overall, export diversification provides dynamic advantages that enhance economic resilience, competitiveness, and long-term growth. By reducing vulnerability, encouraging innovation, and expanding market opportunities, countries can develop diverse and robust economies capable of withstanding external shocks and benefiting from global trade (Coulibaly and Akia 2019).

Institutional Governance - Economic Growth Nexus

The effectiveness of institutional governance, which includes the systems, processes, and institutions that affect the activities of individuals and organizations, has a significant impact on promoting economic growth. Sound institutional governance can improve market efficiency, protect property rights, and foster a favorable investment climate, whereas poor institutional governance can stifle economic growth.

The state of the rule of law and the level of corruption are important elements that can have an impact on economic growth in an array of ways. Businesses and investors may be reluctant to invest in long-term projects and distrust the legal system in societies where corruption is pervasive and the rule of law is weak, resulting in a decline in investment and economic growth. The rule of law is a fundamental economic principle that is needed to ensure effective market operation. A lack of rule of law can make it difficult for businesses to enforce contracts and protect property rights, leading to a decrease in investment and economic growth (Acemoglu et al., 2012).

Furthermore, weak rule of law can lead to increased levels of violence and political instability, both of which have severe economic consequences. Corruption is another significant factor that can influence economic growth. In corrupt societies, businesses may offer bribes to government officials to secure permits or contracts, causing increased costs and reduced competitiveness. Corruption can also result in resource misallocation, with funds channeled to projects that favor corrupt officials rather than the broader economy (Mauro, 1995). Furthermore, corruption can undermine the legitimacy of government institutions, resulting in lower levels of public trust and economic growth. Studies have found that countries with a stronger rule of law tend to have higher levels of economic growth, while corruption can reduce economic growth by up to 1% per year (Mauro, 1995). Therefore, sound institutional governance is critical for economic growth. A robust rule of law can promote public trust in institutions and encourage higher levels of investment, while reducing corruption can improve competitiveness and direct resources more effectively.

Augmented Solow Growth Model

Our empirical model is based on the augmented Solow growth model and closely follows Mankiw et al. (1992). The Solow growth framework offers a simple and theoretically sound approach to investigating the relationship between export diversification and GDP per worker growth (see Hesse, 2008). We concentrate on a few but significant explanatory variables that capture the predictions of the Solow growth model rather than following the extensive empirical literature that uses cross-country regressions and is frequently criticized for its "kitchen-sink" approach of capturing all kinds of potential explanatory factors of growth (see also Hesse, 2008). We extend the model to incorporate corruption and institutional governance factors, which are known to have a significant impact on growth in developing countries, particularly those in SSA. We use Rao and Hassan's (2011) constant return to scale (CRTS) production function with Hicks-neutral technical progress, which is specified as:

$$Y_t = A_t K_t{}^{\eta} L_t{}^{(1-\eta)} \tag{1}$$

where Y_t is the output (*GDP per worker growth*), K is capital, L is labor, A is the present level of technology, and t is the time. η is the elasticity of capital input w.r.t output, $1-\eta$ is the elasticity of labor w.r.t output. The Solow growth model assumes the technological evolution as:

$$A_t = A_0 e^{g^T} \tag{2}$$

where; A_0 is the initial knowledge stock. We further assume that

$$A_t = f(ADCCS_t,\ IG_t,\ CR_t) \tag{3}$$

where; $ADCCS_t$ is export diversification, G_t is governance indicators, and CR_t is corruption. Combining (1) and (3) yields:

$$Y_t = f(ADCCS,\ IG_t,\ CR_t) K_t{}^{\eta} (L_t)^{1-\eta} \tag{4}$$

Empirical Model

In this section, we present the identification strategy and empirical model for estimation.

Identification Strategy

Although the basic assumption, based on theoretical considerations, is that export diversification promotes economic growth, economic growth can also result in more export growth by promoting technology adoption and increased imports utilized as inputs to generate export-oriented commodities. This implies the possibility of "reverse causality" between economic growth and export diversification (see Imbs & Wacziarg, 2003).

Reverse causality issues can also arise from the Solow growth variables, which can bias estimates, particularly between domestic investment and economic growth (Barro and Sala-i-Martin, 1998). Reverse causality may result in conflicting estimations and erroneous inferences, which could then lead to false conclusions and unsuitable interpretations. Many techniques for mitigating simultaneity bias have been developed, including the generalized method of moments (GMM from here forth), the Instrumental Variable (IV), and the Two-Stage Least Squares method (2SLS). The primary drawback of IV and 2SLS is the usage of "external" instrumentation. They frequently fall short of meeting the "validity and relevance" requirements to provide estimates that are unbiased. Moreover, it is difficult to find instruments that are uncorrelated with the stochastic error terms and correlated with the endogenous variables. We use the GMM model to address endogeneity problems.

The GMM model solves the endogeneity problem by "internally transforming the data" (Arellano and Bond, 1991). There are two types of transformation methods: first difference (one-step GMM) and second-order (two-step GMM) (Ullah et al., 2018). Arellano and Bover (1995) suggested the use of a second order transformation (i.e., two-step GMM) to prevent data loss caused by the internal transformation issue with the one-step GMM.[32] Using "forward orthogonal deviations," the two-step GMM subtracts the mean of each future observation from the current value of the variable rather than subtracting the previous observations from that value (Roodman 2009, p.86). This implies that when the two-step GMM model is applied, unnecessary data loss is avoided. When a panel dataset is balanced, the two-step GMM model offers more accurate and

[32] For example, if a variable's current value is missing, the first-difference transformation (which subtracts a variable's past value from its recent value) may result in the loss of more observations (Roodman, 2009).

reliable estimates for the coefficients (Arellano & Bover, 1995). We used the two-step AB estimator in (5)[33] following Arellano and Bond (1991).

$$LNG_{DP_{it}} = \lambda_i LNG_{DP_{i,t-1}} + \boldsymbol{\psi}' LNSOLOW_{it} + \beta_1 ADCCS_{it} + \beta_2 ADCCS_{it}^2 + \boldsymbol{\delta}' IG_{it} + \theta CR_{it} + \mu_i + \nu_i + \varepsilon_{it} \qquad (5)$$

$$i = 1, \ldots N \qquad t = 1, \ldots T_i$$

where: $G_{DP_{it}}$ is GDP per worker growth in country i and t time periods, $G_{DP_{i,t-1}}$ is one period lag operator (previous year GDP per worker), $SOLOW_{it}$ represents augmented Solow growth indicators. $ADCCS_{it}$ represents export diversification (measured as absolute deviation of country commodity shares), $ADCCS_{it}^2$ represent second order export diversification(as used in previous studies including Al-Marhubi, 2000; Cadot et al., 2011; Hodey et al., 2015; Di Salvo and Pelkmans-Balaoing, 2015), IG_{it} represents institutional governance variables, CR_{it} denotes corruption, μ_i is country-specific fixed effects, ν_i is year fixed effects, and ε_{it} represents error term. From (5), we derive the slope of partial relationship between $G_{DP_{it}}$ and $ADCCS$ as

$$\eta_Y = \frac{\partial LNG_{DP_{it}}}{\partial ADCCS} = \beta_1 + 2\beta_2 \overline{ADCCS} = 0 \qquad (6)$$

where $\overline{ADCCS}$ is the mean of export diversification. The optimal $ADCCS$ value is obtained from (6) as $ADCCS^{\boldsymbol{\tau}} = -\frac{\beta_1}{2\beta_2}$. The function is at maximum if $\frac{\partial LNG_{DP_{it}}{}^2}{\partial ADCCS^2} = 2\beta_2 < 0$. The associated elasticity is $\eta_{YADCCS} = \frac{\partial LNG_{DP_{it}}}{\partial ADCCS} \overline{ADCCS}$. The hypothesis that economic growth is hump-shaped with respect to $ADCCS$ implies $\beta_2 < 0$.

[33] Estimation was performed using "xtabond2 in stata." We also checked if the model with both corruption and institutional governance variables are better. We carried out Arellano – Bond test for autocorrelation to ascertain the validity of the AB model.

Robustness Tests

As a robustness test, we applied the country-fixed effect (FE) regression approach as used in Miao (2013). With this estimation, we check what the country-level fixed effects estimations would be if we did not apply the AB estimator to correct the econometric problems associated with (5). The Wooldridge test for autocorrelation is used to check for serial correlation of the idiosyncratic errors (see Miao, 2013).

$$LNG_{DP_{it}} = \lambda_i LNG_{DP_{i,t-1}} + \boldsymbol{\psi}' LNSOLOW_{it} + \beta_1 ADCCS_{it} + \beta_2 ADCCS_{it}^2 + \boldsymbol{\delta}' IG_{it} + \theta CR_{it} + \mu_i + \varepsilon_{it} \tag{7}$$

We also estimated (5) GDP per capita growth as the dependent, as used in all previous studies.

$$LNG_{DPCAP_{it}} = \lambda_i LNG_{DPCAP_{i,t-1}} + \boldsymbol{\psi}' LNSOLOW_{it} + \beta_1 ADCCS_{it} + \beta_2 ADCCS_{it}^2 + \boldsymbol{\delta}' IG_{it} + \theta CR_{it} + \mu_i + \nu_i + \varepsilon_{it} \tag{8}$$

where: $G_{DPCAP_{it}}$ is GDP per capita growth in country i and t time periods, $G_{DPCAP_{i,t-1}}$ is one period lag operator for GDP per capita growth.

Data

We use annual data on economic growth, corruption, and institutional governance from 2000 to 2018 for 39 SSA countries to investigate the relationship between export diversification and economic growth.[34] The macroeconomic data come from the World Bank's World Development Indicator (WDI), the African Development Bank (AfDB), and Penn World Table Version 9.1. Data on corruption and institutional governance are obtained from the Polity IV database, Transparency International (TI), the International Country Risk Guide (ICRG), Worldwide Governance

[34] The countries included are Angola, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Central African Republic, Chad, Congo DR, Congo Republic, Cote d'Ivoire, Djibouti, Ethiopia, Gabon, Ghana, Guinea, Guinea Bissau, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali Mauritania, Mozambique, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, South Africa, Sudan, Swaziland, Tanzania, Togo, Uganda, Zambia, and Zimbabwe.

Indicators (WGI), and the World Bank database. International trade data are from the United Nations Conference on Trade and Development (UNCTAD) and the International Monetary Fund (IMF).

The hypotheses that export diversification, corruption, and institutional governance indicators influence growth is tested by estimating panel data models for GDP per worker growth. The GDP per capita growth is also denoted as GDP growth per capita. We use the absolute deviation of country commodity shares from the world structure ($ADCCS$) as measure of export diversification. The $ADCCS$ ranges from 0 for less diversified export to 1 for more diversified exports. It was computed and widely utilized by United Nations Conference on Trade and Development (UNCTAD) and studies such as Al-Marhubi (2000), Hodey et al. (2015) and others. The index shows whether the export structure of a country or group of countries differs from the export structure of the world. It is computed as

$$ADCCS_j = {\sum_i |h_{ij}| - |h_i|}\Big/_2 \tag{9}$$

where $ADCCS_j$ is the export diversification index of country j, h_{ij} is the share of commodity i in the total exports of country j, and h_i is the share of commodity i in world exports.

The GDP growth per worker is the measure for economic growth as proposed and used by Mankiw et al. (1992). This is a novel application because previous studies that evaluated the effect of export diversification on economic growth only used GDP per capita growth as a proxy for GDP growth per worker (Hesse 2008). GDP per worker growth aligns well with theory (Mankiw et al., 1992; Solow, 1956). Santacreu (2015) notes that GDP per worker does not account for children, full-time students, retirees, those who are unemployed but are looking for work, those who are unemployed but are not looking for work, and those who perform a significant amount of work in the home but are not paid employees. Thus, GDP per worker focuses on productivity rather than overall economic well-being. On the other hand, other factors including fertility and mortality rates, the number of hours worked, and the makeup of the labor force have an impact on GDP per capita. The GDP per worker has been employed as a measure of economic growth in several empirical research, including Well (2007) and Waqar (2015). The augmented Solow growth indicators include growth rate of the labor force denoted as $(n + g + \delta)$, human capital denoted and investment (% GDP). Corruption is captured as (0 = highly corrupt country; 10 = a very clean country). The institutional governance variables include - independence of executive authority (1 = strongest constraints, worst institutional quality; 7 = smallest constraints, best institutional quality), and the rule of law (-2.5 = weak rule of law; 2.5 = strong rule of law).

Table 4.1 contains the descriptive statistics. We find that $\overline{ADCCS}$ is 0.78, on average. The $\overline{ADCCS}$ for each country in our sample is illustrated in Figure 4.1. It ranges from 0.55 to 0.90 with Mali, Guinea, Nigeria, Sudan, Gabon, Zambia, and Zimbabwe having greater $\overline{ADCCS}$ over the years under consideration. We show through Figure 4.2 that $\overline{ADCCS}$ varies across regions.

The GDP growth per worker has a mean of 0.002 with a standard deviation of 0.007. Figure 4.3 displays the variation in GDP growth per worker varies across regions. GDP growth per capita averaged 0.024 with a standard deviation of 0.053. We also show in Figure 4.4, $\overline{ADCCS}$ and mean GDP growth per worker in our sample for all SSA countries sampled; we see variation across countries. Figure 4.5 depicts the correlation between GDP growth per worker and institutional governance as well as corruption variables across countries. Figure 4.6 also displays a scatter plot of GDP per worker against export diversification.

Results and Discussion

The main result from the AB estimator (equation 5) is reported in Table 4.2.[35] We focus on columns (1) and (2), which contain all variables and their interactions with export diversification. The Arellano-Bond test AR (2) in first differences does not reject the null hypothesis of no two-period serial correlation in the residuals in column (1). The lack of significance of the Hansen test ($p = 0.995$) shows that the overidentifying-restrictions are valid, so we conclude that the GMM model is not weakened by too many instruments. Overall, all variables within the specification are statistically significant and have their signs consistent with theory and the findings in Mankiw et al. (1992). We also confirm the positive effect of investment on economic growth (Solow's 1956). Specifically, we find that a 1% increase in investment leads to a 0.01% increase in economic growth, which is consistent with the estimates of Aisen and Veiga (2013) but unlike the estimates for earlier periods shown in Fosu (2001). Also, a 1% increase in growth rate of the labor force $(n + g + \delta)$ results in about 0.01% decrease in economic growth. This finding implies that growth per worker is linearly dependent on technological progress and conforms to the findings of Mankiw et al. (1992).

The column (1) of Table 4.2 shows that the coefficient of export diversification $(\beta_1 = 0.22)$ is significant and positive, supporting results from previous studies that find a positive relationship between $ADCCS$ and economic growth (See for example, Fosu 1990; Herzer & Nowak-Lehnmann, 2006; Aditya & Acharyya, 2013; Rondeau & Roudaut, 2014; Hodey et al., 2015; Teignier 2018; De Sousa et al., 2020; Jongwanich, 2020). The results differ from Akter

[35] We test and reject the null hypothesis that corruption and institutional quality do not affect growth using an F-test. This implies a full model with corruption and institutional variables is preferred to the restricted models.

(2020) who find no relationship between $ADCCS$ and economic growth, and Amin et al. (2000), Hesse (2008) and Karahan (2017) who find a negative relationship.

We tested for an inverted-U shape relationship between export diversification and economic growth and found an inverted-U shape relationship between per worker economic growth and export diversification measures in SSA countries. This is the case because in addition to the significance of the linear term, the square term (export diversification^2) ($\beta_2 = 0.17$) is significant and negative and the two coefficients ate jointly significant. Similar results are found by Cadot et al., (2011), and Di Salvo and Pelkmans-Balaoing (2015). The result is different from that in Hodey et al., (2015) who find no inverted-U shaped relationship between GDP per capita and export diversification in SSA (1995 - 2010).

The existence of inverted-U shape relationship can be used to compute whether more export diversification is required. We find η_Y to be -0.04 (see Table 4.6) indicating that a one-unit increase in export diversification corresponds to a 0.04% decrease in economic growth, all else equal. The associated elasticity, η_{YADCCS} is -0.03 (see Table 4.5) indicating that a 1% increase in export diversification leads to a 0.03% decrease in export growth, all else equal. Finally, the export diversification value that maximizes economic growth, $ADCCS^{\boldsymbol{\tau}}$ is 0.67. Since 0.67 is less than $\overline{ADCCS}$, the export diversification among SSA countries on average is higher than the growth-optimizing level.

The coefficient of the corruption[36] is positive and statistically significant. This indicates that corruption control is beneficial to economic growth among SSA countries because higher values of the index indicate less corruption. This supports Kofele-Kale's (2006) assertion that African governments would see faster economic growth if they implemented contemporary

[36] Note: Higher political corruption index implies lower the corruption. One should be mindful when interpreting the results (see Mauro, 1995; Gyimah & Traynor, 199; Gyimah-Brempong, 2002).

initiatives that are appropriate, pertinent, and sufficient to create societies free from corruption. The findings also imply that if corruption is not managed, it is detrimental to SSA economic growth aligning with the findings of previous research conducted by Mauro (1995), Li et al. (2000), Wei (2000), and Blackburn et al. (2006). Theoretical support for these findings is also provided by Shleifer and Vishny (1993) and Ehrlich and Lui (1999). Additionally, the result confirms the findings of Gyimah-Brempong (2002) in Africa. Other studies, including Mo (2001), argue that corruption favors specific groups while disadvantaging others, leading to unequal opportunities. This inequality, akin to wealth and income inequality, gives rise to discontent, sociopolitical instability, and reduced productivity and this issue maybe being observed in Africa.

The estimated coefficient of rule of law variable, which measures perceptions of the extent to which agents have confidence in and abide by the rules of society, has a positive impact on growth supporting our *a priori* expectation (impact on economic growth positively (Pere, 2015; Omoteso & Mobolaji, 2014). The rule of law level significantly influences economic growth in various ways. In societies where rule of law is weak, businesses and investors may hesitate to engage in long-term projects and lack trust in the legal system. Consequently, this leads to reduced investment and slower economic growth. The rule of law plays a crucial role in facilitating efficient market operations. When the rule of law is lacking, businesses face challenges in enforcing contracts and safeguarding property rights, resulting in decreased investment and hindered economic growth (Acemoglu et al., 2012).

The coefficient of the independent of executive authority is statistically significant and positive. This finding is consistent with the *a priori* expectation and suggests that SSA countries would be better-off in terms of economic growth when the chief executives are given the room to perform independently. A higher level of independence for executive authorities, such as autonomous central banks or regulatory bodies, is often considered to be beneficial for economic growth. The independence of executive authority leads to economic growth in a variety of ways. For example, when the central bank acts autonomously, it can focus on maintaining price stability and controlling inflation. This stability increases investor confidence, long-term planning, and economic growth (see Cukierman et al., 1992; Alesina & Summers, 1993). Without undue political influence, independent regulatory authorities can implement transparent and effective regulations. This fosters a positive business climate, attracts investments, and encourages fair competition, all of which is necessary for growth in the economy (see La Porta et al., 1999; Besley & Burgess, 2004). The independence of the executive indicates a commitment to good governance, accountability, and the rule of law. These fosters trust among domestic and foreign investors, hence stimulating capital inflows, entrepreneurial activity, and economic growth (see Kaufmann et al., 1999; Djankov et al., 2003). Independent executive authorities are better positioned to undertake long-term economic policies that are consistent and long-term economic policies. This predictability and stability foster a favorable climate for firms to strategize and invest, resulting in long-term economic growth (see Hallerberg & von Hagen, 1999).

Table 4.2 column (2) displays results on the interactions of export diversification with both corruption and institutional governance indicators. The Arellano-Bond test AR (2) failed to reject the null hypothesis of no two-period serial correlation in the residuals, and the Hansen test (with $p = 0.998$) also shows that the overidentifying-restrictions are valid. Like column (1), we find the coefficient of export diversification to be significant and positive, and export diversification^2 is significant and negative. We find the partial relationship between export diversification and GDP growth per worker (η_Y) to be -0.04 (see Table 4.6) indicating that a one-unit increase in export diversification leads to about 0.04% decrease in economic growth, all else equal. The

η_{YADCCS} is -0.03 (see Table 4.6) also suggesting that a 1% increase in export diversification leads to a 0.03% decrease in export growth, all else equal. $ADCCS^{\tau}$ has a value of 0.71 which is less than $\overline{ADCCS}$ suggesting that export diversification among SSA countries on average higher than the growth-optimizing level.

The coefficient of (export diversification × corruption) is negative and statistically significant. This shows that for the same level of corruption control, diversifying commodity exports is unfavorable to SSA economic growth; alternatively, given the same level of diversification, countries with higher levels of corruption control will have lower growth. The result is inconsistent with Pomfret and Sourdin (2010) who argue that corruption increases direct trade cost serving as a disincentive to engage in international trade. It also somewhat contradicts Shirazi (2012) who find that low corruption levels increase trade and reveal that corruption hampers trade in an environment of low tariffs and vice versa in the face of high nominal tariffs. Our findings could be attributed to the fact that export tariffs in SSA countries are high, as illustrated by Arieff et al. (2009). According to Arieff et al. (2009), Africa is second only to South Asia in terms of trade restrictions imposed by high tariffs. According to DeRosa (1992), SSA countries maintain escalating tariff rates on labor-intensive processed goods and manufactured goods, and import protection reduces combined annual exports by \$1.3 billion to \$2.7 billion per year. These support the negative coefficient of the interaction between export diversification and corruption ($export\ diversification\ \times\ corruption$) and economic growth in the current study. The coefficient of ($export\ Diversification\ \times\ rule\ of\ law$) is positive and statistically significant suggesting that export diversification impact positively on economic growth in the presence of rule of law.

Robustness Tests Results

In this section, we provide the results of the robustness checks carried out in our study. Firstly, we present the findings obtained from the country fixed effect (FE) estimation, which we utilized as a method to verify the reliability of the estimates produced by the AB estimator employed in Miao's (2013) study. By comparing these two estimators (referred to as model 5 for AB estimator and model 7 for country fixed effect estimator), we can make an informed decision on which one to rely on. Furthermore, we discuss the results related to the estimation of our model when the dependent variable is growth of GDP per capita, rather than GDP per worker.

Test One: Country Fixed Effect Estimations

Table 4.3 presents the outcomes of country-level fixed effects estimations for model (5). We present these results to examine the country-level fixed effects estimations in the absence of applying the AB estimator to address the econometric issues associated with model (5). Our primary focus is on columns (1) and (2), which include all variables and their interactions. The Wooldridge test for autocorrelation reveals the presence of serial correlation in the idiosyncratic errors of our country-level fixed regression. This finding aligns with Miao's (2013) study, where the same model was employed as a robustness check to the AB estimator, and it suggests that the AB estimator performs comparatively better in addressing this issue.

In column (1) of Table 4.3, both export diversification and export diversification[2] are statistically significant with the expected signs, but with greater magnitude compared to our findings in Table 4.2. The estimate for η_Y is -0.10 (see Table 4.6) indicating that a one-unit increase in export diversification leads to about 0.10% decrease in economic growth, all else equal. η_{YADCCS} is -0.08 (see Table 4.5) also indicating that a 1% increase in export diversification leads to a 0.08% decrease in export growth, all else equal. Finally, $ADCCS^{\tau}$ is 0.61. The 0.70 value is less than $\overline{ADCCS}$, which suggests that export diversification among SSA countries is too high and higher than the growth-optimizing level. Also, in column (2), we find η_Y to be -0.05 (see Table 4.5) indicating that a one-unit increase in export diversification leads to about 0.05% decrease in economic growth, all else equal. η_{YADCCS} is -0.04 (see Table 4.6) suggesting that a 1% increase in export diversification leads to a 0.04% decrease in export growth, all else equal. Finally, $ADCCS^{\boldsymbol{\tau}}$ is 0.61. As shown, 0.74 is less than $\overline{ADCCS}$, this suggests that export diversification among SSA countries on average is too high and higher than the growth-optimizing level. Based on our analysis, we reach the conclusion that the country-level fixed effects (FE) method tends to overestimate the influence of export diversification on economic growth. This overestimation can be attributed to the presence of serial correlation in the idiosyncratic errors.

Test Two: GDP per Capita Growth Estimates

Table 4.4 displays the results obtained from the model using the AB estimator, with GDP per capita growth as the dependent variable. Like our primary findings (see Table 4.2), the coefficient associated with export diversification is statistically significant and positive, albeit with a larger effect size. This finding aligns with the work of Al-Marhubi (2000), who observed a higher coefficient for export diversification exceeding two. In this specification, we also observe that the relationship between per capita economic growth and export diversification follows an inverted-U pattern.

For example, in column (1), η_Y is -0.29 (see Table 4.6), indicating that a one-unit increase in export diversification leads to about 0.29% decrease in economic growth, all else equal. This result is in line with our main estimation (see Table 4.2) but of greater magnitude. η_{YADCCS} is -0.23 (see Table 4.6) also indicating that a 1% increase in export diversification leads to a 0.23% decrease in export growth, all else equal. It is also greater than what we obtained in Table (4.2).

Finally, $ADCCS^{\tau}$ is 0.69 which is less than $\overline{ADCCS}$, suggesting that export diversification among SSA countries higher than the growth-optimizing level. Similar result was obtained in column (2). We find η_Y to be -0.18, η_{YADCCS} to be -0.14, and $ADCCS^{\tau}$ is 0.73 (see Table 4.6).

Sensitivity Analysis

To enhance the credibility of our analysis, we conducted an additional investigation that specifically targeted a sample period (2008 - 2018) coinciding with the period after the 2008 financial crisis. Our objective was to evaluate the impact of export diversification on economic growth using the AB estimator. If our findings closely align with those presented in Table 4.2, it will reinforce our confidence in the accuracy and reliability of our estimation. Results are presented in Table 4.5.

For example, in column (1), η_Y is -0.01 (see Table 4.6), indicating that a one-unit increase in export diversification leads to about 0.01% decrease in economic growth, all else equal. This result is in line with our main estimation (see Table 4.2). η_{YADCCS} is -0.01 (see Table 4.6) also indicating that a 1% increase in export diversification leads to a 0.01% decrease in export growth, all else equal like Table (4.2). Finally, $ADCCS^{\tau}$ is 0.76 which is less than $\overline{ADCCS}$, suggesting that export diversification among SSA countries higher than the growth-optimizing level. A similar result was obtained in column (2). We find η_Y to be -0.01, η_{YADCCS} to be -0.01, and $ADCCS^{\tau}$ is 0.76 (see Table 4.6).

Estimated Elasticities for Individual Countries

We used the parameter estimates, export diversification and export diversification^2 in Table 4.2 (columns 1 and 2) to compute elasticities for each country in the sample to check for heterogeneity in our findings.

Results are presented in Tables 4.7 (Figure 7) and 4.8 (Figure 8), respectively. For instance, in Table 4.7 (evaluated by parameter estimates in column 1, Table 4.2), the overall $ADCCS^{\tau}$ is 0.66. Countries with $\overline{ADCCS}$ below 0.66 show some level of export and economic growth, and those above experienced a decrease in both export and economic growth. For example, Angola has an $\overline{ADCCS}$ value of 0.62 and η_Y of 0.02 indicating that a one-unit increase in export diversification leads to about 0.02% increase in economic growth, all else equal, and η_{YADCCS} of 0.01 indicating that a 1% increase in export diversification leads to a 0.01% increase in export growth, all else equal. Finally, the inflection point of the overall $ADCCS^{\tau}$ is 0.66 and it is bigger than 0.62, we conclude that export diversification in Angola is lower than optimal export diversification value (0.66). The same growth and export enhancing results was obtained for Djibouti, and South Africa. Their η_Y (η_{YADCCS}) values are found to be positive. It is interesting to note that since $\overline{ADCCS}$ for Djibouti is near the $ADCCS^{\tau}$ (0.66), the η_Y (η_{YADCCS}) is positive but smaller compared to the other countries with lesser $\overline{ADCCS}$ compared to the $ADCCS^{\tau}$.

As already indicated, countries with higher $\overline{ADCCS}$ experienced negative economic and export growth impacts. For example, Botswana with $\overline{ADCCS}$ of 0.90, has η_Y of -0.08, indicating that a one-unit increase in export diversification leads to a 0.08% decrease in economic growth, all else equal, and the η_{YADCCS} of -0.07 indicates that a 1% increase in export diversification leads to a 0.07% decrease in export growth, all else equal. Its $ADCCS^{\tau}$ is 0.66 which less than $\overline{ADCCS}$, 0.90 suggesting that export diversification in Botswana is too high. Similar results are obtained for all the other countries with $\overline{ADCCS}$ greater than $ADCCS^{\tau}$. We obtained similar results in Table 4.2 (column 2) (see Table 4.8).

Conclusions

Following the African economic renaissance in the 2000s, governments in Sub-Saharan African (SSA) countries have pursued diverse strategies to stimulate growth, including a shift towards export diversification instead of relying heavily on a few primary commodities. This study examines the relationship between export diversification and economic growth in SSA using panel data from 39 countries spanning 2000 to 2018. It is the first study to assess the nexus between export diversification, economic growth, corruption, and governance quality. Employing the Arellano-Bond difference generalized method-of-moment estimator, the analysis reveals that increasing export diversification positively impacts economic growth. However, this effect diminishes as diversification expands, indicating a non-linear (inverted-U shape) relationship between export diversification and economic growth in SSA. Consequently, export diversification among SSA countries exceeds the level that maximizes growth, suggesting the need for caution.

This study uncovers that export diversification, especially in the presence of corruption, is detrimental to economic growth in SSA. Additionally, a positive association is observed between governance quality, specifically rule of law, independence of the executive authority, and economic growth. These findings remain robust when GDP per capita growth is used as an alternative measure of economic growth. However, using GDP per capita growth may overestimate the impact of export diversification on economic growth.

The results hold significant implications for the African Continental Free Trade Area (AfCFTA), which aims to enhance export diversification for bolstering economic growth on the continent. The results caution against further export diversification in the region, as it may lead to reduced economic growth. The study suggests the perhaps replacement of primary commodity exports with industrial commodities could mitigate instability in export earnings resulting from heavy reliance on primary commodities.

Furthermore, administrative obstacles arising from trade restrictions and corruption-related policies, such as domestic entry barriers, border delays, and high business registration costs, pose potential hindrances to export-driven growth in SSA. This study offers insights into the relationship between export diversification, corruption, governance quality, and economic growth in SSA. The findings inform policymakers and stakeholders involved in fostering sustainable economic development in the region, highlighting the need for balanced approaches to export diversification while addressing corruption and streamlining administrative processes.

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Table and Figures

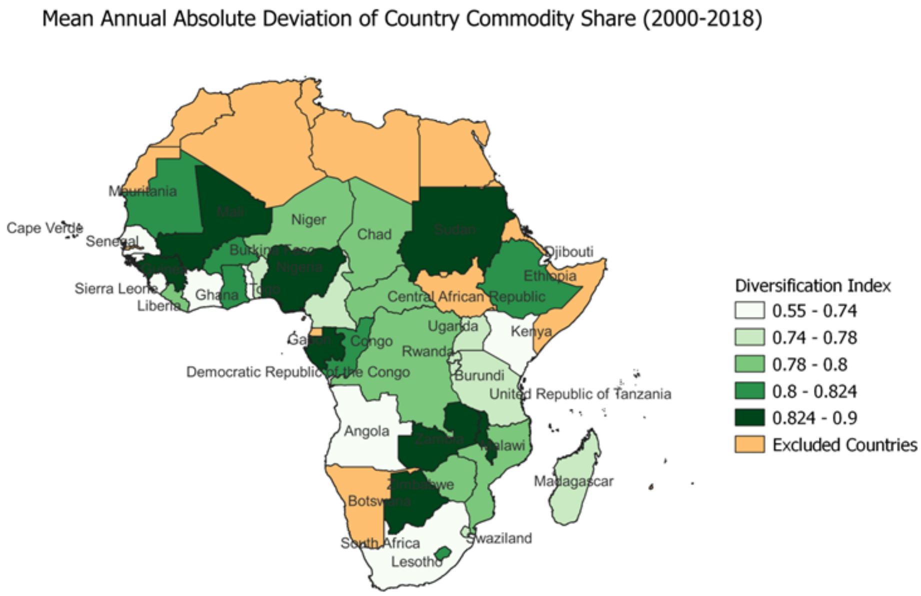


Figure 4.1: Export Diversification Index (ADCCS) by Country

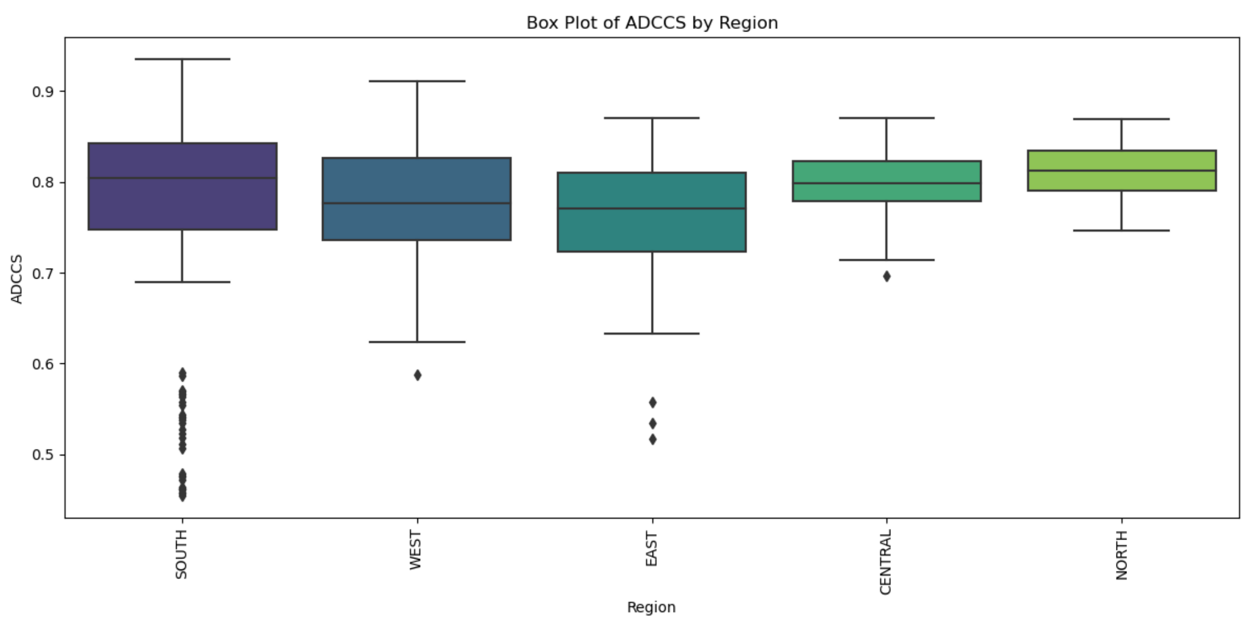


Figure 4.2 Mean Export Diversification Across Regions

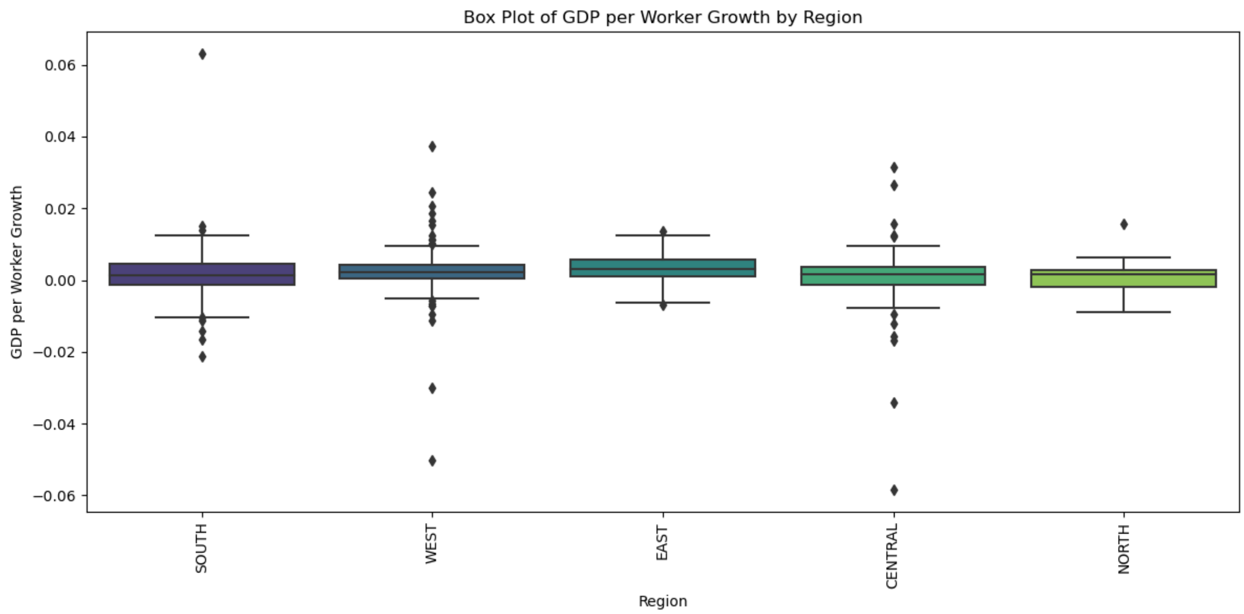


Figure 4.3: Mean GDP per Worker Growth Across Regions

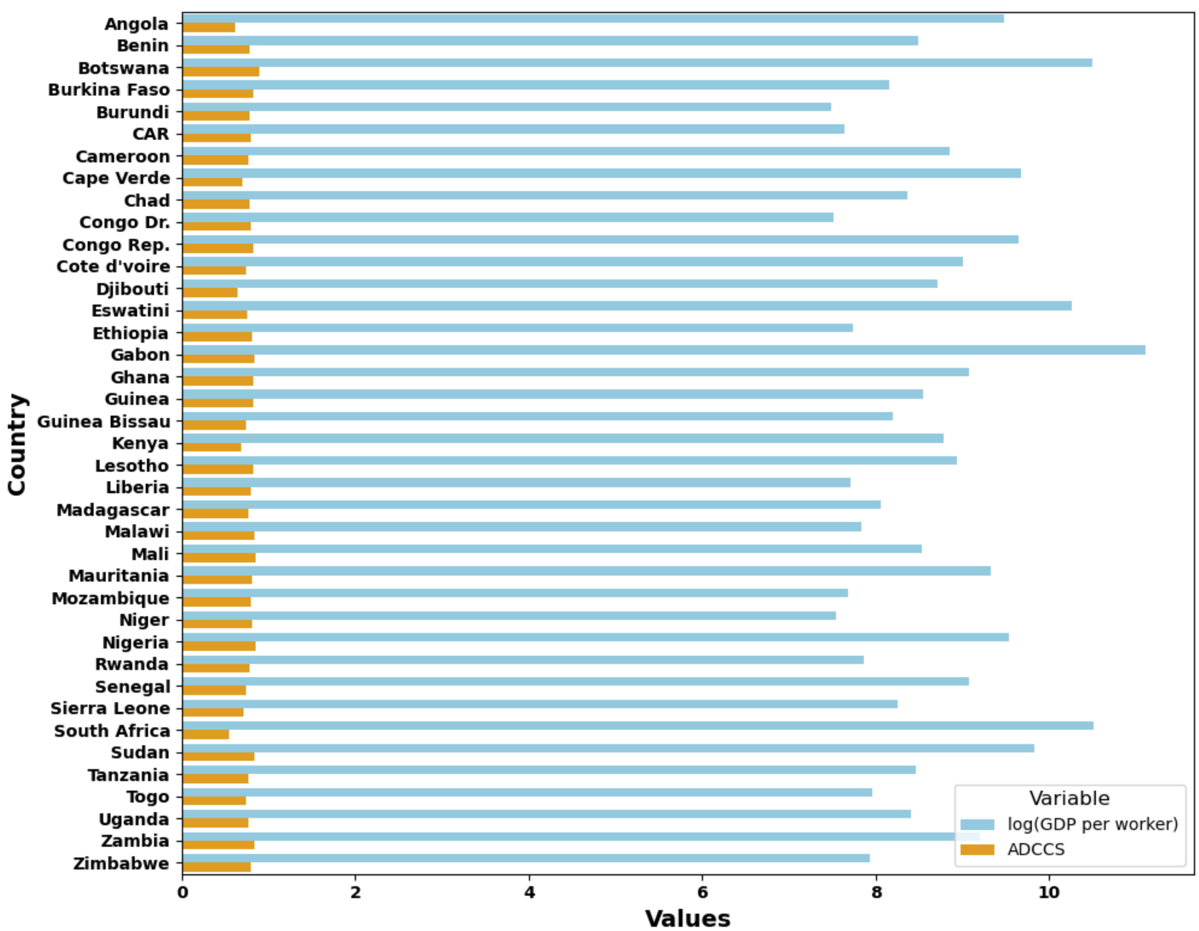
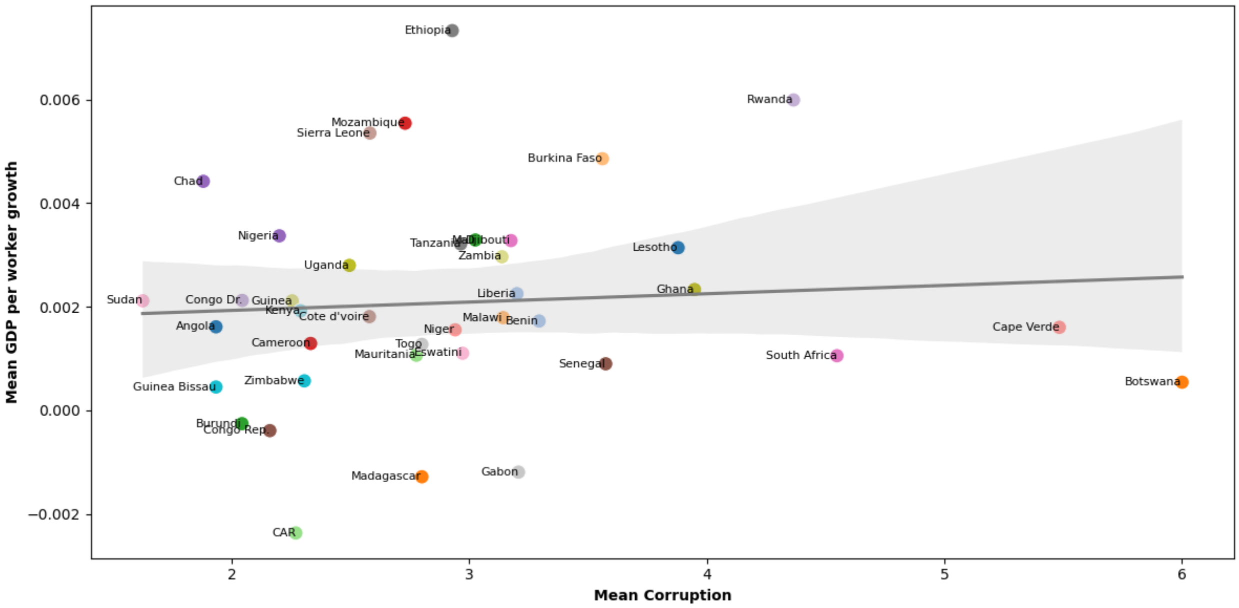
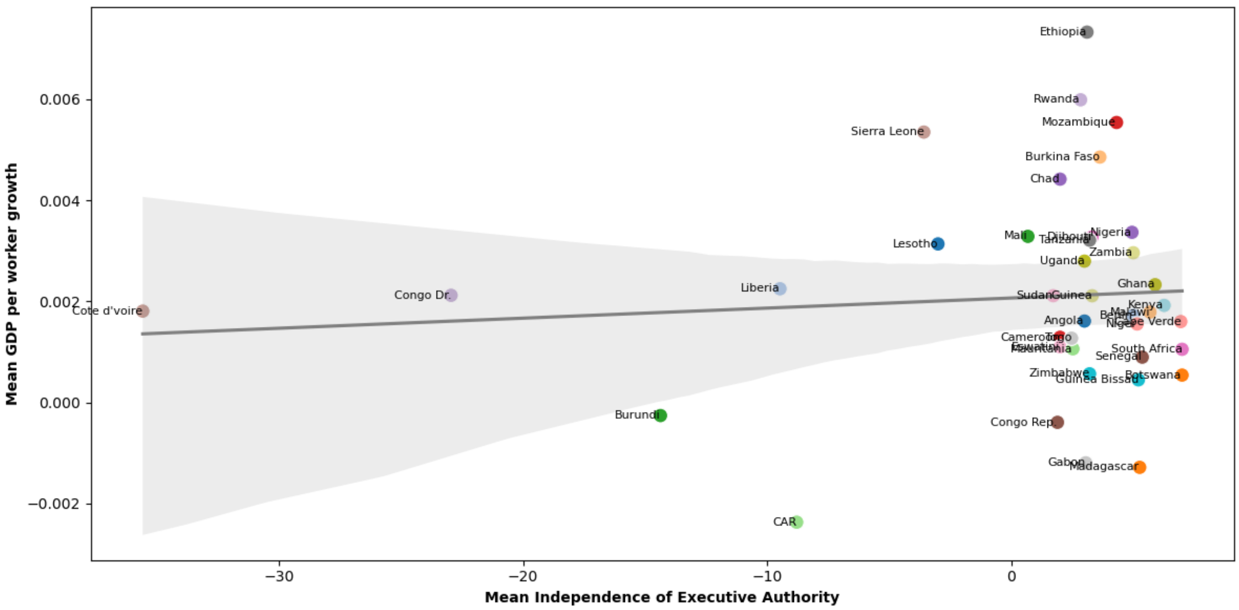


Figure 4.4: Trend of GDP per worker and Export Diversification by country


Mean GDP per worker growth
Mean Corruption
Ethiopia
Rwanda
Mozambique
Sierra Leone
Burkina Faso
Chad
Nigeria
Tanzania
Zambia
Lesotho
Uganda
Liberia
Ghana
Sudan
Congo Dr.
Guinea
Kenya
Cote d'voire
Malawi
Benin
Cape Verde
Angola
Niger
Cameroon
Togo
Mauritania
Eswatini
South Africa
Senegal
Zimbabwe
Guinea Bissau
Botswana
Congo Rep.
Madagascar
Gabon
CAR


Mean GDP per worker growth
Mean Independence of Executive Authority
Ethiopia
Rwanda
Mozambique
Sierra Leone
Burkina Faso
Chad
Nigeria
Lesotho
Mali
Zambia
Uganda
Liberia
Ghana
Congo Dr.
Cote d'voire
Sudan
Guinea
Kenya
Malawi
Angola
Niger
Cape Verde
South Africa
Senegal
Zimbabwe
Guinea Bissau
Botswana
Burundi
Congo Rep.
Madagascar
CAR

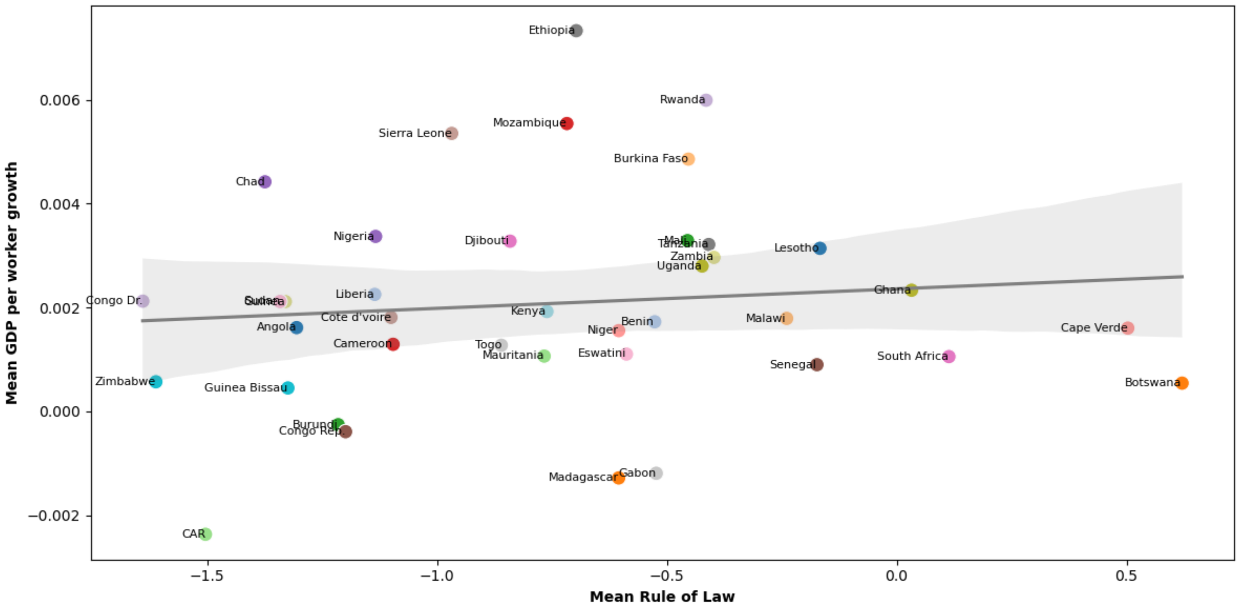


Figure 4.5: GDP per worker growth and Institutional Governance and Political Corruption

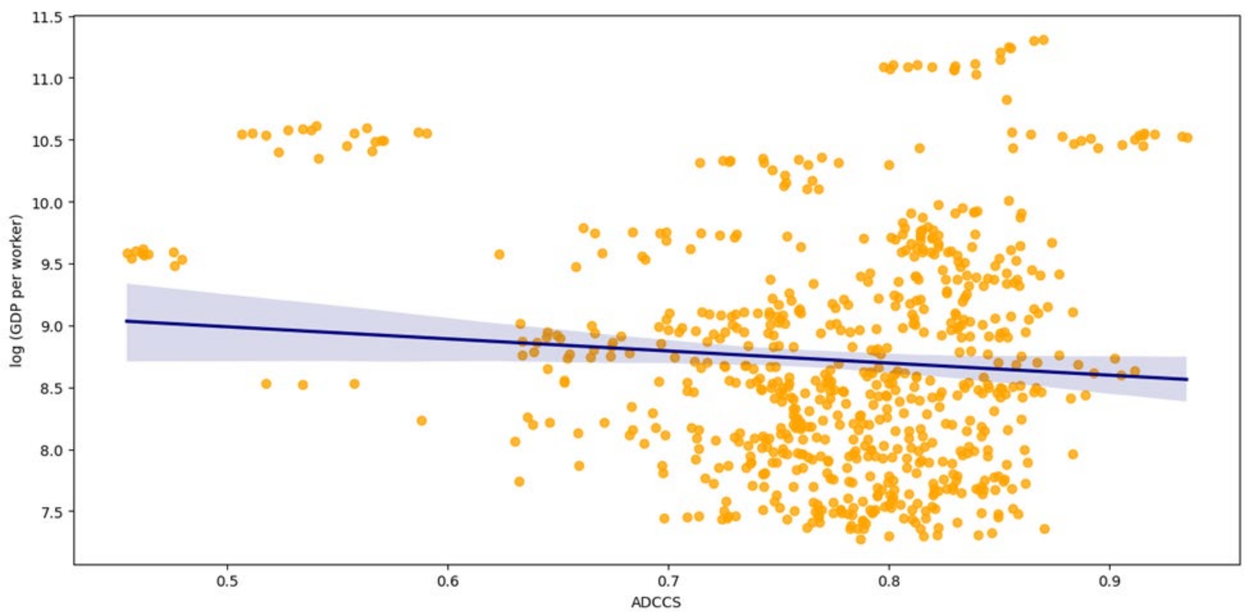


Figure 4.6: Scatter Plot of GDP per worker against Export Diversification

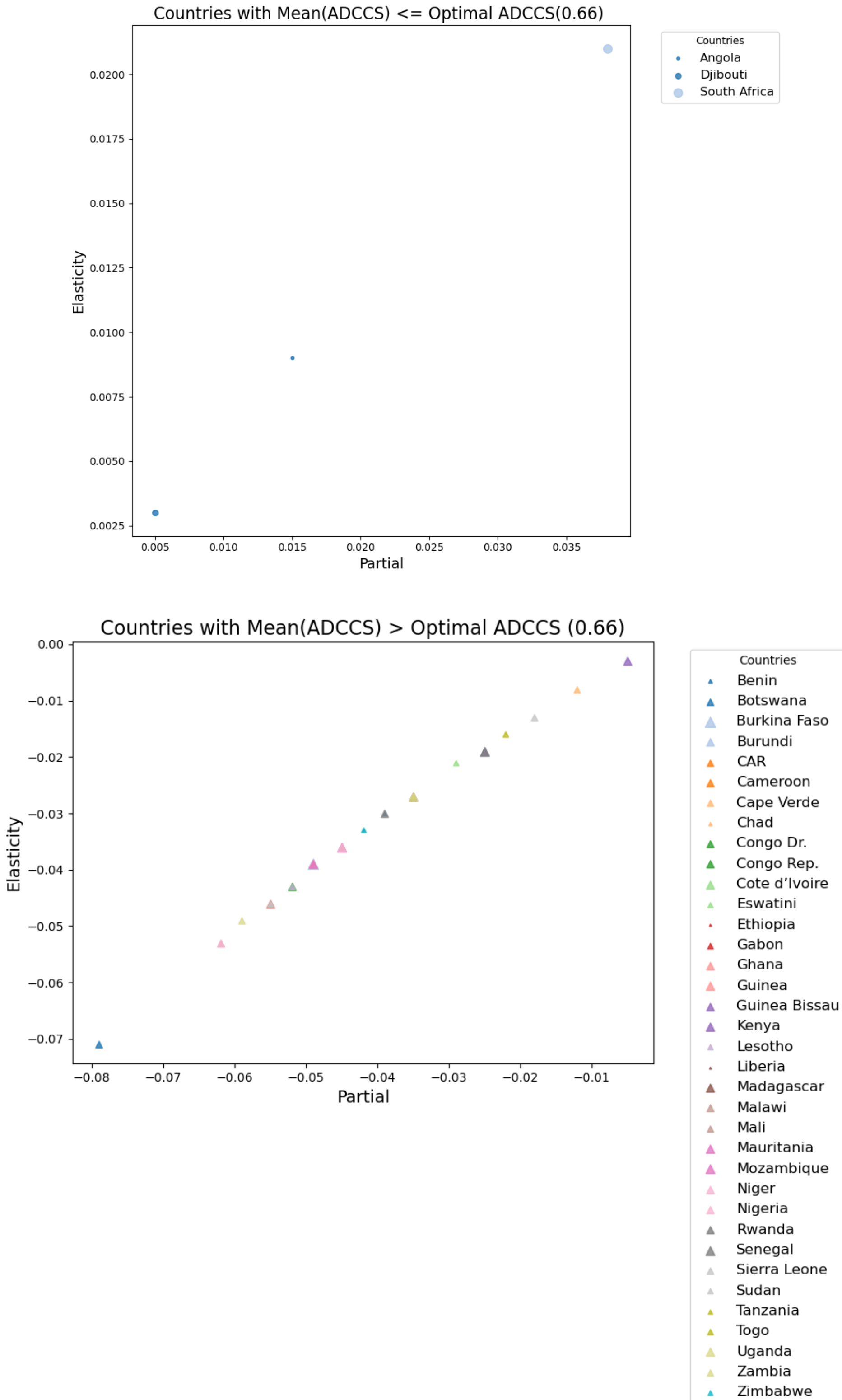
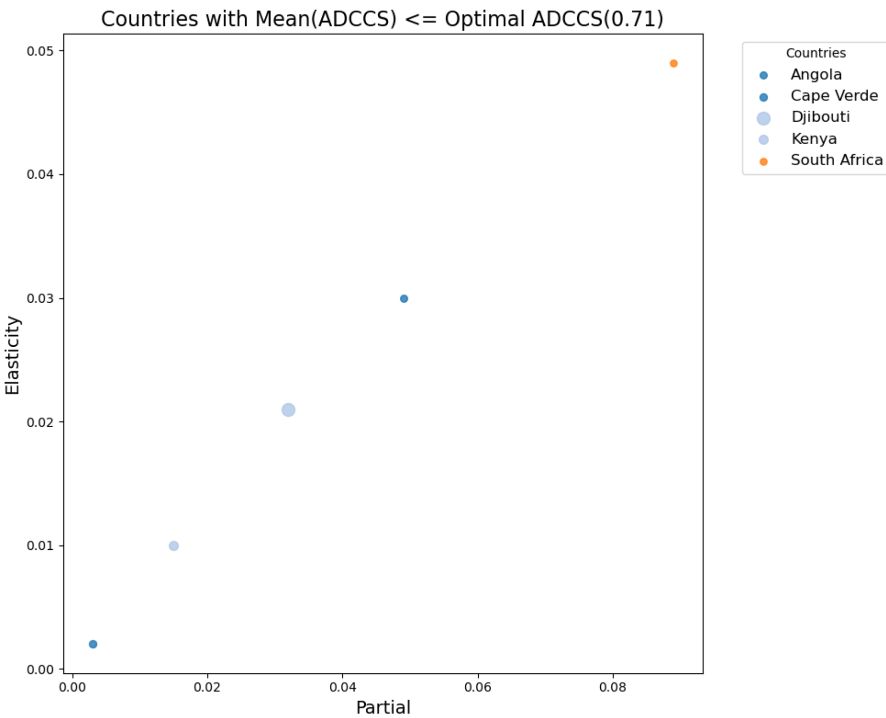


Figure 4.7: Relationship between Elasticities and slope of partial relationship



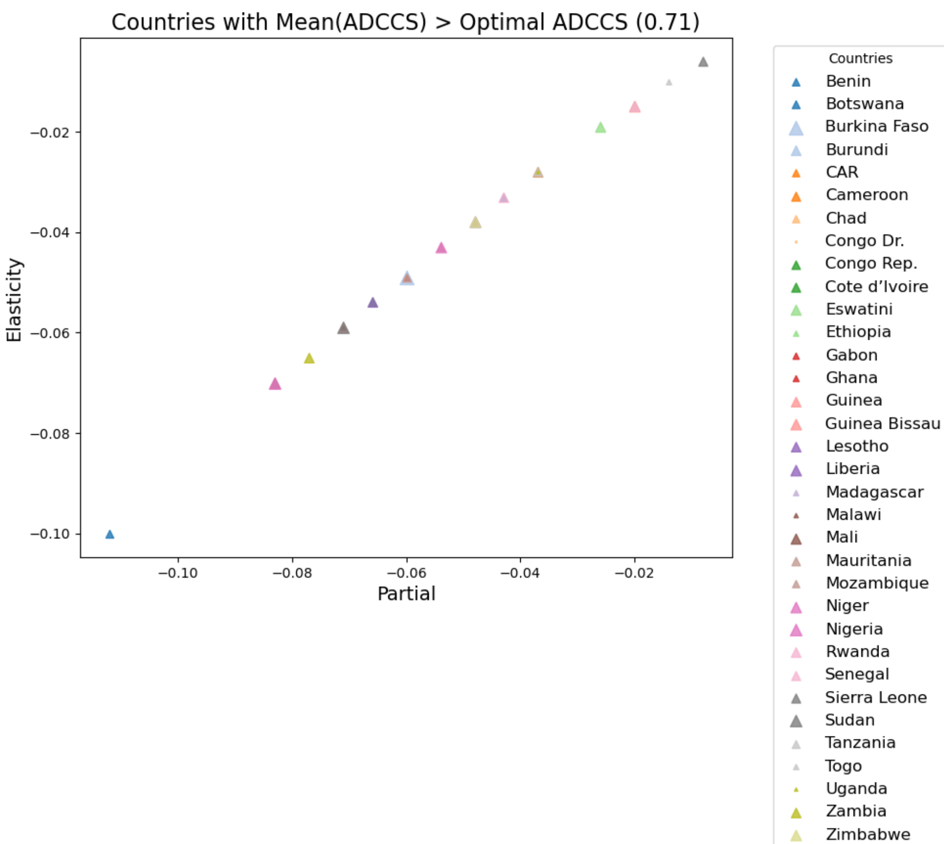


Figure 4.8: Relationship between Elasticities and slope of partial relationship

Table 4.1: Descriptive Statistics

Variables	Variable Description	Obs.	Mean	St. Dev.	Min.	Max.
GDP Growth Per Worker	Dependent variable	663	0.002	0.007	-0.058	0.063
GDP Growth Per Capita	Dependent variable	663	0.024	0.053	-0.368	0.614
$(n + g + \delta)$	The sum of the population growth rate(n), depreciation rate of capital (δ) as well as exogenous rate of technological progress(g).	663	0.079	0.032	-0.238	0.478
Investment (% GDP)	The local investment of an economy.	694	23.306	10.395	4.039	64.852
Human Capital	Index of human capital per person, based on years of schooling and returns to education (an assumed rate of return for primary, secondary, and tertiary education)	612	1.726	0.413	1.069	2.885
Export Diversification	Measured as Absolute Deviation of Country Commodity Shares (0 = less diversified; 1 = more diversified exports).	741	0.775	0.08	0.454	0.935
Corruption	The abuse of 'entrusted power for private gain'. (0 = highly corrupt country), and (10 = a very clean country).	626	2.971	1.024	1	6.5
Independent of the Executive Authority	Proxied as Executive Constraints (Decision Rules). Captures extent to which the chief executive, the head ruler considers the preferences of others when making decisions. (1 = strongest constraints, worst institutional quality) and (7 = smallest constraints, best institutional quality).	710	4.256	1.718	1	7
Rule of Law	The perceptions of the extent to which agents have confidence in and abide by the rules of society (-2.5 = weak rule of law; 2.5 = strong rule of law).	700	-.735	.569	-2.009	.782

Sources: AfDB: African Development Bank database, WDI-WB: World Development Indicator-World Bank, Polity IV: Polity IV database, PWT: Penn World Table Version 9.1 TI: Transparency International database, UNCTAD: United Nations Conference on Trade and Development, IMF: International Monetary Fund database, ICRG: International Country Risk Guide, WGI: Worldwide Governance Indicators.

Table 4.2: AB Estimation of Model (5) of log GDP per worker growth

Variables	(1)	(2)
LN$(n + g + \delta)$	-0.005***	-0.008***
	(0.001)	(0.001)
LN(initial GDP Per Worker)	-0.047***	-0.030**
	(0.004)	(0.014)
LN(Investment(% GDP))	0.007***	0.006
	(0.002)	(0.005)
LN(Human Capital)	0.027***	-0.029*
	(0.009)	(0.016)
Export Diversification	0.222***	0.405**
	(0.0621)	(0.189)
Export Diversification ^{2}	-0.167***	-0.287**
	(0.041)	(0.129)
Corruption	0.002***	0.088***
	(0.001)	(0.020)
Rule of Law	0.005***	0.085***
	(0.001)	(0.029)
Independent of Executive Authority	0.0001*	-0.001
	(0.000)	(0.002)
Export Diversification × Corruption		-0.111***
		(0.026)
Export Diversification × Rule of Law		0.141***
		(0.038)
Export Diversification × Executive Authority		0.002
		(0.002)
Observations	462	462
AR2 Test (p-value)	0.181	0.604
Hansen Test (p-value)	0.995	0.998
Year FE	Yes	Yes
Country FE	Yes	Yes

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
All models contain constant.

Robustness Checks

Table 4.3: FE Estimation Model (7) of log GDP per worker growth

Variables	**(1)**	**(2)**
LN$(n + g + \delta)$	0.028***	0.0282***
	(0.009)	(0.009)
LN(initial GDP Per Worker)	-0.218***	-0.226***
	(0.023)	(0.024)
LN(Investment(% GDP))	0.029***	0.026***
	(0.009)	(0.009)
LN(Human Capital)	0.075	0.031
	(0.051)	(0.054)
Export Diversification	0.975**	0.970*
	(0.410)	(0.540)
Export Diversification ^{2}	-0.693**	-0.657*
	(0.287)	(0.364)
Corruption	0.018***	0.056
	(0.005)	(0.057)
Rule of Law	0.001	-0.098
	(0.008)	(0.112)
Independent of Executive Authority	0.006***	0.002
	(0.000)	(0.002)
Export Diversification × Corruption		-0.048
		(0.072)
Export Diversification × Rule of Law		0.075
		(0.080)
Export Diversification × Executive Authority		-0.002
		(0.003)
Observations	462	462
R-squared	0.240	0.256
First -order autocorrelation	0.000	0.000
Country FE	Yes	Yes

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All models contain constant. The autocorrelation test is a Wooldridge test for autocorrelation in panel data.

Table 4.4: AB Estimation of Model (8) of log GDP per capita growth

Variables	(1)	(2)
LN$(n + g + \delta)$	0.0326***	0.069***
	(0.004)	(0.012)
LN(initial GDP Per Capita)	-0.499***	-0.168**
	(0.066)	(0.061)
LN(Investment(% GDP))	0.058***	0.110**
	(0.015)	(0.047)
LN(Human Capital)	0.420***	-0.523***
	(0.124)	(0.086)
Export Diversification	2.448**	2.738**
	(1.200)	(1.267)
Export Diversification ^{2}	-1.767**	-1.884*
	(0.823)	(0.771)
Corruption	0.023***	0.844***
	(0.006)	(0.151)
Rule of Law	0.022**	1.738***
	(0.008)	(0.407)
Independent of Executive Authority	0.000	0.015
	(0.001)	(0.018)
Export Diversification × Corruption		-1.057***
		(0.196)
Export Diversification × Rule of Law		1.523***
		(0.303)
Export Diversification × Executive Authority		-0.015
		(0.024)
Observations	462	462
AR2 Test (p-value)	0.179	0.533
Hansen Test (p-value)	0.999	0.790
Year FE	Yes	Yes
Country FE	Yes	Yes

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

Table 4.5: AB Estimation (Sensitivity Analysis)

Variables	(1)	(2)
LN$(n + g + \delta)$	-0.003***	-0.001
	(0.000)	(0.001)
LN(initial GDP Per Worker)	-0.078***	-0.112***
	(0.007)	(0.006)
LN(Investment(% GDP))	-0.005**	-0.008**
	(0.002)	(0.004)
LN(Human Capital)	0.022	0.049**
	(0.016)	(0.023)
Export Diversification	0.385***	0.617**
	(0.122)	(0.327)
Export Diversification ^{2}	-0.255***	-0.406*
	(0.080)	(0.206)
Corruption	0.007***	0.021*
	(0.001)	(0.016)
Rule of Law	0.017***	0.090***
	(0.004)	(0.029)
Independent of Executive Authority	0.000*	-0.001
	(0.000)	(0.001)
Export Diversification × Corruption		0.048**
		(0.0228)
Export Diversification × Rule of Law		0.112**
		(0.042)
Export Diversification × Executive Authority		0.001
		(0.001)
Observations	287	287
Hansen Test (p-value)	0.950	0.336
Hansen Test (p-value)	0.129	0.684
Year FE	Yes	Yes
Country FE	Yes	Yes

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
Data from 2008 – 2018 was used. All models contain constant.

Table 4.6: Elasticities Evaluated at mean of export diversification ($\widehat{ADCCS}$)

Table	Column	β_1	β_2	$ADCCS^{\tau} = -\beta_1/2\beta_2$	$\eta_Y = \partial LNY_{it}/\partial ADCCS$	$\eta_{YADCCS} = (\partial LNY_{it}/\partial ADCCS) \times \overline{ADCCS}$
2	1	0.222	-0.167	0.665	-0.037	-0.029
	2	0.405	-0.287	0.706	-0.040	-0.031
3	1	0.975	-0.693	0.703	-0.099	-0.077
	2	0.970	-0.657	0.738	-0.048	-0.037
4	1	2.448	-1.767	0.693	-0.291	-0.225
	2	2.738	-1.884	0.727	-0.182	-0.141
5	1	0.385	-0.255	0.755	-0.010	-0.008
	2	0.617	-0.406	0.760	-0.012	-0.010

Note: Sample $\overline{ADCCS}$ is 0.78

Table 4.7: Individual Countries Elasticities Evaluated mean of export diversification ($\widehat{ADCCS}$) (Table 4.2 Column 1)

Countries	β_1	β_2	$\overline{ADCCS}$	$\eta_Y = \partial LNY_{it}/\partial ADCCS$	$\eta_{YADCCS} = (\partial LNY_{it}/\partial ADCCS) \times \overline{ADCCS}$
Angola	0.222	-0.167	0.620	0.015	0.009
Benin	0.222	-0.167	0.78	-0.039	-0.030
Botswana	0.222	-0.167	0.9	-0.079	-0.071
Burkina Faso	0.222	-0.167	0.81	-0.049	-0.039
Burundi	0.222	-0.167	0.77	-0.035	-0.027
CAR	0.222	-0.167	0.8	-0.045	-0.036
Cameroon	0.222	-0.167	0.77	-0.035	-0.027
Cape Verde	0.222	-0.167	0.7	-0.012	-0.008
Chad	0.222	-0.167	0.79	-0.042	-0.033
Congo Dr.	0.222	-0.167	0.8	-0.045	-0.036
Congo Rep.	0.222	-0.167	0.82	-0.052	-0.043
Cote d'Ivoire	0.222	-0.167	0.74	-0.025	-0.019
Djibouti	0.222	-0.167	0.65	0.005	0.003
Eswatini	0.222	-0.167	0.75	-0.029	-0.021
Ethiopia	0.222	-0.167	0.81	-0.049	-0.039
Gabon	0.222	-0.167	0.83	-0.055	-0.046
Ghana	0.222	-0.167	0.81	-0.049	-0.039
Guinea	0.222	-0.167	0.83	-0.055	-0.046
Guinea Bissau	0.222	-0.167	0.74	-0.025	-0.019
Kenya	0.222	-0.167	0.68	-0.005	-0.003
Lesotho	0.222	-0.167	0.82	-0.052	-0.043
Liberia	0.222	-0.167	0.79	-0.042	-0.033
Madagascar	0.222	-0.167	0.77	-0.035	-0.027
Malawi	0.222	-0.167	0.83	-0.055	-0.046
Mali	0.222	-0.167	0.85	-0.062	-0.053

Mauritania	0.222	-0.167	0.81	-0.049	-0.039
Mozambique	0.222	-0.167	0.8	-0.045	-0.036
Niger	0.222	-0.167	0.8	-0.045	-0.036
Nigeria	0.222	-0.167	0.85	-0.062	-0.053
Rwanda	0.222	-0.167	0.78	-0.039	-0.030
Senegal	0.222	-0.167	0.74	-0.025	-0.019
Sierra Leone	0.222	-0.167	0.72	-0.018	-0.013
South Africa	0.222	-0.167	0.55	0.038	0.021
Sudan	0.222	-0.167	0.83	-0.055	-0.046
Tanzania	0.222	-0.167	0.77	-0.035	-0.027
Togo	0.222	-0.167	0.73	-0.022	-0.016
Uganda	0.222	-0.167	0.77	-0.035	-0.027
Zambia	0.222	-0.167	0.84	-0.059	-0.049
Zimbabwe	0.222	-0.167	0.79	-0.042	-0.033

*Note: $ADCCS^{\tau} = -\frac{\beta_1}{2\beta_2}$ is 0.66 for all the countries.

Table 4.8: Individual Countries Elasticities Evaluated mean of export diversification ($\widehat{ADCCS}$) (Table 4.2 Column 2)

Countries	$\boldsymbol{\beta_1}$	$\boldsymbol{\beta_2}$	$\overline{ADCCS}$	$\eta_Y = \partial LNY_{it}/\partial ADCCS$	$\eta_{YADCCS} = (\partial LNY_{it}/\partial ADCCS) \times \overline{ADCCS}$
Angola	0.405	-0.287	0.620	0.049	0.030
Benin	0.405	-0.287	0.78	-0.043	-0.033
Botswana	0.405	-0.287	0.9	-0.112	-0.100
Burkina Faso	0.405	-0.287	0.81	-0.060	-0.049
Burundi	0.405	-0.287	0.77	-0.037	-0.028
CAR	0.405	-0.287	0.8	-0.054	-0.043
Cameroon	0.405	-0.287	0.77	-0.037	-0.028
Cape Verde	0.405	-0.287	0.7	0.003	0.002
Chad	0.405	-0.287	0.79	-0.048	-0.038
Congo Dr.	0.405	-0.287	0.8	-0.054	-0.043
Congo Rep.	0.405	-0.287	0.82	-0.066	-0.054
Cote d'voire	0.405	-0.287	0.74	-0.020	-0.015
Djibouti	0.405	-0.287	0.65	0.032	0.021
Eswatini	0.405	-0.287	0.75	-0.026	-0.019
Ethiopia	0.405	-0.287	0.81	-0.060	-0.049
Gabon	0.405	-0.287	0.83	-0.071	-0.059
Ghana	0.405	-0.287	0.81	-0.060	-0.049
Guinea	0.405	-0.287	0.83	-0.071	-0.059
Guinea Bissau	0.405	-0.287	0.74	-0.020	-0.015
Kenya	0.405	-0.287	0.68	0.015	0.010
Lesotho	0.405	-0.287	0.82	-0.066	-0.054
Liberia	0.405	-0.287	0.79	-0.048	-0.038
Madagascar	0.405	-0.287	0.77	-0.037	-0.028

Malawi	0.405	-0.287	0.83	-0.071	-0.059
Mali	0.405	-0.287	0.85	-0.083	-0.070
Mauritania	0.405	-0.287	0.81	-0.060	-0.049
Mozambique	0.405	-0.287	0.8	-0.054	-0.043
Niger	0.405	-0.287	0.8	-0.054	-0.043
Nigeria	0.405	-0.287	0.85	-0.083	-0.070
Rwanda	0.405	-0.287	0.78	-0.043	-0.033
Senegal	0.405	-0.287	0.74	-0.020	-0.015
Sierra Leone	0.405	-0.287	0.72	-0.008	-0.006
South Africa	0.405	-0.287	0.55	0.089	0.049
Sudan	0.405	-0.287	0.83	-0.071	-0.059
Tanzania	0.405	-0.287	0.77	-0.037	-0.028
Togo	0.405	-0.287	0.73	-0.014	-0.010
Uganda	0.405	-0.287	0.77	-0.037	-0.028
Zambia	0.405	-0.287	0.84	-0.077	-0.065
Zimbabwe	0.405	-0.287	0.79	-0.048	-0.038

*Note: $\boldsymbol{ADCCS^{\tau} = -\frac{\beta_1}{2\beta_2}}$ is 0.71 for all the countries.