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Disintegration

A Comparative Case Study of Brexit's Impact on the United Kingdom's Services Export to Sweden

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Abstract

This study investigates the consequences for services export from the United Kingdom to Sweden of the Brexit referendum in 2016 and the replacement of the UK's membership of the EU single market by the Trade and Cooperation Agreement (TCA) in 2021. Causal effects are estimated using synthetic control method (SCM) with three different sets of predictors per event and an additional synthetic control method using lasso regression. The estimates indicate losses in export compared to both the counterfactual case without the Brexit referendum and the counterfactual case without the actual exit from the single market, but only the losses from the referendum are significant, suggesting that firms avoided export market entry after the referendum in anticipation of future trade barriers. The average annual losses are between US$1.7 and US$2.9 billion, or 23 and 30 percent of the counterfactual export, since the referendum and between US$0.7 and US$1.3 billion, or 8 and 14 percent, since the TCA. This indicates a smaller impact on services than previous literature on Brexit has found for goods as well as a larger impact on the UK's services export to Sweden than previous literature has found for its services export to the EU.

Keywords: Brexit, Protectionism, Synthetic Control Method, SCM, Trade in Services

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List of Abbreviations

- **Brexit:** The United Kingdom's withdrawal from the European Union
- **DiD:** Difference-in-Differences
- **EEA:** The European Economic Area; the EU member states, Iceland, Liechtenstein and Norway
- **EU:** The European Union
- **GATS:** The WTO's General Agreement on Trade in Services
- **GATT:** The WTO's General Agreement on Tariffs and Trade
- **MFN:** Most Favored Nation, the nation to which a WTO member applies the most favorable trade rules
- **OECD:** The Organization for Economic Cooperation and Development
- **ONS:** The UK Office for National Statistics
- **SCM:** Synthetic Control Method
- **SCUL:** Synthetic Control Using Lasso
- **SDiD:** Synthetic Difference-in-Differences
- **STRI:** The OECD's Services Trade Restrictiveness Index
- **SUTVA:** Stable Unit Treatment Value Assumption
- **TCA:** Trade and Cooperation Agreement between the United Kingdom and the European Union, governing post-Brexit relations since 2021
- **UK:** The United Kingdom of Great Britain and Northern Ireland
- **WTO:** The World Trade Organization

1 Introduction

June 23, 2016 marked an important day in both British and European history, as one of the most long-standing members of the European Union voted to leave the union, by 52% votes in favor of exit against 48% votes to remain (BBC, 2016). The withdrawal from the EU by the United Kingdom, commonly referred to as Brexit, was the first time a member state of the EU decided to leave the union, except from when the much smaller and more concentrated economy of Greenland left the predecessing European Community in 1985 (Daigle et al., 2019), a clear contrast to the many rounds of accessions to the union since its establishment in 1957 (European Commission, nd). The debate about a possible Brexit gained significant momentum in 2013 after Prime Minister David Cameron had declared his support for a referendum on the continued membership of the UK in the EU, and in December 2015, the European Union Referendum Act was passed, concluding that a consultative referendum would be held on whether the UK should leave the EU or remain (Walker, 2021).

Although the mere possibility of a Brexit caused a lot of uncertainty to global markets and investors (Blitz 2016; Moore 2016), few polls pointed to a result in favor of leaving the EU (Duncan, 2016). The vote to leave thus became a shock to international markets with falling stock prices and depreciation of the British pound sterling as a consequence of the high uncertainty about when, and under what conditions, the withdrawal was going to take place (Flanders 2016; Martin 2016). At one end of the set of options was the so-called “hard Brexit” where the UK would leave without any replacement trade agreement, and trade between the UK and the EU would be governed only by the multilateral rules-based trade system of the World Trade Organization, including the General Agreement on Tariffs and Trade (GATT) and its most favored nation tariffs, and the General Agreement on Trade in Services (GATS). At the other end was the "soft Brexit" and the "Norway option" with continued access to the EU single market and its free movement of people, goods, services and capital[1].

After years of negotiations and multiple postponements of withdrawal, the UK officially left

[1]Through the Agreement on the European Economic Area (EEA), Iceland Liechtenstein and Norway have full access to the EU single market without being members of the EU (Eurostat, nd).

the EU in January 2020 (Walker, 2021; European Commission, 2025). However, the UK remained under EU law and jurisdiction, implying that it continued to be a full part of the single market, until the Trade and Cooperation Agreement (TCA) between the UK and the EU was applied provisionally from January 1, 2021 and entered into force fully on May 1, 2021 (European Commission, 2021). A timeline of events with significance for Brexit is presented in Table 1.

The decision by the UK to leave the EU, and the disintegration from the EU single market that it entails, is a clear contrast to the worldwide trend of globalization, integration, and reduction of trade barriers that has been observed since the end of the Second World War. It thus provides a rare opportunity to add to the rich literature on trade liberalization its much less observed and studied inverse.

Date	**Event**
January 23, 2013	Prime Minister David Cameron declares his support for a Brexit referendum (Walker, 2021).
June 25-26, 2015	PM Cameron makes his first notification to the European Council about his plans for a leave-or-remain referendum. This marks the beginning of a renegotiation process on the UK's future role in the EU (Walker, 2021).
October 19, 2015	PM Cameron declares that he seeks to renegotiate the terms for the UK in the four areas economic governance, competitiveness, sovereignty and social benefits, and free movement (Walker, 2021).
December 17, 2015	Queen Elizabeth II formally approves the European Union Referendum Act (Walker, 2021).
February 19, 2016	PM Cameron announces that the UK has reached a deal with the European Council, granting the UK special status in the EU (Walker, 2021).
June 23, 2016	**The UK citizens vote to leave the EU (European Commission, 2025).**
October 13, 2016	President of the European Council Donald Tusk declares that a "hard Brexit" is the only option if the UK leaves the union (European Commission, 2025).
March 29, 2017	The UK triggers EU article 50, a formal declaration to leave the union (European Commission, 2025).
March 31, 2017	President Tusk declares intention to make the process "as smooth as possible", and to "not pursue a punitive approach" (European Commission, 2025).
February 28, 2018	After seven rounds of negotiations, the European Commission presents its draft withdrawal agreement (European Commission, 2025).

Table continues on the next page

March 7, 2018	President Tusk declares that he sees no other option than a free trade agreement between the UK and the EU (European Commission, 2025).
January 11, 2019	The Council of the EU adopts the decision to sign the proposed withdrawal agreement (European Commission, 2025).
April 13, 2019	The Council agrees to extend the deadline for Brexit to October 31 (European Commission, 2025).
October 29, 2019	The Council agrees to extend the deadline for Brexit to January 31, 2020 (European Commission, 2025).
January 29-30, 2020	The UK and the EU ratify the withdrawal agreement (European Commission, 2025).
January 31, 2020	The UK is no longer an EU member state but remains under EU jurisdiction for a transition period until December 31 (European Commission, 2025).
December 30, 2020	The UK-EU Trade and Cooperation Agreement (TCA) is signed (European Commission, 2021).
January 1, 2021	**The TCA is applied provisionally and the UK is no longer part of the EU single market nor the customs union (European Commission, 2021).**
May 1, 2021	**The TCA enters into force fully (European Commission, 2021).**

Table 1: Timeline of Events with Significance for Brexit (Walker, 2021; European Commission, 2021; European Commission, 2025).

1.1 Purpose

The European Commission (2021) describes the TCA as more comprehensive than any traditional free trade agreement, but emphasizes that Brexit still implies a significant decrease in the level of economic integration for the UK. Although the TCA governing the post-Brexit relations between the UK and the EU since 2021 ensures zero tariffs and quotas to trade between the UK and the EU, customs procedures and national regulations entail barriers to trade. Multiple studies (Freeman et al., 2024; Kren and Lawless, 2024; Buigut and Kapar, 2023; Du et al., 2023) find a decrease in goods exports from the UK to the EU since the entry into force of the TCA, and a few studies (Kren and Lawless, 2024; Douch and Edwards, 2021) have found decreases in the bilateral export to Sweden as well. Due to lack of high quality data for services (Douch and Edwards, 2022) and heterogeneity among services sectors (Du and Shepotylo, 2024), the research on consequences for services is much more limited. Services trade is at the same time much more dependent on regulations than tariffs, which mainly affect goods trade (Du and Shepotylo, 2024; Douch and Edwards, 2021; Baldwin et al.,

2024), and many economists have argued that Brexit should have a stronger impact on services than goods (Du and Shepotylo, 2024).

While goods still constitute the largest fraction of international trade, many scholars (Eaton et al., 2019; Baldwin et al., 2024; Guzman et al., 2024) have pointed at a rapid increase of trade in services the last decades. Trade in services is furthermore of high importance for both the UK and the EU. The UK is the world leader in export of insurance and pension services as well as telecommunication services, and the second biggest exporter of services in the world (Department for Business and Trade, 2024). In 2024, services exports from the UK were 59 percent of total exports and 51 percent of the export to the EU (Webb and Ward, 2025). The EU, on the other hand, is the world's largest exporter and importer of services, with half of the services trade being intra-EU trade, and services account for one-fourth of the EU's international trade Eurostat (2024). This study therefore has the following purpose:

Purpose 1: *The purpose of this study is to analyze whether disintegration from the EU single market has an impact on trade in services.*

1.2 Research Questions

The TCA includes paragraphs concerning trade in services and many services sectors maintain access to the EU market through the agreement (Government of the UK, 2024). As mentioned by the Government of the UK, however, the services exports are much more dependent on national and sectoral regulations under the TCA. The Government of the UK specifically mention requirements that senior managers or directors have a particular nationality or residency, as well as limits to how much equity non-nationals can hold, as barriers to services trade after the UK left the EU single market. The free movement of people furthermore ended with the TCA as each EU-member state's immigration laws determine the eligibility of UK nationals to live, work or study in the EU, and similarly the UK requires EU citizens to apply for visas if they wish to live, work or study in the UK (Gower et al., 2023). Short term visits of up to 90 days in any 180 day period, including work visits, however, remained without visa requirements under the TCA, although UK travelers could no longer

use the immigration lane for EU citizens and might be asked to provide supporting documents upon entry into the EU (Gower et al.)[2].

The few studies I have found that estimate the effects of Brexit on the export of services focus on aggregate export to the world (Douch and Edwards, 2021; Ahmad et al., 2023) or aggregate export to the EU (Du and Shepotylo, 2024). While these are interesting research questions from a policy evaluation perspective, I argue that the studies in fact evaluate a variety of interventions at once, since the withdrawal from the EU entails different regulatory changes to different trade relationships (Government of the UK, 2024). My opinion is that it makes conceptually more sense to study each intervention separately. Therefore, in this study, I would like to focus on a single bilateral trade relationship between the UK and a partner country in the EU. To understand the aggregate consequences for the UK's trade with the EU, I suggest an inductive or bottom-up approach in which the estimated effects to each trade relationship between the UK and an EU member state are estimated separately and the effects are summarized. That is the approach of Douch and Edwards (2022) when studying the effects on goods trade.

Due to my own interest and by request from the National Board of Trade Sweden, I have decided to study the UK's export to Sweden. Sweden stands for 3.9 percent of the EU's import of services for third countries and 4.3 percent of the intra-EU import, making it among the top 10 service importers in the EU (Eurostat, 2024). The services exported from the UK to Sweden is mainly in the UN Statistics (2010) EBOPS2010 classification category *Other Business Services* (SJ), which includes research and development services, professional and management consulting services, technical, trade-related and other business services and operating leasing. That is followed by the categories *Financial Services* (SG), *Travel* (SD) and *Charges for the Use of Intellectual Property* (SH). The annual export of services from the UK to Sweden by category is presented in Figure 1.

[2]Since April 2, 2025, travelers to the UK that are not under visa requirement still need an Electronic Travel Authorisation (ETA) (Government of the UK, 2025).

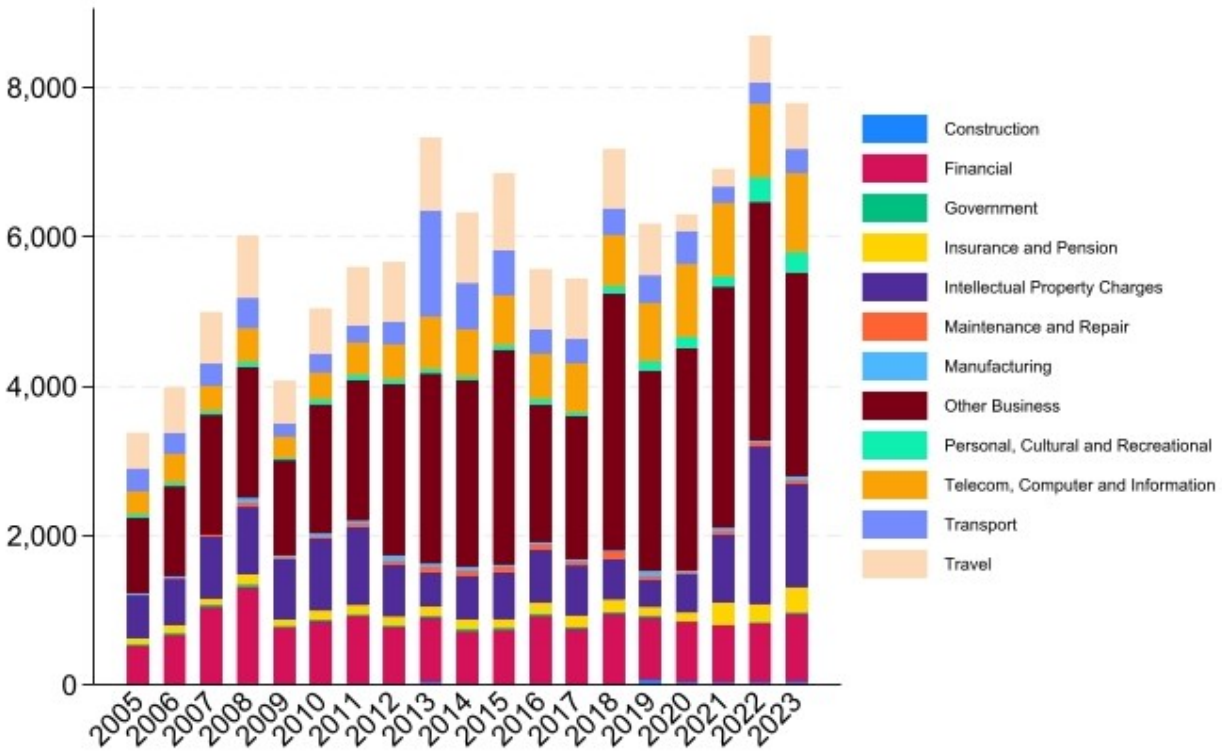


Figure 1: The UK's Annual Export of Services to Sweden (US$ million) by Service Sector (EBOPS2010) (OECD and WTO, 2025)

The UK and Sweden are both, through their membership in the World Trade Organization (WTO), parties to the General Agreement on Trade in Services (GATS), which through the most favored nation (MFN) principle requires its signatories to give each other "treatment no less favourable than that accorded to like services and services suppliers of any other country", with exception for treatment agreed to in free trade agreements (World Trade Organization, ndb). OECD (2025a) provide a Services Trade Restrictiveness Index (STRI) through which the regulatory barriers to trade in services for 51 countries are assessed and assigned a value between zero and one, with one being the highest degree of restrictiveness. In 2024, Sweden was assigned an STRI of 0.20, which was above the OECD average of 0.19 but below the average of 0.22 for all the 51 countries (OECD, 2025a). In the corresponding report for Sweden by the OECD, it is emphasized that trade restrictiveness has increased slightly in recent years and that the barriers to services trade for members of the EU single market are well below Sweden's MFN-levels governed by GATS. Legal services is pointed at as the least restricted sector compared to how it is restricted in other countries, while maritime transport

is the most restricted. Examples of other restrictions highlighted in the report are requirements on representation of residents in Sweden or the EEA[3] among board members and managers, wage parity with local employees, and a minimum level of capital deposited in Swedish banks to register a business in Sweden. Public procurement is furthermore restricted to signatories of certain trade agreements, including the WTO Agreement on Government Procurement (OECD), to which the UK is a signatory (World Trade Organization, nda).

In a recent report, the Stockholm Chamber of Commerce (2025) claim that the export of services from the UK to Sweden has increased since the formal Brexit in 2020. Although it is correct that the value of services export has increased since 2020, which Figure 1 shows, the Stockholm Chamber of Commerce does not take into account what the exports would have been in a counterfactual case where the UK had remained in the EU. The formal Brexit in 2020 is also of minor relevance to trade policy evaluation, as the changes in trade policy occurred at the implementation of the TCA in 2021. In addition, the conclusion is counter-intuitive since the barriers to trade in services between the UK and Sweden have increased since the UK left the EU single market (Government of the UK, 2024; European Commission, 2021; Webb and Ward, 2025; OECD, 2025a). Thus, my first research question is:

Research Question 1: *Did the exit from the EU single market and the implementation of the Trade and Cooperation Agreement (TCA) in 2021 affect the UK's export of services to Sweden?*

The content of the TCA was by no means a surprise to the firms when the agreement entered into force. As evident from Table 1, the implementation was preceded by years of negotiations and draft agreements, which indicated the direction of the post-Brexit outcomes. It is therefore difficult to believe that exporting firms did not take precautionary measures prior to the TCA's entry into force, and only analyzing the effects from 2021 would hence provide a limited understanding of the consequences from Brexit. The most unexpected event with a clear indication of the UK's future integration in the EU in Table 1 is the referendum on June 23, 2016. Therefore, my second research question is:

[3]The EU member states, Iceland, Liechtenstein and Norway.

Research Question 2: *Did the Brexit referendum in 2016 affect the UK's export of services to Sweden?*

To better understand the impact and compare it with studies of goods and studies of services trade at the aggregate UK-EU and UK-World level, my third research question is:

Research Question 3: *If the answer to any of Research Questions 1 or 2 is yes, of what sizes were the gains or losses?*

In Section 2, I review and discuss the literature on theories and empirical evidence of how trade barriers and anticipation of trade barriers affect international trade flows. The section also covers previous studies of Brexit's effects on the UK's services and goods exports. Based on the literature review, I formulate two hypotheses.

For this study, I apply the synthetic control method (SCM) (Abadie and Gardeazabal, 2003; Abadie et al., 2010; Abadie et al., 2015; Abadie, 2021) to estimate the counterfactual services export from the UK to Sweden, had the UK not decided to leave the EU. The methodology is described in Section 3. In Section 4, I provide estimates of the effects that the Brexit referendum and the implementation of the TCA have had on the UK's services export to Sweden and in Section 5, I test whether the results are robust to different methodological decisions. The results and how they answer the research questions are discussed in Section 6 and my conclusion is presented in Section 7.

2 Literature Review

This section covers the most relevant literature for this study. I start by reviewing the literature on the nature and consequences of barriers to trade and anticipation of barriers to trade in Subsection 2.1. That is followed by a review of previous studies analyzing the effects of Brexit on the United Kingdom's exports, with goods export covered in Subsection 2.2 and services covered in Subsection 2.3. The section concludes with a summary and identification of existing gaps in the literature, as well as formulation of hypotheses based on the literature, in Subsection 2.4.

2.1 Barriers to Trade

There is an extensive literature on the benefits of trade liberalization, both from a theoretical and empirical point of view (Ohlin, 1933; Krugman, 1979; Romer, 1990; Melitz, 2003; Sampson, 2016; Trefler, 2004; Pavcnik, 2002; Eslava et al., 2013). Withdrawal from trade agreements, on the other hand, are rare decisions about which the number of studies is low. Daigle et al. (2019) have identified 163 cases where trade agreement have been either suspended or canceled, in contrast to what they describe as a "global trend towards trade liberalization". A clear majority of the 163 cases are trade agreements that have been immediately replaced by an alternative agreement to further liberalize trade, such examples are countries withdrawing from regional trade agreements as part of their accession to the EU. In the period from 1950 to 2019, the authors have identified 29 cases of countries leaving trade agreements without an immediate replacement.

Douch and Edwards (2021) and Ahmad et al. (2023) highlight the anticipation of future trade barriers and the extent of intertemporality in trading decisions as two factors that caused trade to diminish already after the Brexit referendum in 2016 and, as they suggest, perhaps even from some time before the referendum, when Brexit was seen as a realistic outcome. Theoretical models such as those of Melitz (2003), Eaton et al. (2011) and Arkolakis (2010) let firms make their entry decisions into domestic and export markets based on sunk market entry costs. Douch and Edwards (2021) and Ahmad et al. (2023) argue that these sunk costs prevent firms from entering the markets when the expected future returns decrease due to an expected future trade shock, and that firms

therefore decided not to enter the UK's export markets after the Brexit referendum. Either because a negative net present value made entry unprofitable or because firms deferred entry until they had better knowledge about the effects of the expected trade shock and whether a trade shock would actually occur (Douch and Edwards, 2021; Ahmad et al., 2023). Osnago et al. (2015) have estimated the trade effects of the average uncertainty in trade policies to be comparable to average tariffs between 1.7 and 8.7 percent. Handley and Limão (2015) similarly show that reducing tariffs but not the trade policy uncertainty yields just 30 percent of the increase in exports that reducing both tariffs and uncertainty would cause, when studying the case of Portugal's accession to the EU in 1986, and Carballo et al. (2022) show that trade agreements are a crucial component in reducing uncertainty and the probability of uncertainty shocks. Freund and McLaren (1999) furthermore provide evidence of anticipatory increases in trade prior to free trade agreements entering into force. The results of Ait Benasser (2025) however indicate that the effects to trade from protectionism are less significant than those from liberalization. Similarly, in the theoretical trade model by Ravikumar et al. (2024), the welfare gain from trade liberalization is greater than the loss in welfare from a reversal to protectionism since economies accumulate a capital stock under liberalization, which is used to smooth consumption under protectionism.

Historically, barriers to international trade have consisted of tariffs applied as a tax on imported goods. Other common trade barriers are non-tariff barriers such as quotas and customs procedures, which also mainly affect trade in goods. Today, scholars like Baldwin et al. (2024) and Benz and Jaax (2022) argue that services face higher trade barriers than goods, but they are not fiscal like tariffs, since cross-border trade of services is much more difficult to track than goods trade. Baldwin et al. distinguish between regulatory- and technical barriers to trade in services. They argue that technical barriers to services, such as the importance of face-to-face communication, are the highest and make most services untradeable but are being reduced through digital technologies, as opposed to regulatory ones, which they do not believe will be much lower in the near future. Du and Shepotylo (2024), Ahmad et al. (2023) and Douch and Edwards (2021) all focus on regulatory barriers when identifying the implications for the trade in services from Brexit. Du and Shepotylo emphasize

that the EU single market is unique in the degree to which it has liberalized regulation to trade in services, and that the consequences of Brexit therefore will be stronger for services than for goods. According to Lowe (2021), new free trade agreements after Brexit are unlikely to increase the export market access for the UK's firms, but could prevent reversal to a higher degree of protectionism. Lowe furthermore emphasizes that the UK should, in order to maintain access to the EU services market, keep visa application fees for foreign workers low or remove them completely, support free flow of data, and engage in dialogue about regulations with politicians and regulators.

Du and Shepotylo (2024) and Douch and Edwards (2021) emphasize that the wide range of modes of supply and higher degree of heterogeneity among sectors for services compared to goods makes it more difficult to identify the dynamics for services, and that Brexit could have a positive effect for some sectors and negative effects for other sectors. One aspect raised by Du and Shepotylo and Lowe (2021) is the high presence of multinational firms in the UK, previously using the UK as a gateway to the European single market and utilizing the large human capital in finance and business competencies in the UK for those operations. The results of Tamberi (2024) show a diversion of investments into export-platform activities, away from the UK and over to the remaining EU member states instead. Du and Shepotylo argue that limitations to the freedom of movement, EU-restrictions to certain services and discrepancies between the UK's and the EU's data protection laws are among the consequences of Brexit that have made remaining EU members, such as Ireland, more attractive hubs for EU-market access. The financial industry, which is of high importance to the UK, is on the other hand highlighted by both Du and Shepotylo and Douch and Edwards as a sector in which the UK could benefit from having its own regulation instead of adapting to common EU legislation.

2.2 Effects of Brexit on Export of Goods

A number of articles study the effects from Brexit on goods export, looking at both the consequences of the referendum in 2016 and the Trade and Cooperation Agreement (TCA) entering into force in 2021, applying different methods and using different data. Freeman et al. (2024) use quarterly firm-level data from the UK to estimate differences-in-differences (DiD) between the UK's

goods trade with the EU and the UK's goods trade with the rest of the world (RoW), before and after the Brexit referendum in 2016 as well as the entry into force of the TCA in 2021. Within the treatment period following the referendum in June 2016, the authors do not find any significant decline in the UK's trade in goods with the EU relative to its trade with the RoW. Under the TCA from January 2021 to the end of 2022, which is when their data ends, they do, however, estimate a 14 percent reduction in goods export to the EU relative to the RoW.

To handle the potential spillover effects issue of Brexit possibly not only affecting UK-EU-trade but also the UK's trade with the RoW, which Freeman et al. (2024) argue could arise due to supply chain linkages, interdependencies across origins, scale effects, and capacity constraints, the authors conduct an additional analysis of the UK's trade with the RoW. They do so using levels regression, through which they cannot reject that Brexit did not have an effect on UK's exports to the RoW.

Kren and Lawless (2024) argue, in contrast to Freeman et al. (2024), that the UK's trade with the RoW is not an appropriate no-Brexit counterfactual when estimating the effects of Brexit on UK's trade with the EU. Instead they use the EU's trade with the RoW as counterfactual when estimating a DiD specification similar to that of Freeman et al. (2024). They find a 16 percent decrease in goods export to the EU under treatment from the TCA in 2021. Kren and Lawless furthermore look at bilateral changes and find a 25 percent decrease in goods export to Sweden following the TCA.

Buigut and Kapar (2023) apply gravity regression to quarterly data on trade in goods for Q1 2005 to Q3 2022 from the International Monetary Fund (IMF) Direction of Trade Statistics (DOTS), with 53 trading partners including all the EU-members, to analyze the effect from Brexit on goods trade between the UK and the EU, as well as the trade within the EU. The authors split the data into three treatment periods; from the referendum in Q2 2016 to Q4 2019, the transition period from Brexit in Q1 2020 to Q4 2020, and the period under the TCA from Q1 2021 to the end of the data in Q3 2022. They estimate the effect of the referendum to be a decline of 9.8 percent in export of goods to the EU. In both the transition period between Q1 and Q4 2020 and under the TCA from 2021, their

estimated effects are also negative, but not significant. For intra-EU trade, Buigut and Kapar (2023) estimate a 1.5 percent increase from the referendum, no significant effect in the transition period and a 4.6 percent increase under the TCA.

Douch and Edwards (2022) study the effect on the UK's trade in goods from the Brexit referendum in 2016 using synthetic control method (SCM). For the analysis, the authors use monthly data from the IMF DOTS, which covers trade in goods for a pool of 29 countries; the UK, 14 member states of the EU[4] and 14 non-EU members[5]. According to the authors, all EU member states could to some extent be considered affected by the Brexit referendum, and they therefore only use trade between the non-EU members when constructing their synthetic counterfactuals, which are weighted averages of trade flows that best replicate each of the analyzed trade flow between the UK and its counterparts in the period prior to the referendum. Douch and Edwards argue that anticipation effects took place already in 2015 and therefore fit their model until June 2015, a year before the referendum. For the UK's goods export to Sweden, they find a 14.9 percent loss relative to the synthetic counterfactual from July 2015 to June 2016, a 24.3 percent loss from July 2016 to February 2017 and a 20.4 percent loss from March 2017 to March 2018. The UK's bilateral export flows to each of the 14 EU member states considered are all furthermore found to have decreased in each of the three periods studied, except for the export to Austria which increased by 16.9 percent in the year before the referendum. The largest decrease found by Douch and Edwards is the 57.0 percent decrease in the UK's export to Luxembourg in March 2017 to March 2017, and the smallest decrease was an 8.1 percent decrease in export to Italy from July 2015 to June 2016. The authors furthermore find the declines for the 14 EU member states in total to be 20.8 percent in July 2015 to June 2016, 24.9 percent from June 2016 to February 2017 and 20.8 percent from March 2017 to March 2018.

Du et al. (2023) estimate the effect of the TCA on the exports of the United Kingdom by applying both DiD and SCM, as well as the synthetic difference-in-differences (SDiD) method (Arkhangelsky et al., 2021), to quarterly bilateral data on trade in goods provided by UN Comtrade

[4]Germany, France, the Netherlands, Italy, Spain, Belgium, Luxembourg, Portugal, Austria, Finland, Denmark, Sweden, Ireland and Greece.

[5]Australia, Canada, China, Hong Kong, India, New Zealand, USA, South Africa, Brazil, Israel, Japan, South Korea, Mexico and Russia.

for the period Q1 2019 to Q1 2022. They estimate 15.6 percent lower export from the UK to the EU using DiD, 23.7 percent lower using SCM and 22.9 percent lower using SDiD.

2.3 Effects of Brexit on Export of Services

The literature on the effects of Brexit on trade in services is more scarce than that for trade in goods. Studies have, however, shown a strong interlinkage between export of goods and export of services. For instance, Ariu et al. (2019) find that firms prefer to import goods and services from the same origin and that liberalization for trade in goods increase the firms' imports of services from the liberalized markets, while protectionism for goods decrease the import of services. Stremersch et al. (2001) and Tien (2012) have furthermore, among other, pointed at increased integration of services into goods sales, where firms and consumers buy full-service solutions that include goods, maintenance and support.

One remark by Douch and Edwards (2022) is that they lack bilateral monthly data on trade in services. Douch and Edwards (2021) do however study the effects of the Brexit referendum on the UK's trade in commercial services using monthly aggregate data of services export from Q1 2010 to Q2 2018. The authors use synthetic control method (SCM), as Douch and Edwards (2022), and a donor pool that includes the aggregate export of services for 18 EU-members[6], and two non-EU members[7]. The authors find a statistically significant decline of 6.3 percent in total UK services export relative to the synthetic counterfactual services export of the UK, in the post-referendum period until Q2 2018.

Douch and Edwards (2021) extend their analysis by looking at three specific service sectors; transport services, travel services and other commercial services. They find that other commercial services experienced a larger decline after the referendum, -10.7 percent, while transport services and travel services both experienced increases, 0.9 and 17.8 percent respectively.

Du and Shepotylo (2024) use the WTO-OECD Balanced Trade in Services (BaTIS) dataset, in which data gaps have been filled through estimations, to study the effects from the referendum in

[6]Austria, Belgium, Czechia, Germany, Denmark, Spain, Finland, France, Greece, Hungary, Italy, Japan, Lithuania, Luxembourg, Malta, Poland, Portugal and Slovakia.

[7]Brazil and the USA.

2016. They apply DiD, SCM and SDiD to the data and find a 6.07 percent increase using DiD, a non-significant decrease using SCM and a 5.65 percent decrease using SDiD, for aggregate export of services to the world. For aggregate export of services to the EU, Du and Shepotylo apply the SDiD and find an 8.49 percent decrease in the period 2017-2019. When analyzing the export to the EU for specific sectors, Du and Shepotylo find the largest percentage decrease in *Insurance and pension services* (EBOPS2010 classification code SF), -14.98 percent, followed by *Financial services* (SG), for which the decrease was -11.5 percent. Furthermore, they estimate -7.61 percent for *Travel* (SD), -7.06 percent for *Charges for the use of intellectual property n.i.e* (SH) and -7.19 percent for *Other business services* (SJ)[8].

Ahmad et al. (2023) use gravity regression to estimate the effects from uncertainty about Brexit on UK trade in services. Using quarterly bilateral trade in services data from the UK Office of National Statistics over the period 2016Q1 to 2018 Q4, including 67 countries, and proxies for Brexit probability, the authors conclude that increases in the probability of Brexit caused a decrease in the aggregate exports of services of between 20-49 log points. The lower estimate is found using an OLS specification while the higher estimate is from an IV-regression.

2.4 Summary, Identification of Gaps and Hypotheses

Although the TCA entered into force just four years ago, the literature review shows that numerous studies analyzing the effects of the TCA and the referendum have been published already. A clear majority of the studies are on goods trade, mainly due to lack of high quality data for services (Douch and Edwards, 2022) and the high degree of heterogeneity among services sectors (Du and Shepotylo, 2024) making it difficult to study trade in services. The three studies found analyzing the change in services export due to Brexit (Ahmad et al., 2023; Douch and Edwards, 2021; Du and Shepotylo, 2024) furthermore only cover the referendum in 2016 and the effects on the UK's aggregate export to the world and the EU, as opposed to the studies for goods which cover both the referendum and the TCA, as well as both aggregate exports to the EU and bilateral exports to

[8]SD, SG, SH and SJ are the four largest export sectors from UK to Sweden and are selected for the sector robustness analysis of this study.

individual countries. There is hence a clear gap of missing studies on bilateral export of services, as well as studies on services export under the TCA, in the literature on the effects that Brexit has had on the UK's exports.

In more broad terms, considering the rarity in withdrawal from trade agreements (Daigle et al., 2019), Brexit provides a delicate opportunity to study the effects of protectionism and add to the rich literature on trade liberalization its less studied inverse. Studying the effects of Brexit could also contribute to the lack of studies quantifying the economic benefits of EU membership expressed by Campos et al. (2014).

The measured significant effects on export of goods are consistent in being negative, both the effects of the referendum and the TCA, and for both aggregate export to the EU and export to Sweden. For the referendum in 2016, the estimated significant effects on export to the EU range from a 10 percent reduction (Buigut and Kapar, 2023) to a 25[9] percent reduction (Douch and Edwards, 2022), while for the TCA in 2021 the effects are between -14 percent (Freeman et al., 2024) and -24 percent (Du et al., 2023). For bilateral exports from the UK to Sweden, there is just one study per event, and the results are in the upper end of the result ranges for UK-EU trade, with an estimated 24 percent reduction after the referendum (Douch and Edwards, 2022) and a 25 percent reduction under the TCA (Kren and Lawless, 2024). Considering the interlinkage between goods and services found by Ariu et al. (2019), Stremersch et al. (2001) and Tien (2012), it is a plausible hypothesis that the decrease in goods export has caused the services export to decline as well.

The literature studied on export of services after the referendum is not fully consistent in finding negative effects, with the positive DiD estimate for aggregate export by Du and Shepotylo (2024) being the exception. The estimates for aggregate export using SDiD (Du and Shepotylo, 2024) and SCM (Douch and Edwards, 2021) indicate similar losses, 5.7 percent and 6.3 percent respectively. The loss in export to the EU estimated by Du and Shepotylo (2024) is somewhat larger, 8.5 percent, but smaller than than the reductions in goods export to the EU from the referendum, which contradicts the literature suggesting a higher increase in the barriers to services than to goods after Brexit (Baldwin

[9]This effect is for export to a selection of 14 EU member states and not all 27 members.

et al., 2024; Benz and Jaax, 2022; Du and Shepotylo, 2024).

The services export from the UK to Sweden and from the UK to the EU are mainly within the same services sectors. Based on data from the OECD and WTO (2025) Balanced Trade in Services (BaTIS) dataset for the period 2005-2015, most of the UK's services export to Sweden was within the four sectors *Other business services* (SJ), *Financial services* (SG), *Travel services* (SD) and *Charges for the use of intellectual property, not included elsewhere* (SH). For the UK's export to the EU, the four sectors were the same, but with *Charges for the use of intellectual property, not included elsewhere* (SH) replaced by *Transport* (SH) (OECD and WTO, 2025). These sectors all experienced losses after the referendum in the findings by Du and Shepotylo (2024), which in combination with the consistent declines in goods exports to both the EU and Sweden supports the hypothesis that the UK's services exports to Sweden have declined due to the Brexit referendum. The similar sectoral composition of exports to Sweden and to the EU, in combination with the loss in *Transport* (SH) estimated by Du and Shepotylo, 5.68 percent, being just slightly smaller than the 7.06 percent loss for *Charges for the use of intellectual property, not included elsewhere* (SH) furthermore suggests that the decline in UK's export to Sweden should be similar to the decline for UK-EU. Thus, my first hypothesis is as follows:

Hypothesis 1: *The Brexit referendum in 2016 caused a decline in the UK's export of services to Sweden, similar to the 8.5 percent decline in services export to the EU.*

Stating a hypothesis for the effects on services export from the TCA is more difficult since that has not been estimated in any of the studies found. The argumentation of Baldwin et al. (2024) and Benz and Jaax (2022) that services typically face higher barriers than goods, the prediction by Du and Shepotylo (2024) that the EU single market's ability to ensure free movement of services will be difficult to match and that the consequences of Brexit therefore should be more negative for services, and the fact that the TCA provides zero tariffs and quotas (European Commission, 2021), which are typical barriers to trade in goods, suggests a larger decline in services export than goods export. The estimated effects of the referendum (Du and Shepotylo, 2024) indicate the opposite, but important to keep in mind is that many aspects of future trade policies were uncertain immediately after the

referendum, while fully clear at the implementation of the TCA. The reduction in goods trade after the referendum could thus have been an overreaction from expected higher barriers to goods export than what actually became the case under the TCA. If so, that should be corrected at the implementation of the TCA when the uncertainty is removed. Therefore, my second hypothesis is as follows:

Hypothesis 2: *The exit from the EU single market and the implementation of the Trade and Cooperation Agreement (TCA) in 2021 caused a decline in the UK's export of services to Sweden, larger than the 25 percent decline in goods export.*

3 Methodology

In this study, I take an inductive approach in trying to explain the effects of disintegration and protectionism on trade in services. The idea behind the research design is that looking at one particular case of disintegration, the withdrawal from the European Union by the United Kingdom (Brexit), and its effects on services export to specific EU member states, it could be possible to draw general conclusions about whether disintegration boosts or hinders trade in services. The null hypotheses of the study are that neither the Brexit referendum in 2016 nor the UK's exit from the EU single market and entry into the Trade and Cooperation Agreement (TCA) with the EU in 2021 lead to any significant differences in the UK's services export to Sweden compared to if the UK had *ceteris paribus* voted to remain and Brexit never had occured. The referendum and the entry into force of the TCA are referred to as the two treatments of this study. Estimating the UK's services exports in the counterfactual cases where the UK votes to remain and remains in the EU are the main research problems of this study, and I try to solve them through a quantitative approach where the synthetic control method (SCM), described by Abadie and Gardeazabal (2003), Abadie et al. (2010), Abadie et al. (2015) and Abadie (2021), is used to design "synthetic" counterfactual export flows of the UK. The counterfactual services exports to Sweden after the referendum and the TCA are then compared to those of the actual UK.

The section begins with a review of the literature on SCM in Subsection 3.1, and the SCM using lasso regression in Subsection 3.2. Subsection 3.3 covers the method used for the collection of services export data used both as predictors for the synthetic counterfactual exports of services from the UK to Sweden, and to compare the actual services export under treatments with the synthetic counterfactual exports. The subsection also includes the method used for the collection of data on covariates used together with the services data as predictors for the synthetic UK-Sweden exports. In Subsection 3.4, I explain how the sample of trade flows to construct the synthetic counterfactuals have been decided and in Subsection 3.5 I explain the methodology used to construct the synthetic exports of the UK and compare them with the actual. Subsection 3.6 covers the methods used for

statistical inference and robustness tests and in Subsection 3.7, I discuss why I choose the SCM over the alternative difference-in-differences (DiD) and synthetic difference-in-differences (SDiD) methods.

3.1 Synthetic Control Method

The synthetic control method or synthetic counterfactuals method (SCM) for comparative case studies was first introduced by Abadie and Gardeazabal (2003) and has been used and explained further by Abadie et al. (2010), Abadie et al. (2015) and Abadie (2021). Abadie and Gardeazabal (2003) study the effects of terrorist activity on the GDP per capita of the Basque Country by constructing what they describe as a "synthetic control region" from panel data. The synthetic control region is based on a weighted combination of economic parameters of other Spanish regions, intended to resemble the parameters of the Basque Country before terrorism activities began in the late 1960s. The authors propose this as a solution to the problem that no counterfactual or untreated Basque Country, i.e. without terrorism, exists, and that there are unobserved characteristics affecting the economic growth of the different parts of Spain differently, which cannot be controlled for in a comparison between the regions.

To construct the synthetic control region, Abadie and Gardeazabal (2003) use data of economic growth predictors from 17 Spanish regions; 16 control regions and the one treated Basque Country. They let $J = 16$ be the number of control regions and construct a $(J \times 1)$ vector $\mathbf{W} = (w_1, \ldots, w_J)'$ of non-negative weights given to each of the control regions, idicating the extent to which it represents a synthetic Basque Country. The pre-terrorism values of K possible predictors of GDP per capita are contained in the $(K \times 1)$ vector $\mathbf{X_1}$ for the Basque Country and the $(K \times J)$ matrix $\mathbf{X_0}$ for the control regions. Furthermore, the authors let $\mathbf{V}$ be a diagonal matrix giving non-negative weights to each of the K predictors.

By solving

$$\min_{\mathbf{W}} (\mathbf{X_1} - \mathbf{X_0}\mathbf{W})'\mathbf{V}(\mathbf{X_1} - \mathbf{X_0}\mathbf{W}) \text{ s.t. } w_j \geq 0 \text{ for } j = 1, 2, \ldots, J \text{ and } w_1 + \ldots + w_J = 1$$

Abadie and Gardeazabal (2003) conclude that the vector $\mathbf{X_1^*} = \mathbf{X_0}\mathbf{W}^*(\mathbf{V}^*)$, where Catalonia is given the weight $w^* = 0.8508$, Madrid is given the weight $w^* = 0.1492$, and all other regions are given weight $w^* = 0$ in $\mathbf{W}^*$, best represents a synthetic Basque Country before the terrorism began. The matrix of optimal weights $\mathbf{V}^*$ is decided by solving

$$\mathbf{V}^* = \arg\min_{\mathbf{V}\in\mathcal{V}} (\mathbf{Z_1} - \mathbf{Z_0}\mathbf{W}^*(\mathbf{V}))'(\mathbf{Z_1} - \mathbf{Z_0}\mathbf{W}^*(\mathbf{V}))$$

where $\mathbf{Z_0}$ is the $(10 \times J)$ matrix of yearly per capita GDP values in the period 1960-1969, before terrorism activities began, for the J control regions and $\mathbf{Z_1}$ is the (10×1) vector of pre-treatment per capita GDP values in 1960-1969 for the Basque Country. $\mathcal{V}$ is the set of all non-negative diagonal $(K \times K)$ matrices.

By comparing the time series for economic growth of the synthetic Basque Country, i.e. the weighted average economic growth of Catalonia and Madrid, which did not face terrorism, with the time series for economic growth of the actual Basque Country under terrorism, Abadie and Gardeazabal (2003) claim to isolate the effect from terrorist activity on the economic growth of the Basque Country. This is described mathematically as comparing the $(T \times 1)$ vector $\mathbf{Y_1}$ of real per capita GDP for the Basque Country during the T time periods it faced terrorist activity, and the $(T \times 1)$ vector $\mathbf{Y_1^*} = \mathbf{Y_0}\mathbf{W}^*(\mathbf{V}^*)$ where $\mathbf{Y_0}$ is a $(T \times J)$ matrix of real per capita GDP for the other $J = 16$ regions.

To check the validity of their method, Abadie and Gardeazabal (2003) conduct a placebo test where they construct a synthetic replica of a region without terrorist activity and compare it with the economic growth of the actual region in the treatment period of the Basque Country. The authors argue that if the gap between the synthetic and actual Basque Country was solely driven by terrorist activity, there should not be any observed difference between the synthetic and actual Catalonia during the same period.

In their discussion, Abadie and Gardeazabal (2003) mention the issues with spillover effects. According to the authors, the terrorist activities by ETA were not only conducted within the Basque

Country but also other Spanish regions, implying that the treatment also affects the control regions, making the estimated gap smaller than the true gap. The authors do however argue that the spillover effects must be much smaller than the effect on the Basque Country and that the small size of the Basque Country in comparison with the parts of Spain constituting its synthetic replica, also makes the bias from diverted investments small.

Abadie et al. (2010) provide a more extensive model for the synthetic control method, based on the difference-in-difference framework when studying the effects of California's tobacco control program from 1988; Proposition 99. They also add new inferential methods to the model, thus allowing for evaluation of the significance of estimates from the SCM. In describing the model, they let

$$Y_{it} = Y_{it}^N + \alpha_{it} D_{it}$$

be the specification for the outcome variable Y for unit i at time t, where unit is interchangeable with for instance country, state or city. Y_{it}^N is the outcome for unit i at time t if the treatment of interest, e.g. a law, event or shock, is not imposed on unit i at time t. If no treatment is in place, the dummy variable D_{it} will furthermore attain the value 0, while if i is treated at time t, then $D_{it} = 1$. The treatment effect on unit i at time t is thus

$$\alpha_{it} = Y_{it}^I - Y_{it}^N$$

where Y_{it}^I is the outcome for unit i at time t under treatment. The authors specify the outcome without the treatment by

$$Y_{it}^N = \delta_t + \boldsymbol{\theta}_t \mathbf{Z}_i + \boldsymbol{\lambda}_t \boldsymbol{\mu}_i + \epsilon_{it} \tag{1}$$

where δ_t are time fixed effects, $\mathbf{Z}_i$ is a $(r \times 1)$ vector of observed predictors of the outcome variable and $\boldsymbol{\mu}_i$ a $(r \times 1)$ vector of unobserved predictors, that vary by unit i, $\boldsymbol{\theta}_t$ and $\boldsymbol{\lambda}_t$ are $(1 \times r)$ vectors of unobserved coefficients, that vary with time t, and ϵ_{it} is an idiosyncratic shifter. This is according to Abadie et al. (2010) identical to a difference-in-difference model but with the extension that they

allow for the unobserved coefficients $\boldsymbol{\lambda}_t$ to vary with time. In a difference-in-difference model, $\boldsymbol{\lambda}_t\boldsymbol{\mu}_i$ would instead have been replaced by unobserved unit fixed effects.

Abadie et al. (2010) refer to the group of J control units as the donor pool, and similar to Abadie and Gardeazabal (2003), they let the $(J \times 1)$ vector $\mathbf{W} = (w_2, ..., w_{J+1})'$ be the weights given to each unit in the donor pool to represent the treated unit prior to treatment, the so called synthetic counterfactual or synthetic control. The authors' specification for the outcome variable of the synthetic control at each time t is

$$\sum_{j=2}^{J+1} w_j Y_{jt} = \delta_t + \boldsymbol{\theta}_t \sum_{j=2}^{J+1} w_j \mathbf{Z}_j + \boldsymbol{\lambda}_t \sum_{j=2}^{J+1} w_j \boldsymbol{\mu}_j + \sum_{j=2}^{J+1} w_j \epsilon_{jt}$$

They furthermore let there be $t = 1, ..., T$ time periods with $t = 1, ..., T_0$, where $1 \leq T_0 < T$, being the times prior to when the treatment unit $i = 1$ is treated, and the problem of estimating the treatment effect α_{1t} for $t > T_0$ is thus equivalent to estimating Y_{1t}^N for $t > T_0$, since Y_{1t}^I is observed for $t > T_0$. Abadie et al. (2010) prove that under the assumption that there exists a vector $\mathbf{W}^* = (w_2^*, ..., w_{J+1}^*)$ such that

$$\sum_{j=2}^{J+1} w_j^* Y_{j1} = Y_{11}, \quad \sum_{j=2}^{J+1} w_j^* Y_{j2} = Y_{12}, \quad \ldots, \quad \sum_{j=2}^{J+1} w_j^* Y_{jT_0} = Y_{1T_0} \text{ and } \sum_{j=2}^{J+1} w_j^* \mathbf{Z}_j = \mathbf{Z}_1 \tag{2}$$

it holds that

$$Y_{1t}^N = \sum_{j=2}^{J+1} w_j^* Y_{jt} + \sum_{j=2}^{J+1} w_j^* \sum_{s=1}^{T_0} \boldsymbol{\lambda}_t \Big(\sum_{n=1}^{T_0} \boldsymbol{\lambda}_n' \boldsymbol{\lambda}_n \Big)^{-1} \boldsymbol{\lambda}_s' (\epsilon_{js} - \epsilon_{1s}) - \sum_{j=2}^{J+1} w_j^* (\epsilon_{jt} - \epsilon_{1t})$$

at times $t \in \{T_0 + 1, ..., T\}$ if $\sum_{t=1}^{T_0} \boldsymbol{\lambda}_t' \boldsymbol{\lambda}_t$ has a matrix inverse, i.e. is a nonsingular matrix. They furthermore prove that

$$\sum_{j=2}^{J+1} w_j^* \sum_{s=1}^{T_0} \boldsymbol{\lambda}_t \Big(\sum_{n=1}^{T_0} \boldsymbol{\lambda}_n' \boldsymbol{\lambda}_n \Big)^{-1} \boldsymbol{\lambda}_s' (\epsilon_{js} - \epsilon_{1s}) - \sum_{j=2}^{J+1} w_j^* (\epsilon_{jt} - \epsilon_{1t})$$

is bound by a function approaching zero as T_0 increases. Abadie et al. (2010) thus suggest that

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

is a valid estimator of the treatment effect at times $t \in \{T_0 + 1, ..., T\}$ when T_0 is sufficiently large.

It is not very plausible that Condition 2 would hold in real data but according to Abadie et al. (2010), the model provides valid estimates if Condition 2 is met approximately. To find the vector $\mathbf{W}^* = (w_2^*, ..., w_J^*)$ that best meets Condition 2, Abadie et al. (2010) use both pre treatment outcomes and covariates $\mathbf{Z}_1$ and $\mathbf{Z}_0$[10] as predictors of outcomes under treatment when constructing the $(K \times 1)$ vector $\mathbf{X_1}$ and the $(K \times J)$ matrix $\mathbf{X}_0$ of predictors. Similar to Abadie and Gardeazabal (2003), the authors then find the optimal $\mathbf{W}^*$ by minimizing $\|\mathbf{X}_1 - \mathbf{X}_0\mathbf{W}\|$ where

$$\|\mathbf{X}_1 - \mathbf{X}_0\mathbf{W}\|\mathbf{V} = \sqrt{(\mathbf{X}_1 - \mathbf{X}_0\mathbf{W})'\mathbf{V}(\mathbf{X}_1 - \mathbf{X}_0\mathbf{W})}$$

$\mathbf{V}$ is is also chosen the same way as in Abadie and Gardeazabal (2003).

For statistical inference Abadie et al. (2010) build upon the idea of conducting placebo tests proposed in Abadie and Gardeazabal (2003) and let the estimated placebo treatment effects $\hat{\alpha_{2t}}, ..., \hat{\alpha_{J+1t}}$ constitute the distribution of a test statistic. Based on this distribution, the authors estimate the probability of finding a treatment effect as large as that found for the treatment unit. In their particular study they find it to be less than 5%, implying that their results have a significance of $p < 0.05$.

Abadie et al. (2015) suggest running placebo tests where the start of the treatment period is shifted to different times. They also conduct robustness tests through a so called "leave-one-out-analysis" where they iteratively omit one of the units with positive weights in the donor pool from the synthetic control.

Abadie (2021) discusses the importance of assessing the predictive power of $\mathbf{X_0}$ and $\mathbf{X_1}$ on

[10] $\mathbf{Z}_1$ and $\mathbf{Z}_0$ as in specification 1, not to be confused with the notation by Abadie and Gardeazabal (2003) where $\mathbf{Z}_1$ and $\mathbf{Z}_0$ denotes pre-treatment outcomes for the treatment unit and the control units

Y_{1t}^N, which is impossible in the treatmet period $t > T_0$. Abadie emphasizes, however, that Y_{1t}^N is observed in the pre-treatment period and therefore suggests splitting the pre-treatment data into a training period and a validation period, and use the validation period to assess the predictive power of the model fitted in the training period.

3.2 Synthetic Control Using Lasso

Kaul et al. (2015) highlight the difficulties in deciding which pre-treatment outcomes and covariates to use as predictors in $\mathbf{X_1}$ and $\mathbf{X_0}$ by theoretically proving that using the pre-treatment outcomes from all time periods in the pre-treatment period yields zero weight as the optimal weight given to covariates. In addition, they argue that using less pre-treatment outcomes and adding relevant covariates as predictors could yield different measures of the treatment effect, that are less biased. Greathouse (2022) furthermore raises concerns about the decisions of which years' pre-treatment outcomes and covariates to use as predictors typically being arbitrary, yet having an impact on the estimated treatment effect, and also highlights the risks of overfitting from letting ϵ_{it} affect the donor weights to a large extent. Hollingsworth and Wing (2022) also mention the issues with overfitting since the ϵ_{it} fluctuations in the pre-treatment period, captured by a too god fit between pre-treatment outcomes and the synthetic counterfactual in the pre-treatment period, will not persist in the treatment period. One way to deal with the issue is, according to Hollingsworth and Wing, to use a high number of pre-treatment periods.

As an alternative to the researcher making the decisions of which years' pre-treatment outcomes and covariates to use as predictors, synthetic control using lasso (SCUL) (Hollingsworth and Wing, 2022) limits the need for manual predictor selection by the researcher and allows for cross-validation in the pre-treatment period. Through SCUL, the weights in $\mathbf{W}$ given to the donors when constructing the synthetic control are, according to Hollingsworth and Wing, chosen using lasso regression, which is a penalized regression method. The authors thus find the optimal $\mathbf{W}^*$ from

$$\mathbf{W}^*_{\mathbf{lasso}} = \arg\min_{\mathbf{W}} \Big(\sum_{t=1}^{T_0} (Y_{1t} - \sum_{j=2}^{J+1} Y_{jt} w_j)^2 + \lambda |w|_1 \Big) \tag{3}$$

where λ is a penalty parameter and $|w|_1$ is the sum of absolute values of the weights w in $\mathbf{W}$. Since SCUL allows negative weights and weights that do not sum to one, $|w|_1$ is not necessarily equal to 1 as in the methods of Abadie and Gardeazabal (2003) and Abadie et al. (2010), which could cause extrapolation bias (Abadie, 2021). The size of λ affects the number of donors included in the synthetics control, a λ of zero would make Equation 3 identical to the OLS estimator while a large enough λ would give all donors zero weight in the synthetic control $\mathbf{W}^*$ (Hollingsworth and Wing). In SCUL, Hollingsworth and Wing decide the penalty parameter λ through cross-validation in the pre-treatment period by dividing the pre-treatment period into multiple pairs of training- and validation sets. Different penalty parameters from a grid of candidate values for λ are then used to compute $\mathbf{W}^*$ through Equation 3 from training data. For each pair k of training- and validation data, λ_k is the penalty parameter for which the corresponding $\mathbf{W}_k^*$ yields the smallest root mean square prediction error (RMSPE) in the validation data. Hollingsworth and Wing use the median of the different λ_k from the various pairs of training- and validation sets as their penalty parameter in SCUL.

The Stata package *scul*, initially developed for R by Hollingsworth and Wing (2022) and modified for Stata by Greathouse (2022), implements SCUL and divides the pre-treatment period using a rolling-origin h-step ahead algorithm, where the researcher specifies how many steps ahead of each training period the validation should be (Greathouse). Hollingsworth and Wing argue that SCUL is particularly favorable when the number of pre-treatment periods is low and the donor pool is large, specifically when the rule of thumb to not have a larger donor pool than the number of pre-treatment periods is violated.

3.3 Data Collection Methods

Collection of Data on Trade in Services

In this study, I use annual bilateral export data from the Balanced Trade in Services (BaTIS) dataset by OECD and WTO (2025), which contains exports and imports of services for 200 economies throughout the period 2005-2023. The dataset is a compilation of data collected by the OECD International Trade in Services statistics, Eurostat International Trade in Services statistics, UN

Comtrade and national main providers of balance of payments data (OECD, 2025b). According to the OECD, the amount of countries that keep statistics on trade in services is scarce, as only 67 economies publish bilateral trade in services statistics. To fill in gaps, the OECD and WTO have applied interpolation, backcasting and nowcasting for missing values in existing time series of trade flows and used a set of gravity models to predict the trade for the economies where no bilateral data exists at all. OECD (2025b) argue that the gravity specifications have been made as parsimonious as possible, e.g. by excluding policy variables, which should facilitate the use of the data for analysis of different policy interventions. However, as a precautionary measure, I have removed trade flows estimated through gravity regression from my analysis.

OECD and WTO (2025) have also made estimations to balance the gaps where reported export differs from the partner's reported import and vice versa. Since the scope of this study is to look at the export flows of the UK to Sweden and the EU, balanced import and export is not a requirement, while the assumptions made in the method to balance the trade flows could decrease the validity of the data. Therefore, I use the so-called *final value* reported in BaTIS, which contains figures reported by the official statistics agencies for each exporter, adjusted for comparability over years and between economies, as well as estimations of missing data (OECD, 2025b), and not the *balanced value*.

The data of the UK's export to Sweden in OECD and WTO (2025) BaTIS consists of the figures reported by Eurostat Quarterly Balance of Payments statistics for 2005-2009, by Eurostat International Trade in Services statistics for 2010-2015 and by the UK Office of National Statistics for 2016-2023, without any interpolation, backcasting or nowcasting (OECD, 2025b; OECD and WTO, 2025).

It is not feasible to look into the specific methodology of each national statistics agency that has its data included in the BaTIS but one worth mentioning is Eurostat (2025), which provides data of bilateral trade flows reported by the EU member states, its candidate countries, as well as Iceland, Norway and Switzerland. The methodology used by Eurostat is that of MSITS2010 (UN

Statistics, 2010) and BPM6 (IMF, 2013), and has been collected through national enterprise surveys, International Transaction System (ITRS) and administrative records. The Office of National Statistics (ONS, 2024b) in the UK provide trade in services data for the UK based on surveys such as their International Trade in Services Survey and their Financial Services Survey, and have also used the methodology of BPM6.

Collection of Data on Covariates

This study follows the methodology of Douch and Edwards (2021) by including exchange rates in terms of the exporter currency unit per importer currency unit, and the export of goods from the exporter to the importer, as predictors of trade in services in addition to the pre-treatment outcomes of export of services, when constructing the synthetic control. A cheaper exporter currency relative to that of the importer should according to trivial economic theory increase the demand for export and conversely a more expensive currency decreases the demand. The exchange rate should thus have a strong predictive power on the export of services and therefore be a suitable predictor for the synthetic counterfactual. Including data on goods exports as predictors of services export is also reasonable, considering the strong interlinkage between trade in services and trade in goods found by e.g. Ariu et al. (2019), Stremersch et al. (2001), Tien (2012). The GDPs of the exporting and importing economies were also included as predictors at first, as suggested by Douch and Edwards (2021), but they did not receive any non-zero weights when the optimal predictor weights matrix $\mathbf{V}^*$ was computed.

I have collected annual average exchange rates in local currency units per US$ from the World Bank (2025) World Development Indicators and divided the exporters' exchange rates by the importers' in each trade relationship, which yields the local currency units of the exporter per local currency unit of the importer. The goods export data are from UN Comtrade (2024), which contains data collected from official national agencies, and entities including Eurostat, the WTO and the OECD. The OECD's equivalent to the BaTIS for goods, the OECD Balanced International Merchandise Trade dataset (BIMTS), was unfortunately temporarily unavailable during the work of this study. Some data

are missing in UN Comtrade and this study therefore follows the methodology of OECD (2025b) for BaTIS by using linear interpolation when data for a particular unit are missing in single years. The data for the missing years are then estimated as the average of the previous and the following year. Units with missing data at two or more consecutive years have been dropped, implying slightly more conservative interpolation than in BaTIS.

Since the predictor weights in the $\mathbf{V}^*$ matrix are restricted to be non-negative and sum to one (Abadie and Gardeazabal, 2003; Abadie et al., 2010), the covariates have been rescaled to size similar to that of the services export, in order to account for the differences in units between the covariates. Each covariate variable has therefore been either divided or multiplied by a power of ten, in order for the median of each covariate to be of the same power of ten as the median services export in the donor pool[11]. My decisions on which years' pre-treatment outcomes and outcomes of the covariates to include are explained further in Subsection 3.5.

Abadie (2021) argues that the outcome variable should be rescaled when it scales with the size of the unit, which one could argue is the case for export of services, since larger economies should have a higher value of total exports than smaller economies. However, Abadie also mentions that for a counterfactual to be considered credible, reproducing the pre-treatment trend is not always sufficient if it does not also reproduce the level. Considering the large size of the donor pool for this study, compared to many other studies, it is deemed feasible to not rescale the outcome variable and thus impose a requirement for the synthetic control to not only replicate the trend of the treated variable, but also the level.

3.4 Sample Selection

Each combination of exporting country and its reported importing partner country constitutes a unit in the panel dataset and will be interchangeably referred to as unit or export flow. The treatment unit of the study is the export flow from the UK to Sweden. To comply with the suggestion by Abadie et al. (2015), I have only included units with characteristics similar to that of the treatment in the

[11]The values of goods trade have been divided by 10^6 and the exchange rates have been multiplied by 10^3

donor pool, which according to Abadie et al. is necessary to avoid over-fitting and enhance the validity of the counterfactual. The four sectors to which most of the UK's services exports to Sweden in both 2005-2015 and 2005-2020 belong are, in descending order, *Other Business Services* (SJ), *Financial Services* (SG), *Travel* (SD) and *Charges for the Use of Intellectual Property* (SH) (OECD and WTO, 2025). The first step in my sample selection for the donor pool is therefore to sort all units by total export value throughout the period 2005-2021, for each service sector. I then keep only the 1000 units closest to UK-Sweden with a higher export value and the 1000 closest units with a lower value, in each of the four sectors.

In the second step of the sample selection, I remove the units where either the reporter or the partner are not members of the Organization for Economic Cooperation and Development (OECD) and/or the EU. This in order to ensure similar economic characteristics and stages of development within the donor pool as well as between the donor pool and the treatment unit.

The third step of the sample selection is to remove units for which the export values have been estimated using gravity regression. Finally, all units where the UK is either the exporter or the importer, as well as units where Sweden is the importer, are removed in order to avoid violation of the stable unit treatment value assumption (SUTVA) (Cunningham, 2021). This yields 459 units in the donor pool, where all OECD and EU member states are included as both exporter and importer in at least one unit[12].

3.5 Data Analysis

Following the notation by Abadie et al. (2010), I define Y_{it} as the value of services export in trade flow i in year t, such that

$$Y_{it} = Y_{it}^{N} + \alpha_{it} D_{it}$$

where Y_{it}^{N} is the service export if the export flow is not affected by Brexit and D_{it} is a dummy variable that takes value 1 if export flow i is affected by Brexit in year t. The aim of this study is to estimate the

[12] A complete list of units in the donorpool is available upon request.

treatment effect from Brexit $\alpha_{UK-SE,t}$ at times $t > T_0$ where T_0 is the last year before the treatment. I analyze Brexit with $T_0 = 2015$, which was the year before the Brexit referendum, and $T_0 = 2020$, which was the year before the TCA entered into force. D_{it} is thus equal to 1 for all i that include the UK at times $t \geq 2016$ or $t \geq 2021$, depending on the analysis, and 0 otherwise.

To estimate $\alpha_{UK-SE,t}$ at times $t > T_0$, I use the estimator

$$\hat{\alpha}_{it} = Y_{it} - \sum_{j \neq i} w_j^* Y_{jt} \quad \text{for } i = UK - SE \quad t \geq 2016,\ 2021$$

proposed by Abadie et al. (2010), by finding the $(J \times 1)$ vector $\mathbf{W}^*$ for which

$$\sum_{j \neq i} w_j^* Y_{j1} = Y_{i1}, \quad \sum_{j \neq 1} w_j^* Y_{j2} = Y_{i2}, \quad \ldots, \quad \sum_{j \neq i} w_j^* Y_{jT_0} = Y_{iT_0} \text{ and } \sum_{j \neq i} w_j^* \mathbf{Z}_j = \mathbf{Z}_i$$

holds approximately for $i = UK - SE$[13].

In the donor pool described in Subsection 3.4, there are $J = 459$ control units, and in constructing the synthetic replica of the export flows from the UK to Sweden, each control unit is given a weight in the $(J \times 1)$ vector $\mathbf{W} = (w_2, ..., w_{j+1})'$, in accordance with the methodology of Abadie and Gardeazabal (2003) and Abadie et al. (2010). By defining $\mathbf{X}_1$ as a vector of predictors for the treatment unit and $\mathbf{X}_0$ as a matrix of the same predictors for the units in the donor pool, I find the optimal $\mathbf{W}^*$ by minimizing $\|\mathbf{X}_1 - \mathbf{X}_0\mathbf{W}\|$ where

$$\|\mathbf{X}_1 - \mathbf{X}_0\mathbf{W}\|\mathbf{V} = \sqrt{(\mathbf{X}_1 - \mathbf{X}_0\mathbf{W})'\mathbf{V}(\mathbf{X}_1 - \mathbf{X}_0\mathbf{W})}$$

using the *synth* package in Stata (Abadie et al., 2011), which implements the methodology of Abadie et al. (2010) and utilizes Stata's default maximization function[14]. $\mathbf{V}$ is choosen through the same data-driven process as in Abadie and Gardeazabal (2003) and Abadie et al. (2010), which is also implemented in the *synth* package.

[13] $\mathbf{Z}_j$ and $\mathbf{Z}_i$ are the matrix and vector of observed covariates, as in specification 1 in Subsection 3.1

[14] The *synth* package is used through the *synth2* extension by Yan and Chen (2023), which has enhanced tools for inference, robustness tests and visualization.

To handle the difficulties in deciding which predictors to include in $\mathbf{X}_1$ and $\mathbf{X}_0$ raised by e.g. Kaul et al. (2015), Greathouse (2022) and Hollingsworth and Wing (2022), limit the researcher degrees of freedom and keep the design blind to the outcomes, while also ensuring good pre-treatment fit, I analyze four different models; SCM using three different specifications for $\mathbf{X}_1$ and $\mathbf{X}_0$, as well as synthetic control using lasso (SCUL).

In the first model, referred to as the baseline model, the prioritized objective is to ensure as good pre-treatment fit between the synthetic control and the treatment unit as possible. $\mathbf{X}_1$ and $\mathbf{X}_0$ therefore contain all pre-treatment outcomes, i.e.

$$\mathbf{X}_1 = (Services_{UK-SE,2005}, Services_{UK-SE,2006}, \ldots, Services_{UK-SE,T_0})'$$

$$\mathbf{X}_0 = (\mathbf{Services_{j\neq UK-SE,2005}}, \mathbf{Services_{j\neq UK-SE,2006}}, \ldots, \mathbf{Services_{j\neq UK-SE,T_0}})'$$

where $Services_{j\neq UK-SE,t}$ is the vector of services exports for all J units in the donor pool in year t.

To comply with the suggestion by Abadie (2021) to assess the predictive power of the fitted model on Y_{it}^N, the prioritized objective of the two additional specifications is out of sample validity. For these models, the pre-treatment data is splitted into a training period and a validation period. Abadie does not suggest any specific sizes or proportions of the two periods, but rather emphasizes that it depends on application-specific factors. I somewhat arbitrarily apply the 80-20 principle by Pareto (1896) by splitting the data into a training period between 2005-2013 and a validation period between 2014-2015 when studying the effects of the Brexit referendum in 2016, and a training period 2005-2017 and validation period 2018-2020 when studying the TCA in 2021. The 80-20 principle conforms with what is commonly seen in much machine learning literature, where choices like 75-25 and 70-30 are also used frequently. With the low amount of available pre-treament periods in mind, I prioritize training periods that are as long as possible. The set of predictors for which I find the smallest deviation in the validation period are then used for the models. In the model referred to as validation model 1, only pre-treatment outcomes are used as predictors, while validation model 2

includes the covariates goods exports $Goods_{it}$ and exchange rates EX_{it} as well. Hence in validation model 1

$$\mathbf{X}_1 = (Services_{UK-SE,2008}, Services_{UK-SE,2009}, Services_{UK-SE,2013}, \overline{Services_{UK-SE,2005-2013}})'$$

$$\mathbf{X}_0 = (\mathbf{Services_{j\neq UK-SE,2008}}, \mathbf{Services_{j\neq UK-SE,2009}}, \mathbf{Services_{j\neq UK-SE,2013}}, \overline{\mathbf{Services_{j\neq UK-SE,2005-2013}}})'$$

when studying treatment at $t \geq 2016$ and

$$\mathbf{X}_1 = (Services_{UK-SE,2009}, Services_{UK-SE,2013}, Services_{UK-SE,2014}, Services_{UK-SE,2015},$$

$$Services_{UK-SE,2016}, Services_{UK-SE,2017}, \overline{Services_{UK-SE,2005-2017}})'$$

$$\mathbf{X}_0 = (\mathbf{Services_{j\neq UK-SE,2009}}, \mathbf{Services_{j\neq UK-SE,2013}}, \mathbf{Services_{j\neq UK-SE,2014}}, \mathbf{Services_{j\neq UK-SE,2015}},$$

$$\mathbf{Services_{j\neq UK-SE,2016}}, \mathbf{Services_{j\neq UK-SE,2017}}, \overline{\mathbf{Services_{j\neq UK-SE,2005-2017}}})'$$

when studying treatment at $t \geq 2021$.

In validation model 2

$$\mathbf{X}_1 = (Services_{UK-SE,2008}, Services_{UK-SE,2009}, Services_{UK-SE,2013}, \overline{Services_{UK-SE,2005-2013}},$$

$$Goods_{UK-SE,2009}, Goods_{UK-SE,2011}, Goods_{UK-SE,2012}, Goods_{UK-SE,2013},$$

$$\overline{Goods_{UK-SE,2005-2013}}, EX_{UK-SE,2009}, EX_{UK-SE,2010}, \ldots, EX_{UK-SE,2013})'$$

$$\mathbf{X}_0 = (\mathbf{Services_{j\neq UK-SE,2008}}, \mathbf{Services_{j\neq UK-SE,2009}}, \mathbf{Services_{j\neq UK-SE,2013}}, \overline{\mathbf{Services_{j\neq UK-SE,2005-2013}}},$$

$$\mathbf{Goods_{j\neq UK-SE,2009}}, \mathbf{Goods_{j\neq UK-SE,2011}}, \mathbf{Goods_{j\neq UK-SE,2012}}, \mathbf{Goods_{j\neq UK-SE,2013}},$$

$$\overline{\mathbf{Goods_{j\neq UK-SE,2005-2013}}}, \mathbf{EX_{j\neq UK-SE,2009}}, \mathbf{EX_{j\neq UK-SE,2010}}, \ldots, \mathbf{EX_{j\neq UK-SE,2013}})'$$

when studying treatment at $t \geq 2016$ and

$$\mathbf{X}_1 = (Services_{UK-SE,2009}, Services_{UK-SE,2013}, Services_{UK-SE,2016}, Services_{UK-SE,2017},$$

$$\overline{Services_{UK-SE,2005-2017}}, Goods_{UK-SE,2009}, Goods_{UK-SE,2011}, Goods_{UK-SE,2015}, Goods_{UK-SE,2017},$$

$$\overline{Goods_{UK-SE,2005-2017}}, EX_{UK-SE,2013}, EX_{UK-SE,2014}, EX_{UK-SE,2015}, EX_{UK-SE,2017})'$$

$$\mathbf{X}_0 = (\mathbf{Services_{j\neq UK-SE,2009}}, \mathbf{Services_{j\neq UK-SE,2013}}, \mathbf{Services_{j\neq UK-SE,2016}}, \mathbf{Services_{j\neq UK-SE,2017}},$$

$$\overline{\mathbf{Services_{j\neq UK-SE,2005-2017}}}, \mathbf{Goods_{j\neq UK-SE,2009}}, \mathbf{Goods_{j\neq UK-SE,2011}}, \mathbf{Goods_{j\neq UK-SE,2015}},$$

$$\mathbf{Goods_{j\neq UK-SE,2017}}, \overline{\mathbf{Goods_{j\neq UK-SE,2005-2017}}}, \mathbf{EX_{j\neq UK-SE,2013}}, \mathbf{EX_{j\neq UK-SE,2014}}, \mathbf{EX_{j\neq UK-SE,2015}},$$

$$\mathbf{EX_{j\neq UK-SE,2017}})'$$

when studying treatment at $t \geq 2021$.

In addition, the validation period will suffice as an in-time placebo test, as proposed by Abadie et al. (2015), to find out whether significant shocks occurred already in the years prior to the Brexit referendum and the TCA. This furthermore makes the study more compliant with the suggestion by Douch and Edwards (2022) to study effects from the Brexit referendum from the beginning of 2015.

In the fourth model, referred to as the lasso model, I implement the synthetic control method using lasso by Hollingsworth and Wing (2022), using the Stata package *Scul* by Greathouse (2022). Hence the results of the lasso model are fully independent of researcher design decisions, other than which units to include in the donor pool and how many steps ahead to make the predictions. To continue to comply with the 80-20 rule for the two validation models, this study uses 3-step ahead predictions when studying treatment from the TCA in 2021 and 2-step ahead predictions when studying the referendum in 2016. The lasso model should also help the study overcome some validity issues from the low amount of pre-treatment periods (Hollingsworth and Wing, 2022).

3.6 Statistical Inference and Robustness Tests

For statistical inference, I apply the methodology of Abadie et al. (2015) by iteratively running placebo analyses in which each unit from the donor pool is studied as if it was the treatment unit. Through the distribution of placebo treatment effects, I then evaluate the significance of the estimated treatment effects on the UK's exports to Sweden. Due to constraints in computing capacity, roughly half of the units in the donor pool[15] are included in the sample of placebo units. Another consequence of constraints in computing capacity is that the complex fitting algorithm for the lasso model makes it impossible to conduct placebo tests for that model. It is therefore not possible to determine the p-values of the results from the lasso model. A 95% confidence interval for the synthetic counterfactual is provided, but since the treatment has not been randomly assigned, the confidence interval does not have a straight-forward interpretation (Galiani and Quistorff 2017; Abadie et al. 2015).

For robustness test, I apply the leave-one-out analysis proposed Abadie et al. (2015), where each of the units given positive weight in the initial analysis are excluded from the analysis one at a time. By doing so, it is possible to find out whether a possible increase or decrease in services export relative to the synthetic control is highly driven by an increase or decrease relative to one single other trade flow. It is not very plausible that an exogenous trade relationship perfectly replicates that of the UK and Sweden, so results highly driven by a single control unit should be considered less valid. To compute the p-values and do the leave-on-out analyzes, I use the *synth2* package for Stata by Yan and Chen (2023), which is an extension of the *synth* package by Abadie et al. (2011).

As an additional robustness test, I conduct analyzes by service sector in order to find out whether the results are highly driven by effects in one specific sector. The four services sectors in which UK's export to Sweden was of the highest value in both 2005-2015 and 2005-2020 are *Other business services* (SJ), *Financial services* (SG), *Travel services* (SD) and *Charges for the use of intellectual property, not included elsewhere* (SH), and these four sectors are therefore considered in the sector analysis.

[15] Units with even id-numbers and all units with positive weights in the synthetic counterfactual for the UK's export to Sweden.

From the sample selection follows a difficult trade-off between the stable unit treatment value assumption (SUTVA) and similarity between the donor pool and the treatment unit. The units in the donor pool have to be untreated, i.e. no other trade flows than UK-Sweden should be affected by Brexit, in order for the SUTVA to hold (Cunningham, 2021). Vandenbussche et al. (2022) find that Brexit has had significant effects on the EU member states and Douch and Edwards (2022) therefore limit their donor pool to trade flows that do not include any EU member states. Douch and Edwards (2021) include EU member states in their study, but conduct robustness checks where five major service exporters in the EU; Germany, France, Ireland, Spain and Italy are excluded, as well as a robustness test without any EU members in the donor pool. When doing so, their broad results still hold, although the observed effect is somewhat smaller when excluding all EU members from the donor pool.

A plausible consequence of Brexit is that the EU member states shift from UK suppliers to suppliers within the EU single market and that intra-EU trade has therefore increased. This is supported by the findings of Buigut and Kapar (2023), that both the referendum and the TCA led to increases in intra-EU trade. If that is the case, including intra-EU trade flows in the donor pool would bias the synthetic counterfactual upwards and lead to an overestimation of the gap between the actual and synthetic outcomes under treatment. Since Iceland and Norway have access to the EU single market through the European Economic Agreement (EEA) and Switzerland is part of the Schengen agreement, which limits the border controls between Switzerland and most EU countries, the third robustness test excludes intra-EEA and Switzerland trade, i.e. between the EU member states, Iceland, Norway and Switzerland, from the donor pool. Since no other European countries were included in the initial sample selection, this implies exclusion of all intra-Europe trade. I also test the robustness to an even stricter SUTVA by excluding all trade flows that have either a European importer, exporter or both, which is in line with the methodology of Douch and Edwards (2022) for goods.

3.7 Alternative Methods

The synthetic control method is one among multiple econometric methods for comparative case studies. A widely and long-time used method is the difference-in-differences (DiD) method, where one or multiple treatment units are compared to one or multiple control units before and after treatment, typically under control for unit- and time fixed effects, as well as other variables affecting the outcome (Cunningham, 2021).

As shown by Freeman et al. (2024), it is possible to use DiD to study the effects on export from Brexit, but it has a few drawbacks. The key assumption for DiD to be valid is the parallel trends assumption (Cunningham, 2021), which requires the outcome variable of the treatment unit(s) to follow a trend parallel to that of the control unit(s), had the treatment unit not been treated. That is a counterfactual case and hence not testable, instead the assumption requires justification from economic intuition to be valid, and for the assumption to hold, there cannot be any unobserved variables that differ between units and vary with time (Cunningham). Freeman et al. (2024) solve this by controlling for a large set of variables that possibly vary both between units and with time, such as foreign supply and demand shocks, and fluctuations in real exchange rates. One could however argue that there are additional variables, that Freeman et al. have not managed to control for. An important variable is the effects from the Covid-19 pandemic between 2020-2023, which is a highly plausible explanation for the large drop in exports observed for many countries in 2020, as it led to disruptions of supply chains, travel restrictions and global recession. Covid-19 hit different countries differently, at different times, and the measures to prevent the spread of the virus furthermore differed between countries (Yang et al., 2020). That is something a DiD model like that by Freeman et al. (2024) cannot control for unless the differences between countries an the time-varying intensities are observed (Abadie et al., 2010).

Specification 1 for the synthetic control method in Subsection 3.1 includes the term $\lambda_t \mu_i$, which captures unobserved factors that vary between units and affect the outcome differently at different times. In Subsection 3.1, it is explained that Abadie et al. (2010) prove that $\lambda_t \mu_i$ does not bias

the estimated treatment effect when conditions 2 hold approximately and the amount of pre-treatment periods are sufficiently large. Hence SCM should be a better method to control for time-variant, unit specific factors affecting the export of services, such as Covid-19, than DiD. Considering the complexity of individual countries and that the total number of countries existing is relatively small compared to many other types of entities, it is furthermore plausible that many other unobserved time-variant variables differ between the countries, and that no perfect replica of the UK's trade relationship with Sweden, for which the parallel trends assumption holds, exists. Supported by the recommendation of Abadie (2021) to use SCM to study aggregate outcomes of interventions in large units like countries, implemented at an aggregate level, SCM should be a suitable method to study the effects of Brexit on the UK's exports.

Arkhangelsky et al. (2021) present an alternative method called synthetic difference-in-differences (SDiD), by which the authors solve the difficulties in justifying parallel trends in DiD by giving the control units weights that ensure parallel pre-treatment trends, similar to SCM. While time- and unit fixed effects are implicitly controlled for when Conditions 2, Subsection 3.1 hold, the authors claim that an advantage of SDiD over SCM is that SDiD, like DiD, explicitly controls for time- and unit fixed effects, which is beneficial since Conditions 2 barely ever hold exactly in practice. The SDiD method has interesting properties that most likely could improve the methodology of this study. However, considering that SDiD is a much more recently presented method than SCM, and that the literature on previous studies using SDiD therefore is much more scarce than for SCM, this study applies the more established SCM. Du et al. (2023) and Du and Shepotylo (2024) compare the SDiD method with the SCM in their studies and find smaller effects from SCM than from SDiD for services and similar effects for goods, which suggests that the results of this study are more conservative [16].

[16] A simple SDiD estimate without covariates to the research problem of my study, using the Stata package *sdid* (Clarke et al., 2024), yields an average annual export loss of -2,440 US$ million after the referendum, which is within the range of my SCM estimates in Section 4. The annual export loss from the TCA estimated using SDiD is -374 US$ million, which is a smaller average loss than in any of my SCM estimates. These results do not affect my general conclusion.

4 Results

In this section, the results of the four different models described in Subsection 3.5 are presented. Subsection 4.1 covers the results from studying the Brexit referendum in 2016 as treatment and Subsection 4.2 from studying the implementation of the TCA agreement in 2021 as treatment.

4.1 Treatment 2016: Brexit Referendum

Table 2 contains the optimal weights assigned to each unit in the donor pool that has received a non-zero[17] weight in the vector $\mathbf{W}^*$ when analyzing the effects of the Brexit referendum in 2016. Export of services from France to Luxembourg is of high relevance in both the baseline model and the two validation models. The two validation models also include the export from Italy to France and from Finland to Denmark, while the baseline model differs somewhat, indicating that the baseline model could be overfitted.

The lasso model takes advantage of the relaxed condition $\sum_{i \neq UK-SE} w_i = 1$ by assigning a weight of 17.350 to Luxembourg-Mexico. Considering Luxembourg's high concentration on financial services in its export to Mexico and in general, and financial services being among the top exports of the UK to Sweden as well (OECD and WTO, 2025), it is plausible that the fluctuations in Luxembourg's export to Mexico constitute a valid counterfactual to the fluctuations in the UK's export to Sweden. However, the much smaller size of the Luxembourgian economy compared to the British makes it difficult for Luxembourg to reach the level of an export flow of the UK, which is why its exports need to be scaled up to be a fully valid counterfactual. It is important to keep in mind that this makes the counterfactual export under the lasso model a result of extrapolation (Abadie, 2021), which could make it less valid.

[17] Synthetic control using lasso (Hollingsworth and Wing, 2022), as opposed to the SCM by Abadie et al. (2010), allows negative weights although no negative weights were assigned in this optimization.

Trade Flow	**Optimal weight w^* in vector $\mathbf{W}^*$**			
	Baseline	Validation 1	Validation 2	Lasso
Denmark - Germany*	0.5340			0.262
France - Luxembourg*	0.2560	0.4880	0.6160	
France - Denmark*	0.1230			
Ireland - USA*	0.0430			
Greece - USA*	0.0380		0.1420	
Netherlands - Ireland	0.0050			
Italy - France*		0.2810	0.1140	
Finland - Denmark*		0.2310	0.1280	
Luxembourg - Mexico*				17.350
Finland - Canada*				0.214
Greece - Canada*				0.134
Ireland - Poland*				0.342
Sweden - Denmark				0.181

*Time series contains at least one interpolated value.

Table 2: Optimal Unit Weights in the Vector $\mathbf{W}^*$ for Synthetic Export from the UK to Sweden in the Models with Treatment 2016

The optimal weights in matrix $\mathbf{V}^*$ assigned to each predictor of services export are presented in Table 3. By construction, the lasso model does not assign any weights to predictors and that column is therefore empty. When including all pre-treatment outcomes as predictors, as in the baseline model, higher weights are assigned to more recent pre-treatment outcomes than the earlier ones. That is plausible considering that a counterfactual UK-Sweden trade relationship post 2016 should be more similar to the relationship of 2010-2015 than 2005-2009. However, as mentioned by Douch and Edwards (2022), anticipation effects were seen already from the beginning of 2015, and the anticipation could go even further back in time before the referendum. Hence it is possible that the services exports should be considered treated already in 2014-2015, and that high weights to these years' outcomes bias the results.

The covariates added as predictors to the validation model 2 do not have a major impact on the model fit since they are assigned relatively small weights. Together with the average services export in the training period, however, they do reduce the pre-treatment root mean squared prediction error (RMSE) from 455 in validation model 1 without covariates, to 416 in validation model 2 with covariates. The pre-treatment RMSPE of 363 for the lasso model suggests some overfitting of the baseline model, which has an RMSPE of 216, and underfitting of the two validation models.

Predictor	**Variable**	**Optimal weight v^* in matrix $\mathbf{V}^*$**			
		Baseline	Validation 1	Validation 2	Lasso
Services Export, 2005	$Services_{2005}$	0.0483			
Services Export, 2006	$Services_{2006}$	0.0539			
Services Export, 2007	$Services_{2007}$	0.0675			
Services Export, 2008	$Services_{2008}$	0.0824	0.4012	0.0605	
Services Export, 2009	$Services_{2009}$	0.0714	0.2316	0.0800	
Services Export, 2010	$Services_{2010}$	0.0886			
Services Export, 2011	$Services_{2011}$	0.1069			
Services Export, 2012	$Services_{2012}$	0.1131			
Services Export, 2013	$Services_{2013}$	0.1198	0.3672	0.1441	
Services Export, 2014	$Services_{2014}$	0.1308			
Services Export, 2015	$Services_{2015}$	0.1174			
Avg. Services Export, 2005-2013	$\overline{Services_{2005-2013}}$			0.5942	
Goods Export, 2009	$Goods_{2009}$			0.0064	
Goods Export, 2011	$Goods_{2011}$			0.0436	
Goods Export, 2012	$Goods_{2015}$			0.0121	
Goods Export, 2013	$Goods_{2017}$			0.0113	
Avg. Goods Export, 2005-2013	$\overline{Goods_{2005-2017}}$			0.0031	
Currency Exchange Rate, 2009	EX_{2009}			0.0013	
Currency Exchange Rate, 2010	EX_{2010}			0.0006	
Currency Exchange Rate, 2011	EX_{2011}			0.0297	
Currency Exchange Rate, 2012	EX_{2009}			0.0076	
Currency Exchange Rate, 2013	EX_{2009}			0.0057	
R^2		0.96	0.82	0.83	0.91
Pre-Treatment RMSPE		216	455	416	363
Post/Pre RMSPE Ratio		14.40	5.67	5.47	8.09

Note: The R^2 is for the treatment period 2016-2023, the root mean squared prediction errors (RMSPE) are for the pre-treatment period 2005-2015 and the (post-)treatment period 2016-2023.

Table 3: Optimal Predictor Weights in the Diagonal of the Matrix $\mathbf{V}^*$ for Synthetic Export from the UK to Sweden in the Models with Treatment 2016

Figures 2, 5, 7 and 9 all provide comparisons between the actual services exports from the UK to Sweden and the synthetic counterfactuals, for each of the four models, while Figures 3, 6, 8 and 10 illustrate the treatment effects, which are the difference between actual and synthetic outcome of each model. All models have lower actual services export from the UK to Sweden relative to the synthetic counterfactual in each year from the referendum in 2016. Not surprisingly, the baseline model in Figures 2 and 3 best fits the actual pre-treatment outcomes to the synthetic counterfactual. As stated in Table 3, it also has the highest post-treatment RMSPE to pre-treatment RMSPE ratio, 14.40, making it the model that best indicates a causal impact from the referendum.



Figure 2: Baseline Model, Actual and Synthetic Export of Services from the UK to Sweden (US$ million) Under Treatment 2016 and Training Period Until 2015

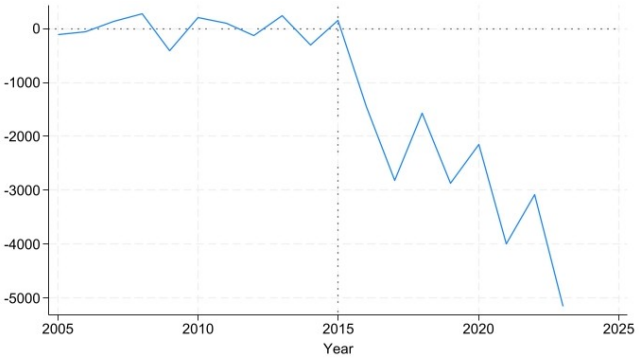


Figure 3: Baseline Model, Treatment Effect on Export of Services from the UK to Sweden (US$ million) under Treatment 2016 and Training Period Until 2015

When the training period of the baseline model is shortened to end in 2013, the synthetic counterfactual still follows the same trend as the actual outcome, as seen in Figure 4, contradicting the suggestion that the baseline model is overfitted.

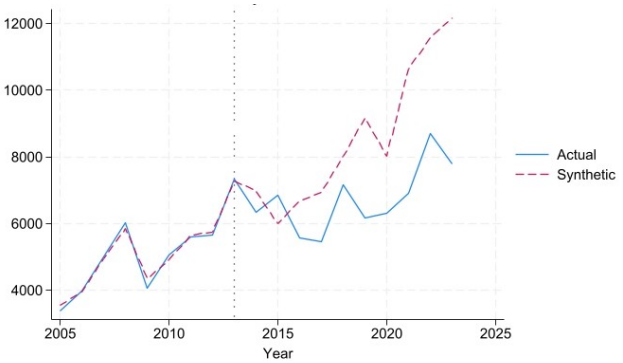


Figure 4: Baseline Model, Actual and Synthetic Export of Services from the UK to Sweden (US$ million) Under Treatment 2016 and Training Period Until 2013

Under the lower number of predictors in validation model 1, Figures 5 and 6, the fit in 2014-

2015 is only improved slightly compared to Figure 4, with a decrease in the average estimated placebo treatment effect for 2014-2015 from 1.71 percent in the baseline model to -1.60 percent in validation model 1, at the cost of a higher pre-treatment RMSPE of 455. The impact of the referendum is furthermore less clear in validation model 1, as seen in Figure 6, with a Post-/Pre-treatment RMSPE ratio of 5.67.

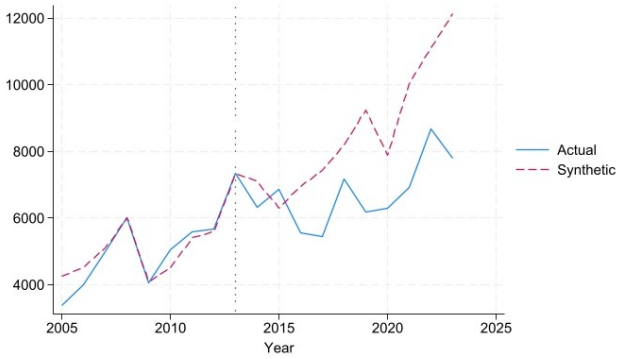


Figure 5: Validation Model 1 (no covariates), Actual and Synthetic Export of Services from the UK to Sweden (US$ million) Under Treatment 2016 and Training Period Until 2013

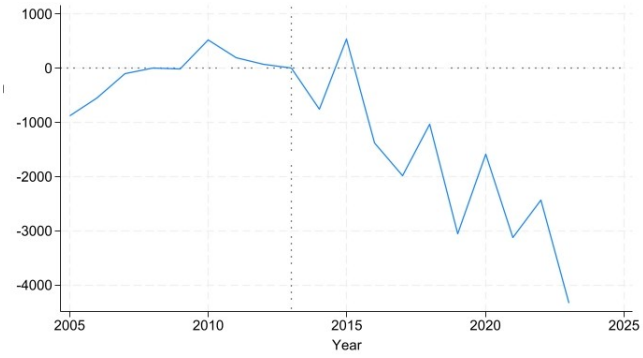


Figure 6: Validation Model 1 (no covariates), Treatment Effect on Export of Services from the UK to Sweden (US$ million) under Treatment 2016 and Training Period Until 2013

Adding the additional covariates in validation model 2, Figure 7 and 8, helps improve the fit in the pre-treatment period to an RMSPE of 416 but increases the estimated placebo treatment effect in 2014-2015 to 3.83 percent and decreases the Post-/Pre-treatment RMSPE ratio to 5.47. The fluctuations in the treatment effect in the treatment period are similar to validation model 1, but at a slightly higher level.

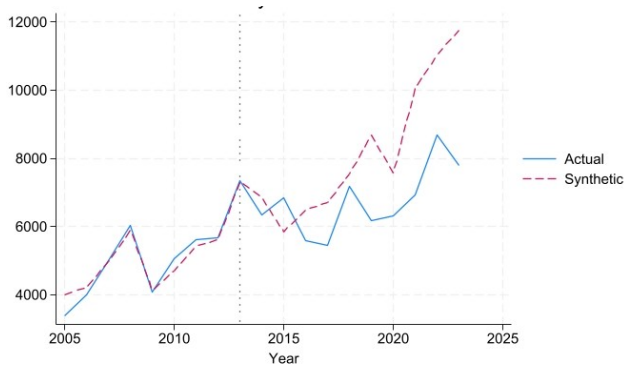
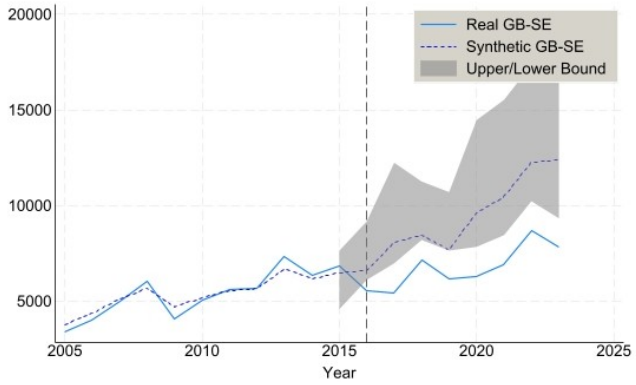


Figure 7: Validation Model 2 (with covariates), Actual and Synthetic Export of Services from the UK to Sweden (US$ million) Under Treatment 2016 and Training Period Until 2013



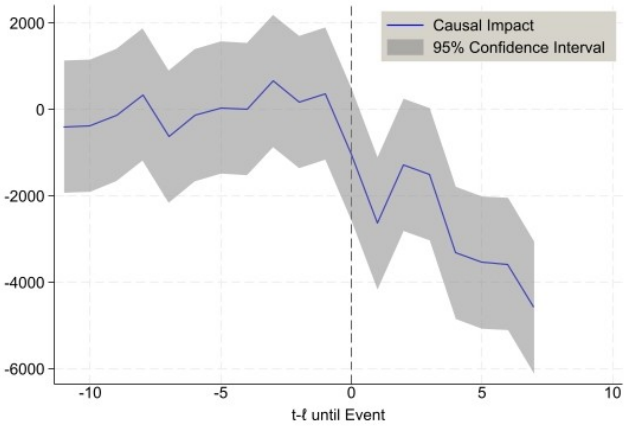
Figure 8: Validation Model 2 (with covariates), Treatment Effect on Export of Services from the UK to Sweden (US$ million) under Treatment 2016 and Training Period Until 2013

The lasso model, Figures 9 and 10, is an improvement from the two validation models in terms of pre-treatment fit, with an RMSPE of 363 in the pre-treatment period, and illustrates a clearer distinction in treatment effect before and after the referendum, with a Post-/Pre-treatment ratio of 8.09. Although the 95% confidence interval should be interpreted with some caution since the treatment has not been randomly assigned (Abadie et al., 2015; Galiani and Quistorff, 2017), it is positive for the significance of the estimated treatment effect that its 95% confidence interval is below zero in all years from the referendum except 2018 and 2019.



Note: The vertical line indicates the start of the treatment (2016), not the end of the training period as is the case for the other models.

Figure 9: Lasso Model, Actual and Synthetic Export of Services from the UK to Sweden (US$ million) Under Treatment 2016 and 2-Step Ahead Predictions



Note: The vertical line indicates the start of the treatment (2016), not the end of the training period as is the case for the other models.

Figure 10: Lasso Model, Treatment Effect on Export of Services from the UK to Sweden (US$ million) under Treatment 2016 and and 2-Step Ahead Predictions

The treatment effects in US$ million and percent, as well as the p-values of the treatment effects, estimated through placebo analyses, are provided in Tables 4, 5, 6 and 7. The two-sided p-value is based on absolute deviation from zero treatment effect and the one-sided takes into consideration whether the effect is positive or negative. For example, in the baseline model, Table 4, the probability of estimating a decrease larger than or equal to 20.71 percent is 0.0129 while the probability of estimating a decrease or an increase greater than or equal to 20.71 percent is 0.0345.

All models yield annual losses of 20 to 40 percent in export from the UK to Sweden, relative to the synthetic counterfactual, in most years from the referendum in 2016, with a few exceptions

where the losses are somewhat smaller. The 20.71 percent loss in 2016 and the 34.10 percent loss in 2017, estimated from the baseline model, Table 4, are the only losses with two-sided significance of $p < 0.05$ and one-sided significance of $p < 0.02$, indicating a strong initial reaction to the outcome of the referendum.

Time	**Treatment Effect (US$ M.)**	**Treatment Effect (%)**	**P-value of Treatment Effect**	
			Two-sided	One-sided
2016	-1,456	-20.71%	0.0345	0.0129
2017	-2,817	-34.10%	0.0302	0.0172
2018	-1,579	-18.05%	0.1121	0.0603
2019	-2,876	-31.75%	0.0905	0.0560
2020	-2,141	-25.36%	0.1336	0.0862
2021	-3,993	-36.61%	0.0862	0.0517
2022	-3,087	-26.21%	0.1293	0.0733
2023	-5,149	-39.78%	0.0776	0.0388
Mean	-2,887	-29.93%		

Table 4: Baseline Model, Treatment Effect on Export of Services from the UK to Sweden in the Treatment Period from 2016, with Training Period Until 2015 and p-values based on 231 Placebo Analyses

In validation model 1, Table 5, the losses in 2016-2017 are similar but somewhat smaller in size, and have a one-sided significance of $p < 0.05$.

Time	**Treatment Effect (US$ M.)**	**Treatment Effect (%)**	**P-value of Treatment Effect**	
			Two-sided	One-sided
2014	*-762*	*-10.74%*	*0.1602*	*0.0390*
2015	*547*	*8.67%*	*0.2294*	*0.1602*
Mean	*-108*	*-1.60%*		
2016	-1,380	-19.85%	0.1082	0.0476
2017	-1,992	-26.78%	0.1039	0.0433
2018	-1,034	-12.61%	0.2208	0.0779
2019	-3,064	-33.14%	0.1039	0.0390
2020	-1,584	-20.08%	0.1991	0.0563
2021	-3,121	-31.10%	0.1429	0.0476
2022	-2,427	-21.83%	0.1602	0.0433
2023	-4,332	-35.72%	0.1082	0.0260
Mean	-2,367	-25.93%		

Table 5: Validation Model 1 (no covariates), Treatment Effect on Export of Services from the UK to Sweden in the Treatment Period from 2016, with Training Period Until 2013 and p-values based on 231 Placebo Analyses

For validation model 2, Table 6, the losses in 2016-2017 are smaller and not significant. A

potential explanation could be that the estimated synthetic counterfactual in validation model 2 lies at a somewhat lower level than the real counterfactual, which is indicated from the high placebo treatment effect of 17.16 percent estimated in 2015 from validation model 2. However, the treatment effects in the validation period are not significant in validation model 2, as opposed to in model 1 where the estimated placebo treatment effect of -10.74 percent in 2014 has a significance of $p = 0.0390$. Thus an alternative explanation is that the synthetic counterfactual in the baseline model and validation model 1 is at a too high level, leading to an overestimation of the losses, or that anticipation of Brexit actually affected the export already in 2014-2015.

Time	**Treatment Effect (US$ M.)**	**Treatment Effect (%)**	**P-value of Treatment Effect**	
			Two-sided	One-sided
2014	*-518*	*-7.56%*	*0.1739*	*0.0696*
2015	*1004*	*17.16%*	*0.1174*	*0.0652*
Mean	*243*	*3.83%*		
2016	-924	-14.22%	0.1174	0.0522
2017	-1,258	-18.77%	0.1348	0.0609
2018	-372	-4.94%	0.4000	0.2130
2019	-2,491	-28.72%	0.0957	0.0522
2020	-1,266	-16.73%	0.2130	0.1000
2021	-3,134	-31.19%	0.1130	0.0435
2022	-2,338	-21.20%	0.1522	0.0478
2023	-3,968	-33.73%	0.1174	0.0391
Mean	-1,738	-22.56%		

Table 6: Validation Model 2 (with covariates), Treatment Effect on Export of Services from the UK to Sweden in the Treatment Period from 2016, with Training Period Until 2013 and p-values based on 231 Placebo Analyses

The estimated losses in the lasso model, Table 7, are similar to the baseline model, indicating that the baseline estimates are valid.

Time	Treatment Effect (US$ M.)	Treatment Effect (%)
2014	*177*	*2.88%*
2015	*374*	*5.77%*
Mean	*275*	*4.37%*
2016	-1,031	-15.61%
2017	-2,630	-32.57%
2018	-1,276	-15.11%
2019	-1,493	-19.45%
2020	-3,314	-34.45%
2021	-3,538	-33.85%
2022	-3,569	-29.11%
2023	-4,582	-37.02%
Mean	-2,679	-28.39%

Note: Due to constraints in computational capacity, I am unable to provide p-values for the lasso model.

Table 7: Lasso Model, Treatment Effect on Export of Services from the UK to Sweden in the Treatment Period from 2016, with 2-Step Ahead Predictions

The decrease in actual services export from the UK to Sweden in 2016-2017 is followed by a sharp increase in 2018 and an upward trend throughout the remainder of the period for which data are available, although it remains below the synthetic counterfactual export all the time in all four models. However, the recoil in 2018 is so strong that the gap between the actual and counterfactual services exported in 2018 is not significant in any of the models, and in validation model 2, Table 6, the treatment effect is smaller than 5 percent. This could be an indication that the initial reaction in 2016-2017 was due to an overestimation of the negative impact that the Brexit referendum would lead to and a consequence of firms preparing for the worst-case scenario of a hard Brexit. For instance, President of the European Council Donald Tusk declared in October 2016 that a "hard Brexit" was the only option (European Commission, 2025). In 2018, the outlook for a soft Brexit was more positive since the first draft of the withdrawal agreement was presented on February 28, 2018 and President Tusk declared that he saw no other option than a free trade agreement between the UK and EU on March 8, 2018 (European Commission, 2025), which possibly could have helped the export recover.

The treatment effect and its significance continue to fluctuate in 2019-2020, most noticeably in validation model 2, Table 6. A possible explanation could be the multiple extensions of the deadline for Brexit in 2019 and the ratification of the TCA in 2020 (European Commission, 2025). From the

implementation of the TCA in 2021 until the end of the data in 2023, the treatment effect remains high and has a one-sided significance of $p < 0.05$ for all years in the two validation models, Tables 5 and 6. 2023 is furthermore the year with largest treatment effect in all four models, between -33.73 and -39.78 percent, and the only year with a one-sided significance of $p < 0.05$ in the three models with p-values. This suggests a small additional reaction when the uncertainty effects are no longer present and the market just has to react to present barriers to trade. Graphs with comparisons of the estimated treatment effects of each model with placebo effects estimated for each placebo unit are provided in Figures 27, 28 and 29 in the appendix, Subsection Additional Treatment- and Placebo Effect Figures.

4.2 Treatment 2021: TCA Agreement

Table 8 contains the optimal weights in matrix $\mathbf{W}^*$ assigned to each unit in the donor pool that has received a non-zero weight in the analysis of the TCA agreement in 2021. For the lasso model, the optimization algorithm reached its limit value for the penalty parameter λ, 984, giving all units of the donor pool zero weight. For the lasso model in Subsection 4.1, the λ value was 309. Minimizing the root mean squared prediction error in the cross-validation for 2005-2020 hence requires a very high penalty parameter, implying that a close matching of the actual data and the synthetic counterfactual in the pre-treatment period has low predictive power on future outcomes and would require overfitting. The lasso model is therefore not suitable for analysis of the TCA in 2021. This is also a strong indication that no synthetic counterfactual replicates the UK's export to Sweden in both 2005-2015 and 2016-2020, which supports the effects from the referendum found in Subsection 4.1.

There is a slightly higher number of units that have received a non-zero weight in these models than when analyzing the effects from the referendum in Subsection 4.1. This is an indication that fewer donors match the export from the UK to Sweden in the period 2005-2020 than 2005-2015, and a higher number of units used to construct the synthetic counterfactual could also be a sign of overfitting. Important to keep in mind is, however, that the longer the pre-treatment period, the more difficult it should intuitively be to find matching donors. Sweden-Germany plays an important role

in the synthetic counterfactual in both the baseline model and validation model 1. Greece-USA is of some importance for both the baseline model and validation model 2, which was also the case under treatment 2016. The commonality between the two validation models is much lower under treatment from 2021 than from 2016, with France-Denmark being the only unit given a large weight in both models.

Trade Path	**Optimal weight w^* in vector $\mathbf{W}^*$**		
	Baseline	Validation 1	Validation 2
Sweden - Germany	0.6650	0.4910	
Ireland - Israel*	0.1060		
Luxembourg - Belgium*	0.0850		
Greece - USA*	0.0590		0.1670
Poland - Germany	0.0390		
Denmark - Germany*	0.0280		
USA - Canada	0.0180		
France - Denmark*		0.2840	0.3930
France - Ireland*		0.1050	
Korea - USA		0.0900	
USA - Japan		0.0190	
France - Germany*		0.0120	0.0660
France - Luxembourg*			0.3190
Sweden - USA			0.0560

*Time series contains at least one interpolated value

Table 8: Optimal Unit Weights in the Vector $\mathbf{W}^*$ for Synthetic Export from the UK to Sweden in the Models with Treatment 2021

The optimal weights assigned to each predictor of services export included in each model under treatment 2021 are presented in Table 9. As suspected from the poor performance of the lasso model, the pre-treatment fit is poorer, with a root mean squared prediction error (RMSPE) of 370 for the baseline model, which is higher than for both the baseline model and the lasso model under treatment 2016. Ignoring the difficulties in ensuring good pre-treatment fit when the number of pre-treatment periods is higher, mentioned earlier, the poor pre-treatment fit could support the results of the models under treatment 2016.

Similarly to the baseline model in Subsection 4.1, this baseline model assigns the highest weights to the most recent pre-treatment outcomes, which is plausible considering that good fit in 2005-2015 has been shown to correspond to large differences throughout 2016-2023. For the same

reason, it is necessary to include all of the most recent pre-treatment outcomes in the training period when constructing validation model 1, to ensure proper fit in the validation period. The covariates included as predictors in validation model 2 are assigned even smaller weights than the covariates in validation model 2 for 2016 and do not improve the RMSPE to validation model 1, while the average estimated placebo treatment effects in the validation period are of the same size (+/- 0.77 percent) for both validation models.

Predictor	**Variable**	**Optimal weight v^* in matrix $\mathbf{V}^*$**		
		Baseline	Validation 1	Validation 2
Services Export, 2005	$Services_{2005}$	0.0275		
Services Export, 2006	$Services_{2006}$	0.0307		
Services Export, 2007	$Services_{2007}$	0.0384		
Services Export, 2008	$Services_{2008}$	0.0469		
Services Export, 2009	$Services_{2009}$	0.0407	0.0540	0.0472
Services Export, 2010	$Services_{2010}$	0.0505		
Services Export, 2011	$Services_{2011}$	0.0608		
Services Export, 2012	$Services_{2012}$	0.0643		
Services Export, 2013	$Services_{2013}$	0.0680	0.0915	0.1079
Services Export, 2014	$Services_{2014}$	0.0743	0.0413	
Services Export, 2015	$Services_{2015}$	0.0663	0.0288	
Services Export, 2016	$Services_{2016}$	0.0726	0.0495	0.0861
Services Export, 2017	$Services_{2017}$	0.0833	0.0366	0.0416
Services Export, 2018	$Services_{2018}$	0.0919		
Services Export, 2019	$Services_{2019}$	0.1062		
Services Export, 2020	$Services_{2020}$	0.0776		
Avg. Services Export, 2005-2017	$\overline{Services_{2005-2017}}$		0.6983	0.6842
Goods Export, 2009	$Goods_{2009}$			0.0012
Goods Export, 2011	$Goods_{2011}$			0.0023
Goods Export, 2015	$Goods_{2015}$			0.0053
Goods Export, 2017	$Goods_{2017}$			0.0055
Avg. Goods Export, 2005-2017	$\overline{Goods_{2005-2017}}$			0.0069
Currency Exchange Rate, 2013	EX_{2013}			0.0028
Currency Exchange Rate, 2014	EX_{2014}			0.0042
Currency Exchange Rate, 2015	EX_{2015}			0.0020
Currency Exchange Rate, 2017	EX_{2017}			0.0026
R^2		0.84	0.79	0.73
Pre-Treatment RMSPE		370	498	500
Post/Pre RMSPE Ratio		2.81	2.44	2.74

Note: The R^2 is for the treatment period 2021-2023, the root mean squared prediction errors (RMSPE) are for the pre-treatment period 2005-2020 and the (post-)treatment period 2021-2023.

Table 9: Optimal Predictor Weights in the Diagonal of the Matrix **V** for Synthetic Export from the UK to Sweden in the Models with Treatment 2021

Figures 11, 14 and 16 provide comparisons between the actual services exports from the UK

to Sweden and the synthetic counterfactuals, and Figures 12, 15 and 17 show the estimated treatment effects, for each of the three models. As opposed to when considering the Brexit referendum in 2016 as treatment, the treatment effect of the TCA is more ambiguous. The baseline model, Figures 11 and 12, indicates a modest decrease in actual export relative to the synthetic counterfactual in 2021, but the actual export recovers immediately and increases above the synthetic counterfactual in 2022. The fluctuations in 2021-2022 are of size similar to the residuals in the pre-treatment period and the post-/pre-treatment RMSPE ratio is very low, 2.81, indicating that only a small treatment effect exists from 2021.

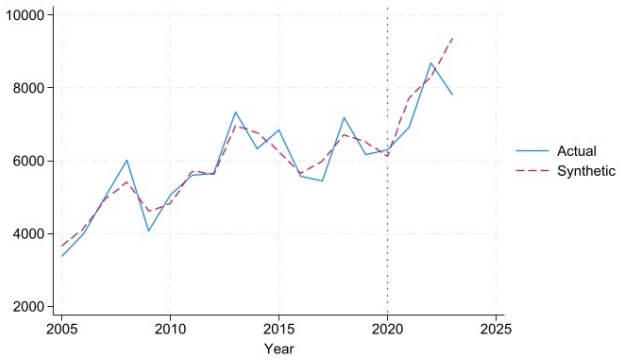


Figure 11: Baseline Model, Actual and Synthetic Export of Services from the UK to Sweden (US$ million) Under Treatment 2021 and Training Period Until 2020

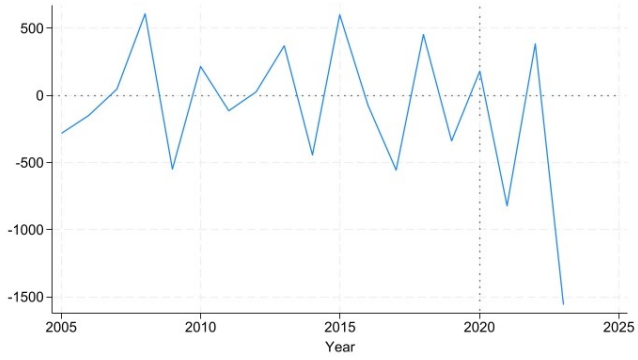


Figure 12: Baseline Model, Treatment Effect on Export of Services from the UK to Sweden (US$ million) under Treatment 2021 and Training Period Until 2020

As seen in Figure 13, shortening the training period to 2017 for the baseline model reveals a low out-of-sample validity, as the synthetic counterfactual follows a trend different from that of the actual outcome in the validation period 2018-2020.

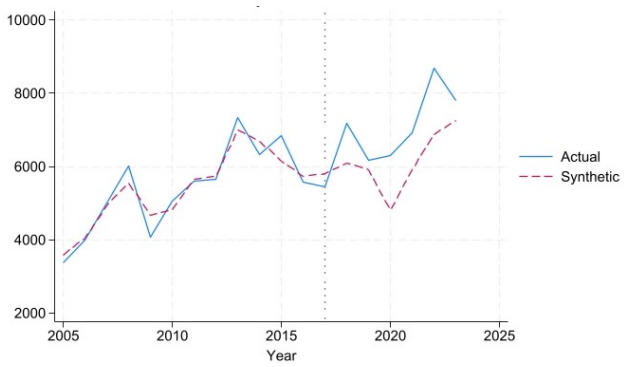


Figure 13: Baseline Model, Actual and Synthetic Export of Services from the UK to Sweden (US$ million) Under Treatment 2021 and Training Period Until 2017

Validation model 1, Figures 14 and 15, and validation model 2, Figures 16 and 17, have a much better fit in the validation period than the baseline model has when shortening the training period, and indicate treatment effects similar to the baseline model, with the major difference that the recovery in 2022 is smaller and does not overshoot the synthetic counterfactual. Due to the poor fit in the pre-treatment period, the two validation models still have a lower poost-/pre-treatment RMSPE ratio, 2.44 and 2.74 respectively, than the baseline model.

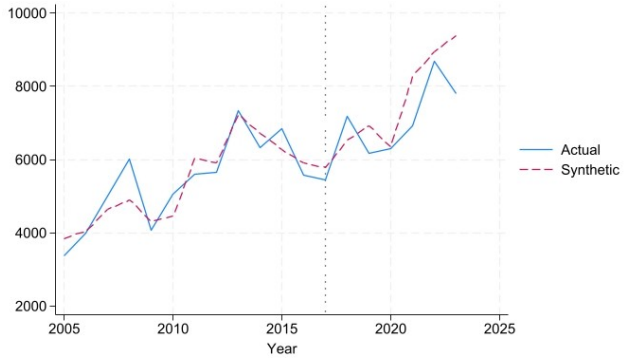


Figure 14: Validation Model 1 (no covariates), Actual and Synthetic Export of Services from the UK to Sweden (US$ million) Under Treatment 2021 and Training Period Until 2017

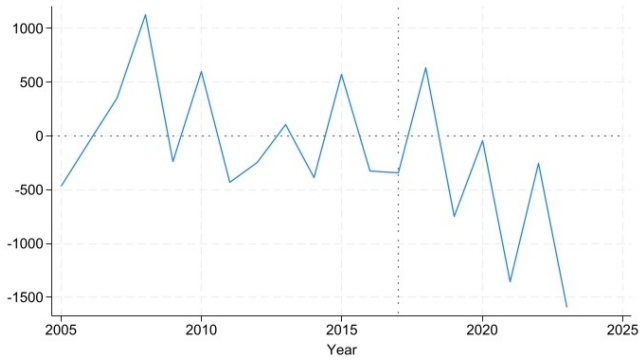


Figure 15: Validation Model 1 (no covariates), Treatment Effect on Export of Services from the UK to Sweden (US$ million) under Treatment 2021 and Training Period Until 2017

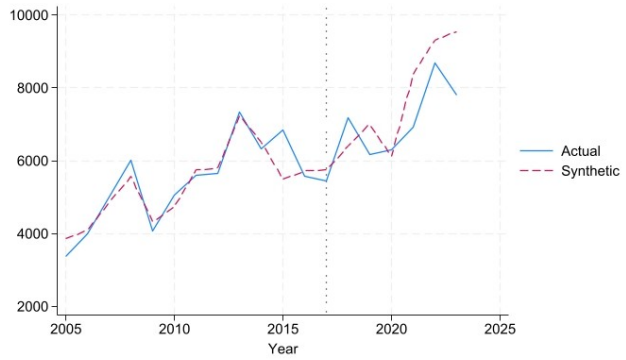


Figure 16: Validation Model 2 (with covariates), Actual and Synthetic Export of Services from the UK to Sweden (US$ million) Under Treatment 2021 and Training Period Until 2017

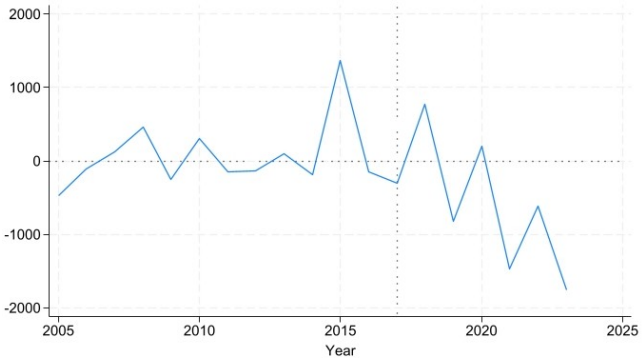


Figure 17: Validation Model 2 (with covariates), Treatment Effect on Export of Services from the UK to Sweden (US$ million) under Treatment 2021 and Training Period Until 2017

The treatment effects in US$ million and percent, as well as the p-values of the treatment effects, estimated through placebo analyses, are provided in tables 10, 11 and 12. None of the estimated treatment effects have a significance of $p < 0.05$. The effects are furthermore smaller than those estimated for 2021-2023 under treatment from 2016. Graphs with comparisons of the estimated treatment effects of each model with placebo effects estimated for each placebo unit are provided in

figures 30, 31 and 32 in the appendix, Subsection Additional Treatment- and Placebo Effect Figures.

Time	**Treatment Effect (US$ M.)**	**Treatment Effect (%)**	**P-value of Treatment Effect**	
			Two-sided	One-sided
2021	-822	-10.62%	0.1717	0.0944
2022	381	-4.59%	0.3734	0.1674
2023	-1,558	-16.66%	0.1545	0.0687
Mean	-666	-7.87%		

Table 10: Baseline Model, Treatment Effect on Export of Services from the UK to Sweden in the Treatment Period from 2021, with Training Period Until 2020 and p-values based on 232 Placebo Analyses

Time	**Treatment Effect (US$ M.)**	**Treatment Effect (%)**	**P-value of Treatment Effect**	
			Two-sided	One-sided
2018	*637*	*9.75%*	*0.1410*	*0.0726*
2019	*-745*	*-10.75%*	*0.1838*	*0.0983*
2020	*-44*	*-0.69%*	*0.9060*	*0.5128*
Mean	*-51*	*-0.77%*		
2021	-1,357	-16.40%	0.2265	0.1453
2022	-254	-2.84%	0.5897	0.3504
2023	-1,593	-16.97%	0.2009	0.0855
Mean	-1,068	-12.04%		

Table 11: Validation Model 1 (no covariates), Treatment Effect on Export of Services from the UK to Sweden in the Treatment Period from 2021, with Training Period Until 2017 and p-values based on 232 Placebo Analyses

Time	**Treatment Effect (US$ M.)**	**Treatment Effect (%)**	**P-value of Treatment Effect**	
			Two-sided	One-sided
2018	*778*	*12.17%*	*0.1169*	*0.0649*
2019	*-823*	*-11.75%*	*0.1558*	*0.0952*
2020	*-196*	*-3.21%*	*0.6494*	*0.2597*
Mean	*50*	*0.77%*		
2021	-1,468	-17.52%	0.2208	0.1342
2022	-615	-6.61%	0.3290	0.1688
2023	-1,754	-18.38%	0.1732	0.0649
Mean	-1,279	-14.09%		

Table 12: Validation Model 2 (with covariates), Treatment Effect on Export of Services from the UK to Sweden in the Treatment Period from 2021, with Training Period Until 2017 and p-values based on 230 Placebo Analyses

5 Robustness Tests

This section covers the results of the robustness tests applied to the models in Section 4. The results from the leave-one-out analyzes are presented in Subsection 5.1, the sector analysis is in Subsection 5.2 and the analyzes excluding intra-Europe trade and trade involving European countries from the donor pool are in Subsection 5.3.

5.1 Leave-One-Out Analysis

This subsection contains the results of the leave-one-out analyzes where the treatment effects are estimated iteratively under exclusion of each control unit assigned non-zero weight in the vectors $\mathbf{W}^*$ for the synthetic controls from the donor pool. Since the results are significant at $p < 0.05$ only for the models with treatment from 2016 and due to constraints in computing capacity that make a leave-one-out analysis of the lasso model too difficult, only the baseline model and the two validation models with treatment from 2016 are included in this robustness test. Figures 18, 19 and 20 illustrate comparisons between the estimated treatment effects provided in the results section and estimated treatment effects under the leave-one-out analyzes. In the baseline model, figure 18, the pre-treatment fit is robust to exclusion of any unit in the donor pool, and so are the significant losses in 2016-2017. The treatment effect remains negative in all leave-one-out analyzes for the baseline model, and in none of the analyzes are the measured treatment effects particularly smaller than in the baseline model with all donors. As seen in figure 18, there is one leave-one-out analysis that yields a much larger treatment effect in 2019-2023. That is when Denmark's services export to Germany is excluded, and considering the good pre-treatment fit of that analysis as well, there is a possibility that the actual treatment effect is stronger than estimated in the results in Section 4.

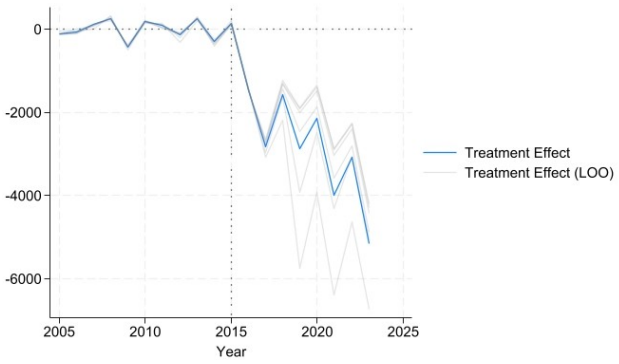


Figure 18: Baseline Model, Leave-One-Out Robustness Test of the Treatment Effect (US$ million) on Export of Services from the UK to Sweden in the Treatment Period from 2016 with Training Period Until 2015

In validation model 1, figure 19, the treatment effect is not robust to the leave-one-out analysis. In 2020-2022, the negative effect is fully driven by France's services export to Luxembourg, as excluding that unit yields a positive effect. Excluding Italy's services export to France from the donor pool furthermore yields a small positive effect in 2018, as does excluding France-Luxembourg. The pre-treatment fit in the training period is relatively robust to the leave-one-out analysis, although not as good as the baseline model, which is plausible considering the lower amount of predictors in the validation model. However, the validation period is more sensitive to the leave-one-out analysis, as excluding France-Luxembourg leads to a stronger negative deviation in 2014 and excluding Italy-France leads to a stronger positive deviation in 2015.

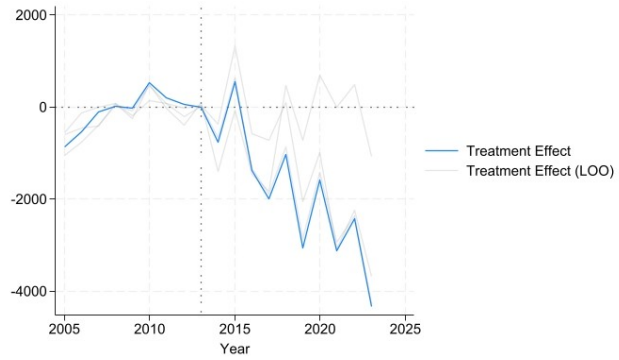


Figure 19: Validation Model 1 (no covariates), Leave-One-Out Robustness Test of the Treatment Effect on Export of Services from the UK to Sweden in the Treatment Period from 2016 with Training Period Until 2013

The negative treatment effect in validation model 2, figure 20, is robust to the leave-one-out analysis, although the treatment effect is close to zero in 2020 and 2022 when excluding France-

Luxembourg or Italy-France, and in 2018 when excluding Finland-Germany or Italy-France. However, it is important to remember that the effect of treatment in 2018 is small when no donor is left out as well, and the robustness test should not behave differently.

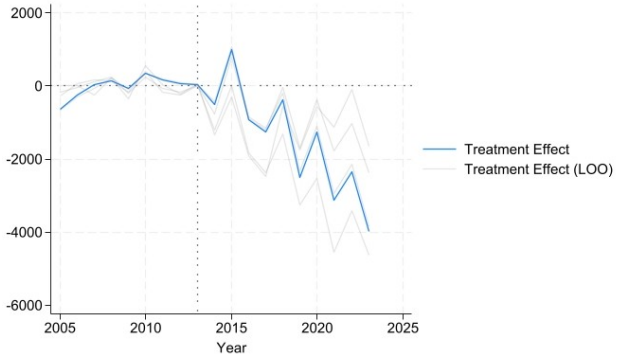


Figure 20: Validation Model 2 (with covariates), Leave-One-Out Robustness Test of the Treatment Effect on Export of Services from the UK to Sweden in the Treatment Period from 2016 with Training Period Until 2013 and Covariates

The maximum and minimum treatment effects estimated under the leave-one-out analysis of the baseline model are presented in US$ and percent in table 13. The gap is at most 25 percentage points or US$3,900 million, with no estimated loss smaller than 14.67 percent or $1,233 million. The baseline model should thus be considered robust to the leave-one-out analysis.

Time	**Treatment Effect**		**Treatment Effect (LOO)**			
	US$ M.	**%**	**Min**	**Max**	**Min (%)**	**Max (%)**
2016	-1,456	-20.71%	-1,486	-1,421	-21.05%	-20.32%
2017	-2,817	-34.10%	-3,082	-2,654	-36.15%	-32.77%
2018	-1,579	-18.05%	-2,176	-1,233	-23.28%	-14.67%
2019	-2,876	-31.75%	-5,745	-1,889	-48.17%	-23.41%
2020	-2,141	-25.36%	-3,916	-1,344	-38.32%	-17.57%
2021	-3,993	-36.61%	-6,381	-2,874	-47.99%	-29.36%
2022	-3,087	-26.21%	-4,625	-2,249	-34.73%	-20.56%
2023	-5,149	-39.78%	-6,727	-4,206	-46.32%	-35.05%

Table 13: Leave-One-Out Robustness Test of Treatment Effect on Export of Services from the UK to Sweden in the Treatment Period from 2016, Baseline Model with Training Period Until 2015

5.2 Sector Analysis

This section covers the estimated treatment effects within specific service sectors. Only the baseline model with treatment 2016 is considered for this robustness test since it best meets

conditions 2 in Subsection 3.1, has out-of-sample validity similar to the two validation models, has the highest significance and is the most robust to the leave-one-out analysis. Figures 21, 22, 23 and 24 provide comparisons between the actual and synthetic exports of services from the UK to Sweden within the four services sectors in which the UK's services exports to Sweden were of the highest total value throughout both 2005-2015 and 2005-2020. These are *Other business services*, figure 21, *Financial services*, figure 22, *Travel services*, figure 23, and *Charges for the use of intellectual property, not included elsewhere*, figure 24 (OECD and WTO, 2025). The loss in export is the most clear and stringent for *Other business services*, although *Travel services* contribute to the sharp decline immediately after the referendum. *Travel services* seem to have declined more than the synthetic counterfactual in 2020 and 2021 as well, but these results should be interpreted more carefully considering that the Covid-19 pandemic severely affected travel services and that, although the SCM ideally should account for time-variant unit-specific fluctuations, the restrictions differed between countries and in time. *Financial services* have a poor fit in the pre-treatment period than the other sectors and should therefore be analyzed with caution, but they appear to contribute to the losses in 2020-2023 as well.



Figure 21: Actual and Synthetic Export of Other Business Services from the UK to Sweden (US$ million) Under Treatment 2016, Baseline Model with Training Period Until 2015

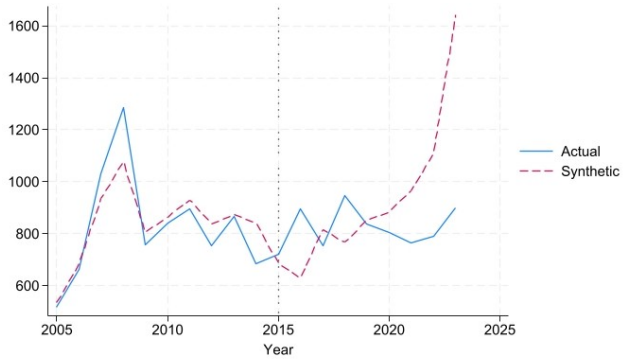


Figure 22: Actual and Synthetic Export of Financial Services from the UK to Sweden (US$ million) Under Treatment 2016, Baseline Model with Training Period Until 2015

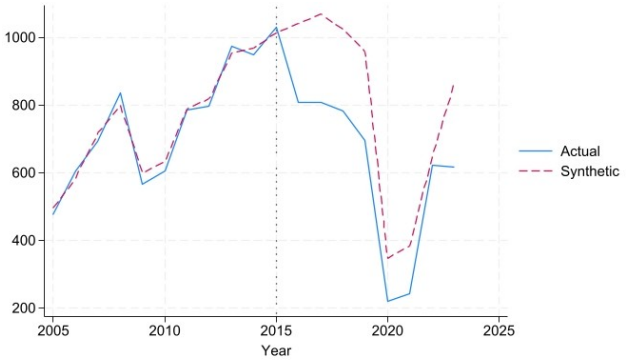


Figure 23: Actual and Synthetic Export of Travel Services from the UK to Sweden (US$ million) Under Treatment 2016, Baseline Model with Training Period Until 2015

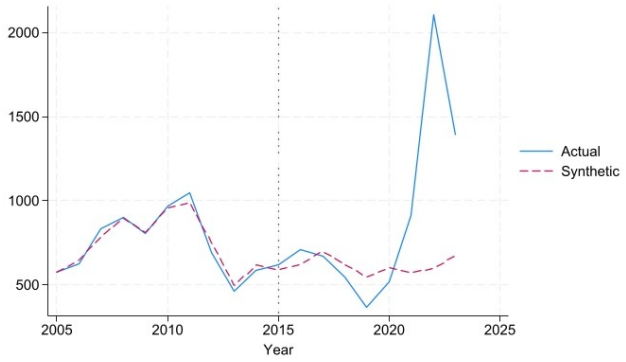


Figure 24: Actual and Synthetic Charges for the Use of Intellectual Property Imposed by the UK on Sweden (US$ million) Under Treatment 2016, Baseline Model with Training Period Until 2015

The treatment effects in US$ million and percent for *Other business services*, as well as the p-values of the treatment effects, estimated through placebo analyses, are provided in Table 14. The negative treatment effect is stronger than for total services export, with an average annual loss of more than 50%. The results furthermore have a two-sided significance of $p < 0.05$, and a one-sided significance of $p < 0.03$, in all treatment years, in contrast to none of the models for total services

showing significant losses in all years.

Time	Treatment Effect (US$ M.)	Treatment Effect (%)	P-value of Treatment Effect	
			Two-sided	One-sided
2016	-1,793	-49.37%	0.0175	0.0105
2017	-2,551	-57.07%	0.0210	0.0140
2018	-1,246	-26.56%	0.0490	0.0280
2019	-3,078	-53.41%	0.0280	0.0210
2020	-2,893	-49.30%	0.0315	0.0245
2021	-3,916	-54.83%	0.0350	0.0245
2022	-4,422	-58.07%	0.0315	0.0245
2023	-5,985	-68.74%	0.0315	0.0175
Mean	-3,236	-54.05%		

Table 14: Treatment Effect on Export of Other Business Services from the UK to Sweden in the Treatment Period from 2016, Baseline Model with Training Period Until 2015 and p-values based on 285 Placebo Analyses

The losses in *Financial services*, figure 22, in 2020-2023 are only one-sided significant at $p < 0.05$ in 2023, when the effect has a one-sided p-value of 0.0364. For *Travel Services*, the losses in 2016 and 2017 are one-sided significant at $p = 0.0425$ and $p = 0.0472$ respectively, while none of the losses in more recent years are significant at $p < 0.05$. The sector analysis indicate that the estimated losses in services export are robust and not only driven by one particular sector, although losses in export of *Other business services* seem to explain most of the drop in export of services.

5.3 Extra-Europe Analysis

This section covers the results from estimating the treatment effects when excluding intra-Europe trade, as well as all trade involving any European country, from the donor pool. For the reasons raised in subsection 5.2, the robustness test is only applied to the baseline model with treatment 2016. As shown in table 15, the EU member states continue to play an important role in constructing the synthetic counterfactual when only excluding intra-Europe trade. When excluding all European countries, all donor units assigned non-zero weight have the USA as either importer or exporter.

Excluding Intra-Europe Trade		Excluding all European Trade	
Trade Flow	**Optimal weight w^***	**Trade Flow**	**Optimal weight w^***
Sweden - Korea*	0.3980	Australia - USA	0.5600
Ireland - USA*	0.2140	Costa Rica - USA*	0.3730
Greece - USA*	0.1630	Mexico - USA*	0.0550
Luxembourg - USA*	0.0990	USA - Korea	0.0120
USA - Korea	0.0660		
USA - Netherlands	0.0490		
Ireland - Australia*	0.0070		
Italy - USA*	0.0030		

*Time series contains at least one interpolated value

Table 15: Optimal Unit Weights in the Vector $\mathbf{W}^*$ for Synthetic Export from the UK to Sweden Under Treatment 2016, Baseline Model with Training Period Until 2015, European Trade Excluded

Figure 25 illustrates a comparison of the actual services export from the UK to Sweden and its synthetic counterfactual constructed under exclusion of intra-Europe trade. The average treatment effect in 2016-2023 is then -42.78%, which is a stronger effect than the -29.93% in the baseline results for 2016-2023. The negative effect is furthermore one-sided significant at $p < 0.05$ in all treatment years except 2020, and two-sided significant at $p < 0.05$ in 2017, 2019, 2021 and 2023.

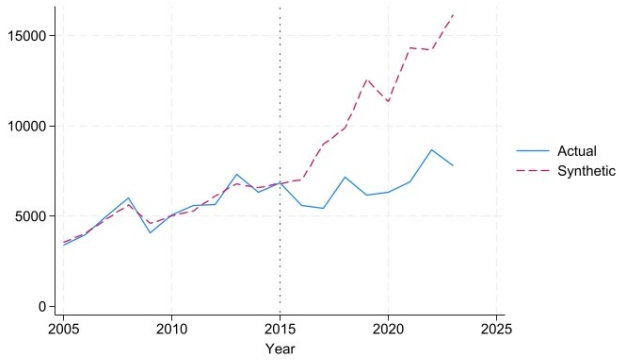


Figure 25: Actual and Synthetic Export of Services from the UK to Sweden (US$ million) Under Treatment 2016, Baseline Model with Training Period Until 2015, Intra-Europe Trade Excluded from Donor Pool

When excluding all trade flows that involve at least one European country, the results (Figure 26) still indicate losses in each year, except for in 2020 when there is a minor gain. The average treatment effect in 2016-2023 is -15.43%, which is smaller than the baseline estimate of -29.93%, and the only year with a one-sided significance of $p < 0.05$ is 2016.



Figure 26: Actual and Synthetic Export of Services from the UK to Sweden (US$ million) Under Treatment 2016, Baseline Model with Training Period Until 2015, Trade with Europe Excluded from Donor Pool

As raised in the methodology discussion, Subsection 3.7, violation of the stable unit treatment value assumption (SUTVA) from including European countries in the donor pool should lead to an overestimation of the gap between the synthetic counterfactual and the actual services export. The results from excluding intra-Europe trade indicate the opposite and the baseline results are thus robust to a more strictly imposed SUTVA. The robustness test where all European countries are excluded yields smaller effects, indicating that the baseline results are not robust to the most strictly imposed SUTVA, but it also yields a poor pre-treatment fit, indicting less validity.

6 Discussion

In this section, I discuss how the results respond to the research questions and whether they are consistent with the hypotheses. The internal- and external validity of the results is discussed in Subsections 6.1 and 6.2 respectively, and in Subsection 6.3, I provide my suggestions for further research studying the consequences to trade from Brexit and other cases of disintegration.

To answer Research Question 2: *Did the Brexit referendum in 2016 affect the UK's export of services to Sweden?* from the results in Section 4 should be relatively easy. Although not significant in each year, all four models lead to estimated losses from the synthetic counterfactual export of services from the UK to Sweden in each year from the referendum in 2016 to the end of data in 2023. Of the four models, the baseline model yields the most significant effects for 2016 and 2017, has the best pre-treatment fit, has out-of-sample validity similar to the two validation models and is robust to the leave-one-out analysis, the sector analysis and to excluding intra-Europe trade from the donor pool. The findings are in agreement with the first part of Hypothesis 1: *The Brexit referendum in 2016 caused a decline in the UK's export of services to Sweden*, and in line with previous studies that have found negative effects on export after the referendum.

Observing significant losses in export already after the referendum is also in agreement with theoretical models (Melitz, 2003; Arkolakis, 2010; Eaton et al., 2011) suggesting sunk export market entry costs and the suggestions of Douch and Edwards (2021) and Ahmad et al. (2023) that expectations of decreased future profits due to an anticipated increase in trade barriers made market entry unprofitable to more firms after the referendum. Furthermore, my findings support the conclusion of Osnago et al. (2015) that uncertainty is a barrier to trade.

To answer Research Question 3, the sizes of the estimated losses after the referendum are larger than predicted in the second part of Hypothesis 1: *The Brexit referendum in 2016 caused a decline in the UK's export of services to Sweden, similar to the 8.5 percent decline in services export to the EU.* The results indicate average annual losses between 23 and 30 percent, depending on the model, and only one loss in one model and year below 8.5 percent. In the study by Du and Shepotylo

(2024), the estimate of 8.5 percent is for the period 2017-2019 and the average treatment effect in 2017-2019 in my study range between 17.98 and 27.90 percent.

The stronger effect on export to Sweden than to the aggregate EU is unexpected considering that both export flows are within similar sectors (OECD and WTO, 2025). The only sector among the top four exports to Sweden that is not among the top four exports to the EU is *Charges for the use of intellectual property*, and the sectorial robustness test in Subsection 5.2 indicates a massive increase in export of intellectual property from 2021 and only minor decreases in 2017-2020. Sectorial differences between export to the EU and export to Sweden are therefore unlikely to explain the difference.

Looking at the OECD (2025a) Services Trade Restrictiveness Index (STRI), I suggest that higher barriers to services trade in Sweden compared to the UK's main export markets in the EU cause the larger decline in the UK's export to Sweden. In 2016-2023, the UK's top four export markets in the EU were, in descending order, Germany, Ireland, the Netherlands and France, to which the values of exports were more than double that of the fifth largest export market (OECD and WTO, 2025). According to the OECD, Germany has an STRI of 0.15, Ireland 0.18 and the Netherlands 0.12, and they are all below the OECD average of 0.19 and Sweden's 0.20. Only France had a higher STRI than Sweden with 0.21 (OECD, 2025a).

The declines in services export are not fully comparable to those for goods export from the UK to Sweden found by Douch and Edwards (2022) since they used monthly data, but my estimates of losses in services of 14 to 21 percent in 2016, and 18 to 34 percent in 2017, compared to estimated losses of 24 percent from June 2016 to March 2017, and 20 percent from March 2017 to March 2018, by Douch and Edwards do not suggest any major differences between goods and services exports. This contradicts the suggestion that Brexit had a greater impact on services than goods, which was expected based on the literature (Du and Shepotylo, 2024; Lowe, 2021; Baldwin et al., 2024; Benz and Jaax, 2022), but it is consistent with the estimated loss in goods export to the EU (Buigut and Kapar, 2023) being similar to the loss in services export to the EU (Du and Shepotylo, 2024) after the referendum. However, I cannot disregard from the fact that the high degree of uncertainty after the

referendum possibly led to firms overestimating the consequences for goods and/or underestimating the consequences for services.

The answer to Research Question 1: *Did the exit from the EU single market and the implementation of the Trade and Cooperation Agreement (TCA) in 2021 affect the UK's export of services to Sweden?* is more ambiguous. Among the models with treatment from 2016, the baseline model and validation model 2 have the highest percentage losses in 2021 and 2023, and all models experience the highest percentage loss in 2023, significant for all models. For the lasso model, in particular, the highest impact is in 2020-2023 and validation model 2 is only able to show significance at $p < 0.05$ in 2021-2023. Although this suggests an additional effect of the TCA, it is important to remember that finding the largest effects of treatment in 2021-2023 when 2016 is the start of treatment does not imply that the impact of the TCA was greater than the impact of the referendum, since exports were already at a low level after the referendum when the TCA was implemented. Therefore it is important to study the TCA treatment separately.

Although all models with treatment from 2021, except the baseline model in 2022, yield losses in 2021-2023, none of the losses are significant. That could be due to the difficulties in finding a proper counterfactual that can capture the consequences of the referendum or because most important consequences of the TCA were anticipated in advance.

There is a strong indication that the first part of Hypothesis 2: *The exit from the EU single market and the implementation of the Trade and Cooperation Agreement (TCA) in 2021 caused a decline in the UK's export of services to Sweden, larger than the 25 percent decline in goods export.* is valid, but I cannot reject that it is not. It is safer to say that the second part does not hold, as all estimated effects of the TCA are below 20 percent. Finding a smaller impact of the TCA than the impact of the referendum indicates that the findings of Carballo et al. (2022), that reduced uncertainty accounts for most of the export gains from free trade agreements, as well as the anticipatory increases in trade before free trade agreements found by Freund and McLaren (1999), are reversible in the case of a retreat to protectionism and disintegration. This reduces some uncertainty about whether

the findings of Carballo et al. (2022) and Freund and McLaren (1999) are reversible, raised from the findings of Ait Benasser (2025) and Ravikumar et al. (2024), that the impact on trade from reversal to protectionism is less significant than the impact from liberalization.

Finding smaller losses in services export than goods export under the TCA contradicts my explanation that the similar size of goods and export losses after the referendum were due to firms overestimating the future barriers to goods or underestimating the barriers for services. If that had been the case, the findings under the TCA, when all regulations are known to the market, should be smaller losses, or even increases, in goods export and/or higher losses in services export. The results instead suggest that the non-tariff barriers to goods export from the UK to Sweden have a stronger impact than the services trade restrictions, in contrast to my expectations based on the literature (Du and Shepotylo, 2024; Lowe, 2021; Baldwin et al., 2024; Benz and Jaax, 2022). A possible explanation could be a high degree of export of digital services, which is a possible mode of supply for the top exports *Other business services*, *Financial services* and *Charges for the use of intellectual property*, and according to Baldwin et al. (2024) a mode of supply subject to less barriers. Another explanation could be the large portion of financial services in the UK's exports to both Sweden and the EU, a sector in which the UK could benefit from having its own regulation (Douch and Edwards, 2021; Du and Shepotylo, 2024). This explanation is, however, contradicted by the large loss in financial services export to Sweden since 2020, found in the sectorial analysis in Subsection 5.2.

6.1 Internal Validity

An important issue mentioned in Subsection 3.3 is the high degree of estimates included in the BaTIS dataset (OECD and WTO, 2025). Although this is a valid concern, it has to be put in relation to the difficulties in studying trade in services. Since BaTIS includes all available data on trade in services, the alternative to this analysis would simply have been not to perform any analysis on trade in services at all or to decrease the donor pool to the extent that it would not have been possible to construct a valid counterfactual. The strategy of this study is therefore to accept some flaws in the methodology and to be as transparent about them as possible. Important to keep in mind

is also that OECD (2025b) claim that the quality of the dataset is sufficient for policy analysis, and that I have removed the trade flows estimated using gravity regression from the donor pool.

Another issue is that the estimated effect potentially could be overestimated from including EU members in the donor pool and violating the stable unit treatment value assumption (SUTVA). That is plausible considering that EU members most likely compensated their decreased import from the UK by importing from other EU members when the trade barriers to the UK increased and in line with Du and Shepotylo (2024), Lowe (2021) and Tamberi (2024) suggesting that multinational companies shift their export hubs for the EU single market to other EU member states. The robustness test excluding intra-Europe trade from the donor pool in Subsection 5.3 indicates the opposite as the estimated losses were higher and more significant. The EU member states could also have diverted their imports to countries outside of the EU, but considering the importance of geographical and cultural proximity in gravity regression models for trade, such as the models by OECD (2025b) and Buigut and Kapar (2023), as well as the close to non-existence of trade barriers within the EU, the diversions were most likely within the EU. The robustness test excluding all European countries from the donor pool in Subsection 5.3 does, however, indicate that the baseline results could be overestimated, but important to remember is the poor pre-treatment fit between actual and synthetic export in this robustness test, which highlights the trade-off between the SUTVA and having a donor pool with characteristics similar to the treatment unit.

The SUTVA-issue is similar to that mentioned by Abadie and Gardeazabal (2003), where the Spanish regions in the donor pool are also affected by terrorism activities, but to a lesser degree than the treated Basque Country. Douch and Edwards (2021) similarly make the argument that Brexit affects the UK to a greater extent than the EU member states, which is also supported by the findings of Buigut and Kapar (2023) that the increase in intra-EU trade from Brexit was smaller than the decrease in UK-EU trade.

A more critical issue is the low number of pre-treatment periods in this study. Douch and Edwards (2022) used monthly data from January 1999 until the Brexit referendum in June 2016 for

their pre-treatment data when looking at the referendum's effect on trade in goods, implying 210 periods as opposed to this study's use of 11-16 periods. Douch and Edwards (2021) furthermore used monthly data from January 2008 until the referendum in their similar study on trade in services, implying 102 pre-treatment periods. Abadie et al. (2015) suggest not using the synthetic control method when the number of pre-treatment periods is low, according to Abadie et al. (2010) time-variant unit-specific factors $\lambda_t \mu_i$ could bias the estimated treatment effect when the number of pre-treatment periods is not sufficiently large, and Hollingsworth and Wing (2022) provide a rule of thumb to not have more units in the donor pool than pre-treatment periods. An important example of a time-variant unit-specific factor is, as mentioned in Subsection 3.7, Covid-19. An advantage of the SCM compared to the DiD is that it should be able to capture such factors, and the synthetic control using lasso can perform well under a low number of pre-treatment periods, but with a number of pre-treatment periods this low, it is possible that Covid-19 and other shocks to some extent have biased the results for 2020-2023.

Considering the issues with trade in services data, it is unavoidable to not make any trade-offs in the methodology. Douch and Edwards (2021) decided to prioritize monthly data at the cost of not using bilateral data. My decision is instead to prioritize bilateral data at the cost of fewer pre-tretment periods since the research questions require studies of the UK's export to a specific partner. It is however important to keep this issue in mind when interpreting the results of the study.

6.2 External Validity

The external validity of my study concerns whether the results are generalizable to other cases of economic disintegration. Considering the large heterogeneity among both countries and service categories, it is difficult to draw any general conclusions about the findings. The cases most similar to my study are the effects to the UK's bilateral export of services to other EU member states and the aggregate services export to the EU. Since there are differences in barriers to trade in services between EU member states (OECD, 2025a), I argue that the size of the effect is not generalizable to other EU member states or the EU. The robustness test in Subsection 5.2 furthermore indicates that

different sectors are affected differently, in accordance with the literature (Douch and Edwards, 2021; Du and Shepotylo, 2024), and the results should therefore not be generalizable to export partnerships reliant on other sectors either. Since my results are in agreement with previous studies finding negative effects to aggregate services export to the EU after the referendum and the similarity in services sectors between the export to the EU and the export to Sweden, I do however argue that finding negative effects to services export from the TCA in 2021 should be generalizable to the UK's export to the EU after the TCA as well.

6.3 Further Research

Since the implication of Brexit is that the UK leaves the EU, it makes sense from a policy-evaluating point of view to study the export of services from the UK to the EU. Most of the studies in the literature review hence also study aggregate exports of the UK to the EU when estimating the impact from Brexit. An interesting step in future research would be to conduct studies similar to this one for the UK's service exports to each EU member state and aggregate the results to find the impact of the TCA on the UK's export to the EU. Before this study, I only managed to find studies on the consequences for services export after the referendum and not after the TCA. They do not provide a complete understanding, as it is difficult to know which consequences of Brexit the firms were anticipating after the referendum.

As shown by Du and Shepotylo (2024), different DiD models, SCM and SDiD yield a wide range of different estimates of the referendum's effect on services export. Future studies should therefore consider applying multiple methods to the same data and treatment in order to ensure that the estimates are robust to methodological choices. Considering the positive characteristics of the SDiD method in accounting for time- and unit fixed effects discussed by Arkhangelsky et al. (2021), Du et al. (2023) and Du and Shepotylo (2024), I highly encourage future studies aimed at comparing different methods to include SDiD.

7 Conclusion

In this study, I investigate the consequences for services export from disintegration from the EU single market by looking at the case of Brexit. Using the synthetic control method (SCM) by Abadie and Gardeazabal (2003) and Abadie et al. (2010), and the Balanced Trade in Services (BaTIS) dataset by OECD and WTO (2025), I estimate four different counterfactual export flows from the UK to Sweden that were never affected by either the Brexit referendum in 2016 or the UK's exit from the EU single market in 2021, and three counterfactual export flows that were affected by the referendum but not the exit. Through the SCM, the counterfactuals are constructed as weighted sums of other bilateral services export flows unaffected by Brexit that best resemble the pre-treatment characteristics of the relationship between the UK and Sweden.

I find that the actual export after the referendum is lower than each of the four counterfactual exports in each year until the data ends in 2023. The average annual loss is between 1.7 and 2.9 billion US$ from the four counterfactual export flows, which corresponds to between 23 and 30 percent of the counterfactual export values. To put these numbers in context, they correspond to 12-20 percent of Sweden's annual military spending (Government Offices of Sweden, 2025), the annual cost of 34,000 to 58,000 employees under the average salary in the UK (ONS, 2024a) or 9 to 16 percent of the UK's annual spending on international development aid (OECD, 2024). The average annual losses to the three counterfactual cases with the referendum but without the exit in 2021 are smaller, between 0.7 and 1.3 billion US$ or 8 to 14 percent of the counterfactual export values, and not significant.

These results suggest that firms managed to predict the consequences of the exit from the EU single market and the implementation of the Trade and Cooperation Agreement (TCA) between the UK and the EU in advance and made their market entry decisions accordingly. The findings also support the literature on uncertainty as a barrier to trade and indicate that the positive effects on trade from reduced uncertainty when entering into free trade agreements are reversible under economic disintegration.

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Appendix

Additional Treatment- and Placebo Effect Figures

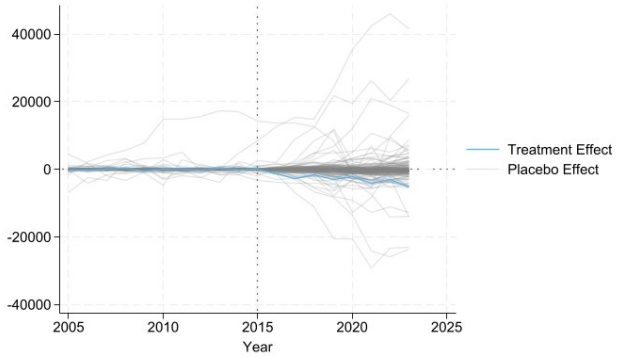


Figure 27: Treatment Effects on Export of Services from the UK to Sweden and 231 Placebo Trade Flows (US$ million), Baseline Model under Treatment 2016 and Training Period Until 2015

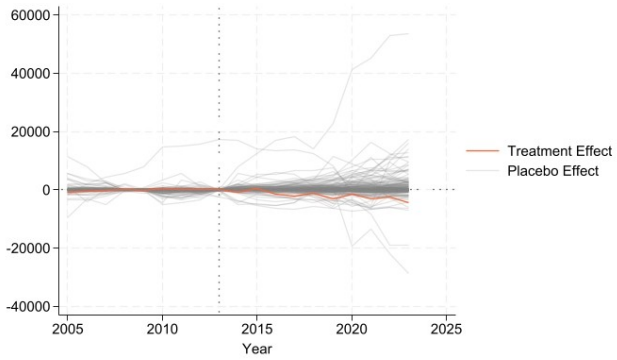


Figure 28: Treatment Effects on Export of Services from the UK to Sweden and 231 Placebo Trade Flows (US$ million), Validation Model 1 under Treatment 2016 and Training Period Until 2013

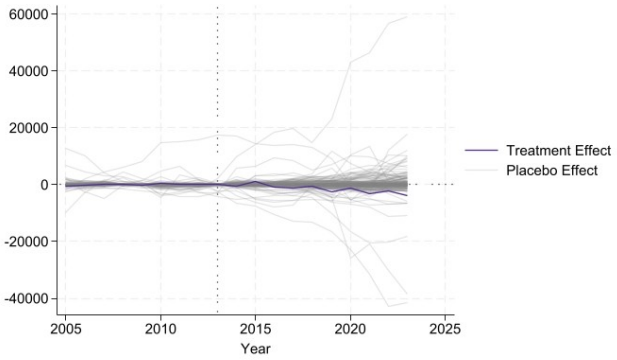


Figure 29: Treatment Effects on Export of Services from the UK to Sweden and 229 Placebo Trade Flows (US$ million), Validation Model 2 under Treatment 2016 and Training Period Until 2013

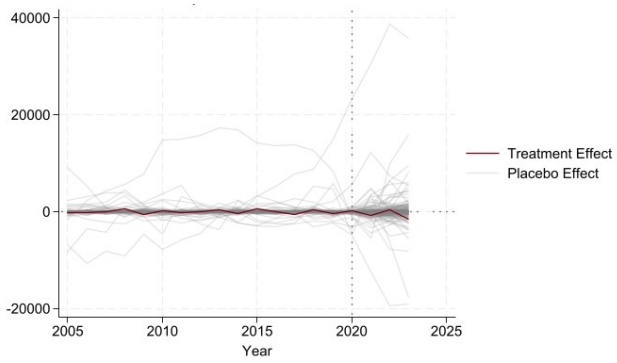


Figure 30: Treatment Effects on Export of Services from the UK to Sweden and 232 Placebo Trade Flows (US$ million), Baseline Model under Treatment 2021 and Training Period Until 2020

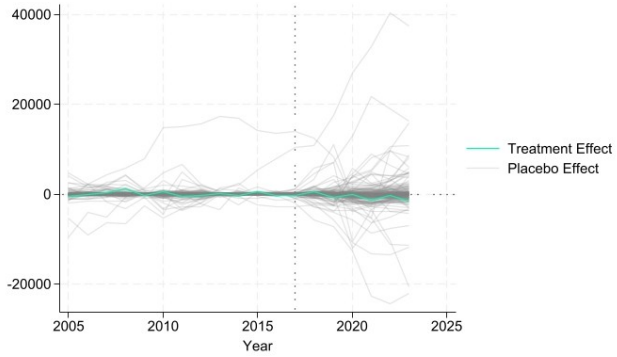


Figure 31: Treatment Effects on Export of Services from the UK to Sweden and 233 Placebo Trade Flows (US$ million), Validation Model 1 under Treatment 2021 and Training Period Until 2017

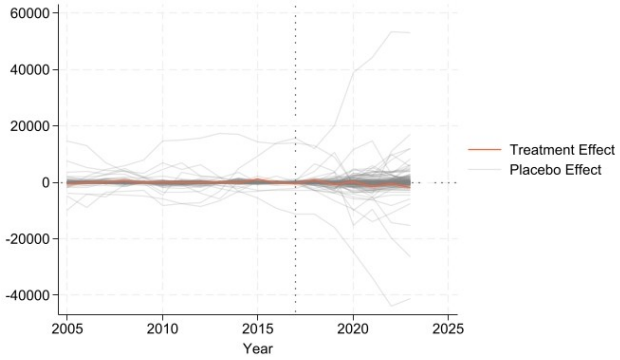


Figure 32: Treatment Effects on Export of Services from the UK to Sweden and 230 Placebo Trade Flows (US$ million), Validation Model 2 under Treatment 2021 and Training Period Until 2017

Use of Generative AI

The process of developing the scripts for Stata has been assisted by the generative AI ChatGPT as a tool to search for appropriate commands, understanding the Stata syntax, and for troubleshooting, which has made the process faster and helped me advance my knowledge in Stata. The suggestions have been double-checked in the appropriate Stata help files and/or the Statalist forum, and the code is my own work. The generative AI Writefull, integrated in Overleaf's LaTeX-environment, has assisted the process of converting the Stata output to LaTeX-syntax tables, which has enhanced the visual quality of my thesis. The risks in doing so have been very limited since all tables have been carefully compared to the initial output.