Master's thesis

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Analyzing the expiration of the United States Generalized System of Preferences and its impact on imports from developing countries

Master's thesis in Economics
Supervisor: Inga Heiland
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Abstract

Preferential trade agreements (PTA) are widely used to stimulate exports from developing countries through reduced tariffs. One of the largest PTAs, the United States Generalized System of Preferences (GSP), was first introduced in 1974. In 2020, it covered imports of over 3500 product categories at the Harmonized System 8-digit level from 119 developing countries. The most recent expiration was on January 1, 2021 following failure to renew funding. Using this expiration as a natural experiment, we analyze the effect of the tariff increase on imports from eligible countries on a panel of 233 countries and 17 867 products at the 10-digit Harmonized Tariff Schedule level between 2018 and 2022. We use a triple difference estimation method to account for heterogeneous shocks due to the COVID-19 pandemic. From our baseline specification, we find that imports of eligible products from eligible countries fell as a result of expiration. We also find that the effect is stronger in more developed countries due to their greater ability to utilize the program. Additionally, countries eligible for alternative PTAs are also impacted by the expiration, even more so than other countries. Due to the variation in estimated effect, the exact impact of expiration cannot be determined, but given the consistency of the magnitude and direction of the result, we conclude that the expiration of the GSP does reduce imports of eligible products from GSP countries to the United States.

Sammendrag

Preferential trade agreements (PTA) er mye brukt for å stimulere til økt eksport fra utviklingsland gjennom redusert toll. Et av de største PTA programmene, USA sitt Generalized System of Preferences (GSP) ble introdusert i 1974. I 2020 dekket programmet import av over 3500 forskjellige produkttyper på 8-siffret Harmonized System nivå fra 119 utviklingsland. Programmet utløp den 1. januar 2021 grunnet manglende finansiering. Vi bruker dette som et naturlig eksperiment for å analysere effekten av tolløkningen på import fra land som inngikk i GSP med paneldata bestående av 233 land og 17 867 produkter på 10-siffret Harmonized Tariff Schedule nivå mellom 2018 og 2022. Ved å bruke en trippel differanse som estimeringsmetode kan vi gjøre rede for de heterogene sjokkene forårsaket av COVID-19 pandemien. Med vår hovedspesifikasjon finner vi at import av godkjente produkter fra GSP land faller på grunn av at programmet løper ut. Vi finner også at effekten er sterkere i mer utviklede land grunnet deres evne til å ta i bruk programmet i større grad. Videre finner vi at land som er kvalifisert for alternative PTA programmer blir påvirket av import nedgang i større grad enn andre land. På grunn av variasjonen i den estimerte koeffisienten kan vi ikke fastslå den nøyaktige effekten. Gitt at størrelsen og retningen på effektene ikke endrer seg i stor grad mellom spesifikasjonene kan vi derimot konkludere med at GSP programmets utløp førte til redusert import av godkjente produkter til USA fra GSP land.

Contents

List of Abbreviations

AGOA	African growth and Opportunity Act
CBI	Caribbean Basin Initiative
CNL	Competitive needs limit
DiD	Difference in differences
DiDiD	Difference in difference in differences (Triple difference)
GSP	Generalized System of Preferences
HTS	Harmonized Tariff Schedule
HS	Harmonized System
LDC	Least developed countries
MFN	Most favored nation
OECD	Organisation for Economic Co-operation and Development
OLS	Ordinary least squares
PPML	Poisson pseudo maximum likelihood
PTA	Preferential trade agreement
RoO	Rules of origin
USTR	United States Trade Representative
WTO	World Trade Organization

1 Introduction

The reaction to the expiration of the United States Generalized System of Preferences (GSP) on January 1, 2020 shows that preferential trade agreements (PTA), which grant reduced tariffs to trade partners, matter to the developing countries receiving preferential access. In February 2023, a group of 27 GSP beneficiary countries called for the renewal of the program, stating:

"For many years, each of our countries has utilized the trade preference program to further our economic development and raise standards of living according to its objectives". The letter continued "Through decades the program boosted growth across many sectors benefiting companies of all sizes." After three years with no access to GSP benefits, the countries consider renewal a high priority: "the urgent re-authorization of the program has become a central piece on the trade agenda of our countries." (Sandler, Travis & Rosenberg, P.A. 2023, para.4).

The idea that PTAs serve to promote development in developing countries is part of the reason why many developed economies have introduced their own system of preferential access. In 2020, the US GSP program covered products valued at $16.9 billion entering the United States duty-free from eligible countries. The total import from these countries was valued at $152 billion, meaning the program covers over 10% of US imports from GSP countries (Wong 2023). If the program truly helps to further economic development and alleviate poverty in the beneficiary countries, renewal is of utmost importance, but whether or not PTAs benefit eligible exporters is uncertain.

While the reduced tariff will, in theory, lead to increased trade, the size of the effect is unknown and depends on many factors. On the one hand, the preferential access reduces prices on eligible products from eligible countries, thus making them more competitive for importers. Using trade data between 178 countries from 1948 to 1999,

Rose (2004) finds that GSP programs approximately doubled trade between issuing and beneficiary countries. On the other hand, Augier, Gasiorek, and Lai Tong (2005) argue that the effectiveness of the program is limited by the cost of fulfilling the rules of origin (RoO) requirements, the strictness of which will reduce utilization. The requirements imply having to source input factors from accepted countries of origin and documenting the supply chain. This is both costly and time-consuming, serving as a barrier to utilization and trade through GSP.

In this thesis, we attempt to answer the following research question:

What is the effect of the 2021 expiration of the United States generalized system of preferences on imports from beneficiary countries?

Other researchers have already looked at the effect of withdrawal of preferential access by both the European Union and the United States. Hakobyan (2020) analyzes the effect of the expiration of the US GSP in 2011 and finds that imports fell by 3% on average from eligible countries. Tanaka (2022) looks at the withdrawal by the EU of preferential access from Cambodia and finds it leads to a reduction in imports from Cambodia by 33.6%. Similarly, Gnutzmann and Gnutzmann-Mkrtchyan (2022) look at the removal of Belarus from the EU GSP program and find an export drop of 27% from Belarus to the EU. Frazer and Biesebroeck (2010), on the other hand, look at an expansion of the African Growth and Opportunity Act (AGOA) PTA program in 2000 and find a 12.7% increase in imports. It is important to note that the US and EU programs differ, likely covering different products and having different RoO strictness. Consequently, direct comparison of results between the two is not possible. This thesis aims to contribute to the existing research by looking at the latest and longest ever expiration of the US GSP program.

Our research question and topic are inspired by Hakobyan (2020) which is, to our knowledge, the first paper analyzing the effect of GSP expiration on trade. The paper looks at a broad range of implications of the expiration, but we identified some weaknesses in the data that limit the credibility of the analysis. Specifically, the article looks at a 10-month long expiration from January 1, 2011 until November 5, 2011, but only uses data from 2010 to 2012 making the validity of the parallel trends assumption difficult to verify. Given that the analysis looks at yearly effects and considers 2011 as the expiration year, the 2-month period where the program was active can lead to an underestimated effect. The short expiration could also mean that longer-term issues such as credit constraint do not have time to fully come into effect. The paper also aggregates products at the Harmonized System (HS) 6-level code which might give less precise estimates.

In our paper, we use data from 2018 to 2022 to analyze the effect of the 2021 expiration of the GSP program. This gives us the possibility to estimate the medium-term effects of expiration. We explore alternative model specifications in detail and discuss their validity. With three pre-treatment years, we thoroughly discuss the parallel trends assumption for all specifications. Additionally, the use of the 10-digit American Harmonized Tariff Schedule (HTS) product code gives us more granularity.

Using a difference in difference in differences (DiDiD) approach, we estimate that the value of imports of eligible products from beneficiary countries decreased by 4.61% post-expiration. Different credible specifications predict a range of estimated effect of similar magnitude and significance. We theorize that the effect is stronger for larger economies due to their ability to better utilize the program. Additionally, eligibility for other preference programs does not seem to reduce this effect since the countries that have alternative eligibilities are more affected by the expiration than the countries who do not.

The coinciding effects of the COVID-19 pandemic in 2020 and 2021 make the analysis difficult due to the probability of heterogeneous shocks. We remedy this by controlling extensively for fixed effects as well as running exhaustive robustness checks. Nevertheless, our results are likely affected by the pandemic.

This thesis is organized as follows: Chapter 2 provides the theoretical background of preferential trade agreements, rules of origin and the US GSP program. Chapter 3 consists of a thorough review of the data and changes to the GSP program between 2018-2022. Chapter 4 describes our model specification and the triple difference model estimation method. In Chapter 5 we present the results from our baseline model and discuss the parallel trends assumption and potential spillover effects. Chapter 6 goes over different modifications and robustness checks to verify the validity of our model. Chapter 7 consists of a discussion of our findings, policy implications and some suggestions for further research. Finally, in Chapter 8 we draw our conclusions.

2 Theoretical and Institutional Background

In this chapter, we discuss the main background topics of our thesis. We begin by examining preferential trade agreements, followed by the rules of origin and conclude with an overview of the United States GSP program.

2.1 Preferential trade agreements

Unilateral preferential trade agreements have been argued to promote economic growth in the beneficiary countries (Tanaka 2021; Sytsma 2021). This argumentation is the basis for large developed economies, such as the US and the EU, having introduced their own versions of a GSP program for trade with developing countries. GSPs are a form of PTA, which is defined by the World Trade Organizations (WTO) as "lower or zero tariffs, which a member may offer to a trade partner unilaterally" (WTO 2024a).

These preferential trade agreements are an alternative to the most favored nation (MFN) tariff put in place by the WTO. The MFN principle states that member countries cannot discriminate between their trading partners. Under the WTO framework generally, if a trade partner is granted a lower tariff rate, all other countries must be granted that same rate. PTAs to developing countries are an exception to this rule, alongside regional trade agreements with common external tariffs (WTO 2024c).

The agreements let the beneficiary countries export to the partner country at a lower tariff rate than with the MFN tariff, thus reducing the import price of goods. This would in theory let them compete with more productive countries which have to pay the MFN tariff. With a basis in the export-led growth theory, PTAs can be viewed as a charitable measure for developing countries, as expressed by the Office of the US Trade Representative (Congressional Research Service 2022).

2.2 Rules of Origin

In order to receive preferential access, the beneficiary countries must fulfill certain requirements. The main one being for the beneficiary country to prove that the product originates from their country. These rules of origin (RoO), as defined by the WTO, serve as "criteria needed to determine the national source of a product." (WTO 2024b). The general rule is that a certain percentage of input factors come from the exporting country or other accepted countries. In our case with the US, the rule requires a minimum of 35% of value to come from a beneficiary developing country (Wong 2023).

The main purpose of the RoO criteria is to limit the possibility for trade deflection, meaning that a firm could export a good into the US through a beneficiary country and benefit from preferential access (Anson et al. 2005). In the absence of RoO, there would thus be a clear possibility for arbitrage. The RoO means that this cannot be done without the final good being sufficiently transformed in the beneficiary country. This way there is an incentive for international corporations to move production to the developing countries or work with local firms to profit from preferential access.

Since supplying enough of the production domestically can be difficult for some countries or sectors, the strictness of the RoO will affect the utilization rate of the PTA. The countries from which input materials can be sourced from and still have the finished product qualify for preferential access therefore have an impact on strictness. Expanding the list of allowed sources with large supplier countries, can increase the amount of production that fulfills the RoO, making it eligible for preferential access. This can lead to more competitive production and thus more exports. As found by Sytsma (2022), allowing apparel producers in Bangladesh to use internationally-sourced textiles, namely from China, lead to significant growth in exports. Another aspect of the RoO is the possibility for protection of domestic production. This comes

from the strictness of the RoO, the inclusion of competitive needs limit (CNL) waivers and the selection of product categories that are included in the PTA. These methods can be used by the issuing country to limit the scope of the PTA (Krishna 2005).

Fulfilling the RoO requirements represents additional fixed and variable costs for exporters. These costs come in the form of having to source input factors from other beneficiary countries to comply with the RoO as well as documenting this fact to the customs office. This is proven by utilization rates being consistently lower than unity. While imperfect information accounts for some of the gap, it would theoretically disappear in the long-term which we do not observe (Krishna et al. 2021).

The decision of the exporter is therefore whether to use the GSP preferential access and fulfill the RoO, or use the MFN. The decision is based on whether or not the cost of complying with the RoO exceeds the tariff reduction gained with preferential access. The more restrictive the RoO, the higher the cost of complying and therefore the higher the tariff reduction needs to be. In essence, the higher the RoO restrictiveness, the higher the MFN tariff must be for compliance to be worth it (Moran and Cebreros 2023). These access limitations reduce the effectiveness of the trade agreements, while at the same time making their introduction more politically viable due to the possibilities of targeted protection (Krishna 2005).

2.3 The United States GSP program

The purported goals of the GSP program in the United States are to promote economic growth in developing countries, support American jobs, uphold competitiveness for American firms as well as promoting American values in the beneficiary countries (United States Trade Representative 2024b). The advantages for beneficiary countries have been discussed above, while the American firms profit from reduced import prices on input factors and therefore more competitive prices on their products. The system can also be used to enforce intellectual property rights and human rights by threatening to remove preferential access if these rights are not respected. Examples of this are the EU's withdrawal of preferential access in key sectors from Cambodia in 2020 after human rights transgression and the removal of Belarus from the EU GSP due to labour rights violation (Tanaka 2022; Gnutzmann and Gnutzmann-Mkrtchyan 2022). In both cases, withdrawal lead to significant drops in exports. While the GSP won't stop violations from happening, the threat of preference withdrawal can serve as deterrence.

The US GSP program was first introduced in the Trade Act of 1974 and needs funding allocated at regular intervals to uphold its function. Since its inception, the program has expired 10 times. Only 5 times has it been renewed prior to its expiration. The previous longest recorded expiration was from July 31, 2013 to July 29, 2015, lasting 728 days. Each renewal has included a retroactive refund of the tariffs paid during the expiration. On the December 31, 2020 the US GSP scheme expired again. It has still not been renewed making it the longest shutdown in the program's history. Given the retroactive effect of renewals up until now, the US Customs and Border Protection is encouraging exporters to still document RoO in order to receive a potential refund when the program eventually gets reinstated (Wong 2023).

The GSP is approved through legislation passed by the US Congress, which allocates funding for the program from a designated start date to an end date. To extend beyond this period, another bill must be approved by Congress. The program has been used by politicians to protect domestic US companies. In 2010, Jeff Session (R-AL), who was a senator at the time, put legislation renewing the GSP on hold at the request of a US-based sleeping bag manufacturer, which led to the program's expiration. The program was not reinstated until the Obama administration agreed to remove GSP eligibility from the products in question (Blanchard and Hakobyan 2015).

The most recent extension of the program was included in the Consolidated Appropriations Act of 2018, ensuring funding until December 31, 2020. However, no subsequent bills passed in 2020 or after has reinstated this funding. At the close of 2020, the US legislature had other priorities, and there was minimal discussion regarding the impeding expiration of the GSP. Factions of both parties are currently trying to reinstate the program, but the necessary support is not there (Williams and Alghazali 2024).

According to the Congressional Research Service, the GSP program encompassed 3,500 products at the HS-8 level before its expiration. Additionally, there were an additional 1,500 products, also at the HS-8 level, designated for least developed countries (LDC). In 2020, the value of products entering the US using the GSP amounted to $16.9 billion, while the total imports from GSP-eligible countries reached $152.0 billion. Comparatively, total imports to the US reached $2.3 trillion (Wong 2023).

These frequent expirations might impede the long-term utilization by requiring exporters to re-learn using the agreements. Krishna et al. (2021) finds that utilization increases with experience. A predictable system would therefore let exporters in beneficiary countries become more efficient in exporting their goods through the GSP, raising both utilization and exports.

Assuming a retroactive compensation of tariffs and signaling of a swift renewal, a short expiration might not lead to significant changes in exports or utilization rate. The current expiration however, having lasted over three years, is likely to affect exporters' willingness to comply with the RoO requirements and pay the costs without receiving the benefits. This is especially the case when the retroactive compensation is not guaranteed (Hakobyan 2020).

To summarize, it is a priori uncertain if the GSP program leads to higher trade due to the RoO strictness and the existence of other PTAs that might overshadow its effect. Assuming there is a positive effect, the impact of an expiration is also a priori unclear given the fairly short expiration spells and the retroactive refund of tariffs. Based on past literature, however, we expect to find a reduction in imports as a result of expiration.

3 Data

In this chapter, we discuss our data, some summary statistics and the main changes to the US GSP program.

3.1 Summary statistics

We use US trade data from the US Census Bureau aggregated by Schott (2008) for use in his 2008 paper. It includes the value of imports for consumption and general imports to the US from all countries between 1989 and 2022 at the 10-digit HTS code level, which provides further detail beyond the international HS code used for global trade. We use the years 2018 to 2022 in our thesis. This is due to a short expiration in 2018. If we included the years prior to this expiration, there would be multiple periods where a treatment could be estimated which would distort our results. We use the list of GSP-eligible countries and territories in 2020 from Wong (2023). The lists of AGOA and Caribbean Basin Initiative (CBI) countries we use are also as defined in 2020 (United States Trade Representative 2020; United States Trade Representative 2024a). A comprehensive list of the countries and the programs they are eligible for can be found in Tables A3 and A4 in the Appendix. We define countries as all US trading partners, which means non-independent countries and territories recognised by the United States are included.

For the GSP eligible products, we have compiled a list of the HTS codes for which GSP countries were granted preferential access in US Trade data in 2019 (*DataWeb USITC* 2024). Using this list we created a dummy variable for product groups that are eligible regardless of whether or not GSP was used. There is a possibility that some GSP eligible products were not traded in 2019 and thus are not counted as GSP products. We argue that if a product was not imported from any trading partner in 2019, the impact it would have if included would be negligible.

In order to use panel data methods in our analysis, we aggregate the city-level imports to national imports. Since not all countries export all goods to the US each year our panel is unbalanced. We use imports for consumption in our analysis because general imports include products that may have been shipped abroad and therefore never entered the US. The dataset does not include zero import observations. If a product was not imported from a country in a certain year, the observation is not in the dataset. There are some zero-value observations as a result of a non zero-value general imports. Since these observations are only a result of the data structure, they are not considered part of the analysis and thus removed from the sample. The impact of excluding these observations is tested in Appendix Table B1. We find that eliminating these observations does not have any significant impact.

Four countries that have had significant changes in their GSP eligibility: Thailand, Ukraine, Turkey and India have been removed completely from the data. These changes are documented in Table A1 in the Appendix, and we test the validity of their exclusion in Chapter 6.2. We discuss changes to the program in Chapter 3.2.

Out of the 229 countries in our data, 116 were eligible for the GSP program. In our sample, however, we define GSP countries as countries that are part of only the GSP program and no other alternative PTAs, leaving us with only 67 GSP countries in our sample. These alternative programs, such as the AGOA, the CBI as well as

country-specific programs for Nepal and Haiti, cover similar products to the GSP program. Countries that are part of both could therefore start using their specific program for exports after the expiration of the GSP, distorting the effect of expiration.

Table 1: Products covered in country groups

	GSP	AGOA	CBI
By own program	3022	1895	513
Common with GSP		777	239
Only covered by GSP		1105	325

Table 1 summarises the overlap of products between the programs. The GSP covers the most products at 3022 with AGOA and CBI at 1895 and 513 respectively. There is a large overlap between the GSP and the two other programs, with 777 products between AGOA and GSP and 239 between CBI and GSP. Interestingly, the 40 AGOA countries, all of which are GSP countries, exported 1105 products categories covered by GSP but not AGOA. The same number for CBI countries is 325, but in this case, only 8 of the 17 CBI countries are included in the GSP. Given that AGOA and CBI export a non-negligible number of GSP-exclusive products, an expiration is likely to have an impact. Still, due to their alternative preferential access, the effect of expiration is likely to be lower than for other countries. We test this hypothesis in Chapter 6.5 by including eligible AGOA and CBI countries in the GSP category.

Table 2: Summary statistics 2019

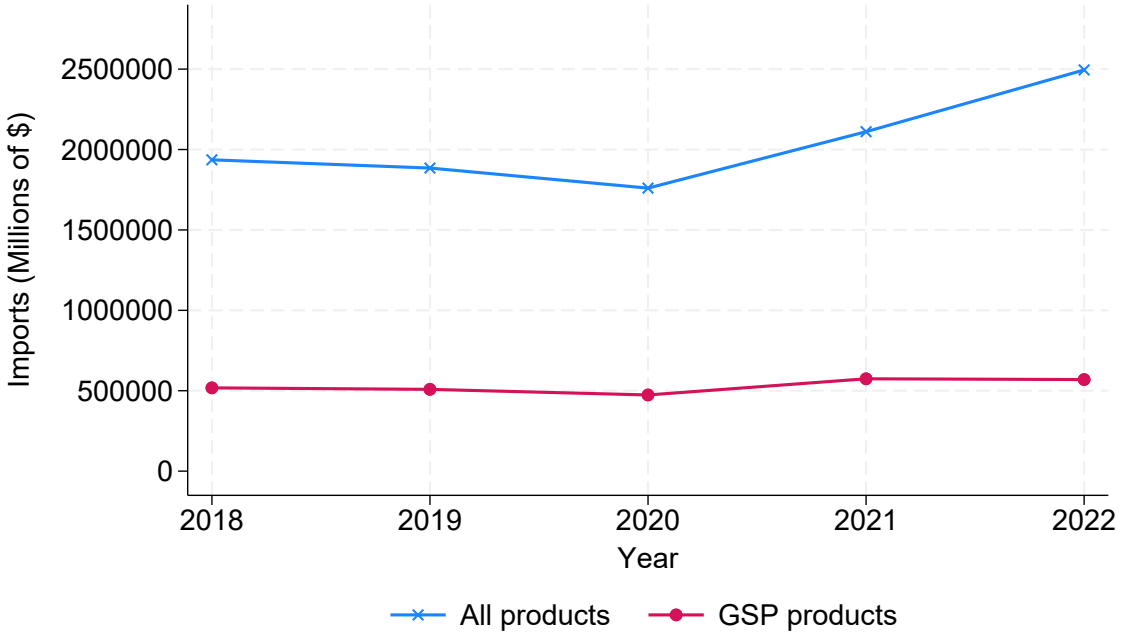
	GSP Countries (67)			
	Log (mean)	Log (st. dev.)	N Products (mean)	N Products
All Products	17.18	3.54	156	10 434
Non-GSP Products	14.82	3.85	116	7744
GSP Products	16.93	3.54	40	2690
	Non-GSP Countries (162)			
	Log (mean)	Log (st. dev.)	N Products (mean)	N Products
All Products	19.33	3.54	110	17 802
Non-GSP Products	16.53	4.12	90	14 587
GSP Products	19.16	3.52	20	3216
	All Countries (229)			
	Log (mean)	Log (st. dev.)	N Products (mean)	N Products
All Products	18.72	3.63	78	17 867
Non-GSP Products	16.05	4.11	64	14 664
GSP Products	18.53	3.66	14	3223

Table 2 reports the summary statistics for US imports in the benchmark year of 2019. There were a total of 17 867 product categories at the HTS level that were imported to the US. Of these, 14 664 were non-GSP products and 3223 were GSP products. The average number of different products imported was 78 and average value of log imports was 18.72.

The US imported 10 725 different products from GSP countries with the average number of products per country being 156, of which 40 were GSP products. The average value of log imports was 17.18. Non-GSP countries export more products at 17 802, but the average per country was lower at 110. This makes sense as these countries include both OECD countries, but also small island nations and nation-dependent territories. The average value of log exports was higher at 19.33.

Figure 1 shows the general trend of US imports between 2018-2022. We see that imports were declining until 2020 and then increased thereafter. We attribute this first downturn in imports to the COVID-19 pandemic and its lockdown measures. The increase after 2020 is attributed to the post-lockdown pent-up demand (U.S Bureau of Labor Statistics 2022).

Figure 1: US imports of GSP- and non-GSP goods from 2018 to 2022



3.2 Changes to the GSP program

Between 2018 and 2022 there were a few changes to country eligibility that affect our model. The largest were the exclusions of India and Turkey in 2019, which were made ineligible for preferential access through GSP by the Trump administration. India was the largest exporter in the program at the time, with 25% of GSP imports coming from India in 2018 (Chauhan 2020). The reason invoked for removing India's preferential treatment was that they had not assured the US equitable and reasonable market access. Turkey was the 6th largest GSP exporter at the time and was removed because of its increased level of economic development (Akhtar and Jones 2019).

There have also been several changes done to the list of GSP-eligible products by the United States Trade Representative (USTR). These changes can be country-specific leading to large variation between countries in the amount of products and trade value covered by the program. This makes these changes difficult to control for. The most notable example is Thailand which received two GSP eligibility reviews from the USTR in 2019 and 2020. In 2019, the USTR suspended GSP eligibility on products with a total value of $1.2 billion. This suspension affected 573 products at the HS-8 level. One of the most notable removals was that of all seafood products. The reason for the removal of these products, according to the US government, was the lack of worker rights and collective bargaining rights. In 2020 another $817 million were to be removed from December 30, 2020 onward. The stated reason for this removal was that Thailand had failed to provide the US with equitable market access to pork products. This suspension coincided with the expiration of the program, so the removal had no effect on trade except through the potential retroactive refund (EY 2019; EY 2020).

In the 2019 review, changes were also made to Ukraine's GSP eligibility. The US reinstated GSP eligibility on 148 product groups at the HS-8 level code. The cited reason was the passing of legislation that introduced collective management organisations to the country that secures rights for US intellectual property owners, such as writers and musicians.

To simplify analysis we assume that all GSP products are eligible for every GSP country, which is not necessarily true. Countries are eligible for different products either through country-specific exclusions like discussed above or through the CNL which removes GSP eligibility on a product from a country if its exports of that product exceeds the $210 million threshold (Williams and Alghazali 2024). See Appendix Table A2 for a full list of CNL exclusions between 2018-2022. These products are still marked as GSP products in our dataset.

Our sample includes a short GSP expiration from January 1, 2018 to April 22, 2018 (Wong 2023). As with the earlier expiration, it was followed by a retroactive refund of tariffs paid during this period. Since the period of expiration was short, trade patterns would likely not be changed and therefore not lead to any long-term effects. We test this hypothesis in Chapter 6.6.

To summarise, we assume that GSP products and countries are constant during our sample period. India and Turkey were removed completely from the program. Thailand had one third of their exports made ineligible for preferential access and Ukraine got their preferential access reinstated. These countries would act as already treated in our model and distort the results. As such, they are consequently removed from the sample. In Chapter 6.2, we analyze the validity of this procedure.

4 Estimation Method and Model Specification

In this chapter, we explore challenges connected to estimating the effect of policy in the presence of heterogeneous shocks. Then, we explain our decision to use a DiDiD model over a conventional difference in differences (DiD) approach.

4.1 Estimation method

The gold standard for causal inference is a truly randomized experiment in which treatment is randomly assigned in the population. In the absence of this, a natural experiment can be exploited for the same aim. We argue that the expiration can be regarded as a natural experiment because it was not connected to the actions of the beneficiary countries. "A natural experiment occurs when some exogenous event - often a change in government policy - changes the environment in which individuals, families, firms or cities operate" (Wooldridge 2018, p.434). A common approach when researching natural experiments is the DiD method. This method enables us to assess the impact of a policy by comparing control and treatment groups and examining the varying responses to policy changes between the two groups, all without needing a true randomized experiment.

In our data, however, there is likely significant heterogeneity present due to the consequences of COVID-19. This makes the estimates of a DiD regression analysis less convincing. To account for these heterogeneities, we include an additional control group by using a DiDiD specification inspired by Frazer and Biesebroeck (2010). We include interactive fixed effects for product-country, product-year and country-year effects to allow for heterogeneous shocks. Due to the additional control group and fixed effects, we argue that this triple difference model gives us a more accurate result than the double difference.

4.2 Model specification

We use the following triple difference model specification:

$$lnImports_{cpt} = \beta_1 GSPcountry_c \times GSPProduct_p \times Expired_t + \gamma_{cp} + \theta_{ct} + \delta_{pt} + \epsilon_{cpt} \quad (1)$$

The dependent variable is the logarithm of US imports in dollars of product p from country c in year t. The explanatory variable is the triple difference $GSPcountry_c \times GSPProduct_p \times Expired_t$. $GSPCountry$ is a dummy variable with value 1 if the import comes from a GSP country and 0 otherwise. $GSPProduct$ is a similar dummy for GSP products and $Expired$ is a dummy with value 1 for years after expiration. We control for country-product γ_{cp}, country-year θ_{ct}, and product-year δ_{pt} interactive fixed effects. These interactive fixed effects allows us to control for trends across the three variables, which is crucial given the likely heterogeneity due to COVID-19.

The triple difference estimator is given by:

$$\begin{aligned}\beta_1 = &((lnImp_{Post}^{EC,EP} - lnImp_{Pre}^{EC,EP}) - (lnImp_{Post}^{EC,NP} - lnImp_{Pre}^{EC,NP})) \\ &-((lnImp_{Post}^{NC,EP} - lnImp_{Pre}^{NC,EP}) - (lnImp_{Post}^{NC,NP} - lnImp_{Pre}^{NC,NP}))\end{aligned} \quad (2)$$

Where EC and NC designate eligible and non-eligible country respectively. The same goes for EP and NP when it comes to product eligibility.

The DiDiD model we use, starts by looking at the difference between imports of GSP products and non-GSP products from GSP countries before and after expiration. This difference in differences by itself cannot be interpreted as the effect of expiration on imports of GSP goods since the estimated effect might be caused by a global trend impacting all products regardless of GSP eligibility. We therefore use the equivalent difference in non-GSP countries as a control group. By differencing these two differences, we account for trends across products and countries that are not due

to the expiration, but impact the estimated effect. The estimate we arrive at, will therefore control for trends unaccounted for by other approaches.

The intuition behind the DiDiD approach is that we use the trends in the control group as the counterfactual scenario: if the program had not expired. This approach relies on the assumption that the trends in imports between the two product groups are the same in GSP and non-GSP countries. We discuss the validity of the parallel trends assumptions in Chapter 5.2.

5 Results

In this chapter, we go through our main regression analysis. Then, we examine the validity of the parallel trends assumption in our model, along with potential spillover effects.

5.1 Main regression and analysis

Estimations are conducted using the Stata packages *reghdfe* (Correia 2016) and *pplmhdfe* (Correia, Guimarães, and Zylkin 2020) in order to control for the high number of fixed effects variables. These estimation methods remove fixed effects groups with only one observation, called singletons. Correia (2015) finds that their inclusion in models with fixed effects can overstate significance and lead to incorrect inference. All regressions are conducted with standard errors clustered on country and product when these are included in the model specification.

Table 3: Results from main regression

VARIABLES	(1) OLS	(2) Main Triple Difference	(3) Triple Difference All Countries	(4) Double Difference
Marginal Effect	-32.82%	-4.61%	-1.58%	
GSPCountry x	-0.398	-0.0472*	-0.0160	
GSPProduct x	(0.290)	(0.0261)	(0.0273)	
Expired				
Marginal Effect				9.53%
GSPCountry x				0.0910**
Expired				(0.0358)
Constant	11.40***	11.67***	11.68***	11.67***
	(0.163)	(0.000328)	(0.000394)	(0.00170)
Observations	1,496,248	1,312,807	1,426,999	1,312,844
Adjusted	0.000	0.873	0.871	0.871
R-squared				
p-value	0.172	0.072	0.560	0.012
Fixed Effects		country-year country-product product-year	country-year country-product product-year	country-product product-year

Notes: Dependent variable is lnImports. Standard errors are clustered at country and product level.
Marginal effects calculated as $(exp(\beta)\text{-}1) \times 100$.
*** p<0.01, ** p<0.05, * p<0.1

The results from the main regression are shown in Table 3. Column (1) reports the result from a basic OLS with no fixed effects. The estimated effect of the triple difference is a reduction in imports of 32.82% post-expiration. The estimate is not significant at a 10% level with a p-value of 0.172 and can therefore not be said to be significantly different from zero.

Column (2) show our main triple difference model. Here the countries of India, Turkey, Thailand and Ukraine are removed from the sample. The estimated effect is -4.61%, which we can interpret as the reduction in imports of GSP products from GSP

countries post-expiration. This effect is significant at the 10% level with a p-value of 0.072. This is consistent with our expectations and indicates that the GSP program did increase trade and that its expiration had an impact. The effect is also of a similar magnitude to the 3% reduction in imports of eligible products found by Hakobyan (2020) during the 2011 GSP expiration.

The result from a regression with all countries included is shown in column (3). As explained in Chapter 3.2, these countries were included or excluded from eligibility during the sample period. Therefore, including them in the sample would bias the result towards zero, which is what we observe. The estimated effect is -1.58% but is not significant at the 10% significance level with a p-value of 0.560. We discuss the validity of excluding these countries further in Chapter 6.2.

In column (4) we run a difference-in-difference model. The explanatory variable here is $GSPCountry \times Expired$ instead of the full triple difference model. Due to this, we can only include country-product and product-year fixed effects. The estimated effect is now positive and both economically and statistically significant with an increase in imports of 9.53% significant at the 1% level. Since the specification is different, so is the interpretation. Here, we estimate the effect of expiration on the total imports from GSP countries. While this result might be correct given the specification, the effect cannot be interpreted directly as the effect of the GSP expiration. It is more likely that the change is largely due to post-COVID recovery, which happened almost simultaneously with the expiration, the implications of which will be further discussed in Chapter 5.2. This result, however, illustrates the necessity for the triple difference specification and the three interactive fixed effects. Results of different specifications must thus be interpreted and compared with caution.

Comparing the results from DiD in column (4) with the results from the main DiDiD in column (2) shows that despite total imports from GSP countries increasing after 2020, imports of GSP products from GSP countries fell. More specifically, imports of GSP products were lower than they would have been in the absence of expiration.

These results should be interpreted as the average treatment effect. This implies that the effect is likely to differ within the treatment group with some countries or products being affected more or less than the average. The impact will depend on factors such as the GSP utilization rate. A country that did not use the program to begin with, will be less affected than a country that depends on it. As a robustness test in Chapter 6.4 we remove LDC countries from the treatment, which might have less diverse exports. Since the results are only valid for the current specification, they cannot be applied to other programs like the EU GSP, nor to other scenarios like the withdrawal of preferences from specific countries.

5.2 The parallel trends assumption

Our analysis relies on the validity of the parallel trends assumption. The assumption implies that if the treatment and control group follow the same parallel trend pre-treatment, they would have continued to do so in the absence of treatment. If the treatment and control group do not follow the same trend before treatment, then the approach is invalid. In that case, the results from a DiD or DiDiD estimation cannot be interpreted as causal. This is due to the method's use of the control group's post-treatment trend as counterfactual. While there is no test to outright verify its validity, we discuss its likelihood based on theory, graphical evidence as well as a placebo test.

The likelihood of parallel trends is higher if the treatment group and control group are similar. In our case these groups are different by design. The GSP program is

designed to help developing countries be competitive against developed economies by reducing tariff costs. While the decision of which countries are included is ultimately political, it is, in theory, made based on which countries need assistance and which do not.

Another significant issue regarding the validity of the parallel trends assumption is the approximately coinciding outbreak of COVID-19 and its many consequences. The impact was not the same for all countries or product categories and thus introduces heterogeneity to imports. Imports fell in 2020 as a result of lockdown, but quickly increased in 2021 and 2022 (Bureau of Economic Analysis 2024). The coinciding post-pandemic boom and GSP expiration makes it difficult to distinguish between the two effects on imports. Since all countries were impacted to different degrees, we are unable to know the counterfactual trend in the absence of COVID-19. These factors make the effect of the US GSP expiration difficult to estimate.

We could imagine that developing countries, which mostly make up GSP-eligible countries, and developed countries, which are mostly non-GSP countries, had different responses to the pandemic that would affect their export capacity. If GSP countries had fewer resources to control the spread and thus had to rely on lockdown, their production capacity would be reduced more than in the European Union or Canada. In a standard DiD across $Country \times Expired$, this could be interpreted as the effect of GSP expiration and the estimate would be downward biased. Alternatively, credit constraints in developing countries could mean they had no choice but to maintain production while developed countries could choose to limit it. This would reversely give the estimate an upward bias.

This concern is alleviated by including an additional control group. While total imports from the two country groups are likely to differ, Olden and Møen (2020) show that in a DiDiD, only one of the difference in differences needs to have parallel trends

for the whole DiDiD model to have parallel trends. It requires that the relative growth of GSP products and non-GSP products trend in the same direction in both GSP and non-GSP countries. These differences are shown in Figure 2 and 3.

Figure 2: Parallel trend: GSP countries

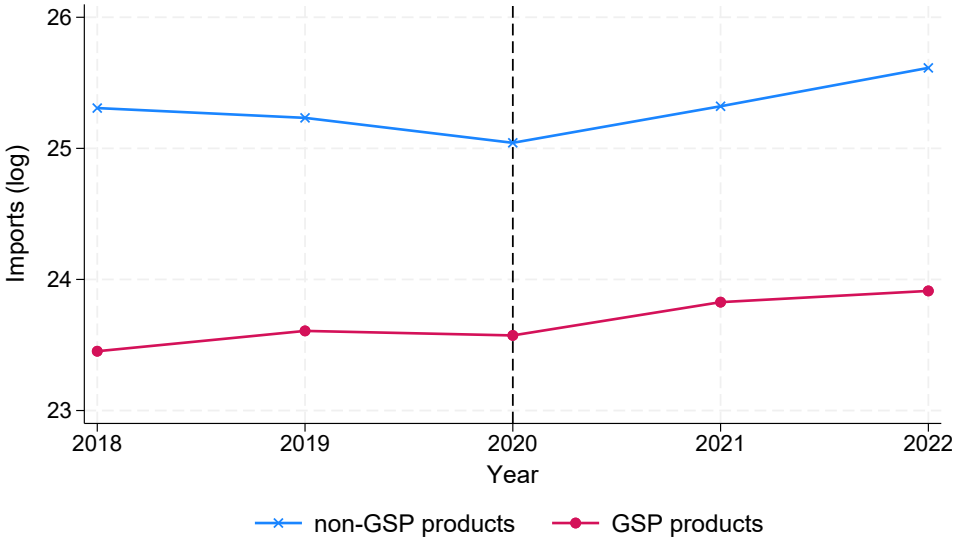


Figure 3: Parallel trend: non-GSP countries

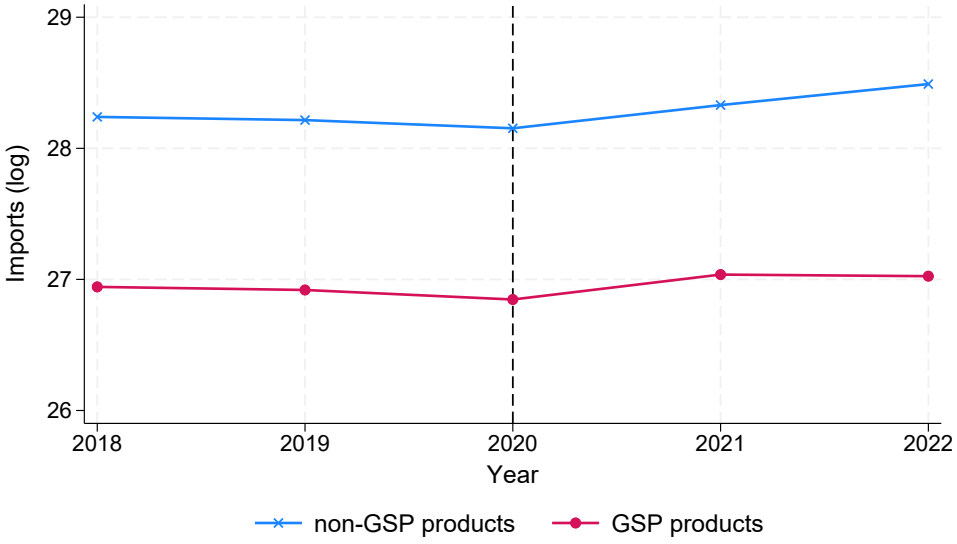


Figure 2 compares imports of GSP and non-GSP products from GSP countries over the years. Figure 3 does the same for non-GSP countries. In both figures, the two curves have an approximately parallel trend before expiration. The biggest gap is in 2018 in GSP countries. This can be explained by the short expiration period from January to April of that year. We see a reduction in imports in both product groups in 2020 coinciding with the pandemic. Post-expiration however, we see a clear difference in trends between the product groups within both country groups, especially in 2022. This lends credibility to the parallel trends assumption.

We also conduct an event study analysis to further verify the validity of the triple difference approach. We specify a treatment dummy for each year $GSPCountry \times GSPProduct \times year_t$ as opposed to the general $GSPCountry \times GSPProduct \times Expired$. We use the following equation:

$$lnImports_{cpt} = \beta_1 GSPcountry_c \times GSPProduct_p \times year_t + \gamma_{cp} + \theta_{ct} + \delta_{pt} + \epsilon_{cpt} \quad (3)$$

The idea is to find out if the treatment is estimated to have had an effect before its implementation. If this is the case, the parallel trends assumption is weakened as the treatment group would have significantly differed from the control group prior to treatment.

The results from the regression where the year of expiration, 2021, is omitted, are reported in Table 4.

Table 4: Results from placebo regression

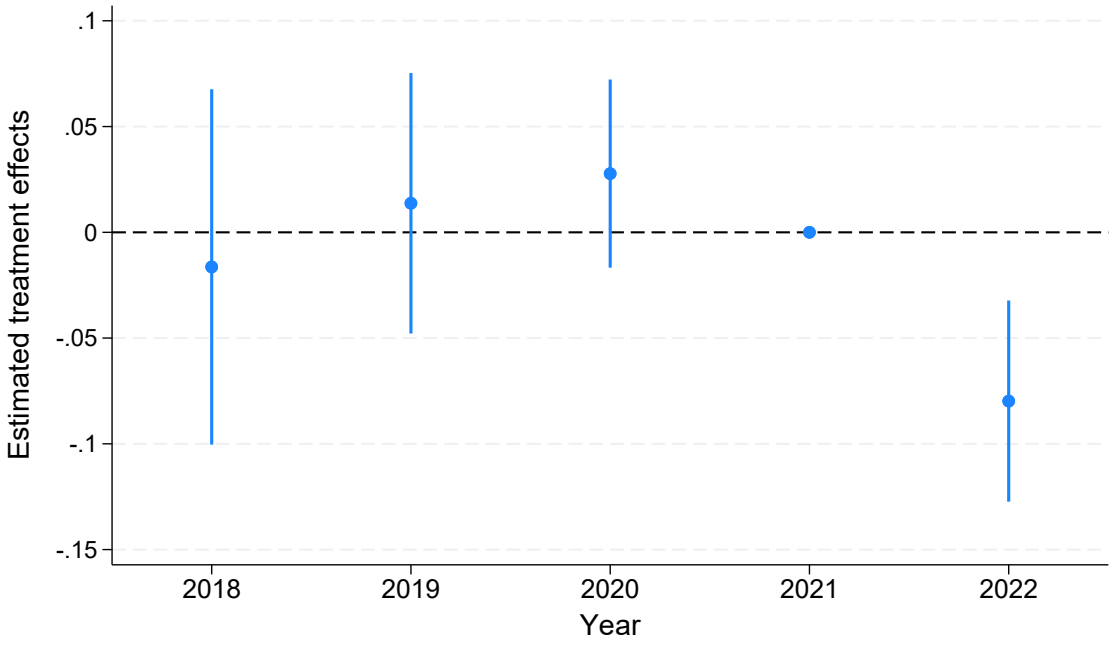
VARIABLES	(1) Placebo
GSPCountry x *GSPProduct* x *2018*	-0.0164
	(0.0426)
GSPCountry x *GSPProduct* x *2019*	0.0137
	(0.0313)
GSPCountry x *GSPProduct* x *2020*	0.0278
	(0.0226)
omitted	
GSPCountry x *GSPProduct* x *2021*	-
GSPCountry x *GSPProduct* x *2022*	-0.0798***
	(0.0241)
Constant	11.67***
	(0.000589)
Observations	1,312,807
Adjusted R-squared	0.873
Fixed Effects	country-year country-product product-year
F-test 2018 2019 2020	
Prob > F	0.3829

Dependent variable is lnImports. Standard errors are clustered at country and product level.
*** p<0.01, ** p<0.05, * p<0.1

We see that the estimated effect of the expiration is not significantly different from zero in any of the years prior to the actual expiration. The p-value from the joint test of significance is 0.38. We reject the hypothesis that the expiration had an effect prior to its implementation at any reasonable level. It is unlikely that exporters anticipated the withdrawal and changed their behavior because of this. The effect post-expiration on the other hand is both economically and statistically significant with an estimate of 8%.

Using these results, we construct a graph visualizing the estimated effect for each year:

Figure 4: Placebo test on main sample



We see from Figure 4 that all estimates of the treatment effect are not significantly different from zero prior to the treatment starting in 2021. Together with the F-test, this reinforces the parallel trends assumption.

Based on these points, we argue that the parallel trends assumption is valid and the estimated effect can be interpreted as causal.

5.3 Spillover effects

A last potential weakness of the parallel trends assumption is the existence of spillover effects. Assuming the expiration of the GSP programs leads to imports of GSP products falling from eligible countries, the parallel trends assumption is valid as long as the imports of GSP products from non-GSP countries do not increase in response. If we observe positive spillover like described above, the estimated effect will be larger, meaning more negative, in our triple difference approach.

We analyze the possibility of spillover effects by conducting two DiD estimations. The first one on only GSP countries, the other on only non-GSP countries. The DiD estimate is $GSPProduct \times Expired$, such that we look at the effect of imports of GSP products before and after expiration in both countries.

Table 5: Results of spillover test

VARIABLES	(1) Only GSP Countries	(2) Only non-GSP Countries
Marginal Effect *GSPProduct* x *Expired*	2.04% 0.0202 (0.0247)	4.95% 0.0483*** (0.0103)
Constant	11.15*** (0.00266)	11.73*** (0.00113)
Observations Adjusted R-squared	153,247 0.834	1,167,012 0.875
p-value	0.417	0.000
Fixed Effects	country-year country-product	country-year country-product

Notes: Dependent variable is lnImports. Standard errors are clustered at country and product level.
Marginal effects calculated as $(exp(\beta)\text{-}1) \times 100$.
*** p<0.01, ** p<0.05, * p<0.1

From the DiD regression in Table 5 on only GSP countries, the estimated effect is

positive of around 2%, but not significantly different from zero. Based on this model specification, the expiration of the GSP program had seemingly no impact on the import of GSP products compared to non-GSP products from GSP countries.

In non-GSP countries, we find a large and significant estimated effect of 4.95%. This would indicate that imports of GSP products from non-GSP countries increased significantly after the expiration compared to non-GSP products, which corresponds with a spillover effect.

These estimates are difficult to compare to our main results because they do not account for the full set of interactive fixed effects. Due to the post-COVID demand shock that increased imports sharply beginning in 2021, the omission of the full set of fixed effects means the effect of COVID-19 and the expiration are difficult to disentangle.

It still seems likely however, that there is a spillover effect in our data that would give a negative bias to our results. The extent of this bias is unknown, but we believe the direction of our findings is not invalidated.

On the other hand, the inclusion of the product-year interactive fixed effect in the main model, equating to the $GSPProduct \times Expired$ effect in Table 5, implies that any shock that happened during the period is controlled for. This would imply the spillover effects are controlled for and thus should not change the estimated effect. Still, while the estimated effect might not be biased, the economic implications are real. If we assume that productivity comes from experience, in essence learning by doing, the transfer of the industry from GSP to non-GSP countries can make the transition back difficult and will then harm the industry in GSP countries permanently.

6 Robustness Checks

In order to test the result from the main model, we conduct several robustness tests to validate our findings. We start by conducting a Poisson pseudo maximum likelihood (PPML) regression analysis, then modify the sample in the main model in the following ways: excluding specific countries with changing eligibility status from the sample, removing LDCs from the sample, including GSP eligible AGOA and CBI countries in the GSP beneficiary category and removing 2018. Finally, we compare the estimation results using imports for consumption and general imports.

6.1 Poisson pseudo maximum likelihood estimation

To start off, we estimate the main model using a PPML regression on our model. Silva and Tenreyro (2006) propose that this method produces less biased results than OLS in the presence of heteroscedasticity in a log-lin model. This method is used by Tanaka (2022) and Gnutzmann and Gnutzmann-Mkrtchyan (2022) as the main estimation method in their work.

Table 6: Results from PPML estimation

VARIABLES	(1) Main Triple Difference	(2) PPML
GSPCountry x *GSPProduct* x *Expired*	-0.0472* (0.0261)	0.000810 (0.0423)
Constant	11.67*** (0.000328)	19.86*** (0.000159)
Observations	1,312,807	1,312,807
Adjusted R-squared	0.873	
Fixed Effects	country-year country-product product-year	country-year country-product product-year

Notes: Dependent variable is lnImports in (1), Imports in (2).
Standard errors are clustered at country and product level.
*** p<0.01, ** p<0.05, * p<0.1

The estimated coefficient for the triple difference using PPML is reported as approximately 0.08 in column (2) of Table 6. The estimate is economically insignificant and cannot be said to be different from zero. The PPML method puts more weight on larger observations, so it is not surprising that the estimate differs from our main specification. However, given that other papers have found estimates consistent with expectations from PPML estimation, our results are made less credible. The fact that the estimated effect is not significantly different from zero is interesting and weakens the conclusion from the main regression.

6.2 Removing countries with eligibility changes

We also test the robustness of our model by analyzing the validity removing India, Turkey, Thailand and Ukraine. As previously discussed, since the eligibility of these countries was changed during the sample period, their inclusion could bias the results. To verify the validity of these exclusions, we test the removal of other countries whose eligibility has been constant and compare the results to the main model with all countries included. These countries remained in the same category and thus their removal should not impact the result significantly.

Egypt, Brazil and Vietnam are the countries we use for testing. These countries cover three continents and are large enough to impact the estimates. Lastly, they cover both GSP and non-GSP countries as Egypt and Brazil are eligible, but Vietnam is not. We test removing the countries together in Table 7.

Table 7: Removing GSP countries

VARIABLES	(1) Turkey, India, Ukraine, and Thailand removed	(2) All Countries	(3) Egypt, Vietnam, and Brazil removed
GSPCountry x *GSPProduct* x *Expired*	-0.0472*	-0.0160	-0.00758
	(0.0261)	(0.0273)	(0.0314)
Constant	11.67***	11.68***	11.67***
	(0.000328)	(0.000394)	(0.000382)
Observations	1,312,807	1,426,999	1,366,154
Adjusted R-squared	0.873	0.871	0.872
p-value	0.072	0.560	0.810
Fixed Effects	country-year country-product product-year	country-year country-product product-year	country-year country-product product-year

Notes: Dependent variable is lnImports. Standard errors are clustered at country and product level.
*** p<0.01, ** p<0.05, * p<0.1

From column (3) in Table 7, we can see that the model with other countries removed weakens the estimated effect both statistically and economically, with a new statistically insignificant coefficient of 0.7. The lower estimated effect in column (3) makes the exclusion of the four countries from the main specification more convincing.

There is still the possibility that the difference we observe are due to randomness in the countries we picked. Maybe the countries we remove all contribute strongly to the significance of the results, while the countries we exclude as a test all randomly contribute to reducing significance. To analyze this, we have removed all the countries individually in Appendix Table B2. While removing Brazil stands out with a low estimated coefficient of -0.8, removing Egypt leads to a much stronger result of -1.9. There is also variation in the results of the countries excluded in the main model, with the removal of Thailand having the greatest effect on the estimate at -3. The

variation in estimates suggests that the effect of removal is not due to the random selection of insignificant countries.

6.3 Removing OECD countries

A concern for the estimation is the fact that the treatment and control groups are dissimilar. We argue that the OECD countries in the control group, being mainly highly developed economies, make the comparison to the treatment group less credible. We conduct a test removing all 38 OECD countries from our sample. The intuition is that the remaining control group will be more comparable so that the estimation returns more credible estimated effects.

Table 8: Removing OECD countries

VARIABLES	(1) Main Triple Difference	(2) Without OECD
Marginal Effect	-4.61%	-9.25%
GSPCountry x *GSPProduct* x *Expired*	-0.0472*	-0.0971***
	(0.0261)	(0.0307)
Constant	11.67***	11.59***
	(0.000328)	(0.00101)
Observations	1,312,807	497,023
Adjusted R-squared	0.873	0.864
p-value	0.072	0.002
Fixed Effects	country-year country-product product-year	country-year country-product product-year

Notes: Dependent variable is lnImports. Standard errors are clustered at country and product level.
Marginal effects calculated as $(exp(\beta)\text{-}1) \times 100$.
*** p<0.01, ** p<0.05, * p<0.1

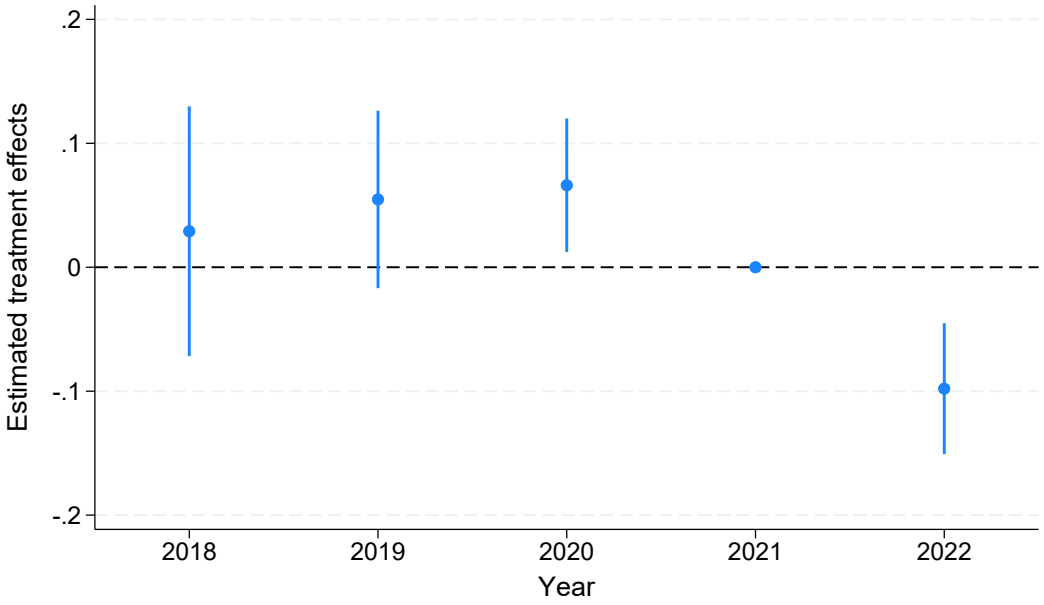
The results from the new model are reported in column (2) of Table 8. The new estimated effect is -9.25% and highly significant, even at the 1% level, compared to -4.61% for the base model which is only significant at the 10% level.

The stronger estimated effect makes intuitive sense. By removing the wealthiest countries from the sample, we are left with countries which are more comparable to the treatment group. The triple difference method then estimates the effect when compared to this new control group. A larger share of the remaining control group countries are recipients of other PTA programs. When compared to these countries which still have preferential access, the effect of the expiration is stronger. We could argue that the result in (2) is more valid than the main estimated in (1) due to the more comparable treatment and control groups, but it could also overestimate the effect if the groups are not as similar as our intuition would suggest.

We analyze this question further by presenting the parallel trends for this new sample and conducting a placebo test similar to the one done in Chapter 5.2. Figures B.1 and B.2 in the appendix show the parallel trends. Figure B.1 is identical to Figure 2 since no change has been made to the GSP category. Figure B.2 is different, but still seems to have parallel trends. Again, the graphs cannot be interpreted credibly by themselves.

In Figure 5, we have conducted an event study to further test the validity of this model. Using the baseline year of 2021, the graph indicates an effect significantly different from zero in 2020. However, since the effect is relative, it only tells us that the difference in effect between 2020 and 2021 is significant. If we use 2020 as a baseline, the graph would be just as expected. The estimated effect seems to increase until 2020 which is likely due to trade increasing after the 2018 expiration. Still, this increase is so slow that it cannot credibly be attributed to the 2021 expiration itself. All in all, the placebo test does not indicate pre-expiration effects, thus we assume that the parallel trends hold.

Figure 5: Placebo test on sample without OECD



If the expiration had been expected by the importers and exporters, it would be a problem for the validity of the parallel trends assumption. As described in Tanaka (2022), anticipation of the end of a preference program is likely to lead to increased exports in the last period preference is available. If the expiration of the GSP program was predicted by American importers prior to it happening, it could have triggered a last-minute import boom from the eligible countries. There are indications that the current long-term expiration was not expected, namely that there were changes made to the list of eligible products as late as November 2020 (EY 2020). Although this does not exclude the possibility of importers anticipating it, neither the expiration nor its duration was signaled by the authorities.

We argue that the exclusion of OECD countries is a credible specification due to unlikely pre-treatment effects. The exclusion leads to stronger, more significant estimates as expected.

6.4 Removing LDC countries

We also conduct a robustness check by removing all eligible least developed countries, non-independent countries and territories from the sample. The intuition is that these smaller countries to a lesser degree are able to utilize the GSP program due to smaller markets and more significant trade barriers. This idea is based on Sytsma (2021) who finds that LDCs are less affected by revision in the RoO strictness. By removing these generally smaller economies from the sample, we expect to find that the estimated effect is stronger.

Table 9: Removing LDC and territories

VARIABLES	(1) Main Triple Difference	(2) LDC and Territories Removed	(3) LDC Removed
Marginal Effect	-4.61%	-5.41%	-5.37%
GSPCountry x *GSPProduct* x *Expired*	-0.0472* (0.0261)	-0.0556** (0.0259)	-0.0552** (0.0259)
Constant	11.67*** (0.000328)	11.69*** (0.000311)	11.69*** (0.000312)
Observations	1,312,807	1,282,012	1,282,817
Adjusted R-squared	0.873	0.874	0.874
p-value	0.072	0.033	0.034
Fixed Effects	country-year country-product product-year	country-year country-product product-year	country-year country-product product-year

Notes: Dependent variable is lnImports. Standard errors are clustered at country and product level. Marginal effects calculated as $(exp(\beta)\text{-}1) \times 100$.
*** p<0.01, ** p<0.05, * p<0.1

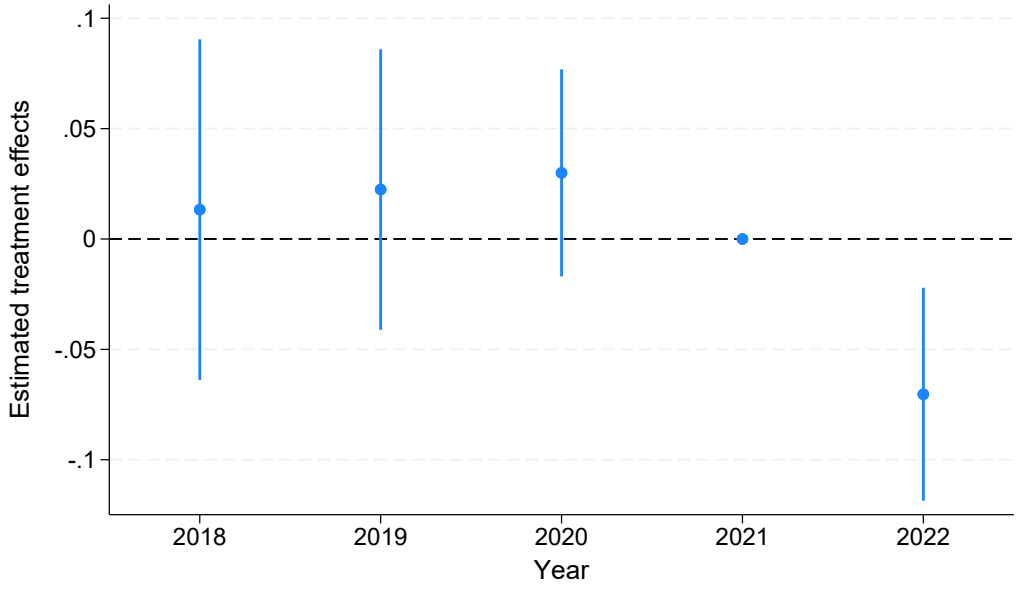
The results from this test are reported in Table 9. In column (2) we report the regression without LDC and territories, while (3) is only without LDC. The reported

results for both columns are similar, as expected due to the negligible impact of the small territories. Focusing on column (2), we find a stronger coefficient than in our main model. The new estimated effect is -5.41% and is significant at the 5% level with a p-value of 3.3%.

These results confirm our intuition. The LDC countries and territories are less impacted by the expiration and thus their removal leads to a stronger estimated effect.

We again conduct an event study to demonstrate the validity of the triple difference model when excluding LDCs and territories shown in Figure 6.

Figure 6: Placebo test on sample without LDCs and territories



It is evident from the figure that before 2021, the treatment effects are not statistically different from zero, reinforcing the validity of the parallel trends assumption. The results therefore seem to be credible with our strongest explanation being that the expiration of the GSP program had a greater impact in more developed economies due to their greater ability to utilize it.

6.5 Including AGOA and CBI

We have, in our model, excluded countries that are also part of alternative PTA programs such as AGOA and CBI from the GSP category. The intuition is that these countries have access to an equivalent preference program they can switch to when the expiration came into effect. As such, they will be affected to a lesser degree than other beneficiary countries that do not have this possibility. We now test if this exclusion is reasonable and analyze the effect of including these countries.

Table 10: Including AGOA and CBI in the GSP category

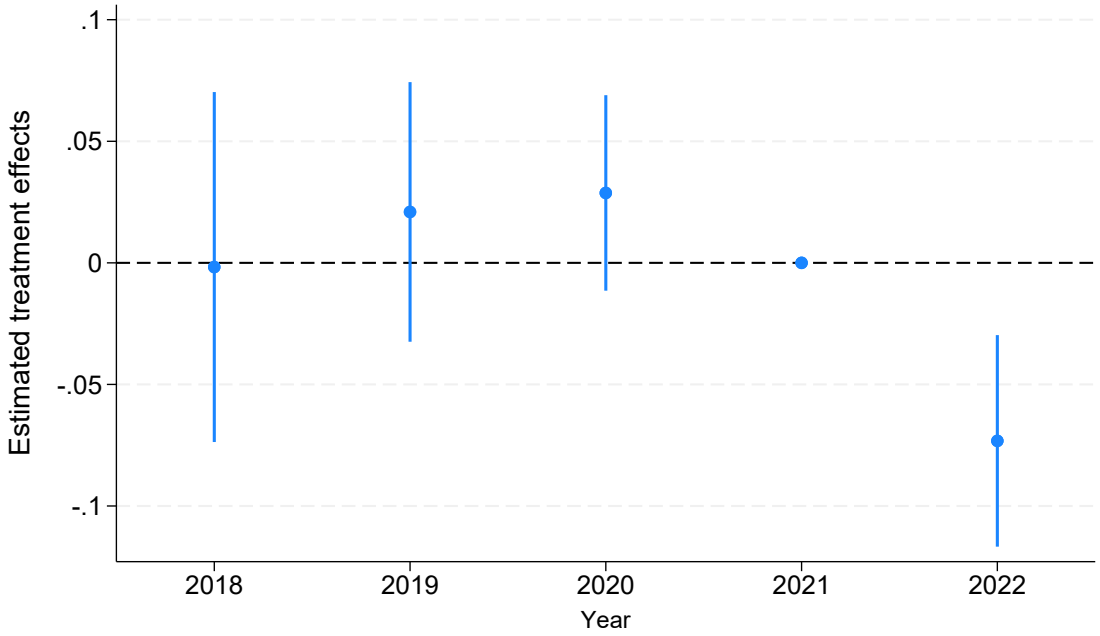
VARIABLES	(1) Main Triple Difference	(2) With AGOA and CBI
Marginal Effect	-4.61%	-5.00%
GSPCountry x *GSPProduct* x *Expired*	-0.0472*	-0.0513**
	(0.0261)	(0.0231)
Constant	11.67***	11.67***
	(0.000328)	(0.000357)
Observations	1,312,807	1,312,807
Adjusted R-squared	0.873	0.873
p-value	0.072	0.028
Fixed Effects	country-year country-product product-year	country-year country-product product-year

Notes: Dependent variable is lnImports. Standard errors are clustered at country and product level.
Marginal effects calculated as $(exp(\beta)\text{-}1)$ x 100.
*** p<0.01, ** p<0.05, * p<0.1

The results are reported in Table 10. Column (2) shows the effect of the triple difference estimator when both the AGOA and CBI countries are included. The marginal effect is -5.00% and significant at the 5% level. This estimate is both stronger and more statistically significant than the estimate that excludes these countries.

Figure 7 shows the results from an event study on the treatment effects prior to 2021, none of which are significantly different from zero. The parallel trends assumption seems to hold.

Figure 7: Placebo test when including AGOA and CBI countries in the GSP category



This result goes against our original intuition and suggests that countries that had access to alternative preference programs were also affected by the expiration. The stronger effect is unlikely to be because of the additional preference programs, but is likely due to common characteristics between these countries. They could for example be generally larger and thus have higher GSP utilization rates and therefore more to lose.

The increased significance associated with including of AGOA and CBI countries challenges the assumption that exclusion of these countries from the GSP category is a better specification. We argue that neither is superior, but only differ in their

interpretation. Our main model estimates the effect on purely GSP countries while this specification estimates the effect on all GSP countries.

6.6 Removing 2018

There was a brief expiration of the GSP from January 1, 2018, to April 22, 2018, as outlined in Chapter 2.3. Given its short duration and the retroactive return of tariffs, we anticipate its impact to be negligible. A regression analysis for the exclusion of 2018 is detailed in Table 11.

Table 11: Removing 2018

VARIABLES	(1) Main Triple Difference	(2) 2019-2022
Marginal Effect	-4.61%	-6.01%
GSPCountry x *GSPProduct* x *Expired*	-0.0472*	-0.0625***
	(0.0261)	(0.0239)
Constant	11.67***	11.74***
	(0.000328)	(0.000374)
Observations	1,312,807	1,024,763
Adjusted R-squared	0.873	0.881
p-value	0.071	0.01
Fixed Effects	country-year country-product product-year	country-year country-product product-year

Notes: Dependent variable is lnImports. Standard errors are clustered at country and product level.
Marginal effects calculated as $(exp(\beta)\text{-}1) \times 100$.
*** p<0.01, ** p<0.05, * p<0.1

Column (2) presents the results after excluding data from 2018. Surprisingly, the coefficient is higher, indicating a marginal effect of -6.01%, which is statistically significant at the 1% level. This suggests that even a brief expiration has a more substantial impact on the trade than anticipated, demonstrating both economic and statistical significance of the GSP expiration.

6.7 Imports for consumption or general imports

Our estimations are based on imports for consumption rather than general imports. This choice is explained in Chapter 3.1. The dataset also contains general imports which could react differently to the expiration than imports for consumption. We therefore conduct a robustness check by conducting our main estimation model on general imports. While the two values can be slightly different on a given observation, we do not predict that it matters for the results since the difference is unlikely to be large. The results are shown in Table 12.

Table 12: Results using general imports in main regression

VARIABLES	(1) Imports For Consumption	(2) General Imports
Marginal Effect	-4.61%	-4.74%
GSPCountry x *GSPProduct* x *Expired*	-0.0472*	-0.0486*
	(0.0261)	(0.0267)
Constant	11.67***	11.67***
	(0.000328)	(0.000336)
Observations	1,312,807	1,311,933
Adjusted R-squared	0.873	0.872
p-value	0.072	0.070
Fixed Effects	country-year country-product product-year	country-year country-product product-year

Notes: Dependent variable is lnImports. Standard errors are clustered at country and product level.
Marginal effects calculated as $(exp(\beta)\text{-}1) \times 100$.
*** p<0.01, ** p<0.05, * p<0.1

The result is as expected: there is a negligible change in both the coefficient and the statistical significance. The new estimated effect in column (2) is slightly stronger at -4.74% and more significant with a p-value of 7%. The choice between the two is therefore of little importance when it comes to inference.

7 Discussion

In this chapter, we summarize the biggest challenges to inference and discuss the implications of the robustness tests. Finally, we argue that the magnitude and direction of the results found are credible.

7.1 Summary and policy implications

Estimating the effect using a DiDiD approach on our main model specification, we find an economically significant average effect of -4.61% on the imports of GSP product from GSP countries post-expiration. This result is reasonably statistically significant with a p-value of 7.2%. There are however, as discussed, multiple factors that make our findings uncertain.

The biggest challenge is the heterogeneous effects of the COVID-19 pandemic across countries and products that make estimation difficult. An example of this is found in Table 3 column (4). A simple DiD between countries that does not distinguish between product eligibility finds that imports from GSP countries increased by 10% after the expiration of the GSP, significant at the 1% level. Due to the unlikelihood that this is a direct effect of expiration, a more credible explanation is the aforementioned shocks. In order to account for these, our main regression is a DiDiD which includes the three interactive fixed effects.

There is also the problem of likely spillover effects, as documented in Chapter 5.3. Since the exports of GSP products increases from non-GSP countries after the expiration, it is likely they have taken over some of the imports of GSP countries. This spillover would increase the estimated effect of the expiration. The concern is alleviated by the inclusion of comprehensive fixed effects. Still, we cannot completely rule out the possibility of spillover effects, which weaken the inference potential of our results.

Another issue is the ever-changing nature of the GSP program. Since the eligibility for the program is decided politically, countries can be removed or included altogether like India and Turkey. Others can get large changes in what products they are eligible to sell at GSP tariffs, like Thailand and Ukraine. While these countries can credibly be removed from the sample, there are countless small changes that are difficult to control for.

The frequent expirations can also make estimation and comparison difficult. The last expiration happened in 2018 which is included in our base sample. Given the findings of Krishna et al. (2021) that GSP utilization increases with experience, the duration since the last expiration is likely a determining factor in utilization, thus creating an unpredictable situation for producers. This is shown by our test of removing 2018 in chapter 11.

Despite this, the robustness checks generally point towards the results from our main specification being credible. The removal of OECD countries from the treatment group increases economic and statistical significance of the estimated effect. Removing LDC countries and territories from the GSP category strengthens the result economically and statistically, suggesting that the LDCs are less able to utilize the GSP program and are thus less affected by its expiration. The inclusion of AGOA and CBI countries in the treatment group also makes the estimated effect stronger. Removing 2018 also reinforces the results. This is likely due to the short four month expiration from January to April, disproving the idea that since the tariffs are retroactively refunded, short expiration should not have an impact. It might be that the refunds reduce the negative trade effect of expiration, but it does not eliminate them.

The PPML estimation gives results inconsistent with the other robustness tests, reporting effects insignificantly different from zero. We argue, however, that given the consistency of the other specifications, the PPML result does not invalidate our general conclusion.

The results from the main model and the credible robustness checks range between -4.61% and -9.25% with some significant at the 1% level. However, given the uncertainty due to the problems discussed above, we refrain from arguing for any one of these being the correct causal effect. Rather, we argue that the consistency of the estimated effect being around -5% and reasonably statistically significant implies the true average effect is likely to be negative and of similar magnitude.

The results we find are also stronger than the ones found by Hakobyan (2020) during the expiration in 2011. Although we cannot rule out that this is due to heterogeneity in post-COVID shocks, we theorize that this might be due to the duration of the expiration. The longer the expiration, the less profitable it is for producers to stick to production inputs that fulfill RoO requirements.

Our thesis adds to the literature about the importance of PTAs for imports. Our comprehensive analysis of credible alternative specifications and thorough discussion about parallel trends validity supports the conclusion of past work on the negative effect of GSP expiration on trade. This implies that predictability and stability are important in order to increase GSP utilization and increase imports. If helping developing countries and maintaining lower input prices for domestic industries is a priority for the United States, more consistent and predictable renewals are beneficial. If utilization rate increases with experience, frequent expiration will hinder producers from gaining experience regardless of the expiration's brevity. They also result in both parties needing to find alternative trading partners and can make restarting GSP trade expensive.

7.2 Further research

While we find that US imports fell due to the 2021 expiration, our results do not directly imply that the beneficiary countries were negatively economically affected as a result. The exports could have been redirected towards other GSP issuing countries, such as into the EU. An interesting extension would thus be to analyze the effect of GSP expiration on the GDP of beneficiary countries. Additionally, trade flows to other partners can be analyzed to see if trade was diverted as a result.

Another important question for future research is the impact of expiration on utilization rate. Since the US customs office still registers if RoO are fulfilled, the effect in the short and long-term can be analyzed. In the same vein, we theorize that the utilization before expiration predicts part of the effect on trade volume. By including categories of utilization rate as control variables, this hypothesis could be tested.

Another point of further research is looking at how the expiration impacts the size of firms in the exporting country. Given that Moran and Cebreros (2023) finds that smaller and larger firms have a lower utilization rate than medium-size firms, the expiration could have different effects based on firm size. Did the expiration lead to the larger firms gaining market share from the medium-size firms?

Besides promoting trade with the beneficiary countries, the GSP also has a goal of upholding human and workers' rights. Does it have this effect? And how is this affected, if at all, by long-term expirations like the current one starting in 2021?

8 Conclusion

In our thesis, we analyze the effect of the US GSP expiration on imports of eligible products from beneficiary countries. Using a triple difference approach, we estimate an average treatment effect of -4.61% of the expiration on imports at a 10% significance level. Using other credible specification, we find similar estimates of up to -9.25% significant at the 1% level. The results of these alternative specifications have two major implications. Firstly, smaller beneficiary countries benefit less from the GSP system and are thus less affected by the expiration. Secondly, eligibility to alternative PTAs does not compensate for the loss of GSP access.

The outcomes observed in our primary analysis and alternative model specifications indicate that the effect on trade of this expiration is larger than found in previous analyses, namely by Hakobyan (2020) who finds a reduction in imports of 3% after the 2011 expiration. We cannot exclude that the difference is due to data disturbance with the COVID-shocks or spillover effect, but it still seems likely that the longer the expiration, the larger the effect. Despite these weaknesses in our data and analysis, we argue that the direction and general magnitude of the effect are likely to be correct.

Our conclusions reinforce the idea that preferential access is important for exports in developing countries and should be maintained predictably.

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Appendix A Data

Table A1: Country spesicic HTS changes

Country Name	year	Estimated value	Amount of HTS affected	Type
Thailand	2020	$ 817 million	-	Removal
Thailand	2019	$ 1.3 billion	573	Removal
India	2019	$ 6.3 billion	-	Removal
Turkey	2019	$ 1.8 billion	-	Removal
Ukraine	2019	-	148	Reinstated

Sources: (EY 2019), (EY 2020) and (Akhtar and Jones 2019)

Table A2: CNL exclusions 2018-2020

Country Name	Year	HTS code	Description
Ecuador	2020	0714.40.10	Fresh or chilled taro (Colocasia spp.)
Argentina	2020	2909.19.14	Methyl tertiay-butyl ether. (MTBE)
Brazil	2020	3805.10.00	Gum, wood or sulfate turpentine oils
Equador	2020	4412.34.32	Plywood sheets n/o 6mm thick
Indonesia	2020	7113.19.29	Gold necklaces and neck chains
Brazil	2020	8502.12.00	Electric generating sets with compression-ignition internal-combustion piston engines.
North Macedonia	2019	8702.10.31	Motor vehicles w/diesel engine, to transport 16 or more persons, incl driver .

Sources: (United States Trade Representative 2019) and (United States Trade Representative 2020)

Table A3:

GSP elegible countries and territories

Belize+	Jamaica+	Haiti+'	Anguilla
Montserrat+	Dominica+	Saint Vincent and the Grenadines+	Grenada+
Guyana+	Suriname	Ecuador	Bolivia
Brazil	Paraguay	Argentina	Falkland Islands (Islas Malvinas)
Ukraine	Armenia	Azerbaijan	Georgia
Kazakhstan	Kyrgyzstan	Moldova (Republic of Moldova)	Uzbekistan
Bosnia and Herzegovina	North Macedonia	Serbia	Kosovo
Montenegro	Albania	Lebanon	Iraq
Gaza Strip administered by Israel	West Bank administered by Israel	Jordan	Yemen (Republic of Yemen)
Afghanistan	Pakistan	Sri Lanka	Burma (Myanmar)
Thailand	Cambodia	Indonesia	Timor-Leste
Philippines	Bhutan	Maldives	Norfolk Island
Cocos (Keeling) Islands	Christmas Island (in the Indian Ocean)	Heard Island and McDonald Islands	Papua New Guinea
Cook Islands	Tokelau	Niue	Samoa (Western Samoa)
Solomon Islands	Vanuatu	Pitcairn Islands	Kiribati
Tuvalu	Wallis and Futuna	Fiji	Tonga
Algeria	Tunisia	Egypt	South Sudan
Mauritania	Cameroon	Senegal*	Mali*
Guinea*	Sierra Leone*	Cote d'Ivoire*	Ghana*
Gambia*	Niger*	Togo*	Nigeria*
Central African Republic*	Gabon*	Chad*	Saint Helena
Burkina Faso*	Benin*	Angola*	Congo, Republic of the Congo *
Guinea-Bissau*	Cabo Verde*	Sao Tome and Principe*	Liberia*
Congo, Democratic Republic of the Congo (formerly Zaire)	Burundi	Rwanda*	Somalia
Eritrea	Ethiopia*	Djibouti*	Uganda*
Kenya*	British Indian Ocean Territory	Tanzania (United Republic of Tanzania) *	Mauritius*
Mozambique*	Madagascar*	Comoros*	South Africa*
Namibia*	Botswana*	Zambia*	Eswatini*
Zimbabwe	Malawi*	Lesotho*	Nepal'

Note:AGOA countries are marked with *. CBI countries marked with +. Countries with country specific PTA marked with '.

Table A4:

Non-GSP countries and territories			
Greenland	Canada	Saint Pierre and Miquelon	Mexico
Guatemala	El Salvador	Honduras	Nicaragua
Costa Rica	Panama	Bermuda	Bahamas+
Cuba	Turks and Caicos Islands	Cayman Islands	Dominican Republic
British Virgin Islands+	Saint Kitts and Nevis+	Antigua and Barbuda+	Saint Lucia+
Barbados+	Trinidad and Tobago+	Sint Maarten	Curacao+
Aruba+	Guadeloupe	Martinique	Colombia
Venezuela	French Guiana	Peru	Chile
Uruguay	Iceland	Sweden	Svalbard and Jan Mayen
Norway	Finland	Faroe Islands	Denmark, except Greenland
United Kingdom	Ireland	Netherlands	Belgium
Luxembourg	Andorra	Monaco	France
Germany (Federal Republic of Germany)	Austria	Czech Republic	Slovakia
Hungary	Liechtenstein	Switzerland	Estonia
Latvia	Lithuania	Poland	Russia
Belarus	Tajikistan	Turkmenistan	Spain
Portugal	Gibraltar	Malta	San Marino
Holy See (Vatican City)	Italy	Croatia	Slovenia
Greece	Romania	Bulgaria	Turkey
Cyprus	Syria (Syrian Arab Republic)	Iran	Israel
Kuwait	Saudi Arabia	Qatar	United Arab Emirates
Oman	Bahrain	India	French Southern and Antarctic Lands
Bangladesh	Vietnam	Laos (Lao People's Democratic Republic)	Malaysia
Singapore	Brunei	Macao	China
Mongolia	North Korea (Democratic People's Republic of Korea)	South Korea (Republic of Korea)	Hong Kong
Taiwan	Japan	Australia	New Zealand
New Caledonia	French Polynesia	Marshall Islands	Micronesia
Palau	Nauru	Morocco	Libya
Sudan	Western Shara	Equatorial Guinea	Seychelles
Mayotte	Reunion		

Note:AGOA countries are marked with *. CBI countries marked with +

Appendix B Robustness tests

In order to verify the validity of excluding zero-import observations from the sample, we run the model with and without zero-import observations to see if it would introduce bias in the OLS estimator. As seen in Table B1, column (1) and (2) suggest that removing these observations does not introduce systematic bias in the results as both estimates are not significantly different from zero and thus cannot be said to be different.

Table B1: Removing zero observations

VARIABLES	(1) Drop 0	(2) Keep 0
GSPCountry x *GSPProduct* x *Expired*	0.000810 (0.0423)	0.000395 (0.0423)
Constant	19.86*** (0.000159)	19.86*** (0.000158)
Observations	1,312,807	1,315,783
Fixed Effects	country-year country-product product-year	country-year country-product product-year

Notes: Dependent variable is Imports. Standard errors are clustered at country and product level.
*** p<0.01, ** p<0.05, * p<0.1

Table B2: Removing individual countries

VARIABLES	(1) Drop Turkey	(2) Drop India	(3) Drop Ukraine	(4) Drop Thailand	(5) Drop Egypt	(6) Drop Vietnam	(7) Drop Brazil
GSPCountry x *GSPProduct* x *Expired*	-0.0190	-0.0186	-0.0246	-0.0305	-0.0191	-0.0122	-0.00807
	(0.0274)	(0.0275)	(0.0269)	(0.0274)	(0.0139)	(0.0273)	(0.0307)
Constant	11.69***	11.66***	11.69***	11.68***	11.68***	11.66***	11.69***
	(0.000404)	(0.000411)	(0.000375)	(0.000339)	(0.000892)	(0.000402)	(0.000377)
Observations	1,397,052	1,377,964	1,419,392	1,399,442	1,420,492	1,398,571	1,401,093
Adjusted R-squared	0.872	0.871	0.871	0.872	0.874	0.875	0.876
p-value							
Fixed Effects	country-year country-product product-year	country-year country-product product-year	country-year country-product product-year	country-year country-product product-year	country-year country-product product-year	country-year country-product product-year	country-year country-product product-year

Notes: Dependent variable is lnImports. Standard errors are clustered at country and product level.
*** p<0.01, ** p<0.05, * p<0.1

Figure B.1: GSP countries parallel trends without OECD

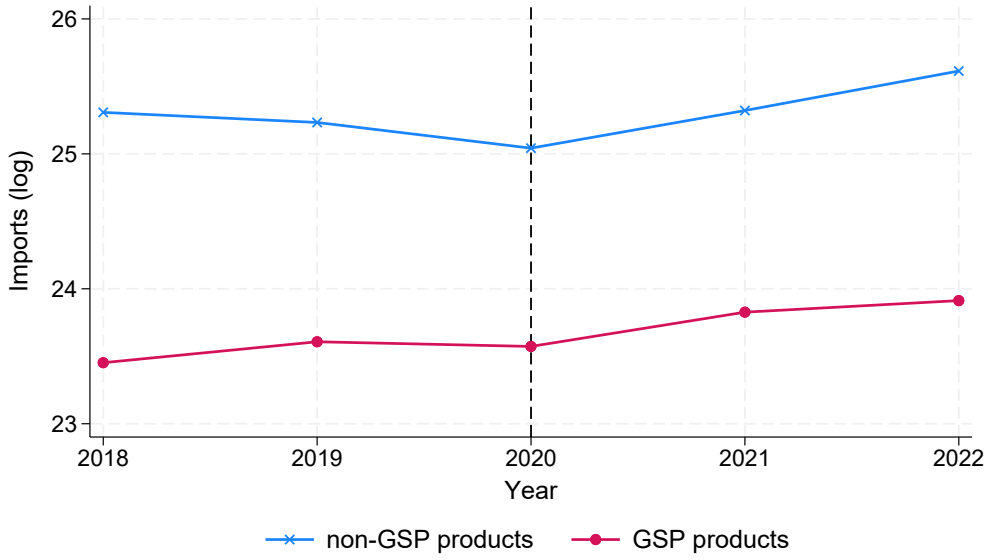
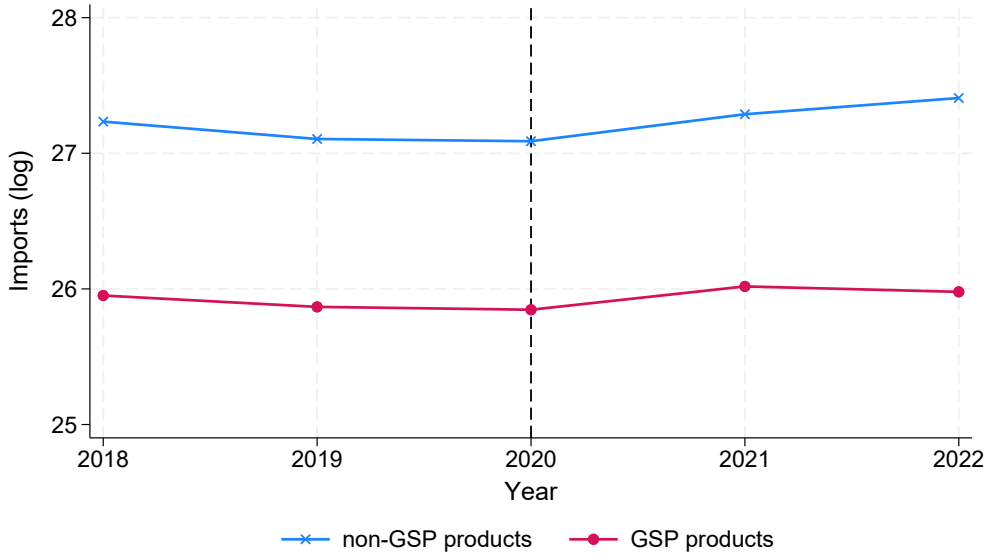
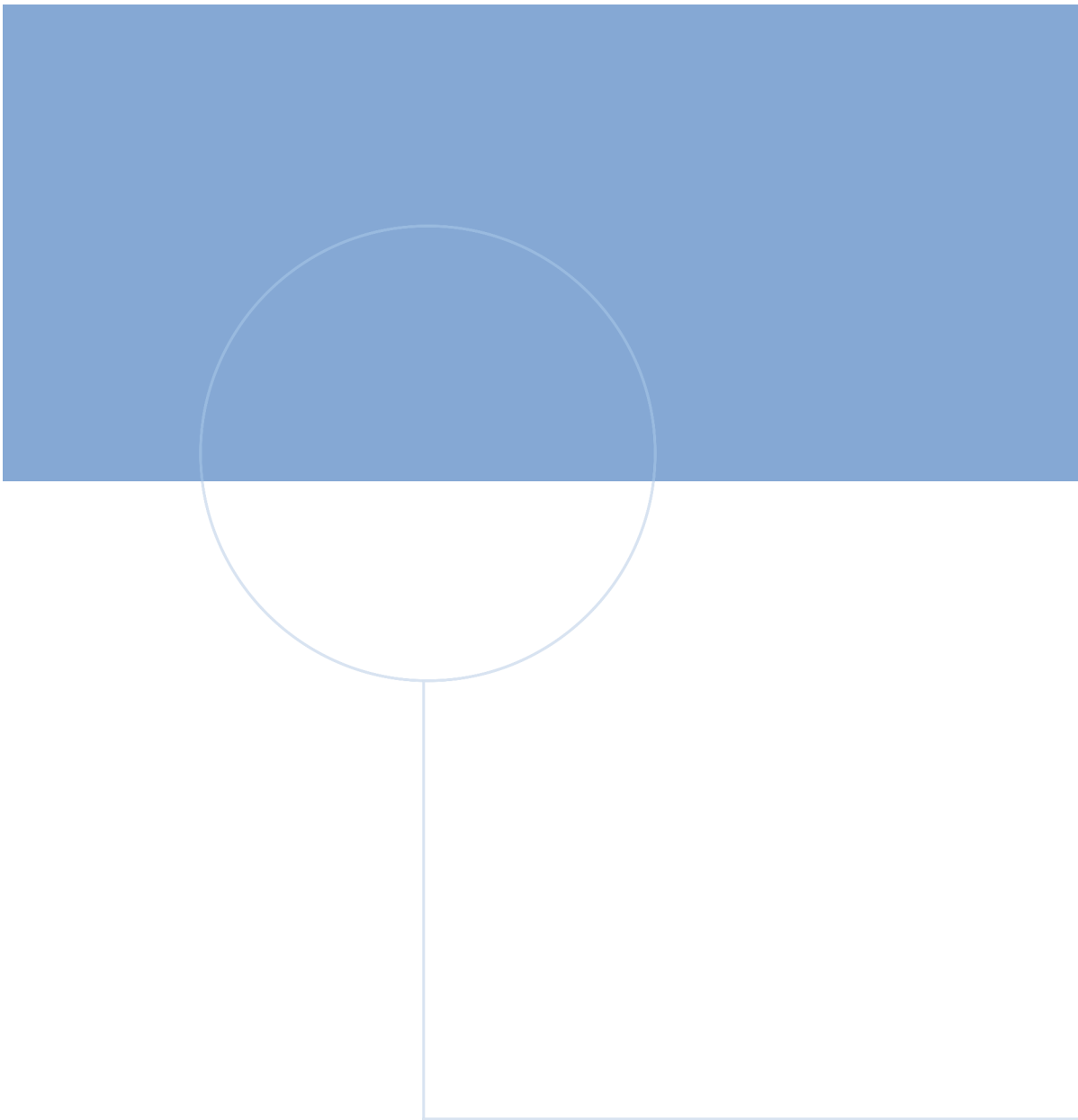


Figure B.2: non-GSP countries parallel trends without OECD





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