Revisiting the Effects of Preferential Trade Agreements

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Abstract

Every country in the world is a member of at least one Preferential Trade Agreement (PTA). Since countries choose to become members, selection bias is a key challenge in identifying the effects of these agreements on trade. I build a comprehensive dataset, and use the blocking estimator from the causal inference framework to address the selection issue. I find that after accounting for selection, PTAs increase bilateral trade by 48% fifteen years after entry into force. The effects kick in gradually, with one third realizing five years prior to the agreement. Anticipation effects are only present for 'non-natural' trading partners – geographically distant countries that trade little, and have a low probability of signing an agreement. I show that the methods that do not account for selection may substantially overestimate the effects. Equipped with the empirical estimates, I build a model to analyze the general equilibrium effects of a recent important PTA.

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1 Introduction

Virtually all countries in the world are a member of at least one preferential trade agreement (PTA). As of 2021 a total of 354 trade agreements were in force, corresponding to 578 notifications from the World Trade Organization (WTO) members. Counting agreements not notified to the WTO, the total number of agreements ever concluded exceeds one thousand. The provisions of these agreements cover 80-90% of bilateral trade between signatories and govern more than half of the total world trade.

What are the effects of preferential trade agreements on bilateral trade between their members? If trade agreements were randomly assigned, we could compare the trade outcomes of member country pairs with those of the outsiders and get an unbiased estimate of the causal effects. The main issue, however, is that PTAs are endogenous trade policy decisions of countries.

Dealing with this sort of selection is a challenging task. The reason is that trade and PTA assignment are intrinsically related: bigger and closer countries have larger trade volumes and are more likely to form PTAs (Baier and Bergstrand (2004); Magee (2003); Egger et al. (2011)). I use the blocking estimator from the causal inference framework of Imbens and Rubin (2015) to account for the probability of signing an agreement given past trade, and to estimate the effects without relying on functional form assumptions. To implement this strategy I build a dataset that tracks virtually all country pairs in the world over a period of 60 years.

I find that PTAs have sizeable effects even after accounting for selection. In particular, bilateral trade outcomes are 48% higher for pairs with a PTA fifteen years after the agreement's entry into force. The effects kick-in gradually, with one third of the total increase observed in anticipation (five years prior to entry into force). These effects are heterogeneous across different types of country pairs. Natural trading partners – geographically close countries with high initial trade levels and higher probability to conclude a trade deal – do not react in anticipation. The entire anticipation effect is thus driven by country pairs with larger bilateral distances, lower pre-PTA trade volumes, and low probability of having a PTA (non-natural trading partners). In the long run, however, the percentage increase in trade of country pairs with PTAs relative to their non-member counterparts is similar for all country pairs across the board. Additionally, I find that selection is important: the effects are halved when compared to the results of the alternative empirical specifications. In the second half of the paper I build a simple general equilibrium model to demonstrate how my empirical estimates can be used to study large PTA formations, with an application to one of the most recent trade agreements.

To study the effects of PTAs, I construct the most comprehensive dataset on bilateral trade. By combining all existing trade data sources, I recover almost one million observations from 1960 to 2019, which would otherwise be considered missing. The dataset allows me analyze the dynamic effects of a large set of trade agreements concluded between different types of country pairs.

Selection into PTA membership is the main challenge for the identification of the effects of PTAs on trade. In order to address selection, I use the blocking estimator from the causal framework of Imbens and Rubin (2015). The main idea is to find country pairs with PTAs (treated) and without PTAs (control) which are otherwise similar in all characteristics, and also the probability of signing an agreement. Some of the characteristics, such as geographical, cultural and historical ties of country pairs, as well as past trade, predict the probability of signing a trade agreement, but are not affected by its presence. From an empirical viewpoint we can thus condition the probability of signing a PTA and trade outcomes on these covariates.

The issue of economic size is more subtle. Since PTAs increase trade and economic size by reducing trade costs, controlling for size would not be a viable empirical strategy.[1] I deal with this issue by defining a 'size-free' measure of bilateral trade following Santamaría et al. (2020). The trade outcome is defined as a market share of an origin i in a destination j normalized by the average share of i in all markets. The advantage of using such normalized market shares is that they are not mechanically affected by the size of origin or destination countries.

The identification strategy consists of three distinct stages. In the first stage – design (Rubin (2005)) – no outcome data is used, the focus is solely on the PTA indicators and covariates. I model the probability of entering a PTA as a function of geographical, cultural, and historical characteristics of country pairs, as well as their past trade. I trim the dataset by dropping country pairs with values of covariates such that they have no counterparts in the other treatment group. The reason is that no general estimation procedure would give robust estimates in a sample with 'incomparable' units (Imbens (2014)).[2] After trimming the dataset, I construct subsamples (blocks) of country pairs such that, within each subsample, the conditional probability of receiving a PTA in the future is similar, and country pairs with and without PTAs, to the extent possible, do not differ in observable covariates. This design allows to treat PTA assignment within subsamples as random.

[1]Since trade volumes and size have a positive association with a PTA, controlling for size would lead to overestimating the effects of PTAs.

[2]The intuition is similar to extrapolation bias in a linear regression: if covariate distributions of treated and control units are substantially different, conventional regression methods can be very sensitive to minor changes in the specification.

The second stage is diagnostics and robustness analysis. First, I explore the differences in covariate distributions by treatment status across and within blocks. This diagnostic highlights the importance of separately estimating the effects of PTAs for different types of trading partners. Second, I investigate the presence of treatment heterogeneity: PTAs differ across multiple dimensions (Dür et al. (2014); Hofmann et al. (2019)), and different types of PTAs might have different effects (Magee (2008)). I collect data on characteristics of PTAs, and discover that while there is some variation across blocks, the covariates within each subsample cannot predict the features of a PTA that a pair will sign. Therefore, we can treat PTA types as random within each subsample. Third, I examine the extent of the missing values problem. Conditioning the analysis on positive trade flows might induce a downward bias in my estimates. To understand the severity of the issue, I conduct an interpolation exercise to recover some low-trade observations. Since the proportion of missing values within blocks is small, the final estimates appear to be robust to the reconstruction of the trade matrix.

The next stage – analysis – involves estimating the PTA effects and their sampling variances. I proceed by applying regression adjustment within each subsample to account for the residual differences in covariate distributions. The estimator of the average effects of PTAs on their members in the entire trimmed sample is then calculated as the weighted average of individual treatment effects within each subsample (Rosenbaum and Rubin (1984)). At this stage the outcome variable is defined taking into account the dynamic adjustment of trade outcomes in response to PTA shocks (Magee (2008); Egger et al. (2020)). To estimate the time-varying responses to PTAs, I define outcomes corresponding to different time windows around agreements' entry into force: anticipation, short-run, medium-run and long-run.[3]

The magnitude of the quantitative estimates obtained in this paper is lower compared to those obtained by applying alternative research designs. For example, when I estimate a standard empirical gravity equation with fixed effects in the full sample, the effects almost double.[4] I build a simple numerical simulation which indicates that the self-selection bias might not be picked up by the alternative estimators. Since PTA participants tend to have higher levels of trade (normalized market shares), be closer to each other in geographical and cultural dimensions, we expect the bias in the full sample to be positive. Causal in-

[3]Anticipation corresponds to the average trade outcomes in the five years prior a PTA's entry into force (approximately corresponding to a mean negotiation period across different agreements); short-run outcome measures a five-year average following a PTA's entry into force; medium-run and long-run outcomes are defined as averages of five to ten and ten to fifteen years respectively.

[4]As I show later, both trimming and blocking into subsamples play a role in reducing the size of the bias associated with self-selection into PTAs.

ference framework applied in this setting is aimed at reducing the selection bias, and thus predicts smaller effects.

Importantly, my empirical estimates should be interpreted as partial equilibrium effects, i.e. the effects of a PTA on its members under the assumption that "everything else" stays equal. In my setting, we could think of partial equilibrium effects as increases in bilateral trade of a country pair with a PTA, *regardless* of whether this trade increase is driven by pure substitution from other destinations (or from domestic trade). Another way to think about this assumption is that country pairs are small, and endowing one of them with a PTA will not affect the trade outcomes for all other pairs. Here, we are not interested in predicting what happens in a world where all pairs get a PTA, but rather what would happen to a randomly selected country pair if it gets assigned a PTA.

Partial equilibrium interpretation may seem to contrast with a large body of existing literature using structural gravity model to study the effects of PTAs. Identifying partial equilibrium estimates, however, was a focus of many empirical studies (see, for example, Soloaga and Wintersb (2001); Baier and Bergstrand (2009); Egger and Tarlea (2017)), and plays an important role in determining the general equilibrium effects (Egger et al. (2011)).

I take this idea further in the second half of the paper and build the general equilibrium model to make predictions about the changes in welfare (real consumption) and trade patterns following a shock to trade costs. The model is the simplest version of the quantitative structural gravity setup, defined in Costinot and Rodríguez-Clare (2014). Solving the model in changes using the 'exact hat algebra' approach, I study the static counterfactual equilibrium which would result from the changes in trade costs.

Equipped with the partial equilibrium estimates from the empirical part, I use them in a general equilibrium setup to study the effects of the Regional Comprehensive Economic Partnership Agreement (RCEP). RCEP is one of the most important and largest recent trade agreements involving fifteen countries in Asia.[5] It was signed in 2020, and started entering into force from the beginning 2022. The agreement is set to reduce trade barriers on 90% of goods trade. Moreover, it influences trade more broadly by covering multiple regulatory aspects relating to goods, services, investment, economic and technical cooperation, and creating new rules for electronic commerce, intellectual property, government procurement, competition, and small and medium sized enterprises. The contents and contemporaneity of RCEP makes it a relevant policy question to study.

I use the model to conduct two counterfactual exercises. The first one endows RCEP countries with a trade agreement using the long run average estimates obtained in the

[5] China, Japan, South Korea, Australia, New Zealand, and ten Southeast Asian economies (Brunei, Cambodia, Indonesia, Laos, Malaysia, Myanmar, the Philippines, Singapore, Thailand, and Vietnam.

empirical part of the paper. The second one exploits the full heterogeneity of the empirical estimates. Although the model does not feature any dynamics, I use it in a series of static exercises to study changes in real consumption and trade reallocation in the anticipation period and long run.

Both exercises predict increases in welfare for the RCEP members. Smaller countries (like Myanmar and Cambodia) experience the largest gains, while the larger countries (like China, Japan and Korea) experience negligible increases in welfare. The changes in the average welfare in the rest of the world (weighted by the countries' initial size) are small, but positive. The changes in trade, however, are much larger in magnitude. The model predicts a substantial amount of trade creation, i.e. a disproportionate increase in trade of RCEP economies within the PTA, and the outsiders among themselves.

The general equilibrium exercise highlights two important implications. First, I show how well-identified partial equilibrium estimates can be used in conjunction with the model to study the consequences of the real-world PTA formation. Such estimates are crucial for policy makers to take informed decisions about entering PTA negotiations. Second, I demonstrate that the magnitudes of the partial equilibrium estimates matter for the general equilibrium predictions. In particular, the larger estimates that do not account for selection substantially amplify the predictions regarding trade diversion and trade creation.

Literature Review. This paper builds on the large body of empirical literature studying the effects of PTAs. The dominant paradigm to approach this question is using an empirical form of a structural gravity equation, where the volumes of bilateral trade flows are regressed on PTAs and covariates, or sets of fixed effects. Head and Mayer (2014) provide an extensive overview of the gravity literature, and note that typically studies find large contemporaneous point estimates (a 43% increase in bilateral trade), although the standard deviation is also large, meaning that the estimates vary greatly across studies. In the same vein, Ghosh and Yamarik (2004) and Baier and Bergstrand (2007) note that estimates of PTA effects in a gravity setting are highly unstable.

One reason is that empirical implementations of the structural gravity model are susceptible to changes in the estimation methodology.[6] However, even when using similar methods, there is little consensus on the magnitude of the point estimates. For example,

[6] Yotov et al. (2016) summarize the best practices. Among other recommendations, they recommend applied researchers to estimate the gravity equation accounting for multilateral resistance terms (Anderson and van Wincoop (2003); Feenstra (2004); Olivero and Yotov (2012)); to use Poisson Pseudo Maximum Likelihood estimator to include zero trade flows (Santos Silva and Tenreyro (2006)); and to account for trade policy endogeneity by adding country-pair fixed effects (Baier and Bergstrand (2007).)

when adding dyadic fixed effects, Baier and Bergstrand (2007) find that the PTA estimate is multiplied by more than a factor of two, while Head et al. (2010) find that the coefficient is halved.

Another reason gravity estimates for PTAs 'are not reliable', as noted by Head and Mayer (2014), is that they fail to correctly address the endogeneity of trade policy.[7] To understand the PTA formation mechanism, Baier and Bergstrand (2004) explore the role of the economic determinants. Magee (2003) additionally finds that, empirically, past trade is an important predictor of PTAs. Egger and Larch (2008) conclude that interdependence is positively correlated with the formation and enlargement of PTAs. Given these insights, Egger et al. (2011) model the selection into PTA membership and use the predicted probability as a regressor in a gravity model. They find that the estimated PTA coefficient increases by more than a factor of 5 (from 42% to 220%). In my paper, I rely on these earlier studies highlighting the determinants of PTAs – such as geographical and cultural characteristics, past trade, and the number of PTAs already concluded – in calculating the conditional probability of PTA membership. I, however, depart from the empirical gravity framework to address selection.

The literature this paper relates the most uses non-parametric estimation techniques to evaluate the effect of endogenous PTAs on trade. Egger et al. (2008) look at effects of PTAs on trade volumes and intra-industry trade in the subsample of OECD member countries. Using matching estimators, they conclude that a simple difference-in-difference estimator without accounting for self-selection into new PTA membership is downward-biased by 62-86% depending on the type of matching. Baier and Bergstrand (2009) explore cross-sections of data for 96 countries in different years using a matching estimator. They report estimates of average treatment effects of in between 0.68 for the year 2000 (implying an effect of about 97 percent) and 2.36 for the year 1990 (around 900%). Their preferred estimate – average treatment effect on the treated – is more economically plausible, implying a 132% increase in trade. Egger and Tarlea (2017) employ entropy balancing to "compare apples to apples," i.e. PTA members with the outsiders with the same values of observable covariates.[8] They show, in contrast to earlier non-parametric studies – and similarly to this paper – that enforcing covariate balance actually reduces the estimates of PTA effects.

These papers are similar to mine in terms of the underlying assumptions for identifica-

[7] While dyadic fixed effects forces identification to come from the within dimension of the data, the estimate cannot be interpreted as causal, since there are may be other factors, along with PTAs, that vary at the country-pair-time level, which will be picked up by the coefficient.

[8] Entropy balancing is equivalent to estimating the weights as a log-linear model of the covariate functions (Hainmueller (2012)), and involves minimizing divergence from a set of baseline weights chosen by researchers, i.e. the method might be inconsistent unless the correct functions are specified.

tion. However, the implementation of the causal framework differs in several dimensions. First, the methodology used in my paper does not involve matching techniques (like in Egger et al. (2008) and Baier and Bergstrand (2009)). Unlike propensity score matching, blocking allows to balance covariate distributions.[9] Moreover, blocking procedure uncovers cross-sectional heterogeneity by classifying country pairs into groups with similar characteristics. Such classification allows me to compare the effects for natural and non-natural trading partners, which turn out to be, indeed, substantially different. Second, I construct a dataset tracking almost all country pairs in the world for the period of 60 years, and fill in many missing trade flows. This allows me to enlarge the pool of potential control pairs prior to the analysis, so as to let the data-driven algorithms choose appropriate controls for each treated unit. Moreover, the constructed data allows me to account for an additional important covariate which robustly predicts PTAs – past bilateral trade. In addition, by using normalized market shares I can abstract from identification issues related to economic size. Finally, I look at PTAs in dynamics, uncovering important country-pair heterogeneity in phase-in effects.

This paper is structured as follows. Section 2 describes the sources and construction of the data. Section 3 explains the study's empirical design and the identification strategy. Section 4 presents and discusses the results of the empirical framework. Section 5 builds a general equilibrium model, and, using the empirical estimates obtained earlier, applies it to study the effects of RCEP formation. Section 6 concludes and discusses avenues for future research.

2 Data ▲

One of the contributions of this paper is the construction of a comprehensive dataset containing most complete data on bilateral trade flows and domestic trade. I show that by assembling data from virtually all existing data sources, we can additionally gain more than one million bilateral trade observations over the period from 1960 to 2019, which would otherwise be treated as missing. The dataset also includes extensive information on the characteristics of country pairs, as well as features of preferential trade agreements.

Data on bilateral exports is constructed by combining several data sources: UN Comtrade Database, CEPII Gravity Database, World Trade Flows (WTF) bilateral cross-sectional data, and IMF Direction of Trade Database. These trade flows are complemented by data

[9] As demonstrated by King and Nielsen (2019), propensity score estimation should not be used for matching, since it implies matching on a uni-dimensional vector, thus potentially increasing covariate imbalance, inefficiency, model dependence, and bias.

on international trade and domestic trade from WTO Structural Gravity Database, USITC International Trade and Production Database for Estimation (ITPD-E), and UNIDO Industrial Statistics. I use the values reported by the destination as default, and complement them with values reported by origin, whenever available. As most sources have varying number of missing trade flows, the addition of different data sources helps to fill in many of the missing values. Appendix A details the exact procedure to construct the dataset.

Even after combining different sources to get a fuller matrix of bilateral trade flows, many missing values remain. In particular, over the entire sample period 61% of international trade flows are missing. One approach to deal with missing flows in trade literature is to declare them as zeros. It is, however, virtually impossible to distinguish missing values from zero trade flows. In fact, the mere combination of different trade data sources helped recover a substantial amount of missing trade flows, suggesting that many observations are not zeros after all. In addition, there are some data patterns that suggest that some flows might indeed be missing. For example, when we observe large trade flows at time t and $t+2$, but a missing value at $t+1$.

In order to deal with the missing data problem, I employ imputation to predict trade flows. The main purpose of imputing the missing values is to gain statistical power for the subsequent analysis, and to carefully deal with the participation bias (see Section 3.4 for further discussion). Appendix B lays out a detailed procedure to impute the missing trade flows. To summarize, the imputation procedure uses a flexible form of the empirical gravity model to impute values of trade for those pairs that have all the necessary covariate data available. This procedure leads to imputing additional 428,267 observations, decreasing the number of missing values in the full sample to 45%. Later on, in Section 3.4, I additionally report the results using the data obtained by applying an interpolation procedure. Interpolation additionally recovers 97,618 bilateral trade flows, reducing the number of missing values to 35%.

Importantly, in all the data exercises I never use the imputed *values* of trade flows in the analysis directly. I use trade volumes to construct normalized market shares, which depend not only on the bilateral flows, but on the whole matrix of flows. In this sense, imputation helps to recover more precise shares, but does not bias the results. In Appendix F I show that the differences in the distributions of imputed and raw market shares are small, with imputed shares having a slightly lower mean and variance. Appendix F implements the whole procedure without imputation, and demonstrates that the main conceptual results remain unchanged.

Having obtained the matrix of bilateral flows, I construct normalized market shares following Santamaría et al. (2020):

$$s_{ij} = \frac{V_{ij}/E_j}{Y_i/E} \tag{1}$$

where V_{ij} are the sales from origin i to destination j; $E_j = \sum_i V_{ij}$ is the total expenditure of j; $Y_i = \sum_j V_{ij}$ is the total income of i; and $E = \sum_j E_j$ is the world's total expenditure. If market j has above average importance for i, i.e. $V_{ij}/E_j > Y_i/E$, the normalized market share is above one. The important feature of normalized market shares is that the economic size of origin and destination does not mechanically affect them.

To get more intuition about this feature, consider as an example a bilateral trade flow from Israel to the US (Table 1). In 1960, only 0.16% of all American purchases were allocated to Israeli goods. However, Israel was also a very small economy, supplying only 0.21% of the world market. The normalized market share in this case is 0.76: Israel is less of an average important partner for the US, but, normalizing by size the share is close to one. In contrast, consider the opposite flow – from the US to Israel. In 1960 the share of Israel's expenditure that went to goods from the US was more than 28%. At the same time, US supplied 22% of the total world's purchases, resulting in normalized market share for this country pair being 1.27 – above average importance market for the US, but not nearly as big as one might consider given the size of the (non-normalized) market share.

Table 1: Normalized Market Shares of Israel and the USA.

	$(V_{ij}/E_j)^{1960}$	$(Y_i/E)^{1960}$	s_{ij}^{1960}
Israel-USA	0.16%	0.21%	0.76
USA-Israel	28.06%	22.03%	1.27

Importantly, to construct theory-consistent normalized market shares, s_{ij} should measure the i's share in j normalized by i's share in all markets, including itself. Unfortunately, before 1980 data for domestic trade (or production data used to construct it) is available only for a very limited set of countries. To overcome this issue, I construct the matrix of domestic trade flows by combining multiple data sources (see detailed procedure in Appendix A), and check whether the normalized market shares with and without domestic trade differ in my sample after 1980. Appendix C discusses in detail the various checks I conduct, but here it suffices to say that the differences between the two methods of calculating normalized market shares are very small. I thus proceed to construct normalized market shares without domestic trade for all country pairs and years.

To construct the treatment dummy, and extract information on the characteristics of PTAs, I use the Design of Trade Agreements Dataset (Dür et al. (2014)). It contains information on both agreements notified to the WTO, as well as those that were not notified. I delete partial scope agreements, and consider only fully enforced deals (free trade areas and customs unions). For each given treated county pair, the date of agreement's entry into force is coded as the earliest agreement. This way, a balanced panel can be created, without superseding or overlapping PTAs, amendment protocols, or revisions. Appendix A provides more detail about the precise steps and examples of how I clean the dataset. Table 1 in Appendix D provides the descriptive statistics of PTAs in the full sample.

Geographic and cultural characteristics come from CEPII's Gravity Dataset. I then complement those with other geographical variables using NASA's Earth Observing System Data and Information System (EOSDIS).

The full dataset includes 210 unique customs territories, with 319 PTAs, over the period 1960-2019. There are a total of 43,890 country pairs in cross-section, 16.13% of which are treated by year 2019. In comparison, the number of pairs with a PTA in 1970 was less than one percent. In a panel setting, only 6.37% have a PTA out of more than 2.5 million observations (Table 2).

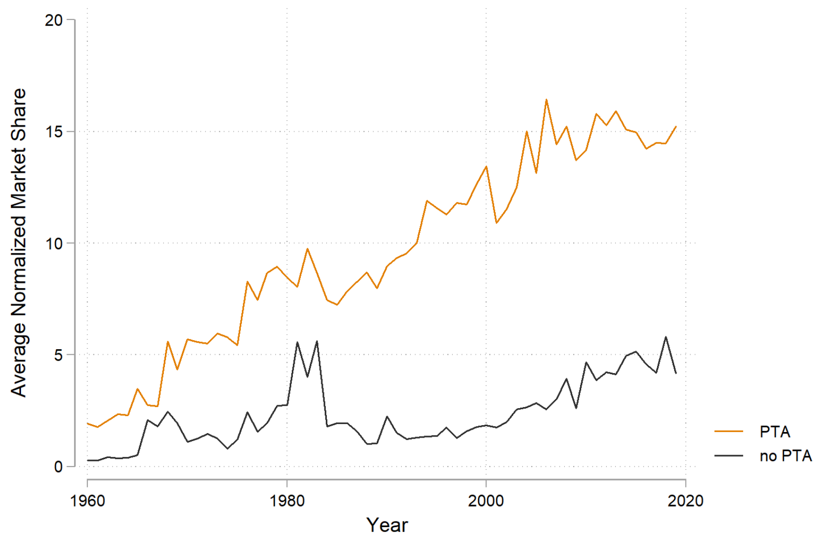
▲ Table 2: Full Sample Characteristics.

	Cross-section (2019)	Percent (2019)	Panel (1960-2019)	Percent (1960-2019)	Mean Share (1960-2019)
No PTA	36,812	83.87	2,465,521	93.63	2.55
PTA	7,078	16.13	167,879	6.37	17.69
Both	43,890		2,633,400		3.51

Figure 1 plots average normalized market shares for pairs which had a PTA at any point in time, and those that never had a PTA. The treated country pairs have always had higher bilateral trade, and the gap with the control pairs has been increasing over the entire period of time. The question is: how much of this increase can be attributed to the effects of PTAs, and how much is driven by other factors? The next section lays out the empirical design aimed at tackling the issue of self-selection into PTAs.

3 Empirical Strategy ▲

In order to estimate the causal effect of PTAs, we need to understand what would be the outcomes of the treated units had they not received the treatment. In what follows I will define the setup using Imbens and Rubin (2015) causal inference framework.



▲ Figure 1: Average normalized market shares for pairs with and without PTAs, 1960-2019.

3.1 Setup, Assumptions and the Blocking Estimator ▲

For each country pair with origin i and destination j there are two potential normalized market shares at a given time $T = \{A, S, M, L\}$ (anticipation, short, medium and long run), denoted as $s_{ij}^T(0)$ and $s_{ij}^T(1)$ – without and with a PTA respectively. The effect of a PTA at a given time is defined as the percentage change in average normalized market shares with PTA's entry into force:

$$\tau_{ij}^T = \ln \frac{s_{ij}^T(1)}{s_{ij}^T(0)} \tag{2}$$

▲

Each pair, however, is observed to either receive or not receive a binary treatment, $\text{PTA}_{ij} = 1$ or $\text{PTA}_{ij} = 0$. The realized (and observed) outcome for a pair is denoted with a subscript "obs" to distinguish it from the potential outcome which is not always observed:

$$s_{ij}^{T,obs} = \begin{cases} s_{ij}^T(0), & \text{if } \text{PTA}_{ij} = 0 \\ s_{ij}^T(1), & \text{if } \text{PTA}_{ij} = 1 \end{cases}$$

For each country pair there is also a K-component covariate Z_{ij}. The key characteristic of these covariates is that they are known not to be affected by the treatment: these are geographical, cultural and historical characteristics of country pairs, as well as past trade (the next section will discuss the covariate selection in more detail). For all pairs in the sample we thus observe a triple ($s_{ij}^{T,obs}$, PTA_{ij}, Z_{ij}).

In order to define an estimator for the average treatment effect which can be expressed in terms of the joint distribution of the observed data $(s_{ij}^{T,obs},\ \mathrm{PTA}_{ij},\ Z_{ij})$, we need to make a few assumptions. The first key assumption is unconfoundedness (Rubin (1990)) or conditional independence (Dawid (1979)):

$$\mathrm{PTA}_{ij} \perp \left(s_{ij}^T(0), s_{ij}^T(1)\right) |Z_{ij}$$

Intuitively, this assumption states that, conditional on the set of covariates, potential outcomes are independent of the treatment. In my setting it means that after conditioning on geographical, cultural and historical characteristics of country pairs, there are no such qualities on which trade outcomes depend that also relate to selection into PTAs. Being an identification assumption, unconfoudedness cannot be directly tested.

The second key assumption is overlap (Rosenbaum and Rubin (1983)):

$$0 < e(z) < 1$$

where $e(z) = \mathbb{E}\left(\mathrm{PTA}_{ij}|Z_{ij} = z\right) = \Pr\left(\mathrm{PTA}_{ij} = 1|Z_{ij} = z\right)$ is the propensity score. This assumption means that all country pairs have a non-zero probability of assignment to each treatment condition (either having or not having a PTA). The probability of concluding a PTA between two countries may be very small, but not zero, especially given the dynamic nature of trade.

The combination of these two assumptions implies that we can estimate the average effects by adjusting for difference in covariates between treated and control pairs. The main statistical challenge is now to understand how to estimate objects such as:

$$\tau^T = \mathbb{E}(\ln s_{ij}^T|\mathrm{PTA}_{ij} = 1, Z_{ij} = z) - \mathbb{E}(\ln s_{ij}^T|\mathrm{PTA}_{ij} = 0, Z_{ij} = z) \tag{3}$$

The goal is to provide an estimate $\hat{\tau}^T$ without relying on strong functional form assumptions on the conditional distributions. We would also like the estimator to be robust to minor changes in the implementation.

The estimator I am going to use is the blocking estimator. It relies on the initial estimate of the propensity score and uses sub-classification (Rosenbaum and Rubin (1983), Rosenbaum and Rubin (1984)), combined with regression adjustment within the blocks.

Conceptually, the advantage of the blocking estimator is its flexibility compared to a single weighted regression. In my setting, the blocking estimator serves several important goals. First, it approximately averages the propensity score and ensures the balance in covariate distributions across treatment groups within the blocks. The implication is that I am going to compare similar pairs which have similar probability of signing an agreement. Second, as Section 3.3 shows, there are large differences across blocks. In this setting the blocking estimator allows to perform inference within blocks without relying on functional form assumptions and heavy extrapolation. Third, diving the sample into blocks also uncovers additional heterogeneity across different types of country pairs, which would not be possible to analyze with a simple average effects estimator.

Implementing the estimator requires the estimation of the propensity score, $\hat{e}(z)$. The range of the propensity score is then partitioned into B intervals of the form $[m_{b-1}, m_b)$ for $b = 1 \dots B$. Let $B_{ij}(b) \in \{0, 1\}$ be a binary indicator for the event that the estimated propensity score for a country pair ij satisfies $m_{b-1} < \hat{e}(z) \leq m_b$. Within each block the average treatment effect in each time period is estimated using linear regression with covariates, and the indicator for the treatment (the time period T superscripts are omitted for simplicity):

$$\left(\hat{\alpha}_b, \hat{\tau}_b, \hat{\beta}_b\right) = \operatorname{argmin}_{\alpha,\tau,\beta} \sum_{ij=1}^{N} B_{ij}(b) \left(s_{ij} - \alpha - \tau \mathrm{PTA}_{ij} - \beta' Z_{ij}\right)^2 \tag{4}$$

▲

This leads to B estimates of $\hat{\tau}_{ij}$ for each $T = \{A, S, M, L\}$, one for each block. To obtain the average estimate over the B blocks, I use the proportion of treated units in each block, $N_{\text{treat},b}$ as weights:

$$\tau_{\text{block, treat}} = \sum_{b=1}^{B} \frac{N_{\text{treat},b}}{N_{\text{treat}}} \hat{\tau}_{ij} \tag{5}$$

▲

In the next sections I will show exactly how I implement the estimator in my setting. Section 3.2 explains the procedures I use to estimate of the propensity score and to find the right number of blocks to perform inference. Section 3.5 discusses the regression adjustment and the standard error estimation.

3.2 Design: PTA Assignment and Blocking ▲

Understanding the assignment of preferential trade agreements is central to the empirical strategy. This section builds an empirical model of selection to estimate the probability of concluding a PTA for different types of country pairs.

I choose the treatment period lasting from 1970 to 2005. This would allow me to estimate the anticipation and the long term effects of PTAs.[10] In this setup, the treated country pairs are those that had a PTA entering into force in this period, while the pool of potential

[10] Having collected the data on negotiation and implementation periods of PTAs, I conclude that the mean negotiation period is about 4 years, while the mean implementation period is around eight years.

control country pairs is comprised of those pairs which never had a PTA. Country pairs which had a PTA before 1970 or after 2005 are excluded from the sample.

How do countries decide whether to enter a trade agreement or not? The existing literature on the topic is scarce. Baier and Bergstrand (2004) develop a simple theoretical model, which gives predictions about economic factors influencing the likelihood of PTA formation. In their setting, a pair is more likely to conclude a PTA if (1) countries are closer in distance; (2) a pair is more remote from the rest of the world; (3) countries are larger and more similar in size; (4) countries are different in capital-labor ratios; and (5) a pair's difference in capital-labor ratios is smaller with respect to the rest of the world. The economic factors highlighted by Baier and Bergstrand (2004) are informative, but since the theoretical model is stylized, the list of factors is not exhaustive. For example, the static model at the core of Baier and Bergstrand (2004) does not allow to incorporate another strong predictor of PTA formation – the past level of trade, as highlighted in Magee (2008). Additionally, there are numerous other geographical, cultural and political characteristics affecting the likelihood of a PTA formation.

My approach to understand the formation of PTAs is informed by the literature, but is ultimately data driven.[11] I collect comprehensive information on country pair characteristics which are relevant for the PTA assignment, and are also correlated with the trade outcomes. A step-wise procedure suggested by Imbens and Rubin (2015) selects a set of covariates that maximizes the predictive power of the empirical model.

The first set of covariates relates to geographical characteristics of country pairs, and includes variables such as bilateral distances, remoteness,[12] indicators for contiguity,[13] being an island, and being landlocked. Since larger geographical barriers increase trade costs, we would expect the coefficients of these variables to be negative. The second set of covariates relates to cultural and historical characteristics. Here I include the following variables: an indicator for common language, common colonizer, an existence of colonial relationship in the past, and a common type of the legal system. These characteristics relate to the closeness of two countries, and their expected contribution to the probability of forming a trade agreement is expected to be positive. Finally, there is a set of variables related to trade regulatory environment: membership in the GATT, membership in the EU, and the total number of preferential trade agreements concluded by 1965. The latter

[11]While the theory of PTA formation is outside the scope of this paper, in conclusion I discuss some avenues for future research on this front.

[12]The remoteness of a country is calculated as the sum of the bilateral distance from that country to every other country in the sample. The country-pair remoteness is the average remoteness of the two countries.

[13]Contiguity was not selected by the step-wise covariate selection procedure into the final estimation equation.

intends to capture the increasing likelihood of concluding more agreements in the future in case the countries had PTA experience in the past. Finally, I include past trade as a predictor of future PTAs. The idea is that natural trading partners would be more likely to form preferential trade agreements. An important implication of including past trade is that I will conduct my entire analysis conditional on positive trade. In the next sub-section I discuss the question of participation in trade in more detail.

I estimate the probability of having a PTA using a logit regression. The left two columns in Table 3 show the results of the estimation in the full sample: the coefficients, the standard errors and the marginal effects. The marginal effects are computed at the means for the continuous variables, and as a switch from zero to one for the binary ones (keeping all the other variables at their means). For example, for a country pair with an average distance, the marginal effect of the distance is a 16% reduction in the probability of signing a PTA (holding other variables fixed at the means). Having a common language, on the other hand, increases the probability of signing and agreement by 6% (again, fixing all the other variables at their means). Table 3 shows that the biggest factors contributing negatively to the estimated probability are distance and remoteness. Variables such as having a common language and a common colonizer increase the probability of having a PTA.

The lower panel of Table 3 shows the predictive properties of the model. The pseudo R-squared is equal to 0.39, representing a good fit.[14] The next indicator calculates the area under the Receiver Operating Characteristics (ROC) curve. Since the ROC curve is a probability curve, the area under it indicates how capable the model is to distinguish between the treatment groups: the closer is the value to one, the better are predictive properties of the model. The value of 0.89 means that there is a 89% chance that the model will be able to distinguish the two treatment groups. Finally, if we assume that all the pairs with the predicted probability higher than 0.5 are treated, the model is able to correctly classify 87.4% of all the pairs.

To estimate the object in Equation 3 we need to find units that would be similar in terms of overlap in their covariate distributions. At the extremes of the propensity score support (close to zero or one) such overlap is lacking, and thus these pairs should be dropped since they have no counterparts in the other treatment group. A data-driven trimming procedure suggested by Crump et al. (2009) would result in a more robust estimation. The optimal cutoff of the propensity score distribution deletes 8.3% of the support on both sides. The last two columns of Table 3 present the coefficients, the standard errors and the marginal effects after trimming.

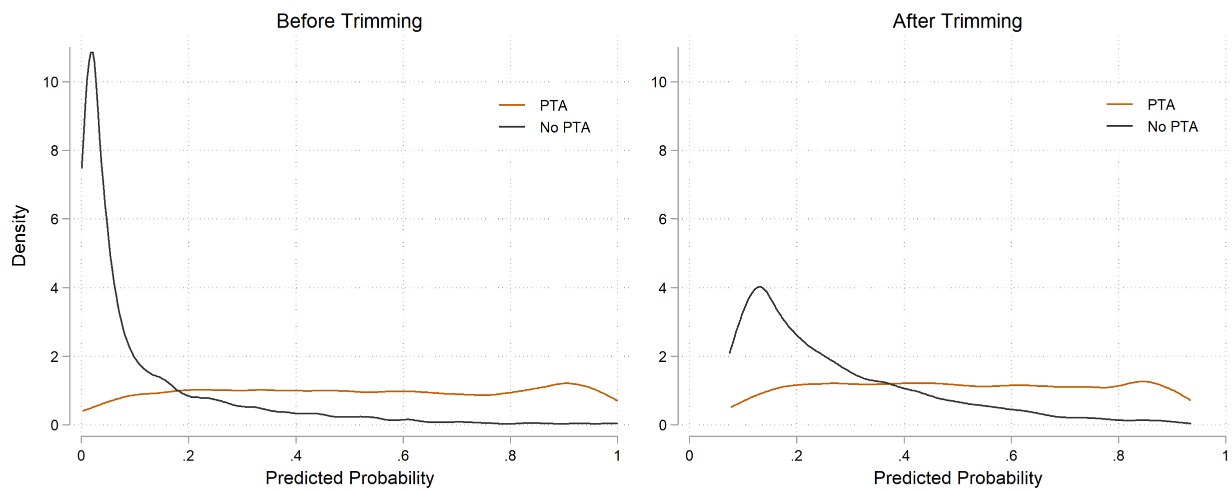
[14] Pseudo R-square represents an improvement from a model without any independent variables to a full model. Typically values from 0.2 are considered to indicate a good fit.

▲ Table 3: Results of the logit estimation of the probability of having a PTA in 1970-2005.

	Raw Sample		**Trimmed Sample**	
	Coefficient (Std. Err.)	Marginal Effect (Std. Err.)	Coefficient (Std. Err.)	Marginal Effect (Std. Err.)
Distance	-1.96**	-0.16	-2.07***	-0.45
	(0.05)	(0.004)	(0.07)	(0.014)
Remoteness	-5.26***	-0.42	-5.23***	-1.16
	(0.30)	(0.02)	(0.35)	(0.07)
Small Island	-0.94***	-0.06	-0.96***	-0.18
	(0.08)	(0.004)	(0.09)	(0.015)
Landlocked	0.46***	0.04	0.55***	0.12
	(0.05)	(0.005)	(0.06)	(0.014)
Common Language	0.64***	0.06	0.67***	0.15
	(0.07)	(0.008)	(0.07)	(0.017)
Common Colonizer	0.58***	0.06	0.69***	0.16
	(0.09)	(0.01)	(0.09)	(0.022)
Colonial Relationship	-0.63**	-0.04	-0.81***	-0.15
	(0.19)	(0.1)	(0.21)	(0.03)
Legal System	0.14*	0.01	0.13*	0.03
	(0.05)	(0.004)	(0.06)	(0.01)
GATT Membership	0.22***	0.02	0.12	0.03
	(0.06)	(0.005)	(0.07)	(0.016)
EU Membership	0.91***	0.09	0.90***	0.21
	(0.06)	(0.01)	(0.09)	(0.02)
Pre-treatment Share	0.08***	0.006	0.07***	0.014
	(0.02)	(0.001)	(0.02)	(0.004)
Pre-treatment PTAs	0.11	0.008	0.09	0.02
	(0.07)	(0.006)	(0.07)	(0.02)
Intercept	62.02***		62.72***	
	(2.69)		(3.37)	
N treated	3,200		2,612	
N control	13,392		4,673	
N Total	16,592		7,285	
Pseudo R-squared	0.39		0.19	
Area under ROC	0.89		0.78	
Correctly classified (0.5)	87.4		74.7	

Note: Standard errors in parenthesis. Levels of statistical significance correspond to: * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Figure 2 plots the distribution of the predicted probabilities for different treatment groups before and after trimming. The majority of observations without a PTA are concentrated on the lower end of the propensity score, and those are the ones being trimmed. Trimming procedure noticeably improves the overlap in the propensity score and covariate distributions. Table 2 in Appendix D shows the results of the t-test for balance in covariates and the standardized differences in covariate distributions before and after trimming.



▲ Figure 2: Distribution of the propensity score by treatment group before and after trimming.

Note: The figure plots the predicted probability of having a PTA for control and treated pairs before and after trimming. The propensity score is estimated using a logit regression. The trimming cutoff is determined by an optimal data-driven cutoff (Imbens and Rubin (2015)).

After trimming, however, there still remain substantial differences in the distributions of covariates for the observations at the opposite spectrums of the propensity score. This suggests using the blocking estimator proposed by Imbens and Rubin (2015), and described earlier Section 3.1. The blocking procedure partitions the sample into subclasses (blocks), based on the values of the estimated propensity scores, so that within the subclasses, the estimated probabilities are approximately constant. This way the systematic biases in comparisons of outcomes for treated and control pairs associated with observed covariates can be eliminated. We can then estimate causal effects within each subclass as if the PTA assignment was at random. Regression adjustment within each subclass eliminates the remaining differences in covariate distributions across treatment groups. Because the covariates are approximately balanced within the blocks, the regression does not rely heavily on extrapolation as it might do if applied to the full sample.

The main decision in the implementation of the blocking estimator is the number of blocks to partition the data into. I follow the data-dependent procedure for selecting both the number of blocks and their boundaries implemented by Becker and Ichino (2002). The algorithm starts with the entire sample, and checks whether the average estimated propensity score of treated and control pairs differs. If the test fails in one interval, the algorithm splits the interval at the median value of the propensity score and tests again, continuing until the average propensity score does not differ between treated and control pairs within the interval. As a result, I get my data split into nine blocks. Table 4 shows the inferior value of the propensity score and the number of treated and control pairs within each block. In the next subsection I will characterize the resulting blocks.

▲ Table 4: Inferior of the propensity score and the number of observations in each block.

	B1	B2	B3	B4	B5	B6	B7	B8	B9
Inferior of PS	0.08	0.125	0.1875	0.25	0.375	0.5	0.625	0.75	0.875
N Control	1,008	1,028	657	873	524	312	153	81	24
N Treated	115	186	180	387	405	380	352	360	247
N Total	1,123	1,214	837	1,260	929	692	505	441	271

Note: The inferior of the propensity score within each block is the lowest predicted probability of having a PTA within each block after trimming.

3.3 Diagnostics: Understanding the Blocks ▲

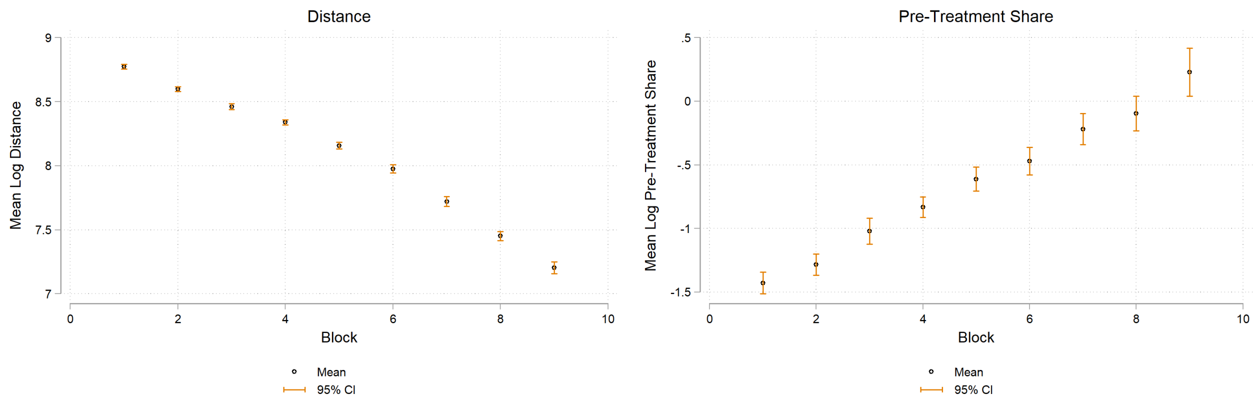
The blocking algorithm sorts country pairs into different sub-samples according to their probability of having a PTA. Lower block numbers correspond to a lower probability of a PTA being concluded. This probability, in turn, is correlated to with country pair characteristics: lower block pairs are, for example, far away from each other, and trade less pre-treatment (non-natural trading partners). Higher ranked blocks contain pairs which we can generally think of as natural trading partners: geographically close countries which trade a lot, and have a high probability of signing a trade agreement.

Figure 3 plots as examples the means and the confidence intervals for the two covariates – distance and pre-treatment normalized market shares – for each block. It demonstrates the substantial differences between pairs classified to lower and upper blocks. In this setting, using the entire sample to fit, for example, a linear regression, will not correctly account for the covariate imbalances.

One necessary diagnostic is to formally test the balance of each covariate between pairs with and without a PTA *within* each block. This is important, since, even if the probability of a PTA within blocks is similar, we may still fail to correctly estimate the treatment effects if the covariate distributions are very different across treated and control groups. Assessing the balance in covariate distributions is also indicative of the importance of applying the regressions adjustment at the analysis stage.

Table 5 presents the normalized differences between treated and control pairs for each block.[15] The normalized differences are more suitable to analyze covariate imbalances than the simple t-statistic, since they do not increase with the sample size (Imbens and

[15]The normalized differences are calculated as the difference in average covariate values, normalized by the square root of the average of the two within-treatment group sample variances.



▲ Figure 3: Mean and confidence intervals for distance and pre-treatment normalized market share, by block.

Rubin (2015)).[16] To simplify the analysis of the insights from Table 5, we could use the rule of thumb suggested by Austin (2009), stating that an absolute normalized difference of 0.10 or more indicates that covariates are imbalanced between groups.

A few important conclusions emerge from the diagnostic analysis. First, the differences in covariate distributions within each block are substantially lower than in the full sample. The only exception is block nine, where, for some variables, the differences still remain large. Second, some differences remain only for a few covariates, suggesting to apply regression adjustment within blocks. To visually confirm the intuition that blocking procedure ensures a much better balance in covariate distributions in Figure 1 and Figure 2 in Appendix D I plot the distributions of the pre-PTA normalized market shares and bilateral distances in treated and control groups by block. Again, with the exception of the last block, we can see that the distributions match well across treatment groups.

3.4 Robustness Analysis ▲

In this subsection I explicitly discuss the plausibility of two elements of the empirical design. The first one is the assumption on the uniqueness of the potential outcome, or lack of treatment heterogeneity. The second relates to conditioning the analysis on positive trade flows, or the extent of the bias associated with the missing trade flows.

Treatment Heterogeneity. In Section 3.1, I have assumed that the unobserved potential outcome of a country pair, $s^T_{ij}(0)$ or $s^T_{ij}(1)$, is unique: with or without a PTA. However, in my setting, it is clear that there are many different types of PTAs, so each PTA type could

[16] For completeness, I present the t-test results as well, in Table 3 in Appendix D

▲ Table 5: The normalized differences by block.

	B1	B2	B3	B4	B5	B6	B7	B8	B9	All
Pre-treatment Share	-0.17	-0.05	0.04	0.10	-0.02	-0.01	-0.02	0.14	-0.49	-0.30
Distance	-0.001	0.26	0.11	0.26	0.05	0.02	-0.51	-0.29	-1.11	0.82
Remoteness	0.02	-0.14	-0.18	-0.06	0.01	0.01	0.27	0.19	0.55	0.27
Small Island	-0.09	-0.12	0.05	-0.11	0.01	-0.003	0.32	-0.13	0.61	0.07
Common Language	-0.23	0.12	0.06	0.0005	-0.08	0.05	0.05	-0.06	0.03	-0.19
EU Membership	-0.07	0.06	-0.21	0.11	0.14	-0.06	-0.11	-0.03	-0.15	-0.11
Landlocked	0.20	0.02	-0.02	0.27	-0.12	0.09	-0.41	-0.38	-0.92	-0.04
Common Colonizer	0.09	0.15	0.10	0.05	0.03	-0.02	-0.33	-0.09	-0.82	-0.14
Colonial Relationship	0.21	0.04	-0.02	0.08	-0.23	0.01	0.01	0.15	0.38	0.01
GATT Membership	0.17	0.06	0.15	0.18	0.05	-0.09	-0.33	-0.58	-0.45	0.03
Legal System	-0.06	0.15	0.15	-0.01	-0.08	0.02	-0.32	0.13	0.11	-0.15
Pre-treatment PTAs	0.14	0.15	0.14	0.01	0.05	-0.18	-0.34	-0.41	-0.79	-0.19

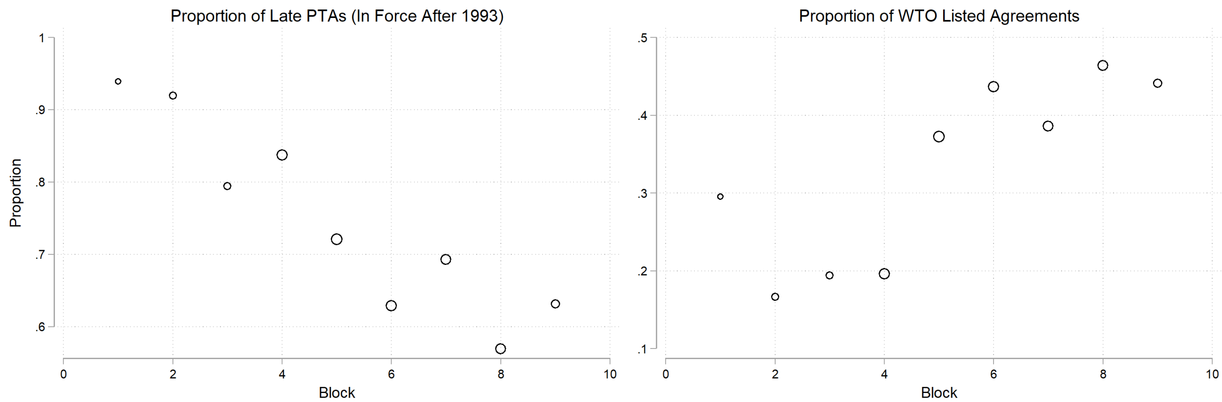
Note: The normalized differences are calculated using the method of Yang and Dalton (2012).

potentially have a distinct unobserved potential outcome. Next I investigate the extent of treatment heterogeneity *across* and *within* blocks.

I collect the data on various characteristics of PTAs: timing of entry into force, type (free trade area or customs union), composition (bilateral or plurilateral), notification in the WTO, presence of national treatment and third-party MFN provisions. As an example, Figure 4 shows the differences in selected PTA characteristics across blocks. The left panel shows the proportion of PTAs that entered into force after 1993 in each block. While among non-natural trading partners almost all agreements were concluded after 1993, for natural trading partners only around 60% of all treated pairs have 'late' agreements. The right panel shows the difference in WTO notification status of the agreement across blocks. Again, natural trading partners seem to be more likely to notify their agreements, compared to the pairs in the lower-index blocks.

Figure 4 shows that there is some variation in the types of agreements that different pairs choose to sign. These differences, however, are not entirely defined by the type of pairs: in the case of notification, for example, half of natural trading partners still choose to not notify the agreement. Therefore, the type of agreement and the type of pair signing it are confounded. The average effect of PTAs on trade across all blocks will inevitably reflect both the differences in types of agreements, and the types of country pairs. Similar patterns emerge for other characteristics of trade agreements: type, composition, national treatment provision, and third-party MFN provision.[17]

[17]Ideally, we would like to disentangle the affects of different types of treatment from the reactions of different pairs. Unfortunately, given the large number of characteristics, estimating the effects of each type separately is not possible due to the lack of statistical power.



▲ Figure 4: The proportion of late PTAs (entering into force after 1993), and the proportion of PTAs notified to the WTO, by block.

Note: The size of circles in each graph is proportional to the number of treated units within the block.

The estimation, however, is done separately for each block. We need to assume that the unobserved potential outcome of a country pair is unique *within* the block. In settings where there is treatment heterogeneity, Imbens and Rubin (2015) recommend redefining the treatment such that the estimates are going to reflect the effects of a randomly selected treatment type. To provide evidence that the treatment types can be treated as random *within* blocks, I test whether the covariates can predict various treatment characteristics. Table 4 through Table 9 in Appendix D show the results of regressions of PTA types on covariates by block, where most of the coefficients appear to be not statistically significant.

To sum up, while we cannot disentangle the effects of different types of agreements, we can reasonably assume that the types of PTAs within each block are random. Thus, we can interpret the individual block estimates as the effects of a randomly selected agreement, while the average estimate across all blocks will represent a combination of effects of different types of agreements on different types of pairs.

Missing Values. The second element of design that deserves attention is conditioning the entire analysis on positive trade flows. In the pre-treatment period, more than half of country pairs have missing trade flows. Since in the raw sample we cannot distinguish missing trade flows from zeros, I apply the imputation procedure discussed earlier in Section 2 and in Appendix B. Imputation recovered 10,804 bilateral trade observations in the pre-treatment period. 50% of the imputed trade flows correspond to low-trade pairs, with exports below 5 thousand USD per year. 90% of the normalized market shares calculated using the imputed data are below one. The majority of these low-trade observations are later cut away by the trimming procedure: trimming deletes 21% of the total sample corresponding to the low values of the estimated propensity score.

I use all the available data resulting from the imputation procedure to model treatment assignment. Since the level of trade in the pre-treatment period is one of the determinants of the treatment assignment, the probability of concluding a PTA is only defined for countries that trade in the pre-treatment period. Thus, pairs which have missing values in the pre-treatment period, and are not recovered by the imputation procedure, are dropped from the subsequent analysis.

The missing value problem, however, persists in the periods after the treatment. In particular, I observe that some pairs which were trading in 1960-1965 have missing values in the anticipation, short, medium, and long run. The missing trade flows could appear as a result of countries ceasing to trade, or as an artifact of the imputation procedure: I had enough data to impute their trade in the pre-treatment period, but ran into data availability problems for later years. I conduct a simple diagnostic of the problem: Table 10 through Table 13 in Appendix D calculates the proportion of missing values which were imputed in the pre-treatment period (in every block, for a given time period, and by treatment status). For example, in anticipation period in block 1 there are a total of 13,104 country-pair-year observations without a PTA, 334 of which are zeros. Out of these 334 observations, trade values for 125 were imputed in 1960-1965. In the same block and time period, out of 115 treated units, there is only one missing value, and it was not missing (or imputed) in the pre-treatment period. The diagnostic is aimed to check whether the problem is inherently related to the lack of data, or to the lack of balance within blocks.

A few patterns of missing data emerge. First, there are many more missing values detected in every time period for lower-index blocks than for higher-index blocks. The *proportion* of missing values, however, is similar across blocks. Second, the share of missing values in total observations within block does not exceed 3%: in the example above there are only 2.55% of total values missing in the first block. Additionally breaking by the treatment status, however, a difference is uncovered: there are on average 2.67% of values missing for the control pairs (across all time periods), and only 1.65% for the treated pairs. Third, the proportion of values that were initially imputed is roughly half for the control units in the lower-index blocks.

In sum, the results of the diagnostic reveal that the proportion of missing values differs by treatment status: there are more missing values in the control group than in the treatment group. Moreover, there are more non-traders corresponding to the lower index blocks. Finally, at least half of the missing data problem cannot be attributed to the lack of data to conduct imputation. These insights point to a problem: for the lower blocks the number of non-traders is not balanced between the treated and the control groups. If those pairs were instead trading, their normalized market shares would likely be small. Excluding these pairs from the analysis would thus produce a downward bias in my baseline estimates.

To understand the severity of the problem induced by missing values, I additionally recover some of the low-trade observations using an interpolation procedure. By applying simple linear interpolation I additionally recover 97,618 observations for the entire sample period. Interpolation recovers around half of the zeros in anticipation, around 35% in the short run, and 20% in the medium and long run. After recovering the trade flows, I calculate new normalized market shares. As suspected, the majority of these observations – 75% of the total – are low-trade observations, with normalized market shares below one.

I conduct the full analysis using the average normalized market shares calculated after interpolation. The final estimates of PTA effects are indeed lower when using the fuller matrix of trade flows, but the differences with the baseline estimates are negligible: the changes appear to be in the second decimal of the point estimates (see Figure 4 in Appendix D for comparison of the final estimates). Thus, while it is difficult to test directly the extent of the missing value problem, interpolation exercise suggests that the estimates are robust to the partial reconstruction of the trade matrix.

3.5 Analysis: Estimation and Inference ▲

Finally, the last step in the implementation of the blocking estimator is the regression adjustment within blocks, described in Equation 4 in Section 3.1. Within each block, I run a linear regression with the same set of covariates that were used for predicting the probability of PTA formation, since those are the factors that can directly influence trade. I also control for year-into-force fixed effects.[18] Within each block and for each time period $T = \{A, S, M, L\}$ the regression takes the form:

$$s_{ij} = \alpha + \tau \mathrm{PTA}_{ij} + \delta \mathbf{Z}_{ij} + \varepsilon_{ij} \tag{6}$$

▲

This leads to nine estimates of $\hat{\tau}$ for each T, one for each block (standard errors are clustered at country-pair level). To estimate the effect of PTAs in the entire sample, I average the within-block estimated treatment effects, weighted by the number of treated units in each block, as shown in Equation 5 in Section 3.1.

There is an important aspect of the estimation that relates to the fact that PTAs are being concluded in different points in time. The outcome variable is the average normalized market share at different horizons before and after agreement's entry into force. For

[18] Note that these are still defined for control units, corresponding to the treatment years within the block.

treated units, the year of entry into force is well defined and known, so the average shares are easily constructed around that year. Each of the nine blocks contains agreements with different years of entry into force. For example, in the first block, the short run outcome for Israel-USA pair is calculated as the average normalized market share from 1985 (the year of entry into force) to 1989; the short run outcome for Canada-Israel is the average share from 1997 (the year of entry into force) to 2001.

For control units, however, by definition there is no year of entry into force. Since within each block there are treated pairs with different years of entry into force, normalized market shares for the control group are calculated for the same control pairs around those different treatment years. Continuing the example, in the first block, USA-Denmark is a control pair (i.e. never had a PTA), so I calculate the average short run normalized market share for USA-Denmark in both 1985-1989 and 1997-2001. Thus, we could think of this data structure as a form of re-sampling outcomes from the control distribution for different treatment years within the block.

The question is whether the re-sampled structure of the data makes a difference for how we interpret the outcomes. For the point estimates there will be no difference, since the coefficient will still show the difference between the average outcomes for treated and control pairs within each block. More precisely, the type of variation used in such estimation is still cross-sectional, where same pairs in different years are treated as different pairs.

Such data structure, however, would require a special inference procedure. Appendix E details two different methods to derive the distribution of standard errors and point estimates. The first method is a standard bootstrap procedure applied within each block: it samples observations with replacement, performs the regression as in Equation 6, calculates the mean and the standard error, and repeats these steps one thousand times. The second method performs the same regression analysis with the same number of iterations, but the re-sampling method is tailored specifically for my data: it samples observations only from the control group, while keeping the treated observations intact at each re-sampling step. Both procedures show that the point estimates, i.e. the nine $\hat{\tau}$ coefficients for each block, correspond to the means of the simulated distributions, while the standard errors are systematically lower without re-sampling. In what follows I will report the (more conservative) standard errors which correspond to the means of the distributions resulting from the bootstrap procedure.

4 Results ▲

This section summarizes the main results of the estimation, including the estimates in the full sample and across blocks. I also reveal and discuss the selection bias that arises in case of using alternative research designs.

4.1 Average PTA Effects

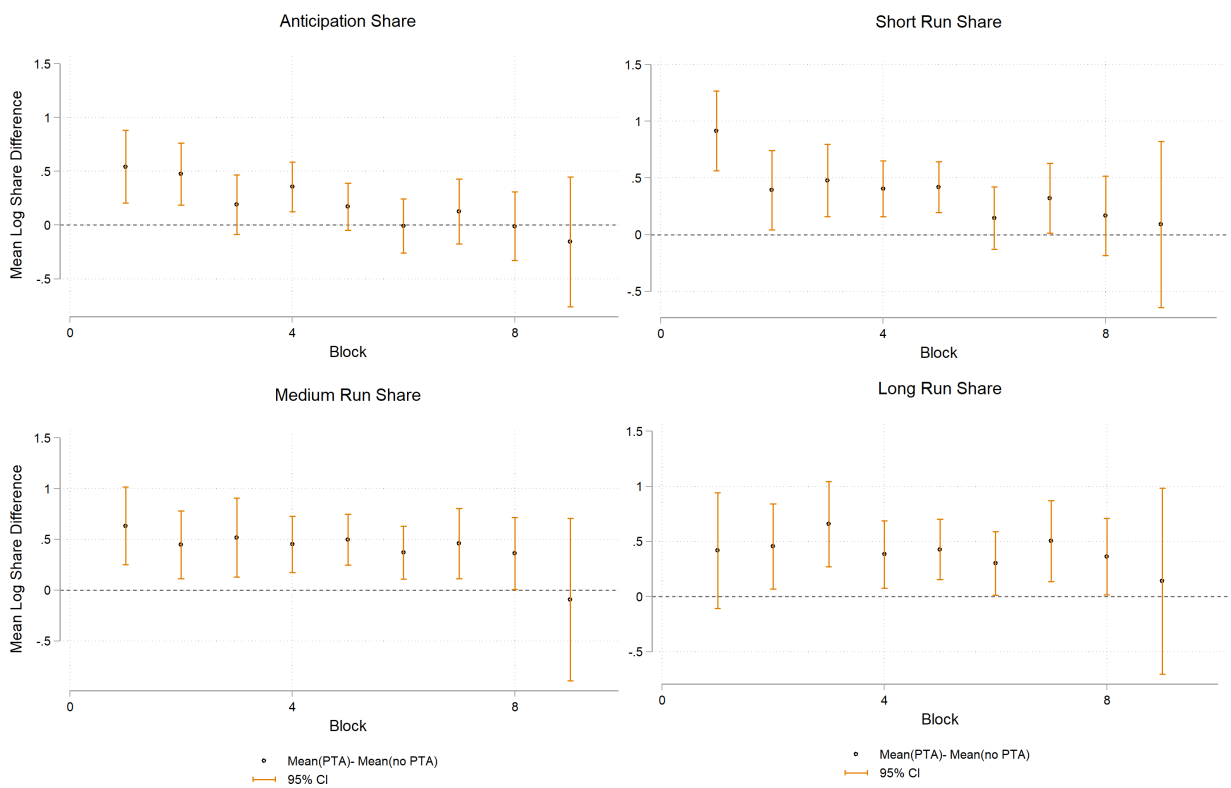
The first set of estimates I am going to focus on is the average effects of PTAs in different time periods across all blocks. Recall that the estimates are obtained by taking a weighted average of all individual block estimates for a particular time period. Table 6 shows the average treatment effects of PTAs on their members and the bootstrapped standard errors. The estimates represent the percentage increase in the average normalized market shares caused by PTAs. The full effect of a PTA is a 48% increase in the normalized market share after ten years since the entry of agreement into force. One third of that total effect (16%) is already realized in anticipation, i.e. five years prior to agreement's entry into force. The implementation in the short run (five years since entry into force) is responsible for additional 20 percentage points.

▲ Table 6: Average PTA effects in different time periods.

	Anticipation [t-5; t=0)	Short Run (t=0; t+5]	Medium Run (t+5; t+10]	Long Run (t+10; t+15]
Coefficient	0.15	0.32	0.39	0.39
Std. Err.	0.054	0.061	0.065	0.069
Percent	16%	37%	48%	48%

Note: 'Coefficient' is the weighted average of the block estimates from estimating Equation 6 for each block within a given time period. 'Standard error' is the mean of the standard error distribution from the bootstrap procedure described in Appendix E. The percentage increase of normalized market shares of treated pairs relative to controls is calculated using the standard formula for interpreting dummy variable coefficients: $\exp(\hat{\tau}) - 1$.

These average effects, however, are not the same across blocks. An additional intuition unveils when we compare the dynamics of PTA effects across different types of country pairs. Figure 5 shows the point estimates for each of the nine blocks in anticipation, short, medium and long run. The anticipation effect (the upper-left panel of Figure 5) is driven entirely by country pairs in lower-index blocks, while there are no effects for natural trading partners. In the long run these differences in effects across blocks largely disappear, and the same effects are observed within every block (the lower-right panel of Figure 5).



▲ Figure 5: Average treatment effects within blocks in different time periods.

Note: The figure plots the point estimates of $\hat{\tau}$'s from Equation 6 for each of the nine blocks and each time period. The 95% confidence interval is calculated using the standard errors obtained from the bootstrap procedure described in Appendix E

The anticipation effects of PTAs have been highlighted in the previous literature, and the suggested mechanisms include the actual reduction in trade costs prior to agreement's official entry into force; and firm behavior. The first explanation has been described by policy makers (see, for example US Trade Representative description of the steps involved into PTA implementation), and relates to a technical procedure which ensures that countries comply with their PTA obligations on the day of entry into force. PTA implementation is a complex process involving the cooperation of many government bodies (ministries, agencies, customs authorities), and the gradual preparation for the actual day of entry into force is necessary. The second explanation relates to the idea that firms adjust their behavior in expectation. Higher future profits would encourage firm to invest more into the new markets and increase trade before the agreement's entry into force.

In light of the second finding – that the dynamic effects are different across types of country pairs – both explanations are reasonable. First, the reduction in trade barriers for less natural trading partners is likely to be disproportionately large relative to country pairs which are close and trade a lot with each other. This explanation emphasizes the potential heterogeneity in the size of trade cost shock, rather than the varying responses of country pairs. Second, the trade behavior of firms may also differ across different destinations. In distant markets characterized by weak trade connections, firms might want to establish market presence in anticipation of the reduction in trade barriers. In markets of natural trading partners firms might be willing to wait until the barriers are actually reduced.

4.2 Estimates and Selection

In this subsection I discuss the magnitudes of the resulting PTA effects and compare them to the estimates obtained by applying alternative research designs. Recall that the main purpose of the empirical strategy in this paper is to reduce the size of the bias associated with countries' selection into PTAs. In cases which correspond to the standard gravity setup, we expect the sign of the bias to be positive and thus the coefficient overestimating the effects of PTAs, since both the probability and past trade are associated positively with the treatment and the outcome variable.[19]

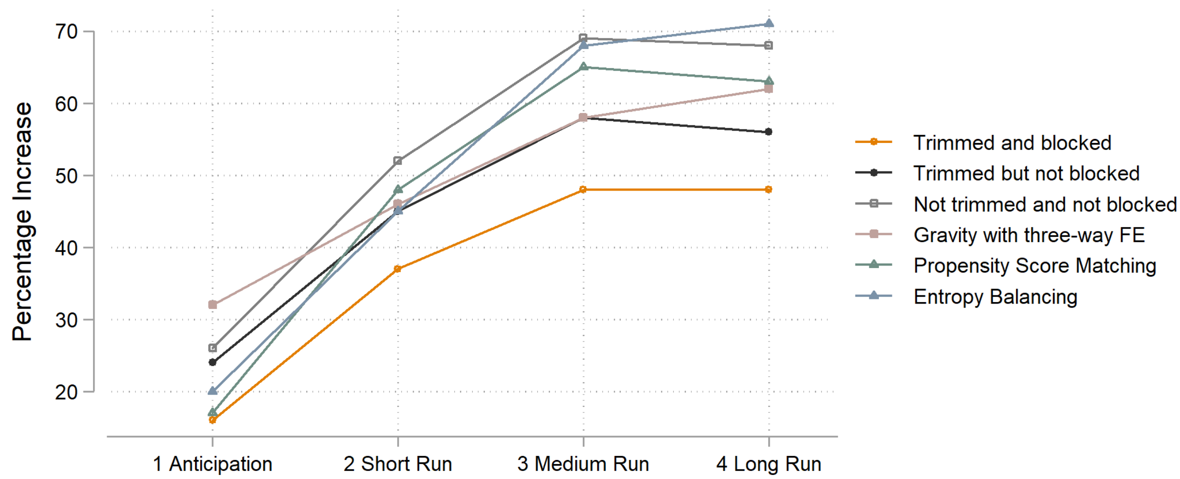
Besides accounting for the probability of selecting into PTAs and past trade, additional bias reduction comes from improving the balance in covariate distributions between treated and control pairs. Each step of the design – trimming and blocking – is aimed at reducing a part of the bias by ensuring that pairs are comparable. Trimming helps to get rid of pairs which do not have counterparts in the other treatment group in terms of their probabilities to get a PTA. Blocking further groups observations such that the propensity scores are similar and the covariate distributions within each block are balanced, to make comparisons closer to the randomized experiment setup. Finally, regression adjustment takes care of the remaining differences in covariate distributions without heavy reliance on extrapolation or functional form.

Another source of bias which would arise in the standard form of the empirical gravity equation is the incorrect form of controlling for economic size. As mentioned earlier, since size is affected by PTAs, a simple conditioning on size may introduce a bias in the treatment estimates. The sign of this bias is likely positive due to the structure of correlation between trade outcomes, size and treatment. This form of bias can persist even when including exporter-time and importer-time fixed effects, since those encompass all

[19] A negative selection bias would only arise if there were omitted variables which would be either correlated positively with trade outcomes and negatively with PTAs, or correlated negatively with trade outcomes and positively with PTAs. Since I control for all the confounders which previous research has been using, the potential remaining omitted variable biases would remain the same.

the factors that are varying across origin or destination countries and time.

Figure 6 presents the estimates from alternative designs: without blocking, without trimming and blocking, an empirical gravity model with three-way fixed effects, and two non-parametric techniques. The latter are propensity score matching as in Baier and Bergstrand (2009) and entropy balancing as in Egger and Tarlea (2017). Each dot represents the percentage increase implied by the point estimate in a given period using a given estimator.



▲ Figure 6: Estimates of PTA effects using alternative research designs.

Note: The percentage increase in the outcome variable of treated pairs relative to controls is calculated using the standard formula for interpreting dummy variable coefficients: $\exp(\hat{\tau}) - 1$.

First, it is clear that the estimates presented in Table 6 and Figure 5 are substantially lower than all the other alternatives estimates in the long run. Second, each part of the research design – trimming and blocking – is responsible for reducing a fraction of the positive selection bias. For example, not blocking the dataset would increase the estimates in each time period by 8-10 percentage points. Trimming has an important bias-reduction property for the long run coefficients: PTA effects are reduced by 20 percentage points when applying the preferred design as opposed to running a regression in the full sample. Third, the comparison to the gravity estimates demonstrates the importance of dealing with economic size. The model with three-way fixed effects (origin-time, destination-time, and country-pair fixed effects) doubles the effects in anticipation - from 16 to 32%, and overestimates the long run effects by 14 percentage points. Finally, an interesting comparison emerges when looking at propensity score matching and entropy balancing estimators: they give very similar estimates in anticipation, but overestimate the effects in the long run by 15% and 23% respectively.[20]

As mentioned, an empirical gravity-type specification may not take into account the positive selection bias of country pair choosing to form a PTA. This may imply that the log-linearized version of the gravity equation may not be a good empirical model to measure the effects of PTAs, or that the real data-generating process is not coming from the theoretical structural gravity equation. Setting aside the second possibility, Appendix G provides a numerical example which demonstrates that the empirical version of the gravity equation may indeed not take into account the self-selection bias. In this example I assume that the data generating process is indeed a structural gravity equation (following a simple model presented in the next section), and thus in this example I rule out the questions related to the structure of trade. I simulate 500 panel datasets for two cases: random and non-random assignment of PTAs (see the details in Appendix G), and estimate the average PTA effect for each of those. As Figure 1 in Appendix G shows, this stylized numerical example clearly demonstrates that the gravity-type estimator may substantially overestimate the effects of non-random PTAs. It is also clear from that figure that the blocking estimator cannot eliminate the bias in the point estimate entirely for the majority of iterations. When combined with the estimates for the standard errors at each iteration, however, the blocking estimator is the only one that includes the true value (see Figure 2 in Appendix G).

5 From Partial to General Equilibrium: an Application ▲

The estimates presented in Table 6 and Figure 5 should be interpreted as the average partial equilibrium effects. For example, if we pick two small countries and endow them with a PTA, their bilateral normalized market shares are expected to increase on average by 48% in the long run, while there will be virtually no effects on other countries. This assumption is plausible if countries are small: diversion of trade from other trading partners would likely be economically small and statistically insignificant. However, it does not mean that the estimates are not suitable for studying bigger PTA formations.

In this section I use a standard trade model of Costinot and Rodríguez-Clare (2014) to make predictions about the changes in welfare and trade patterns following the entry into

[20]For the propensity score matching estimator, the size of the bias remains due to the ultimate lack of balance in the covariate distributions, as shows by King and Nielsen (2019). Entropy balancing is essentially a weighting procedure: it calibrates unit weights so that the reweighted treatment and control group satisfy a set of prespecified balance conditions that incorporate information about known sample moments. The calibration, however, minimizes the divergence from a set of baseline weights chosen by researchers, and thus the method could be inconsistent unless the correct functions are specified.

force of the Regional Comprehensive Economic Partnership Agreement (RCEP). RCEP is a free trade area formed between China, Japan, South Korea, Australia, New Zealand and ten Southeast Asian economies (Brunei, Cambodia, Indonesia, Laos, Malaysia, Myanmar, the Philippines, Singapore, Thailand, Vietnam). The 15 member countries of RCEP account for about 30% of the world's population (around 2.2 billion people) and about 30% of global GDP (29.7 trillion USD), making it the largest trade bloc in history. The agreement was signed in November 2020, and entered into force in the beginning of 2022. The scale and the timing of RCEP make it an interesting and a policy-relevant PTA to study.

6.1 The 'Off-the-Shelf' Model

To study the general equilibrium effects and conduct counterfactual exercises I use the simplest quantitative trade model: the Armington model.[21] In the setup and notations I follow closely Costinot and Rodríguez-Clare (2014), which I briefly repeat here.

The world economy is composed if $i = 1, \ldots, N$ countries, each endowed with Q_i units of distinct good $i = 1, \ldots, N$. A representative agent in each country has preferences characterized by the Constant Elasticity of Substitution (CES) utility function:

$$C_j = \left(\sum_{i=1}^{N} \psi_{ij}^{(1-\sigma)/\sigma} C_{ij}^{(\sigma-1)/\sigma} \right)^{\sigma/(\sigma-1)}$$

where C_{ij} is the demand for good i in country j; ψ_{ij} is an exogenous preference parameter, and $\sigma > 1$ is the elasticity of substitution of goods between different countries. The price of good i in country j is P_{ij}, and the consumer price index in country j is given by:

$$P_j = \left(\sum_{i=1}^{N} \psi_{ij}^{(1-\sigma)} P_{ij}^{1-\sigma} \right)^{1/1-\sigma}$$

Trade costs are assumed to be of the iceberg form: $\tau_{ij} > 1$, with $\tau_{ii} = 1$. The price of good i in country j is equal to $P_{ij} = \tau_{ij} P_{ii}$. The domestic price $P_{ii} = Y_i/Q_i$, where Y_i denotes country i's total income. Thus, we can express the price of good i in country j as $P_{ij} = Y_i \tau_{ij}/Q_i$.

Let X_{ij} denote the total value of country j's imports from i, and $E_j = \sum_{i=1}^{N} X_{ij}$ denote country j's total expenditure. Bilateral trade flows satisfy:

[21] The gravity equation, which is a centerpiece of the this model, can be derived from a variety of micro-theoretical foundations and economic environments. The reason to use the simplest model is that it has relatively low data requirements, yet it still captures the main components of the counterfactual exercise. The welfare and trade predictions generated by this model can be interpreted as the lower bound for gains from trade, as shown in Tables 1 and 2 of Costinot and Rodríguez-Clare (2014).

$$X_{ij} = \left(\frac{\psi_{ij} P_{ij}}{P_j} \right)^{1-\sigma} E_j$$

Combining the expression for bilateral trade flows, the price index, and the price of good i in country j, we obtain the gravity equation:

$$X_{ij} = \frac{(Y_i \tau_{ij})^{-\varepsilon} \chi_{ij}}{\sum_{l=1}^{N} (Y_l \tau_{lj})^{-\varepsilon} \chi_{lj}} E_j \tag{7}$$

where $\chi_{ij} = (Q_i/\psi_{ij})^{\sigma-1}$, and $\varepsilon = \sigma - 1$ is the trade elasticity.

In competitive equilibrium the budget constraint and the goods market clearing imply $E_i = Y_i$, and $Y_i = \sum_{j=1}^{N} X_{ij}$ for all countries. Equation 7 together with these two conditions yields the system describing the world income distribution:

$$Y_i = \sum_{j=1}^{N} \frac{(Y_i \tau_{ij})^{-\varepsilon} \chi_{ij}}{\sum_{l=1}^{N} (Y_l \tau_{lj})^{-\varepsilon} \chi_{lj}} Y_j \tag{8}$$

In principle, with a simplification that preference parameters do not vary across destinations $\psi_{ij}^{(1-\sigma)/\sigma} = \theta_i$, a numeraire rule for the distribution of incomes ($\sum_i Y_i = 1$), and the data on X_{ij} and Y_i, we could calibrate the model to find the trade costs and demand parameters by jointly solving Equation 7 and Equation 8. This, however, is not necessary if the goal is to conduct counterfactual exercises using the model. Instead, I use the approach which became known as the "exact" version of Jones's hat algebra (see, for example, Dekle et al. (2008)).

Consider a shock to trade costs from $\boldsymbol{\tau} = \{\tau_{ij}\}$ to $\boldsymbol{\tau'} = \{\tau'_{ij}\}$ (for example, PTA entry into force). Denote all changes in variables with a 'hat', where $\hat{v} = v'/v$ is the proportional change in any variable v between the initial and the counterfactual equilibria. Let $\lambda_{ij} = X_{ij}/\sum_l X_{lj}$ be the share of expenditure of country j on goods coming from country i. Since the gravity equation holds in both initial and counterfactual equilibria, the change in the expenditure shares can be expressed using changes in income distributon, changes in trade costs, and the initial expenditure shares:

$$\hat{\lambda}_{ij} = \frac{(\hat{Y}_i \hat{\tau}_{ij})^{-\varepsilon}}{\sum_{l=1}^{N} \lambda_{lj} (\hat{Y}_l \hat{\tau}_{lj})^{-\varepsilon}} \tag{9}$$

To then compute changes in the income distribution we can use the observation that in the counterfactual equilibrium Equation 8 implies:

$$Y_i' = \sum_{j=1}^{N} \lambda_{ij}' Y_j'$$

Combining the two previous expressions we obtain the system of equations defining the changes in the income distribution as follows:

$$\hat{Y}_i Y_i = \sum_{i=1}^{N} \frac{\lambda_{ij}(\hat{Y}_i \hat{\tau}_{ij})^{-\varepsilon} \hat{Y}_j Y_j}{\sum_{l=1}^{N} \lambda_{lj}(\hat{Y}_l \hat{\tau}_{lj})^{-\varepsilon}} \tag{10}$$

Equation 10 shows that we can compute the counterfactual changes in income without having to estimate trade costs, endowments or preference shifters. After we determine the changes in the income distribution, we can compute changes in expenditure shares using Equation 9. Finally we can compute the changes in real consumption (welfare)[22] using changes in domestic expenditure shares on domestic goods:[23]

$$\hat{C}_j = \hat{\lambda}_{jj}^{-1/\varepsilon} \tag{11}$$

An important thing to note here is that this version of the model studies the static counterfactual equilibrium which would result from the changes in *iceberg trade costs*. In particular, the changes in welfare defined in Equation 11 do not take into account the changes in tariff revenue.

There are at least two reasons why this model structure is suitable to study the implications of trade cost reductions such as PTAs. First, in order for the tariff revenue to make a difference for the predictions of the model, the changes in tariffs have to be substantial.[24] For example, Costinot and Rodríguez-Clare (2014) estimate the welfare changes as a function of tariff size, and show that the optimal tariff of around 20% is associated with modest gains from trade (ranging from 0.3% for the US to 1.3% for Ireland). At the same time, the world applied weighted average tariffs since 1988 have not exceeded 10%, and have steadily declined since 1994, reaching 2.7% in 2017 (see Figure 5 in Appendix D). The data on applied tariffs before 1988 is scarce, but as Bown and Irwin (2015) show, even by the beginning of the Kennedy Round of multilateral trade negotiations in 1964,

[22]In the context of the Armington model we use the words 'real consumption changes' and 'welfare changes' interchangeably, meaning the percentage change in income that the representative agent would be willing to accept in the lieu of the trade shock.

[23]For the details on the derivation of this result see Costinot and Rodríguez-Clare (2014).

[24]The change in welfare in that model would be defined as $\hat{C}_j = \left(\frac{1-\pi_j}{1-\pi_j'}\right) \hat{\lambda}_{jj}^{-1/\varepsilon}$, where π_j and π_j' are the share of tariff revenues in the initial and counterfactual equilibria (see section 4.1 of Costinot and Rodríguez-Clare (2014)).

the average tariffs for the major players in the GATT were about 15%. The average tariffs were reduced to below 10% for the GATT members by the end of the round, and pushed further down by the subsequent multilateral negotiations and the admission of the new members into the GATT.

Second, PTAs include multiple provisions regulating trade in goods which go beyond plain tariff reduction (see, for example, Limão (2016)). Especially since the 1990s, when the majority of PTAs in my sample enter into force, trade agreements aim at reducing non-tariff barriers to trade, harmonizing rules, enhancing the efficiency of customs, and covering trade-related rules (such as intellectual property provisions or labor regulations). Therefore, if we were to model PTAs as purely tariff reductions, we would likely substantially underestimate the trade and welfare effects.

Thus, the view about the counterfactual trade cost reductions in this paper is such that PTAs have effects beyond tariffs, and the losses in tariff revenue due to a PTA are not large enough to offset the gains from trade. In fact, the agreement I am going to study – RCEP – represents a good case in point. RCEP covers multiple areas relating to trade in goods, trade in services, investment, economic and technical cooperation, and creates new rules for electronic commerce, intellectual property, government procurement, competition, and small and medium sized enterprises.

At the same time, the applied weighted average tariffs in RCEP countries in the year of signature were at 2.12%, comparable with the average world applied tariffs (see Figure 5 in Appendix D). Table 14 in Appendix D additionally demonstrates that the highest tariffs among RCEP countries in 2020 were applied by Cambodia (6.21%) and Korea (5.48%), but all the other members have tariffs well below 5%. In fact, the bilateral tariffs of RCEP countries are even lower than the average applied tariffs, since many country pairs had a pre-existing free trade agreement. In particular, the PTA among the ten Southeast Asian nations (ASEAN) was signed in 1992, and completely eliminated tariffs in mutual trade between five countries (Malaysia, Brunei Darussalam, Indonesia, the Philippines, Singapore and Thailand) by 2010, while substantially reducing tariffs among the remaining members. ASEAN as a bloc signed a trade deal with Japan in 2008, with Australia and New Zealand in 2009, with China in 2010, and with Korea in 2010. Thus, since tariff revenue losses are not large for the RCEP countries after the formation of the free trade area, the model outlined in the previous section is suitable to study the effects of this trade agreement.

6.2 Application: Regional Comprehensive Economic Partnership

This section shows how we can combine the empirical estimates from the first part of the paper with the model to study policy-relevant questions. To conduct counterfactual exercises, I use data for the year 2015, with 88 countries (see Table 15 in Appendix D) forming 7744 country pairs.[25] I use trade flows for that year and compute the income distribution as a share of each country in the total world income.

I conduct two types of counterfactual exercises. The first one endows RCEP countries with a trade agreement using the long run average estimates obtained in the empirical part of the paper.[26] Setting the trade elasticity $\varepsilon = 5$,[27] and given the long run estimate of 48% increase in normalized market shares, the shock corresponds to a 9.6% reduction in iceberg trade costs for the RCEP members. The second type of the counterfactual exercise exploits the full heterogeneity of the empirical estimates. Although the model does not feature any dynamics, we can still use it in a series of static exercises to study changes in real consumption and trade reallocation in the anticipation period and long run.

Counterfactuals: Long Run. Given the 9.6% reduction in iceberg trade costs for all RCEP members, the model predicts the simple average reduction in welfare of 0.05% (see Figure 6 in Appendix D for the distribution of changes in welfare, income, expenditure shares and normalized market shares across all countries). Weighted by the initial share in the total world income, however, the average change in welfare is predicted to be positive, although negligible (0.0005%). Figure 7 maps the percentage changes in real consumption following trade shock.

Naturally, RCEP members are the winners in terms of welfare after the PTA formation. The biggest gain is recorded for Myanmar, with a 18.3% increase in real consumption. The effect comes from both the increase in size by 9%, and the reduction in the price index by 7.9% (see Table 15 in Appendix D for the decomposition of the changes in real welfare into size and price effects). Myanmar experiences by far the largest effect, followed by

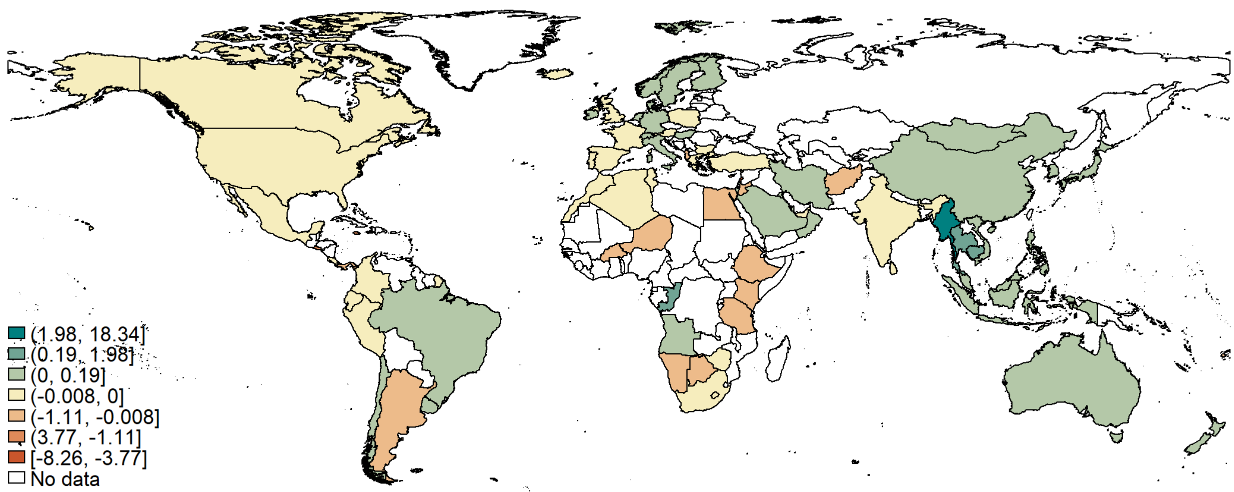
[25]In 2015 I observe most of the domestic trade flows necessary to conduct the general equilibrium exercise, in particular, among RCEP members. At the time of writing, UNIDO manufacturing production data for 2019 and 2018 is unavailable for such countries as China, Japan, and Korea, as well as many others.

[26]Since the estimates in the empirical part do not differentiate between different types of trade agreements, this simplification implies that I treat RCEP as an 'average' trade agreement. It is a plausible approximation, since RCEP includes many of the elements of modern trade agreements, while tariff levels among its members are at the world average.

[27]Appendix H provides the sensitivity checks using alternative values of elasticity. The role of elasticity is two-fold in the model: on one hand, it amplifies the trade effects of trade cost changes, but on the other hand it decreases the magnitude of reductions in iceberg trade costs. Appendix H demonstrates that the values of elasticity influence primarily the distribution of the growth rates of normalized market shares for the RCEP economies, while having relatively little differences in the welfare growth rate distribution.

Cambodia with 1.98%. The simple average gain for RCEP economies equals 1.75% (0.24% without Myanmar). However, since large gains are recorded for smaller countries, like Myanmar and Cambodia, while China, Japan and Korea gain less than one percent each, the weighted average gains are quite modest (0.0018%).

For the rest of the world changes in real consumption are negligible, constituting less than half a percent change on average. The biggest gains outside of the block are recorded for Congo, with the increase of 1.1%. The main losers from the formation of RCEP in the long run are small countries outside of the block, such as French Polynesia (8.3% reduction in real consumption), Lebanon (6.2% reduction), and El Salvador (3.1% reduction). For all of these countries, even though the price index is decreasing, the reduction in size dominates (again, for the decomposition see Table 15 in Appendix D).

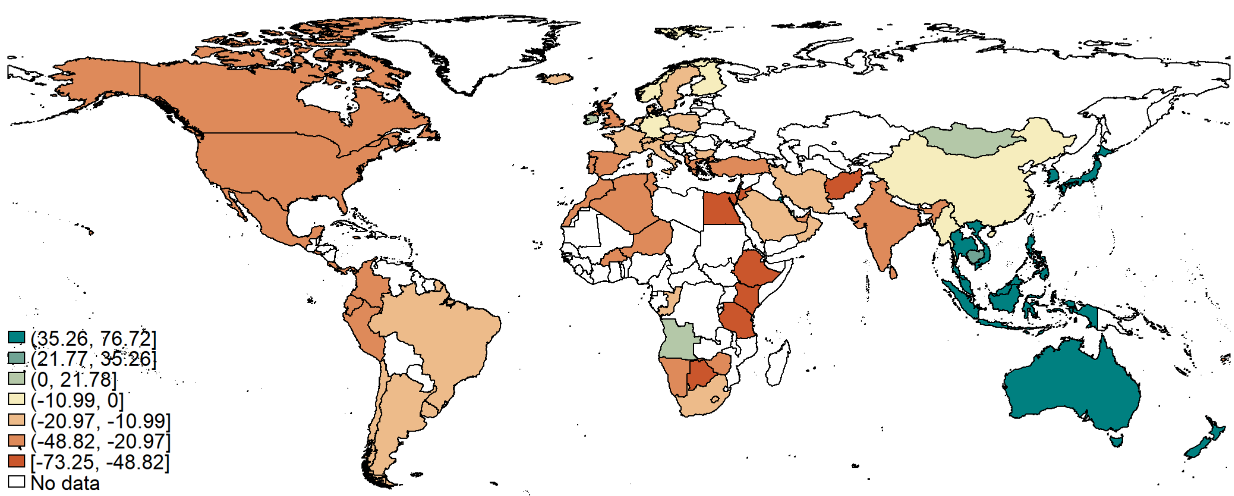


▲ Figure 7: Percentage changes in welfare following the RCEP entry into force in the long run.

Note: The shock corresponds to a 9.6% reduction in iceberg trade costs for the RCEP members (using the estimated PTA effects and the trade elasticity of $\varepsilon = 5$).

We can also analyze the changes in the trade patterns following the shock. Figure 7 in Appendix D plots the distributions of the gross growth rates of normalized market shares of different groups of countries. In the new equilibrium, almost all RCEP members redirect trade towards each other (on average their normalized market shares increase by 56.24%), while reducing exports to the outside world (on average normalized market shares with the outsiders fall by 23.65%). Similarly, the countries outside of RCEP start trading more within themselves (on average, outsiders' normalized market shares in mutual exports increase by 21.37%). As an example of trade pattern change, Figure 8 maps the changes of China's normalized market shares with other countries. China increases its normalized market shares primarily with the RCEP countries, such as Malaysia (76.72%), South Korea (67.95%), and Indonesia (58.08%). Among the countries that China trades less with in the new equilibrium are small economies, which are highly dependent on China's trade, but are not a part of RCEP, such as Macao (-73.26%) and Hong Kong (-64.58%). Notably, China decreases the share with its largest market – the domestic one – by a considerable 2.3%.

As shown in Section 4, alternative research designs tend to overestimate the effects of preferential trade agreements. Appendix I repeats the counterfactual exercise using the estimates from the three-way fixed effects gravity regression. In particular, I use the partial equilibrium estimate of 68% as predicted by the empirical gravity model, and the same value of elasticity, $\varepsilon = 5$, which translates into 13.6% reduction in iceberg trade costs. The average changes in real consumption are very similar, and the simple t-test cannot reject the null hypothesis that the difference is equal to zero for the two welfare vectors. Since the changes in real consumption are negligible for most of the countries in both versions of the model, this result is not surprising. There are, however, substantial differences in growth rates of normalized market shares for the RCEP countries. While the mean increase in the baseline model is 56.2%, it is almost double when using the gravity estimates (90.9%). Appendix I demonstrate that correctly identifying partial equilibrium estimates matters for the predictions of normalized market shares growth rates of the directly affected countries.



▲ Figure 8: Percentage changes in China's normalized market shares with other countries following the RCEP entry into force in the long run.

Note: The shock corresponds to a 9.6% reduction in iceberg trade costs for the RCEP members (using the estimated PTA effects and trade elasticity of $\varepsilon = 5$).

Counterfactuals: Transition to the Long Run. To construct the reductions in iceberg trade costs for different time periods, I use point estimates from the empirical part corresponding to different blocks and the value of the trade elasticity $\varepsilon = 5$,[28]. In anticipation there are substantial differences for point estimates, while in the long run they are similar across blocks (with the exception of block nine). Table 16 in Appendix D gives more details in the coefficients and the corresponding reductions in iceberg trade costs used in the counterfactual general equilibrium exercise. Among the RCEP economies, there is only one country pair which belongs in the first block (i.e. the lowest probability of forming a trade agreement), which is Myanmar and New Zealand. Other examples of pairs in lower-index blocks include Myanmar and Korea or Australia and Cambodia. Blocks nine and eight have the most number of pairs (32 and 33 pairs respectively), indicating that the majority of RCEP members are natural trading partners. Those blocks include pairs such as Vietnam and Thailand, or China and Korea. I apply the trade costs reductions sequentially, i.e. the counterfactual equilibrium resulting from the shocks in anticipation period is used as a baseline equilibrium for the long run shocks.[29]

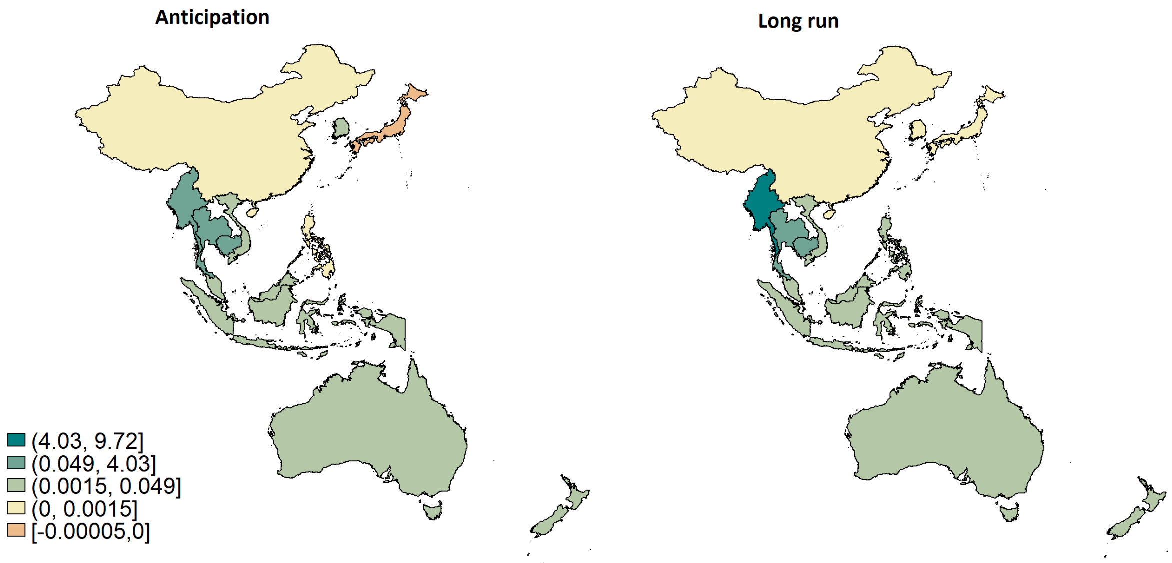
Figure 9 maps the percentage changes in real consumption in anticipation and long run using the heterogeneous block estimates. In anticipation the only country which experiences a decline in welfare (although negligible) is Japan.[30] With the exception of Myanmar, which increases its real consumption by 4.03%, changes in welfare for RCEP countries in anticipation are negligible (simple average of 0.06%, and weighted average of 0.0005%). In the long run, again, Myanmar's gain of 9.72% by far exceed those of other RCEP countries (simple average gain of 0.11% and weighted average gain of 0.0007%).

Similarly to the previous counterfactual exercise, we can also analyze the changes in trade patterns after the shock in anticipation and in the long run. Table 17 in Appendix D provides the model-implied average changes in normalized market shares of RCEP members by block. Normalized market shares of RCEP members in trade with each other increase on average by 15.76% in anticipation, and by 25.84% in the long run. In anticipation less natural trading partners within RCEP (blocks 1-4) experience growth in mutual normalized market shares, by 36.37% on average, while natural trading partners do not experience any substantial changes in bilateral trade. In the long run, on the contrary, natural trading partners are the ones experiencing most growth (41.16%), while pairs distributed

[28] Again, Appendix H provides the sensitivity checks using alternative values of elasticity. It demonstrates that trade flows (normalized market shares) can be sensitive to elasticity values, while it is not true for the welfare growth rates.

[29] I.e. the reduction in the iceberg trade costs from the anticipation to long run period is defined as the difference between these two periods.

[30] This happens because all country pairs including Japan as an exporter or importer are sorted into higher-index blocks, which have no reductions in trade costs in anticipation.



▲ Figure 9: Percentage changes in welfare in anticipation of RCEP formation, and in the long run.

Note: The shock corresponds to reductions in iceberg trade costs specified in Table 16 in Appendix D for different blocks (using the estimated PTA effects and the trade elasticity of $\varepsilon = 5$).

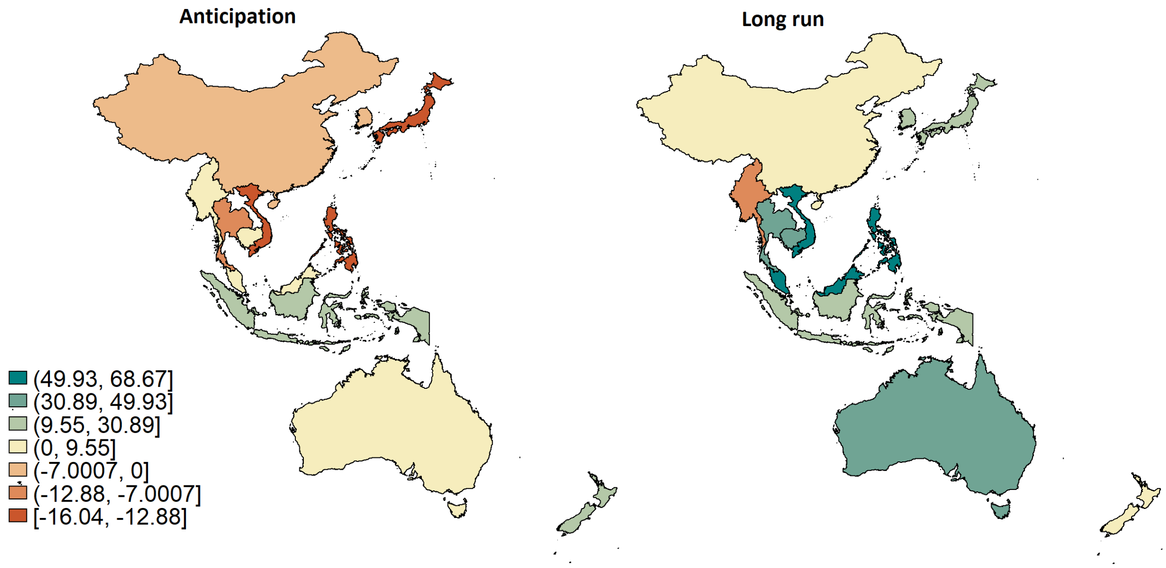
to lower blocks experience mild changes in trade patterns.[31]

Figure 10 maps percentage changes of China's normalized market shares with its RCEP partners in anticipation and long run. In anticipation China decreases trade with a few 'more natural' trading partners, such as Japan in block nine (-16.04%), Philippines in block nine (-15.28%), and Vietnam in block five (-13.81%); while redirecting trade towards Indonesia in block four (30.89%), and New Zealand in block two (25.53%). In the long run, China increases its trade with all RCEP members (except Myanmar), with normalized market shares for Vietnam (+68.67%), Philippines (+51.63%) and Malaysia (+50.25%) rising the most. China also reduces domestic trade in anticipation of RCEP (by 2.54%), while there is almost no change in it in the long run (0.08%).[32]

Drawing on the conclusions from the two exercises presented in this section, the trade shocks from RCEP formation in this model have small effects on the real consumption of

[31] With the exception of block one, which contains only one country pair (Myanmar and New Zealand).

[32] The two counterfactual exercises provide different perspectives on the formation of PTAs. The first exercise assumes larger changes in trade costs over the period over 20 years years (long run). The estimate used in this exercise is a weighted average of the individual block estimates. The second one considers smaller changes in the years preceding PTA formation and in the first five years after the PTA enters into force (anticipation and short run), followed by additional reductions some ten years after that. The estimates used in this exercise are different across pairs. Thus, the cumulative gains from the anticipation and long run of the second exercise are not supposed to add up to the gains from the first exercise.



▲ Figure 10: Percentage changes in China's normalized market shares with other RCEP members in anticipation of RCEP formation, and in the long run.

Note: The shock corresponds to reductions in iceberg trade costs specified in Table 16 in Appendix D for different blocks (using the estimated PTA effects and the trade elasticity of $\varepsilon = 5$).

most countries. The largest effects are trade creation, i.e. the disproportionate increase in trade (normalized market shares) of RCEP economies within the PTA, and the outsides among themselves.

6 Conclusion and Future Research ▲

My paper estimates the effects of preferential trade agreements on trade between members. Using the causal inference framework to address the self-selection bias, I show that PTAs gradually increase bilateral trade, starting prior to the agreement's entry into force. Fifteen years later member countries trade 48% more relative to similar pairs without an agreement. Although the long-run effects are similar across country pairs, there is substantial heterogeneity in the dynamic responses. In particular, only non-natural trading partners react in anticipation.

The findings in this paper open up a few important questions for future research. A natural first question is what is driving the increase in trade between non-natural trading partners prior to the actual reduction in trade costs. One potential explanation requires looking closer at the firm behavior. To understand this phenomenon we need both an appropriate theoretical model and a thorough empirical investigation. On the theory side, we could think about alternative models of firm behavior, where, for example, firms would want to invest in a future prospective market, acquire or expand a customer base, and reap the benefits of the first-mover advantage in remote markets.

To empirically check these theoretical alternatives, in a separate project I use Colombian monthly firm shipment-level data across different destinations. I collect data on Colombia's PTA partners from 2006 to 2020, as well as detailed information about the negotiation process (the announcement of the negotiations, the timing of the negotiation rounds, the treaty signature, etc.). Colombia has signed a number of trade agreements both with natural (for example, Peru), and non-natural trading partners (for example, Israel). Exploiting the variation across destinations for Colombian firms exports, I analyze trade patterns over the life cycles of trade agreements. The universe of transactions helps me disentangle the contributions of extensive and intensive margins of trade.

The results of the empirical investigation of Colombia's firm-level trade flows across destinations open the next set of questions: why do we observe these differences in responses across destinations? Are different types of firms selecting into exporting to a particular type of the destination? Or are the same firms behaving differently in different destinations? These are the questions I aim to answer in my next project.

There are alternative explanations to the observed anticipatory effects that also deserve attention in future research. One possibility of why firms might increase their trade volumes in anticipation of a PTA is that the change in regulation itself might involve an additional cost. An example of such costs are the complex rules of origins that some PTAs impose. Combining product-level data that I have for Colombia with specific provisions for rules of origins or standards within PTAs could shed light on whether firms react to the associated regulatory changes by increasing their trade volumes prior to the actual change.

Another possible reason for the anticipatory increase in trade may be that there are actual trade cost reductions prior to a PTA's entry into force. Since trade treaties represent a complex set of rules, the regulators might want to start implementing the changes beforehand, to avoid a customs blockage on the day of entry into force. Checking this hypothesis would require high-quality and high-frequency data on applied tariffs or other regulations. Collecting such data from the bilateral schedules laid out in detail in the attachments to each trade agreement would contribute to the efforts of the trade economists to understand the impacts of well-defined trade policies.

The second prospective avenue for future research concerns a puzzle uncovered in this paper: the type of country pair does not seem to define the type of the trade agreement this pair will sign. In principle, we would expect that natural trading partners will tend to sign more comprehensive and binding trade agreements. In this paper, however, I estimate the effects of an 'average' trade agreement precisely because the characteristics of country pairs cannot predict the various features of PTAs (for example, being a customs union or a free trade area) within blocks. In addition, there is limited variation in agreement types across blocks. Since the empirical design used in this paper does not allow me to study each type of trade agreement separately due to power issues, in my future research I plan to tackle this issue in a more hands-on manner.

The first step is to classify PTAs according to their structure, content, coverage, depth, length, and legal enforceability. Given the high dimensionality of PTA heterogeneity, the first challenge is to understand the features of agreements that matter most for trade flows. In an ongoing project I utilize the digitized legal texts of concluded preferential trade agreements to construct a novel dataset with individual characteristics of PTAs. The next step is to use this new comprehensive dataset to study the effects of the relevant regulatory features of PTAs on trade flows.

Finally, this project draws attention to the importance of selection bias in estimating the effects of trade policy. While we have tools to estimate the consequences and welfare implications of implemented PTAs, we have limited knowledge about the mechanisms that lead to PTA formation in the first place.

In this paper I used a prediction model to empirically estimate the probability of self-selecting into a PTA given characteristics of country pairs. While some factors – such as economic size, geographical proximity and past trade – have been highlighted in the literature, they still do not explain all the patterns of PTA formation. For example, given these factors, we should observe a strict hierarchy in PTA formation (akin to that to firms exports across destinations). Yet, there is a large number of trade agreements concluded between non-natural trading partners. Additionally, comparative advantage or factor endowments can act as possible economic drivers behind the formation of PTAs. Besides economic forces, we could think of a number of political economy factors (military coalitions or past war conflicts) that matter for the formation of PTAs. While the model of selection is ultimately a theory question, it should be informed by stylized empirical facts. Given that the results of this paper indicate that self-selection produces a large bias that may ultimately misinform policy decisions, the understanding of the assignment mechanism (i.e. how countries decide to form PTAs) should be high on the research agenda.

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▲Appendix A. Data Construction

Bilateral Trade

To construct trade flows from origin i to destination j, I unite the following databases: International Trade and Production Database for Estimation (ITPD-E); WTO Structural Gravity Database; IMF Direction of Trade Statistics Database (data retrieved in 2018); World Trade Flows (WTF) bilateral cross-sectional data; NBER-United Nations Trade Data; and CEPII Gravity Dataset.

Table 1 shows the parameters of each raw dataset: the number of unique countries and country pairs, the time span of the data, the number of observations, the number of missing values; and whether the dataset is a balanced panel. Since ITPD-E, WTO, IMF and WTF datasets only report positive trade flows, they do not contain missing values. However, these datasets, if transformed into balanced panels, will contain a lot of gaps in both cross-sectional and time dimensions. The CEPII Dataset itself collects trade data from several sources, including UN Comtrade, CEPII BACI Database, and IMF Direction of Trade Statistics. The number of missing values varies across different sources.

▲ Table 1: Metadata for raw bilateral trade datasets.

Name	Countries	Pair	Years	Observations	Balance	Missing
ITPDE-E	237	43,623	2000-2016	714,951	No	0
WTO	229	48,711	1980-2016	972,692	No	0
IMF	218	47,030	1948-2017	2,710,148	No	0
WTF	263	50,456	1984-2015	750,556	No	0
NBER	201	23,750	1962-2000	926,250	Yes	499,365
CEPII	248	61,034	1948-2019	3,661,898	No	UN exporter: 2,843,970 UN importer: 2,731,663 BACI: 3,056,279 IMF exporter: 2,770,880 IMF importer: 2,687,346

Note: The number of observations for the CEPII Gravity Dataset is reported after deleting non-existing countries and domestic trade flows.

Since the datasets use different country identifiers, I use concordances to use ISO-3 codes as identifiers throughout. I also make sure that the values are reported in USD across all data sources. I proceed to unite the datasets in the following order:

1. Merge ITPD-E and WTO datasets, gaining 193,597 trade flow observations.

2. Merge the resulting dataset with IMF data, gaining additionally 561,915 observations.

3. Merge the WTF and NBER datasets, and then merge the resulting dataset with the one created at the previous step, resulting in 242,534 additional observed trade flows.

4. Finally, I unite the dataset resulting from step 3, with the CEPII dataset, and construct the final trade volume variable in the following order:

   - Start with IMF data reported by the exporter;
   - Substituting the missing values with UN Comtrade data reported by the exporter (gaining 188,441 observations);
   - Substituting the missing values with UN Comtrade data reported by the importer (gaining 118,152 observations);
   - Substituting the missing values with IMF data reported by the importer (gaining 30,860 observations);
   - Substituting the missing values with BACI data reported by the exporter (gaining 1,228 observations);
   - Substituting the missing values with data constructed in steps 1-3 (gaining 611,237 observations);

I then delete countries that did not exist throughout the whole period of time from 1960 to 2019. The resulting dataset contains 210 unique customs territories, forming 43,890 pairs over the period of 1960-2019. The total number of observations is 2,633,400 in a balanced panel. The number of missing observations is 1,613,684. I then use this dataset for imputation (see Appendix B).

Domestic Trade

In order to construct domestic trade flows from i to i, I complement the data from ITPD-E and WTO with data from TradeProd Database and UNIDO's INDSTAT Rev. 4 Database. Table 2 shows the characteristics for the datasets with domestic flows (for ITPD-E and WTO datasets) and production (for TradeProd and INDSTAT databases): the number of unique countries, year coverage, and the number of observations.

▲ Table 2: Metadata for raw domestic trade datasets.

Name	Countries	Years	Observations
ITPD-E	115	2000-2014	1,356
WTO	160	1980-2016	3,645
TradeProd	180	1980-2006	4,514
INDSTAT	137	1980-2016	3,349

ITPD-E and WTO datasets contain ready-made information on domestic trade flows for some countries and years. In particular, after merging them I have information on 3,084 domestic flows out of the total 7,104 observations (for 192 unique exporters over the period from 1980 to 2016). I then add observations from CEPII TradeProd database, additionally gaining 2,286 observations. I then add observations from INDSTAT Database, gaining 256 observations. Note that since CEPII TradeProd and INDSTAT report production data, I calculate the domestic trade flows as the difference between production and total exports of a country in a given year. I then use this dataset to show that normalized market shares calculated with and without domestic trade flows do not have substantial differences (see Appendix C).

PTAs

To construct the PTA indicator and extract the information about the agreements, I use Design of Trade Agreements Database (DESTA version 2.0, Dür et al. (2014)). The dataset contains all trade agreements ever concluded, both notified and not notified to the WTO, as well as:

- Superseding agreements: for example, Andean Group was formed through a series of agreements – Cartagena Agreement 1969, Quito Protocol 1988, Trujillo Protocol 1997, Sucre Protocol 2003;
- Overlapping agreements: for example, Colombia and Peru are both in Andean Group (Bolivia, Colombia, Ecuador, and Peru) and in Pacific Alliance (Chile, Colombia, Mexico and Peru);
- Accessions: for example, Venezuela joined Andean Community in 1973;
- Withdrawals: for example, Venezuela withdrew from Andean Community in 2006.

To take into account agreements' dynamic, I use the following cleaning protocol:

1. Start with the list of all baseline treaties (without accessions or withdrawals);
2. Filter only Free Trade Areas (FTAs) and Customs Unions (CUs), i.e. delete all Partial Scoope Agreements (PSAs), Framework Agreements, Services Agreements, and Sectoral Agreements;
3. Clean from superseding agreements, amendment protocols, revisions, leaving only the earliest agreements;

4. Represent the dataset in dyadic form;
5. Clean from overlapping agreements[33];
6. Separately recode accessions and withdrawals to dyadic form. For accessions,the entry into force is coded as the year of accession (there are 852 of such country pairs over the whole period). For withdrawals, I code only 'real' withdrawals, i.e. only the cases when countries stop having any type of formal preferential trade arrangement:
   - Brazil-Venezuela from 2006 to 2012: Venezuela exited Andean Community to join MERCOSUR, but was not a member until 2012;
   - Eritrea with Angola, Lesotho, Mozambique, Namibia, Tanzania when the latter exited COMESA;
   - Georgia with Belarus, Kyrgyzstan, Tajikistan when Georgia exited CIS;
   - The rest of the 486 country pairs which formally withdrew from PTAs had another PTA in place. For these pairs, the withdrawal is related to restructuring, for example, joining the EC and thus withdrawing former agreements, while joining those that the EC has.
7. Create a symmetric matrix.

The resulting dataset contains a total of 9,168 symmetric dyads in 398 unique PTAs (410 PTAs counting accessions). I also collect the metadata for the agreements available in DESTA: the type of agreement (FTA or CU), regional composition, the year of signature, entry into force, the implementation period, the composition (bilateral, plurilateral, region-region), notification to the WTO, the presence of national treatment and third-party MFN provisions. Table 1 in Appendix D presents the descriptive statistics for the final PTA dataset, after it is merged with trade flows and other variables.

Other Variables

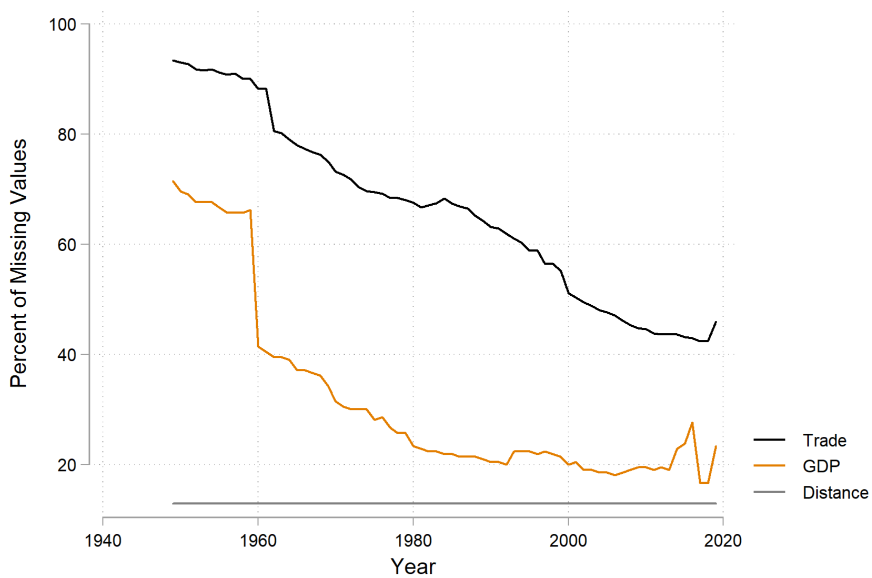
Geographical and cultural characteristics come from CEPII Gravity Dataset. In particular, I use bilateral distances, information on common language, colonial past, legal system, and information on GATT and EU membership. I construct a measure of remoteness as the sum of bilateral distances from a given country to every other country in the sample.

[33] If two overlapping agreements were in the same year, leave the 'strongest' in terms of agreement characteristics (has a national treatment clause, is a Customs Union, is a bilateral agreement, has the metadata available); if two overlapping agreements were in different years, leave the earliest agreement

To get a country-pair remoteness, I average the remoteness of two countries. I complement these variable with the information from NASA's Earth Observing System Data and Information System (EOSDIS), where I take information on insularity (small island developing economy), and the indicator for being landlocked.

▲Appendix B. Imputation

As shown in Appendix A, even after combining all available data sources containing trade flows, many missing values remain: out of 2,633,400 observations 1,613,684 (or 61%) are missing. Figure 1 shows the percentage of missing observations for selected variables: trade volume, GDP and distance. Almost 90% of trade data and 70% of GDP data is missing for the period before 1960. Therefore, in everything that follows, I will focus only on the period after 1960.



▲ Figure 1: Percentage of missing observations in the final dataset, 1950-2019.

One way to treat missing observations is to declare them as zeros, assuming that countries do not trade in a given year. The main problem is that it is virtually impossible to distinguish true zero trade flows and non-reported trade volumes. Appendix A demonstrated that adding up various data sources may substantially reduce the number of missing observations, suggesting that some of those flows are not zeros after all. Additionally, there are 35,411 missing trade flow observations for active PTAs (21.09% of all country-pairs with active PTAs). It is unlikely that countries would spend resources to negotiate an agreement if they do not trade. Moreover, there are some data patterns that suggest that some flows might indeed be missing, namely:

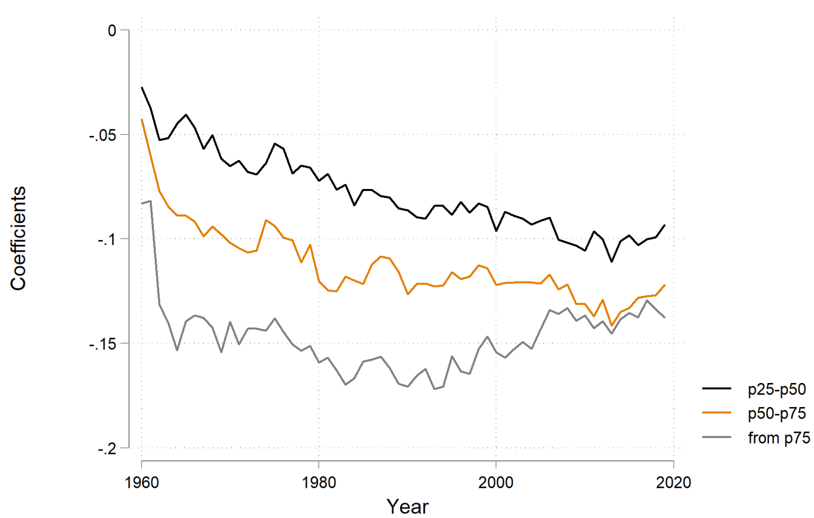
- 45,742 observations not missing at t and $t+2$, but missing at $t+1$;
- 21,259 observations not missing at t and $t+3$, but missing at $t+1$ and $t+2$;

- 11,621 observations not missing at t and $t+4$, but missing at $t+1$, $t+2$, and $t+3$;
- 4,664 missing observations for neighbouring countries.

In order to predict the values of missing trade flows, I use the fact that the empirical gravity relationship – even though not suitable for causal interpretation – has very high predictive power. I use a flexible form of log-linearized gravity equation, where I interact bilateral distance with the year indicators, to take account of the change in trade costs over the past 60 years. Using all available data, I estimate the 266 parameters of the following equation:

$$\log(X_{ijt}) = \beta_0 + \beta_1 \log(GDP_{it}) + \beta_2 \log(GDP_{jt}) + \sum_{q=2}^{4} \gamma_{qt} Dist_{ij} \times \delta_t + \beta_3 Colony_{ij} +$$
$$+ \beta_4 Comcol_{ij} + \beta_5 Language_{ij} + \beta_6 Contiguity_{ij} + \beta_7 Legal_{ij} + \beta_8 GATT_{it} + \beta_9 GATT_{jt} +$$
$$+ \beta_{10} EU_{it} + \beta_{11} EU_{jt} + \beta_{12} PTA_{ijt} + \beta_{13} NumPTA_{it} + \beta_{14} NumPTA_{jt} + \beta_{15} Landlock_{ij} +$$
$$+ \beta_{16} SIDS_{ij} + \beta_{17} SameReg_{ij} + \beta_{18} \log(Pop_{it}) + \beta_{19} \log(Pop_{jt}) + \varepsilon_{ijt} \quad (12)$$

Since the regression is estimated without domestic trade flows (recall that domestic trade data is only available after 1980), the distance puzzle persists in the estimation (Yotov (2012)). The problem is less pronounced, however, since I am using the flexible specification with distance quartiles: the interaction coefficients for the 75th percentile in Figure 2 almost do not change, while the ones for the 25th percentile fall only from -0.05 to -0.1, relative to the baseline.



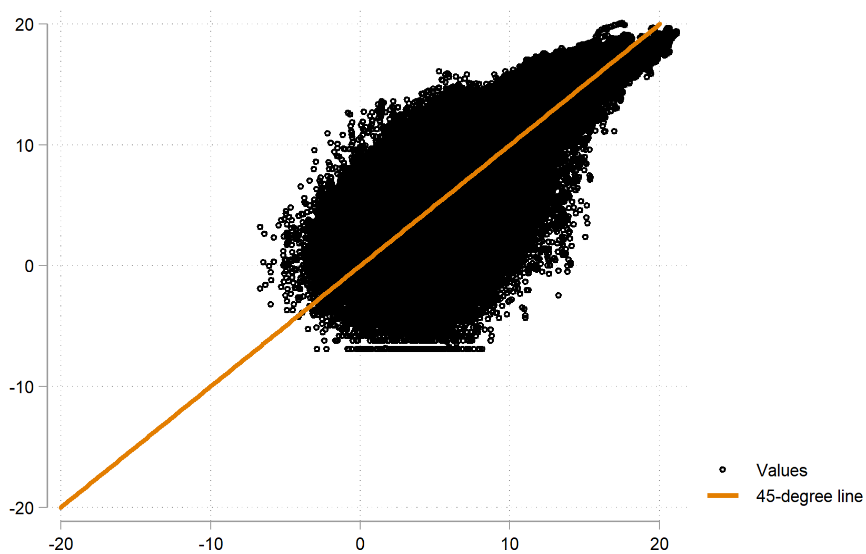
▲ Figure 2: Distance-Year Interaction Coefficients for Various Distance Percentiles.

After estimating the parameters, I use them to predict the missing trade flows, for country pairs for which I have all the necessary data available. This procedure leads to imputing additional 428,267 missing observations (see Table 1).

▲ Table 1: The number of missing observations before and after imputation.

	Missing	Total	Percent Missing
Trade	1,613,663	2,633,400	61.28
Predicted Trade	1,185,396	2,633,400	45.01

The parameters of the model fit are as follows. The adjusted R-squared is 0.62. The 10-fold cross-validation root mean squared error is 2.5 (compared to the mean of 6.64 in the full sample). Figure 3 plots the actual values of trade against the predicted ones, showing that a large number of observations lie along the 45-degree line.



▲ Figure 3: Actual vs. predicted values of (log) trade.

Importantly, the imputed volumes of trade are never directly used for the blocking procedure or estimation. Instead, I use the values to construct the normalized market shares, which depend not only on trade volumes between two countries, but on the whole matrix of bilateral trade. In this sense, imputation helps me to recover the distribution of normalized market shares. Appendix F implements the whole procedure without imputation, and demonstrates that the conceptual results are unchanged, while the standard errors are larger due to the reduced power.

▲Appendix C. Domestic Trade

To calculate the normalized market shares in the way consistent with the theoretical framework, I need to take into account the domestic trade. As Santamaría et al. (2020) show, the (log) normalized market shares are (log) deviations between the data and the predictions of the naïve gravity model:

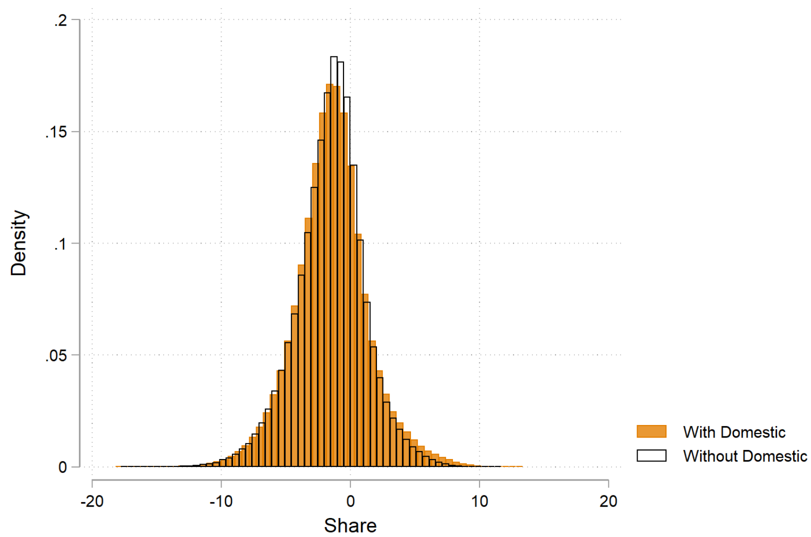
$$\ln s_{ij} = \ln\left(\frac{V_{ij}}{E}\right) - \ln\left(\frac{Y_i}{E}\frac{E_j}{E}\right)$$

where V_{ij} are the sales from origin i to destination j; $E_j = \sum_i V_{ij}$ is the total expenditure of j on all goods, including those coming from j itself; $Y_i = \sum_j V_{ij}$ is the total income of i, including from selling goods to i itself; and $E = \sum_j E_j$ is the total expenditure on all goods, including those sold within the country.

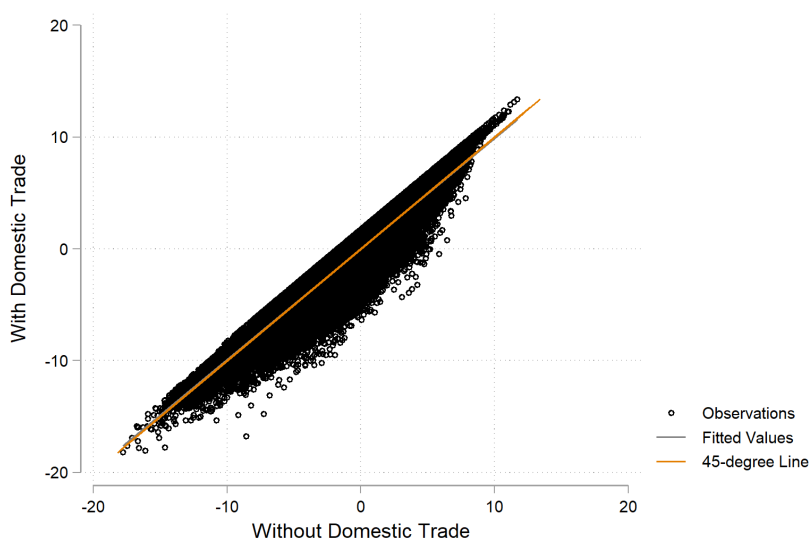
However, the data on production or domestic trade (which is calculated as production minus exports across all destinations) exists only for a very limited number of countries before 1980. To overcome this issue, I collect all available data on domestic trade after 1980 (see Appendix A), construct normalized market shares with and without domestic trade, and compare the two.

Figure 1 plots the distributions of the normalized market shares with and without the domestic trade. Clearly, the differences in the two measures are very small. Similarly, Figure 2 shows that the two variables plotted against each other are concentrated along the 45-degree line.

Finally, I run two regressions (with and without covariates) of normalized market shares calculated with domestic trade, s_{ijt}, on normalized market shares calculated without domestic trade, $\tilde{s}_{ijt}$. The results are presented in Table 1. The coefficient of the univariate regression is 0.99 with the intercept of -0.006, indicating that there is high level of correlation between the two measures. The coefficient of the regression with covariates is slightly smaller, 0.97, but leads to the same conclusion: the normalized market shares calculated with and without domestic trade are highly correlated. I thus proceed to calculate normalized market shares using only international trade data for all years before 1980.



▲ Figure 1: The distributions of normalized market shares calculated with and without domestic trade after 1980.



▲ Figure 2: Normalized market shares without domestic trade against the normalized market shares with domestic trade after 1980.

▲ Table 1: The coefficients of regressions of normalized market shares with domestic trade on normalized market shares without domestic trade.

	Univariate	Multivariate
$\tilde{s}_{ij}$	0.99***	0.97***
PTA		-0.01*
ln(GDP origin)		-0.02***
ln(GDP destination)		-0.08***
ln(Pop origin)		-0.11***
ln(Pop destination)		-0.6***
ln(Dist)		-0.06***
ln(Area origin)		-0.04***
ln(Area destnation)		-0.02***
Landlock origin		0.25***
Landlock destination		0.16***
Same country		0.08***
Colony		0.04***
Common language		-0.01**
Contiguity		0.05***
Intercept	-0.006***	3.93***
Number of obs.	636,957	549,031
Adj. R-squared	0.95	0.82

Note: Levels of statistical significance correspond to: * p<0.05, ** p<0.01, *** p<0.001.

▲Appendix D. Various Tables and Figures

▲ Table 1: Characteristics of PTAs in the final dataset

Indicator		Number of observations	Percentage
Type	FTA	4,065	57.08
	CU	3,057	342.92
Participation	Base Treaty	6,291	88.58
	Accession	811	11.42
Notification	Notified	3,427	48.42
	Not Notified	3,651	51.58
National Treatment	Yes	4,820	67.75
	No	2,294	32.25
Composition	Bilateral	262	3.68
	Plurilateral	3,220	45.21
	Plurilateral and 3rd country	1,192	16.74
	Region-Region	1,637	22.99
	Accession to a PTA	566	7.95
	Inheritance accession	245	3.44
Region	Africa	2,740	38.47
	Americas	382	5.36
	Asia	250	3.51
	Europe	778	10.92
	Oceania	114	1.60
	Intercontinental	2,858	40.13

▲ Table 2: The standardized differences and t-test for covariate distributions before and after trimming.

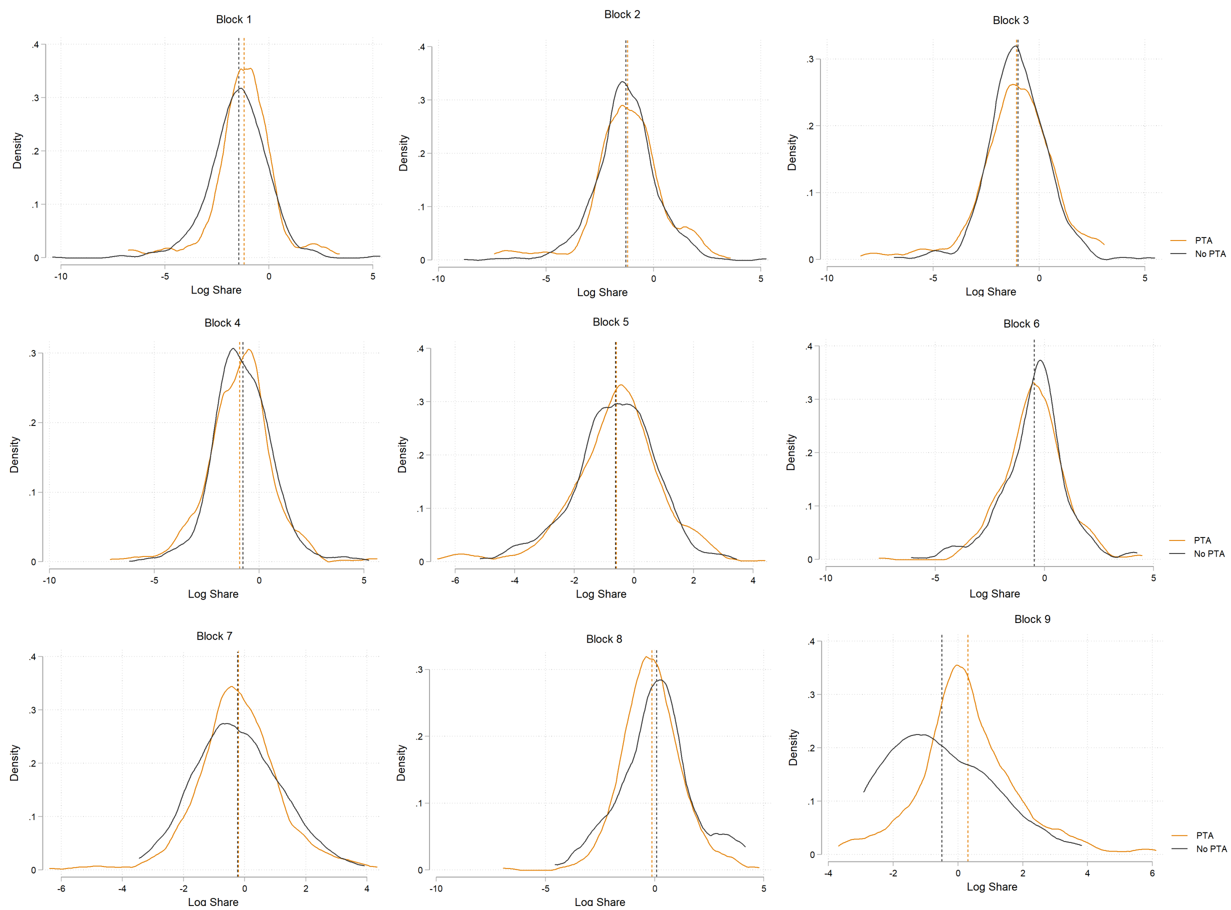
	Before Trimming					After Trimming				
	Mean PTA=0	**Mean PTA=1**	**Diff.**	**(Std.Err.)**	**Std. Diff.**	**Mean PTA=0**	**Mean PTA=1**	**Diff.**	**(Std.Err.)**	**Std. Diff.**
Pre-treatment Share	-1.55	-0.40	-1.15***	(0.03)	-0.72	-0.99	-0.53	-0.46***	(0.04)	-0.31
Distance	9.04	7.91	1.13***	(0.01)	1.62	8.42	7.98	0.43***	(0.013)	0.83
Remoteness	9.08	8.96	0.12***	(0.002)	1.05	8.97	8.94	0.03***	(0.002)	0.27
Small Island	0.43	0.19	0.24***	(0.008)	0.53	0.12	0.10	0.02***	(0.008)	0.07
Common Language	0.19	0.3	-0.11***	(0.006)	-0.26	0.21	0.29	-0.08***	(0.01)	-0.19
EU Membership	0.04	0.13	-0.09***	(0.003)	-0.35	0.12	0.16	-0.04***	(0.008)	-0.11
Landlocked	0.25	0.39	-0.14***	(0.006)	-0.29	0.37	0.39	-0.02	(0.012)	-0.04
Common Colonizer	0.14	0.18	-0.04***	(0.006)	-0.11	0.11	0.16	-0.05***	(0.008)	-0.14
Colonial Relationship	0.007	0.014	-0.007***	(0.001)	-0.07	0.02	0.02	0.002	(.003)	0.01
GATT Membership	0.48	0.667	-0.19***	(0.008)	-0.39	0.78	0.77	.0012	(0.01)	0.03
Legal System	0.28	0.47	-0.19***	(0.007)	-0.39	0.38	0.45	-0.07***	(0.01)	-0.15
Pre-treatment PTAs	0.50	1.05	-0.55***	(0.014)	-0.53	1.06	1.28	-0.22***	(0.03)	-0.19
N treated			3,200					2,612		
N control			13,392					4,673		
N Total			16,592					7,285		

Note: Standard errors in parenthesis. Levels of statistical significance correspond to: * p<0.05, ** p<0.01, *** p<0.001. The difference is calculated as the mean(no PTA) - mean(PTA). The standardised differences are calculated using the method of Yang and Dalton (2012). An absolute standardized difference of 0.10 or more indicates that covariates are imbalanced between groups (Austin (2009)).

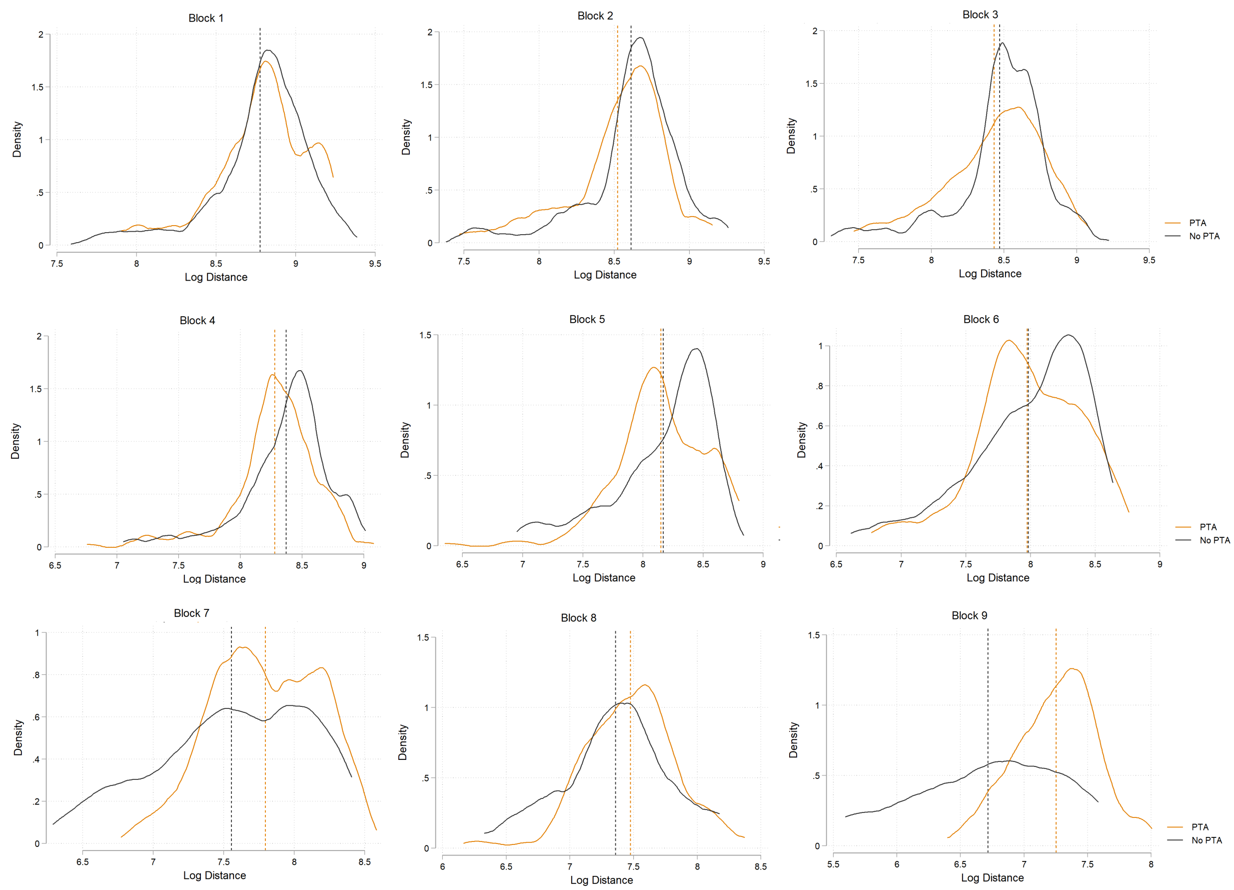
▲ Table 3: Balancing t-test of covariates by block

	B1	B2	B3	B4	B5	B6	B7	B8	B9
Pre-treatment Share	-0.25*	-0.08	0.07	0.15*	-0.03	-0.01	-0.03	0.22	-0.80**
	(0.14)	(0.11)	(0.12)	(0.08)	(0.09)	(0.11)	(0.13)	(0.17)	(0.33)
Distance	-0.0003	0.09***	0.04	0.09***	0.02	0.009	-0.23***	-0.11**	-0.53***
	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.04)	(0.04)	(0.08)
Remoteness	0.001	-0.012*	-0.02*	-0.005	0.0009	0.0009	0.03**	0.02*	0.05***
	(0.007)	(0.006)	(0.007)	(0.005)	(0.006)	(0.006)	(0.009)	(0.009)	(0.01)
Small Island	-0.03	-0.03	0.01	-0.03*	0.005	-0.0008	0.11***	-0.03	0.21***
	(0.03)	(0.03)	(0.02)	(0.18)	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)
Common Language	-0.17***	0.03	0.06	0.11***	0.13***	0.16***	0.17***	-0.17**	-0.21**
	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)	(0.04)	(0.04)	(0.06)	(0.11)
EU Membership	-0.02	0.01	-0.07**	0.03*	0.05*	-0.02	-0.04	-0.01	-0.06
	(0.03)	(0.02)	(0.02)	(0.01)	(0.03)	(0.03)	(0.03)	(0.04)	(0.09)
Landlocked	0.09*	0.009	-0.009	-0.13***	0.06*	-0.04	-0.19***	-0.17***	-0.37***
	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.05)	(0.06)	(0.10)
Common Colonizer	0.02	0.04	0.03	0.02	0.01	-0.01	-0.12***	-0.03	-0.3***
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)	(0.04)	(0.09)
Colonial Relationship	0.02	0.004	-0.004	0.01	-0.03***	0.001	0.002	0.02	0.07***
	(0.01)	(0.009)	(0.01)	(0.007)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
GATT Membership	0.07*	0.02	0.06*	0.07**	0.02	-0.04	-0.14***	-0.27***	-0.22**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.04)	(0.05)	(0.08)
Legal System	-0.03	0.07*	0.07*	-0.005	-0.04	0.01	-0.15***	0.06	-0.05
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.05)	(0.06)	(0.11)
Pre-treatment PTAs	0.08**	0.1***	0.07**	0.03	0.02	-0.09***	-0.13***	-0.16***	-0.35***
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.05)	(0.06)	(0.1)

Note: Standard errors in parenthesis. Levels of statistical significance correspond to: * p<0.05, ** p<0.01, *** p<0.001. The difference is calculated as the mean(no PTA) - mean(PTA).



▲ Figure 1: Distribution of the pre-PTA normalized market shares by treatment group and by block.



▲ Figure 2: Distribution of log distance by treatment group and by block.

▲ Table 4: The probability of having a customs union by block.

	B1	B2	B3	B4	B5	B6	B7	B8	B9
Pre-treatment Share	0.0557	-0.0160	-0.0126	-0.0527	-0.0913	-0.0800	0.0603	-0.167	0.0831
	(0.58)	(-0.23)	(-0.17)	(-1.05)	(-1.39)	(-1.05)	(0.65)	(-1.87)	(0.67)
Distance	-1.626	-1.499	-3.258**	-2.308***	-0.941	-0.0774	-0.668	-0.480	-0.887
	(-1.19)	(-1.46)	(-2.67)	(-4.31)	(-0.81)	(-0.06)	(-0.50)	(-0.49)	(-0.49)
Remoteness	-12.28**	-7.008**	-10.40**	-6.506***	-0.966	-0.229	-3.188	4.668	-2.059
	(-3.23)	(-2.58)	(-3.19)	(-4.07)	(-0.31)	(-0.07)	(-0.89)	(1.62)	(-0.40)
Small Island	-0.239	0.0307	-1.760**	-1.059**	-0.743	0.463	-0.700	-0.404	-2.524*
	(-0.34)	(0.06)	(-2.66)	(-2.97)	(-1.20)	(0.68)	(-0.93)	(-0.67)	(-2.04)
Common Language	1.066*	0.471	0.341	0.200	-0.0120	-0.434	-0.190	0.669	1.412
	(2.04)	(1.09)	(0.70)	(0.83)	(-0.03)	(-1.04)	(-0.36)	(1.60)	(1.88)
EU Membership					-1.887**	-3.147***	-1.533	-2.614***	-0.793
					(-2.92)	(-4.18)	(-1.93)	(-3.57)	(-0.84)
Landlocked	0.589	0.758*	1.455***	0.667**	1.371***	0.798*	1.426***	1.277***	1.371*
	(1.32)	(2.31)	(3.86)	(3.26)	(3.92)	(2.10)	(3.31)	(3.40)	(2.30)
Common Colonizer	0.228	0.104	1.320*	0.762*	0.566	1.131*	1.367**	0.472	2.335**
	(0.35)	(0.21)	(2.37)	(2.48)	(1.27)	(2.26)	(2.64)	(0.94)	(2.85)
Colonial Relationship		0.349					0.160		
		(0.38)					(0.14)		
GATT Membership	-0.173	0.132	0.382	0.268	0.570**	0.974***	1.100***	1.775***	2.541***
	(-0.57)	(0.58)	(1.40)	(1.52)	(2.61)	(3.68)	(3.79)	(5.44)	(5.07)
Legal System	0.0274	-0.299	-0.0394	0.165	0.0619	-0.242	0.817**	1.196***	-0.440
	(0.09)	(-1.29)	(-0.16)	(1.04)	(0.32)	(-1.15)	(3.02)	(3.94)	(-1.11)
Pre-treatment PTAs	-2.839***	-1.199***	-1.155**	-0.493*	-0.648**	0.371	-0.948**	-0.207	-0.981*
	(-4.16)	(-3.41)	(-3.26)	(-2.47)	(-2.83)	(1.48)	(-3.05)	(-0.63)	(-1.98)
Constant	122.2**	73.61*	118.3**	75.65***	14.42	0.656	31.69	-40.93	22.14
	(2.77)	(2.29)	(3.07)	(4.20)	(0.40)	(0.02)	(0.77)	(-1.29)	(0.39)
N Obs	1123	1214	837	1260	929	692	505	441	271
Pseudo R-squared	0.120	0.051	0.075	0.038	0.144	0.169	0.231	0.311	0.401

Note: t-statistics in parenthesis. Levels of statistical significance correspond to: * p<0.05, ** p<0.01, *** p<0.001.

▲ Table 5: The probability of having a national treatment provision by block.

	B1	B2	B3	B4	B5	B6	B7	B8	B9
Pre-treatment Share	0.0266	-0.00181	-0.0678	-0.0441	-0.0308	-0.0614	0.0881	-0.151	0.0186
	(0.33)	(-0.03)	(-0.98)	(-0.94)	(-0.54)	(-0.87)	(1.04)	(-1.79)	(0.17)
Distance	2.018*	-0.585	0.439	-1.306**	-0.956	0.751	-3.226**	0.882	3.159*
	(2.32)	(-0.65)	(0.51)	(-2.88)	(-0.97)	(0.67)	(-2.64)	(0.99)	(2.03)
Remoteness	2.188	-2.405	0.451	-3.593**	-2.726	2.146	-9.226**	5.192	5.967
	(0.87)	(-1.01)	(0.19)	(-2.60)	(-1.04)	(0.71)	(-2.87)	(1.89)	(1.33)
Small Island	1.089*	0.353	-0.0673	-0.395	-0.938	0.158	-2.524***	0.139	0.579
	(2.16)	(0.72)	(-0.13)	(-1.25)	(-1.73)	(0.25)	(-3.58)	(0.25)	(0.56)
Common Language	0.147	0.0802	-0.490	0.00621	0.0916	-0.644	0.882	-0.573	-1.597*
	(0.40)	(0.20)	(-1.25)	(0.03)	(0.27)	(-1.66)	(1.84)	(-1.49)	(-2.53)
EU Membership	1.428	1.428	1.428	1.428	-1.580**	-2.871***	0.847	-0.220	-2.318**
	(1.53)	(1.53)	(1.53)	(1.53)	(-3.20)	(-4.46)	(1.25)	(-0.41)	(-2.80)
Landlocked	-0.768*	0.370	0.233	0.159	0.893**	-0.0261	1.303***	0.592	-0.194
	(-2.34)	(1.24)	(0.82)	(0.86)	(2.97)	(-0.07)	(3.36)	(1.74)	(-0.39)
Common Colonizer	-1.475**	-0.368	-0.245	0.139	0.359	0.623	2.198***	1.202*	0.613
	(-2.74)	(-0.80)	(-0.55)	(0.48)	(0.91)	(1.34)	(4.50)	(2.57)	(0.93)
Colonial Relationship		0.391					-2.326*		
		(0.44)					(-2.27)		
GATT Membership	-0.652*	0.0889	0.102	0.297	0.264	0.603**	0.884***	1.169***	0.693
	(-2.39)	(0.41)	(0.40)	(1.75)	(1.33)	(2.59)	(3.34)	(4.20)	(1.73)
Legal System	0.0246	-0.249	-0.128	0.0284	-0.0590	-0.641**	0.217	-0.985***	-0.437
	(0.10)	(-1.15)	(-0.57)	(0.19)	(-0.35)	(-3.25)	(0.89)	(-3.74)	(-1.31)
Pre-treatment PTAs	-1.815***	-1.193***	-0.988**	-0.353	-0.0327	0.294	0.00153	0.886**	0.387
	(-3.86)	(-3.53)	(-3.04)	(-1.93)	(-0.17)	(1.23)	(0.01)	(2.90)	(0.94)
Constant	-38.75	24.81	-9.251	41.72**	-25.75	0.656	105.5**	-53.70	-74.93
	(-1.35)	(0.88)	(-0.34)	(2.70)	(-0.73)	(0.02)	(2.84)	(-1.79)	(-1.53)
N Obs	1123	1214	837	1260	929	692	505	441	271
Pseudo R-squared	0.060	0.035	0.026	0.020	0.133	0.169	0.107	0.185	0.164

Note: t-statistics in parenthesis. Levels of statistical significance correspond to: * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table 6: The probability of having a third-party MFN provision by block.

	B1	B2	B3	B4	B5	B6	B7	B8	B9
Pre-treatment Share	0.0543	-0.116	-0.0864	0.0137	-0.0214	0.00621	-0.0282	-0.118	0.0188
	(0.54)	(-1.65)	(-0.96)	(0.25)	(-0.35)	(0.09)	(-0.30)	(-1.37)	(0.16)
Distance	-0.844	1.216	-2.095	-0.767	-0.551	-1.704	-1.026	2.035*	3.855*
	(-0.53)	(0.96)	(-1.09)	(-0.94)	(-0.51)	(-1.41)	(-0.77)	(2.19)	(2.21)
Remoteness	-11.51**	0.404	-6.685	-1.866	-1.597	-4.100	-4.676	7.930**	8.078
	(-2.67)	(0.12)	(-1.33)	(-0.86)	(-0.55)	(-1.27)	(-1.31)	(2.79)	(1.65)
Small Island	0.379	1.131	-2.153*	-0.772	-0.931	-1.724*	-2.461**	-0.879	0.884
	(0.47)	(1.74)	(-2.16)	(-1.70)	(-1.59)	(-2.45)	(-3.03)	(-1.38)	(0.78)
Common Language	0.848	-0.463	0.147	-0.225	-0.0487	0.424	0.0420	-0.262	-0.612
	(1.45)	(-0.94)	(0.21)	(-0.74)	(-0.13)	(1.07)	(0.08)	(-0.65)	(-0.89)
EU Membership	0.597	-0.847	1.799*	-0.514	-0.0392	1.327*	0.505	-1.543**	-3.451***
	(0.53)	(-1.06)	(2.03)	(-1.12)	(-0.08)	(2.18)	(0.68)	(-2.76)	(-3.64)
Landlocked	0.384	-0.127	1.126*	-0.0585	0.836**	1.066**	1.790***	0.749*	0.424
	(0.80)	(-0.33)	(2.12)	(-0.23)	(2.64)	(2.98)	(4.27)	(2.17)	(0.79)
Common Colonizer	-0.105	-0.848	1.066	0.114	0.269	1.461**	1.904***	-0.325	-0.905
	(-0.15)	(-1.51)	(1.46)	(0.32)	(0.64)	(3.01)	(3.56)	(-0.66)	(-1.24)
Colonial Relationship			0.398	-0.539	2.405***	-0.374	0.917		
			(0.42)	(-0.81)	(3.55)	(-0.49)	(0.89)		
GATT Membership	-0.215	-0.0438	0.363	0.0555	0.254	0.928***	0.845**	1.880***	0.819*
	(-0.69)	(-0.20)	(1.24)	(0.32)	(1.24)	(3.68)	(2.92)	(6.03)	(2.00)
Legal System	-0.237	-0.472*	-0.0309	0.0812	0.213	0.0430	0.346	0.315	0.639
	(-0.80)	(-2.13)	(-0.12)	(0.52)	(1.27)	(0.22)	(1.29)	(1.17)	(1.74)
Pre-treatment PTAs	-2.753***	-1.368***	-1.038**	0.0491	-0.431*	0.345	-0.524	-0.133	-0.0000712
	(-4.05)	(-4.06)	(-2.99)	(0.27)	(-2.13)	(1.51)	(-1.73)	(-0.43)	(-0.00)
Constant	108.6*	-15.63	75.29	22.06	17.58	47.89	48.09	-87.45**	-99.56
	(2.14)	(-0.40)	(1.25)	(0.86)	(0.51)	(1.27)	(1.18)	(-2.82)	(-1.85)
N Obs	1123	1214	837	1260	929	692	505	441	271
Pseudo R-squared	0.119	0.044	0.064	0.018	0.056	0.084	0.229	0.225	0.298

Note: t-statistics in parenthesis. Levels of statistical significance correspond to: * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

▲ Table 7: The probability of notification to the WTO by block.

	B1	B2	B3	B4	B5	B6	B7	B8	B9
Pre-treatment Share	0.194	0.417**	-0.126	0.245**	0.0927	0.0555	-0.222	0.0169	0.182
	(1.41)	(2.67)	(-0.79)	(2.58)	(1.06)	(0.62)	(-1.88)	(0.18)	(1.09)
Distance	3.003**	-0.640	-3.031	-4.937***	-1.293	-4.260**	0.802	-0.949	3.104*
	(2.84)	(-0.30)	(-1.16)	(-3.89)	(-0.86)	(-2.90)	(0.50)	(-0.94)	(1.96)
Remoteness	10.59**	6.950	-3.867	-16.97***	-4.212	-15.43***	-0.239	-8.999**	-38.66***
	(3.07)	(1.23)	(-0.54)	(-4.38)	(-1.07)	(-3.84)	(-0.06)	(-2.60)	(-4.20)
Small Island		-0.963	-1.363	-0.547	-0.831	-1.732*	0.127	-0.862	
		(-0.81)	(-1.11)	(-0.93)	(-0.99)	(-2.08)	(0.13)	(-1.32)	
Common Language	-0.500	-0.816	3.006**	2.453***	0.205	1.445**	-0.142	1.175**	-0.930
	(-0.80)	(-0.76)	(2.88)	(4.69)	(0.38)	(2.93)	(-0.22)	(2.71)	(-1.28)
EU Membership	0.320	2.859*	10.84***	3.330***	0.741	3.133***	1.199	2.214***	0.291
	(0.44)	(2.54)	(4.93)	(4.75)	(1.05)	(4.24)	(1.38)	(3.66)	(0.29)
Landlocked	-2.604**	-1.420	-0.485	-2.521**	-1.423**	0.00120	-1.462**	-0.381	-2.134**
	(-3.26)	(-1.68)	(-0.58)	(-3.17)	(-3.01)	(0.00)	(-2.83)	(-1.02)	(-3.06)
Common Colonizer					-0.994	-0.237	-1.589*	-1.778**	-2.952**
					(-1.48)	(-0.34)	(-2.15)	(-2.69)	(-2.99)
Colonial Relationship		0.338	2.592	-2.298*	1.195	-1.267	-0.0745	-0.239	
		(0.27)	(1.92)	(-2.44)	(1.47)	(-1.45)	(-0.06)	(-0.20)	
GATT Membership	-0.860	-0.473	-1.757**	-2.011***	-0.333	-0.258	-0.332	0.403	-2.275***
	(-1.36)	(-0.84)	(-2.95)	(-5.50)	(-1.07)	(-0.86)	(-0.90)	(1.34)	(-3.83)
Legal System	0.0124	0.415	0.0559	-0.167	0.527*	0.278	-0.666	-0.646*	-0.690
	(0.03)	(0.90)	(0.10)	(-0.56)	(2.36)	(1.12)	(-1.92)	(-2.26)	(-1.32)
Pre-treatment PTAs	1.066	0.306	-8.059***	1.703***	0.862***	1.108***	2.006***	0.602	1.686*
	(1.86)	(0.40)	(-3.70)	(4.45)	(3.34)	(3.93)	(5.50)	(1.90)	(2.32)
Constant	-124.6***	-60.38	56.40	190.2***	46.43	169.1***	-5.034	86.01*	324.3***
	(-3.30)	(-0.89)	(0.66)	(4.30)	(1.00)	(3.65)	(-0.10)	(2.35)	(3.87)
N Obs	1123	1214	837	1260	929	692	505	441	271
Pseudo R-squared	0.221	0.258	0.382	0.289	0.176	0.223	0.328	0.261	0.619

Note: t-statistics in parenthesis. Levels of statistical significance correspond to: * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

▲ Table 8: The probability of having a late agreement (after 1993) by block.

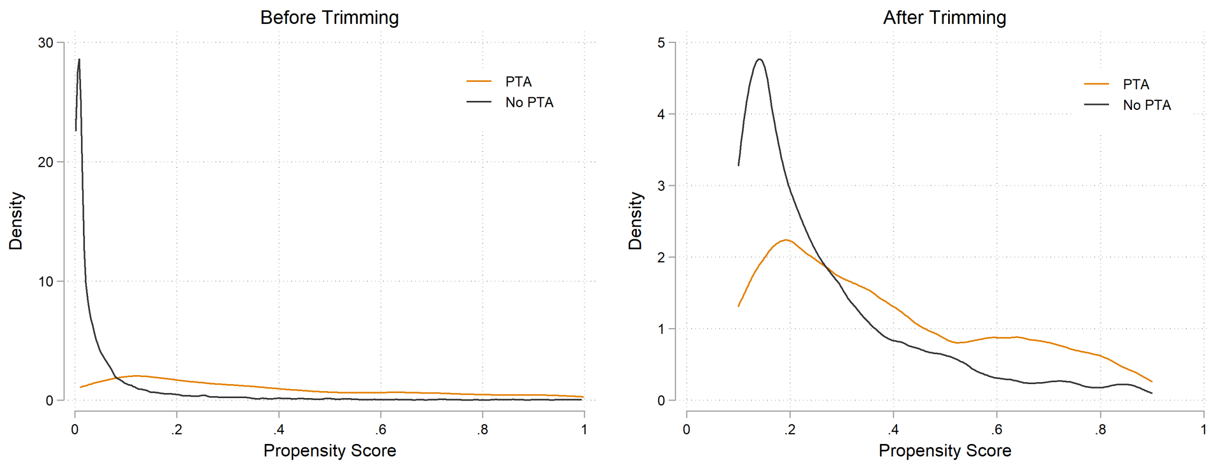
	B1	B2	B3	B4	B5	B6	B7	B8	B9
Pre-treatment Share	0.103	-0.0874	-0.0505	-0.0410	0.0929	0.0454	0.0861	0.0173	0.201
	(1.26)	(-1.26)	(-0.74)	(-0.82)	(1.58)	(0.70)	(0.99)	(0.22)	(1.75)
Distance	1.207	2.385	-3.221**	-2.995***	-6.487***	-5.091***	-1.052	2.621**	2.298
	(0.95)	(1.94)	(-2.93)	(-5.32)	(-8.46)	(-6.98)	(-0.84)	(3.02)	(1.36)
Remoteness	-0.202	6.454*	-5.869*	-7.179***	-16.82***	-10.44***	-6.419	2.766	-1.317
	(-0.06)	(2.04)	(-2.05)	(-4.47)	(-7.84)	(-4.93)	(-1.94)	(1.06)	(-0.27)
Small Island	0.799	1.509*	-1.875**	-1.184***	-3.373***	-2.860***	-0.885	2.616***	1.517
	(1.21)	(2.38)	(-3.10)	(-3.39)	(-7.59)	(-6.08)	(-1.25)	(4.18)	(1.35)
Common Language	0.203	-0.891	0.820	0.772**	2.070***	1.447***	0.845	-1.196**	-0.243
	(0.43)	(-1.86)	(1.96)	(3.26)	(7.13)	(5.09)	(1.72)	(-3.26)	(-0.38)
EU Membership	0.459	-1.126					-1.185	-3.582***	-1.456
	(0.64)	(-1.55)					(-1.65)	(-6.14)	(-1.67)
Landlocked	-0.645	-0.493	1.045**	0.418*	2.068***	1.303***	1.241**	-0.316	0.0544
	(-1.63)	(-1.33)	(3.03)	(2.09)	(8.18)	(4.98)	(3.10)	(-0.97)	(0.10)
Common Colonizer	-1.116	-1.309*	0.836	0.468	2.102***	2.670***	1.328**	-0.145	1.093
	(-1.77)	(-2.39)	(1.64)	(1.56)	(6.47)	(7.81)	(2.68)	(-0.33)	(1.57)
Colonial Relationship				-1.950*	-2.043*		-1.760		1.134
				(-2.43)	(-2.42)		(-1.69)		(0.75)
GATT Membership	-0.652*	-0.0830	0.0832	-0.0959	0.221	0.320	-0.429	-0.254	-0.516
	(-2.36)	(-0.38)	(0.35)	(-0.59)	(1.14)	(1.46)	(-1.58)	(-0.97)	(-1.21)
Legal System	-0.158	-0.594**	-0.0515	0.123	0.259	-0.227	0.622*	0.102	-0.0743
	(-0.65)	(-2.73)	(-0.23)	(0.82)	(1.49)	(-1.18)	(2.44)	(0.39)	(-0.22)
Pre-treatment PTAs	-1.108**	-1.186***	-1.045**	-0.305	0.0504	0.546*	0.221	0.512	-0.0646
	(-2.75)	(-3.72)	(-3.11)	(-1.61)	(0.25)	(2.36)	(0.77)	(1.76)	(-0.15)
Constant	-10.02	-79.63*	77.94*	87.93***	200.8***	131.6***	64.55	-43.38	-3.944
	(-0.25)	(-2.08)	(2.28)	(4.77)	(8.11)	(5.50)	(1.69)	(-1.51)	(-0.07)
N Obs	1123	1214	837	1260	929	692	505	441	271
Pseudo R-squared	0.041	0.035	0.067	0.054	0.142	0.133	0.167	0.156	0.202

Note: t-statistics in parenthesis. Levels of statistical significance correspond to: * p<0.05, ** p<0.01, *** p<0.001.

Table 9: The probability of having a plurilateral agreement by block.

	B1	B2	B3	B4	B5	B6	B7	B8	B9
Pre-treatment Share	0.0996	-0.0542	-0.0462	-0.0116	0.0280	-0.130	0.0350	-0.0464	-0.134
	(1.09)	(-0.69)	(-0.62)	(-0.22)	(0.44)	(-1.88)	(0.41)	(-0.54)	(-0.95)
Distance	2.140	2.486	-1.993	-3.085***	-2.553*	-0.425	-2.790*	-1.836	5.526*
	(1.47)	(1.78)	(-1.62)	(-5.15)	(-2.24)	(-0.39)	(-2.24)	(-1.94)	(2.15)
Remoteness	-0.402	4.272	-4.803	-9.390***	-5.651	-2.239	-5.730	-3.000	1.002
	(-0.10)	(1.19)	(-1.50)	(-5.45)	(-1.87)	(-0.76)	(-1.77)	(-1.05)	(0.14)
Small Island	1.067	1.511*	-1.794**	-1.609***	-2.291***	-0.792	-3.074***	-2.848***	-1.755
	(1.41)	(2.11)	(-2.58)	(-4.24)	(-3.72)	(-1.25)	(-4.21)	(-4.44)	(-1.13)
Common Language	-0.492	-1.049	0.596	0.828***	0.705	-0.0186	0.845	1.655***	0.740
	(-0.89)	(-1.91)	(1.27)	(3.33)	(1.78)	(-0.05)	(1.73)	(4.03)	(0.70)
EU Membership	0.226	-0.758			-2.079**	-2.869***	-0.0530	0.580	-3.041*
	(0.29)	(-0.96)			(-3.17)	(-4.33)	(-0.08)	(1.06)	(-2.31)
Landlocked	-0.771	-0.431	0.986**	0.531*	0.857*	0.0216	0.687	1.009**	0.595
	(-1.77)	(-1.04)	(2.61)	(2.54)	(2.57)	(0.06)	(1.76)	(2.89)	(0.71)
Common Colonizer	-0.818	-0.999	0.652	0.698*	0.849*	0.948*	1.785***	1.078*	1.691
	(-1.21)	(-1.69)	(1.20)	(2.25)	(1.97)	(2.06)	(3.57)	(2.19)	(1.29)
Colonial Relationship		1.638		-1.772*	-0.550		-2.466*	-1.393	4.914
		(1.70)		(-2.19)	(-0.58)		(-2.39)	(-1.17)	(1.24)
GATT Membership	-0.969**	-0.477*	-0.0825	-0.173	0.297	0.275	0.999***	1.492***	1.032
	(-3.23)	(-2.09)	(-0.33)	(-1.04)	(1.48)	(1.23)	(3.76)	(5.15)	(1.70)
Legal System	-0.135	-0.417	0.0488	0.252	0.0499	-0.485*	0.381	-0.0199	-0.787
	(-0.51)	(-1.78)	(0.20)	(1.63)	(0.28)	(-2.51)	(1.54)	(-0.07)	(-1.70)
Pre-treatment PTAs	-0.877*	-1.336***	-1.781***	-0.367	0.156	0.435	0.508	1.023**	0.932
	(-2.09)	(-3.62)	(-4.30)	(-1.87)	(0.78)	(1.83)	(1.79)	(3.19)	(1.46)
Constant	-16.31	-60.86	58.09	108.3***	70.01*	22.77	71.29	38.41	-47.18
	(-0.35)	(-1.40)	(1.52)	(5.49)	(1.97)	(0.66)	(1.90)	(1.22)	(-0.61)
N Obs	1123	1214	837	1260	929	692	505	441	271
Pseudo R-squared	0.046	0.034	0.085	0.055	0.132	0.145	0.124	0.181	0.403

Note: t-statistics in parenthesis. Levels of statistical significance correspond to: * $p<0.05$, ** $p<0.01$, *** $p<0.001$.



▲ Figure 3: The estimated probability of concluding a PTA in 1970-2005 for pairs that do not trade in 1960-1965.

The probability is estimated using a logit model, where all covariates are the same as in the baseline estimation, except the logarithm of the pre-treatment normalized market share is substituted by the value of the pre-treatment normalized market share (including zeros). The trimming cutoff is the same as in the baseline exercise.

▲ Table 10: The number of missing observations in anticipation imputed in the pre-treatment period, by block and treatment status .

		Total obs.	Unique obs.	Total zeros	Unique zeros	Unique imputed
Block 1	PTA=1	115	115	1	1	0
	PTA=0	13,104	1008	334	82	32
Block 2	PTA=1	186	186	0	0	0
	PTA=0	13,364	1028	348	99	38
Block 3	PTA=1	180	180	1	1	0
	PTA=0	8,541	657	242	60	16
Block 4	PTA=1	387	387	3	3	0
	PTA=0	16,587	873	373	56	11
Block 5	PTA=1	405	405	2	2	0
	PTA=0	10,480	524	109	23	7
Block 6	PTA=1	380	380	0	0	0
	PTA=0	7,488	312	107	16	1
Block 7	PTA=1	352	352	0	0	0
	PTA=0	3,366	153	41	8	4
Block 8	PTA=1	360	360	2	2	0
	PTA=0	2,106	81	62	12	0
Block 9	PTA=1	247	247	3	3	0
	PTA=0	480	24	9	3	0

Note: in every block control units are re-sampled for every year of the treatment. Thus, the number of unique control units is smaller than the total number of units. The unique number of zeros counts country pairs which had a zero average normalized market share in any period for which they were re-sampled. The number of unique imputed values represents the number of country pairs which had a zero in anticipation, and had a missing value imputed in the pre-treatment period.

▲ Table 11: The number of missing observations in the short run imputed in the pre-treatment period, by block and treatment status .

		Total obs.	Unique obs.	Total zeros	Unique zeros	Unique imputed
Block 1	PTA=1	115	115	8	8	7
	PTA=0	13,104	1008	386	77	30
Block 2	PTA=1	186	186	5	5	3
	PTA=0	13,364	1028	440	86	32
Block 3	PTA=1	180	180	5	5	4
	PTA=0	8,541	657	352	56	14
Block 4	PTA=1	387	387	12	12	7
	PTA=0	16,587	873	475	51	9
Block 5	PTA=1	405	405	7	7	5
	PTA=0	10,480	524	153	23	7
Block 6	PTA=1	380	380	5	5	5
	PTA=0	7,488	312	133	14	1
Block 7	PTA=1	352	352	2	2	2
	PTA=0	3,366	153	37	8	4
Block 8	PTA=1	360	360	1	1	1
	PTA=0	2,106	81	87	12	0
Block 9	PTA=1	247	247	0	0	0
	PTA=0	480	24	17	3	0

Note: in every block control units are re-sampled for every year of the treatment. Thus, the number of unique control units is smaller than the total number of units. The unique number of zeros counts country pairs which had a zero average normalized market share in any period for which they were re-sampled. The number of unique imputed values represents the number of country pairs which had a zero in the short run, and had a missing value imputed in the pre-treatment period.

▲ Table 12: The number of missing observations in the medium run imputed in the pre-treatment period, by block and treatment status .

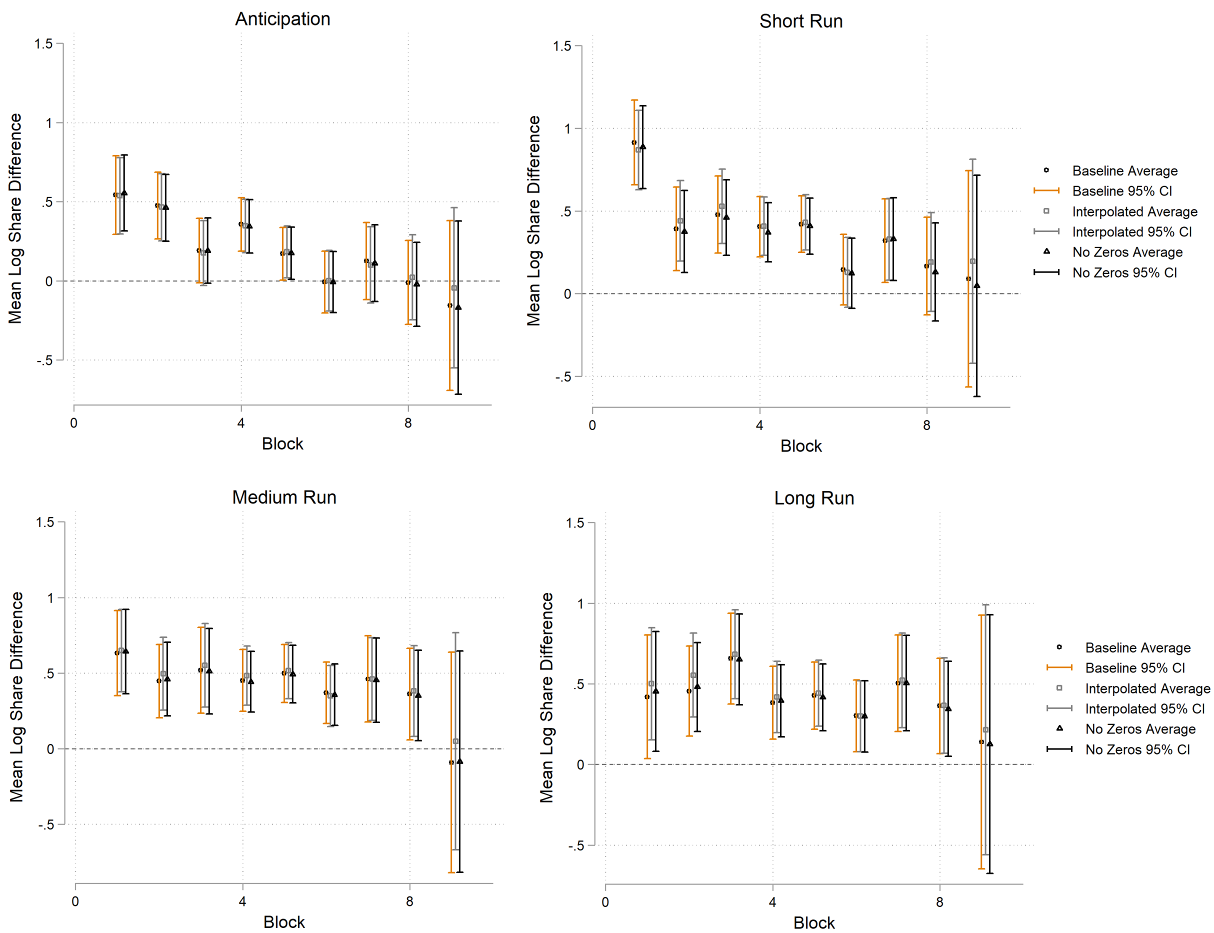
		Total obs.	Unique obs.	Total zeros	Unique zeros	Unique imputed
Block 1	PTA=1	115	115	6	6	6
	PTA=0	13,104	1008	376	66	27
Block 2	PTA=1	186	186	6	6	4
	PTA=0	13,364	1028	451	83	36
Block 3	PTA=1	180	180	6	6	6
	PTA=0	8,541	657	366	50	13
Block 4	PTA=1	387	387	10	10	6
	PTA=0	16,587	873	517	55	11
Block 5	PTA=1	405	405	6	6	5
	PTA=0	10,480	524	169	25	8
Block 6	PTA=1	380	380	6	6	6
	PTA=0	7,488	312	135	16	2
Block 7	PTA=1	352	352	4	4	4
	PTA=0	3,366	153	13	6	3
Block 8	PTA=1	360	360	1	1	1
	PTA=0	2,106	81	126	13	2
Block 9	PTA=1	247	247	2	2	0
	PTA=0	480	24	22	2	0

Note: in every block control units are re-sampled for every year of the treatment. Thus, the number of unique control units is smaller than the total number of units. The unique number of zeros counts country pairs which had a zero average normalized market share in any period for which they were re-sampled. The number of unique imputed values represents the number of country pairs which had a zero in the medium run, and had a missing value imputed in the pre-treatment period.

▲ Table 13: The number of missing observations in the long run imputed in the pre-treatment period, by block and treatment status .

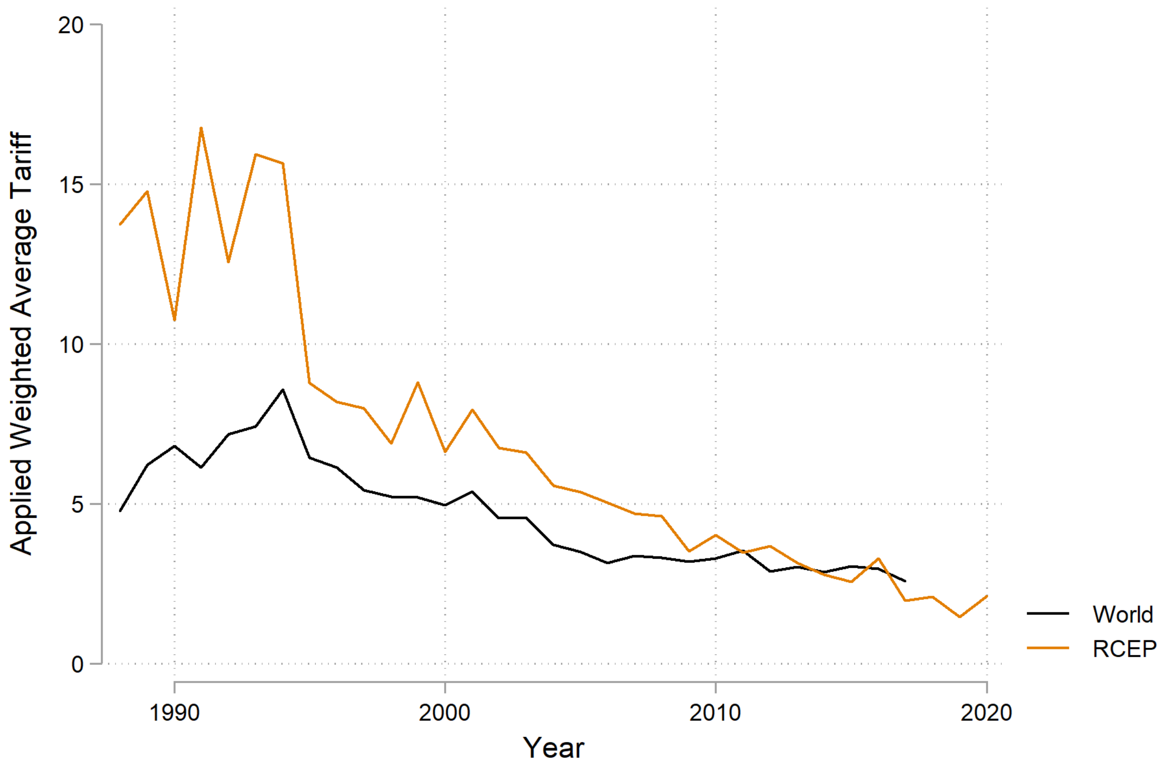
		Total obs.	Unique obs.	Total zeros	Unique zeros	Unique imputed
Block 1	PTA=1	115	115	4	4	4
	PTA=0	13,104	1008	427	73	28
Block 2	PTA=1	186	186	6	6	4
	PTA=0	13,364	1028	506	90	34
Block 3	PTA=1	180	180	7	7	7
	PTA=0	8,541	657	402	52	13
Block 4	PTA=1	387	387	7	7	6
	PTA=0	16,587	873	602	58	14
Block 5	PTA=1	405	405	6	6	6
	PTA=0	10,480	524	214	31	10
Block 6	PTA=1	380	380	4	4	4
	PTA=0	7,488	312	158	15	2
Block 7	PTA=1	352	352	2	2	2
	PTA=0	3,366	153	18	8	6
Block 8	PTA=1	360	360	1	1	1
	PTA=0	2,106	81	151	15	4
Block 9	PTA=1	247	247	0	0	0
	PTA=0	480	24	25	3	0

Note: in every block control units are re-sampled for every year of the treatment. Thus, the number of unique control units is smaller than the total number of units. The unique number of zeros counts country pairs which had a zero average normalized market share in any period for which they were re-sampled. The number of unique imputed values represents the number of country pairs which had a zero in the long run, and had a missing value imputed in the pre-treatment period.



▲ Figure 4: Comparison of the estimates obtained using (1) the baseline procedure; (2) Interpolated sample; and (3) Sample excluding zeros in average calculations.

Note: baseline procedure conditions the analysis on positive trade flows the the pre-treatment period in the imputed sample. The average normalized market share is calculated assuming zeros for missing values for those pairs that used to trade in the pre-treatment period, but have missing values in later periods. The interpolation estimate implements the same procedure as the baseline estimate, but does so for the imputed and interpolated sample. The estimate obtained without considering zeros is still using data conditional on positive trade flows in the pre-treatment period, but does not assume missing trade flows as zeros in later years. The average normalized market shares are calculated using only the available data, discarding the missing values.



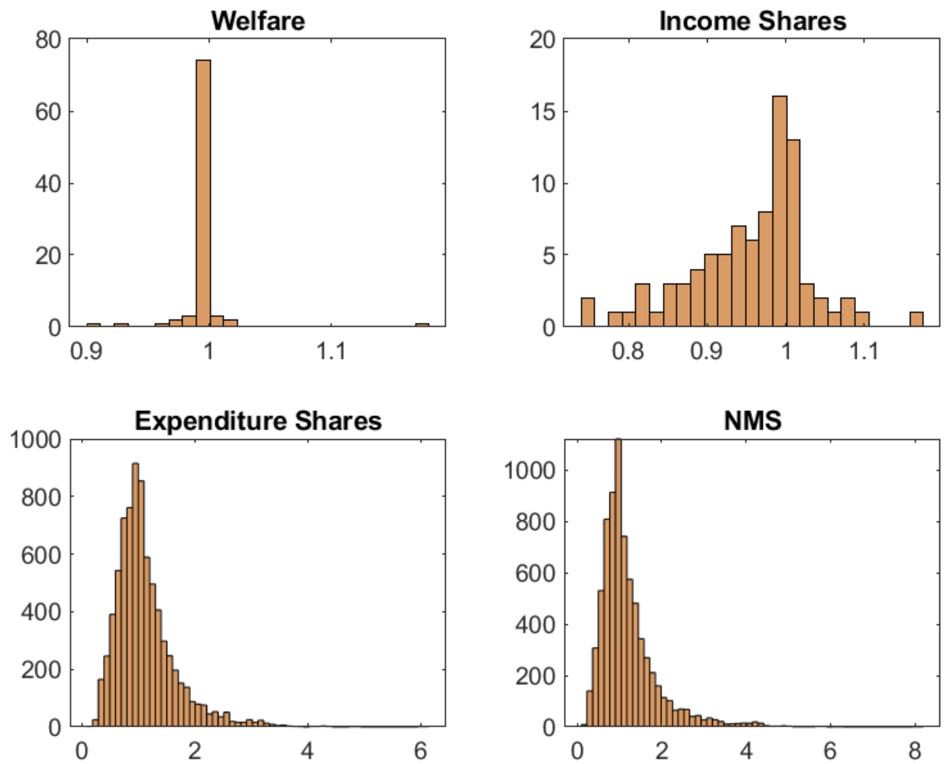
▲ Figure 5: Applied weighted average tariffs in the world and in RCEP countries, 1988-2020.

Source: World Bank

▲ Table 14: Applied weighted average tariffs by RCEP country in 1988 and 2020.

Country / Region	1988	2020
Australia	18.56*	0.71
Brunei Darussalam	4.43*	0.02
China	32.17*	2.47
Indonesia	14.54*	2.04
Japan	4.12	2.22
Cambodia	16.43*	6.21
Korea, Rep.	13.95	5.48
Lao PDR	14.06*	0.97
Myanmar	4.13*	1.81*
Malaysia	14.4	3.6
New Zealand	11.24*	0.85
Philippines	22.5	1.67
Singapore	3.26*	0.05
Thailand	33.65*	3.52*
Vietnam	15.19*	1.34
RCEP	14.84	2.20
World	4.79	2.59*

Note: values indicated with stars are not available for the corresponding year, and are presented for the nearest available year. In particular, Myanmar in 2020 is in Myanmar 2019; Thailand in 2020 is Thailand in 2015; World in 2020 is World in 2017; Australia in 1988 is Australia in 1991; Brunei in 1988 is Brunei 1992; China in 1988 is China in 1992; Indonesia in 1988 is Indonesia in 1989; Cambodia in 1988 is Cambodia in 2001; Laos in 1988 is Laos in 2000; Myanmar in 1988 is Myanmar in 1996; New Zealand in 1988 is New Zealand in 1992; Singapore in 1988 is Singapore in 1989; Thailand in 1988 is Thailand in 1989; Vietnam in 1988 is Vietnam in 1994.
Source: World Bank



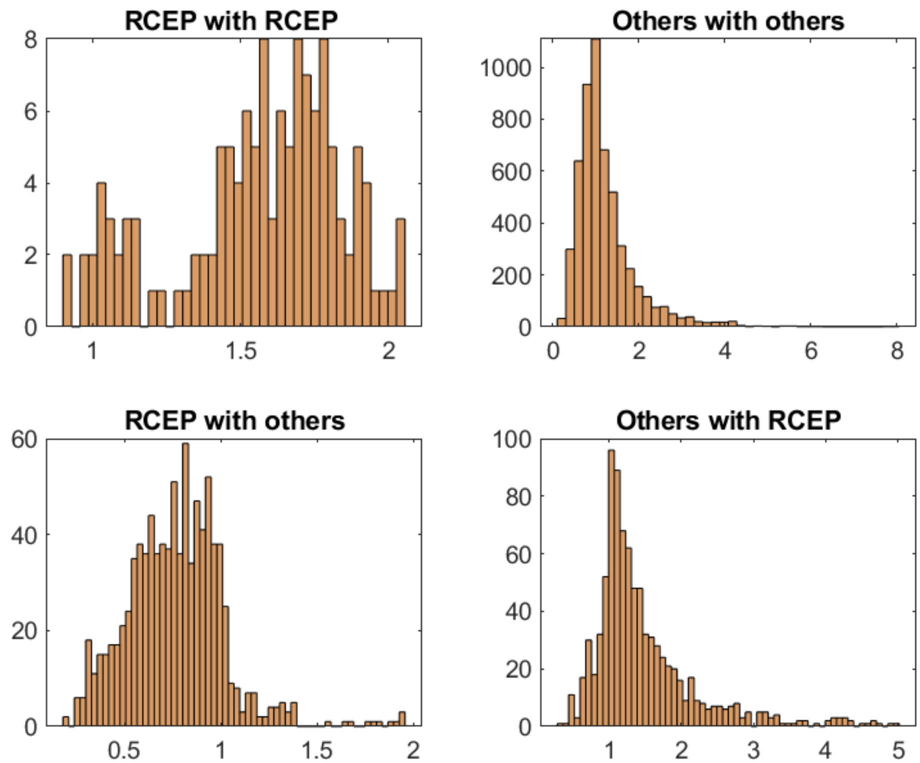
▲ Figure 6: Distributions of gross growth rates of welfare (real consumption), income shares, expenditure shares, and normalized market shares (NMS) after RCEP formation, in the long run, for all countries.

Note: Top left panel plots the distribution of the gross growth rates of real consumption / welfare ($\hat{C}_j$). Top right panel plots the distribution of the gross growth rates of income shares ($\hat{E}_j$). Bottom left panel plots the distribution of the gross growth rates of expenditure shares ($\hat{\lambda}_{ij}$). The bottom right panel plots the distribution of the gross growth rates of normalized market shares ($\hat{s}_{ij}$).

▲ Table 15: Decomposition of welfare changes into size and price effects: ross growth rates of size ($\hat{E}$), price ($\hat{P}$) and welfare ($\hat{C}$).

Country name	Size	Price	Welfare	Country name	Size	Price	Welfare
Afghanistan	0.7919	0.7991	0.9910	Jamaica	0.9360	0.9589	0.9762
Angola	1.0548	1.0546	1.0002	Jordan	0.8901	0.8902	0.9999
Albania	0.9237	0.9240	0.9997	Japan	1.0014	1.0014	1.0000
Andorra	0.9697	0.9830	0.9864	Kenya	0.8560	0.8561	0.9999
United Arab Emirates	0.9737	0.9737	1.0000	Cambodia	0.9984	0.9790	1.0198
Argentina	0.9910	0.9917	0.9993	South Korea	1.0268	1.0267	1.0001
Australia	1.0107	1.0106	1.0001	Kuwait	1.1414	1.1412	1.0001
Austria	0.9973	0.9973	1.0000	Lebanon	0.8531	0.9097	0.9378
Burkina Faso	0.9714	0.9791	0.9921	Sri Lanka	0.9243	0.9243	1.0000
Bulgaria	0.9818	0.9818	1.0000	Lesotho	0.9631	0.9841	0.9787
Bahrain	0.9270	0.9270	0.9999	Luxembourg	0.9898	0.9904	0.9995
Bermuda	0.8831	0.8851	0.9977	Macao	0.7885	0.7900	0.9981
Brazil	1.0013	1.0013	1.0000	Morocco	0.9420	0.9420	0.9999
Botswana	0.8816	0.8818	0.9998	Maldives	0.8232	0.8232	1.0000
Canada	0.9535	0.9535	1.0000	Mexico	0.9587	0.9588	1.0000
Switzerland	0.9866	0.9866	1.0000	Myanmar	1.0901	0.9212	1.1834
Chile	0.9990	0.9989	1.0000	Mongolia	1.0636	1.0634	1.0001
China	1.0236	1.0236	1.0000	Mauritius	0.9100	0.9112	0.9988
Congo	1.0009	0.9903	1.0107	Malaysia	1.0374	1.0372	1.0002
Colombia	0.9472	0.9472	1.0000	Namibia	0.9114	0.9117	0.9997
Costa Rica	0.9380	0.9380	1.0000	Niger	0.9232	0.9235	0.9996
Cyprus	0.9194	0.9196	0.9998	Netherlands	0.9936	0.9935	1.0001
Germany	1.0138	1.0137	1.0000	Norway	1.0272	1.0271	1.0001
Denmark	1.0047	1.0047	1.0000	New Zealand	1.0000	0.9999	1.0001
Algeria	0.9606	0.9607	1.0000	Oman	1.0007	1.0007	1.0001
Ecuador	0.9580	0.9580	1.0000	Panama	0.9145	0.9147	0.9998
Egypt	0.8983	0.8984	0.9999	Peru	0.9752	0.9752	1.0000
Spain	0.9779	0.9779	1.0000	Philippines	1.0045	1.0044	1.0001
Ethiopia	0.8450	0.8452	0.9997	Poland	0.9970	0.9970	1.0000
Finland	1.0092	1.0092	1.0000	Portugal	0.9739	0.9739	1.0000
Fiji	0.9003	0.9006	0.9998	Fr. Polynesia	0.8648	0.9427	0.9173
France	0.9871	0.9871	1.0000	Qatar	1.0652	1.0650	1.0002
United Kingdom	0.9581	0.9581	1.0000	Saudi Arabia	1.0002	1.0002	1.0000
Greece	0.9400	0.9401	0.9999	El Salvador	0.9049	0.9339	0.9690
Greenland	1.0079	1.0043	1.0035	Sweden	1.0034	1.0034	1.0000
Hong Kong	0.8279	0.8356	0.9908	Thailand	1.0115	1.0053	1.0062
Hungary	1.0061	1.0061	1.0000	Tunisia	0.9625	0.9626	1.0000
Indonesia	1.0144	1.0144	1.0001	Turkey	0.9571	0.9571	1.0000
India	0.9473	0.9473	1.0000	Tanzania	0.8794	0.8797	0.9997
Ireland	1.0315	1.0314	1.0001	Uruguay	0.9986	0.9970	1.0016
Iran	1.0006	1.0006	1.0000	United States of America	0.9426	0.9426	1.0000
Iceland	0.9863	0.9863	1.0000	Vietnam	0.9994	0.9992	1.0001
Israel	0.9834	0.9834	1.0000	South Africa	0.9905	0.9905	1.0000
Italy	1.0034	1.0034	1.0000	Zimbabwe	0.9521	0.9521	0.9999

Note: Welfare is defined as the change in real consumption, $C_j = E_j/P_j$, where E_j is the total expenditure, and P_j is the price index. This table decomposes the changes in welfare into changes in size ($\hat{E}_j$) and changes in the price index ($\hat{P}_j$).



▲ Figure 7: Distributions of gross growth rates of normalized market shares of different country groups, in the long run.

Note: Top left panel plots the distribution of the gross growth rates of normalized markets shares of RCEP countries with other members of RCEP (excluding domestic trade). Top right panel plots the distribution of the gross growth rates of normalized markets shares of countries outside of RCEP among each other (including domestic trade). Bottom left panel plots the distribution of the gross growth rates of normalized markets shares of RCEP members (as exporters) with countries outside of RCEP (as importers). Bottom right panel plots the distribution of the gross growth rates of normalized markets shares of countries outside of RCEP (as exporters) with RCEP members (as importers).

▲ Table 16: Block coefficients and corresponding percentage iceberg trade cost reductions use in the counterfactual general equilibrium exercise.

Block	Number of RCEP pairs	Anticipation coefficient	Anticipation iceberg trade cost reduction	Long run coefficient	Long run iceberg trade cost reduction
1	1	0.54	10.83	0.63	12.63
2	16	0.39	7.98	0.46	9.11
3	15	0.19	3.81	0.52	10.41
4	14	0.36	7.15	0.44	8.97
5	15	0	0	0.50	10.00
6	3	0	0	0.37	7.43
7	6	0	0	0.50	10.08
8	30	0	0	0.37	7.37
9	32	0	0	0.15	3.05

Note: The coefficients correspond to regression adjustment coefficients for each block, resulting from a blocking procedure applied to year 2015, following the methodology outlined in the empirical section of the paper. Zero coefficients correspond to block point estimates that were not statistically significant. The corresponding iceberg trade cost reductions were calculated using the trade elasticity of $\varepsilon = 5$.

▲ Table 17: Average percentage change of normalized market shares in RCEP members' mutual trade, in anticipation and long run.

Block	Anticipation	Long run
1	38.06	-24.55
2	47.85	0.57
3	20.59	36.39
4	38.99	14.29
5	0.43	53.16
6	1.92	51.17
7	-1.04	47.78
8	-2.79	40.01
9	-2.15	13.69

Note: The counterfactual exercise is carried out using block coefficients and corresponding iceberg trade cost shocks presented in Table 16.

▲Appendix E. Standard Errors

As described in the body of the paper, at the analysis stage the structure of the data is such that the same control country pairs appear multiple times for different time periods within each block. This appendix deals with the consequences of such setup for the estimation of the means and the sampling variances within each block. In order to relax the assumption that the control units are independent observations, I run two simulation exercises: bootstrap and re-sampling from control distribution. Both methods demonstrate that the point estimates of the $\hat{\tau}$ from Equation 6 are very close to the mean of the simulated distribution; while standard errors are systematically higher in the simulations.

Bootstrap

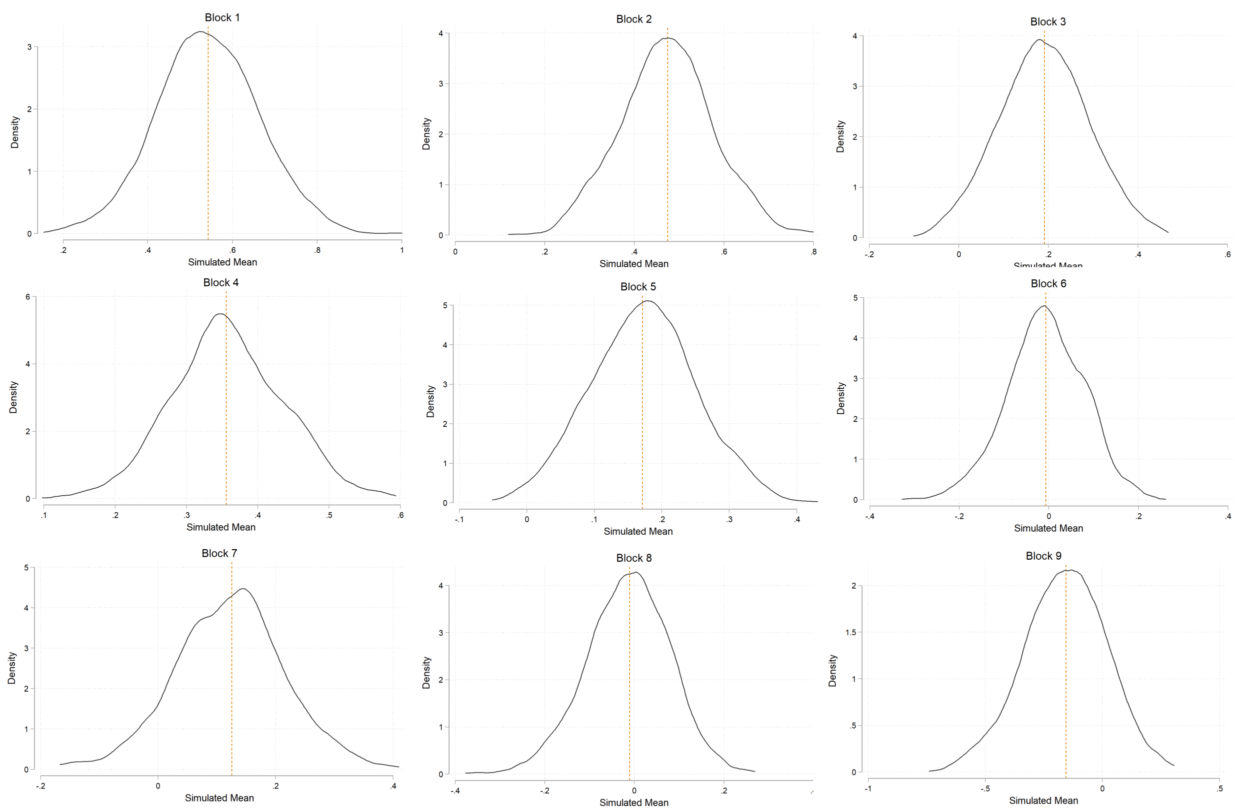
The first method is a standard bootstrap procedure. For each $T = \{A, S, M, L\}$ and for each block, I re-sample observations with replacement, run the regression using Equation 6, calculate the mean and the standard error at each iteration; perform this procedure one thousand times. This will give me a whole distribution of block means and standard errors. Since I do this for each time period (pre-treatment, anticipation, short, medium, and long run) and each of the nine blocks, there are a total of 45 distributions. In the interest of space, I will report the means of the simulated point estimates and standard errors distributions along with the their counterparts without re-sampling; and provide a visualisation of the typical distribution.

Table 1 reports the results for $\hat{\tau}$'s and the means for their simulated distributions obtained using bootstrap. With the exception of the pre-treatment period, all the point estimates of the mean are almost exactly the same as the means of the simulated distributions. The slightly higher differences between the two estimates for the pre-treatment period, however, do not change the conceptual results, as the point estimates are still not statistically significant, given the standard errors. Figure 1 shows simulated distribution and the point estimate for the anticipation period for the nine blocks, visually re-enforcing the reported results in the table. This is a typical picture for all other periods as well.

Similarly, Table 2 reports the means of the simulated distributions for the standard errors, as well as standard errors obtained using the data without re-sampling. The main conclusion is that the bootstrapped standard errors are systematically higher than their counterparts in the full sample. Figure 2 confirms this conclusion visually.

▲ Table 1: The point estimates and the means of the simulated distributions using bootstrap, by block and time period

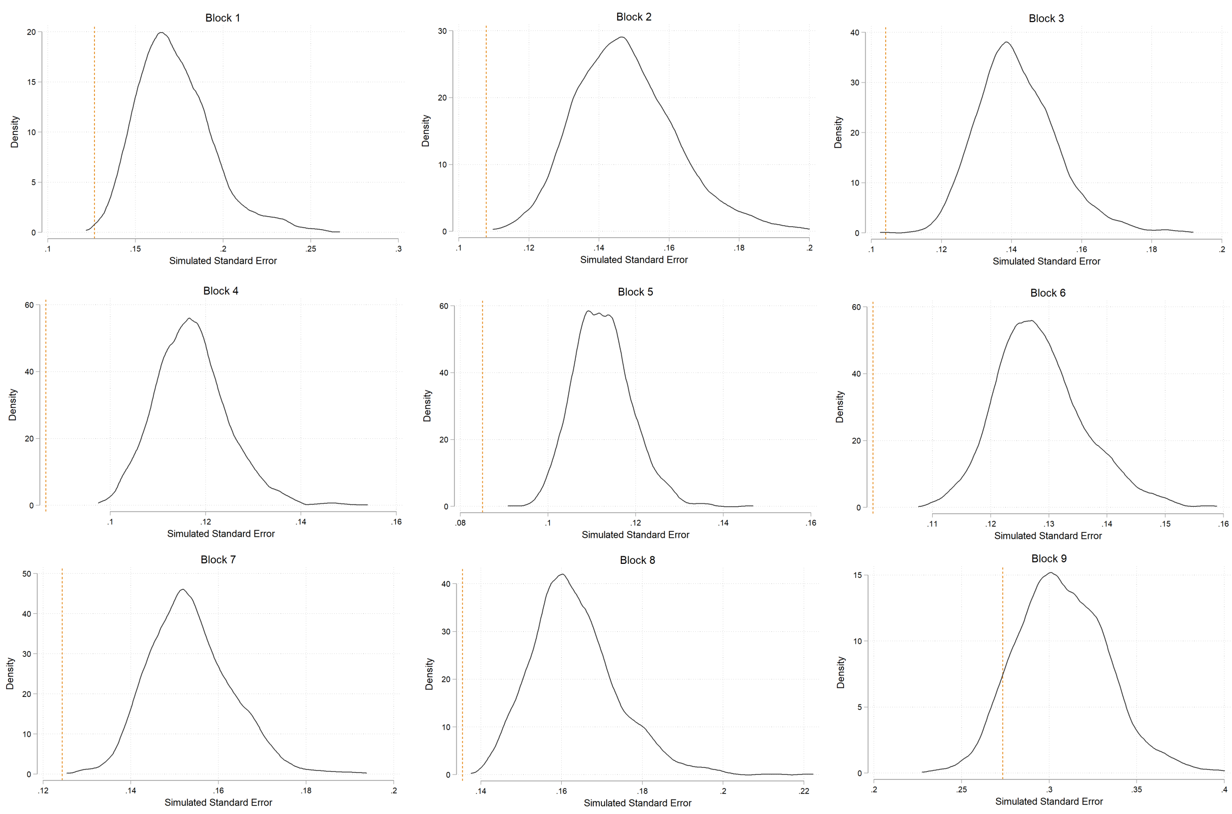
	Pre-Treatment		Anticipation		Short Run		Medium Run		Long Run	
	Point Estimate	Distribution Mean	Point Estimate	Distribution Mean	Point Estimate	Distribution Mean	Point Estimate	Distribution Mean	Point Estimate	Distribution Mean
B1	0.296	0.258	0.543	0.542	0.914	0.914	0.633	0.632	0.419	0.421
B2	-0.002	-0.065	0.475	0.473	0.393	0.399	0.448	0.455	0.455	0.444
B3	-0.144	-0.127	0.191	0.190	0.478	0.481	0.519	0.520	0.657	0.648
B4	-0.095	-0.173	0.356	0.358	0.405	0.403	0.451	0.449	0.383	0.380
B5	0.025	0.110	0.171	0.173	0.420	0.416	0.498	0.500	0.428	0.433
B6	0.066	0.238	-0.007	-0.007	0.146	0.146	0.370	0.372	0.301	0.304
B7	0.065	0.210	0.125	0.122	0.321	0.327	0.461	0.455	0.504	0.504
B8	-0.042	0.236	-0.011	-0.014	0.167	0.168	0.361	0.360	0.364	0.369
B9	1.080	1.226	-0.156	-0.161	0.089	0.104	-0.091	-0.073	0.140	0.153



▲ Figure 1: The bootstrap-simulated distributions and the point estimates of $\hat{\tau}$ for anticipation period, by block.

▲ Table 2: The standard errors and the means of the simulated distributions of the standard errors using bootstrap, by block and time period

	Pre-Treatment		Anticipation		Short Run		Medium Run		Long Run	
	Full Sample	Distribution Mean	Full Sample	Distribution Mean	Full Sample	Distribution Mean	Full Sample	Distribution Mean	Full Sample	Distribution Mean
B1	0.128	0.201	0.127	0.173	0.131	0.179	0.143	0.195	0.196	0.268
B2	0.124	0.173	0.108	0.148	0.129	0.178	0.124	0.170	0.143	0.197
B3	0.135	0.209	0.104	0.142	0.119	0.162	0.145	0.199	0.144	0.197
B4	0.093	0.128	0.086	0.117	0.093	0.125	0.104	0.141	0.115	0.156
B5	0.090	0.132	0.085	0.112	0.087	0.114	0.098	0.129	0.106	0.139
B6	0.105	0.156	0.100	0.129	0.109	0.141	0.104	0.133	0.113	0.147
B7	0.131	0.155	0.124	0.154	0.129	0.157	0.145	0.176	0.152	0.187
B8	0.189	0.228	0.135	0.163	0.150	0.179	0.154	0.180	0.151	0.176
B9	0.340	0.363	0.273	0.307	0.333	0.373	0.371	0.408	0.401	0.430



▲ Figure 2: The bootstrap-simulated distributions of standard errors and the estimates of standard errors using the full sample in anticipation period, by block.

Re-Sampling From Control Distribution

In the bootstrap exercise all observations were randomly re-sampled with replacements. However, given the structure of my data, I know that the non-independent observations are only the ones in the control group. Thus, I perform a simulation which is more tailored to my data structure. In particular, it randomly re-samples observations only from the control group, while leaving the treated observations the same within each sample. The algorithm is similar to the bootstrap: for each $T = \{A, S, M, L\}$ and for each block, I sample the observations, run the regression using Equation 6, calculate the mean and the standard error at each iteration; perform this procedure one thousand times. The number of control observations sampled at each iteration is approximately equal to the number of unique control pairs within each block.

Similarly to bootstrap results, Table 3 shows that there are no big differences between the point estimates of the $\hat{\tau}$'s and the means of the simulated distributions. Differently from the bootstrap, however, the results suggest that the means should be slightly higher for every block and time period. For most blocks, however, the differences are small, as confirmed visually in Figure 3, which plots the distributions for the nine blocks in the anticipation period as an example.

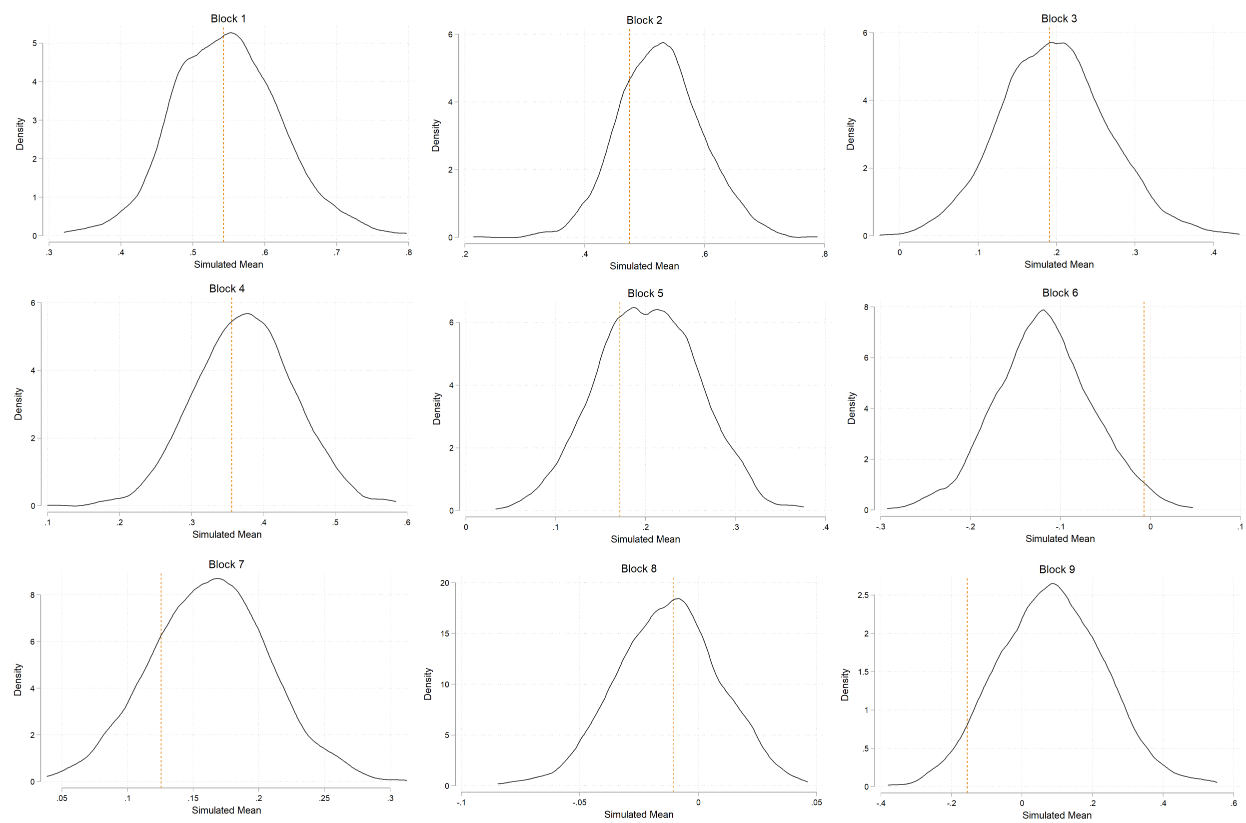
Similarly, the re-sampling method confirms the results of the bootstrap estimation for the standard errors. Table 4 compares the standard errors obtained from the full sample estimation and the mean of the simulated distribution of the standard errors. Again, the simulated standard errors are systematically higher than those from the full sample.

Comparison

Finally, Figure 5 compares the standard errors obtained with three different methods: by estimating the full sample, by performing a bootstrap procedure, and by re-sampling from the control distributions, in different time periods, across all blocks. The conclusion is that the bootstrap standard errors are larger than those obtained by the other two methods. I therefore use these more conservative standard errors in the body of the paper to report the statistical significance of the point estimates.

▲ Table 3: The point estimates and the means of the simulated distributions using re-sampling from the control group, by block and time period

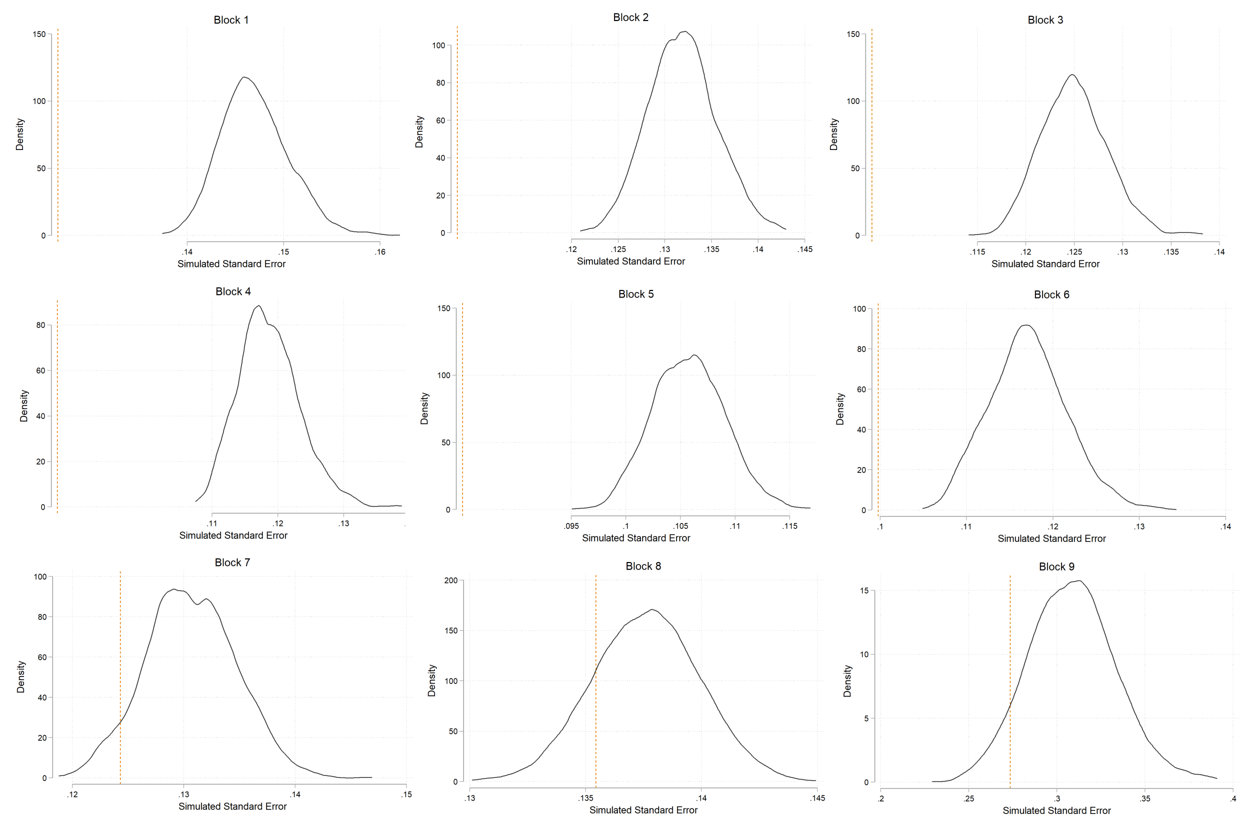
	Pre-Treatment		Anticipation		Short Run		Medium Run		Long Run	
	Point Estimate	Distribution Mean	Point Estimate	Distribution Mean	Point Estimate	Distribution Mean	Point Estimate	Distribution Mean	Point Estimate	Distribution Mean
B1	0.296	0.318	0.543	0.547	0.914	0.922	0.633	0.681	0.419	0.509
B2	-0.002	0.046	0.475	0.526	0.393	0.454	0.448	0.522	0.455	0.579
B3	-0.144	-0.122	0.191	0.197	0.478	0.508	0.519	0.564	0.657	0.694
B4	-0.095	-0.029	0.356	0.374	0.405	0.435	0.451	0.583	0.383	0.551
B5	0.025	0.132	0.171	0.202	0.420	0.457	0.498	0.553	0.428	0.518
B6	0.066	0.137	-0.007	-0.119	0.146	0.058	0.370	0.311	0.301	0.224
B7	0.065	0.082	0.125	0.163	0.321	0.353	0.461	0.498	0.504	0.566
B8	-0.042	-0.038	-0.011	-0.013	0.167	0.162	0.361	0.361	0.364	0.362
B9	1.080	1.128	-0.156	0.084	0.089	0.270	-0.091	0.004	0.140	0.327



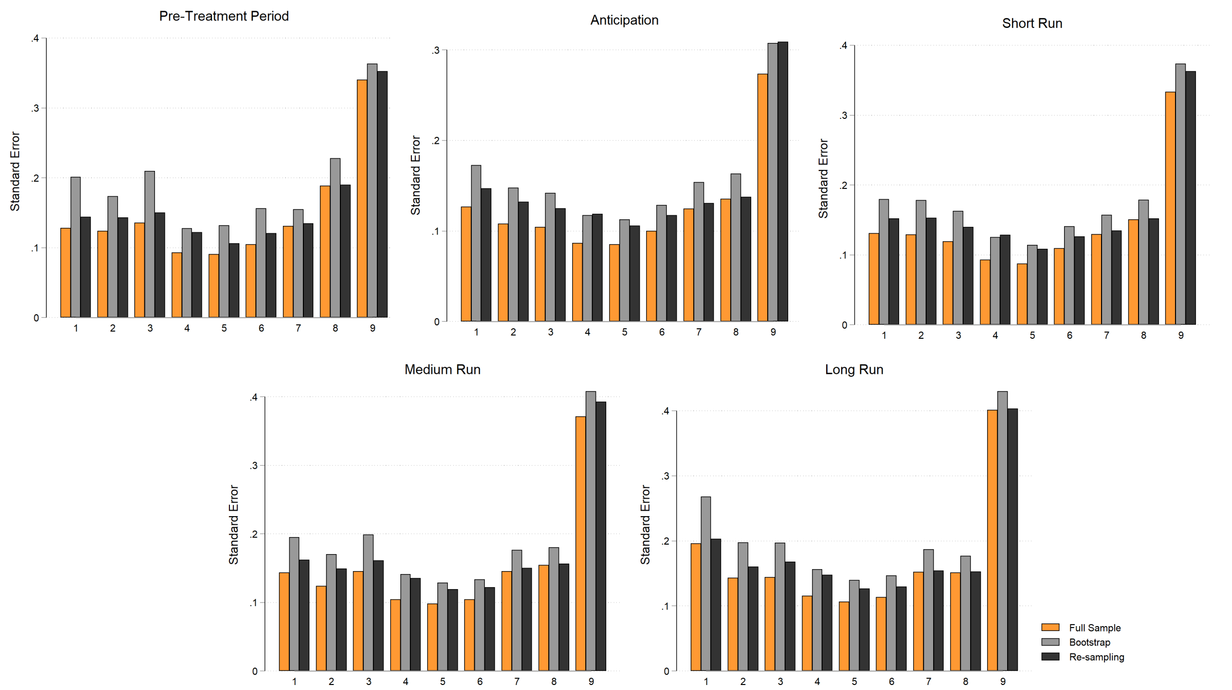
▲ Figure 3: The simulated distributions using re-sampling from the control group, and the point estimates of $\hat{\tau}$ for anticipation period, by block.

▲ Table 4: The standard errors and the means of the simulated distributions of the standard errors using re-sampling from the control distribution, by block and time period

	Pre-Treatment		Anticipation		Short Run		Medium Run		Long Run	
	Full Sample	Distribution Mean	Full Sample	Distribution Mean	Full Sample	Distribution Mean	Full Sample	Distribution Mean	Full Sample	Distribution Mean
B1	0.128	0.144	0.127	0.147	0.131	0.152	0.143	0.162	0.196	0.203
B2	0.124	0.143	0.108	0.132	0.129	0.153	0.124	0.149	0.143	0.160
B3	0.135	0.150	0.104	0.125	0.119	0.140	0.145	0.161	0.144	0.168
B4	0.093	0.122	0.086	0.119	0.093	0.128	0.104	0.135	0.115	0.148
B5	0.090	0.106	0.085	0.106	0.087	0.108	0.098	0.119	0.106	0.126
B6	0.105	0.121	0.100	0.117	0.109	0.126	0.104	0.122	0.113	0.129
B7	0.131	0.135	0.124	0.131	0.129	0.135	0.145	0.150	0.152	0.154
B8	0.189	0.190	0.135	0.138	0.150	0.152	0.154	0.156	0.151	0.152
B9	0.340	0.352	0.273	0.309	0.333	0.363	0.371	0.393	0.401	0.403



▲ Figure 4: The simulated distributions of standard errors using re-sampling from the control group, and the estimates of standard errors in the full sample, for anticipation period, by block.



▲ Figure 5: The comparison of the standard errors obtained by estimating the full sample, using the bootstrap, and the re-sampling from the control distribution.

▲Appendix F. Results without Imputation

This appendix implements the causal inference framework on the data without imputed values. The main conclusion is that the conceptual results remain intact: PTAs gradually increase trade; and in anticipation only non-natural trading partners react to the PTA shock. However, the estimates are noisier, and the standard errors are higher due to the reduced power. Moreover, the magnitude of the averages is slightly reduced.

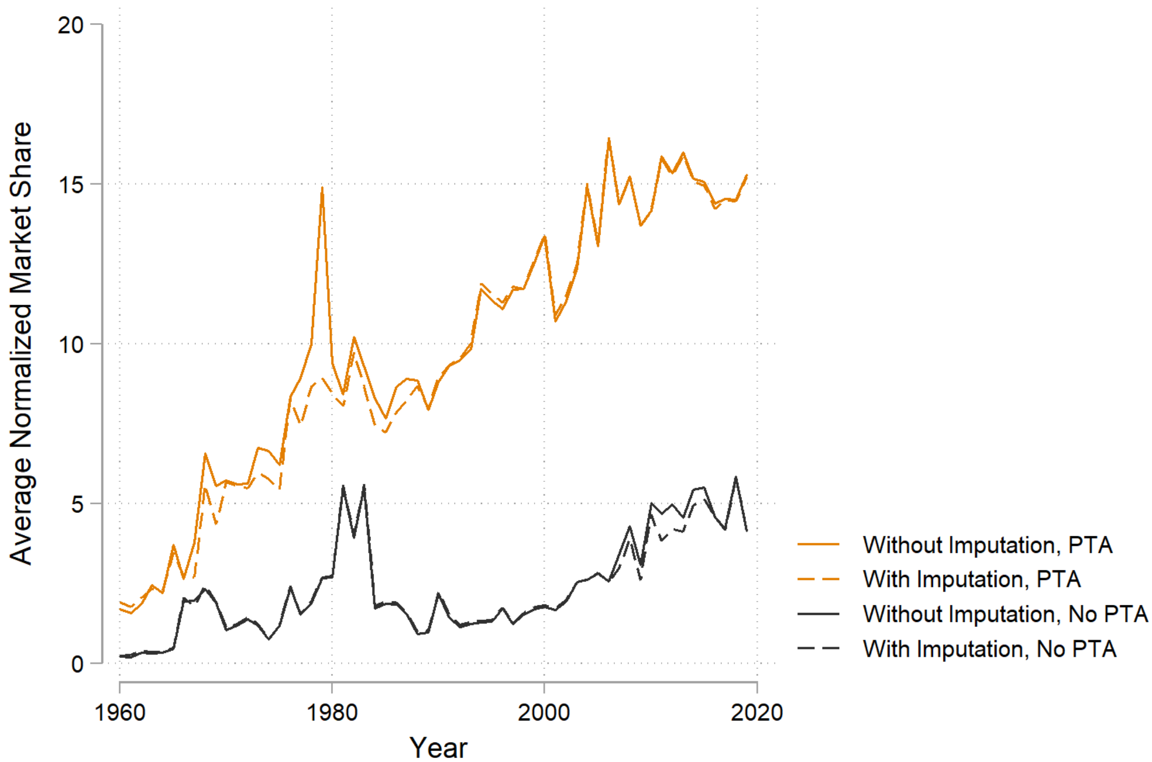
To understand why, let me first present the comparison between the normalized makers shares calculated using raw data and the data with imputed values. The correlation between the two shares is 0.98, and 0.99 between their logs. Table 1 shows the summary statistics for the raw (not imputed) shares and shares obtained after imputing the trade volumes. First, the number of observations is substantially higher for the (log) shares calculated with imputed data. The differences in means across the entire sample suggest that imputation leads to lower average shares for both pairs with and without PTAs. The standard deviation for the raw shares is slightly higher for all types of pairs.

▲ Table 1: Summary statistics of normalized market shares calculated with and without imputation

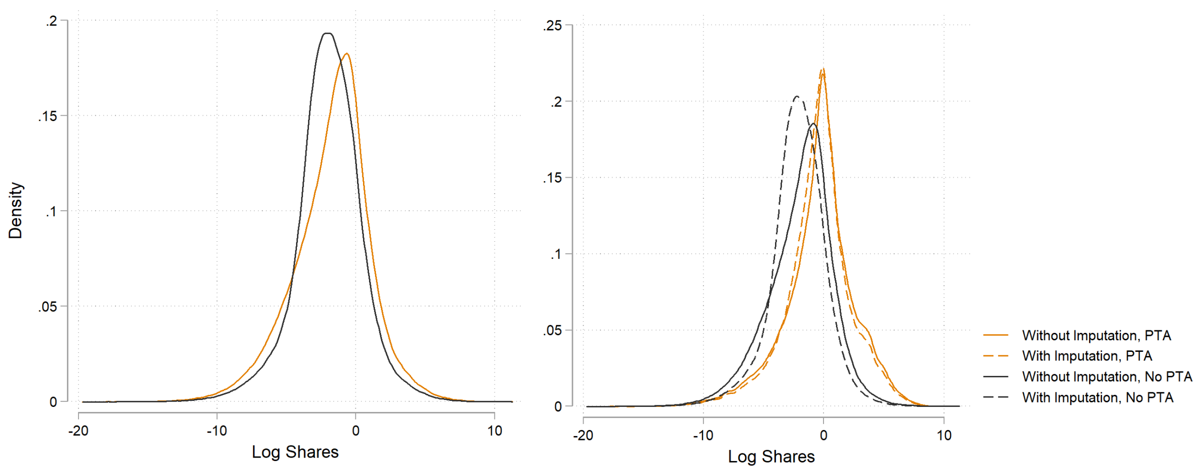
		N Obs	Mean	Std. Dev	Min	Max
PTA=0	Raw	2,465,521	2.60	161.33	0	78,081
	Imputed	2,465,521	2.55	159.21	0	78,249
	log(Raw)	887,269	-1.94	2.71	-19.72	11.26
	log(Imputed)	1,455,399	-2.12	2.29	-19.70	11.27
PTA=1	Raw	167,897	18.24	165.09	0	22,826
	Imputed	167,897	17.69	143.95	0	12,672
	log(Raw)	132,468	-0.38	2.92	-17.81	10.03
	log(Imputed)	157,681	-0.48	2.75	-17.81	9.45

Note: The normalized market shares are substituted with zeros whenever they are missing.

Figure 1 plots the average normalized market shares by year for countries with and without PTAs. For both series the shares using imputed trade track closely the shares calculated in the raw data. Figure 2 reveals the main differences between the two shares: the distribution for the shares with imputed data is slightly skewed to the right (left panel), and particularly so for the control units (right panel). Such situation occurs because because many missing values (i.e. values that are imputed) occur for smaller and poorer countries which tend to under-report their trade.



▲ Figure 1: Average normalized market shares calculated using raw data and data with imputed trade volumes for pairs with and without PTAs, 1960-2019.



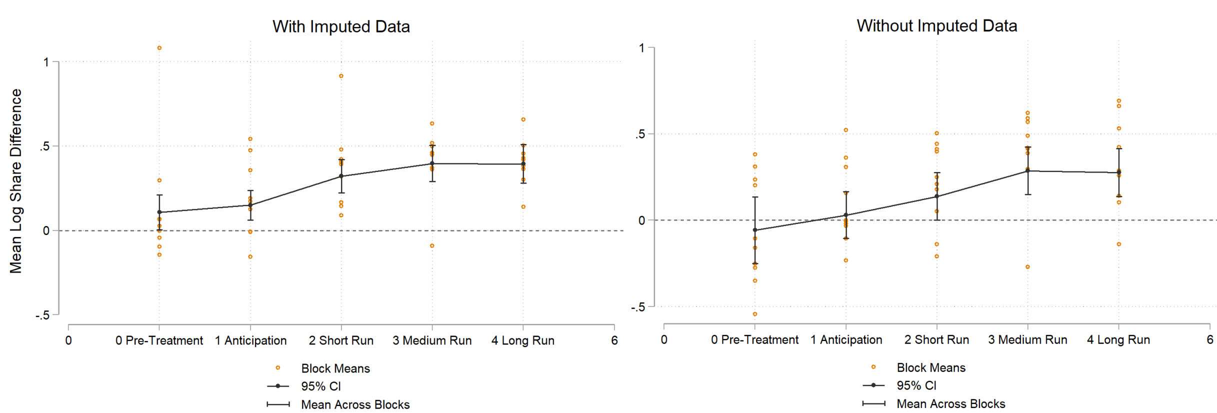
▲ Figure 2: The distribution of normalized market shares calculated using raw data and data with imputed trade volumes in the full sample (left panel) and by treatment group (right panel)

Now let me report the results of the entire study, using the dataset with normalized market shares where the trade volumes were not imputed. I use exactly the same procedure as in the body of the paper. The blocking procedure groups pairs into ten subsamples. Table 2 shows the percentage increases in normalized market shares of the country pairs with PTAs relative to control pairs for different time periods. Comparing the results with Table 6, we can conclude the magnitudes of the point estimates are lower. Moreover, the estimates for the anticipation and short run period are not statistically significant. This happens due to both the decreased average estimates, and the increased standard errors (recall that the standard deviation of the measures is higher in the case of raw data). Figure 3 plots the means of each block, and the weighted average across blocks, along with 95% confidence intervals. Overall, it visually confirms the result of PTA effects kicking in gradually over time.

▲ Table 2: Average PTA effects in different time periods.

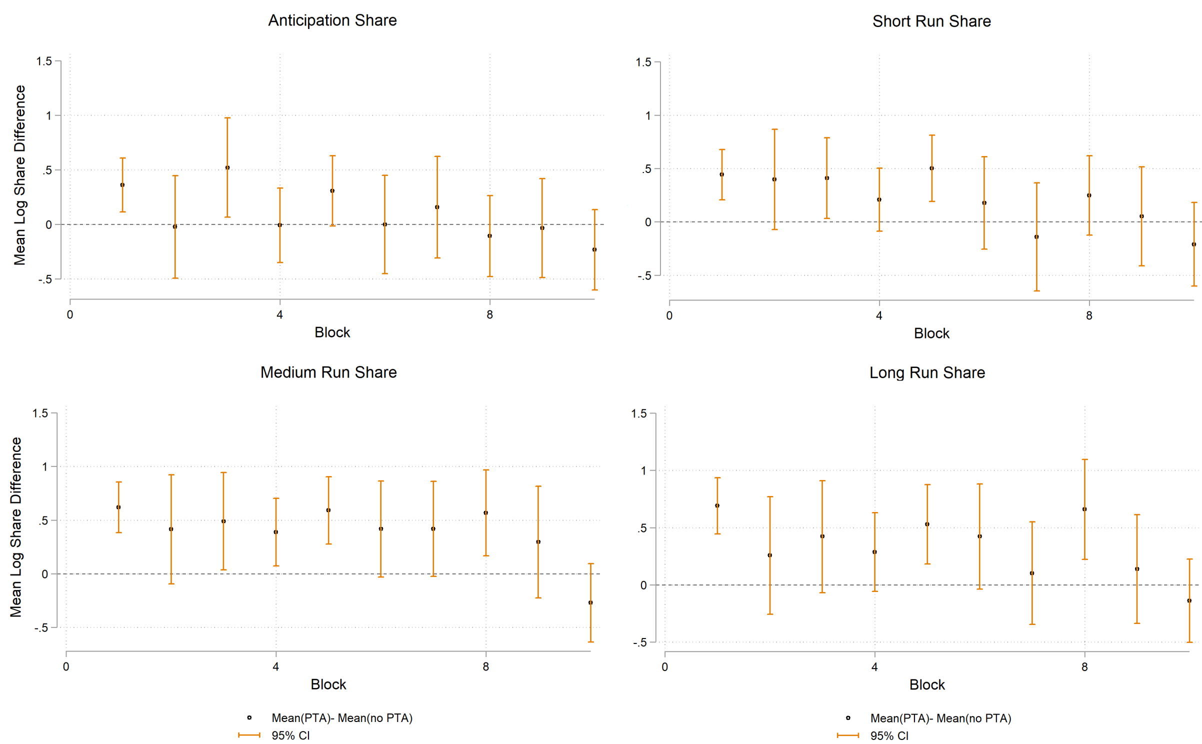
	Anticipation [t-5; t=0)	Short Run (t=0; t+5]	Medium Run (t+5; t+10]	Long Run (t+10; t+15]
Coefficient	0.03	0.14	0.29	0.28
Std. Err.	0.07	0.07	0.07	0.07
Percent	3%	15%	34%	32%

Note: 'Coefficient' is the weighted average of the block estimates from estimating Equation 6 for each block within a given time period. 'Standard error' is the mean of the standard error distribution from the bootstrap procedure described in Appendix E. The percentage increase of normalized market shares of treated pairs relative to controls is calculated using the standard formula for interpreting dummy variable coefficients: $\exp(\hat{\tau}) - 1$.



▲ Figure 3: Block means and average PTA effects for different time periods for normalized market shares calculated using imputed trade volumes (left panel) and using raw data (right panel)

Finally, Figure 4 shows the point estimates for each block and each time period. In general, the results appear to be much noisier, in particular for the anticipation and the long run. However, we can still observe that for some of the lower-index blocks – corresponding to non-natural trading partners – the anticipation effects are present and are statistically significant; and are on average higher than for natural trading partners. In the short and medium run we observe a gradual increase in point estimates for all types of country pairs. These results are carried on to the long run period, although with increased standard errors for many blocks.



▲ Figure 4: Average treatment effects within blocks in different time periods.

Note: The figure plots the point estimates of $\hat{\tau}$'s from Equation 6 for each of the nine blocks and each time period. The 95% confidence interval is calculated using the standard errors obtained from the bootstrap procedure described in Appendix E.

▲Appendix G. Numerical Simulation for Different Estimation Methods

Figure 6 shows the point estimates of the blocking estimator are consistently lower than those obtained by applying other methods. This appendix constructs a numerical simulation which demonstrates that the blocking estimator performs better in a model with non-random PTA assignment.

This stylized numerical example starts off by creating an economy using the gravity model described in Section 6. A small modification concerns the structure of trade costs which are now assumed to have two components: transport costs t_{ij} and trade policy cost β_{ij}. The total trade cost then has the following form:

$$\tau_{ij} = t_{ij}\beta_{ij}$$

where $\beta_{ii} = 1$ and $t_{ii} = 1$.

Then, every period I augment the trade cost, and use the 'exact hat algebra' in a series of static model exercises to get the new equilibrium income distribution and trade flows. In each simulation iteration the parameters of the initial economy are drawn from uniform distributions, and are then augmented by trade shocks, generating panel datasets of trade flows. I simulate 500 such datasets with 50 countries and 10 periods each. In each period the transport costs t_{ij} reduce by 5% for all country pairs where $i \neq j$. The 10% reductions in trade policy costs β_{ij} (reflecting a PTA formation) are designed in two distinct cases:

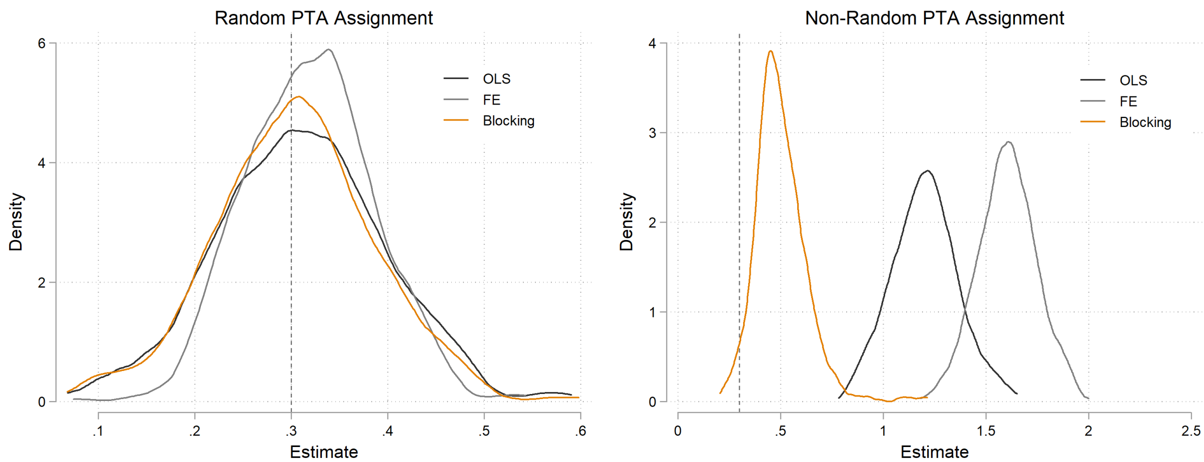
1. Random PTA assignment: any country pair gets a PTA with a probability of 30%.

2. Non-random PTA assignment: country pairs which are more important to each other than their average trading partner have a higher probability of getting a PTA. In particular, recalling the intuition behind the normalized market shares, if $\bar{s}_{ij} = 1/2(s_{ij} + s_{ji}) > 1$, then a pair gets a PTA with a probability of 60%, while other pairs get a PTA with a probability of 30%.

In each simulated panel dataset I estimate the effects of PTAs (reductions in trade policy costs) using three different estimators: the Ordinary Least Squares (OLS) estimator, the Fixed Effects (FE) estimator, and the blocking estimator.[34]

[34] In this stylized numerical example the application of propensity score matching and entropy balancing does not make much sense, since there are no covariates. Due to the lack of covariates, the blocking estimator blocks on the the distribution of normalized markets shares.

Figure 1 plots the distribution of the estimates obtained by different estimation methods. The left panel corresponds to the datasets simulated using the random PTA assignment, which the right panel shows the distribution of estimates in case of non-random PTA assignment. The dashed vertical line in both cases indicates the true reduction in trade costs. In case of random PTAs, as expected, all estimators are able to capture correctly the true trade cost reductions. [35] For the non-random reductions, however, the OLS and the FE estimators tent to overestimate the effects of PTAs to a larger extent than the blocking estimator.

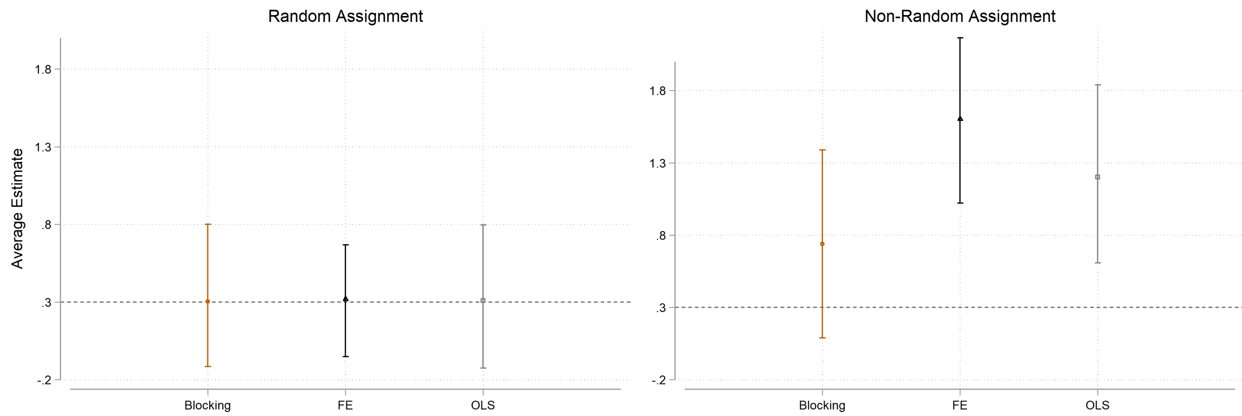


▲ Figure 1: The distribution of the estimates obtained by applying different types of estimators in the simulated datasets.

Note: For each type of estimator the kernel density is estimated using the 500 point estimates from different estimators. The dotted vertical line indicates the true reductions in trade costs.

This numerical simulation demonstrates that the blocking estimator performs better in case of non-random PTA assignment, even when data is generated by the gravity model. Clearly, as highlighted by the applied empirical literature, the non-parametric methods are not immune to biases, and this stylized simulation confirms this fact for the distribution of the means. However, when combined with the confidence interval estimation, the blocking estimator is the only one that includes the true value, as shown in Figure 2.

[35] The distributions in Figure 1 represent only the point estimates, which, combined with the standard errors would contain the true estimate in the confidence intervals.



▲ Figure 2: The mean and the standard errors of the estimates obtained by applying different types of estimators in the simulated datasets.

Note: For each type of estimator the kernel density is estimated using the 500 point estimates from different estimators. The dotted vertical line indicates the true reductions in trade costs.

▲Appendix H. Sensitivity of General Equilibrium Estimates to the Values Of Elasticity

In the baseline version of the counterfactual exercises the trade elasticity is set at the conventional value, $\varepsilon = \sigma - 1$ with $\sigma = 6$. This appendix repeats two counterfactual exercises presented in the main body of the paper using different values of trade elasticity.

The first exercise is conducted for the counterfactual equilibrium in the long run, i.e. when there is no heterogeneity across types of country pairs. The reduction in the iceberg trade costs is defined using the estimate from the empirical part of the paper of 48% increase in the long run, and the reductions in trade costs are defined using the different values of elasticity: $\varepsilon = 3$ (reduction in iceberg trade costs is 16%), $\varepsilon = 5$ (baseline reduction of 9.6%) and $\varepsilon = 7$ (reduction in iceberg trade costs of 6.86%). Thus, the role of elasticity is two-fold in the model: on the one hand, it amplifies the trade effects of trade cost changes, but on the other hand it decreases the magnitude of reductions in iceberg trade costs.

Table 1 below shows the main moments of the distributions of gross growth rates for different variables. The first one is the distribution of changes in welfare (real consumption): in the baseline model specification the average change in real consumption is 0.05%, and is very similar across different specifications. The larger the value of elasticity, the smaller is the standard deviation: the distribution 'shrinks', with minimum values rising (from -17.09% to =6.15%), and maximum values decreasing (from 28.48% to 12.27%). With larger values of trade elasticity the average normalized market shares for all countries also become smaller, with average growth of 26.08% for $\varepsilon = 3$ and 18.21% for $\varepsilon = 7$. Similarly, the dispersion of the distribution reduced with larger elasticity values. Unpacking the changes in the shares into trade between RCEP members and outsiders shows that the countries that are directly affected by the shock increase their shares more with higher value of trade elasticity: the mean increase is 32.34% for $\varepsilon = 3$, while with $\varepsilon = 7$ normalized market shares of RCEP countries more than double. At the same time, the outsiders are redirecting trade relatively less for higher values of elasticity: the increase in normalized market shares for low values of elasticity is 26.08%, while it is 18.21% for higher elasticity value.

The second counterfactual exercise presented in the main text utilizes the heterogeneity in point estimates across blocks. Table 2 presents the point estimates and the corresponding percentage reductions in iceberg trade costs by block, depending on the value of elasticity. These reductions are used in the counterfactual exercises to compute the changes in welfare and normalized market shares in anticipation and short run.

▲ Table 1: Descriptive statistics for the distributions of gross growth rates of real consumption, and normalized market shares, following the trade cost shock in the long run, for different values of elasticity.

Statistic	$\varepsilon = 3$	$\varepsilon = 5$	$\varepsilon = 7$
Welfare (real consumption)			
Mean	0.9977	0.9995	0.9995
Std	0.0401	0.0233	0.0161
Min	0.8291	0.9173	0.9385
Max	1.2848	1.1834	1.1227
NMS of all countries			
Mean	1.2608	1.1958	1.1821
Std	0.7707	0.6866	0.6694
Min	0.1549	0.1474	0.1460
Max	8.6934	8.0558	7.9141
NMS of RCEP with RCEP			
Mean	1.3234	1.5624	1.5274
Std	0.1868	0.2827	0.2662
Min	0.8671	0.9010	0.9066
Max	1.6890	2.0531	1.9943
NMS of others with others			
Mean	1.2608	1.2137	1.1821
Std	0.7707	0.7009	0.6694
Min	0.1549	0.1474	0.1460
Max	8.6934	8.0558	7.9141

Note: The values are calculated using the model presented in Section 6 for different values of the elasticity of substitution. The values correspond to different statistics in the distributions of gross growth rates of different variables. The top panel is the mean, the standard deviation, the minimum and the maximum values of the growth rates for welfare (real consumption) for all country pairs. The second panel presents the statistics for the distribution of the growth rates of normalized market shares for all countries. The third panel presents the statistics for the distribution of the growth rates of normalized market shares of RCEP members trading with each other. Finally, the last panel presents the statistics for the distribution of the growth rates of normalized market shares of pairs outside of RCEP trading with each other.

▲ Table 2: Block coefficients and corresponding percentage iceberg trade cost reductions use in the counterfactual general equilibrium exercise, for different values of elasticity.

Block	Anticipation coefficient	Anticipation iceberg trade cost reduction			Long run coefficient	Long run iceberg trade cost reduction		
		$\varepsilon = 3$	$\varepsilon = 5$	$\varepsilon = 7$		$\varepsilon = 3$	$\varepsilon = 5$	$\varepsilon = 7$
1	0.54	18.05	10.83	7.74	0.63	21.05	12.63	9.02
2	0.39	13.30	7.98	5.70	0.46	15.18	9.11	6.51
3	0.19	6.34	3.81	2.72	0.52	17.34	10.41	7.43
4	0.36	11.92	7.15	5.11	0.44	14.95	8.97	6.41
5	0	0	0	0	0.50	16.67	10.00	7.14
6	0	0	0	0	0.37	12.39	7.43	5.31
7	0	0	0	0	0.50	16.80	10.08	7.20
8	0	0	0	0	0.37	12.29	7.37	5.27
9	0	0	0	0	0.15	5.09	3.05	2.18

Note: The coefficients correspond to regression adjustment coefficients for each block, resulting from a blocking procedure applied to year 2015, following the methodology outlined in the empirical section of the paper. Zero coefficients correspond to block point estimates that were not statistically significant. The corresponding iceberg trade cost reductions were calculated using different values of trade elasticity.

Table 3 compares the changes in welfare (real consumption) for different values of trade elasticity for the RCEP members. The values are presented in percentage changes, and it is clear from the table that with the exception of Myanmar and Cambodia, RCEP members experience negligible changes in welfare. The differences in welfare generated by varying the levels of trade elasticity are also small, with larger values of elasticity generating slightly smaller gains in anticipation and long run. This happens due to the fact that larger values of trade elasticity correspond to lower reductions in iceberg trade costs, as shown in Table 2. For countries that are most affected, varying the levels of elasticity has large effects: for Myanmar, for example gains in anticipation are 7.05% for the value of $\varepsilon = 3$, and 'only' 2.82% for $\varepsilon = 7$. Similarly, Table 4 presents the percentage changes in average normalized market shares by block in anticipation and long run, for varying levels of elasticity, and demonstrates that, on average, larger elasticity values produce smaller changes in normalized market shares (again, due to reduced size of the shock).

▲ Table 3: Percentage changes in welfare (real consumption) for RCEP members following the trade cost shock in anticipation and long run, for different values of elasticity.

Country	Period	$\varepsilon = 3$	$\varepsilon = 5$	$\varepsilon = 7$
Australia	Anticipation	0.0045	0.0026	0.0019
	Long run	0.0104	0.0061	0.0043
China	Anticipation	0.0006	0.0004	0.0003
	Long run	0.0004	0.0002	0.0002
Indonesia	Anticipation	0.0045	0.0027	0.0019
	Long run	0.0048	0.0028	0.0020
Japan	Anticipation	-0.0001	-0.0001	-0.00001
	Long run	0.0019	0.0011	0.0008
Cambodia	Anticipation	1.0833	0.6686	0.4844
	Long run	1.6171	0.9255	0.6478
South Korea	Anticipation	0.0025	0.0016	0.0011
	Long run	0.0025	0.0015	0.0010
Myanmar	Anticipation	7.0461	4.0294	2.8212
	Long run	16.1542	9.7187	6.9716
Malaysia	Anticipation	0.0127	0.0080	0.0059
	Long run	0.0141	0.0082	0.0058
New Zealand	Anticipation	0.0059	0.0034	0.0024
	Long run	0.0033	0.0019	0.0014
Philippines	Anticipation	0.0016	0.0010	0.0008
	Long run	0.0078	0.0046	0.0032
Thailand	Anticipation	0.1237	0.0787	0.0578
	Long run	0.5283	0.3061	0.2155
Vietnam	Anticipation	0.0036	0.0022	0.0016
	Long run	0.0162	0.0092	0.0064
Average	Anticipation	0.6907	0.3999	0.2816
	Long run	1.5301	0.9155	0.6550

Note: The values are calculated using the model presented in Section 6 for different values of the elasticity of substitution. The trade elasticity parameter is defined in the model as $\varepsilon = \sigma - 1$. The values correspond to percentage changes in real consumption for RCEP members in anticipation and long run. Trade cost shocks in different periods are defined using the values specified in Table 16.

▲ Table 4: Percentage changes in average normalized market shares of RCEP members' trade with each other, by block, following the trade cost shock in anticipation and long run, for different values of elasticity.

Block	Period	$\varepsilon = 3$	$\varepsilon = 5$	$\varepsilon = 7$
1	Anticipation	39.89	38.06	37.32
	Long run	-25.61	-24.55	-24.11
2	Anticipation	49.27	47.85	47.30
	Long run	0.32	0.57	0.67
3	Anticipation	20.30	20.59	20.75
	Long run	37.18	36.39	36.07
4	Anticipation	39.45	38.99	38.86
	Long run	14.45	14.29	14.24
5	Anticipation	0.24	0.43	0.53
	Long run	55.55	53.16	52.22
6	Anticipation	1.66	1.92	2.05
	Long run	52.93	51.17	50.48
7	Anticipation	-1.35	-1.04	-0.89
	Long run	49.59	47.78	47.09
8	Anticipation	-3.28	-2.79	-2.56
	Long run	41.26	40.01	39.51
9	Anticipation	2.64	-2.15	-1.92
	Long run	13.69	13.69	13.69
Average	Anticipation	15.95	15.76	15.72
	Long run	26.59	25.84	25.54

Note: The values are calculated using the model presented in Section 6 for different values of the elasticity of substitution. The trade elasticity parameter is defined in the model as $\varepsilon = \sigma - 1$. The values correspond to percentage changes average normalized market shares of RCEP members' trade with each other, by block, in anticipation and long run. Trade cost shocks in different periods are defined using the values specified in Table 16.

▲Appendix I. Comparison of General Equilibrium Estimates

In Figure 6 I demonstrated that the point estimates of the empirical gravity model with three-way fixed effects are larger in the long run, compared to the blocking estimator (68% vs. 48% increase in normalized market shares). This appendix aims to answer the question of how much this difference in partial equilibrium estimates translates into the general equilibrium predictions. I use the 68% point estimate in the baseline version of the model with trade elasticity $\varepsilon = 5$, which translates into 13.6% reduction in iceberg trade costs in the long run (compared to 9.6% in the baseline).

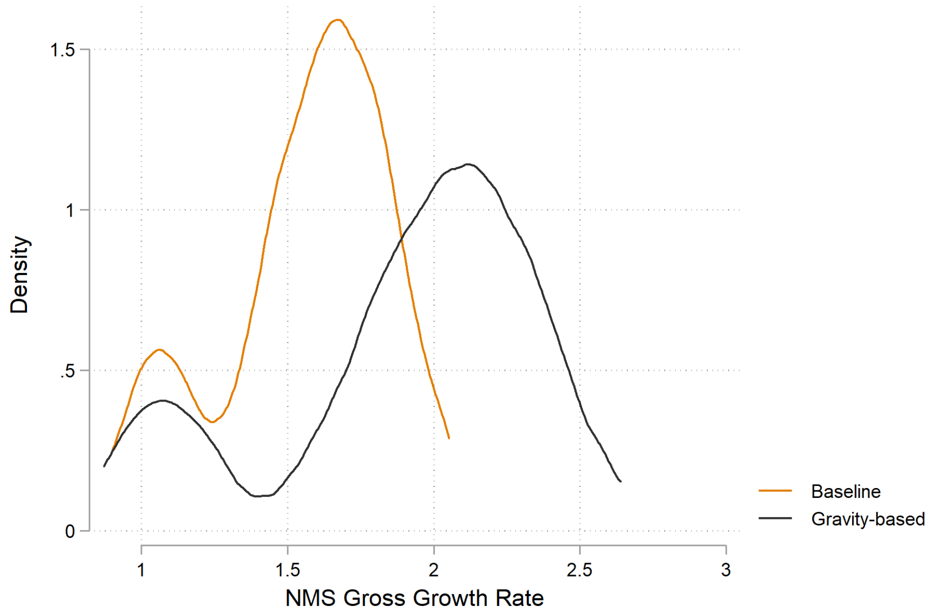
Table 1 presents the main moments of the distributions of gross growth rates for different variables for the case of iceberg trade cost reductions obtained using the blocking estimator (baseline) and empirical gravity model with three-way fixed effects. The average changes in real consumption are very similar (a simple t-test cannot reject the null hypothesis that the difference is equal to zero for the two welfare vectors). This, however, is not surprising, given the magnitudes of the changes in welfare: they are negligible for a vast majority of countries in the sample. Similarly, we cannot reject the null hypothesis of no difference between the means of vectors of normalized market shares for all countries (panel two of Table 1).

The averages for all countries, however, are hiding some important differences between the two model which matter for individual countries. For example, there is a considerable difference for Myanmar, with 18.3% increase in real consumption predicted by the baseline model, and 27.7% increase predicted by the gravity-based model. Moreover, panel three of Table 1 shows that the normalized market shares of RCEP countries trading with each other are very different for the two estimates: the mean increase in normalized market shares for the baseline model is 56.24%, while it is almost double of that when using gravity-based trade cost estimate (90.87% increase). Figure 1 clearly demonstrates the large differences in the two distributions (t-statistics for the difference in means is -23.89). Table 2 also shows the percentage increases in average normalized market shares for RCEP members for the baseline and gravity based estimates. Gravity-based estimate predicts average shares which are 61.56% larger on average than the baseline model averages. Thus, if we were to use gravity estimates in the general equilibrium exercise we would substantially overestimate the trade reallocation for the RCEP countries.

▲ Table 1: Descriptive statistics for the distributions of gross growth rates of real consumption, and normalized market shares, following the trade cost shock in the long run, for baseline and gravity-based estimates.

Statistic	Baseline	Gravity-based
Welfare (real consumption)		
Mean	0.9995	1.0007
Std	0.0233	0.0323
Min	0.9173	0.9175
Max	1.1834	1.2772
NMS of all countries		
Mean	1.1958	1.1984
Std	0.6866	0.6950
Min	0.1474	0.1468
Max	8.0558	8.0811
NMS of RCEP with RCEP		
Mean	1.5624	1.9087
Std	0.2827	0.4421
Min	0.9010	0.8717
Max	2.0531	2.6393
NMS of others with others		
Mean	1.2137	1.1984
Std	0.7009	0.6950
Min	0.1474	0.1468
Max	8.0558	8.0811

Note: The values are calculated using the model presented in Section 6 for different values of iceberg trade cost reductions. The values correspond to different statistics in the distributions of gross growth rates of different variables. The top panel is the mean, the standard deviation, the minimum and the maximum values of the growth rates for welfare (real consumption) for all country pairs. The second panel presents the statistics for the distribution of the growth rates of normalized market shares for all countries. The third panel presents the statistics for the distribution of the growth rates of normalized market shares of RCEP members trading with each other. Finally, the last panel presents the statistics for the distribution of the growth rates of normalized market shares of pairs outside of RCEP trading with each other.



▲ Figure 1: The distribution of gross growth rates of normalized market shares for RCEP countries' mutual trade, for baseline and gravity-based estimates.

▲ Table 2: Average percentage increase in normalized market shares for RCEP members in trade with each other, following the trade cost shock in the long run, for baseline and gravity-based estimates.

Country	Baseline	Gravity-based
Australia	61.08	94.08
China	48.33	85.86
Indonesia	57.30	94.64
Japan	70.95	109.93
Cambodia	75.82	122.85
Korea	45.40	76.99
Myanmar	5.91	8.16
Malaysia	35.92	66.78
New Zealand	72.61	109.49
Philippines	67.60	105.17
Thailand	60.70	96.40
Vietnam	73.29	120.04