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Essays in International Trade: Estimating the Impact of Economic Preferentialism

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Preface

This thesis was written while I was a PhD student at the KOF Swiss Economic Institute of the Swiss Federal Institute of Technology (ETH Zurich). I am indebted to my advisor, Prof. Dr. Peter H. Egger. In addition to the opportunity of working on my dissertation, he provided academic guidance of utmost quality, as well as a role model of professionalism and character. His constant support of any endeavour that would enhance academic strength allowed me to attend doctoral training modules, conferences and workshops. I am honoured to have been a doctoral student at Peter's chair. My colleagues here have provided a challenging and fruitful work environment, for which I am thanking them (in alphabetical order): Simon Bosenberg, Sophia Ding, Katharina Erhardt, Pinar Kaynak, Andrea Lassman, Michaela Kesina, Nicole Loumeau, Gerard Masllorens Fuentes, Susie Rao, Nora Strecker, Benedikt Zoller-Rydzek. Special thanks go to Sergey Nigai, who apart from colleague, was a consistently good friend, providing an example of modesty, clear-mindedness and a great economist.

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Filip Tarlea, September 2018

Contents

List of Figures

List of Tables

Summary of the thesis

In an era of a widening and deepening international integration, a precise understanding of factors shaping trade patterns is crucial for economists and policymakers alike. The international economics literature has carefully documented and reached consensus on a host of factors that determine countries to import and export. The most important driver of trade is the economic size, thus larger and more highly populated countries trade more. Additionally it has been shown that countries are more likely to trade the more similar their partners. Having a common language, common religion, even sharing a history together such as being part of the same empire, has a positive effect on bilateral trade. Not negligibly, geography plays a big role, with countries close to each other, or even sharing a border, trading more than distant ones. Noticeably however, governments are left with little room for maneuver in influencing the direction and magnitude of trade. The one instrument at hand is the signing of bilateral or multilateral so-called preferential trade agreements (PTAs). This thesis takes up the challenge of improving the estimation of the effect that such agreements have historically had on international trade.

The first paper analyzes the consequences of ignoring the multi-indexed structure with cross-sectional and panel-data gravity models of bilateral trade for inference. We estimate log-linear and generalized-linear gravity equations of bilateral trade. Ignoring multi-way clustering in the data at hand leads to misleading inference

regarding the relevance of preferential trade-agreement memberships of different kinds.

The second paper of the thesis remains in the realm of estimating the impact of economic preferentialism on trade. Including bilateral investment treaties (BITs) and double taxation treaties (DTTs) among instruments available to governments for enhancing or deterring international transactions, in addition to PTAs, it addresses the econometric problem of endogeneity. In particular this paper documents the poor level of unbalancedness in the probability of a country pair being in a PTA between country pairs with and without a PTA, which makes difference-in-means estimators biased. It then proposes a solution, employing a remedy of this bias through entropy balancing, which demonstrates that there is an upward bias of PEIA effects on trade flows from lack of covariate balancing. Finally we quantify the bias for partial as well as general equilibrium effects.

The third paper takes one step back in time from the moment when preferential trade agreements are enforced. For the first time in the international economics literature, this paper tries to isolate the impact of lengthy negotiations on trade, by including the duration thereof as a potential determinant. It does so by relying on a dose response-function estimator, permitting continuous treatment and many non-treated units, and documents a negative relation between bilateral export growth and the length of negotiations. With PTA negotiation duration ranging between 316 and 5125 days, we find that on average prolonging negotiations by 16 months undermines trade growth by 13 percentage points.

The fourth paper provides an empirical assessment of the effects of preferential trade liberalization by way of trade agreement membership on the stock prices of the universe of listed firms in Datastream for the period 1988-2014. The paper documents that stock prices appear to systematically increase in firm size after trade deals are announced or become public - well in advance of actual tariff reductions. Moreover, the firm-size effect also appears to vary systematically with planned or expected tariff reductions in the context of trade agreements. Expected tariff reductions tend to raise stock prices of the majority of firms in liberalizing sectors, while undermining the stock price for a few of the very large firms.

Zusammenfassung der Dissertation

In Zeiten stets zunehmender internationalen Integration ist ein genaues Verständnis über die Faktoren welche die Handelsmuster prägen von entscheidender Bedeutung. Das gilt gleichermassen für ökonomen als auch für politische Entscheidungsträger. Die Wirtschaftsliteratur hat eine Vielzahl von Faktoren, die den Import und Export von Ländern bestimmen, sorgfältig dokumentiert. Die Haupterkenntnis aus dieser Literatur ist, dass die wirtschaftliche Grösse eines Landes der wichtigste Handelsfaktor ist. Das heisst, dass grössere und bevölkerungsreichere Länder mehr Handel treiben. Es wurde darüber hinaus bewiesen, dass je ähnlicher zwei Länder sind, desto höher die Wahrscheinlichkeit, dass sie Handel miteinander treiben. Eine gemeinsame Sprache, Religion, und sogar eine gemeinsame Geschichte, wie zum Beispiel ein Teil desselben Imperiums gewesen zu sein, wirken sich positiv auf den bilateralen Handel aus. Nicht zu vernachlässigen ist, dass Geografie eine grosse Rolle spielt. Länder, die nahe beieinander liegen oder sogar eine Grenze teilen, treiben mehr Handel als entfernte Länder. Bemerkenswerterweise bleibt den Regierungen jedoch wenig Spielraum, um die Richtung und das Ausmass des Handels zu beeinflussen. Die Unterzeichnung von bilateralen oder multilateralen Präferenzhandelsabkommen (PTAs) ist ein der wenigen Instrumenten, das dies ermöglicht. Diese Dissertation nimmt die Herausforderung an, die Einschätzung der Auswirkungen solcher Abkommen auf den internationalen Handel zu verbessern.

Kapitel 1 analysiert die Konsequenzen der Nichtberücksichtigung der multi-indexierten Struktur mit Querschnitts-und Panel-Daten-Gravitätsmodellen des bilateralen Handels auf eine genaue Inferenz. Wir schätzen logarithmische und generalisierte lineare Gravitationsgleichungen des bilateralen Handels. Die Nichtberücksichtigung von Multi-Way-Clustering in den vorliegenden Daten führt zu irreführenden Rückschlüssen. Es kann zu einer falschen Einschätzung der Relevanz von präferenziellen Mitgliedschaftsabkommen unterschiedlicher Art führen.

Kapitel 2 dieser Dissertation beschäftigt sich ebenfalls mit der Schätzung der Auswirkungen des wirtschaftlichen Präferenzsystems auf den Handel. Das Papier verwendet bilaterale Investitionsabkommen (BITs) und Doppelbesteuerungsabkommen (DBA), zusätzlich zu den PTAs, als Instrumente, die den Regierungenzur Verfügung stehen, um internationale Transaktionen zu fördern oder behindern, um das ökonometrische Problem der Endogenität zu analysieren .Insbesondere wird in diesem Artikel dokumentiert, dass das niedrige Niveau der Unsausgewogenheit in den Wahrscheinlichkeiten, dass ein Länderpaar in einer PTA zwischen Länderpaaren mit und ohne PTA liegt, , dazu führt, dass die Difference-in-means-Schätzer verzerrt sind.. Das Paper schlägt eine Lösung vor, die eine Korrektur dieser Verzerrung durch Entropieausgleich vorsieht, was zeigt, dass die PEIA-Effekte auf die Handelsströme aufgrund fehlender Ausgleich der Kovariaten nach oben gerichtet sind. Schliesslich quantifizieren wir die Verzerrung sowohl für partielle als auch für allgemeine Gleichgewichtseffekte.

Kapitel 3 befasst sich mit dem Zeitpunkt, in dem die Präferenzhandelsabkommen durchgesetzt werden. Zum ersten Mal in der internationalen Wirtschaftsliteratur versucht dieser Aufsatz, die Auswirkungen langwieriger Verhandlungen auf den Handel zu isolieren, indem er deren Dauer als potentielle Determinante einbezieht.

Das Paper stützt sich dabei auf einen dose-response.functionSchätzer, der eine kontinuierliche Behandlung und viele nicht behandelte Einheiten erlaubt. Es dokumentiert einen negativen Zusammenhang zwischen dem bilateralen Exportwachstum und der Länge der Verhandlungen. Bei einer PTA-Verhandlungsdauer zwischen 316 und 5125 Tagen ist festzustellen, dass eine Verlängerung der Verhandlungen um durchschnittlich 16 Monate das Handelswachstum um 13 Prozentpunkte schwächt.

Kapitel 4 liefert eine empirische Analyse der Auswirkungen der Präferenzhandelsliberalisierung durch die Mitgliedschaft in Handelsabkommen auf die Aktienkurse der börsennotierten Unternehmen in Datastream für den Zeitraum 1988-2014. Das Papier dokumentiert, dass die Aktienkurse mit der Unternehmensgrösse systematisch nach der Bekanntgabe von Handelsgeschäften steigen - deutlich vor den tatsächlichen Zollsenkungen. Darüber hinaus scheint sich der Effekt der Unternehmensgrösse auch systematisch mit geplanten oder erwarteten Zollsenkungen im Rahmen von Handelsabkommen zu variieren. Erwartete Zollsenkungen erhöhen tendenziell die Aktienkurse der meisten Unternehmen in liberalisierten Sektoren, während der Aktienkurs für einige wenige sehr grosse Unternehmen geschwächt wird.

Introduction

In an era of a widening and deepening international integration, a precise understanding of factors shaping trade patterns is crucial for economists and policymakers alike. The international economics literature has carefully documented and reached consensus on a host of factors that determine countries to import and export. The most important driver of trade is the economic size, thus larger and more highly populated countries trade more. Additionally it has been shown that countries are more likely to trade the more similar their partners. Having a common language, common religion, even sharing a history together such as being part of the same empire, has a positive effect on bilateral trade. Not negligibly, geography plays a big role, with countries close to each other, or even sharing a border, trading more than distant ones. Noticeably however, governments are left with little room for maneuver in influencing the direction and magnitude of trade. The one instrument at hand is the signing of bilateral or multilateral so-called preferential trade agreements (PTAs). This thesis takes up the challenge of improving the estimation of the effect that such agreements have historically had on international trade.

The first paper analyzes the consequences of ignoring the multi-indexed structure with cross-sectional and panel-data gravity models of bilateral trade for inference. We estimate log-linear and generalized-linear gravity equations of bilateral trade. Ignoring multi-way clustering in the data at hand leads to misleading inference regarding the relevance of preferential trade-agreement memberships of different kinds.

The second paper takes a wider approach on economic preferentialism and in addition to PTAs, it accounts for bilateral investment treaties (BITs) and double taxation treaties (DTTs) among instruments available to governments for enhancing or deterring international transactions. The dominant paradigm of the estimation of causal partial effects of such preferential economic integration agreements (PEIAs) on trade costs and trade flows is to rely on a selection on observables, with propensity-score matching being the leading example. Conditional on some compact metric (the score) of observable joint determinants of PEIAs and trade flows, the causal partial effect of PEIAs on trade is obtained from a simple mean comparison of trade flows between members and non-members. A key prerequisite for this approach to obtain consistent estimates is that the score is balanced: similarity of country pairs in the score (the propensity of PEIA membership) means similarity in each and everyone of the observables. A violation of this assumption may lead to biased estimates of the effects, mis-ascribing effects of differences in individual observables to PEIA membership. We employ a remedy of this bias through entropy balancing, demonstrate that there is an upward bias of PEIA effects on trade flows from lack of covariate balancing, and quantify the bias for partial as well as general equilibrium effects.

The third paper takes one step back in time from the moment when preferential trade agreements are enforced. PTAs are signed between two or more countries following the conclusion of the negotiation process. The duration of this process varies considerably across existing trade agreements and ranges between 316 and 5125 days. This paper presents the consequences of the length of the negotiation process on trade growth. The contribution of this paper to the literature is threefold.

Firstly, it includes as a determinant of trade a new variable that captures negotiations duration for the largest number of PTAs possible, covering all such events from January 1988 until October 2014. This unveils yet another previously ignored feature of PTAs (as trade driver) that leaves results based on a dichotomous PTA status in question. Secondly, this paper evaluates for the first time the *anticipation* effects of a PTA, concentrating solely on the negotiation period. Lastly, methodologically, this paper introduces for the first time in the international economics literature a dose response-function approach permitting continuous treatment and many non-treated units, and documents a negative relation between bilateral export growth and the length of negotiations.

The fourth paper provides an empirical assessment of the effects of preferential trade liberalization by way of trade agreement membership on the stock prices of the universe of listed firms in Datastream for the period 1988-2014. The paper documents that stock prices appear to systematically increase in firm size after trade deals are announced or become public - well in advance of actual tariff reductions. Moreover, the firm-size effect also appears to vary systematically with planned or expected tariff reductions in the context of trade agreements. Expected tariff reductions tend to raise stock prices of the majority of firms in liberalizing sectors, while undermining the stock price for a few of the very large firms. On the other hand, there is an import competition effect which depresses profits of those import-competing firms. The export opportunity effect materialized through lowering of tariffs is however dominant.

Chapter 1

Multi-way clustering estimation of standard errors in gravity models[1]

1.1 Introduction

This paper is concerned with estimating standard errors on (or confidence bounds around) point estimates of trade-cost variables in structural gravity models of bilateral trade. Recent work suggests that the conditional expectations in gravity models involve cross-sectional dependence across exporters and across importers in the cross section due to the joint determination of all countries' factor or goods prices and incomes in multi-country general equilibrium (see Anderson and van Wincoop, 2003). Moreover, strong inertia in bilateral trade relationships suggests that there is also interdependence over time (see Egger and Pfaffermayr, 2012). And a sluggish adjustment of prices and incomes in general equilibrium to trade-cost shocks induces adjustment costs of countries' and country pairs' determinants of trade flows

[1]This chapter is based on Egger and Tarlea (2015)

over time. Hence, it appears natural to permit stochastic (random) shocks to bilateral trade to feature a pattern of interdependence in several dimensions (exporters, importers, and time).

Using data on cross-border trade for all possible pairs among 51 large economies and 51 years and an approach of multi-way clustering along the lines of Cameron, Gelbach, and Miller (2011), we illustrate that ignoring interdependence of the disturbances in multiple dimensions leads to drastically biased standard errors of the coefficients of interest in structural gravity models of international trade. Accounting for multi-level clustering has large effects on the standard errors of trade-cost variables, no matter of whether country and time or country-pair and time fixed effects are included in structural gravity models. Hence, multi-level clustering matters a lot for the confidence bounds around ad-valorem-equivalent trade costs as well as of comparative-static effects of changes to those trade costs.

The next section outlines the structure of the model, potential blocks in the variance-covariance matrix of the model disturbances, and the multi-way clustering approach. Section 3 summarizes empirical findings based on a panel data-set of 51 countries and 51 years, using four indicator variables to capture different aspects of preferential goods-trade liberalization.

1.2 The model

Let us use indices i, j, and t to refer to exporting country, importing country, and time (year). Moreover, let X_{ijt} denote aggregate bilateral exports of goods from i to j at time t. Denoting the size of the labor force, per-worker income, the consumer-price index, and GDP in country i at time t by L_{it}, W_{it}, P_{it}, and Y_{it}, respectively, a structural, monopolistic-competition increasing-returns-to-scale-based gravity equation as employed in Behrens, Ertur, and Koch (2012) or Bergstrand, Egger, and Larch (2013) may be written as

$$\tilde{X}_{ijt} \equiv \underbrace{\frac{X_{ijt}}{L_{it}Y_{jt}}\left(\frac{P_{jt}}{W_{it}}\right)^{\kappa}}_{\equiv\tilde{\tilde{X}}_{ijt}} = D_{ijt}U_{ijt}, \tag{1.1}$$

where κ is the partial elasticity of trade with respect to trade costs,[2] and D_{ijt} is the ad-valorem-equivalent observable trade-cost factors. D_{ijt} is typically modelled as the exponentiated value of a linear index – a scalar product of a vector of observable trade-cost measures in logs and an unknown parameter vector on them, $D_{ijt} = \exp(\sum_{k=1}^{K} Z_{k,ijt}\alpha_k)$ – and U_{ijt} is a scalar capturing the influence of unobservables. We specify D_{ijt} as to contain four binary preferential trade-agreement (PTA) indicators – customs unions (CUA), economic integration associations (EIA), free-trade agreements (FTA), and partial scope agreements (PSA) – and several other, time-invariant factors such as log bilateral distance, contiguity, common official language, common spoken language, and three colonial relationship indicators (common colonizer after 1945, historical colonial relationship, and colonial relationship after 1945). The binary PTA variables are unity in case a PTA of the respective kind

[2] Following Caliendo and Parro (2015) and using τ_{ijt} to denote bilateral applied tariffs on imports by country j from i at time t, the partial trade elasticity can be obtained from

$$\kappa = \ln\left(\frac{X_{ijt}X_{hit}X_{jht}}{X_{jit}X_{iht}X_{hjt}}\right) / \ln\left(\frac{\tau_{ijt}\tau_{hit}\tau_{jht}}{\tau_{jit}\tau_{iht}\tau_{hjt}}\right) + error_{ijht}. \tag{1.2}$$

prevails between i and j at time t and zero else. We generically specify

$$
\begin{aligned}
U_{ijt} &\equiv U_{ijt}^{FE} U_{ijt}^{RES} & (1.3)\\
U_{ijt}^{FE} &\equiv \exp(a_i + b_j + c_t) \text{ versus } \exp(d_{ij} + c_t) & (1.4)\\
U_{ijt}^{RES} &\equiv \exp(\eta_{ij}) \text{ versus } \exp(\epsilon_{it} + \zeta_{jt} + \eta_{ij} + \varepsilon_{ijt}), & (1.5)
\end{aligned}
$$

where $\{a_i, b_j, c_t\}$ are fixed main effects and $\{d_{ij}, c_t\}$ are fixed pair and time effects which have a (structural) interpretation of unobservable trade costs, U_{ijt}^{FE} captures these costs together, and U_{ijt}^{RES} includes random error components.[3] Clearly, conditioning on fixed effects d_{ij} in U_{ijt}^{FE} means that D_{ijt} cannot include any time-invariant observable trade costs due to collinearity with d_{ij}.

This structure permits accounting for correlation of unobservables within clusters associated with dimensions $\{i, j, t, ijt, jt, ij\}$. Hence, the shocks to a specific country, country- pair, or country-pair-time unit may be drawn from a different distribution than the others (heteroskedasticity) and, when indexed identically as another one, be correlated with each other (clustering). Reasons for the variation and correlation of shocks in the dimensions listed in (1.5) may stem from unobservable trade costs (or technology and endowments) which correspond to these dimensions. As said before, general equilibrium effects establish a correlation of incomes and consumer-price indices (Y_{jt}, P_{jt}) in (1.1) across importers at a given time as well as a correlation of factor prices (W_{it}) in (1.1) across exporters at a given time. A sluggish adjustment of economic variables such as incomes and prices to a new equilibrium after a shock

[3] In a variation of the specification in (1.5) we consider a dyadic error-components structure where η_{ij} is permitted to be symmetric so that $\eta_{ij} = \eta_{ji}$ so that there are bigger clusters at the country-pair level than under the assumption of asymmetric pair-specific components.

in trade costs leads to serial correlation within exporters, importers, and country-pairs. Hence, there are arguments for all considered dimensions of clustering from a theoretical point of view. The joint consideration of these clustering levels is important, when making inference about the parameters in the trade-cost function, α_k.

Following Cameron, Gelbach, and Miller (2011) broadly and using $\circ$ to denote the Hadamard (elementwise) matrix product, an estimate of the multi-cluster-robust variance-covariance matrix about the vector α with typical element α_k can be written as

$$\hat{\mathbf{V}}_\alpha = (\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}'(\hat{\mathbf{U}}\hat{\mathbf{U}}' \circ \mathbf{S})\mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}, \tag{1.6}$$

where $\mathbf{Z}$ is an $n \times K$ matrix of trade costs (in logs) with typical element $Z_{k,ijt}$, $\hat{\mathbf{U}}$ is a stacked vector of estimated (potentially fixed-effects) residuals with typical element U_{ijt}^{RES}, and, using superscripts E, M, and T to refer to the exporter, importer, and time dimensions (or main effects) in the data, the $n \times n$ matrix $\mathbf{S}$ is composed of individual, dimension-specific indicator matrices as follows:

$$\mathbf{S} \;\; = \;\; \mathbf{S}^E + \mathbf{S}^M + \mathbf{S}^T - (\mathbf{S}^{EM} + \mathbf{S}^{ET} + \mathbf{S}^{MT}) + \mathbf{S}^{EMT}, \tag{1.7}$$

where double- and triple-superscripted matrices are obtained as Hadamard (elementwise) products of the respective single-superscripted matrices.

The cluster structure in (1.7) involves a large number of clusters in each dimension and, according to the results in MacKinnon and Webb (2014), the estimator of the variance-covariance matrix should involve a small bias.[4]

[4]Notice that the cluster sizes are equal in the data at hand.

1.3 Regression analysis

In this section, we use balanced data on bilateral exports among the 51 largest economies on the globe[5] over the 51 years between 1960 and 2010 from the United Nations' Comtrade database, population (as a proxy for labor force) and GDP from the World Bank's World Development Indicators, goods-price indices from the International Comparison Program as contained in the Penn World Tables, and data on trade-cost variables from the Centre d'Études Prospectives et d'Informations Internationales. This permits constructing $\tilde{X}_{ijt}$ in (1.1) and specifying the log-linear index underlying the trade-cost function in D_{ijt} on the right-hand side of (1.1).

We estimate a log-linear model (by OLS) and two generalized-linear model versions – one Gaussian and one Poisson pseudo-maximum likelihood (PML). In Table 1 we assume all error components to be random, while we assume fixed pair-specific effects in Table 2.[6] In each table, we provide parameter point estimates and standard errors based on four types of clustering: Huber-White-type (no clusters), pair-wise one-way clustering, multi-way clustering assuming asymmetric pair-wise components, and multi-way clustering assuming symmetric pair-wise (dyadic) components.

Clearly, being based on sandwich estimation of the variance-covariance matrix of the parameters, clustering does not affect the point estimates. However there is

[5] Algeria, Argentina, Australia, Austria, Belgium, Brazil, Canada, Chile, China, Colombia, Czech Republic, Denmark, Egypt, Finland, France, Germany, Greece, Hong Kong, Hungary, India, Indonesia, Iran, Ireland, Israel, Italy, Japan, Korea, Malaysia, Mexico, Netherlands, New Zealand, Nigeria, Norway, Pakistan, Philippines, Poland, Portugal, Romania, Russia, Saudi Arabia, Singapore, South Africa, Spain, Sweden, Switzerland, Thailand, Turkey, United Arab Emirates, United Kingdom, United States, and Venezuela.

[6] Using time-invariant fixed effects instead of parameterized time-invariant trade costs leads to bigger standard errors on trade-cost estimates which include the fixed effects. Hence, in the interest of estimating comparative static effects more precisely, using fixed country-pair effects is inferior.

a substantial difference between the standard errors across the considered variance-covariance estimates within a regression type (log-linear versus generalized-linear form). For brevity, we focus on no clustering versus single-level clustering within country-pairs versus multi-way clustering as described above. Moreover, we largely focus on the discussion of the Poisson PML-GLM, since it has the advantage of accommodating error heteroskedasticity and lacks the potential endogeneity problem of log-linear OLS due to mis-specification of the exponential functional form (see Santos Silva and Tenreyro, 2006).

All models in Table 1 obtain positive parameters on the FTA and PSA indicators but negative ones on CUA and EIA membership. This may be an outcome of the sample composition (containing only large economies over a long time span) and of the inclusion of country (exporter as well as importer) and time fixed effects.[7] With clustering of any kind, CUA and EIA effects become insignificant, while they are not with Huber-White standard errors. Using multi-way rather than pair clustering appears particularly important with PSA membership under Poisson PML-GLM. There is a drastic difference between the standard errors about coefficients of binary trade-cost indicator variables. Apart from PSA, consider the variation in standard errors on the three colonial and the two language indicator variables employed. Overall, not only the precision of partial effects of trade-cost factors but also of their effects in general equilibrium are highly sensitive to the correlation structure of the variance-covariance matrix considered. With multi-indexed data on bilateral trade, it appears natural to consider a richer pattern of error correlation than is widely assumed.

[7]There is no need to include those fixed effects from a structural modelling perspective, since (2) accounts for country-time general-equilibrium effects. However, statistical tests on these fixed effects suggest that they should not be ignored. From a theoretical point of view, these effects are unobservable trade-cost effects.

Table 1.1: Main fixed effects: importer, exporter, and time

Dep. Var*: $\tilde{X}_{ijt}$	**Log-linear OLS**				**Gaussian GLM - PML**				**Poisson GLM - PML**			
	Cluster dimensions				Cluster dimensions				Cluster dimensions			
Indep. Vars.:	None[§]	One[†]	Three[‡]	Three[⋆]	None[§]	One[†]	Three[‡]	Three[⋆]	None[§]	One[†]	Three[‡]	Three[⋆]
Customs Union Agreement (CUA)	-0.0259	-0.0259	-0.0259	-0.0259	-0.2656	-0.2656	-0.2656	-0.2656	-0.0712	-0.0712	-0.0712	-0.0712
	(0.0372)	(0.0688)	(0.1135)	(0.1136)	(0.0680)	(0.1665)	(0.1845)	(0.1846)	(0.0368)	(0.0702)	(0.1065)	(0.1037)
Economic Integration Agreement (EIA)	-0.0318	-0.0318	-0.0318	-0.0318	-0.0798	-0.0798	-0.0798	-0.0798	-0.0764	-0.0764	-0.0764	-0.0764
	(0.0180)	(0.0422)	(0.1086)	(0.1059)	(0.0709)	(0.1786)	(0.1682)	(0.1734)	(0.0239)	(0.0551)	(0.1158)	(0.1138)
Free Trade Agreement (FTA)	0.2777	0.2777	0.2777	0.2777	0.3486	0.3486	0.3486	0.3486	0.4385	0.4385	0.4385	0.4385
	(0.0191)	(0.0440)	(0.1166)	(0.1173)	(0.0958)	(0.2307)	(0.2386)	(0.2384)	(0.0319)	(0.0793)	(0.1601)	(0.1604)
Partial Scope Agreement (PSA)	0.3297	0.3297	0.3297	0.3297	0.4999	0.4999	0.4999	0.4999	0.2158	0.2158	0.2158	0.2158
	(0.0321)	(0.1215)	(0.1132)	(0.1214)	(0.1408)	(0.4736)	(0.4443)	(0.4476)	(0.0383)	(1.0900)	(0.1190)	(0.1168)
Log distance (km)	-1.2123	-1.2123	-1.2123	-1.2123	-1.1644	-1.1644	-1.1644	-1.1644	-1.0406	-1.0406	-1.0406	-1.0406
	(0.0095)	(0.0321)	(0.0762)	(0.0767)	(0.1004)	(0.2284)	(0.2491)	(0.2529)	(0.0148)	(0.0417)	(0.0804)	(0.0816)
Contiguity	0.1235	0.1235	0.1235	0.1235	0.5262	0.5262	0.5262	0.5262	0.3217	0.3217	0.3217	0.3217
	(0.0342)	(0.1200)	(0.1443)	(0.1542)	(0.1531)	(0.4416)	(0.6019)	(0.6041)	(0.0381)	(0.1275)	(0.2352)	(0.2372)
Common language (official)	0.2002	0.2002	0.2002	0.2002	-0.2941	-0.2941	-0.2941	-0.2941	-0.2468	-0.2468	-0.2468	-0.2468
	(0.0372)	(0.1310)	(0.1710)	(0.1780)	(0.1376)	(0.3270)	(0.3039)	(0.3083)	(0.0600)	(0.1218)	(0.1330)	(0.1310)
Common language (>9% of population)	0.2347	0.2347	0.2347	0.2347	-0.1917	-0.1917	-0.1917	-0.1917	0.1272	0.1272	0.1272	0.1272
	(0.0347)	(0.1215)	(0.1132)	(0.1214)	(0.1934)	(0.4736)	(0.4443)	(0.4476)	(0.0603)	(0.11676)	(0.1190)	(0.1168)
Colony (ever)	0.3889	0.3889	0.3889	0.3889	0.0320	0.0320	0.0320	0.0320	0.3228	0.3228	0.3228	0.3228
	(0.0286)	(0.1034)	(0.1179)	(0.1247)	(0.1132)	(0.2954)	(0.3262)	(0.3289)	(0.0420)	(0.1334)	(0.1724)	(0.1729)
Colony (common colonizer after 1945)	0.0883	0.0883	0.0883	0.0883	0.0498	0.0498	0.0498	0.0498	0.1492	0.1492	0.1492	0.1492
	(0.0605)	(0.1637)	(0.2068)	(0.2175)	(0.1441)	(0.3439)	(0.3688)	(0.3777)	(0.0574)	(0.1575)	(0.2294)	(0.2292)
Colony (in colonial relationship after 1945)	0.0252	0.0252	0.0252	0.0252	0.8695	0.8695	0.8695	0.8695	0.3765	0.3765	0.3765	0.3765
	(0.0525)	(0.1957)	(0.2410)	(0.2549)	(0.2455)	(0.6671)	(0.6711)	(0.6748)	(0.0627)	(0.2146)	(0.2771)	(0.2782)
Rho	0.8144				0.2440				0.2341			
Observations	41,661				43,050				43,050			

In general, we suppress estimates of the fixed effects in this table. $*$ $\tilde{X}_{ijt}$ *are normalized nominal exports from* i *to* j *at* t *as in equation* (1.1)*, where the* κ *is otained as explained in equation* (2)*.* [§] *Huber-White-type robust standard errors without clustering.* [†] *Standard errors are clustered at (and may be correlated over time within) country pairs.* [‡] *Standard errors are clustered at (and may be correlated within) base groups (importer, exporter, and year), as well as every combination of the three.* [⋆] *same as* [‡]*, except for country-pairs being dyadic (symmetric for* ij *and* ji*).*

In Table 2, we report results for models that include fixed country-pair effects. Let us again focus on a discussion of Poisson GLM-PML results. All estimates suggest that CUAs and PSAs affect bilateral trade positively and are statistically significant at conventional levels while the effect of FTAs is (positive but) statistically insignificant. However, multi-pay clustering suggests that EIAs induce a negative, statistically significant effect while other standard-error estimates suggest an insignificant effect. Hence, irrespective of whether time-invariant trade costs are parameterized in terms of observable variables or pair-specific fixed effects, the consideration of multi-level clustering is quantitatively important for the precision of trade-cost estimates, and it will be important for the precision of quantified comparative-static effects thereof in general equilibrium.

Table 1.2: Country-pair and time fixed effects

Dep. Var*: $\tilde{X}_{ijt}$	**Log-linear OLS**				**Gaussian GLM - PML**				**Poisson GLM - PML**			
	Cluster dimensions				Cluster dimensions				Cluster dimensions			
Indep. Vars.:	None§	One†	Three‡	Three*	None§	One†	Three‡	Three*	None§	One†	Three‡	Three*
Customs Union Agreement (CUA)	0.1897	0.1897	0.1897	0.1897	-0.1473	-0.1473	-0.1473	-0.1473	0.0437	0.0437	0.0437	0.0437
	(0.0260)	(0.0491)	(0.0989)	(0.1010)	(0.0588)	(0.1496)	(0.1689)	(0.1634)	(0.0236)	(0.0607)	(0.0095)	(0.0101)
Economic Integration Agreement (EIA)	-0.2890	-0.2890	-0.2890	-0.2890	-0.0259	-0.0259	-0.0259	-0.0259	-0.0614	-0.0614	-0.0614	-0.0614
	(0.0161)	(0.0305)	(0.1189)	(0.1201)	(0.0535)	(0.1200)	(0.0958)	(0.1044)	(0.0209)	(0.0512)	(0.0126)	(0.0139)
Free Trade Agreement (FTA)	0.1744	0.1744	0.1744	0.1744	-0.1240	-0.1240	-0.1240	-0.1240	0.0119	0.0119	0.0119	0.0119
	(0.0163)	(0.0323)	(0.1320)	(0.1326)	(0.0559)	(0.0691)	(0.0888)	(0.0859)	(0.0237)	(0.0435)	(0.0087)	(0.0097)
Partial Scope Agreement (PSA)	2.1057	2.1057	2.1057	2.1057	0.5004	0.5004	0.5004	0.5004	0.3806	0.3806	0.3806	0.3806
	(0.1512)	(0.3364)	(0.4784)	(0.5148)	(0.0876)	(0.1222)	(0.0874)	(0.0912)	(0.1089)	(0.0865)	(0.0308)	(0.0263)
Rho	0.9596				0.2603				0.3304			
Observations	41,661				43,050				43,050			

In general, we suppress estimates of the fixed effects in this table. $*$ $\tilde{X}_{ijt}$ *are normalized nominal exports from* i *to* j *at* t *as in equation* (1.1), *where the* κ *is otained as explained in equation* (2). § *Huber-White-type robust standard errors without clustering.* $\dagger$ *Standard errors are clustered at (and may be correlated over time within) country pairs.* $\ddagger$ *Standard errors are clustered at (and may be correlated within) base groups (importer, exporter, and year), as well as every combination of the three.* $\star$ *same as* $\ddagger$, *except for country-pairs being dyadic (symmetric for* ij *and* ji*).*

Chapter 2

Comparing Apples to Apples:

Estimating Consistent Partial Effects of Preferential Economic Integration Agreements[1]

2.1 Introduction

Obtaining valid estimates of the partial (or direct) effects of the membership in preferential economic integration agreements (PEIAs) on bilateral trade flows is the primary object of interest in empirical work on trade policy (see, e.g., Ghosh and Yamarik, 2004; Carrére, 2006; Baier and Bergstrand, 2007, 2009; Egger, Egger, and Greenaway, 2008; Chang and Lee, 2011), and using consistent estimates in quantitative models is vital to obtain reasonable estimates of general equilibrium (or total) economic responses to PEIA membership (see Egger and Larch, 2011; Egger, Larch, Staub, and Winkelmann, 2011; Caliendo and Parro, 2015). The econometric

[1]This chapter is based on Egger and Tarlea (2017)

problem with this task is that PEIAs are meant to stimulate trade,[2] and, according to economic theory, concluding PEIAs has greater benefits for natural trading partners than otherwise (see Frankel, Stein, and Wei, 1996; Baier and Bergstrand, 2004; Egger and Larch, 2008).[3] Hence, PEIA membership is not randomly assigned to country pairs, which is confirmed by a glance on the frequency of such agreements across types of countries and country pairs in terms of observable characteristics capturing country size, per-capita income, geography, and remoteness. An influential paper by Baier and Bergstrand (2004) illustrated that the fundamental drivers of trade flows alone explain a lion's share in the variation of binary preferential trade-agreement (PTA) indicators as one form of PEIAs. Egger and Wamser (2013) demonstrate that this is the case also for other forms of PEIAs such as bilateral investment treaties (BITs) or double-tax treaties (DTTs), all of which exist for the sake of stimulating trade flows (see the quotes in Egger and Wamser, 2013, to substantiate this argument).

The theoretical arguments put forward in earlier work suggest that it will be hard if not impossible to find fundamentals which directly determine PEIAs while influencing trade flows exclusively through PEIA membership. Econometrically speaking, this means that it will be virtually impossible to find identifying instruments for PEIAs for which exclusion restrictions are met in trade-flow regressions, as would be required for instrumental-variables regression. Consequently, the leading assumption in empirical work geared towards estimating PEIA effects on economic outcome is

[2]On a broader scheme, PEIAs do not only include preferential trade agreements, but even preferential investment agreements and double-taxation treaties explicitly aim at stimulating trade beyond investment.

[3]While this literature abstains from strategic aspects of trade policy as, e.g., reviewed and assessed in Bagwell and Staiger (2011, 2017), the covariates determining "natural trading partners" (such as size, endowments, and remoteness) reflect also determinants of trade-policy under strategic interaction. Hence, customary empirical reduced-form specifications of PEIA membership may cover even more ground than suggested by the theoretical literature on natural trading partners.

one of the so-called *selection on observables.* According to this framework, it should be possible – guided by economic theory as, e.g., in Baier and Bergstrand (2004) – to (i) identify all *joint determinants* of PEIA membership and trade flows, and (ii) to condition in some way on them so that the remainder (conditional) variation in PEIA membership and trade flows reveals the causal effect of the former on the latter.

While earlier work used a log-linear-index regression approach for the identification of partial PEIA treatment effects conditional on observables (see Aitken, 1973; Soloaga and Winters, 2001), more recent work resorted to nonparametric estimation techniques (see, e.g., Egger, Egger, and Greenaway, 2008; Baier and Bergstrand, 2009). The latter – with the most prominent example in related applied work being propensity-core matching (see Rosenbaum and Rubin, 1983) – relies on the idea of obtaining a compact metric which captures the joint fundamentals behind PEIA membership and trade flows, and which permits determining similar country pairs which more or less solely differ in terms of PEIA membership for identification of the treatment effect. A prerequisite for this approach is that similarity in terms of the compact, scalar-valued score metric (for the propensity of PEIA membership) is not an artifact which could flow from largely different individual observable fundamentals whose differences between PEIA members and non-members are eliminated through aggregation into the score. If that were the case, one would compare PEIA-member *apples* to -non-member *oranges.* Econometrically, this problem is referred to as a lack of *balancing* of the observables, whereby members and non-members of PEIAs with similar-valued propensity scores of being a PEIA member would have very different moments in the distribution of at least some of the observables the score is based on. Lack of balancing may lead to a bias in the estimates of partial PEIA effects on outcomes such as bilateral trade flows.

The goal of this paper is to illustrate that the usually-employed observables lack balancing in the data, to enforce balancing by a relatively modern method, namely entropy balancing (see Hainmüller, 2012), and to compare PEIA-effect estimates on trade flows (partial effects) and welfare (general-equilibrium effects) based on customary methods with the proposed estimates.

In a large panel of 434,895 observations for all years in 1961-2008 and (at least) three types of PEIAs (PTA-, BIT-, DTT-membership, and all combinations thereof – distinguishing further between PTAs of different type in some of the analysis) the paper demonstrates that the lack of balancing of the covariates in a customary nonparametric selection-on-observables approach leads to substantially upward-biased PEIA effects. For instance, the partial impact of a membership in an average PTA alone is estimated to be almost 7 percentage points lower with enforced covariate balancing than without it. The partial effect of a membership in an average BIT alone is estimated to be almost 15 percentage points lower with enforced covariate balancing than without it, and the bias in the estimated partial impact of a membership in an average DTT alone is estimated at a similar magnitude. We illustrate that the quantitative importance of proper conditioning on the covariates in nonparametric selection-on-observables approaches relative to not doing so is of a similar magnitude as the difference between simple (biased) ordinary-least-squares (OLS) estimates and simple (and also biased) selection-on-observables estimates of partial PEIA treatment effects as relied upon in earlier work. Hence, conventional approaches towards estimating causal PEIA effects tend to overestimate the effects of PEIAs to a nontrivial extent, and the associated bias materializes also in largely biased quantitative effects in general equilibrium.

The remainder of the paper is organized as follows. The subsequent section briefly portrays nonparametric selection-on-observables estimates of PEIA treatment effects as weighting estimators and distinguishes between covariate-balancing-enforcing and -not-enforcing approaches. Section 3 introduces the specification of the vector of observables and the underlying data considered, summarizes estimates of the comparison (propensity) score, illustrates the degree of lack of balancing of the covariates, and contrasts the estimates of partial effects on bilateral exports as well as the general-equilibrium effects on real consumption of PEIAs between the covariate-balancing-enforcing and -not-enforcing methods. The last section concludes with a brief summary of the main findings.

2.2 Causal partial PEIA-effects estimation as weighting regression

Customary conditioning-on-observables approaches towards estimating causal partial PEIA effects can all be portrayed as variants of weighting regressions (see Wooldridge, 2007; Huber, 2014). With this in mind, the simple linear conditioning approach in the form of ordinary least squares of log bilateral exports on one or more PEIA indicator variables and a linear function of observable control variables conforms to an approach with identical weights for each observation. Also matching on the propensity score (of PEIA membership) can be represented as a weighting regression.[4] However, neither linear regression nor matching on the propensity score

[4]For instance, STATA users could see this when comparing treatment-effect estimates of PEIAs from a propensity-score matching command such as "psmatch2" with weighting-regression-based ones where outcome (bilateral exports) is regressed on PEIA indicators with the "regress" command using weights ("_weight") as determined by the "psmatch2" routine. With nearest-neighbor matching, these weights would be based on the frequency of control (e.g., non-PEIA-member)

ensure that the distributions of *all* the joint determinants (the observables) are the same between PEIA members and non-members. But only then the two groups would be fully comparable, and we could speak of a quasi-randomization of PEIA membership. The reason is that the linear index with OLS or the nonlinear index with propensity-score matching may take on similar values when the individual covariates are quite different in a few or many dimensions of the observables, and similarity is an artifact of the linear or nonlinear aggregation.[5] In order to avoid this problem, there are weighting approaches which are capable of ensuring comparability in a defined set of moments of the distributions of the observables. One such weighting approach is entropy balancing, which is based on optimally-chosen weights as a function of the distributions of observables for the treated and the untreated (see Hainmüller, 2012; Imai and Ratkovic, 2014; Zubizarreta, 2015). In what follows, we will briefly describe this approach in comparison to inverse-probability-weighting regression (which is equivalent to propensity-score matching).

2.2.1 Notation

Let us use $\Phi_{ijs}^{\theta 0}$ to denote the propensity score of exporter i and importer j to be members of a PEIA of type θ rather than being a member in no PEIA whatsoever at time s. Denoting the binary indicator for specific PEIA memberships by $T_{ijs}^{\theta 0}$ and

country pairs being matched onto treated (certain PEIA-member) country pairs. Such a weighting regression would simply discard the "incomparable" untreated country pairs and take into account multiple matches for one control pair to treated country pairs. The weighting regression for radius matching works similarly, and the weights based on kernel matching would be based on kernel rather than frequency weights. See Lechner (2001) for formal insights on this with multiple treatments as is required with more than one type of PEIAs.

[5] In empirical work, it is sometimes tested whether the individual averages (means, first moments of the distribution) of the observables are the same between the treated or not. However, even that is not sufficient, as also higher moments of the covariate distributions ought to be the same between the treated and the control observations (see Huber, 2011).

the specific realization of θ for ijs by Θ_{ijs}. $T_{ijs}^{\theta 0}$ is unity in case that i and j have a PEIA of type θ at time s and zero else, and denoting the vector of observables determining membership for observations in state θ or 0 by $H_{ijs}^{\theta 0}$, the propensity score is defined as the conditional probability of having treatment θ relative to 0 on the joint determinants of outcome and $T_{ijs}^{\theta 0}$, $H_{ijs}^{\theta 0}$:

$$\widehat{\Phi}_{ijs}^{\theta 0} = P(T^{\theta 0} = 1 | H_{ijs}^{\theta 0}). \tag{2.1}$$

In this paper, we consider at least three types of binary PEIA indicators, so that there there are at least $2^3 = 8$ possible combinations of agreement types, one being no PEIA agreement of any kind in place which will serve as the general *control* or comparison state in this paper.[6] The remaining seven combinations are

$$\Theta_{ijs} = \theta \in \begin{cases} PTA & \text{if } PTA_{ijs} = 1, BIT_{ijs} = 0, DTT_{ijs} = 0 \\ BIT & \text{if } PTA_{ijs} = 0, BIT_{ijs} = 1, DTT_{ijs} = 0 \\ DTT & \text{if } PTA_{ijs} = 0, BIT_{ijs} = 0, DTT_{ijs} = 1 \\ PTA\&BIT & \text{if } PTA_{ijs} = 1, BIT_{ijs} = 1, DTT_{ijs} = 0 \\ PTA\&DTT & \text{if } PTA_{ijs} = 1, BIT_{ijs} = 0, DTT_{ijs} = 1 \\ BIT\&DTT & \text{if } PTA_{ijs} = 0, BIT_{ijs} = 1, DTT_{ijs} = 1 \\ PTA\&BIT\&DTT & \text{if } PTA_{ijs} = 1, BIT_{ijs} = 1, DTT_{ijs} = 1 \end{cases} \tag{2.2}$$

each of which we will refer to as one form (or status) of *treatment*.[7]

[6] In theory, any other state than complete PEIA nonmembership could serve as a comparison. However, for the sake of simplicity, and given the extent amount of control units in that state, we choose it as a natural reference point in this paper. It should be borne in mind that what we do in the comparison state is to switch off all PEIA types at the same time.

[7] In general, with M treatment types there are 2^M possible combinations. In any case, we always compare $2^M - 1$ states to the all-zero state for the sake of brevity. Clearly, it would be possible to

We use N^θ and N^0 for the *number* of treatment-θ and 0-control observations, respectively, and we define the number of observations in the two states as $N^{\theta 0} = N^\theta + N^0$. Moreover, we refer to the *sets* of observations corresponding to these numbers by $\mathscr{N}^\theta$, $\mathscr{N}^0$, and $\mathscr{N}^{\theta 0} = \mathscr{N}^\theta \bigcup \mathscr{N}^0$, respectively.

2.2.2 Assumptions behind consistent estimates of partial treatment effects of the treated

The goal of selection-on-observables approaches – upon choice of untreated units (here, indicated by super-script 0) being the single reference group – is to estimate the *average treatment effect* of PEIA membership from a comparison of outcome Y^θ of the units $\{ijs\} \in \mathscr{N}^\theta$ with observable characteristics $H^{\theta 0}_{ijs}$ to outcome Y^0 of the units $\{ijs\} \in \mathscr{N}^0$. In order to not misattribute the average difference in Y^θ and Y^0 to differences in $H^{\theta 0}$, the vector of propensity-scores $\Phi^{\theta 0}$ – which is a compact vector representation of the matrix $H^{\theta 0}$ – is used for weighting in some way, depending on the required similarity between treated and control units in terms of $\Phi^{\theta 0}$ specified by the researcher. With matching, the similarity of treated and control units in terms of $\Phi^{\theta 0}$ is specified by way of k-nearest neighbor matching, radius matching, or kernel matching. The matching-function type determines the nature of the weights based on the propensity scores. However, this approach only leads to consistent estimates of the average treatment effect under the following assumptions.

Balancing of the observables $H^{\theta 0}_{ijs}$ with regard to $\widehat{\Phi}^{\theta 0}_{ijs}$:

The first key condition is the aforementioned balancing of the covariates. Informally, balancing makes sure that the propensity score is a meaningful metric of comparison.

compute – depending on treatment-group size – up to $2^{2M} - 2^M$ average treatment effects of the treated and up to $(2^{2M-1} - 2^{M-1})$ average treatment effects. See Lechner (2001) on this point.

Notice that this is the case only, if, for units $\{ijs\}$ and $\{i'j's'\}$, any similarity in $\Phi_{ijs}^{\theta 0}$ and $\Phi_{i'j's'}^{\theta 0}$ means a pairwise similarity for *all* columns in $H_{ijs}^{\theta 0}$ and $H_{i'j's'}^{\theta 0}$, respectively. Otherwise, the similarity of $\Phi_{ijs}^{\theta 0}$ and $\Phi_{i'j's'}^{\theta 0}$ would be an artifact, and estimating the average treatment effect from comparison groups of treated and untreated units with similar propensity score will eventually be inconsistent, as the effect might reflect differences in the distribution of the elements in $H^{\theta 0}$ among the country pairs in the sets $\mathscr{N}^{\theta}$ and $\mathscr{N}^{0}$.

However, the assumption about balancing of the covariates is testable regarding the first as well as higher moments (see Huber, 2011), and remedies against a lack of balancing are available. One such remedy is entropy-balancing weighting regression (see Hainmüller, 2012; Imai and Ratkovic, 2014; Zubizarreta, 2015), where covariate balancing can be enforced for several moments.

Unconfoundedness or conditional mean independence of treatment:

The second key condition is (weak) unconfoundedness. It means that, for the same unit $\{ijs\}$ and conditional on the observable determinants of its treatment status, $H_{ijs}^{\theta 0}$, the hypothetical outcomes Y_{ijs}^{θ} and Y_{ijs}^{0} for that unit are independent of the treatment θ. Formally, using $Y_{ijs}^{\theta 0}$ for all units with either treatment θ or control units (i.e., an element of the vector $Y^{\theta 0} = (Y^{\theta \prime}, Y^{0 \prime})'$):

$$Y_{ijs}^{\theta 0} \perp T_{ijs} | H_{ijs}^{\theta 0}. \tag{2.3}$$

The latter means that $H_{ijs}^{\theta 0}$ needs to include all joint determinants of outcome $Y_{ijs}^{\theta 0}$ and treatment T_{ijs}^{θ} (and, hence, $\Phi_{ijs}^{\theta 0}$).

Consistency of the functional form of $\widehat{\Phi}_{ijs}^{\theta 0}$:

An inconsistency of the propensity-score estimates could flow from an erroneous assumption about the functional form of the distribution for the mapping of $H_{ijs}^{\theta 0}$ into $\Phi_{ijs}^{\theta 0}$. Then, maximum-likelihood estimates of the scores $\Phi_{ijs}^{\theta 0}$ would be biased and inconsistent.[8] However, an inconsistency of the treatment effects would only emerge for comparison estimators where the ranking of units $\{ijs\}$ would be inconsistent, and the degree of similarity of two comparison units would be largely affected (i.e., similarity in $\widehat{\Phi}_{ijs}^{\theta 0}$ would not mean similarity of all columns in $H_{ijs}^{\theta 0}$ through the choice of an inadequate aggregator).

2.2.3 Treatment-effect estimation through weighting regression

In this subsection, we present two alternative types of weighting regression for a framework of selection on observables, each of which involves a specific first stage to determine the weights and an outcome which corresponds to weighted least squares. In each case, the second stage is run on a subset of the data where either $\Theta_{ijs} = \theta$ or $\Theta_{ijs} = 0$, namely $\mathscr{N}^{\theta 0}$. For convenience, let us also introduce a subvector of the joint determinants of PEIA membership and bilateral trade, H_{ijs}, which we refer to as Z_{ijs}. We introduce this subvector in order to be able to indicate that one may (and we do) condition on some (or even all) of the covariates in H_{ijs} – in particular, the country-time fixed effects – *after* conditioning on the propensity score. For instance, such a procedure is suggested by Blundell and Costa Dias (2009) to reduce the bias from a lack of covariate balancing ex post.

[8] Leading estimators and functional forms in applied work are probit (normality) and logit. In estimating the propensity of PEIA membership, probit is used in most applications (e.g., see Baier and Bergstrand, 2004, 2009; Egger, Egger, and Greenaway, 2008; Egger and Wamser, 2013). In principal, $\Phi_{ijs}^{\theta 0}$ could be estimated by any parametric or nonparametric consistent estimator.

Inverse-probability weighting (IPW) regression

With inverse-probability weighting, the first stage of the approach is concerned with estimating the response (or PEIA-membership) probabilities, $\Phi^{\theta 0}_{ijs}$. Response probabilities are typically estimated parametrically by a maximum-likelihood estimator for nonlinear probability models. We follow Wooldridge (1995) by generally estimating $\Phi^{\theta 0}_{ijs}$ by probit and year by year for each treatment θ and set $\mathscr{N}^{\theta 0}$ (ensuring that all propensities including the one for zero treatment add up properly to unity). The (inverse) estimated propensities from the first stage are the weights used in the second stage of the IPW regression framework. Formally, the (conditional) propensity score is obtained by conditioning on H^{θ}_{ijs} from eq. (2.1).

In the second stage, we may condition on the covariates Z_{ijs}, which we suspect to be unbalanced or have an impact of their own on bilateral exports, Y_{ijs}. Doing so, we obtain parameters from two weighting expressions, namely for the treated as

$$\min_{\alpha^{\theta},\beta^{\theta}} \sum_{ijs \in \mathscr{N}^{\theta}} \frac{(Y_{ijs} - \alpha^{\theta} - Z_{ijs}\beta^{\theta})^2}{\widehat{\Phi}^{\theta 0}_{ijs}}, \tag{2.4}$$

and for the controls as

$$\min_{\alpha^{0},\beta^{0}} \sum_{ijs \in \mathscr{N}^{0}} \frac{(Y_{ijs} - \alpha^{0} - Z_{ijs}\beta^{0})^2}{1 - \widehat{\Phi}^{\theta 0}_{ijs}}. \tag{2.5}$$

Using the notation $\overline{Z}^{\theta 0}$ and $\overline{Z}^{\theta}$ to denote row vectors containing the average values of Z_{ijs} in the subsets of the observations in $\mathscr{N}^{\theta 0}$ and $\mathscr{N}^{\theta}$, respectively, the average treatment effect (ATE) and the average treatment effect of the actually treated (ATT) of a type-θ PEIA membership in comparison to no treatment at all with inverse-probability weighting are then defined as

$$\widehat{ATE}^{\theta 0}_{ipwra} = (\widehat{\alpha}^{\theta} - \widehat{\alpha}^{0}) + \overline{Z}^{\theta 0}(\widehat{\beta}^{\theta} - \widehat{\beta}^{0}) \tag{2.6}$$

and

$$\widehat{ATT}^{\theta 0}_{ipwra} = (\widehat{\alpha}^{\theta} - \widehat{\alpha}^{0}) + \overline{Z}^{\theta}(\widehat{\beta}^{\theta} - \widehat{\beta}^{0}), \tag{2.7}$$

respectively.

One major advantage of this framework is its simplicity. However, a fundamental drawback, as indicated above, is that it assumes covariate balancing in all columns of H_{ijs}, even beyond just the first moment (i.e., the averages of observables), which in reality is often rejected by the data, especially, if H_{ijs} contains many columns. As argued before, a lack of covariate balancing may lead to a bias of the second-stage weighting-regression estimates and, hence, of $\widehat{ATE}^{\theta 0}_{ipwra}$ and $\widehat{ATT}^{\theta 0}_{ipwra}$.

Covariate-balance-enforcing (CBE) weighting regression

The second approach to PEIA-treatment-effect estimation by weighting regression differs from the one in the previous subsection only with respect to the first stage. In contrast to a propensity-score model, the weights here are obtained by following the approach in Hainmüller (2012) and Hainmüller and Xu (2013).

Define an ex-ante unknown weight for unit $\{ijs\} \in \mathscr{N}^0$, e_{ijs}, a base weight, q_{ijs}, and a distance metric between the two as

$$f(e_{ijs}) = e_{ijs} log(e_{ijs}/q_{ijs}). \tag{2.8}$$

Then, the weights e_{ijs} are chosen so as to minimize the loss function

$$\min_{e_{ijs}} F(e) = \sum_{\{ijs \in \mathscr{N}^0\}} f(e_{ijs}) \tag{2.9}$$

subject to the set of balance constraints

$$\sum_{\{ijs \in \mathscr{N}^0\}} e_{ijs} c_{r,ijs}(H_{ijs}) = m_r^\theta \tag{2.10}$$

where $c_{r,ijs}(H_{ijs})$ is the moment function for the covariates H_{ijs} among the control observations $\{ijs\} \in \mathscr{N}^0$ up to moment r and the r-th moment of the (base-unweighted) treated observations $\{ijs\} \in \mathscr{N}^\theta$, m_r^θ, and subject to the normalization constraints

$$e_{ijs} \geq 0, \text{ and } \sum_{\{ijs \in \mathscr{N}^0\}} e_{ijs} = 1. \tag{2.11}$$

Let us denote the solution for e_{ijs} of this procedure by $\widehat{e}_{ijs}^{\theta 0}$. Using this estimate, we may formulate the entropy-balancing counterparts to equations (2.4) and (2.5) for the treated and control observations as

$$\min_{\alpha^\theta, \beta^\theta} \sum_{ijs \in \mathscr{N}^\theta} \frac{(Y_{ijs} - \alpha^\theta - Z_{ijs}\beta^\theta)^2}{\widehat{e}_{ijs}^{\theta 0}} \tag{2.12}$$

and

$$\min_{\alpha^0, \beta^0} \sum_{ijs \in \mathscr{N}^0} \frac{(Y_{ijs} - \alpha^0 - Z_{ijs}\beta^0)^2}{\widehat{e}_{ijs}^{\theta 0}} \tag{2.13}$$

respectively. The corresponding treatment effects are then defined as

$$ATE_{balance}^{\theta 0} \quad = \quad ATT_{balance}^{\theta 0} = \widehat{\alpha}^\theta - \widehat{\alpha}^0. \tag{2.14}$$

There are two notable and desirable differences between eq. (2.14) and eqs. (2.6) and (2.7). First, any difference in the targeted moments of H_{ijs} (and Z_{ijs}) is eliminated by the minimization in eq. (2.9) subject to the aforementioned constraints. Hence, differences in the targeted moments of H_{ijs} between the treated and control units do not influence average treatment effects. Second, for the same reason, the average treatment effect (ATE) is identical to the average treatment effect of the treated (ATT) which is not automatically the case with inverse-probability weighting, and which is typically not the case in earlier nonparametric work on PEIA effects on economic outcome.

2.3 Estimating partial PEIA effects with covariate-balancing-enforcing versus non-enforcing methods

This section is organized in two subsections. The first one provides an overview of the panel data we use in the empirical analysis of PEIA effects on trade flows and real consumption in this paper. In the second subsection we summarize the empirical findings based on covariate-balancing-enforcing versus non-enforcing methods. The latter will present estimates of both partial (direct) effects on trade flows which do not account for adjustments of prices in general equilibrium and total (general-equilibrium) effects on welfare which do account of such adjustments.

2.3.1 Data

We cover annual data of bilateral nominal exports and their determinants over the time interval of 1961-2008. The trade data are collected from the United Nations' (UN) Comtrade Database, and we restrict our interest to positive exports.[9] The

[9]Notice that the vector of observables, H_{ijs}, includes exporter-time and importer-time fixed effects, and so does the vector Z_{ijs}. Conditional on these fixed effects, it turns out that there is no significant bias associated with sample selection into positive exports (see Wooldridge, 1995, for the consistency of fixed-effects estimates in the case of specific forms of sample selection.)

main regressor of interest to this paper is a country pair's membership status in one of the considered PEIA typss – PTA, BIT, DTT, or combinations thereof – which we may refer to as the treatment indicator, referred to as $T_{ijs}^{\theta 0}$ above. Information on PTA membership is collected from World Trade Organization's (WTO) website. The variable PTA_{ijs} is unity, if trade between countries i and j is covered by a PTA in year s (we will even discern between deeper and more shallow types of PTAs in some of the subsequent analysis). Information on DTT and BIT membership is collected from the website of the United Nations Conference on Trade and Development (UNCTAD). BIT_{ijs} and DTT_{ijs} are unity if investment between countries i and j is covered by a BIT or a DTT, respectively, in year s.

We use other covariates based on variables contained in the World Bank's World Development Indicators (WDI) and the Centre d'Études Prospectives et d'Informations Internationales' (CEPII) gravity data-set. We summarize the acronyms, provide a brief description, and report the source of all variables except the fixed country-time effects in Table 2.1. We suppress subscripts in the table but would like to note that the outcome, the treatment indicators, and the first-stage-only covariates in H_{ijs} (i.e., those in H_{ijs} but not in Z_{ijs}) all vary in the three dimensions i, j, and s, whereas the geographical, cultural, and historical variables included in both stages (i.e., the members of Z_{ijs}) vary only in dimensions i and j but not s.

Among the first-stage-only covariates, we have four regressors in the spirit of Baier and Bergstrand (2004). These measure total economic size between countries i and j in year s in terms of their log total real GDP ($RGDP_{ijs}$), the dissimilarity in economic size between countries i and j in year s in terms of the absolute difference of their real GDP ($DRGDP_{ijs}$), the difference in capital-labor ratios of countries i and j in year s approximated by the absolute difference in log real per-capita income (DKL_{ijs}), and the difference in capital-labor ratios of countries i and j together in year s and the rest of the world (in our case, 165 countries) approximated by the absolute difference in log real per-capita income ($DROWKL_{ijs}$). The results in Baier and Bergstrand (2004) suggest that such measures successfully co-determine the propensity of signing at least a PTA, and the findings in Egger and Wamser (2013) suggest that similar conclusions apply for BITs and DTTs. Such regressors are historically known to determine the volume of trade and, in particular, of intra-industry trade (see Helpman, 1987). Hence, the result in Baier and Bergstrand (2004) and others suggest that PEIAs are concluded primarily among *natural trading partners*, i.e., countries which would trade and direct invest a lot with each other in the absence of political barriers.

The other covariates, which are included in the first- as well as the second-stage models are also standard in empirical research, and they capture geographical, cultural, historical, and political factors determining bilateral trade as well as PEIA membership. All of those covariates are time-invariant. The geographical factors include log bilateral distance ($DIST_{ij}$) and common land border ($BORDER_{ij}$). The cultural variables include common official language ($LANG1_{ij}$) and common ethnological language when spoken by a sufficiently large base ($LANG1_{ij}$). The historical variables are four indicators which capture colonial relationships of some form ($COLONY1_{ij}$, $COLONY2_{ij}$, $COLONY3_{ij}$, $COLONY4_{ij}$). Finally, we measure some special political relationship between entities i and j, if they did or currently do belong to the same country ($SMCTRY_{ij}$). Again, for of these variables it is documented that they successfully co-explain bilateral trade flows as well as PEIA membership.[10]

[10]Some earlier empirical work included third-country averages of explanatory variables (see Baltagi, Egger, Pfaffermayr, 2008; Egger and Larch, 2008; Baldwin and Jaimovich, 2012). In the present setting and data, these are highly collinear with the included fixed effects. Therefore, we omit the respective variables to improve the stability of the nonlinear econometric models.

Table 2.1 provides descriptive statistics for all variables involved, including the dependent variable, log bilateral exports, Y_{ijs}. We suppress a detailed discussion of those statistics but would like to single out the following observations. First, the overall data-set which is used for estimation contains 434,895 observations (made up of 158 exporters, 160 importers, and 50 years). Second, in the data the most frequent PEIAs are ones with a PTA or a DTT only (PTA, DTT). This state prevails for 10% of the observations each. The least frequent states are having a PTA combined with a BIT or a DTT ($PTA\&BIT$, $PTA\&DTT$) which prevails for about 2% of the observations each. In any case, Table 2.10 reports on the absolute number of the treatment states behind the percentages in Table 2.9 and suggests that all states occur frequently enough so that we should be able to estimate average treatment effects of PEIAs – if there are any – with sufficient precision given the large number of observations.

Tables 2.9 and 2.10 reflect the base case of treatment configurations considered in this paper. However, recent research by Horn, Mavroidis, and Sapir (2010) and Dür and Elsnig (2014) suggests that it could be useful to distinguish between different types of PTAs, as these PTAs contain a host of different provisions. The customary approach to this issue in empirical and quantitative work is to distinguish among important types of PTAs (see Horn, Mavroidis, and Sapir, 2010; Dür and Elsig, 2014; Egger and Nigai, 2015). In this paper, we distinguish between four of them along the following lines, using the Design of Trade Agreements (DESTA) database:[11] the first type of PTA is PTA1, a category where most-favored-nation tariffs are abolished for only a limited number of tariff lines and no other important provisions regarding services trade, investments, standards, public procurement, competition,

[11] See http://www.designoftradeagreements.org. and Tables A3 and A4 in the Appendix for the list of PTAs included in the four categories PTA1-PTA4.

Table 2.1: Determinants of PEIAs and trade (description and sources of data)

Variables	Description	Source
	Outcome variable	
Y	Log bilateral exports	UN
	Binary treatment variables	
PTA	Preferential trade agreement only	WTO
BIT	Bilateral investment treaty only	UNCTAD
DTT	Double-taxation treaty only	UNCTAD
PTA&BIT	Combination of the above	
PTA&DTT	Combination of the above	
BIT&DTT	Combination of the above	
PTA&BIT&DTT	Combination of the above	
	1st-stage-only covariates	
RGDP	The sum of two countries' log real GDPs	WDI
DRGDP	The absolute difference of two countries' log real GDPs	WDI
DKL	The absolute difference in the two countries' log real per-capita incomes	WDI
DROWKL	The average absolute difference in log per-capita incomes of two countries with the rest of the world	WDI
	1st- and 2nd-stage covariates	
DIST	The log distance between two countries' economic centers	CEPII
BORDER	Binary common country border	CEPII
LANG1	Binary for common official primary language	CEPII
LANG2	Binary for common language if spoken by at least 9% of the population	CEPII
COLONY1	Binary for colonial relationship (ever)	CEPII
COLONY2	Binary common colonizer post 1945	CEPII
COLONY3	Binary for pair currently in colonial relationship	CEPII
COLONY4	Binary for pair in colonial relationship post 1945	CEPII
SMCTRY	Binary for entities that were or are part of the same country	CEPII

or intellectual property rights are implemented; the second type is PTA2, where tariffs are also abolished for only a limited number of tariff lines but also some provisions in the six aforementioned areas are implemented; the third type of PTA is PTA3 which capture full free-trade areas which do not include provisions in the six areas mentioned before; and the last category is PTA4 which are free-trade areas that also account for provisions as in $PTA2$. While PTAs in category PTA3 are clearly deeper than ones in PTA1, we should not expect a clear-cut ordering regarding the quantitative effects of those agreement types otherwise. The reason is that the average tariff cuts in category PTA2 may be smaller on average than the ones in category PTA1. Moreover, other provisions included in PTAs might represent obstacles to rather than drivers of trade. For the latter reason alone, there is no clear-cut hypothesis regarding the quantitative importance of PTA2 relative to PTA1 or PTA4 relative to PTA3 (and even the other PTA types).

Table 2.2: PTA types in the data

PTA Types	**Total**	**%**
1. No provision (PTA1)	20,707	60.75
2. At least one provision (PTA2)	5,283	15.50
3. Full FTA (PTA3)	4,444	13.04
4. Full FTA + at least one provision (PTA4)	3649	10.71
Total	34,083	

The overall total should add up to 34,870 as per the number of observations with a PTA in Table 2.10. However, the DESTA database covers slightly fewer PTAs than those recorded by WTO.

Table 2.2 provides a summary of the frequency of these PTAs in the data. Of course, doing so leads to 19 treatment states (except for the null state without any PEIA) relative to the 7 states (except for the null state without any PEIA) in Tables 2.9 and 2.10. For this more fine-grained definition of PEIAs we generate Table 2.11

as a counterpart to Table 2.10. We will use both the coarse and the fine-grained set of PEIA definitions in the subsequent analysis.

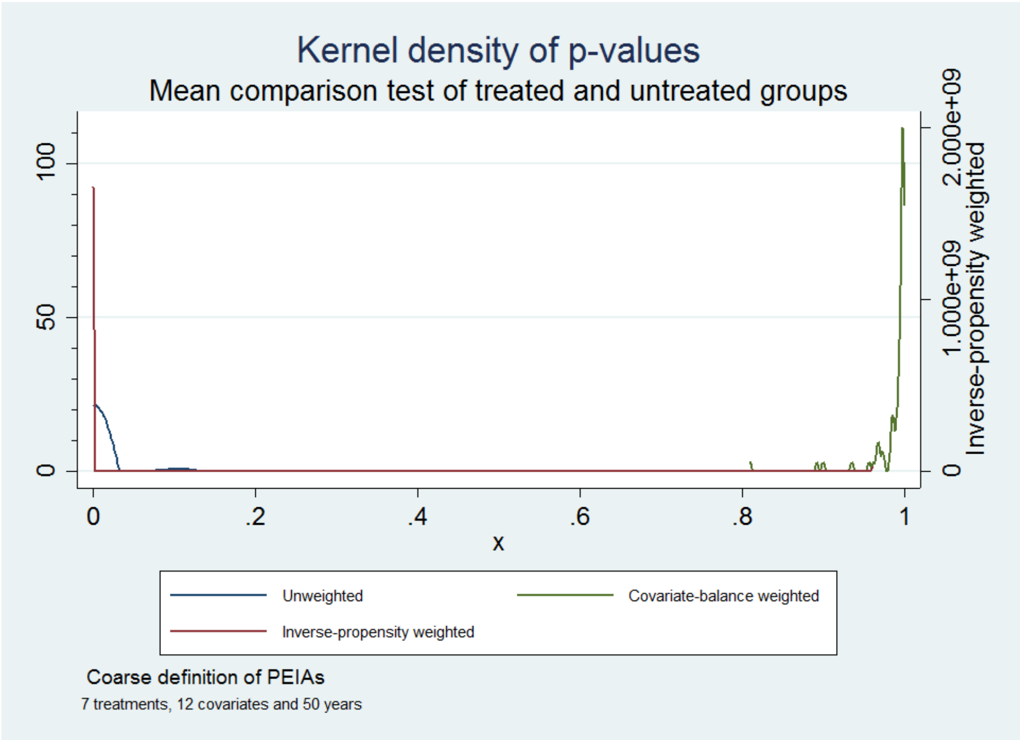
2.3.2 Results and discussion

Notice that with 50 years of data and annual probit estimates, we would have to report an enormous number of parameter estimates for the first-stage models. We suppress those in the main text and relegate the respective presentation to the accompanying online appendix (See Tables A1 and A2 in the Appendix for a summary of PEIA-propensity estimates using the coarse- and the fine-grained definition of PEIAs). In any case, those parameter estimates are of limited interest for two reasons: first, the probit models – as first-stage models in general – do not have a structural interpretation and, second, only the signs of the parameter estimates are interpretable due to the nonlinear model structure. In what follows, we will for that reason focus on partial second-stage and general-equilibrium PEIA-effect estimates and, as a prerequisite, on covariate-balancing tests.

Covariate-balancing tests

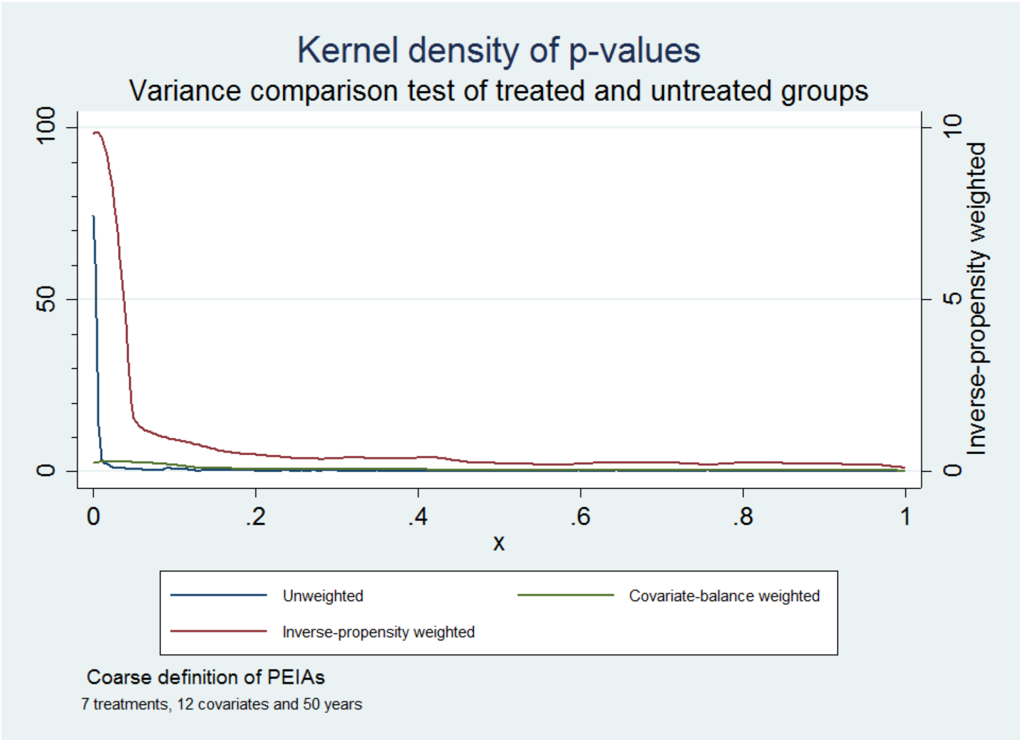
Before turning to effect estimates, let us focus on covariate balancing. Notice that with 13 main covariates (beyond the country-time fixed effects) in Z_{ijs}, 50 years of data, and 7 or 19 treatment states there are $13 \cdot 50 \cdot 7 = 4,550$ or $13 \cdot 50 \cdot 19 = 12,350$, respectively, tests for the equality of, say, the first and the second moment of the variables in H_{ijs} each among the treated and the control observations. It would not be convenient to present the associated results by way of tables, but we present figures of kernel-density plots of p-values of mean- and variance-comparison tests to assess the differences of the first and second moments of the covariates between the treated and the control observations. We generally report three kernel-density loci on p-values for all covariates together per moment: one for the simple OLS model, one for the customary IPW model, and one for the CBE model.

Figure 2.1: Density of p-values of mean-comparison tests with a coarse PEIA definition



The density plots suggest that the mode (peak) of the distributions for both the first and the second moment occurs obviously at the lowest p-value for the OLS model, and it occurs also obviously at a lower level for the IPW model than for the CBE model. In fact, the median of the p-value for the equality-of-means tests between the treated and the control units in Figure 2.1 amounts to 0.997 for the CBE model, while it is numerically zero for both IPW and OLS. The median of the p-value for the equality-of-variances tests between the treated and the control units in Figure 2.2 amounts to 0.266 for the CBE model, and it is 0.001 for IPW

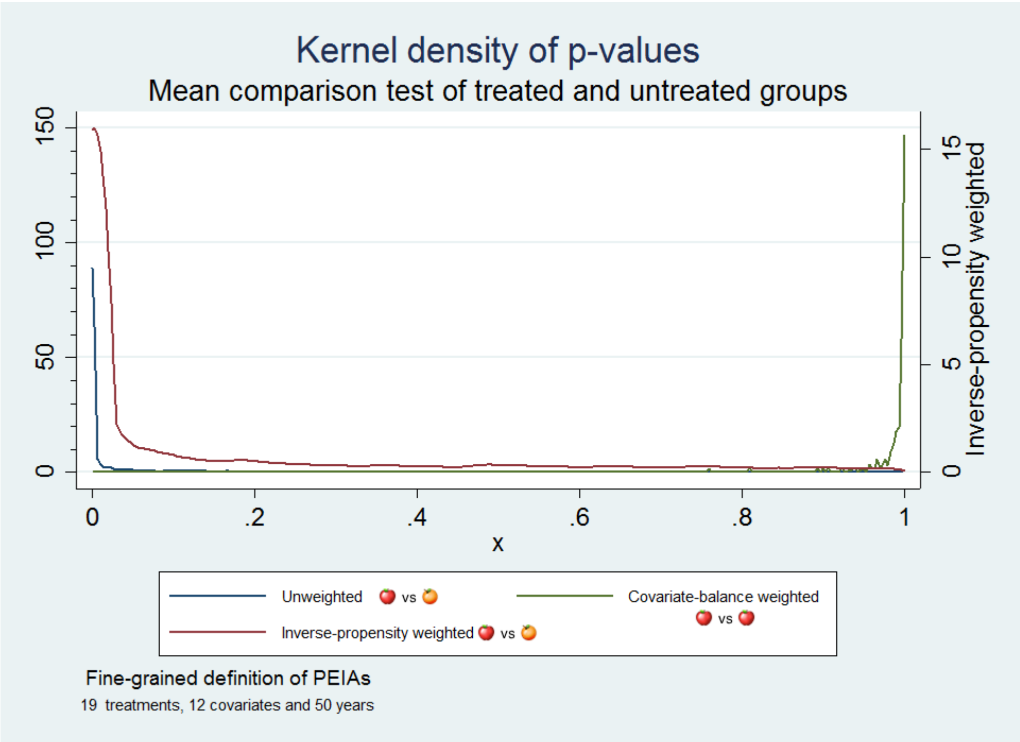
Figure 2.2: Density of p-values of variance-comparison tests with a coarse PEIA definition



and numerically zero for OLS.[12] Hence, the first two moments differ starkly between the treated and the controls before any weighting and even when weighting by the propensity score, and we have a situation where one fundamental assumption for not only IPW regression but also propensity-score matching to obtain consistent partial effects is starkly violated. As a consequence, these estimators should not be expected to produce consistent estimates of the average treatment effects of interest for the average treatment of interest here. The CBE model apparently removes this sources of bias.

[12] All means and medians of p-values from mean- and variance-comparison tests are reported in Table 2.12.

Figure 2.3: Density of p-values of mean-comparison tests with a fine-grained PEIA definition

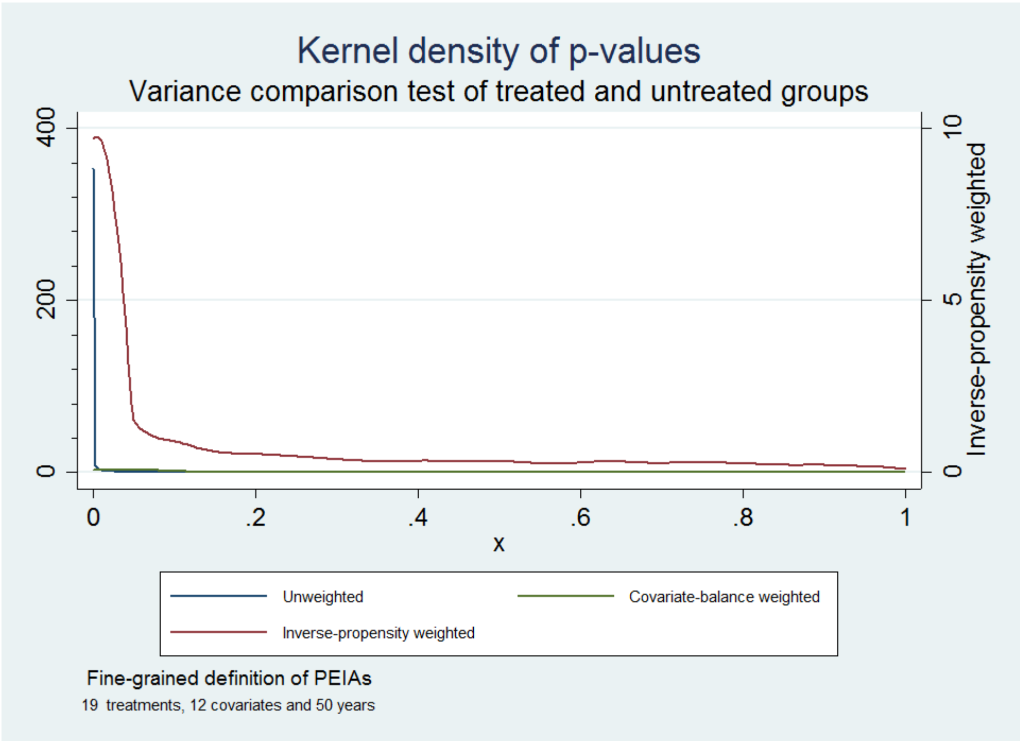


Partial effects of PEIAs on bilateral exports

The tables and text in this sub-sub-section summarize partial effects of PEIA treatments on bilateral exports estimated with the coarse-grained (7 treatment states; Table 2.3) and the fine-grained (19 treatment states; Table 2.4) differentiation of PEIAs.

Tables 2.3 and 2.4 follow the same principal organization. Apart from the column referring to the treatment at stake, the columns labelled OLS, IPW, and CBE pertain to average treatment effects associated with the respective estimators. The numbers in italics reported below the ATE parameters are bootstrapped standard errors. The columns Obs. and Treated refer to numbers of all and respective treated

Figure 2.4: Density of p-values of variance-comparison tests with a fine-grained PEIA definition



observations which estimates are based on, and Treated % expresses Treated in percent of Obs. Clearly, the number of control observations with zero PEIA treatment is always Obs. -Treated=311,974. Since the only difference between Tables 2.3 and 2.4 is that overall PTA treatment in Table 2.3 is split up into four categories in Table 2.4, only those ATE estimates differ between the tables, where any PTA treatment is involved.

It is apparent from the comparison of columns OLS and CBE on the one hand and of columns IPW and CBE on the other hand – especially, when recalling the substantial lack of covariate balancedness behind propensity scores – that inverse-probability weighting is quite problematic in the data. The ranking of the magnitude of the partial effects is quite similar between CBE and OLS on average in both tables across the ATEs estimated, while it is quite different with IPW. The CBE estimates tend to be non-trivially smaller than the OLS estimates in absolute value, while no clear-cut pattern emerges for IPW relative to OLS. Of all the ATEs, four of the signs in Tables 2.3 and 2.4 differ between IPW and OLS, only one of the signs differs between CBE and OLS, and five of the signs differ between IPW and CBE.

Table 2.3: ATE of PEIAs on log bilateral exports: coarse-grained PEIA definition

	OLS	**IPW**	**CBE**	**Obs.**	**Treated**	**Treated %**
PTA	0.8046	1.2617	0.5256	346,844	34,870	5.21
SE	*0.0008*	*0.0576*	*0.0009*			
BIT	0.6776	1.0251	0.4720	329,708	17,734	2.65
SE	*0.0007*	*0.1918*	*0.0008*			
DTT	0.7758	0.1878	0.6374	344,730	32,756	4.89
SE	*0.0006*	*0.0963*	*0.0006*			
PTA&BIT	1.5943	-0.8580	1.1565	318,199	6,225	0.92
SE	*0.0012*	*0.0826*	*0.0017*			
PTA &DTT	1.6625	2.9734	1.3157	317,454	5,480	0.82
SE	*0.0011*	*0.1159*	*0.0012*			
BIT&DTT	1.2782	-1.9000	0.9659	328,796	16,822	2.51
SE	*0.0008*	*0.3452*	*0.0009*			
PTA&BIT&DTT	1.9674	-0.0633	1.2721	321,008	9,034	1.35
SE	*0.0009*	*0.1587*	*0.0017*			

ATE: Partial average treatment effect. OLS: coefficient estimates of a least-squares regression of the dependent variable Y_{ijs} on covariates shown in Table 2.1. IPW: inverse-probability weighted regression. CBE: covariate-balance-enforcing weighted regression. Exporter×year fixed effects and importer×year fixed effects included in both the probit and the outcome equations (with OLS, IPW and CBE). Standard errors are generally obtained through bootstrapping based on 100 draws.

Let us consider the magnitude of the differences between the partial ATE estimates across all lines in Tables 2.3 and 2.4 together. For this, compute the simple (unweighted) average of the absolute differences between the ATEs under OLS versus IPW (which is 1.5249 for the two tables and all ATEs together), OLS versus CBE (which is 0.3504 for the two tables and all ATEs together), and IPW versus CBE (which is 1.5542 for the two tables and all ATEs together). These numbers clearly indicate that the bias induced by the lack of balancing under IPW is actually

Table 2.4: ATE of PEIAs on log bilateral exports: fine-grained PEIA definition

	OLS	**IPW**	**CBE**	**N**	**Treated**	**Treated %**
PTA1	0.5279	0.1882	0.3295	318,729	6757	2.12
SE	*0.0313*	*0.1295*	*0.0124*			
PTA2	0.7318	1.8870	0.0156	313,744	1757	0.56
SE	*0.0570*	*0.3458*	*0.0197*			
PTA3	1.0713	0.7526	0.7643	312,533	563	0.18
SE	*0.0116*	*0.4322*	*0.0224*			
PTA4	0.9346	2.3113	0.2794	337,760	25771	7.63
SE	*0.0190*	*0.2377*	*0.0126*			
BIT	0.6776	1.0251	0.4720	329,708	17738	5.38
SE	*0.0025*	*0.4004*	*0.0026*			
DTT	0.7758	0.1878	0.6374	344,730	32749	9.50
SE	*0.0013*	*0.4169*	*0.0018*			
PTA1&BIT	1.4508	2.1255	1.3789	312,434	469	0.15
SE	*0.0685*	*0.3224*	*0.0281*			
PTA2&BIT	1.2698	4.2840	0.6233	312,140	156	0.05
SE	*0.0413*	*0.6026*	*0.0297*			
PTA3&BIT	1.8255	1.2578	1.5240	312,174	187	0.06
SE	*0.0746*	*0.5943*	*0.0420*			
PTA4&BIT	1.3934	1.0592	0.7687	317,373	5395	1.70
SE	*0.0060*	*0.4932*	*0.0073*			
PTA1&DTT	1.6660	1.5496	1.3065	312,514	531	0.17
SE	*0.0506*	*0.3095*	*0.0444*			
PTA2&DTT	1.0806	0.1346	0.9561	312,254	281	0.09
SE	*0.0090*	*0.3535*	*0.0084*			
PTA3&DTT	1.8550	6.0443	1.6676	312,134	156	0.05
SE	*0.0116*	*0.3650*	*0.0200*			
PTA4&DTT	1.2118	3.1360	-5.1567	316,474	4494	1.42
SE	*0.0250*	*0.3855*	*0.6884*			
BIT&DTT	1.2782	-1.9000	0.9659	328,796	16834	5.12
SE	*0.0025*	*0.9989*	*0.0030*			
PTA1&BIT&DTT	1.9219	0.8238	1.5053	312,358	375	0.12
SE	*0.1121*	*0.7419*	*0.0979*			
PTA2&BIT&DTT	1.9800	1.9170	1.4761	312,397	437	0.14
SE	*0.0486*	*0.4338*	*0.0407*			
PTA3&BIT&DTT	2.1628	0.1289	1.6443	312,157	187	0.06
SE	*0.0179*	*0.3473*	*0.0303*			
PTA4&BIT&DTT	1.8342	1.4772	1.0828	320,018	8032	2.51
SE	*0.0072*	*2.3977*	*0.0121*			

ATE: Partial average treatment effect. OLS: coefficient estimates of a least-squares regression of the dependent variable Y_{ijs} on covariates shown in Table 2.1. IPW: inverse-probability weighted regression. CBE: covariate-balance-enforcing weighted regression. Exporter×year fixed effects and importer×year fixed effects included in both the probit and the outcome equations (with OLS, IPW and CBE). Standard errors are generally obtained through bootstrapping based on 10 draws (due to the long computation times we used 10 draws rather than 100, here). PTA1-PTA4 are indicator variables for PTAs of different types.

larger than the one of OLS, and the applied economist would have done better to ignore any self-selection into PEIAs rather than doing IPW or matching!

The biggest biases of OLS relative to CBE in absolute terms emerge for the combination of PTA with BIT and DTT (PTA&BIT&DTT), amounting to 0.6953 in Table 2.3, and for the deepest PTA form with BIT and DTT (PTA4&BIT&DTT), amounting to 0.9197 in Table 2.4. The biases of IPW versus CBE for these treatments in the two tables amount to 1.3354 and 8.0764, respectively, and they are not even the biggest ones of the IPW estimator across different treatments. Most of the biases of OLS relative to CBE amount to substantially less than 0.5.

It is very important to see that the above insights are not an outcome of the simultaneous treatment of more PEIA types than is customary in the literature, nor are they an outcome of the consideration of a longer panel than is often used, as the probability models for IPW are estimated for each treatment type and year separately. Hence, the takeaway message is that (propensity- or other such as Mahalanobis-distance-) score-based nonparametric selection-on-observables estimators likely suffer from a lack of covariate balancing for any PEIA treatment type considered in empirical international economics. The lack of covariate balancing in the data even leads to a bias in the signs of some of the partial PEIA-treatment effects with IPW relative to CBE.

Putting the estimates in context of the literature, we have to aggregate (compute properly-weighted averages of) the average treatment effects in Tables 2.3 and 2.4 (see Lechner, 2001), since earlier work rarely provided a simultaneous analysis of PTAs, BITs, and DTTs. When weighting the respective cells in Table 2.3, where a PTA was involved, we obtain an average partial effect of BITs on bilateral exports, unconditional on other treatment states. In a similar vein, we may obtain average partial effects of BITs and DTTs from the same table. And we may proceed in a similar way in computing such effects from Table 2.4. We report these weighted-average partial effects of PEIAs based on either table and the three estimators under consideration in Table 2.5.

Table 2.5: Weighted average ATE of PEIAs

Coarse PEIAs				Fine-grained PEIAs			
	OLS	**IPW**	**CBE**		**OLS**	**IPW**	**CBE**
PTA	1.4926	0.8348	1.0559	PTA1	1.3873	1.1669	1.1260
SE	*0.0010*	*0.1096*	*0.0014*	*SE*	*0.0720*	*0.4368*	*0.0558*
BIT	1.3736	-0.4470	0.9629	PTA2	1.2650	2.0552	0.7670
SE	*0.0009*	*0.2178*	*0.0014*	*SE*	*0.0430*	*0.4459*	*0.0274*
DTT	1.4078	0.2771	1.0392	PTA3	1.7284	2.0454	1.3998
SE	*0.0008*	*0.2042*	*0.0012*	*SE*	*0.0392*	*0.4455*	*0.0299*
				PTA4	1.3381	2.0557	-0.7540
				SE	*0.0164*	*1.2565*	*0.3457*
				BIT	1.5732	1.2029	1.1388
				SE	*0.0535*	*0.8574*	*0.0373*
				DTT	1.7475	1.4762	0.6738
				SE	*0.0358*	*0.8406*	*0.2543*

ATE: Partial average treatment effect. The entries in the table build on the estimates in Tables 2.3 and 2.4.

The figures in this table can be interpreted as follows. First, the partial average treatment effect of PTA membership irrespective of other PEIA treatments is estimated at 1.0559 with CBE, while it is estimated to be much higher with OLS and much lower with IPW. That pattern is similar with DTTs, and IPW even fails to estimate the (positive) sign of the partial effect correctly when considering BITs.

Let us use compare the results regarding average effects of PTAs to the ones in Baier and Bergstrand (2007, 2009) and in Egger and Wamser (2013), all of whom considered fixed-effects, some of them additionally considered propensity-score techniques – though using somewhat different country and time samples. Average effects of BITs and DTTs on bilateral exports here can be compared to the ones in Egger and Wamser (2013). In any case, neither Baier and Bergstrand (2007, 2009) nor Egger and Wamser (2013) enforced covariate balancing, and it will be primarily interesting to see whether the difference in the CBE estimates to simple OLS and IPW

Table 2.6: Comparison of weighted average PEIA ATEs with estimates in the literature

	CBE	**BB2007**	**EW2013**	**BB2009**			
				OLS FE		**Matching FE**	
				1980	**2000**	**1980**	**2000**
PTA	1.0599	0.46	1.65	-0.78	-0.08	0.75	0.61
SE	*0.0014*	-	0.41	-	-	-	-
BIT	0.9629	-	2.34	-	-	-	-
SE	*0.0014*	-	0.25	-	-	-	-
DTT	1.0392	-	1.79	-	-	-	-
SE	*0.0012*	-	0.15	-	-	-	-

ATE: Partial average treatment effect. CBE repeats the results in Table 2.5. BB2007 repeats the results in Column 1 of Table 5 in Baier and Bergstrand (2007). EW2013 repeats the results in Table 8 of Egger and Wamser (2013) using properly-weighted values (see Lechner, 2001). BB2009 OLS FE repeats the results in Column 4 of Table 5 and Matching FE the ones in Column 7 of Table 5 in Baier and Bergstrand (2009). Notice that Baier and Bergstrand (2007, 2009) do not report standard errors but only $t-$ and $z-$statistics, respectively. The reported result from BB2007 is statistically significant at one percent, while all the results from BB2009 except the one for OLS FE 2000 (which is not statistically significant at a customary level) are statistically significant at least at five percent.

in this paper – which is exclusively caused by differences in estimation techniques – is of a comparable magnitude to the difference between the estimates in Baier and Bergstrand (2007, 2009) and Egger and Wamser (2013) and the CBE estimates here – which is caused jointly by differences in estimation techniques and sample composition. Table 2.6 summarizes this comparison, building on Table 2.5.

In a pure fixed-effects-estimation approach, Baier and Bergstrand (2007) found the average treatment effect of a PTA to be 0.46, compared to 1.06, here. Hence, the consideration of a bigger sample here leads to larger effects (notice that the OLS fixed-effects estimates of PTAs in Table 2.5 are more than twice as high as those in Baier and Bergstrand, 2007). Egger and Wamser (2013) found an ATE of PTAs on bilateral exports of 1.65 in a smaller sample than the present one; this effect is considerably larger than the comparable IPW estimate in Table 2.5. However, it should be noted that Egger and Wamser (2013) accounted for dynamic adjustments whereas we focus on short-run effects, here.[13] They estimated an ATE of BITs on log exports of 2.34 and one of DTTs of 1.79 – the comparable estimates here are 0.96 and 1.04, respectively. Hence, we conclude that a quantitative comparison between earlier estimates and ours is difficult, since the sample composition causes even bigger deviations than estimation techniques do. However, a comparison of the estimates across the columns in Table 2.5 as well as in earlier tables suggests that, even in comparison to sample-composition-induced differences among the estimates in the literature, covariate balancing is of a nontrivial relative importance for the magnitude of the estimates.

General-equilibrium effects of PEIA membership on welfare

While empirical economists tended to stop at reporting partial effects of PEIAs less than a decade ago, it is now customary to quantify such effects when taking into account general-equilibrium repercussions (see, e.g., Egger and Larch, 2011; Egger, Larch, Staub, and Winkelmann, 2013; Caliendo and Parro, 2015).

For the counterfactual analysis, we build on Dekle, Eaton, and Kortum (2007). Based on the aggregate bilateral consumption of consumers in j from producers in i in year t, Y_{ijs}, we may define aggregate sales of producers in i in that year as $Y_{is} = \sum_j Y_{ijs}$ and aggregate consumption of consumers in i in that year as $E_{is} = \sum_j Y_{jis}$. Country i's trade deficit in that year is $D_{is} = E_{is} - Y_{is}$. We may define $D_{is} = d_{is} Y_{is}$. For counterfactual analysis, we keep d_{is} constant. For an outline of the procedure to compute counterfactual model values corresponding to shocks in

[13] It turns out that it is easier to identify short-run than long-run effects in general. The reason is that, with sluggish adjustment, not only the covariates in some initial period matter but also the ones in between that period and the time of the measurement of the long(er)-run effects. Obviously, it is extremely demanding to ensure covariate balancing over an adjustment period of several years.

PEIA membership, let us use prime to denote counterfactual values of any variable and dot to denote counterfactual-to-benchmark changes.

Furthermore, let us use $y_{ijs} = Y_{ijs}/Y_{js}$ for bilateral expenditure shares, use T_{ijs} for iceberg trade costs and $\eta < 0$ to denote the elasticity of trade flows with respect to trade costs, and recognize that in the customary class of quantitative one-sector general equilibrium models the change in expenditures, $\dot{E}_{is}$, is proportional to the change in factor costs and producer prices. Then, we may follow the idea of Dekle, Eaton, and Kortum (2007) to define

$$y'_{ijs} = \frac{\dot{E}_{is}{}^{\eta}\dot{T}^{\eta}_{ijs}y_{ijs}}{\dot{P}_{js}{}^{\eta}}, \tag{2.15}$$

where

$$\dot{P}^{\eta}_{js} = \sum_i \dot{E}_{is}{}^{\eta}\dot{T}^{\eta}_{ijs}y_{ijs} \tag{2.16}$$

is the consumer price index for goods consumed in j at time t.[14]

Provided an estimate of $\dot{T}^{\eta}_{ijs}$, which corresponds to $\exp[ATE^{\theta 0}_{balance}(\Theta'_{ijs} - \Theta_{ijs})]$ in our case, and given y_{ijs} for all countries i and j at time t as well as η, one may solve for $\dot{E}_{is}$ for all countries except one (the numéraire), and all the other endogenous variables of interest, $\{\dot{y}_{ijs}, \dot{P}_{js}\}$, are then determined for all countries i and j in year t by the above model structure.

[14]Note that we ignore any redistribution effects of tariffs here for the sake of simplicity. In any case, the results in Egger and Larch (2011) suggest that the tariff-related effects of PTA membership are small relative to the associated non-tariff effects, on average.

Relying on a utilitarian welfare criterion, the change in consumer welfare in response to PEIA treatment, $\dot{W}_{is}(\dot{\Theta})$, is determined as the change in real consumption:

$$\dot{W_{is}}^{\eta} = \frac{\dot{E}_{is}}{\dot{P}_{is}}. \tag{2.17}$$

In what follows, we use data for the year 2006 to illustrate the relevance of covariate balancing for the quantitative analysis in terms of welfare changes. Tables 2.7 and 2.8 summarize the corresponding results for the coarse and fine-grained definitions of PEIAs, respectively. These tables are organized vertically in terms of different PEIA treatments and horizontally to provide results based on OLS, IPW, and CBE estimates of the partial average PEIA treatment effects.

Table 2.7: Average real-consumption (welfare) effects from all country pairs having a PEIA of the specified kind relative to the status quo in 2006 (using the coarse PEIA definition)

	% **Welfare Change**		
	OLS	IPW	CBE
PTA	5.45	21.51	2.18
BIT	2.09	6.89	1.13
DTT	7.60	0.98	4.88
PTA&BIT	44.20	0.10	17.38
PTA&DTT	44.02	386.62	20.75
BIT&DTT	25.87	0.19	11.34
PTA&BIT&DTT	142.69	0.26	44.26

Table 2.8: Average real-consumption (welfare) effects from all country pairs having a PEIA of the specified kind relative to the status quo in 2006 (using the fine-grained PEIA definition)

	% **Welfare Change**		
PEIAs	OLS	IPW	CBE
PTA1	1.0141	0.6495	0.7520
PTA2	1.3477	16.4702	0.5770
PTA3	2.4036	1.3477	0.8156
PTA4	3.8031	75.0455	0.8156
BIT	2.1048	6.8221	1.1605
DTT	7.0127	0.9861	4.5293
PTA1&BIT	4.0050	18.7968	3.3563
PTA2&BIT	5.5533	649.5653	1.2667
PTA3&BIT	4.4642	1.4470	2.4046
PTA4&BIT	17.1303	8.7078	4.3552
PTA1&DTT	14.8228	11.4417	6.4192
PTA2&DTT	1.8160	0.5937	1.4115
PTA3&DTT	11.3812	1,439.7520	8.2098
PTA4&DTT	5.7759	133.2927	0.4273
BIT&DTT	24.6472	0.2712	10.7307
PTA1&BIT&DTT	26.3835	2.6418	11.8842
PTA2&BIT&DTT	22.7528	20.4637	9.1668
PTA3&BIT&DTT	33.7238	0.6542	13.7982
PTA4&BIT&DTT	62.2179	34.6331	16.2719

In general terms, the associated results suggest that OLS (which assumes exogeneity of PEIA membership conditional on a linear index of the observables) always over-estimates the treatment effect of PEIAs on outcome. Even more drastically, IPW (which erroneously ascribes part of the effect of PEIAs on outcome to treatment rather than differences in the covariates behind the nonparametric propensity-score-based index) does even worse and over- and underestimates treatment effects of PEIAs by factors of up to 20 and 170, respectively. In the interest of brevity, let us focus on problems of IPW here. For instance, the general-equilibrium effect for all countries without any PEIA to adopt a PTA on bilateral trade is overestimated by a factor of ten when using IPW rather than CBE estimates, according to the first line in Table 2.7. The general-equilibrium effect of adopting a PTA and a DTT jointly is even overestimated by a factor of almost twenty by IPW. Conversely, the general-equilibrium effects of PTA&BIT and even more so those of PTA&BIT&DTT are severely downward biased by factors of 170 and 173, respectively. If anything, the CBE results in Table 2.7 appear much more realistic than the IPW ones, as we would clearly expect deeper integration (liberalizing in more domains than just tariffs) to have larger effects on welfare than a more shallow one.

The results using the more fine-grained definition of PEIAs in Table 2.8 speak the same language: the average gap between OLS and even more so IPW results and CBE is nothing else than drastic, and the pattern of CBE results appears much more coherent than that of OLS and even more so of IPW.

2.4 Conclusions

A relatively large body of academic and applied policy work is concerned with an ex-ante analysis of different types of preferential economic integration agreements (PEIAs) – historically mostly preferential trade agreements (PTAs), but increasingly also bilateral investment agreements (BITs) and double-taxation treaties (DTTs) – on economic outcome and, in particular, welfare.

A prerequisite for such an analysis are consistent estimates of partial effects of PEIAs on trade flows. In the wake of that literature, ordinary least squares (OLS) was used to obtain such estimates, but it was recognized more recently that doing so leads to bias flowing from the self-selection of countries and country pairs into

PEIAs. Since theoretical work provides good guidance regarding the joint determining factors of PEIA membership and bilateral trade flows, the leading paradigm in work on the determinants and the causal effects of PEIAs on economic outcome became a framework of conditional mean independence (e.g., through matching or inverse-probability weighting, which have been shown to be equivalent). A key prerequisite of such an approach is that the moments of the observable joint determinants of PEIA membership and trade flows are the same between the treated (PEIA members) and the controls (PEIA non-members). This equality of moments is referred to as covariate balancing in the literature.

In a large panel data-set of bilateral trade flows and PEIA membership, the present paper documents that covariate balancing is violated in the data and, as a consequence, customary conditional-mean-independence-based results may be biased. To overcome this problem, the present paper utilizes a relatively novel approach, entropy balancing of the covariates (CBE) proposed by Hainmüller (2012). Rather than assuming covariate balancing for a number of moments and testing for it ex post, that approach enforces it ex ante by imposing an appropriate number of nonlinear constraints in estimation.

This approach yields estimates that do not only differ starkly from (self-selection-biased) OLS estimates as had been used at the wake of this research line but even more so, which is surprising, from IPW estimates. This indicates that the lack of covariate balancing in IPW estimates (e.g., propensity-score matching or inverse-probability weighting regression) aggravates the bias of PEIA treatment effects rather than removing it. As a consequence, not only partial average IPW-based treatment effects of PEIA membership on bilateral trade flows are substantially biased but so are the associated general equilibrium effects, e.g., on real consumption as a utilitarian measure of welfare. For some (prominent) treatment examples such as PTA membership alone or in combination with BITs and DTTs the IPW-based bias amounts to factors of larger than ten! Hence, we advise researchers in the literature on PEIA effects to always enforce covariate balancing in order to avoid a severe bias in parametric or nonparametric PEIA-treatment-effects estimates and associated quantifications based on structural trade models.

Tables and figures

Table 2.9: Descriptive statistics

Variables	Obs.	Mean	Std. Dev.	Min	Max
Outcome variable					
Y	434,895	1.35	3.36	-6.91	12.78
1st-stage covariates only					
RGDP	434,895	48.96	2.80	37.36	59.45
DRGDP	434,895	2.83	2.02	0	10.97
DKL	434,895	0.03	0.07	0	0.64
DROWKL	434,895	1.42	0.62	0	3.85
1st- and 2nd-stage covariates					
DIST	434,895	8.57	0.87	4.09	9.89
BORDER	434,895	0.03	0.17	0	1
LANG1	434,895	0.18	0.39	0	1
LANG2	434,895	0.19	0.39	0	1
COLONY1	434,895	0.03	0.16	0	1
COLONY2	434,895	0.09	0.29	0	1
COLONY3	434,895	0.00	0.02	0	1
COLONY4	434,895	0.02	0.13	0	1
SMCTRY	434,895	0.02	0.13	0	1

Table 2.10: Presence of PEIAs: coarse-grained PEIA definition

PEIA Types	**Observations**	**Percentage**
Null-state	311,974	71.74
PTA	34,870	8.02
BIT	17,734	4.08
DTT	32,756	7.53
PTA&BIT	6,225	1.4
PTA&DTT	5,480	1.26
BIT&DTT	16,822	3.87
PTA&BIT&DTT	9,034	2.08
Total	434,895	100.00

Table 2.11: Presence of PEIAs: fine-grained PEIA definition

PEIA Types	**Observations**	**Percentage**
Null-state	311,974	71.88
PTA1	20,707	4.77
PTA2	4,444	1.02
PTA3	5,283	1.22
PTA4	3,649	0.84
BIT	17,734	4.09
DTT	32,756	7.55
PTA1&BIT	1932	0.45
PTA2&BIT	1,213	0.28
PTA3&BIT	603	0.14
PTA4&BIT	2,456	0.57
PTA1&DTT	1,954	0.45
PTA2&DTT	1,248	0.29
PTA3&DTT	770	0.18
PTA4&DTT	1,464	0.34
BIT&DTT	16,822	3.88
PTA1&BIT&DTT	2,291	0.53
PTA2&BIT&DTT	894	0.21
PTA3&BIT&DTT	982	0.23
PTA4&BIT&DTT	4,842	1.12
Total	434,018	100

The overall total should add up to 434,895 as per the number of observations in Table 2.10. However, the DESTA database covers slightly fewer PTAs than those recorded by the WTO.

Table 2.12: P-values of covariate-balancing tests

		7 Treatments		19 Treatments	
		Median	Mean	Median	Mean
Means' comparison test	OLS	0.0000	0.0186	0.0000	0.0875
	IPW	0.0000	0.0456	0.0002	0.1297
	CBE	0.9968	0.9870	0.9977	0.9844
Variance comparison test	OLS	0.0000	0.0907	0.0000	0.0819
	IPW	0.0013	0.1405	0.0021	0.1458
	CBE	0.2664	0.2664	0.0923	0.2529

Appendix

Tables A1 and A2 summarize the first and second moments of the distribution of estimated PEIA response probabilities based on probit for selected years (the first year in the data as well as every decennial year afterwards) using the corase-grained and the fine-grained PEIA definitions, respectively. Clearly, on average, these response probabilities are low, since the likelihood of any PEIA type in the data is relatively low. Due to an increased frequency of PEIAs with time, the average response probabilities tend to be higher in more recent than in less recent years. Tables A3 and A4 list the PTAs included in the four categories PTA1-PTA4.

Table A3: Preferential Trade Agreements of Type 1 and 2

PTA1	PTA2
Andean Countries MERCOSUR	Afghanistan India
Andean Group Cartagena Agreement	Algeria EC
Arab Maghreb Union	Algeria Tunisia
Argentina Brazil	Andean Community Brazil
Argentina Colombia	Andean Countries Argentina
Argentina Cuba	Argentina Brazil
Argentina Ecuador	Argentina Chile
Argentina Mexico	Argentina Ecuador
Argentina Paraguay	Argentina Venezuela
Argentina Peru	Armenia Estonia
Argentina Uruguay	Arusha Agreement II
Argentina Venezuela	Asia Pacific Trade Agreement (Bangkok..
Armenia Iran	Association of Southeast Asian Nation..
Association of Caribbean States	Baltic Free Trade Area (BAFTA) Non Ta..
Australia Canada	Baltic Free Trade Area (BAFTA) agricu..
Bolivia Uruguay	Bangkok Agreement
Brazil Cuba	Belize Guatemala
Brazil Guyana	Bolivia Chile
Brazil Suriname	Bolivia Cuba
Brazil Uruguay	Bolivia Paraguay
Canada New Zealand	Brazil Mexico
Canada Portugal	Brazil Peru
Canada Spain	Caribbean Community (CARICOM) Colombia
Canada US Automotive Products Trade A..	Caribbean Community (CARICOM) Costa R..
Central American Integration System	Caribbean Community (CARICOM) Cuba
Chile Uruguay	Caribbean Community (CARICOM) Dominic..
Colombia Costa Rica	Caribbean Community (CARICOM) Venezuela
Colombia El Salvador	Central America Chile
Colombia Guatemala	Chile Colombia
Colombia Honduras	Chile Cuba
Colombia Nicaragua	Chile Ecuador
Costa Rica Mexico	Chile India
Costa Rica Nicaragua Panama	Chile Mexico
Costa Rica Panama	Chile Venezuela
Costa Rica Venezuela	Colombia Cuba
Cuba Mexico	Colombia Northern Triangle
Cuba Uruguay	Colombia Panama
Dominican Republic Panama	Cotonou Agreement
EC Egypt Agreement	Croatia EC
EC Israel	Cuba Ecuador
EC Jordan	Cuba Guatemala
EC Lebanon	Cuba MERCOSUR
EC Morocco	Cuba Paraguay
EC Morocco Association Agreement	Cuba Peru
EC Slovenia	Cuba Venezuela
EC Spain	D8 PTA
EC Syria	EC Egypt
EC Tunisia	Ecuador Uruguay
EC Tunisia Association Agreement	Egypt Morocco
EC Turkey Association Agreement (Anka..	El Salvador Panama
Economic Cooperation Organization (EC..	Group of Three Auto Agreement
Economic Cooperation Organization Tra..	Guatemala Panama
Ecuador Mexico	India MERCOSUR
Ecuador Paraguay	India Nepal
Egypt Jordan	Iran Pakistan
Egypt PLO	Iran Syria
Egypt Syria	Latvia Ukraine Agriculture
El Salvador Mexico	LomÃƒÂ© I
El Salvador Venezuela	LomÃƒÂ© II
European Coal and Steel Community	LomÃƒÂ© III
Global System of Trade Preferences (G..	LomÃƒÂ© IV
Guatemala Mexico	MERCOSUR Mexico Auto Agreement
Guatemala Venezuela	MERCOSUR Peru
Guinea Morocco	MERCOSUR Southern African Customs Uni..
Guyana Venezuela	Malawi Mozambique

Honduras Mexico
Honduras Panama
Honduras Venezuela
India Nepal
Inter-Arab Trade Agreement
Iraq Jordan
Israel Jordan
Jordan Morocco
Jordan Qatar
Jordan Saudi Arabia
Jordan Sudan
Jordan Sudan amended
Jordan Syria
Kuwait UAE
Laos Thailand
Latin American Free Trade Area (LAFTA)
Latin American Integration Associatio..
Mauritania Morocco
Melanesian Spearhead Group (MSG)
Mexico Nicaragua
Mexico Panama
Mexico Peru
Mexico Uruguay
Morocco Saudi Arabia
Morocco Senegal
Namibia Zimbabwe
Nicaragua Panama
Nicaragua Venezuela
Paraguay Venezuela
Protocol on Trade Negotiations (PTN)
South Africa Zimbabwe
South Asian Association for Regional ..
Southern African Development Communit..
Trinidad and Tobago Venezuela
Tripartite Agreement
Uruguay Venezuela
Mauritius Pakistan
PTA for Eastern and Southern African ..
Peru Thailand
Peru Venezuela
South Pacific Trade and Economic Co O..
United States Vietnam
YaoundÃƒÂ© I

[!hbt]

Table A1: Moments of estimated probit propensity scores of PEIA membership by type using the coarse definition of PEIAs

		1961		**1970**		**1980**		**1990**		**2000**		**2010**	
		Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
PTA	*Control*	0.0031	0.0162	0.0139	0.0398	0.0237	0.0530	0.0392	0.0659	0.0815	0.0930	0.0860	0.0938
	Treated	0.0655	0.1027	0.1589	0.1945	0.1449	0.1576	0.1637	0.1826	0.2566	0.2075	0.2677	0.2174
BIT	*Control*	.	.	0.0036	0.0114	0.0076	0.0206	0.0151	0.0366	0.0377	0.0709	0.0465	0.0803
	Treated	.	.	0.0226	0.0163	0.0506	0.0380	0.1029	0.0674	0.2076	0.1583	0.2385	0.1643
DTT	*Control*	0.0087	0.0314	0.0196	0.0528	0.0294	0.0653	0.0359	0.0744	0.0454	0.0839	0.0515	0.0873
	Treated	0.0697	0.0980	0.1556	0.1610	0.1915	0.1667	0.2197	0.1749	0.2515	0.1912	0.2456	0.1934
PTA&BIT	*Control*	.	.	.	.	0.0011	0.0082	0.0040	0.0213	0.0218	0.0535	0.0269	0.0593
	Treated	.	.	.	.	0.0003	0.0004	0.2345	0.2527	0.2430	0.2289	0.2301	0.2279
PTA&DTT	*Control*	0.0003	0.0060	0.0016	0.0166	0.0037	0.0214	0.0054	0.0279	0.0079	0.0365	0.0179	0.0568
	Treated	0.0704	0.0986	0.2332	0.2469	0.3002	0.2786	0.1279	0.1390	0.1686	0.1710	0.2344	0.2061
BIT&DTT	*Control*	.	.	0.0013	0.0093	0.0043	0.0225	0.0106	0.0432	0.0294	0.0804	0.0442	0.1006
	Treated	.	.	0.0123	0.0129	0.0618	0.0926	0.1803	0.1854	0.3647	0.2472	0.4095	0.2500
PTA&BIT&DTT	*Control*	.	.	.	.	0.0104	0.0433	0.0094	0.0398	0.0064	0.0309	0.0073	0.0331
	Treated	.	.	.	.	0.0018	0.0000	0.4987	0.2850	0.3226	0.2865	0.2302	0.2347

[!hbt]

Table A2: Moments of estimated probit propensity scores of PEIA membership by type using the fine-grained definition of PEIAs

		1961		**1970**		**1980**		**1990**		**2000**		**2010**	
		Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
PTA1	*Control*	0.0121	0.0159	0.0155	0.0185	0.0173	0.0198	0.0175	0.0175	0.0176	0.0190	0.0202	0.0214
	Treated	0.0622	0.0364	0.0697	0.0417	0.0543	0.0404	0.0362	0.0362	0.0314	0.0291	0.0358	0.0325
PTA2	*Control*	0.0110	0.0222	0.0139	0.0262	0.0160	0.0290	0.0176	0.0176	0.0207	0.0373	0.0221	0.0394
	Treated	.	.	0.0498	0.0306	0.0732	0.0618	0.0672	0.0672	0.0862	0.0733	0.0925	0.0788
PTA3	*Control*	0.0037	0.0162	0.0039	0.0167	0.0037	0.0167	0.0036	0.0036	0.0029	0.0154	0.0028	0.0158
	Treated	0.0869	0.0767	0.0738	0.0684	0.0617	0.0729	0.0731	0.0731	0.0457	0.0535	0.0477	0.0488
PTA4	*Control*	0.0124	0.0238	0.0143	0.0263	0.0145	0.0261	0.0150	0.0150	0.0150	0.0250	0.0143	0.0259
	Treated	0.0926	0.0755	0.0428	0.0562	0.0694	0.0780	0.0595	0.0595	0.0690	0.0667	0.0525	0.0659
BIT	*Control*	.	.	0.0144	0.0330	0.0186	0.0384	0.0198	0.0198	0.0199	0.0403	0.0238	0.0451
	Treated	.	.	0.0668	0.0396	0.0867	0.0481	0.1031	0.1031	0.0981	0.0876	0.1136	0.0935
DTT	*Control*	0.0174	0.0444	0.0278	0.0592	0.0346	0.0660	0.0361	0.0361	0.0358	0.0648	0.0423	0.0712
	Treated	0.0999	0.1326	0.1703	0.1703	0.1938	0.1658	0.2019	0.2019	0.1949	0.1554	0.2037	0.1657
PTA1&BIT	*Control*	.	.	.	.	0.0009	0.0027	0.0009	0.0009	0.0007	0.0022	0.0010	0.0026
	Treated	.	.	.	.	0.0029	0.0027	0.0049	0.0049	0.0068	0.0112	0.0090	0.0128
PTA2&BIT	*Control*	.	.	.	.	0.0015	0.0049	0.0019	0.0019	0.0023	0.0083	0.0029	0.0096
	Treated	.	.	.	.	0.0214	0.0161	0.0385	0.0385	0.0351	0.0362	0.0364	0.0384
PTA3&BIT	*Control*	.	.	.	.	0.0003	0.0016	0.0003	0.0003	0.0002	0.0011	0.0002	0.0012
	Treated	.	.	.	.	0.0002	.	0.0028	0.0028	0.0045	0.0054	0.0075	0.0065
PTA4&BIT	*Control*	.	.	.	.	0.0039	0.0132	0.0038	0.0038	0.0033	0.0098	0.0034	0.0106
	Treated	.	.	.	.	0.0169	0.0158	0.1733	0.1733	0.0715	0.1037	0.0628	0.0936
PTA1&DTT	*Control*	0.0007	0.0060	0.0011	0.0080	0.0014	0.0068	0.0014	0.0014	0.0011	0.0057	0.0015	0.0073
	Treated	0.0213	.	0.0738	0.0927	0.0639	0.0954	0.0200	0.0200	0.0201	0.0245	0.0192	0.0266
PTA2&DTT	*Control*	0.0010	0.0083	0.0015	0.0109	0.0016	0.0105	0.0018	0.0018	0.0020	0.0108	0.0024	0.0108
	Treated	.	.	.	.	0.1051	0.1122	0.1186	0.1186	0.1132	0.1539	0.0977	0.1565
PTA3&DTT	*Control*	0.0005	0.0055	0.0008	0.0070	0.0008	0.0046	0.0008	0.0008	0.0005	0.0048	0.0006	0.0043
	Treated	.	.	0.0172	.	0.0181	0.0193	0.0295	0.0295	0.0229	0.0309	0.0132	0.0177
PTA4&DTT	*Control*	0.0014	0.0142	0.0024	0.0181	0.0028	0.0146	0.0026	0.0026	0.0019	0.0128	0.0022	0.0124
	Treated	0.1932	0.1458	0.1943	0.2040	0.2558	0.2127	0.1728	0.1728	0.0718	0.0898	0.0632	0.1016
BIT&DTT	*Control*	.	.	0.0142	0.0463	0.0180	0.0525	0.0181	0.0181	0.0150	0.0445	0.0179	0.0490
	Treated	.	.	0.2032	0.1563	0.1414	0.1368	0.1767	0.1767	0.1636	0.1391	0.1656	0.1404
PTA1&BIT&DTT	*Control*	.	.	.	.	0.0008	0.0035	0.0008	0.0008	0.0007	0.0040	0.0008	0.0039
	Treated	.	.	.	.	0.0183	0.0208	0.0333	0.0333	0.0208	0.0384	0.0228	0.0392
PTA2&BIT&DTT	*Control*	.	.	.	.	0.0008	0.0052	0.0009	0.0009	0.0009	0.0066	0.0013	0.0063
	Treated	.	.	.	.	0.0026	0.0012	0.0599	0.0599	0.0514	0.0667	0.0493	0.0772
PTA3&BIT&DTT	*Control*	.	.	.	.	0.0008	0.0044	0.0007	0.0007	0.0004	0.0036	0.0004	0.0026
	Treated	.	.	.	.	0.0594	0.0815	0.0285	0.0285	0.0188	0.0330	0.0202	0.0355
PTA4&BIT&DTT	*Control*	.	.	.	.	0.0052	0.0291	0.0045	0.0045	0.0027	0.0194	0.0029	0.0192
	Treated	.	.	.	.	0.2570	.	0.4764	0.4764	0.3229	0.2511	0.2159	0.2363

Table A4: Preferential Trade Agreements of Type 3 and 4

PTA3	PTA4
African Common Market	African Economic Community
Algeria Jordan	Agadir Agreement
Andean Community Auto Agreement	Albania Bosnia and Herzegovina
Andean Community Sucre Protocol	Albania Bulgaria
Andorra EC	Albania Croatia
Arab Common Market	Albania EC SAA
Argentina Uruguay	Albania EFTA
Armenia Moldova	Albania Kosovo
Armenia Russia	Albania Macedonia
Armenia Turkmenistan	Albania Moldova
Armenia Ukraine	Albania Romania
Australia New Zealand Free Trade Agre..	Albania Serbia
Australia Papua New Guinea	Albania Turkey
Azerbaijan Georgia	Algeria EC Euro-Med Association
Bahrain Jordan	Algeria Morocco
Belarus Russia (Union State)	Andean Community Trujillo Protocol
Belarus Ukraine	Andean Group Quito Protocol
Bhutan India	Armenia Georgia
Bulgaria Finland	Armenia Kazakhstan
Caribbean Community (CARICOM)	Armenia Kyrgyzstan
Caribbean Free Trade Association (CAR..	Arusha Agreement I
Central African Customs and Economic ..	Association of Southeast Asian Nation..
Central American Common Market (CACM)	Association of Southeast Asian Nation..
Central American Common Market (CACM)..	Association of Southeast Asian Nation..
Central American Free Trade Area (CAF..	Association of Southeast Asian Nation..
Czechoslovakia Finland	Association of Southeast Asian Nation..
EC Finland	Association of Southeast Asian Nation..
EC Israel	Association of Southeast Asian Nation..
EC Malta	Association of Southeast Asian Nation..
EC Portugal	Association of Southeast Asian Nation..
EC Portugal Additional Protocol	Association of Southeast Asian Nation..
EC San Marino	Australia Chile
EC Sweden	Australia China
EC Yugoslavia	Australia Japan
EFTA Finland	Australia Korea
Economic Community Of West African St..	Australia Malaysia
Economic Community of Central African..	Australia New Zealand (ANZCERTA)
Economic and Monetary Community of Ce..	Australia New Zealand (ANZCERTA)
Egypt Jordan	Australia Singapore
El Salvador Guatemala	Australia Thailand
El Salvador Nicaragua Free Trade Area	Australia US
Equatorial Customs Union	Austria EC
Equatorial Customs Union Cameroon Ass..	Azerbaijan Ukraine
Estonia Finland	Bahrain US
Estonia Sweden	Baltic Free Trade Area (BAFTA) indust..
Estonia Ukraine	Belarus Serbia
Eurasian Economic Community (EAEC)	Benelux Economic Union
Faroe Islands Switzerland	Bolivia MERCOSUR
Finland German Democratic Republic	Bolivia Mexico
Finland Hungary	Bosnia and Herzegovina Bulgaria
Finland Latvia	Bosnia and Herzegovina Croatia
Finland Lithuania	Bosnia and Herzegovina EC SAA
Finland Poland	Bosnia and Herzegovina Macedonia
France Monaco	Bosnia and Herzegovina Moldova
France Tunisia Customs Union Convention	Bosnia and Herzegovina Romania
Georgia Kazakhstan	Bosnia and Herzegovina Serbia Montene..
Georgia Russia	Bosnia and Herzegovina Slovenia
Georgia Turkmenistan	Bosnia and Herzegovina Turkey
Georgia Ukraine	Bosnia and Herzogovina EFTA
Ghana Upper Volta Trade Agreement	Brunei Japan
Greater Arab Free Trade Agreement	Bulgaria EC
Gulf Cooperation Council (GCC)	Bulgaria EFTA
India Sri Lanka	Bulgaria Estonia
Iraq UAE	Bulgaria Israel
Ireland UK Free Trade Area	Bulgaria Latvia
Jordan Kuwait	Bulgaria Lithuania
Jordan Lebanon	Bulgaria Macedonia

Jordan Libya
Jordan PLO
Jordan Tunisia
Jordan UAE
Kazakhstan Kyrgyzstan
Kazakhstan Ukraine
Kyrgyzstan Moldova
Kyrgyzstan Russia
Kyrgyzstan Ukraine
Kyrgyzstan Uzbekistan
Libya Morocco
Lithuania Sweden
Mano River Union
Organisation of Eastern Caribbean Sta..
Pacific Island Countries Trade Agreem..
Romania Turkey
Russia Ukraine
Saudi Arabia UAE
South Africa Southern Rhodesia Custom..
South Asian Free Trade Area (SAFTA)
Treaty of Economic Association
Turkmenistan Ukraine
West African Economic Community (CEAO)

Bulgaria Moldova
Bulgaria Serbia
Bulgaria Slovakia
Bulgaria Slovenia
Bulgaria Turkey
CARIFORUM EC EPA
Canada Chile
Canada Colombia
Canada Costa Rica
Canada EC (CETA)
Canada EFTA
Canada Honduras
Canada Israel
Canada Jordan
Canada Korea
Canada Panama
Canada Peru
Canada US
Caribbean Community (CARICOM) Protoco..
Caribbean Community (CARICOM) revised
Central America Dominican Republic
Central America EC
Central America EFTA
Central America Panama
Central American Free Trade Agreement..
Central American Free Trade Agreement..
Central European Free Trade Agreement..
Chile China
Chile Colombia
Chile EC
Chile EFTA
Chile Hong Kong
Chile Japan
Chile Korea
Chile MERCOSUR
Chile MERCOSUR Services
Chile Malaysia
Chile Mexico
Chile Panama
Chile Peru
Chile Thailand
Chile Turkey
Chile US
Chile Vietnam
China Costa Rica
China Hong Kong
China Iceland
China Korea
China Macao
China New Zealand
China Pakistan
China Pakistan Services
China Peru
China Singapore
China Switzerland
Colombia Costa Rica
Colombia EFTA
Colombia Israel
Colombia Korea
Colombia Mexico Venezuela
Colombia Panama
Colombia Peru EC
Colombia US
Common Economic Zone
Common Market for Eastern and Souther..
Commonwealth of Independent States (C..
Costa Rica Mexico
Costa Rica Peru
Costa Rica Singapore
Cote d'Ivoire EC EPA
Croatia EFTA
Croatia Lithuania

Croatia Macedonia
Croatia Macedonia (amended)
Croatia Moldova
Croatia Serbia Montenegro
Croatia Slovenia
Croatia Turkey
Cyprus EC
Czech Republic EC
Czech Republic Estonia
Czech Republic Israel
Czech Republic Latvia
Czech Republic Lithuania
Czech Republic Slovakia
Czech Republic Slovenia
Czech Republic Turkey
Czech and Slovak Republic EFTA
EC
EC Amsterdam
EC Egypt Euro-Med Association Agreement
EC Estonia
EC Estonia Europe Agreement
EC Faroe Islands
EC Georgia
EC Greece Additional Protocol
EC Greece Association Agreement
EC Hungary
EC Iceland
EC Israel Euro-Med Association Agreem..
EC Jordan Euro-Med Association Agreem..
EC Korea
EC Latvia
EC Latvia Europe Agreement
EC Lebanon Euro-Med Association Agree..
EC Lisbon
EC Lithuania
EC Lithuania Europe Agreement
EC Maastricht
EC Macedonia SAA
EC Mexico
EC Moldova
EC Montenegro SAA
EC Morocco Euro-Med Association Agree..
EC Nice
EC Norway
EC Poland
EC Romania
EC Serbia SAA
EC Singapore
EC Single European Act
EC Slovakia
EC Slovenia Europe Agreement
EC South Africa
EC Switzerland Bilaterals I
EC Switzerland Liechtenstein
EC Tunisia Euro-Med Association Agree..
EC Turkey
EC Turkey Additional Protocol
EC Turkey Supplementary Protocol
EC Ukraine
EC Vietnam
EFTA
EFTA Egypt
EFTA Estonia
EFTA GCC
EFTA Hong Kong
EFTA Hungary
EFTA Israel
EFTA Jordan
EFTA Korea
EFTA Latvia
EFTA Lebanon
EFTA Lithuania

EFTA Macedonia
EFTA Mexico
EFTA Montenegro
EFTA Morocco
EFTA Peru
EFTA Poland
EFTA Romania
EFTA Serbia
EFTA Singapore
EFTA Slovenia
EFTA Southern African Customs Union (..
EFTA Spain
EFTA Tunisia
EFTA Turkey
EFTA Ukraine
EFTA services
East African Community (EAC)
East Caribbean Common Market
Economic Community Of West African St..
Economic and Monetary Community of Ce..
Egypt MERCOSUR
Egypt Turkey
El Salvador Honduras Taiwan
Estonia Faroe Islands
Estonia Hungary
Estonia Norway
Estonia Slovakia
Estonia Slovenia
Estonia Switzerland
Estonia Turkey
European Economic Area (EEA)
Faroe Islands Finland
Faroe Islands Iceland
Faroe Islands Norway
Faroe Islands Poland
GUAM/GUUAM Organization for Democracy..
Georgia Turkey
Group of Three
Guatemala Peru
Guatemala Taiwan
Gulf Cooperation Council (GCC) Singap..
Hong Kong New Zealand
Hungary Israel
Hungary Latvia
Hungary Lithuania
Hungary Slovenia
Hungary Turkey
India Japan
India Korea
India Malaysia
India Singapore
Indonesia Japan
Israel MERCOSUR
Israel Mexico
Israel PLO
Israel Poland
Israel Romania
Israel Slovakia
Israel Slovenia
Israel Turkey
Israel US
Japan Malaysia
Japan Mexico
Japan Mongolia
Japan Peru
Japan Philippines
Japan Singapore
Japan Switzerland
Japan Thailand
Japan Vietnam
Jordan Kuwait
Jordan Morocco

Jordan Singapore
Jordan Sudan
Jordan Syria
Jordan Turkey
Jordan US
Korea Peru
Korea Singapore
Korea Turkey
Korea US
Latvia Norway
Latvia Poland
Latvia Slovakia
Latvia Slovenia
Latvia Sweden
Latvia Switzerland
Latvia Turkey
Lithuania Norway
Lithuania Poland
Lithuania Slovakia
Lithuania Slovenia
Lithuania Switzerland
Lithuania Turkey
MERCOSUR
MERCOSUR Southern African Customs Uni..
MERCOSUR services
Macedonia Moldova
Macedonia Romania
Macedonia Slovenia
Macedonia Turkey
Macedonia Ukraine
Malawi Zimbabwe
Malaysia New Zealand
Malaysia Pakistan
Malaysia Turkey
Mauritius Turkey
Mexico Nicaragua
Mexico Northern Triangle
Mexico Panama
Mexico Uruguay
Moldova Romania
Moldova Serbia
Moldova Ukraine
Montenegro Turkey
Montenegro Ukraine
Morocco Tunisia
Morocco Turkey
Morocco UAE
Morocco US
New Zealand Singapore
New Zealand Taiwan
New Zealand Thailand
Nicaragua Taiwan
North American Free Trade Agreement (..
Oman US
Pacific Alliance
Pakistan Sri Lanka
Panama Peru
Panama Singapore
Panama Taiwan
Panama US
Peru Singapore
Peru US
Poland Turkey
Romania Serbia
Romania Slovakia
Serbia Turkey
Singapore US
Slovakia Slovenia
Slovakia Turkey
Slovenia Turkey
Southern Africa Customs Union (SACU)
Southern African Development Communit..

Syria Turkey
Tajikistan Ukraine
Trans Pacific Strategic EPA
Transpacific Partnership (TPP)
Tunisia Turkey
Ukraine Uzbekistan
West African Economic and Monetary Un..

Chapter 3

The Suspense of Trade Agreements[1]

3.1 Introduction

Over the last two decades, the international economics literature has been focusing on evaluating the influence that governments exercise on trade. With the general movement towards deregulating the global market, understanding the consequence of such measure is crucial. It is without much debate acknowledged that countries (or firms) tend to self-select into trading internationally. This is often decided on the basis of evaluating their own potential to benefit from trade. The potential is directly related with a series of individual characteristics, such as GDP (total assets), capital-to-labor ratio (productivity) and partner characteristics such as whether it shares the same language and the same border. Importantly, the potential of benefiting from trade is inversely related to the geographical distance to the trading partner. These have built the basis for the so-called gravity equation as first introduced by Tinbergen (1962), which acted as a workhorse for international trade models ever since. With all these factors cumulatively explaining the lion's share of the trade happening between two countries, not much is left for governments to control such cross-border transactions. However, the one instrument at hand, able to shape trade to a certain extent, is the materialization of economic preferentialism.

[1]This chapter is based on Tarlea (2018)

This usually takes the form of preferential economic integration agreements (PEIAs) such as preferential trade agreements (PTAs), bilateral investment treaties (BITs) or double taxation treaties (DTTs). The most extensively used and known of these three is the PTA, and it is this one alone the makes the object of our analysis.
The liberalization of trade has been extensively investigated and shown to benefit economic growth (Matoo et. al, 2008; Wacziarg and Welsch, 2008), productivity (Pavcnik, 2002; Arnold et al. 2010, 2011; Topalova and Khandelwal, 2011) while hurting unemployment (Egger and Kreickemeier, 2009; Dutt et. al, 2009; Helpman and Itskhoki, 2010) and poverty (Winters et. al, 2004; Topalova, 2010). Analyzing the effect of a PTA on cross-border trade is also not a novel topic. Jan Tinbergen (1962) is the first to include the binary variable capturing the presence or absence of a PTA respectively in the gravity equation, finding no significant effects on trade. Since then, a plethora of literature has brought overwhelming evidence of the beneficial impact that PTAs have on trade. Frankel (1997) finds positive significant effects from Mercosur and negative significant effects from the European Community. Further studies aiming to improve the estimation of PTA effect on trade of which notably Egger et. al (2008) and Baier and Bergstrand (2009) find strong evidence in favor of contemporaneous and long-term trade-creating consequences of trade agreements respectively. Anderson and Yotov (2011) confirm these findings using panel data. Chang and Lee (2011) confirm the direction of the effect of GATT/WTO membership on bilateral trade. More recently Baier et. al (2014) find evidence that both intensive and extensive margins of trade are positively impacted by such materialization of preferentialism, or as they call it economic integration agreements.
The econometric problem with estimating the effects of preferentialism is, however, that PTAs are meant to stimulate trade, and, according to economic theory, concluding PTAs has greater benefits for natural trading partners than otherwise (see Frankel, Stein, and Wei, 1996; Baier and Bergstrand, 2004; Egger and Larch, 2008). While earlier work used a log-linear-index regression approach for the identification of partial PTA treatment effects conditional on observables (see Aitken, 1973; Soloaga and Winters, 2001), more recent work resorted to nonparametric estimation techniques (see, e.g., Egger, Egger, and Greenaway, 2008; Baier and Bergstrand, 2009, Egger and Tarlea, 2017). These

latter papers prominently pave the way for a new cottage literature using non-parametric estimators to evaluate the effect of preferentialism on trade. Egger et. al (2008) use a difference-in-difference matching approach and find particularly strong effects on intra-industry trade. Baier and Bergstrand (2009) use matching on a host of metrics to compare bilateral trade between country pairs with a signed PTA with very similar country pairs without one.[2] The similarity is evaluated on the basis of the standard gravity covariates such as GDP, common language, adjacency and distance. They find that estimates obtained through non-parametric approaches are much more stable across different years and return more economically plausible magnitudes compared to the typically-estimated linear gravity equations.

Although to a great extent non-parametric estimation is relatively flexible and assumes much less than parametric estimators, one potentially problematic assumption is still made. In order to non-parametrically estimate the average treatment effect (ATE) of PTAs, all studies so far implicitly assume treatment homogeneity, that is, any two countries bounded by a PTA have qualitatively an identical status, all else equal. In reality however, no PTA is identical to another. This violates the assumption of treatment homogeneity and leads to inexact inference with regards to the impact of each such PEIA on its respective bilateral trade.

A few studies aim to tackle this issue, by revealing as finely grained as possible PTA-specific characteristics. Famously, Dür et. al (2014) have developed a dataset describing the design of PTAs, covering 587 of them, between 3318 countries (one country can have a PTA with several partners) over 10 sectors during the 1945-2009 period. It has been the most widely used dataset distinguishing between PTAs, with studies suggesting PTAs do have differential impact on trade.[3] The most recent and potentially most exhaustive dataset capturing the heterogeneity of PTAs is introduced by Hoffman et. al (2017) and covers 279 PTAs notified during the period 1958 - 2015, for which 52 different provisions are mapped.

Despite the richness of these afore-mentioned studies, one PTA-specific characteristic has been consistently ignored as a trade determinant by the international trade literature, namely the negotiations duration of each PTA (see

[2]A more extensive discussion on the benefits of non-parametric estimators of trade is provided in the Empirical Strategy section.

[3]Among others Baldwin (2014); Egger et. al (2015); Felbermayr et. al (2015); Bagwell et. al (2016); Kohl et. al (2016).

Table 3.5 in the Tables Appendix 1 for a complete list).[4] This paper aims to fill this gap and unveils a negative robust causal relationship between the suspense (or the duration) of negotiations and the anticipation effect this has on trade between countries directly involved. It is structured as follows: section 1 discusses how negotiations duration may impact trade. Section 2 describes the data capturing trade and negotiations duration. Section 3 explains the estimator choice, introducing the empirical model. Section 4 presents the results and the last section concludes.

3.2 Trade during PTA negotiations

Let us first introduce the conceptual expectations with regards to the potential impact of PTA negotiations duration on trade. The setup is simple. Two non-PTA partnering countries decide to bilaterally negotiate the signing of a trade agreement. As we know from Baier and Bergstrand (2009), similar country and country-pair characteristics that determine trade volumes also determine the decision to enter an agreement. Furthermore, Egger et. al (2011) document that PTAs are virtually never signed among country pairs where there is no trade, allowing us to make the informed assumption that pre-negotiation (or pre-treatment) there is positive trade between the two countries. This means that with non-preferential tariffs, there are still firms that find it profitable to export. There are also firms who don't find it profitable to export so they produce for the domestic market. Following the conclusion of negotiations, there are two potential outcomes: negotiation success leads to a *tariff drop* and negotiation failure leaves *tariffs unchanged*. The question we ask is what happens during negotiations. The uncertainty of what goes on behind the stage at the negotiation table generates a lot of suspense and forces firms to speculate. Firms may interpret each of these two potential outcomes in a *positive* way (leading them accelerate trade) or a *negative* way (and slow down trade). Thus, the start of negotiations generates a 2×2 decision (read speculation) matrix for the average firm, as described below:

[4] One notable exception is Moser and Rose (2012), only they look at negotiations duration the other way around, i.e. aiming to identify its causes.

Perception (trade action)		Tariffs: drop	Tariffs: unchanged
	+	(1.1) Exports will become cheaper	(1.2) Current exporters face no extra competition
	−	(2.1) Current exporters face more competition	(2.2) Exports will remain expensive

Scenario 1.1: Future decrease in tariffs is certainly good for the firm

Lower tariffs would benefit currently-exporting firms by decreasing the so-called iceberg costs. These firms therefore continue to increase the export volumes. Previously non-exporting firms reassess their exporting profitability in the light of the potentially future reduced transport costs, and some of them might begin to trade and incur losses in anticipation that PTA will be enforced and tariffs will drop, by which time they will be in the market already. Overall, export accelerates with the start of negotiations.

Scenario 1.2: Stagnation in tariffs is certainly good for the firm

While exports will not become cheaper for exporting firms, the stagnating tariffs ensure limited competition, as the less productive firms cannot afford to enter the export market. Even if this scenario doesn't play out and tariffs will drop, the more productive firms will be able to lower prices. In anticipation of this, exporters start to trade even more during negotiations.

Scenario 2.1: Future decrease in tariffs is uncertainly good for the firm

Lower tariffs would allow more (less productive firms) to enter the export market. This channel would negatively affect the market for firms currently exporting. Less productive firms that might benefit from lower tariffs cannot yet enter the market. In addition, the beginning of negotiations could alter the sense of predictability for some firms. This leads to reduction in bilateral trade growth (deceleration in trade) or even to a negative growth rate (decrease in trade).

Scenario 2.2: Stagnation in tariffs is uncertainly good for the firm

Exports will remain expensive so until they know for sure that this will be the case, there is no reason for current exporters to accelerate trade.
This paper sheds light on which of these scenarios plays out, or to be more precise, which of the negative and positive anticipation is predominant in terms of change in exports. To do so we rely on the data presented in the next section.

3.3 Data

The dataset that this paper relies on combines a number of different sources and types of data that will be discussed next. Summary statistics of the full sample as well as the estimation sample are reported in a reduced format in Table 3.1 below and in an extended format in Table 3.6 of the Table Appendix 1. We observe complete information for all variables during the period of 1988-2014.
Dependent variable (Exports growth): The dependent variable is based on country-level data on bilateral trade from the United Nations' (UN) Comtrade Database. Since the level of trade is always positive, we would like to capture exporters attitude even when that stays positive only to a lesser degree. We therefore construct year-on-year export growth variable, as the difference in logs between current exports and previous-year exports, and use it as our outcome variable.
Duration of negotiation: The starting point of the analysis are all PTAs notified to and reported by the WTO's PTA-database during the sample period. We use data on beginning and conclusion of negotiations from 1988 to 2008 as collected by Moser and Rose (2014) and updated by Egger, Moser and Tarlea (2017) to include all new PTAs that have been notified to the WTO and were in force by October 15, 2014. The event dates are identified through a careful full-text analysis on LexisNexis, where we mainly focus on international newswires, press releases, and well-known newspapers published in English (see Egger, Moser and Tarlea (2018) for a full account of data collection). In order to quantify the duration of negotiations we subtract the date corresponding to the start of negotiations from the date corresponding to the conclusion of negotiations and calculate the number of days, varying from 316 to 5125.

GDP Growth: Exporter and importer sizes are captured by the value of real GDP and are obtained from the World Bank's World Development Indicators (WDI). The traditional gravity variables (at the country-pair level) are taken from the Centre d'Études Prospectives et d'Informations Internationales' (CEPII) geographical and gravity data-sets. We report summary statistics of the difference in logs between current GDP and previous-year GDP for each exporter and importer.

Exporter - Importer Dissimilarity Growth: We calculate the difference in logs between exporter and importer GDP and log-differentiate its current value and its previous-year value.

Table 3.1: Summary Statistics

	Mean	**Min**	**p95**	**Max**	**N**
	Full sample				
Exports growth	0.088932	-20.8472	2.399181	17.33638	438928
Duration of negotiations	25.17608	0	0	5125	480409
Duration on 0-100 scale	0.513145	0	0	100	459902
1 if negotiating, 0 otherwise	0.062905	0	1	1	480409
Exporter GDP Growth	4.649505	-48.5686	12.06586	760.6343	442013
Importer GDP Growth	4.803759	-89.9624	13.29907	4173.503	422035
X - M Dissimilarity Growth	0.338499	-96.5452	9.997232	1082.782	414149
	Bottom 95th percentile export growth				
Exports growth	-0.11887	-20.8472	1.444262	2.387197	416805
Duration of negotiations	27.19739	0	0	5125	420337
Duration on 0-100 scale	0.557666	0	0	100	399997
1 if negotiating, 0 otherwise	0.070484	0	1	1	420337
ExporterGDP Growth	4.393599	-48.5686	11.1783	760.6343	412883
Importer GDP Growth	4.5318	-89.9624	12.37288	4173.503	394374
X - M Dissimilarity Growth	0.303096	-96.5452	9.610764	1082.782	387298
	Bottom 95th percentile export growth (treated only)				
Exports growth	0.052716	-7.80652	0.648054	2.384346	9287
Duration of negotiations	1230.975	3	3193	5125	9287
Duration on 0-100 scale	24.01903	0.058537	62.30244	100	9287
1 if negotiating, 0 otherwise	1	1	1	1	9287
Exporter GDP Growth	2.937639	-17.5788	8.354553	27.8607	9287
Importer GDP Growth	2.949098	-62.0759	8.59375	77.2011	9287
X - M Dissimilarity Growth	0.11658	-36.452	6.486697	199.5458	9287

A more detailed version of this table is available in Table 3.6 in the Table Appendix 1.

3.4 Empirical strategy

The aim of our paper is estimating the average marginal effect (or average treatment effect, henceforth ATE) of a PTA while accounting for a specific kind of PTA heterogeneity, namely its respective negotiation duration. The standard approach in the last decades for estimating PTA effect on trade has been to employ a log-linear form of the gravity equation and theoretically interpret the coefficient on the PTA dummy as the reduced form from a general equilibrium model, as per Eaton and Kortum (2002) or van Wincoop (2003). However, due to likely non-linearities in the data, combined with the potential argument for countries self-selecting into trade agreements, OLS estimation of PTA effects may suffer from a bias. The non-parametric estimators come to the rescue by accommodating any form of relation between outcome and explanatory variable, be it non-linear, as well as removing the concern of non-random selection of country pairs into a PTA (or in the treatment evaluation jargon - non-random assignment of treatment).

3.4.1 Non-parametric estimation

With that in mind, we would proceed to calculating the ATE of PTAs (and later negotiations duration thereof) as the difference between the average outcome (i.e. export growth) of country pairs during PTA negotiations and average outcome for those same pairs not negotiating a PTA. However, the main obstacle of the non-parametric techniques is that these two outcomes can *never* be observed simultaneously. The next best is calculating ATE as the difference between average export growth of negotiating country pairs and average export growth of non-negotiating country pairs. However, unless country pairs are randomly assigned to negotiating PTAs, this estimated ATE suffers from a self-selection bias. Rosenbaum and Rubin (1993) introduce propensity score matching as a way to correct for this bias, implying the comparison of units similar in terms of observable characteristics. However, applying their model to our data structure would mean ignoring the treatment heterogeneity (i.e. remember we want to account for country pairs taking different amounts of time to conclude a PTA negotiations). Imbens (2000) extends the binary case to categorical multivalued treatment and finally Hirano and Imbens (2004) extend it to continuous multivalued treatments. Following Rosenbaum and Rubin (1983), they make an unconfoundedness assumption, which allows them to remove all biases in comparisons by treatment status by adjusting for differences in a set of covariates. Then they define a generalization of the propensity score for the binary case - henceforth labeled generalized propensity score (GPS) - which has many of the attractive properties of the binary-treatment propensity score. The one shortcoming of this model is that it relies on the assumption that the treatment intensity d conditional on covariates X, $d|X$ is drawn from a normal distribution. With data structures such as ours, where many units are not treated, there is a non-zero probability mass at zero, i.e. $Pr(d = 0) > 0$. The Hirano and Imbens (2004) model is therefore untenable for estimating the ATE relative to a non-treated base, with the spike of the distribution of zero suggesting discontinuity, thus violating the normality assumption.

3.4.2 ATE with a continuous (as opposed to binary) treatment: the Dose-Response function

As an extension to the Hirano and Imbens (2004) model, Cerulli (2014) proposes an econometric model for estimating dose response-function through a regression approach when treatment is continuous. Since the model works within a control function approach, it does not need to specify a GPS. Compared with Hirano and Imbens (2004), the model does not need a full normality assumption and is well-suited to accommodate many untreated units. Specifically, it models the dose response-function as approximated by a third degree polynomial. In our context, if we imagine a country pair not negotiating a PTA as the control unit, a country pair starting to negotiate a PTA as the treated unit, and the number of days it takes until it reaches an agreement as the intensity or the dose of treatment it receives, we can model the yearly growth rate in bilateral exports as a dose response-function. This application we introduce next.

Let X be a vector of confounding factors in the export growth equation consisting of exporter GDP growth, importer GDP growth, and the growth of the dissimilarity between exporter GDP and importer GDP. Following the continuous treatment approach, and assuming a parametric form for the unit response function $g(X)$ to the vector of confounding X as $g_0(X) = X\delta_0$ and $g_1(X) = X\delta_1$ we start with a potential outcome model adapted to the context of continuous treatment:

$$
\begin{aligned}
w = 1 &: Y_1 = \alpha_1 + \delta_1 X + h(d) + \epsilon_1 \\
w = 0 &: Y_0 = \alpha_0 + \delta_0 X + h(d) + \epsilon_0
\end{aligned}
\tag{3.1}
$$

where d is the intensity of the treatment captured by the duration of negotiations, and $w = \mathbf{1}[d > 0]$ is the binary treatment indicator that equals 1 for the country pairs observed during negotiations. We code all country pairs that never enter or have not entered yet a negotiation for a PTA as control units and all country pairs that enter negotiation as treated units. Country pairs that have concluded negotiations (i.e. are already in an enforced PTA) are excluded from the sample

$$
w_{ijt} = \begin{cases} 0, & \text{if } PTA_{ijt} = 0 \quad \& \quad ij \text{ are not negotiating} \\ 1, & \text{if } PTA_{ijt} = 0 \quad \& \quad ij \text{ are negotiating} \\ -, & \text{if } PTA_{ijt} = 1 \end{cases}
\tag{3.2}
$$

Y_1 and Y_0 are the two mutually exclusive potential outcomes for a particular subject and $\delta_1 X$ and $\delta_2 X$ are the subject's response to the vector of observed confounding variables X when the subject is treated and untreated, respectively. Finally, $h(d)$ is a flexible function of the treatment level.
We can therefore formulate the average treatment effect (ATE) as

$$
\begin{aligned}
ATE(X, d) = E(Y_1 - Y_0) &= \begin{cases} (\alpha_1 - \alpha_0) + X(\delta_1 - \delta_0) + h(d) & if \quad d > 0 \\ (\alpha_1 - \alpha_0) + X(\delta_1 - \delta_0) & if \quad d = 0 \end{cases} \\
&= \begin{cases} \alpha + X\delta + h(d) & if \quad d > 0 \\ \alpha + X\delta & if \quad d = 0 \end{cases}
\end{aligned}
\tag{3.3}
$$

thereby getting

$$
\begin{aligned}
ATE(X,d,w) &= \begin{cases} ATE(X,d>0) & if \quad d>0 \\ ATE(X,d=0) & if \quad d=0 \end{cases} \\
&= I(d>0)[\alpha + X\delta + h(d)] + I(d=0)[\alpha + X\delta] \\
&= w[\alpha + X\delta + h(d)] + (1-w)[\alpha + X\delta]
\end{aligned}
\tag{3.4}
$$

By averaging on x, d, w we obtain

$$
ATE = \frac{N_T}{N}(\alpha + \overline{X}_{d>0}\delta + \overline{h}_{d>0}) + \frac{N_{NT}}{N}(\alpha + \overline{X}_{d=0}\delta) \tag{3.5}
$$

By definition $ATE = p(w=1) \times ATET + p(w=0) \times ATENT$ so the analytical form based on Equation 3.5 for each treatment effect is

$$
\begin{cases}
ATE = p(w=1)(\alpha + \overline{X}_{d>0}\delta + \overline{h}_{d>0}) + p(w=0)(\alpha + \overline{X}_{d=0}\delta) \\
ATET = \alpha + \overline{X}_{d>0}\delta + \overline{h}_{d>0} \\
ATENT = \alpha + \overline{X}_{d=0}\delta
\end{cases}
\tag{3.6}
$$

Then by simple algebra we obtain

$$
\begin{cases}
ATE = p(w=1)(\alpha + \overline{X}_{d>0}\delta + \overline{h}_{d>0}) + p(w=0)(\alpha + \overline{X}_{d=0}\delta) \\
ATET(x,d) = ATE(x,d,w=1) = ATET + (X_{d>0} - \overline{X}_{d>0})\delta + (h(d) - \overline{h}_{d>0}) \\
ATENT = \alpha + \overline{X}_{d=0}\delta
\end{cases}
\tag{3.7}
$$

and we can define the Dose Response-function by averaging $ATE(X,d)$ over X:

$$
ATE(d,w) = E_X\{ATE(X,d,w)\} = w \times [ATET + h(d) - \overline{h}_{d>0}] + (1-w) \times ATENT \tag{3.8}
$$

so

$$ATE = \begin{cases} ATET + h(d) - \overline{h}_{d>0} & if \quad d > 0 \\ ATENT \quad if \quad d = 0 \end{cases} \tag{3.9}$$

In order to consistently estimate the causal parameters, we start from the potential outcome model as formulated in Equation 3.1:

$$Y_0 = \alpha_0 + \delta_0 X + \epsilon_0$$
$$Y_1 = \alpha_1 + \delta_1 X + h(d) + \epsilon_1$$

We can rewrite the observable outcome $y = y_0 + w(y_1 - y_0)$ as

$$Y = Y_0 + w(Y_1 - Y_0) = (\alpha_0 + \delta_0 X + \epsilon_0) - w(\alpha_1 + \delta_1 X + h(d) + \epsilon_1) - w(\alpha_0 + \delta_0 X + \epsilon_0)$$
$$= \alpha_0 + w(\alpha_1 - \alpha_0) + -\delta_0(X) + w[\delta_1(X) - \delta_0(X)] + wh(d) + \epsilon_0 + w(\epsilon_1 - \epsilon_0)$$

By adding and subtracting $w\overline{X}\delta$ and $w\overline{h}$ we get

$$Y = \alpha_0 + w[(\alpha_1 - \alpha_0) + \overline{X}\delta + \overline{h}] + X\delta_o + w(X - \overline{X})\delta + w(h(d) - \overline{h}) + \epsilon_0 + w(\epsilon_1 - \epsilon_0) \tag{3.10}$$

By assuming Conditional Mean Independence (CMI), namely that given X both w and d are endogenous in Equation 3.10 we can rewrite the regression line for Y as

$$E(Y|X, w, d) = \alpha_0 + X\delta_0 + w \underbrace{[(\alpha_1 - \alpha_0) + \overline{X}\delta + \overline{h}]}_{ATE} + w(X - \overline{X})\delta + w(h(d) - \overline{h}) \tag{3.11}$$

as CMI implies that $E[\epsilon_0 + w(\epsilon_1 - \epsilon_0)|X, w, d] = E[\epsilon_0 + w(\epsilon_1 - \epsilon_0)|X] = 0$.[5] We end up estimating the following regression equation:

[5]See Cerulli (2014) for proof that $ATE = (\alpha_1 - \alpha_0) + \overline{X}\delta + \overline{h}$.

$$E(Y|X,w,d) = \alpha_0 + X\delta_0 + wATE + w(X - \overline{X})\delta + w(h(d) - \overline{h}) \quad (3.12)$$

Under CMI we assume a third-degree polynomial for the treatment intensity function $h(d)$ in the form of $h(d) = ad + bd^2 + cd^3$ which we plug into Equation 3.12 which becomes

$$\begin{aligned} E(Y|X,w,d) &= \alpha_0 + X\delta_0 + wATE + w(X - \overline{X})\delta + \\ &+ w[ad + bd^2 + cd^3 - (aE(d) + bE(d^2) + cE(d^3)] \\ &= \alpha_0 + X\delta_0 + wATE + w(X - \overline{X})\delta + \\ &+ a[t - E(t)]w + b[t^2 - E(t^2)]w + c[t^3 - E(t^3)]w \end{aligned} \quad (3.13)$$

Assuming CMI, we can consistently estimate parameters $\hat{\alpha_0}$, $\hat{\delta_0}$, $A\hat{T}E$, $\hat{\delta}$, $\hat{a}$, $\hat{b}$ and $\hat{c}$ in Equation 3.13 using least-squares regression. Finally, on the back of these parameters we can estimate the Dose Response-function of this form:

$$\begin{aligned} \widehat{ATE}(d_{ij}) = w[\widehat{ATET} + \widehat{a}(d_{ij} - \frac{1}{N}\sum_{\substack{i=1 \\ j=1}}^{N} d_{ij}) + \overline{b}(d_{ij}^2 - \frac{1}{N}\sum_{\substack{i=1 \\ j=1}}^{N} d_{ij}^2) + \\ + \overline{c}(d_{ij}^3 - \frac{1}{N}\sum_{\substack{i=1 \\ j=1}}^{N} d_{ij}^3)] + (1-w)\widehat{ATENT} \end{aligned} \quad (3.14)$$

In order to obtain the analytical standard errors we define $T_1 = t - E(t)$, $T_2 = t^2 - E(t^2)$ and $T_3 = t^3 - E(t^3)$. Then it follows that the standard error of the Dose Response-function is

$$\widehat{\sigma}_{\widehat{ATE}(d)} = \{T_1\widehat{\sigma}_a^2 + T_2\widehat{\sigma}_b^2 + T_2\widehat{\sigma}_c^2 + 2T_1T_2\widehat{\sigma}_{a,b} + 2T_1T_3\widehat{\sigma}_{a,c} + 2T_2T_3\widehat{\sigma}_{b,c}\}^{\frac{1}{2}} \quad (3.15)$$

which gives us the 95% confidence interval of $\widehat{ATE}(d)$ for each d

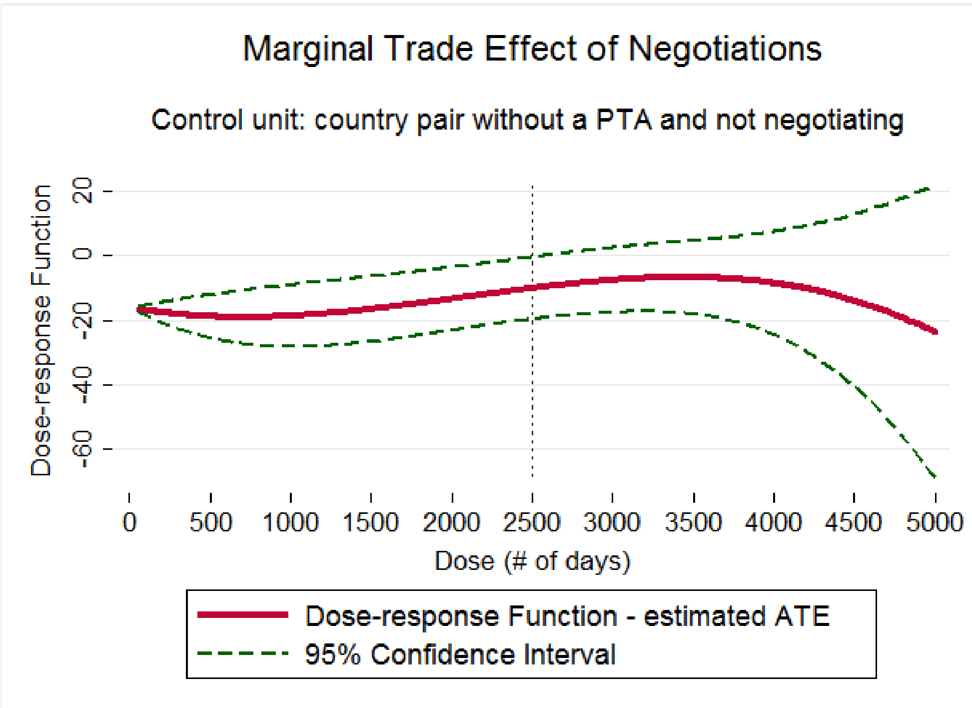
$$\{\widehat{ATE}(d) \pm 1.96\widehat{\sigma}_{\widehat{ATE}(t)}\} \quad (3.16)$$

In the forthcoming Results section we report $\widehat{ATE}(d)$ as an average over all dosage levels (i.e. durations of PTA negotiations) as well as a breakdown thereof. In addition we present results for $\widehat{ATET}(d)$ to which we separately construct 95% confidence interval using bootstrapped standard errors.

3.5 Results

Figure 3.1 shows the dose-response function (DRF), in particular the response of export growth to different doses of treatment (i.e. negotiation duration) compared to the untreated country pair units (i.e. those who don't negotiate and have no PTA). The negative effect of negotiations *increases* in magnitude from 18 percentage points in the first year to about 20 in the second and third year. After that negotiations taking from 4 up to 7 years have a smaller yet still negative effect on export growth of around 11 percentage points. The effect becomes even smaller later but after year 7 it is no longer statistically significant at 95% confidence level. One might say that after a long enough period, parties no longer react to the suspense of negotiations in a systematic manner.

Figure 3.1: Comparison to Non-negotiators



At this point it is worth making a few clarifications with relation to how these results reconcile with the literature on PTA effects on trade. Firstly, and most importantly, this paper does not deal with the effect of the enforcement of a PTA. This would imply a reduction of tariff already in place, and we know from a host of papers cited in the introductory section that PTAs or their implicit tariff reduction enhance trade. What this paper evaluates is the behavior of countries (or firms) facing uncertainty with regards to tariffs. Temporally this translates in distinguishing between trade changes *after* negotiations and *during* negotiations - when dealing with the suspense is an additional factor in the exporting decision process. The second point is that between two naturally trading partners (as theory predicts 2 countries negotiating a PTA to be), trade likely increases with time. For that reason, we look at the rate at which trade increases, and whether this is impacted by the length of negotiations. A negative coefficient estimated for export growth rate may or may not thus imply less trade, but it will certainly imply a deceleration of trade. Importantly, these results are not indicative of an actual slowdown in trade during negotiations in absolute terms. The economic performance (GDP growth) of each country in the pair and how they converge in size with each other are important drivers of bilateral exports. The story that the negative coefficients on the negotiation duration say is that once we abstract away the positive effect of economic growth and country-pair convergence, longer duration of negotiation undermines trade growth.

Table 3.2: Negotiations duration effect on bilateral export growth

		OLS		**DRF**
	M1	**M2**	**M3**	**M4**
	b/se	b/se	b/se	b/se
Duration of negotiations	-0.006***			
	0			
Duration on 0-100 scale		-0.358***		
		-0.05		
1 if negotiating, 0 otherwise			-22.536***	-13.143***
			-0.88	-4.05
Exporter GDP Growth	0.755***	0.699***	0.722***	0.688***
	-0.04	-0.04	-0.04	-0.04
Importer GDP Growth	0.088***	0.079***	0.081***	0.077***
	-0.02	-0.02	-0.02	-0.02
X - M Dissimilarity Growth	-0.450***	-0.434***	-0.443***	-0.427***
	-0.04	-0.04	-0.04	-0.04
N	387439	367406	387437	367404

*** p<0.01, ** p< 0.05 * p<0.1; robust standard errors reported under coefficients; .

We run multiple regressions: M1 of Table 4.2 shows results from OLS on the duration (variable ranging from 3 to 5125 days). M2 shows results from least-squares regression using a scaled measure of duration, ranging from 0-100.

M3 shows results for OLS on binary treatment variable that takes value 1 for country pairs during PTA negotiations and zero for all the remaining country pairs (including those who never negotiate and those who have not started yet). M4 shows the ATE using the DRF method (or a control function) allowing for different levels of treatment. The displayed value is the ATE averaged in turn over all levels of treatment. The interpretation is therefore that the effect of an average negotiation duration dose (with the 0-100 dose covering negotiations up to 5125 days, a one-unit dose is approximatrly 16 months) compared to country pairs who do not negotiate is to reduce trade growth by 13%. To exemplify, the same country pair trading 50% more this year compared to last year, should it hypothetically be engaged in PTA negotiations for 16 months, it would only increase its trade by $50 - (50 \times 0.13) = 43.5\%$.
In order to better visualize the ATE of different durations of PTA negotiations on exports growth, we tabulate the ATE for the decile values of the treatment as displayed by Figure 3.1. The estimated coefficients represent the average effect of a PTA having been negotiated for X number of days on the bilateral export growth between the two countries involved, or more formally, when the dose response-function is evaluated at different dose levels. When evaluated at the dose level of 10 (approximately equivalent to 500 days of negotiation), the estimated ATE coefficient suggests duration decreases export growth rate by 19 percentage points, a result that is highly statistically significant at 1% significance level. As negotiations continue, we can observe the magnitude as well as the statistical significance diminishing to 11 percentage points at 2500 days of negotiation and only significant at 10% significance level. Beyond that, duration has no longer a statistically significant effect different from 0.
Implicitly, the comparison group for results in column 1 consists of all country pairs that do not negotiate in that year. One might wonder however about the relative ATE when comparing negotiators to country pairs that have only been negotiating for a short while. We therefore construct an alternative control group consisting of those country pairs that have only been negotiating for a maximum of 485 days (the equivalent of the bottom 5th percentile in negotiations duration) and run the same dose response-function procedure. Column 2 of Table 3.3 shows the results obtained this way. Magnitudes are consistently larger at almost all dosage levels when using short-time negotiators as control group instead of non-negotiators. This result could be explained by the fact that the first one or two years of negotiations (period captured by

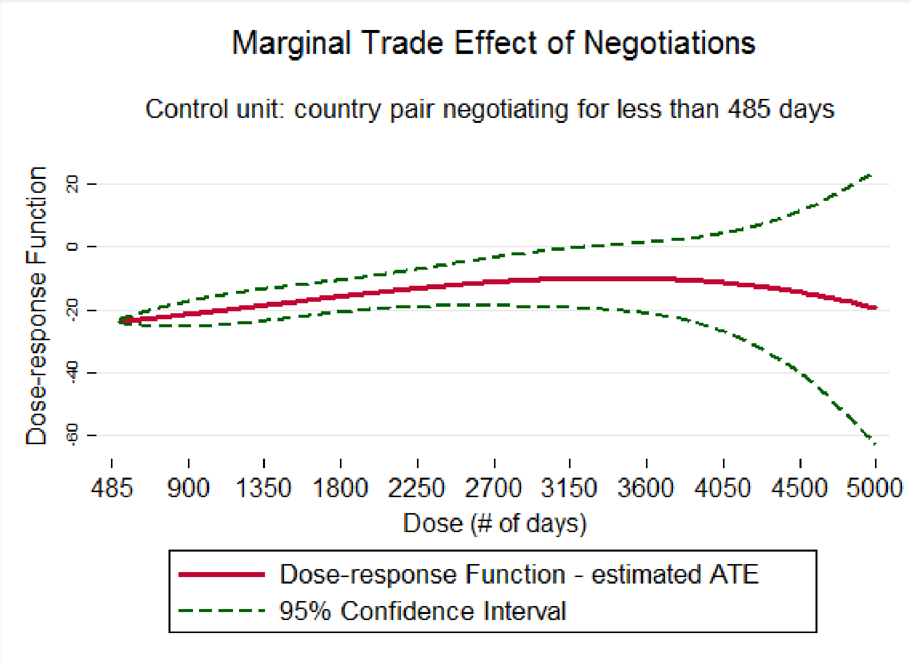
Table 3.3: DRF: Negotiations duration ATE on bilateral exports growth

X	ATE evaluated at X level of treatment Control group: duration in days 0	 < 485 (5th percentile)
10	-18.8780***	-21.4104***
30	-17.2717***	-15.6811***
50	-10.0725*	-11.1175***
70	-6.8100	-9.9827*
90	-14.0248	-14.5399
N treated	9210	8656
N untreated	358,194	554
N	367,404	9210

*** p<0.01, ** p< 0.05 * p<0.1; standard errors in parantheses; Results are obtained using the Dose Response Function model; all estimation models include the same covariates listed in Table 4.2.

the dosage of the newly-created control group) systematically give subjects in matter a positive perception about the international trading scene. This leads to faster trade growth during that period. Consequently, when comparing the mean trade growth in the treatment group (negotiations > 485 days) with that in the new control group, the difference is larger than in the absence of such positive perception, as is the case in the old control group. The bigger picture of this result can be observed in Figure 3.2, where the interval of duration with an estimated effect that is statistically significant is more apparent, ranging between dose levels of about 5 to 25, or 250 to 625 days.

Figure 3.2: Comparison to Short-time Negotiators

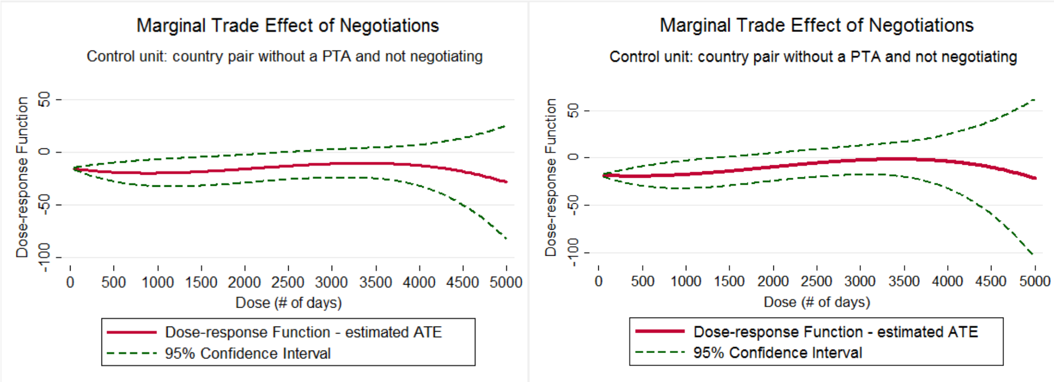


Negotiating a PTA implies at least two countries trying to reach those terms generating most benefits while compromising the least. The position at the negotiation table is rarely equitable (see for example Bagwell and Staiger, 2011). The imbalance is often generated by each country's economic strength. We therefore proceed to testing whether country imbalances generate different effects of negotiations duration on export growth. We calculate the discrepancy between importer and exporter GDPs as one's share to the other and estimate the dose response-function of negotiations duration for the its 100th, 95th, 75th, and 50th percentile. In the context of a negotiation between two countries of relatively different sizes, it would be informative to see in which direction does the negatively-impacted trade growth go, from the richer to the poorer or vice-versa. [6]

Looking at Figure 3.3 above, we see that when the exporter is smaller than the importer (X<M), the magnitude of ATE is only significantly negative until dose level 30 (approximately 4 years of negotiation), while it is of larger magnitude and significantly negative until dose level 55 (approximately 7 years) when the exporter is larger (X>M). This suggests that prolonged negotiations

[6]The percentile values correspond to the factors of 508512, 597, 24, and 2 for exporters larger than importers and 10074, 769, 45, and 9 for exporters smaller than importers respectively.

Figure 3.3: Dissimilarity at the Negotiation Table



Exporter > Importer   Importer > Exporter

have on average a larger detrimental effect on export growth for the larger countries.

However, the results reported in Table 3.4 show that for the average treatment effect on the treated (ATET), i.e. the average effect on countries negotiating only the opposite is true. The results indicate one clear pattern: the magnitude of the deceleration effect of negotiations on exports is larger when the exporter is smaller than the importer. The trade balance of the smaller country is therefore impacted more drastically by the prolonged PTA negotiations.

Furthermore, as we move from the full sample (100th percentile) to the sub-sample of country pairs in the bottom half of dissimilarity (or the most similar 50%) the effect is consistently reduced, yet still statistically significant at 1% level. This suggests that prolonged negotiations have a smaller effect on trade growth between more similar countries.

We run a series of robustness checks, which reinforce the sturdiness of our base results that, on average, long negotiations slow down trade, and present the tabulated results in Table Appendix 2. Table 3.6 shows the ATE of an increase in treatment dose level, namely of number of days of negotiations. We report values for 500-day increments from 500 to 4500. Furthermore, we address the concern of a phasing-in effect. The act of observing the duration of negotiations between two countries today could potentially only impact trade patterns tomorrow. Of course, since our data is observed yearly, this issue is not so pressing. We include a one-year lead of the outcome variable nevertheless, run similar specifications as in Table 3.3, and report results in Table 3.7.

Table 3.4: Negotiations duration ATE on bilateral exports growth

	Exporter - Importer Dissimilarity < x percentile							
x	**100th**		**95th**		**75th**		**50th**	
	X>M	**X<M**	**X>M**	**X<M**	**X>M**	**X<M**	**X>M**	**X<M**
ATET	-14.89	-17.47	-15.34	-17.45	-14.88	-17.45	-12.84	-16.49
ATE	-13.27	-8.60	-13.62	-8.44	-12.50	-7.97	-7.20	-6.86
N treated	4861	4345	4858	4344	4472	3902	3394	2786
N control	214149	142792	202457	136514	156627	106647	99780	70169
N total	219010	147137	207315	140858	161099	110549	103174	72955

All coefficients are significant at 99% confidence level. Estimation sample restricted to the export growth top 95th percentile. Confidence intervals are constructed based on estimated robust standard errors for ATE and bootstrapped standard errors obtained through 100 draws with replacement for ATET and ATENT. X denotes exporter GDP and M denotes importer GDP.

3.6 Conclusions

The main focus of the international economics literature is identifying the factors that shape world trade patterns, and estimating the effect thereof. This paper directly adds to it. In particular, the contribution is threefold: firstly, it sheds light on a feature of preferential trade agreements (PTAs) relatively ignored in the literature - the duration of their negotiation - and investigates it as a potential trade determinant. This helps to further disentangle the heterogeneous PTAs. Secondly, as opposed to evaluating PTA effects after enforcement, this paper focuses on PTA *anticipation* effects, by looking at the period prior to the enforcement, i.e. during negotiation. Thirdly, it makes use of an estimation technique that accommodates continuous treatment as well as non-linearities between outcome and explanatory variables, never before used to estimate trade determinants, and provides clear and robust results of a negative effect of PTA negotiation duration on trade growth.

It is important to clarify the meaning of this result. The negative and statistically significant relation between negotiation duration and trade growth is by no means indicative of a slowdown in exports in absolute terms, or even stronger - a drop therein - every time there is a PTA negotiation. What this result reveals is that two countries negotiating a trade agreement will on average increase their exports to each other by a lower rate than, hypothetically, the exact same two countries, should they not negotiate. This result therefore

doesn't contradict the strong positive marginal effects of trade agreements on trade documented by the literature (Egger et. al, 2009; Baier and Bergstrand, 2009; Anderson and Yotov, 2011; Chang and Lee, 2011; Baier et. al; (2014); Egger and Tarlea, 2017). So a conclusion we should *not* draw from this study is that trade agreements are detrimental to trade. Conversely, the policy implication we would highlight is that lengthier negotiations lead to a marginal drop in the growth rate of trade due to the suspense (or uncertainty) that governs this period. To the extent of our knowledge, there is no prior empirical or theoretical attempt to investigate trade growth during PTA negotiations.
While shortening PTA negotiations is not immediately clear from this paper to be the optimal long-run strategy for policymakers, their duration is certainly not of negligible impact to trade. Furthermore, future work estimating PTA effect on trade should estimate and discount the (trade) expenses of that PTA that were spent during negotiations, before concluding on the marginal benefit once negotiations had succeeded.
Several extensions of this paper are possible. Before explicitly suggesting that shortening PTA negotiations is certainly beneficial, clarifying another issue would be in order. Do longer negotiations actually lead to deeper PTAs, which in turn ensure a relatively larger boost in trade compared to shallower ones. Clarifying this would require extending the current result with a statistical test comparing the potential trade lost during negotiations and the trade gained once the PTA is in place. Additionally, in the future we will evaluate third party effects of PTA negotiation, namely on trade growth between exporter (and in turn importer) and non-PTA members, PTA members, different-PTA members, and future-PTA members. All these should complement the picture of the PTA negotiations duration effect on shaping international trade patterns and shed further light on this relatively ignored period in the life cycle of a PTA.

Table Appendix 1

Table 3.5: PTAs and their negotiation duration

Name of PTA	PTA Negotiation Duration (# of days)
Pan-Arab Free Trade Area (PAFTA)	316
ASEAN Free Trade Area (AFTA)	451
Common Economic Zone	452
New Zealand - Singapore	478
Mercosur	485
Canada - Chile	554
China - Hong Kong	557
US - Jordan	559
EFTA - Croatia	560
Thailand - New Zealand	620
Chile-Japan	623
EFTA - Korea	630
EC - Croatia	677
EFTA - Singapore	691
EFTA - Mexico	757
Japan - Singapore	769
Canada - Israel	770
Turkey - Tunisia	774
US - Australia	779
EEC/EC/EU enlargment Finland	788
Jordan - Singapore	789
Pakistan - China	816
Australia - Chile	847
China - Singapore	860
Korea - Singapore	861
EEC/EC/EU enlargment Sweden	884
Panama - Singapore	888
India - Singapore	909
Canada - Peru	921
Chile - India	939
Japan - Malaysia	945

ASEAN - China (S)	969
Peru - Korea	983
Singapore - Australia	985
Canada - Costa Rica	1005
Chile - Colombia	1016
EFTA - Serbia	1033
Pakistan - Malaysia	1049
Japan - Indonesia	1126
US - Chile	1127
US - Singapore	1141
Korea - Turkey	1148
CAFTA	1148
US - Bahrain	1168
Japan - Peru	1193
North American Free Trade Agreement (NAFTA)	1195
Israel - Mexico	1229
EEC/EC/EU enlargment Austria	1250
EEC/EC/EU enlargment Austria/Sweden/Finland	1250
TPSEP	1309
Turkey - Chile	1313
India - Malaysia	1316
Chile - China	1340
US - Morocco	1349
Australia - Thailand	1366
Japan - Viet Nam	1389
Japan - Mexico	1396
Hong Kong - Chile	1403
China - New Zealand	1412
Switzerland - China	1418
Japan - Thailand	1421
SAPTA	1447
EFTA - Morocco	1454
EU - Colombia and Peru	1471
EFTA - Chile	1472
European Economic Area (S)	1474
EFTA - Lebanon	1481

ASEAN - China	1500
US - Oman	1508
EC - Mexico	1510
Canada - Colombia	1530
Peru - Chile	1623
EEC/EC/EU enlargment Latvia et. Al	1662
EC - Chile	1686
India - Japan	1689
Singapore - Peru	1716
EFTA - Colombia	1730
Pakistan - Sri Lanka	1746
EU - Serbia	1756
Gulf Cooperation Council (GCC)	1777
EFTA - SACU	1806
EU - Korea	1870
ASEAN - Japan	1881
US - Peru	1902
Canada - Jordan	1907
EC - South Africa	1909
Japan - Switzerland	1912
Chile - Malaysia	1923
New Zealand - Malaysia	1949
Korea - Chile	1963
CEFTA - Croatia	1995
Korea - US	2294
GCC - Singapore	2470
EEC/EC/EU enlargment Cyprus et. Al	2481
EC - Jordan	2515
Chile - Vietnam	2602
EC - Lebanon	2650
EC - Israel	2799
Malaysia - Australia	2826
Egypt - Turkey	3006
US-Colombia	3101
EC - Cote d'Ivoire	3193
US-Panama	3270

India - Sri Lanka	3299
EFTA - Tunisia	3432
EEC/EC/EU enlargment Bulgaria/Romania	3456
EC - Egypt	3526
Asia Pacific Trade Agreement (APTA) - China	3664
Hong Kong - New Zealand	3706
EU (28) Enlargement	3791
EFTA - GCC	4188
EFTA - Canada	4227
EFTA - Egypt	4254
EFTA (S)	4396
SAFTA	5125

	Full sample							
	Mean	Min	p25	Median	p75	p95	Max	N
Exports growth	0.088932	-20.8472	-0.32484	0.078133	0.492066	2.399181	17.33638	438928
Duration of negotiations	25.17608	0	0	0	0	0	5125	480409
Duration on 0-100 scale	0.513145	0	0	0	0	0	100	459902
1 if negotiating, 0 otherwise	0.062905	0	0	0	0	1	1	480409
Yearly X GDP Growth	4.649505	-48.5686	1.777778	3.816794	6.086957	12.06586	760.6343	442013
Yearly M GDP Growth	4.803759	-89.9624	1.714286	3.875969	6.352941	13.29907	4173.503	422035
X - M Disimilarity Growth	0.338499	-96.5452	-3.06895	-0.03819	3.114926	9.997232	1082.782	414149
	bottom 95th percentile export growth							
Exports growth	-0.11887	-20.8472	-0.36287	0.049277	0.39508	1.444262	2.387197	416805
Duration of negotiations	27.19739	0	0	0	0	0	5125	420337
Duration on 0-100 scale	0.557666	0	0	0	0	0	100	399997
1 if negotiating, 0 otherwise	0.070484	0	0	0	0	1	1	420337
Yearly X GDP Growth	4.393599	-48.5686	1.744186	3.768844	5.995204	11.1783	760.6343	412883
Yearly M GDP Growth	4.5318	-89.9624	1.699996	3.831667	6.235997	12.37288	4173.503	394374
X - M Disimilarity Growth	0.303096	-96.5452	-3.02832	-0.04541	3.057272	9.610764	1082.782	387298
	bottom 95th percentile export growth (treated only)							
Exports growth	0.052716	-7.80652	-0.08473	0.072394	0.220374	0.648054	2.384346	29606
Duration of negotiations	1230.975	3	446	1105	1777	3193	5125	9287
Duration on 0-100 scale	24.01903	0.058537	8.702439	21.56098	34.67317	62.30244	100	9287
1 if negotiating, 0 otherwise	1	1	1	1	1	1	1	9287
Yearly X GDP Growth	2.937639	-17.5788	1.317778	2.831858	4.672897	8.354553	27.8607	9287
Yearly M GDP Growth	2.949098	-62.0759	1.269841	2.851324	4.755784	8.59375	77.2011	9287
X - M Disimilarity Growth	0.11658	-36.452	-2.14878	-0.05439	2.111943	6.486697	199.5458	9287

Tables Appendix 2

Table 3.6: Negotiations duration ATE on bilateral exports growth

Δ	**ATE(t;Δ)**	**Std. Dev.**	**P>t**	**95% confidence interval**	
10	2.6106	1.1562	0.0240	0.3445	4.8768
20	2.4066	2.4891	0.3340	-2.4719	7.2851
30	0.3013	3.4489	0.9300	-6.4585	7.0611
40	-2.7917	3.0415	0.3590	-8.7529	3.1695
50	-5.9590	1.4468	0.0000	-8.7947	-3.1233
60	-8.2871	1.9604	0.0000	-12.1295	-4.4448
70	-8.8626	3.4327	0.0100	-15.5906	-2.1346
80	-6.7719	5.1170	0.1860	-16.8010	3.2571
90	-1.1017	7.5573	0.8840	-15.9136	13.7103

Estimation sample restricted to the export growth top 95th percentile. Δ denotes the increment in dose level and is evaluated at levels from 10 to 90, corresponding to duration of negotiations from 500 to 4500 days.

Table 3.7: Negotiations duration ATE on bilateral exports growth

	τ-year lead of export growth	
τ	**1**	**2**
ATE	-18.500***	-25145
ATET	-16.93***	-15.56***
ATENT	-18.52***	-15.22***

*** p<0.01, ** p< 0.05 * p<0.1.
Estimation sample restricted to the export growth top 95th percentile. Estimation period restricted to one and two years before conclusion of negotiations respectively. Confidence intervals are constructed based on estimated robust standard errors for ATE and bootstrapped standard errors obtained through 100 draws with replacement for ATET and ATENT.

Chapter 4

Winners and Losers from Trade Liberalization: A Global Capital Market Perspective[1]

4.1 Introduction

Multilateral trade liberalization under the auspices of the World Trade Organization (WTO) has stalled over the last two decades. However, this does not mean that trade liberalization at large has stalled. By way of contrast, since around the time of the Uruguay Round, we have seen the hitherto biggest surge in the signing and implementation of preferential trade agreements (PTAs), which permit a deviation from the most-favored-nation rule by granting preferential market access among a set of specified countries. Figures published by the WTO show that there are more than 400 such agreements in force to date.

While PTAs have potentially negative consumer-welfare effects (see Viner, 1950), even for PTA-member countries, a host of empirical work suggests that, on average, the more or less reciprocal reduction of bilateral policy barriers within PTAs tends to increase a country's real income and consumption (see for instance Freund and Ornelas, 2010, for a survey). The reason is that consumer prices are reduced and the associated increase in cross-border sales

[1]This chapter is based on Egger, Moser and Tarlea (2018)

and consumption has been documented to outweigh the eventual losses of domestic sellers and of diversion of sales to and from from countries outside trade agreements (see Krugman, 1991). However, the associated work largely focused on actual tariff reductions and associated responses in overall and cross-border sales as well as in prices, but it largely abstracts from economic effects of expectations as, e.g., reflected in firms' stock market prices. While there is some evidence on the latter with respect to individual PTAs and the associated countries, there is a lacuna of knowledge regarding the average effects and their distribution across the board of countries.

This paper contributes to the still small empirical literature that investigates PTA effects through the lens of the stock market. On the one hand, there is a small number of studies that typically analyze individual stock market reactions in one country for a given PTA (see Thompson, 1993, 1994; Rodriguez, 2003; Breinlich, 2014). These papers exploit the Canada-United States Trade Agreement (CUSFTA) and the North American Free Trade Agreement (NAFTA). On the other hand, Moser and Rose (2014) investigate the overall stock market reaction to important PTA news for a large number of countries and 120 PTAs. But to date there is no systematic evidence at the individual (firm-specific) stock market level for a large number of countries and PTAs. The present paper fills this void by documenting the heterogeneity of stock market returns across PTAs, countries, sectors, and firm types.

To accomplish this task, we combine a number of different sources and types of data. In particular, we consider all new PTAs that had been signed between 1988 and mid-October 2014 as notified to the WTO and (at least some of) whose member countries daily and liquid stock prices for individual firms are available at the time of important PTA-announcements. For an inclusion of a country in the data, we require at least 10 listed firms and associated stock prices to be present per country-PTA-event. The dependent variable is based on the cumulative abnormal returns (CARs) of individual firms around important news on PTAs. All the stock market data are gathered from Datastream/Eikon. Regarding important news on future PTAs, we consider two types, namely the announcement of the start of negotiations and the announcement of general agreement. We use the dataset from Moser and Rose (2014) as a point of departure for this and update the data on the aforementioned events by considering all new PTAs that have been notified to the WTO and that were in force by October 15, 2014. The event dates have been identified through a rigorous full-text analysis on LexisNexis, where we mainly focus on international newswires, press releases, and well-known newspapers published in English. We augment the aforementioned information with data on firms' main sector affiliation as well as firm size and the short- and long-term reductions in tariffs within the PTA in a firm's main business sector.

The associated results can be summarized as follows. First, there is a robust and statistically significant positive impact of size (log sales) on the stock-market price around an announcement event of a PTA on average. An increase in expected tariff reductions raises the stock-market price as well, but less so for larger firms. These results suggest that investors expect positive effects from PTAs. However, the tariff-related effects for very large firms are reduced, with multinational production and foreign affiliate sales (which might act as substitutes for cross-border trade) being one potential channel that might explain this particular pattern in the data.

This paper contributes to a small empirical literature that investigates trade liberalization through the lens of the stock market. On the one hand, there are a small number of studies that typically analyze individual stock market reactions in one country for a given RTA (Thompson, 1993, 1994; Rodriguez, 2003; Breinlich, 2014). These papers exploit the Canada-United States Trade Agreement (CUSFTA) and the North American Free Trade Agreement (NAFTA). On the other hand, Moser and Rose (2014) investigate the overall stock market reaction to important RTA news for a large number of countries and over 120 RTAs. But to date there is no systematic evidence at the individual stock market level for a large number of countries and RTAs. This paper fills this void by carefully documenting the heterogeneity of stock market returns across RTAs, countries, sectors and firm types.

The remainder of the paper is organized as follows. Section 2 describes the data, section 3 formalizes the empirical model while section 4 describes the results. The last section concludes with a summary of the main findings.

4.2 Data

We collect a very rich dataset by combining information from various sources.

Announcement data

One important component of our dataset is a unique set of announcement dates. We follow Moser and Rose (2014) and consider two types of PTA-news, namely the day when it was announced that negotiations on a PTA will start at some future date ("start") and the day when an agreement on the PTA was actually reached ("deal"). We identify the exact announcements through a full-text search via LexisNexis. Thereby, we mainly rely on international newswires, press releases and well-established newspapers published in English. In the case of PTAs, announcements about the start or completion of trade negotiations are typically communicated by the President, Prime Minister or the Minister in charge of the international trade agenda. It is important to note that also deal date usually preceeds the official signature date by several weeks and months. We build on the event dates from Moser and Rose (2014) that go through 2009 and update the dataset up to the year 2014, including all additional PTAs for which we have liquid stock market data and that have been notified to the WTO and that went into force no later than January 15, 2015. Hence, we cover PTAs being annnounced in the years from 1988 to 2014.

Financial market data

All our financial market data is drawn from Datastream/Eikon. In particular, we have gathered daily stock market prices for basically the universe of listed firms worldwide. We focus on the main stock of any given firm and, hence, do not consider for instance secondary quotes or other financial firm assets like American Depositary Receipts (ADRs). All our individual stocks are denoted in local currency. Furthermore, we have collected two very well-established daily world stock market indices, namely the MSCI World and the MSCI World (AC) in US dollars. Note that the later index is based on industrialized and emerging markets, whereas the former focuses on industrialized markets. Both indices are composed of a very large number of firms and serves as a good proxy for world market returns.

Firm and tariff data

Firm size is captured by the variable sales, which refers to net sales or revenues. This variable is available through Datastream/Eikon (WC01001) at a yearly-basis and is collected by Worldscope from a firm's balance sheet. Our sales variable represents gross sales and other operating revenue less discounts, returns and allowances.

Tariffs are gathered from the TRAINS database. We use bilateral tariffs at 4 digit level as categorized by the Standard Industrial Classification (SIC) nomenclature. The form of tariff used in this analysis is ad-valorem tariff, representing the customs duty calculated as a percentage of the value of the product. Tariffs are of course set at a much more disaggregated level then four digits (usually up to eight digits). However we only observe in the TRAINS database the more aggregated four-digit level. This is available computed as a simple average and as a weighted average. The simple average (in percentage points) consists of simple average tariff of included 6-digit lines. The 6-digit tariff is itself an average of included tariff line level lines. The weighted average is calculated as the average of tariffs weighted by their corresponding trade value. We focus on the tariff measure reported by the WTO as *Effectively Applied Tariffs.* According to WTO, these are defined as the lowest available tariff.[2]

4.3 Empirical methodology

Our empirical model follows the logic of a classical event study (MacKinlay, 1997). We proceed in two steps by first computing abnormal returns around

[2]With tariff data being relatively scarce the more disaggregated the level of observation, we use different techniques to fill in some of the gaps, as explained below and exemplified in Table 4.4 of the Appendix with $Tariff_{ipt}$ for the case of Austria.

1. Imputation: We take the biggest non-member trading partner (by GDP) for a specific year and a specific product and fill in for all other non-member partners with missing tariff data.
2. Interpolation: following the imputation, we fill in all the gaps with the value of the most recent non-missing value before the missing one as long as that equals the most recent non-missing value after the missing one (should it exist). E.g. if we have data for year 3 and year 8, and they are the same, we can fill in years 4, 5, 6 and 7 with that value.

Table 4.1: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Cummulative abnormal returns (CAR)					
1 day before, 3 after	662,433	-0.12	10.63	-919.59	772.24
1 day before, 7 after	662,433	-0.24	13.98	-917.70	857.01
Sales	522,066	11.54	2.41	0.00	19.95
Tariff	581,445	-0.14	0.35	-2.24	3.54
Tariff*	514,299	-0.20	0.38	-2.50	2.14

Sales represents the natural logarithm of firm-level sales; Tariff is captured by the variable $Tariff_{ipt}$ and represents the change in the effectively applied tariff on country i's exports to all of its new PTA partners two years after PTA is enforced compared to the year before enforcement; Tariff* is captured by the variable $Tariff^{*}_{ipt}$ and represents the change in the effectively applied tariffs on all of i's PTA partners' exports to i.

important announcements on new PTAs and, then, by explaining these returns with important firm-level determinants. This empirical strategy allows us to provide a first systematic take on the heterogeneity of stock market returns to trade liberalization within and across countries.

First, we compute the **cumulative abnormal returns**, henceforth abbreviated as CARs, as follows. We regress a firm's individual stock return on the market return for an estimation window prior to the PTA-announcement day T:

$$R_{fipt} = \alpha_f + \beta_f R_t + \epsilon_{fipt} \text{ where } t \in \{T-395, T-29\} \tag{4.1}$$

Note that R_{fsit} is the one-day return on an individual stock f of product p in country i realized at the end of day t. We use the MSCI World (AC) stock market index as our benchmark world market return R_t. It includes a very broad set of individual stocks in industrialized and emerging economies.

Then, we compute abnormal returns for each individual, liquid firm (around the announcement date) in those countries that are about to form a RTA.[3]

$$\widehat{AR}_{fipt} = R_{fipt} - \widehat{alpha}_f - \widehat{\beta_f} * R_t \tag{4.2}$$

[3] We define firms as illiquid in a given month, if we observe zero returns for over 50% of the trading days per month. This liquidity measure builds on Lesmond et al. (1999).

We cumulate these firm-specific abnormal returns over time around the dates of RTA announcements. Our default measure of cumulative abnormal return (CAR_{fipt}) aggregates abnormal returns starting a day before the news event, and continuing through three business days afterward. That is, we form

$$CAR_{fipt} = \sum_{T-1}^{T+3} \widehat{AR}_{fipt} \tag{4.3}$$

From the base measure of CAR we construct another variation based on different time windows of the set $(1,3),(1,7)$.
Second, we want to explain the variation in these individual abnormal returns with two types of variables. In this paper, we focus on two important channels through which trade liberalization can affect the expected profits of firms in a country whose government is about to sign a PTA. First, the Melitz-model predicts that larger, more productive firms profit disproportinally from opening up to international trade. We follow Breinlich (2014) and measure firm size by total sales (in logarithmic form). We would expect a positive coefficient on sales from the following baseline regression.

Specification 1

$$CAR_{fipt} = \alpha_1 SALES_{fipt} + u_{ipt} \tag{4.4}$$

Second, it is important to account for the tariff heterogeneity across and within PTAs. Obviously, PTAs vary in the degree of trade liberalization and, even within a given PTA, some sectors might experience a much stronger reduction in tariffs than others. It is also important to keep in mind that in a given industry and PTA tariff reductions granted to and by new PTA members are not necessarily the same. We estimate the following augmented second-stage regression.

Specification 2

$$\begin{aligned}CAR_{fipt} = &\alpha_1 SALES_{fipt} + \alpha_2 TARIFF_{ipt} + \alpha_3 TARIFF^*_{ipt} + \\ + \quad &\alpha_4 [SALES_{fipt} \times TARIFF_{ipt}] + \\ + &\alpha_5 [SALES_{fipt} \times TARIFF^*_{ipt}] + \mu_s + u_{ipt}\end{aligned} \tag{4.5}$$

where $TARIFF_{ipt}$ is the change in the effectively applied tariff charged to exports of product p from country i to its PTA partners by its PTA partners at time t. $TARIFF^*_{ipt}$ is the change in effectively applied tariff charged by country i on all of its PTA partners' exports of product p at time t. We approximate these tariff changes by computing the change in tariffs in the year prior to the enforcement of a new PTA relative to two years after its implementation. Note that a tariff change in the case of trade liberalization is usually equivalent to a tariff cut. We would expect a positive coefficient for the variable $TARIFF_{ipt}$, since a tariff reduction grants domestic firms a better market access to the new PTA-partners, leading to larger expected profits. In contrast, if the home government grants substantial reductions on import tariffs, we would expect that import competition increases, potentially depressing profits for domestic firms. We also include in all our regression an sectoral fixed effect (μ_s) for 10 sectors and a dummy variable being one for PTAs that cover goods and services and zero otherwise. We exclude the last quarter of the year 2008 from our sample, since the onset of the Subprime Crisis has been characterized by very high stock market volatility and uncertainty. In this setting, it seems implausible that the announcement of a PTA is the most important news to stock market participants. We report standards errors that are clustered at the country-PTA-event-level.

4.4 Empirical results

We now turn to the empirical results. Table 4.2 reports our main specifications for our benchmark stock market index, the MSCI World (AC), for two different event windows. The first three columns refer to the shorter event window [1,+3] and columns 4 through 6 to the longer event window [1,+7]. The first specification for each event window focuses on the size-effect. We find that abnormal returns in response to trade liberalization are larger for bigger stock-listed firms. Consistent with Melitz (2003) expected future profits due to a new PTA are more concentrated among larger firms.

Table 4.2: Baseline MSCI World (AC)

# days [*before*, *after*] PTA news	[-1, +3]			[-1, +7]		
Sales	0.056**	0.047*	0.063**	0.109***	0.084*	0.091*
	-0.02	-0.03	-0.03	-0.04	-0.05	-0.05
Tariff		-1.155	-4.374*		-1.287	-5.521**
		-0.96	-2.63		-0.9	-2.75
Tariff*		1.292	2.876		1.696*	4.484*
		-1.08	-2.43		-0.88	-2.41
Sales X Tariff			0.266*			0.349**
			-0.14			-0.16
Sales X Tariff*			-0.128			-0.228
			-0.12			-0.15
N	368261	295381	295381	368261	295381	295381

*** p<0.01, ** p< 0.05 * p<0.1; standard errors clustered at the country-PTA-event-level reported under coefficients; Sales represents the natural logarithm of firm-level sales; Tariff is captured by the variable $Tariff_{ipt}$ and represents the change in the effectively applied tariff on country i's exports to all of its new PTA partners two years after PTA is enforced compared to the year before enforcement; Tariff* is captured by the variable $Tariff^{*}_{ipt}$ and represents the change in the effectively applied tariffs on all of i's PTA partners' exports to i.

This result confirms and generalizes one main finding of Breinlich (2014), who also documents a positive size-effect for Canadian firms during the announcement of the CUSFTA at the end of the 1980s for a very large set of PTAs and countries.

We report first results on the tariff variables in columns (2) and (5) of Table 1. In our discussion we focus on the richest specification in columns (3) and (6) that also allows for interaction terms between the two tariff variables and log sales. We find that a strong and significantly negative effect for the variable $TARIFF_{ipt}$. As expected, domestic firms' stock price increase more, if larger tariff concessions are made by the new PTA-partner countries. The preferential margins vis-a-vis other international competitors is likely to rise the larger the tariff cuts granted. Hence, better market access due to lower tariff-barriers boosts expected profits. We call this effect the *export opportunity effect.* The opposite holds true for those tariffs that the home economy grants to the new PTA-partners. We indeed find a positive and significant effect for the variable $TARIFF^{*}_{ipt}$, which we coin the *import competition effect*. With lower trade barriers import competition is expected to rise and profits for import competing firms are depressed. It is noteworthy that the export opportunity effect tends to dominate the import competition effect with the point coefficients being larger.

But the picture gets a bit more nuanced regarding the export opportunity effect for large firms, since the interaction term betwen $TARIFF_{ipt}$ and $Sales$ is statistically significant at the 95% confidence level. Let us use β_{Tariff} to denote the regression parameter on the respective tariff variable and $\beta_{Tariff} \times Sales$ the coefficient on the interaction term between the tariff and (log) sales variable. Then, after skipping the index on the sales variable, the marginal effect of a change in tariffs when acknowledging the interaction term with log sales is defined as $\beta_{Tariff} + \beta_{Tariff \times Sales} Sales$. Notice that in the tables the estimate $\widehat{\beta}_{Tariff} < 0$ whereas the estimate $\widehat{\beta}_{Tariff \times Sales} > 0$. Hence, the overall marginal effect of tariffs depends on the level of log sales. It turns out that at the average of $Sales$ in our sample (which amounts to 12.08), the marginal effect is negative. Hence, only for extremely large firms will a PTA-associated expected reduction in tariffs reduce the cumulative abnormal returns. That this could be the case for very large firms could be rationalized by an increased competitive pressure of these firms' foreign affiliates (and their sales) abroad through the entry and larger sales of exporters.

So far our empirical findings suggest that large firms profit more from trade liberalization. Furthermore, we find evidence for a positive export opportunity effect and negative import competition effect. Now we would like to investigate one important dimension of potential heterogeneity across countries. Are the reported economic effects similar in industrialized and emerging countries? Interestingly, these two broad groups of countries are indeed quite different in two important dimensions. First of all, the well-established size-effect that we would expect from the Melitz-model is confined to OECD-countries. While the variable $Sales$ is positive and statistically significant at the 99% confidence level for OECD-countries, we do not find any significant effects for this variable in the sample of non-OECD countries.

In order to shed some light on this issue, we split our full sample into OECD and non-OECD countries. Table 4.3 reports the results for the longer event window [1,+7]. Columns 1-3 [4-6] refer to the OECD [non-OECD] countries.[4] This is an important qualification to the results of Breinlich (2014) that are based on one industrialized country (Canada). Our empirical results suggest that the Melitz-model seems to fit developed economies much better than developoing economies. Obviously, the Melitz-model should not be reduced to one prediction, but the size prediction is without a doubt a key prediction of the Melitz model. Second, it is also interesting to note that the relative magnitude and the statistical significance of the export opportunity and import competition effect is quite different between those two groups of countries.

Table 4.3: Baseline MSCI World (AC): OECD vs. Non-OECD

# days [*before*, *after*] PTA news	**[-1,+7]**					
	OECD			**Non-OECD**		
Sales	0.124***	0.112***	0.132***	0.024	-0.041	-0.059
	-0.04	-0.04	-0.05	-0.09	-0.11	-0.12
Tariff		-0.732**	-3.159**		-2.352	-6.784
		-0.36	-1.5		-2.06	-4.19
Tariff*		0.654	1.502		3.319	8.203*
		-0.42	-1.46		-2.1	-4.31
Sales X Tariff			0.194*			0.386
			-0.11			-0.3
Sales X Tariff*			-0.063			-0.424
			-0.11			-0.34
N	260263	211972	211972	107998	83409	83409

*** p<0.01, ** p< 0.05 * p<0.1; standard errors clustered at the country-PTA-event-level reported under coefficients; Sales represents the natural logarithm of firm-level sales; Tariff is captured by the variable $Tariff_{ipt}$ and represents the change in the effectively applied tariff on country i's exports to all of its new PTA partners two years after PTA is enforced compared to the year before enforcement; Tariff* is captured by the variable $Tariff^{*}_{ipt}$ and represents the change in the effectively applied tariffs on all of i's PTA partners' exports to i.

[4]We classify all those 24 countries as OECD countries that were member of the OECD already at the beginning of the 1990s (Austria, Australia, Belgium, Canada, Denkmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, Turkey, United Kingdom and United States).

While for richer economies the coefficient on $TARIFF_{ipt}$ is about twice as high as the $TARIFF^{*}_{ipt}$, with only the first one being statistically significant, for poorer economies the later coefficient is larger than the former and only the later on is statistically significant at the 90% confidence level. We interpret these results in the following way. The export opportunity effect clearly dominates the import competition effect in OECD countries. The picture is less positive for non-OECD countries. For non-OECD countries, there is some weak evidence that the import competition channel is more important. Hence, investors expect profit losses from increased import competition rather in non-OECD than OECD countries.

Our results are neither sensitive to the exact choice of the event window nor the choice of the world market return. Further results ipresented in Table 4.5 and Table 4.6 of the Appendix replicate our main results for an alternative world market index that does not include emerging markets and, hence, puts a greater weight on stock markets in industrialized economies.

4.5 Conclusions

This paper undertakes a large-scale analysis of the universe of all stock-market prices around the prior time of announcement of new preferential trade agreements (PTAs) that had been signed and notified to the World Trade Organization (WTO) between the beginning of 1988 and mid-October 2014. Overall, we established a unique dataset of $368,261$ observations involving $37,330$ firms in 67 countries and associated coded news on the announcements of 153 (mostly multilateral) PTAs as well as agreements on the terms of the PTA.

The analysis led to the following insights. First, on average and across the board of firms, countries, PTAs, and years, firm size is an important predictor of the effects of new PTA announcements. Trade theory suggests that expected profits – and associated dividends – increase in the case of a reciprocal trade liberalization for highly-productive – i.e., ex-ante large – firms. The results in this paper supports this hypothesis. We deem this an important bit of evidence, as it does not suffer from the endogeneity of sales and productivity in cross sections of data or around the time of the actual coming into force of PTAs. Second, ex-ante higher tariffs and a larger planned and envisaged reduction of tariff barriers does not benefit all firms but only the very large listed ones on average.

While these results mainly speak to average responses across firms, countries, and PTAs, we plan to dissect the associated effects in future work. In any case, considering effects of announcements on real economic outcome through expectations appears important. Eventually, it will help us gauging a better understanding of the overall effects of liberalization, as some of those effects may be masked by the anticipation of future changes even prior to actual modifications of trade barriers.

Appendix

Table 4.4: Filling in missing data on tariffs for Austria

	Missing out of total 1,091,862					
		After:				
	Before	Imputation	Interpolation	Extrapolation	First 2	All 3
Tariff	376,438	50,382	343,682	152,041	49,911	43,888

Imputation: assign product-level tariff of the highest GDP non-member (of all PTAs) to all non-members with missing tariff. *Interpolation:* assign product-level tariff of the last non-missing observation of the same country-pair-year-product if equal to the next non-missing tariff of the same cluster, to all missing values in between. *Extrapolation:* copy downwards the last non-missing value to fill in the following missing ones.

Table 4.5: Baseline MSCI World

# days [*before*, *after*] PTA news	**[-1, +3]**			**[-1, +7]**		
Sales	0.055**	0.046*	0.061*	0.107***	0.081*	0.086
	-0.02	-0.03	-0.03	-0.04	-0.05	-0.05
Tariff		-1.195	-4.392*		-1.335	-5.580**
		-0.96	-2.63		-0.9	-2.75
Tariff*		1.292	2.902		1.714*	4.592*
		-1.08	-2.43		-0.88	-2.4
Sales X Tariff			0.264*			0.350**
			-0.14			-0.16
Sales X Tariff*			-0.13			-0.235
			-0.12			-0.15
N	368255	295378	295378	368236	295359	295359

*** p<0.01, ** p< 0.05 * p<0.1; standard errors clustered at the country-PTA-event-level reported under coefficients; Sales represents the natural logarithm of firm-level sales; Tariff is captured by the variable $Tariff_{ipt}$ and represents the change in the effectively applied tariff on country i's exports to all of its new PTA partners two years after PTA is enforced compared to the year before enforcement; Tariff* is captured by the variable $Tariff^{*}_{ipt}$ and represents the change in the effectively applied tariffs on all of i's PTA partners' exports to i.

Table 4.6: Baseline MSCI World

# days [*before*, *after*] PTA news			**[-1,+7]**			
		OECD			**Non-OECD**	
Sales	0.123***	0.111**	0.129***	0.021	-0.04	-0.059
	-0.04	-0.04	-0.05	-0.09	-0.11	-0.12
Tariff		-0.770**	-3.299**		-2.392	-6.683
		-0.37	-1.52		-2.05	-4.2
Tariff*		0.693*	1.678		3.256	8.098*
		-0.42	-1.45		-2.1	-4.31
Sales X Tariff			0.202*			0.373
			-0.11			-0.3
Sales X Tariff*			-0.074			-0.421
			-0.11			-0.35
N	260246	211957	211957	107990	83402	83402

*** p<0.01, ** p< 0.05 * p<0.1; standard errors clustered at the country-PTA-event-level reported under coefficients; Sales represents the natural logarithm of firm-level sales; Tariff is captured by the variable $Tariff_{ipt}$ and represents the change in the effectively applied tariff on country i's exports to all of its new PTA partners two years after PTA is enforced compared to the year before enforcement; Tariff* is captured by the variable $Tariff^{*}_{ipt}$ and represents the change in the effectively applied tariffs on all of i's PTA partners' exports to i.

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