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Published in:
Applied Economics

DOI:
[10.1080/00036846.2015.1016211](https://doi.org/10.1080/00036846.2015.1016211)

IMPORTANT NOTE: You are advised to consult the publisher's version (publisher's PDF) if you wish to cite from it. Please check the document version below.

Document Version
Publisher's PDF, also known as Version of record

Publication date:
2015

[Link to publication in University of Groningen/UMCG research database](#)

Citation for published version (APA):

Kohl, T., & Trojanowska, S. (2015). Heterogeneous trade agreements, WTO membership and international trade: an analysis using matching econometrics. *Applied Economics*, 47(33), 3499-3509.
<https://doi.org/10.1080/00036846.2015.1016211>

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To cite this article: Tristan Kohl & Sofia Trojanowska (2015) Heterogeneous trade agreements, WTO membership and international trade: an analysis using matching econometrics, Applied Economics, 47:33, 3499-3509, DOI: [10.1080/00036846.2015.1016211](https://doi.org/10.1080/00036846.2015.1016211)

To link to this article: <http://dx.doi.org/10.1080/00036846.2015.1016211>

Published online: 23 Feb 2015.

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Heterogeneous trade agreements, WTO membership and international trade: an analysis using matching econometrics

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This article explores the heterogeneous effects of trade agreements (TAs) and World Trade Organization (WTO) membership on the volume of international trade. We extend Baier and Bergstrand's (2009a) application of matching econometrics by distinguishing between different types of TAs and WTO participation and account for the endogenous nature of trade policy. For a panel data set covering 1960–2005 and 187 countries, we find that the treatment effect on international trade systematically varies with the type of TA and WTO membership.

Keywords: matching econometrics; international trade; trade agreements; world trade organization; gravity model

JEL Classification: F13; F15

I. Introduction

The empirical literature on the impact of trade agreements (TAs) on international trade has recently benefited from several methodological improvements addressing the endogenous nature of trade policy. Baier and Bergstrand (2007, 2009a) demonstrated that theoretically motivated gravity equations of international trade can account for endogeneity bias in the parameter estimates of the binary trade agreement variable by using country-time and dyad fixed effects, first differencing, or matching econometrics.

In another new strand of the literature, researchers argue that lumping all types of trade agreements together in one binary dummy variable in the gravity

equation leads to measurement error (Kohl *et al.*, forthcoming). The heterogeneous nature of trade agreements and the varying size of their effect on trade can be further understood by systematically distinguishing between levels of economic integration stipulated in the trade agreements (see, e.g., Magee, 2008; Roy, 2010).

Nevertheless, the empirical tools recently presented to account for endogenous trade policy in gravity equations have until now not been widely combined with the insight of trade agreement heterogeneity. More specifically, to our knowledge, we are the first to bridge this gap by using matching techniques to determine the effect of different types of TAs, as well as different degrees of countries' involvement

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in the World Trade Organization (WTO), on the volume of international trade.¹

The contribution of this article is twofold. First, we build on Baier and Bergstrand (2009a) matching technique by explicitly distinguishing between different levels of economic integration embodied in TAs. For this, we rely on Baier, Bergstrand and Mariutto's (2014) categorization of TAs: nonreciprocal preferential trade agreements (NRPTAs), reciprocal preferential trade agreements (PTAs), free trade agreements (FTAs), customs unions (CUs), common markets (CMs) and economic unions (EUs). Second, we show that a similarly fine-grained approach can be applied to countries' level of commitments in the World Trade Organization (WTO) by explicitly accounting for their roles in the multilateral trade system as formal members, informal participants or outsiders (as in Tomz *et al.*, 2007).

By addressing the heterogeneous nature of TAs, we find that the magnitude and significance of the treatment effects of TAs are strongly related to the type of TA, with more extensive agreements (in terms of economic integration) typically showing larger treatment effects. We also demonstrate that there is a systematic ordering to the treatment effects of WTO membership.

The remainder of this article is structured as follows. Section II reviews the empirical literature on TA effects. Section III presents the methodology, with results in Section IV. Section V discusses the main findings, after which Section VI concludes.

II. Literature

Ever since Tinbergen (1962), the gravity equation has become the standard empirical tool to, in the context of the present article, estimate the *ex post* impact of trade policy on international trade flows (see, among others, Dahi and Demir, 2013; Martínez-Zarzoso, 2013; Baltagi *et al.*, 2014; Sheng *et al.*, 2014; Head and Mayer, 2015).

However, a key problem when it comes to estimating trade policy's impact on trade is that such policy is not exogenous. In other words, trade policy may well be the result of the level of international trade. Indeed, the endogenous nature of trade policy has been widely addressed, both theoretically and empirically (see Trefler, 1993; Magee, 2003; Egger *et al.*, 2008, 2011).

In their study on how to empirically address the issue of endogeneity bias with panel data, Baier and Bergstrand (2007, 2009a) demonstrated how three complementary techniques may be successfully used to obtain plausible estimates of the impact of trade agreements.

First, Baltagi *et al.* (2003) and Baier and Bergstrand (2007) provide a theoretically motivated time-varying variant of Anderson and van Wincoop's (2003) multilateral resistance terms with country-year fixed effects. In addition, the inclusion of country-pair fixed effects accounts for unobserved dyad-specific characteristics. Although the inclusion of importer-year, exporter-year and dyad fixed effects may be computationally cumbersome in large panels, it has set the new standard in the field (see also Egger and Pfaffermayr, 2003; Baldwin and Taglioni, 2006; Magee, 2008; Fugazza and Nicita, 2013; Nuroğlu and Kunst, 2014).

A second technique that Baier and Bergstrand (2007) discuss is a first-differenced form of their augmented gravity equation with country-time and dyad fixed effects. This alternative method has the advantage of eliminating the time-invariant bilateral effects, thereby increasing efficiency (see also Felbermayr and Jung, 2009; Baier *et al.*, 2014a).

A third solution is to use nonparametric techniques such as matching econometrics, as illustrated in Baier and Bergstrand (2009a). Drawing on time-varying multilateral price indices developed in Baier and Bergstrand (2009b), the authors estimate the 'treatment' of having a trade agreement on members' trade for countries that share similar characteristics in terms of their bilateral distance, joint economic size, adjacency, linguistic commonality and multilateral prices. The authors find that the long-run average treatment effect for a large number of trade agreements is about 100%, which is consistent with their earlier findings. Similar applications can be found in Chang and Lee (2011), Aichele and Felbermayr (2013), Baghdadi *et al.* (2013) and Montalbano and Nenci (2014).

The first two solutions to addressing the endogeneity issue – modified fixed effects and first differencing – are now readily finding their way into empirical studies on the effects of trade agreements *in general*. In the first study to address endogeneity and trade agreement heterogeneity together, Kohl (2014) uses both of these techniques to

¹ Throughout this article, WTO also implies the General Agreement on Tariffs and Trade (GATT).

estimate the long-run trade effects of 166 *individual* trade agreements. Interestingly, in addition to finding substantial variation in the trade impacts of these unique agreements, he also shows that their coverage of different trade policy areas helps to partially explain why some agreements are more effective than others.

The next step is to verify that trade agreement heterogeneity matters for trade effects with nonparametric techniques. Where Baier and Bergstrand (2009a) show that matching econometrics is a useful way to ‘check’ the plausibility of long-run treatment effects when all trade agreements are lumped together, this article adds to their analysis the notion that the magnitude of these trade effects will depend on the *level* of economic integration specified in the trade agreement.

In a similar vein, we contribute to the literature by accounting for countries’ participation in the WTO. In the related literature on the effect of WTO membership on international trade, Tomz *et al.* (2007) showed the relevance of specifically distinguishing between formal members and informal participants. Doing so solved, amongst others, Rose’s (2004) mystery of implausible WTO effects.

In particular, Tomz *et al.* (2007) explain the institutional evolution of the WTO and how colonies were not formal members to the multilateral agreement. However, in several cases, colonial empires applied certain rights (i.e., market access) and/or obligations (i.e., tariff concessions) to select colonies, rendering them informal participants. Once the decolonization process was underway, these colonies could then decide how to continue their involvement. Few left and became outsiders, others started accession procedures to become formal members, while, undecided, the remainder kept on being informal participants. It was only upon the completion of the Uruguay Round in 1994 that informal participants had to either apply for formal membership or exit altogether.

In their extensive empirical work on the WTO’s trade effect, Chang and Lee (2011) find that the WTO’s effect on trade is stronger when both countries in a pair are participants than when only one of them is involved in the multilateral trade system. However, the authors do not go beyond this distinction, while it stands to reason that the distinction between formal membership and informal participation should not be overlooked.

III. Methodology

Method

We now turn to explaining our empirical approach. The matching approach enables the research to estimate average effects of a binary treatment (two groups, one with and the other without the TA – or interchangeably WTO – as treatment) on some observed outcome, which in this case are trade flows (TF) where

$$TF = TF(TA) = \begin{cases} TF(0) & \text{if } TA = 0 \\ TF(1) & \text{if } TA = 1 \end{cases} \quad (1)$$

The basic idea of the matching method is to find untreated units that are similar to treated units in terms of their covariates except for the treatment. The self-selection bias is bypassed through a random assignment of the units into a treatment and a control group according to similarity in the covariates (Imbens and Wooldridge, 2009, p. 35). In the context of international trade, this implies that similar country-pairs form two groups: those that participated in a TA and those that did not. Therefore, countries are identified that are similar to the ones that participated in a TA, but were nonparticipants. These country-pairs are matched according to their similarity (Abadie *et al.*, 2004, p. 292).

The researcher can choose between estimating different treatment effects, that is, the average treatment effect (ATE) of the whole population, the average treatment effect of the treated countries (ATT) and the average treatment effect of the untreated countries (ATU; see Imbens and Wooldridge, 2009, p. 15).

Another advantage is that matching does not require a specific distribution and does not assume any functional relationship, so even when nonlinearities among covariates exist, this will not be an issue (Imbens, 2004, p. 4). The matching estimator is nonparametric, meaning that the dependent variable can be estimated by computing the mean of TF across the treated and untreated countries. For this, the dependent variable is constructed by deriving information from the data, so that the gravity equation does not need to be fixed in its functional form.

Assumptions. According to Abadie and Imbens (2006), the matching estimator must fulfil three

assumptions to retrieve unbiased results. The first is conditional independence, meaning that TA participation is independent from $TF(0)$ and $TF(1)$, given exogenous variables x . This assumption ensures that once there is control for a set of covariates, treatment is random and therefore exogenous. Theoretical insights provided by Anderson and Van Wincoop (2003) are used as a guide for the selection on observables such as a country-pair's joint GDPs, bilateral distance, contiguity and language. Multilateral resistance terms of these variables are calculated as linear approximations following Baier and Bergstrand (2009b), explained below.

The second prerequisite is the overlap assumption, which states that observations of both treated and untreated country-pairs for each value of x need to be collected so that there is an overlap area in both of the distributions for treated and untreated dyads. The underlying data set is very detailed and includes a large number of dyads with and without trade agreements, so that this assumption is not likely to be violated for TAs.

Third, stable-unit treatment values (SUTVA) are assumed. This can be done when (i) the establishment of a TA between countries does not exert an impact on untreated countries or is impacted by any other dyad, and (ii) the treatment is exactly the same for all treated country-pairs. The former is ensured by including Baier and Bergstrand's (2009b) reduced-form function of linear combinations of the 'exogenous' variables (explained below) to account for general equilibrium effects. However, the latter assumption is arguably unlikely to hold when trade agreement heterogeneity is ignored, i.e., customs unions impose a different treatment on their members than free trade agreements. This is a further justification to examine the partial equilibrium effects of different types of trade agreements on trade volumes.

Specification. Coming to the specification of the selection on observables, we will now describe which independent variables are used for the matching process. Unlike with parametric applications of the gravity model, the nonparametric estimation using the matching estimator requires a different way to account for multilateral resistance terms. This means that to account for unobserved prices, fixed effects can no longer be used. Fortunately, Baier and Bergstrand (2009b) provide a solution in the form of exogenous multilateral resistance terms

using a first-order log-linear Taylor-series expansion. This means that a variable such as distance, e.g., is calculated by (i) the distance between country i and country j and (ii) the importer's mean distance to all countries other than the exporter and (iii) the exporter's mean distance to all countries other than the importer. This yields a reduced-form function of linear combinations of the 'exogenous' variables presented below:

$$\ln(TF_{ijt}) = \beta_0 + \beta_1 TA_{ijt} + \beta_2 SGDP_{ijt} + \beta_3 BVD_{ij} + \beta_4 BVL_{ij} + \beta_5 BVB_{ij} + \varepsilon_{ijt} \quad (2)$$

where

$$BVD_{ij} = \ln(D_{ij}) - \frac{1}{N} \sum_{i=1}^N \ln(D_{ij}) - \frac{1}{N} \sum_{j=1}^N \ln(D_{ij}) + \frac{1}{N^2} \sum_{i=1}^N \sum_{j=1}^N \ln(D_{ij}) \quad (3)$$

$$BVL_{ij} = L_{ij} - \frac{1}{N} \sum_{i=1}^N L_{ij} - \frac{1}{N} \sum_{j=1}^N L_{ij} + \frac{1}{N^2} \sum_{i=1}^N \sum_{j=1}^N L_{ij} \quad (4)$$

$$BVB_{ij} = B_{ij} - \frac{1}{N} \sum_{i=1}^N B_{ij} - \frac{1}{N} \sum_{j=1}^N B_{ij} + \frac{1}{N^2} \sum_{i=1}^N \sum_{j=1}^N B_{ij} \quad (5)$$

$SGDP$ is the sum of GDPs in country-pair ij in year t , D is the bilateral distance in kilometres, L is a binary variable that is 1 if the country-pair shares a common language and is zero otherwise, B is a binary variable that is 1 if the country-pair shares a common border and is zero otherwise, N is the number of countries, and ε is the error term.

Estimation. Following Baier and Bergstrand (2009a), we use a matching estimator with a replacement mechanism, leading to matches with higher

quality by increasing the set of possible matches. Thus, this matching estimator increases the variance and lowers the bias, as compared to matching without replacement (Abadie *et al.*, 2004, p. 290). In the first step, this matching estimator imputes the unobserved outcome for the respective observed trade flow $TF^*(0)$ for countries that did not participate in a TA and $TF^*(1)$ for countries that did, as a weighted average of the observed outcomes $TF(0)$ and $TF(1)$, thereby creating a counterfactual. After the imputation process, it is possible to obtain the estimator for the ATE, which takes the average across all observations N ,

$$ATE = \frac{1}{N} \sum_{i=1}^N [TF_i^*(1) - TF_i^*(0)] \quad (6)$$

and the ATT that takes only those countries into account that actually do have a trade agreement,

$$ATT = \frac{1}{N} \sum_{TA_i=1}^N [TF_i(1) - TF_i^*(0)] \quad (7)$$

The ATE and the ATT are estimated by comparing the trade flows of the treated and the control group and by taking the average difference of their trade flows (Baier and Bergstrand, 2009a, p. 71). The imputation process of the matching estimator is determined by several options. For example, it is optional how many nearest neighbours one may choose for matching. There are several weights one may want to apply that lead to different estimators, e.g. the 'nearest neighbour' estimator, where the closest nonparticipant match for each participant is selected. We follow Baier and Bergstrand (2009a) in using the three nearest neighbours. In doing so, the closest three nonparticipant matches, in terms of TA participation, for each participating country are found and the estimator is then a mean over the differences between the participating country and its three matched nonparticipating countries.

For each cross-sectional estimation round, the matching procedure described above yields a data set of observations that are used to calculate the cross-sectional treatment effect, indicating both the

treated and untreated observations. These cross-sectional data are then combined to construct a panel of treated and matching untreated observations. The added value of doing this is that it allows one to regress the dependent variable, real trade flows, on the treatment dummy, as well as time fixed effects to correct for unobserved time-varying phenomena (see also Baghdadadi *et al.*, 2013).²

Continuous variables bias. Another important issue when applying matching was raised by Abadie and Imbens (2006), who show that matching estimators may suffer from a conditional bias term of order $N^{-1/k}$ or larger (with k denoting the number of covariates) if the set of independent variables includes two or more continuous variables. Depending on the selection on observables, among the independent variables of the matching specification, there are at least two continuous variables that may cause the conditional bias, namely SGDP and D .

Technically, matching with continuous variables is not precise, so that the values of the covariates will not be identical within the matches. Variables that are continuous provide more heterogeneity, so that intuitively speaking it is more difficult to match variables that are continuous, in contrast to binary variables. This leads to biased and inconsistent estimators of the ATE, ATT and ATU that will yield unreliable confidence intervals, so that inference from these estimators will no longer be reliable.

If an estimator is consistent, then its distribution converges to the true value of the estimator with increasing sample size (Wooldridge, 2004, p. 163). This implies that the true parameter value will not be revealed even when N goes to infinity. Abadie and Imbens suggest a bias-adjusted matching estimator, which leads to a $N^{1/2}$ -consistent and asymptotically normal matching estimator (Abadie *et al.*, 2004; Abadie and Imbens, 2006, 2011).³

Data

Our panel covers 187 countries and contains annual observations for the period 1960–2005 so that the panel has a maximum of $18 \times 7 \times 186 / 2 \times 46 = 799\,986$ observations by construction. However, there are 476 520 missing values, leaving 323 466 observations (including zero trade flows)

² Our cross-sectional estimates are qualitatively similar to those in Baier and Bergstrand (2009a) and available upon request.

³ In STATA, the adjustment is included in the *biasadj* option of the *nnmatch* command.

Table 1. Countries in data set

Afghanistan	Albania	Algeria	Angola	Antigua and Barbuda	Argentina
Armenia	Aruba	Australia	Austria	Azerbaijan	Bahamas
Bahrain	Bangladesh	Barbados	Belarus	Belgium	Belize
Benin	Bermuda	Bhutan	Bolivia	Bosnia and Herzegovina	Botswana
Brazil	Brunei	Bulgaria	Burkina Faso	Burundi	Cambodia
Cameroon	Canada	Cape Verde	Central African Republic	Chad	Chile
China	Colombia	Comoros	Costa Rica	Croatia	Cuba
Cyprus	Czech Republic	D.R. Congo	Denmark	Djibouti	Dominica
Dominican Republic	Ecuador	Egypt	El Salvador	Equatorial Guinea	Eritrea
Estonia	Ethiopia	Faeroe Islands	Fiji	Finland	France
Gabon	Gambia	Georgia	Germany	Ghana	Greece
Greenland	Grenada	Guatemala	Guinea	Guinea-Bissau	Guyana
Haiti	Honduras	Hong Kong	Hungary	Iceland	India
Indonesia	Iran	Iraq	Ireland	Israel	Italy
Ivory Coast	Jamaica	Japan	Jordan	Kazakhstan	Kenya
Kiribati	Kuwait	Kyrgyzstan	Laos	Latvia	Lebanon
Lesotho	Liberia	Libya	Lithuania	Luxembourg	Macao
Macedonia	Madagascar	Malawi	Malaysia	Maldives	Mali
Malta	Mauritania	Mauritius	Mexico	Moldova	Mongolia
Morocco	Mozambique	Namibia	Nepal	Netherlands	New Zealand
Nicaragua	Niger	Nigeria	Norway	Oman	Pakistan
Palau	Palestinian Authority	Panama	Papua New Guinea	Paraguay	Peru
Philippines	Poland	Portugal	Qatar	Republic of Congo	Romania
Russia	Rwanda	Samoa	Sao Tome and Principe	Saudi Arabia	Senegal
Seychelles	Sierra Leone	Singapore	Slovak Republic	Slovenia	Solomon Islands
South Africa	South Korea	Spain	Sri Lanka	St. Kitts and Nevis	St. Lucia
St. Vincent and Grenadines	Sudan	Suriname	Swaziland	Sweden	Switzerland
Syria	Tajikistan	Tanzania	Thailand	Timor-Leste	Togo
Tonga	Trinidad and Tobago	Tunisia	Turkey	Turkmenistan	Tuvalu
Uganda	Ukraine	United Arab Emirates	United Kingdom	United States	Uruguay
Uzbekistan	Vanuatu	Venezuela	Vietnam	Yemen	Zambia
Zimbabwe					

and 227 209 observations (positive trade flows only).⁴ Table 1 lists the countries included in the data set.

Bilateral nominal imports and exports are from the IMF's (2013) Direction of Trade Statistics and nominal GDP and GDP deflators from the World Bank's (2013) World Development Indicators. Real exports and real GDP were obtained using the GDP deflators (with 2005 as the base year). Our distance

measure, the simple distance between the most populated city in i and j in kilometres, is from Mayer and Zignago (2011). The same source was used to obtain data on country-pairs' common borders and languages.

The data on the different types of trade agreements are from Baier *et al.* (2014b), with descriptive statistics provided in Table 2. Around 88% of the dyads in the data set have no TA. Among the dyads with TAs,

⁴ In the case that zero trade flows are explicitly included, they are recoded from 0 to 1 so that $\ln(\text{TF}) = 0$.

Table 2. Descriptive statistics of the types of TA integration

(1)	(2)	(3)
Type of integration	Count (%)	% of TAs
0 (No TA)	200 400 (88.20)	
1 (NRPTA)	5696 (2.51)	21.25
2 (PTA)	9027 (3.97)	33.67
3 (FTA)	7945 (3.50)	29.64
4 (CU)	2254 (0.99)	8.41
5 (CM)	1228 (0.54)	4.58
6 (EU)	549 (0.29)	2.46
Total observations	227 209 (100.00)	
... of which with TA	26 809 (11.80)	100.00

about a fifth use nonreciprocal PTAs. Around a third of them use reciprocal PTAs or FTAs, respectively. CUs, CMs and EUs are observed less frequently.⁵

Data on the type of WTO participation are from Tomz *et al.* (2007). According to Table 3, around two-thirds of the dyads had both countries participating in the WTO (see column 2). Around 30% of the remaining dyads involved country-pairs with only one country participating in the WTO. Overall, and in contrast to the pattern observed with TA data, only 3.5% of the dyads involve country-pairs in which neither country is either a formal member of the WTO or an informal participant. When it comes to further distinguishing between the types of WTO participation, recall that up until the conclusion of the Uruguay Round in 1994, a participating nation

could either be a formal member of an informal participant. With the creation of the WTO as an international organization in 1995, informal participants had to either opt in and become formal members, or opt out and become outsiders. We can therefore only distinguish between formal and informal participation up to 1994 (see column 3).

IV. Results and Discussion

An overview of all results is presented in Table 4. Initially, we base our estimates only on observations with positive trade flows in column (2) and (3). To verify the robustness of our findings, we repeat our estimates even if they involve zero trade flows in column (4) and (5). We also explore the consistency of our main results by using propensity score matching as an alternative to nearest neighbour matching. The estimates in column (2) and (4) are based on nearest neighbour matching, while those using propensity-score matching are displayed in column (3) and (5). In general, both strategies yield relatively similar outcomes. However, the quality of the former method's matches is more satisfactory than those obtained with the latter and explains some minor differences between the results obtained with these two matching techniques.

First, we assume that the treatment administered to participants is equal across all TAs, regardless of whether these are nonreciprocal PTAs, PTAs, FTAs,

Table 3. Descriptive statistics of the types of WTO participation

(1)	(2)	(3)
Type of participation	Count (%)	Count (%)
Both in WTO	149 361 (65.74)	
–Formal and formal		71 168 (31.32)
–Formal and informal		28 352 (12.48)
–Informal and informal		49 841 (21.94)
One in WTO	69 755 (30.70)	
–Formal and outsider		41 821 (18.41)
–Informal and outsider		27 934 (12.29)
None in WTO	8093 (3.56)	8093 (3.56)
Total observations	227 209 (100.00)	227 209 (100.00)

⁵ For analytical purposes, we will also explore the trade effects when these three types of agreements are combined in one treatment, together representing about 15% of the 'treated' dyads.

Table 4. Panel treatment effects (ATTs) by type of agreement and WTO participation

(1)	(2)	(3)	(4)	(5)
Treatment	ATT	ATT	ATT	ATT
TA	0.754*** (0.019)	0.884*** (0.032)	1.396*** (0.032)	1.448*** (0.056)
–NRPTA	0.076* (0.033)	–0.111 (0.061)	0.743*** (0.065)	0.332** (0.116)
–PTA	0.483*** (0.032)	0.559*** (0.053)	1.080*** (0.057)	1.080*** (0.098)
–FTA	0.736*** (0.025)	0.606*** (0.046)	1.148*** (0.040)	1.018*** (0.071)
–CU	1.103*** (0.074)	0.918*** (0.118)	2.512*** (0.116)	1.809*** (0.189)
–CM	1.316*** (0.053)	1.163*** (0.097)	1.448*** (0.077)	1.315*** (0.140)
–EU	1.333*** (0.119)	1.425*** (0.188)	1.269*** (0.169)	1.984*** (0.306)
–CUCMEU	1.253*** (0.053)	1.369*** (0.083)	2.073*** (0.079)	1.738*** (0.126)
Both in WTO	0.316*** (0.007)	0.176*** (0.013)	1.174*** (0.012)	1.061*** (0.024)
–Formal and formal	0.214*** (0.040)	–0.012 (0.073)	0.280*** (0.082)	0.154 (0.147)
–Formal and informal	0.145*** (0.016)	0.173*** (0.028)	0.755*** (0.033)	1.084*** (0.059)
–Informal and informal	0.086*** (0.012)	0.119*** (0.022)	0.519*** (0.023)	0.700*** (0.040)
One in WTO	–0.215*** (0.010)	–0.194*** (0.019)	–0.855*** (0.017)	–0.985*** (0.033)
–Formal and outsider	–0.072** (0.025)	–0.366*** (0.048)	0.155** (0.055)	0.122 (0.100)
–Informal and outsider	–0.190*** (0.016)	–0.132*** (0.027)	–1.083*** (0.029)	–1.188*** (0.049)
Technique	Nearest neighbour	Propensity score	Nearest neighbour	Propensity score
Zeros included	No	No	Yes	Yes

Notes: ***/**/* denotes significance at the 1/5/10% level. The SEs are in parentheses. The dependent variable is $\ln(\text{real trade flows})$. Year fixed effects are included but not reported for brevity.

CUs, CMs or EUs. We estimate the average treatment effect for the treated country-pairs that have a TA. The covariates are SGDP, BVD, BVL and BVB as specified in Equations 2–5. In all cases, this strategy of lumping all types of TAs together into a single treatment confirms that TAs are trade-enhancing.

We then differentiate between types of TA treatments, which is a key contribution of this study. Recall that until now, matching techniques have not been used to estimate the effects of different types of trade agreements on international trade flows. To do so, we administer the treatment of, e.g., a nonreciprocal PTA and only use dyads without any kind of TA as a control group. Therefore, all observations involving PTAs, FTAs, CUs, CMs or EUs are ignored. We apply this same procedure analogously by type of TA. We estimate the ATTs for real trade flows following our approach above.

We expect to find that treatment effects differ across types of TAs, as each type embodies its own degree of economic integration. Given that the benchmark case, or the control group, is always the same, we can compare the parameter estimates across the different types of agreements.

Strikingly, the expected differences between treatments of different types of TAs are observed quite well. Nonreciprocal PTAs have the weakest effect on

trade, followed by regular PTAs and FTAs. The estimated treatment effects of CUs, CMs and EUs do not necessarily follow this increasing order of magnitude – perhaps due to the relatively small number of relevant observations – but their combined treatment effects are in line with the hypothesis that deeper integration entails larger treatment effects. Our results confirm that TA heterogeneity matters: a trade agreement's level of integration is positively related to its treatment effect on trade.

We now turn to exploring how the type WTO participation can be associated with its impact on the volume of international trade. The purpose of doing so is to determine whether WTO participants' gains from trade are related to the extent to which they have (credible) multilateral commitments. This study is the first to employ matching techniques while accounting for a detailed disaggregation of the type of WTO participation. Indeed, we find that the treatment effects are larger in magnitude when both countries in a country-pair participate in the WTO as opposed to when only one of the pair is involved. In either case, the control group is country-pairs in which neither country is a WTO participant.

Finally, we distinguish between the different combinations of formal membership, informal participation and nonparticipation. Dyads with two

formal members are expected to have the largest ATTs, followed by dyads with one formal and one informal member. It is ambiguous to determine *ex ante* whether dyads with two informal participants would benefit more or less from treatment than dyads with one formal member and one outsider. It is also ambiguous to determine *ex ante* how trade will be affected if one trade partner is a WTO participant and the other is not. On the one hand, trade with non-participants may increase due to unilateral liberalization induced by the trading partners' WTO participation. On the other hand, involvement in the WTO may divert trade from nonmembers to other members because of WTO membership reduces (institutional) trade barriers.

Table 4 confirms a pattern of the treatment effects by type of WTO participation. The treatment effect is positive for trade between two WTO members, while being negative for trade with an outsider. The results are consistent across columns (2)–(5). Also, note that dyads with one country as informal participant and one as outsider tends to get the lowest treatment. By and large, these findings are comparable to Tomz *et al.* (2007)'s findings with OLS and Kohl (forthcoming)'s results based on modified Poisson models.

Further distinguishing between types of participation in column (2) reveals our expected pattern: formal membership, requiring the most substantial form of trade liberalization, yields the largest treatment effects. The effects are smaller for informal participants with less extensive multilateral commitments. The pattern in column (2) is repeated in columns (3) and (5), with three exceptions. In these cases, the quality of the matches obtained with propensity-score matching was inferior to those obtained with nearest neighbour matching and may be less reliable estimates.

When comparing column (2) and (4), the difference in the magnitude of the treatment effects is due to the inclusion of zero trade flows. However, this hardly changes the systemic pattern of treatment affects that vary by type of WTO participation. Interestingly, the treatment effect of trade between formal members is lower than for informal participants. An explanation might be that WTO participation has a stronger tendency to induce new (extensive margin) trade between countries that initially had very little or no trade, compared to country-pairs

that were already trading as formal WTO members. Another explanation may be that countries were forced to either exit the WTO or to become formal members with the conclusion of the Uruguay Round. This means that some country-pairs were recoded from being informal participants to formal members, even though the scope of their multilateral trade liberalization schemes did not necessarily increase dramatically overnight (Barton *et al.*, 2006).

V. Conclusion

This article extends Baier and Bergstrand (2009a) analysis of the impact of trade agreements on international trade flows using matching econometrics as an alternative, nonparametric technique to account for the endogenous nature of international trade policy. Rather than assuming that all trade agreements provide an equal treatment to cross-border trade, however, this study is among the first to allow for the level of treatment to vary across different types of agreements, while employing some of the latest techniques to control for endogeneity bias. In a similar fashion, the same has been done for types of WTO membership.

Our specific contributions are twofold. First, we show that addressing the heterogeneous nature of trade agreements yields different treatment effects for the volume of international trade and that, by and large, greater economic integration is associated with larger trade effects. Second, we systematically find that pairs of countries in which both countries participated in the WTO experienced larger treatment effects than pairs with only one participant. These findings also hold at a more fine-grained level once we distinguish between formal membership and informal participation. Overall, trade agreements and WTO participation are positively associated with the magnitude of treatment effects for real trade flows. However, the outcomes systematically vary and depend on the type of treatment.

Acknowledgement

The authors thank Steven Brakman, Harry Garretsen, Inmaculada Martínez-Zarzoso and an anonymous referee for helpful suggestions and comments.

Disclosure Statement

No potential conflict of interest was reported by the authors.

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