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Reciprocal trade agreements in gravity models: a meta-analysis

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Abstract

Over the time a large number of reciprocal preferential trade agreements (RTAs) have been concluded among countries. Recently many studies have used gravity equations in order to estimate the effect of RTAs on trade flows between partners. These studies report very different estimates, since they differ greatly in data sets, sample sizes, and independent variables used in the analysis. So, what is the "true" impact of RTAs? This paper combines, explains, and summarizes a large number of results (1827 estimates included in 85 papers), using a meta-analysis (MA) approach. Notwithstanding quite an high variability, studies consistently find a positive RTAs impact on bilateral trade: the hypothesis that there is no effect of trade agreements on trade is easily and robustly rejected at standard significance levels. We provide pooled estimates, obtained from fixed and random effects models, of the increase in bilateral trade due to RTAs. Finally, information collected on each estimate allows us to test the sensitivity of the results to alternative specifications and differences in the control variables considered.

JEL classification: C10; F10.

Keywords: Free Trade Agreements; Gravity equation; Meta-regression analysis; Publication bias.

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1. Introduction.

Preferential agreements are discriminatory policies entailing trade liberalization with respect to a subset of trading partners. The world trading system is characterized by a wide variety of preferential agreements, which can be broadly categorized into two major types: reciprocal (bilateral), entailing symmetric trade liberalization, and nonreciprocal (unilateral), entailing asymmetric trade liberalization aimed at providing support to the country which gains improved market access without being required to open up its own domestic market. The latter, as it well known, have been widely utilized as an instrument for integrating the developing countries into the world trading system.

Traditionally, reciprocal preferential agreements occurred between geographically contiguous countries with already established trading patterns. However, the configuration of these agreements is presently diverse and becoming increasingly complex with overlapping agreements spanning within and across continents in what Bhagwati calls a "spaghetti bowl" of trade relationships.[1] The world has witnessed a veritable explosion of reciprocal preferential trade agreements (RTAs) in the past 15 years. More than half of world trade now occurs within actual or prospective trading blocs, and nearly every country in the world is a member of one or more agreements (Clarete et al., 2003).

RTAs take many forms. The most common are the free trade area (FTA)—where trade restrictions among member countries are removed, but each member maintains its own trade policies towards nonmembers—and the customs union—a FTA where members adopt a common external trade policy. Deeper forms of integration include a common market—a customs union that also allows for the free movement of factors of production—and economic unions, which involve some degree of harmonization of national economic policies. New RTAs, indeed, place considerable emphasis on liberalisation of services, investments and labour markets, government procurement, strengthening of technological and scientific cooperation, environment, common competition policies or monetary and financial integration.

In the literature there are numerous studies analysing the economic impacts of RTAs. The focus of this paper is on estimates of the effects on trade. RTAs might be expected to increase trade between partners, since cheaper imports within the agreement may replace domestic production –trade creation – or crowd out imports from the rest of the world – trade diversion (Viner, 1950; Meade, 1955). However, intra-agreement trade flows may increase even before the formal signature of the agreement, the increases reflecting the impact of unilateral and

[1] As a consequence we decided not to use the term "regional", which is traditionally used as a convenient shortcut, but is inconsistent with the plethora of agreements linking countries around the globe.

multilateral liberalization, as well as the simple fact that agreements may be due to, rather than allow for, growing trade relationships.[2]

The purpose of this review is to use a Meta-Analysis (MA) approach to summarize and analyse the RTAs trade effects estimated in the literature, mostly through gravity models assessing the difference between potential and actual trade flows (see Appendix 1 for details on the agreements considered). The approach takes as individual observations the point estimates of relevant parameters from different studies. An MA can improve the assessment of the parameter describing the RTAs impact by combining all of the estimates, investigating the sensitivity of the overall estimate to variations in underlying assumptions, identifying and filtering out publication bias, and by explaining the diversity in the study results in relation to the heterogeneity of study features through meta-regression analysis (MRA).

In this paper, we firstly consider all point estimates provided in the literature, i.e. including multiple estimates coming from a single study. We test for correlation within and between studies, and estimate meta-regression models using weighted least squares (WLS), checking the robustness and sensitivity of our results. Then, we focus on the effect on bilateral trade of specific trade agreements. Finally, we run a probit regression in order to assess what are the most important factors explaining a positive (and significant) impact of the agreements on bilateral trade flows.

The paper is structured as follows. In Section 2 we briefly review the literature studying the impacts of RTAs on trade, while in Section 3 we present some methodological issues regarding the MA approach. In Section 4 we discuss the explanatory variables and present the econometric results. Finally, Section 5 concludes.

2. The impact on trade of reciprocal preferential trade agreements

Empirical research applies econometric approaches to historical trade data in order to assess the impact of trade agreements on bilateral trade flows. Usually, these approaches use gravity models, based upon Newton's Law of Gravitation, predicting that the volume of trade between two economies increases with their size (proxies are real GDP, population, land area) and decreases with transaction costs measured as bilateral distance, adjacency, cultural similarities (Baldwin, 1994; Eichengreen and Irwin, 1996; Feenstra, 1998; Anderson and van Wincoop, 2003).

The standard formulation expresses the bilateral trade between country *i* and country *j* as:

$$\ln T_{ij} = \beta_0 + \beta_1 \ln(Y_i) + \beta_2 \ln(Y_j) + \beta_3 \ln(Dist_{ij}) + \varepsilon_{ij} \quad (1)$$

[2] Also in the case of multilateral agreements, recent empirical work (Rose, 2004) does not find significant differences between the trade patterns of countries before and after their accession to the GATT/WTO.

where T_{ij} is the country pairs' trade flow, $Y_{i(j)}$ indicate GDP or GNP of i and j, $Dist_{ij}$ is the distance between i and j, finally ε_{ij} is the error term. Most applications of the gravity model search for evidence of actual or potential effects by adding dummy variables for common languages, for common land borders and for the presence or absence of a RTA. Then, the gravity model is estimated as:

$$\ln T_{ij} = \beta_0 + \beta_1 \ln(Y_i) + \beta_2 \ln(Y_j) + \beta_3 \ln(Dist_{ij}) + \beta_4 Adj_{ij} + \beta_5 Lang_{ij} + \gamma RTA_{ij} + \varepsilon_{ij} \quad (2)$$

where Adj_{ij} is a binary variable assuming the value 1 if i and j share a common land border and 0 otherwise, $Lang_{ij}$ is a binary variable assuming the value 1 if i and j share a common language and 0 otherwise, RTA_{ij} is a binary variable assuming the value 1 if i and j have a reciprocal trade agreement in place and 0 otherwise. A positive coefficient for the RTA variable indicates that it tends to generate more trade among its members. In MA, the parameter of interest (estimate of γ) is commonly referred to as the "effect size".

Many papers find positive and statistically significant RTAs dummies, although they are not primarily interested in estimating the RTA effect, i.e. the existence of an RTA is only included as a control variable. On the other hand, if there is a particular interest on specific RTAs, different dummies may be introduced for each agreement.

Some authors distinguish between the increase in the volume of trade within the bloc and the decrease in trade from countries outside the bloc (i.e., trade diversion) by including two dummies for intrabloc and extrabloc trade. An example of a gravity equation that takes into account the trade creation and diversion effects is:

$$\ln T_{ij} = \beta_0 + \beta_1 \ln(Y_i) + \beta_2 \ln(Y_j) + \beta_3 \ln(Dist_{ij}) + \beta_4 Adj_{ij} + \beta_5 Lang_{ij} + \gamma_1 RTA_{kij} + \gamma_2 RTA_{ki-j} \varepsilon_{ij} \quad (3)$$

where RTA_{kij} is a dummy taking value 1 if both i and j are members of bloc k and zero otherwise, and $RTA_{ki\text{-}j}$ is a dummy taking value 1 if i is a member of the bloc but j is not. Accordingly, γ_1 is the coefficient measuring the extent to which trade is influenced by the agreement between i and j (intrabloc trade), and γ_2 is the coefficient associated with extrabloc trade.

Greenaway and Milner (2002) claim that although the impact of any trade agreement is a combination of trade creation and diversion effects, gravity modellers rarely tried to decompose these effects by using dummy variables for members of trade blocs and for non-members, with the expectation of negative coefficients for the latter. Frankel, Stein and Wei (1995) and Frankel and Wei (1997) find evidence of trade creation in European trading blocs from 1970 to 1990, as well as Martìnez-Zarzoso et at (2003), and Mayer and Zignano (2005) for EU and MERCOSUR during the 1990s. Also, Jayasinghe and Sarker (2004) show positive effects for NAFTA in trade of selected agrifood products. Rauch (1996) and Sapir (2001) find negative and significant effect for EFTA. Other RTAs as LAIA and MERCOSUR appear to have been net trade creating in some studies (Gosh and Yamarik, 2002; Elliott and Ikemoto, 2004; Soloaga and Winters, 2000)

and net trade diverting in some others (Carrère, 2006; Krueger, 1999). More recent works (Gosh and Yamarik, 2002; Elliott and Ikemoto, 2004; Cheng and Tsai, 2005; Lee and Park, 2005; Martìnez-Zarzoso and Horsewood, 2005; Carrère, 2006) support the idea that free trade arrangements are generally trade creating.

Recent works investigate the robustness of the determinants of international of trade by means of extreme-bounds analysis (Levine and Renelt, 1992). Ghosh and Yamarik (2004) show that the trade-creating effect is highly sensitive to the choice of other variables included or excluded from the gravity model. Thus, the empirical evidence seems to be rather "fragile". Another work by Baxter and Kouparitsas (2006) tests the robustness of the RTA dummy in gravity equations using three different extreme-bounds approaches. Their analysis gives a mixed view of the relationship between free trade areas and the level of bilateral trade: different methods lead to different outcomes, so results are inconclusive.

The standard estimation method used in gravity equations is the ordinary least squares (OLS). A recent work by Egger (2005) compares four different estimators with respect to their suitability for cross-section gravity models. He recommends a Hausman–Taylor approach that provides consistent parameter estimates, while OLS or the traditional random-effects model are biased.

Most of the articles run regressions from cross-section data either for a single year or for multiple years. Even if panel data allow to pin down the estimates of persistent effects with more accuracy, only very recently gravity equations have been estimated using panel data techniques.

Usually, empirical studies do not take account the endogeneity problem, since countries might enter into a RTA for reasons unobservable to the econometrician and possibly correlated with the level of trade. Baier and Bergstrand (2005) address the endogeneity problem using instrumental variables, Heckman's control-function techniques (Heckman, 1997), and panel-data estimates. They find that the best method to estimate the effect of RTAs on bilateral trade flows is through differenced panel data, while instrumental variables applied to cross-section data are biased and underestimated.

The Global Economic Prospects (2005) of the World Bank provides a meta-analysis of the literature on the impact of regional trade agreements on intra- and extra-regional trade. It finds that although these agreements typically have a positive impact on intra-regional trade, their overall impact is uncertain. The analysis considers 17 research studies providing 362 estimates of the impact on the level of trade between partners. The mean value of these estimates is positive, but there is a high degree of variance about the mean.

In this study we collect papers that: (1) use gravity models for analysing bilateral trade flows; (2) include dummy variables for the presence of RTAs; (3) estimate coefficients through cross-section or panel analyses.

3. Methodological issues

MA is a set of quantitative techniques for evaluating and combining empirical results from different studies (Rose and Stanley, 2005). The central concern of MA is to test the null hypothesis that different point estimates, treated as individual observations (γ), are equal to zero when the findings from this entire area of research are combined.[3]

MA has recently been growing in popularity in economics.[4] Empirical economists have increasing employed meta-analysis methods to summarize regression results particularly in environmental economics (van den Bergh et al, 1997; Florax, 2002, Jeppesen et al 2002), labour economics (Card and Krueger, 1995; Jarrel and Stanley, 1990; Stanley and Jarrel, 1998; Ashenfelter et al., 1999; Longhi et al, 2005; Nijkamp and Poot, 2005; and Weichselbaumer and Winter-Ebmer, 2005), monetary economics (Knell and Stix, 2005) and international trade (Disdier and Head, 2004; Rose and Stanley, 2005).

Although MA is an appealing technique for evaluating and combining empirical results, there is a risk to analyze completely different outcome variables or different explanatory variables (the "Apples and Oranges Problem" as referred to by Glass et al, 1981). In this respect, it is crucial the first step of any MA, namely the construction of a database of estimates. In this application, we only used papers written in English. Papers were selected via extensive search in Google and in databases, such as EconLit and Web of Science. EconLit provides coverage since 1969 to the economics literature including 750 journals. Web of Science provides access to current and retrospective multidisciplinary information from approximately 8700 of the most prestigious, high impact research journals in the world (199 journals in the field of economics), covering the time period from 1992 to the present. With the search in Google, we get papers and working papers that are not published in academic journals. Finally, we traced some specific papers cross-referenced in other works.

The keywords searched for were: "trade agreements", "gravity equation or gravity model" in the title, the abstract or the subject. The first keyword permits to get papers dealing with trade agreements, while the second keyword sorts out papers using a gravity approach. Among the first group of papers we select the papers analyzing trade agreements focusing on bilateral trade

[3] Under the null hypothesis of no effect ($\gamma = 0$), no publication selection and independence, the statistic minus twice the sum of the logarithms of the p-values is distributed approximately as a χ^2 with $2n$ degrees of freedom (Fisher, 1932).

[4] In 2005, the Journal of Economic Surveys dedicated a Special Issue (Vol.19, No. 3) to the use of meta-regression analysis.

flows; in the second group, we selected those studies including trade agreements as a control variable in the gravity equation.

The final sample includes 85 papers (38 published in academic journals, 47 are working papers or unpublished studies) providing 1827 point estimates of the impact of RTAs on bilateral trade: i.e., the coefficient γ or γ_1 in equations (2) and (3), respectively (see Appendices 2 and 3 for details). In case some agreements changed their nature from "unilateral" to "reciprocal" over time, we did not consider the estimates referring to periods when there were only preferential tariff reductions.

It happens quite often that a study provide multiple estimates of the effect under consideration. The presence of more that one estimated reported per study is problematic, because the assumption that multiple observations from the same study are independent draws becomes too strong. On the other hand, counting all estimates equally would tend to overweight studies with many estimates (Stanley, 2001).

Various solutions have been suggested in the literature. Some authors include a dummy variable (fixed effect) for each study that provided more than one observation (Jarrell and Stanley, 1990), others use a panel specification (Jeppesen et al., 2002, Disdier and Head, 2004). Alternatively, one may decide to represent each study with a single observation, identifying a "preferred" estimate, using averages or medians of the estimates from each paper, or randomly selecting one estimate (Card and Krueger, 1995; Stanley, 2001; and Rose and Stanley, 2005). In this case, though, important information is lost in the grouping process and it is not clear which estimate one should use (Jeppensen et al, 2002).

Pooling different estimates into a large sample for meta-analysis raises the question of within-study versus between study heterogeneity. In order to take this into account, a distinction between a fixed effect (FE) and a random effect (RE) models can be made: the former assume that differences across studies are only due to within-variation; the latter consider both between study and within-study variability, and assume that the studies are a random sample from the universe of all possible studies (Sutton et al., 2000).

More specifically, the fixed-effects model assumes that a single, "true" effect ($\hat{\theta}_F$) underlies every study. Following Higgins and Thompson (2002), the $\hat{\theta}_F$ is calculated as a weighted average of the study estimates, using the precisions as weights:

$$\hat{\theta}_F = \frac{\sum_{i=1}^{n} \hat{\theta}_i w_i}{\sum_{i=1}^{n} w_i} \tag{4}$$

where $\hat{\theta}_i$ is the individual estimate of the RTA effect (our γ_i), and the weights w_i are inversely proportional to the square of the standard errors:

$$w_i = \frac{1}{Se(\hat{\theta}_i)^2} \tag{5}$$

So that studies with smaller standard errors have greater weight that studies with larger standard errors.

A field of the literature showing high heterogeneity cannot be summarized by the fixed-effects estimate under the assumption that a single "true" effect underlies every study. As a consequence, the fixed-effects estimator is inconsistent and the random effects model is more appropriate.

The random-effects model assumes that there are real differences between all studies in the magnitude of the effect. Unlike the fixed effects model, the individual studies are not assumed to be estimating a true single effect size, rather the true effects in each study are assumed to have been sampled from a distribution of effects, assumed to be Normal with mean 0 and variance τ^2. The weights incorporate an estimate of the between-study heterogeneity, $\hat{\tau}^2$, so that the random effects estimate ($\hat{\theta}_R$) is equal to (Higgins and Thompson, 2002):

$$\hat{\theta}_R = \frac{\sum_{i=1}^{n} \hat{\theta}_i w^*_i}{\sum_{i=1}^{n} w^*_i} \tag{6},$$

where the weights are equal to:

$$w^*_i = (w_i^{-1} + \hat{\tau}^2)^{-1} \tag{7}.$$

Allowing for the between-study variation has the effect of reducing the relative weighting given to the more precise studies. Hence, the random effects model produces a more conservative confidence interval for the pooled effect estimate.

A test of homogeneity of the θ_i is provided by referring the statistic

$$Q = \sum_{i=1}^{n} w\left(\hat{\theta}_i - \hat{\theta}_F\right)^2 \tag{8}.$$

to a χ^2 distribution with $n-1$ degrees of freedom. If Q exceeds the upper-tail critical value, the observed variance in estimated effect sizes is greater than what we would expect by chance if all studies shared the same 'true' parameter (Higgins and Thompson, 2002).[5]

The Q test should be used cautiously, among other things because its power is low (Sutton 2000): when we have a large sample of observations, for example, Q is likely to be rejected even when the individual effect sizes do not differ much. Anyway, its computation is an intermediate step to compute the preferred tests – H^2 and I^2 – that we are going to use in our analysis.

The statistic H^2 provides a possible measure of the amount of heterogeneity:

$$H^2 = \frac{Q}{n-1} \tag{9}$$

through the ratio of Q over its degrees of freedom. In absence of heterogeneity

$$E[Q] = n-1 \tag{10},$$

so that $H^2 = 1$ indicates homogeneity in effect sizes.

The I^2 statistic, on the other hand, measures the percentage of variability in point estimates that is due to heterogeneity rather than sampling error:

$$I^2 = \frac{H^2 - 1}{H^2} = \frac{Q - n + 1}{Q} \tag{11}$$

In the following, after multiplying the I^2 statistic by 100, we will assign adjectives of low, moderate, and high to values of I^2 lower or equal to 25%, 50%, and 75%. respectively.

The simple mean of estimates could be misleading in presence of more than one mode or outliers in the sample of estimates, because a large part of the estimates may lie to one side of the mean value. If the distribution is multimodal or there are outliers (as extreme data points) the mean could be biased. Skewness is usually tested by comparing mode, median and mean of the distribution. However, this would not be true in the case of symmetrically distributed outliers, since they tend to cancel out each other, or when outliers have smaller statistical weights than other data points so that they contribute less to the mean. In any case, some authors prefer to remove the outliers, since they compress the variation of the rest of the sample and are likely to lead to fragile findings (Disdier and Head, 2004); while others claim that removing outliers and extreme results at an early stage of the meta-analysis could introduce (substantial) bias into the meta-results, and the influence of removing outliers should be explored in a sensitivity analysis (Stanley 2001, Florax 2002).

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[5] A moment-based estimate of $\hat{\tau}^2$ may be obtained by (8) equating the observed value of Q with its expectation $E[Q] = \hat{\tau}^2\left(\sum_{i=1}^{n} w_i - (\sum_{i=1}^{n} w_i^2 \Big/ \sum_{i=1}^{n} w_i)\right) - n + 1$ yielding $\hat{\tau}^2 = \dfrac{Q - n + 1}{\sum_{i=1}^{n} w_i - (\sum_{i=1}^{n} w_i^2 \Big/ \sum_{i=1}^{n} w_i)}$.

Finally, there is a general belief that publication bias occurs when researchers, referees, or editors have a preference for statistically significant results. The publication bias may greatly affect the magnitude of the estimated effect. Several meta-regression and graphical methods have been envisaged in order to differentiate genuine empirical effect from publication bias (Stanley, 2005).

The simplest and conventional method to detect publication bias is by inspection of a funnel graph diagram. The funnel graph is a scatter diagram presenting a measure of sample size or precision of the estimate on the vertical axis, and the measured effect size on the horizontal axis. The most common way to measure precision is the inverse of the standard error (1/Se). Asymmetry is the mark of publication bias: in the absence of such a bias, the estimates will vary randomly and symmetrically around the true effect. The diagram, then, should resemble an inverted funnel, wide at the bottom for small-sample studies, narrowing as it rises.

A Meta-regression Analysis (MRA) model can be used to investigate and correct publication bias. The model regresses estimated coefficients (γ_i) on their standard errors (Card and Krueger, 1995; Ashenfelter et al 1999):

$$\gamma_i = \beta_1 + \beta_0 Se_i + \varepsilon_i \tag{12}$$

In the absence of publication selection, the magnitude of the reported effect will vary randomly around the 'true' value, β_1, independently of its standard error. Then, when the standard error of the effect of RTA is not significantly different from 0 at any conventional level, the publication bias is not a major issue.[6]

Since the studies in the literature may differ greatly in data sets, sample sizes, independent variables, variances of these estimated coefficients may not be equal. As a consequence, meta-regression errors are likely to be heteroscedastic, though the OLS estimates of the MRA coefficients remain unbiased and consistent.

A weighted least squares (WLS) corrects the MRA for heteroscedasticity, and permits to obtain efficient estimates of equation (12) with correct standard errors. The WLS version of equation (12) is obtained dividing regression equation by the individual estimated standard errors:

$$\frac{\gamma_i}{Se_i} = t_i = \beta_0 + \beta_1 (1/Se_i) + e_i \tag{13}$$

where t_i is the conventional t-value for γ_i, the intercept and slope coefficients are reversed and the independent variable becomes the inverse of Se_i.[7] The potential for heteroscedasticity, then,

[6] In such a case, the standard error can be omitted from the regression.

[7] Longhi et al. (2006) weight each effect size by the square root of the sample size from which it is estimated. Since there is no relationship between the standard errors of the estimated effect sizes and the sample sizes from which

causes the meta-analyst to direct his attention towards the reported t-statistics (Stanley and Jarrell, 2005). Equation (13) is the basis for the funnel asymmetry test (FAT), and it may now be estimated by OLS. In the absence of publication selection the magnitude of the reported effect will be independent of its standard error, then β_0 will be zero.

Stanley (2001) proposes a method to remove or circumvent publication selection by using the relationship between a study's standardized effect (its t-value) and its degrees of freedom or sample size n as a means of identifying genuine empirical effect rather than the artefact of publication selection:

$$\ln|t_i| = \alpha_0 + \alpha_1 \ln n_i + v_i \tag{14}$$

When there is some genuine overall empirical effect, statistical power will cause the observed magnitude of the standardized test statistic to vary with n: this method is known as meta-significance testing (MST).

Information on interpretation of meta-regression tests is summarized in Table 1. In the next section we will use these approaches in order to assess genuine empirical effects beyond random and selected misspecification biases.

Table 1: ***MR tests for publication bias and empirical significance***

Test	MRA model	H_1	Implications
Funnel asymmetry *Precision-effect*	$t_i = \beta_0 + \beta_1(1/Se_i) + e_i$	$\beta_0 \neq 0$ $\beta_1 \neq 0$	*Publication bias* *Genuine empirical effect*
Meta-significance	$\ln\|t_i\| = \alpha_0 + \alpha_1 \ln n_i + v_i$	$\alpha_1 > 0$	*Genuine empirical effect*
Joint precision-effect/ *meta-significance*	*Both of the above MRA tests*	$\beta_1 \neq 0$ $\alpha_1 > 0$	*Genuine empirical effect*

Source: Stanley, 2005

4. Meta-analysis regression

The standard meta regression model includes a set of explanatory variables (X) to integrate and explain the diverse findings presented in the literature:

$$\gamma_{ji} = \beta_1 + \beta_0 Se_{ji} + \sum_{k=1}^{K} \alpha_k X_{jik} + \varepsilon_{ji} \tag{15}$$

where γ_{ji} is the reported estimate i of the jth study in literature, β expresses the true value of the parameter of interest, X_{jik} is the independent variable which measures relevant characteristics of an empirical study and explains its systematic variation from other results in the literature, α_k is

they are estimated, standard errors can still be used as an explanatory variable in the meta-regression in order to correct for publication bias.

the regression coefficient which reflects the biasing effect of particular study characteristics, and ε_{ji} is the disturbance term.

As it was mentioned in the previous section, meta-regression errors are likely to be heteroscedastic. Accordingly, a common practice in meta-regression analysis is to weigh each effect by some measure of precision of the estimated effect and then explain the heterogeneity in study results by means of a linear regression model estimated with weighted least squares (WLS). Dividing (15) by the standard error of the estimates we get:

$$\frac{\gamma_{ji}}{Se_{ji}} = t_{ji} = \beta_0 + \beta_1(1/Se_{ji}) + \sum_{k=1}^{K}(\alpha_k X_{jik}/Se_{ji}) + e_{ji} \qquad (16).$$

The previous regression may still lead to inefficient, though consistent, estimators, since it does not take into account the dependence among estimates obtained in the same study. In order to get correct standard errors, we adopt a "robust with cluster" procedure, adjusting standard errors for intra-study correlation.[8] Each cluster identifies the study the estimate belongs to: this changes the variance-covariance matrix and the standard errors of the estimators, but not the estimated coefficients themselves.

Finally, we adopt a specification that investigates factors influencing whether the estimated effects are positive and significantly different from zero. The estimated model is given by:

$$s_{ji} = a + \sum_{k=1}^{K} b_k X_{jik} + e_{ji} \qquad (17)$$

where the dependent variable is a dummy (s) that takes the value 1 if the estimated effect size is positive and statistically significant The probability that an estimated effect size is positive and significant is explained by a set explanatory variables (X) and estimated running a probit regression.

4.1 Explanatory variables

The set of variables X in equation (16) can be partioned in two groups: the first includes dummies explaining the diversity in the results from a methodological point of view; the second includes dummies regarding features of the studies considered.

The methodological dummies included in the MRA are based on a recent survey of the errors in the empirical literature applying gravity equations carried out by Baldwin and Taglioni (2006). They rank the major errors assigning different medals according to the seriousness of the consequences implied. The gold medal of classic gravity model mistakes arises from the correlation between the omitted variables and the trade-cost terms: this leads to biased estimates.

[8] The "robust" specification adopts the Huber/White/sandwich estimator of variance in place of the traditional one. Some authors (Jeppensen et al, 2002; Disdier and Head, 2004) adopt a panel specification, but such a choice seems questionable: since any ordering of estimates is arbitrary, the data do not form a proper panel.

In particular, the estimated trade impact will be upward biased if the omitted variables and the "variable of interest" (RTAs, in our case) are positively correlated. "The point is that the formation of currency unions is not random but rather driven by many factors, including many of the factors omitted from the gravity regression›" (Baldwin and Taglioni, 2006, p. 9): apparently, the same point may be raised with reference to RTAs.

Possible solutions to the gold medal problem include country effects (a dummy that is one for all trade flows that involves a particular country) and pair effects (a dummy that is one for all observations of trade between a given pair of countries). Country dummies remove the cross-section bias, but not the time-series one, and this is a serious shortcoming since omitted factors affecting bilateral trade costs often vary over time. Accordingly, pair dummies perform better with panel data, but they cannot work be used with cross-section data (the number of dummies equals the number of observations) and, in any case, they provide a partial answer to the gold-medal bias. In this respect, it is worth recalling that point estimates in our sample are obtained from different datasets: cross-section data, pooled cross-section time series or panel data. The most recent gravity model estimations, though, tend to use panel data regression techniques,[9] since cross-sectional and pooled regression models may be affected by the exclusion or mismeasurement of trading pair–specific variables (Baldwin, 2006).

The silver medal mistake arises from the fact that different measures of bilateral trade flows. Even if some studies focus on directional trade using only data on bilateral import or export flows, the most frequently used measure is the average of bilateral trade, namely the average of the two-way exports. However, gravity models are usually estimated in log form: in such a case, computing the wrong average trade (the arithmetic average corresponding to the log of the sums, rather than the geometric average corresponding to the sum of the logs) tends to overestimate the trade effects. Moreover, it should be recalled that the difference between the sum of the logs and the log of the sums gets larger in case of unbalanced trade flows (Baldwin, 2006).

Another problem related to the log specification is due to the existence of zero trade flows. Several methods have been proposed to tackle this issue: a large part of empirical studies simply drops the pairs with zero trade from the data set and estimate the log-linear form by OLS;[10] some authors estimate the model using a Tobit estimator with $T_{ij}+1$ (where T_{ij} represents the bilateral trade) as the dependent variable; others employ a Poisson fixed effects estimator. Generally, though, all of these procedures lead to inconsistent estimates (Silva and Tenreyro, 2003 and 2005).

[9] In our set of papers there are a few using dynamic panel techniques, but most of them rely on static panel gravity models.

[10] When the zero values are thrown away, we face a selection problem: this can be handled through an Heckman two-steps procedure.

The bronze medal mistake refers to a common practice in the literature, namely to deflate the nominal trade values by the US aggregate price index. Given that there are global trends in inflation rates, such a procedure probably creates biases via spurious correlations (Baldwin and Taglioni, 2006).

Finally, one of the most widely cited theoretically grounded gravity model (Anderson and van Wincoop, 2003) shows that the typical gravity equation should account for the so-called "multilateral resistance" term, since what really matters is bilateral *relative* (rather than absolute) openness. An omission of this term may lead to inconsistent estimates. Anderson and van Wincoop (2003) derive a practical way of using the full expenditure system to obtain a specification of a gravity equation that can be interpreted as a reduced form of a model of trade with micro foundations. Since this solution is based on the assumption of constant trade costs, its application is only consistent with cross-section data analysis.

Coming to the dummies describing different features of the studies considered, we expect that RTAs and their impact on trade may have changed over time. Accordingly, we use four dummies –before 1970, the 70s, the 80s, and the 90s – in order to collect studies using data only referred to a specific time period. Moreover, it seems worth distinguishing published from unpublished studies, as well as papers primarily interested in estimating the RTA effects from the papers that include it as a mere control variable. In both cases we do expect to find larger RTA effects, as a consequence of the preference of researchers, and especially those specifically interested in RTAs, for positive and possibly significant results.

4.2 Econometric results

- Sample of 85 estimates

The use of a single observation for each study begs the question of how to make the choice. Some authors identify a "preferred" estimate (Card and Krueger, 1995; Rose and Stanley, 2005). Others use averages or medians of the estimates from each paper, or select a single measure either randomly or using a more objective statistical procedure, such as the highest R^2 for the corresponding regression (Disdier and Head, 2004). Bijmolt and Pieters (2001) show that the procedures using a single value for each study generate misleading results. Indeed, if we look at the fixed and random effects estimates based on study's minimum, median and maximum estimate of γ, we obtain very different results (Table 2).

Table 2: Sensitivity of the choice of "preferred" estimate

		Pooled Estimate	Lower Bound of 95% CI	Upper Bound of 95% CI	p-value for H_0: no effect	test Q (p-value)	H^2	I^2
Min	**Fixed-effects**	0.013	0.006	0.019	0.00	0.00	49.38	98%
	Random-effects	0.113	0.049	0.178	0.00			
Median	**Fixed-effects**	0.088	0.078	0.097	0.00	0.00	40.04	98%
	Random-effects	0.531	0.455	0.608	0.00			
Max	**Fixed-effects**	0.414	0.400	0.427	0.00	0.00	99.50	99%
	Random-effects	1.354	1.188	1.520	0.00			

In all cases we reject the null hypothesis of homogeneity among estimates and both the H^2 and I^2 statistics confirm the results of the Q test. Apparently, pooled estimates are decreasing as one moves towards the lower percentiles within studies.

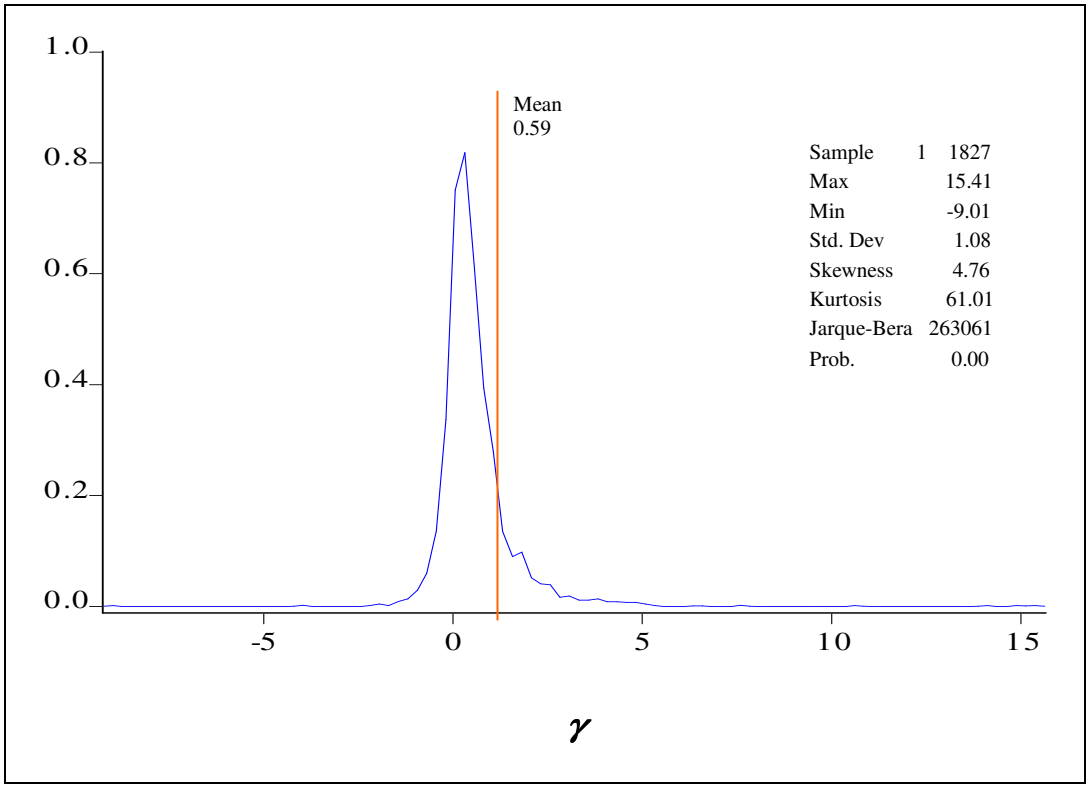
All the confidence bounds are positive and strongly reject the null hypothesis of no effect. The lowest estimate (minimum estimates – random effects) implies an increase in trade of 12% ($e^{0.11} - 1 = 0.12$), while the highest estimate (maximum estimates – random effects) would be larger than 285% ($e^{1.35} - 1 = 2.85$).

Given these results, and considering that we would lose valuable information especially from studies that estimate gravity equations for multiple years. In the following, then, we present the results obtained from the largest sample of available observations.

- Sample of 1827 individual estimates

Our database consists of 1827 effect sizes collected from 85 papers estimating the effect of RTAs on international trade. Figure 1 provides the kernel density estimate of the effect sizes. The mean RTA effect (vertical line) is 0.59 and the median is 0.38. These simple statistics do not make use of any information on the precision of each estimate. However, if we combine these effect sizes to test the null hypothesis that $\gamma = 0$, the F-test shows that this hypothesis is rejected at any standard significance level (prob. F-statistic = 0.00).

Figure 1: ***Distribution of RTA effects (γ).***



The estimated trade coefficients range from -9.01 to 15.41, though the majority of coefficients are clustered between zero and one. We employ the Grubbs test in order to detect the existence of outliers (Disdier and Head, 2004), finding 38 extreme values. Since the removal of these extreme values could bias the meta-results, we prefer to deal with them inserting a dummy variable (equal to 1 for outliers and 0 otherwise) in the MRA.

The distribution in the Figure 1 is clearly lopsided, because few estimates (312 out of 1827) report negative RTAs effects. The values are not symmetrically distributed, with a longer tail to the right than to the left, and the distribution appears to be positively skewed. This is certainly not surprising, since economic theory predicts a positive impact of RTAs on trade.

Table 3 shows combined meta-estimates of γ together with the p-values associated with the tests for the lack of any effect and the homogeneity of the data. Also in this case, the Q test is supplemented with the H^2 and I^2 statistics. All the test consistently reject the homogeneity hypothesis, and the heterogeneity between estimates leads to large differences among fixed and random effects results.

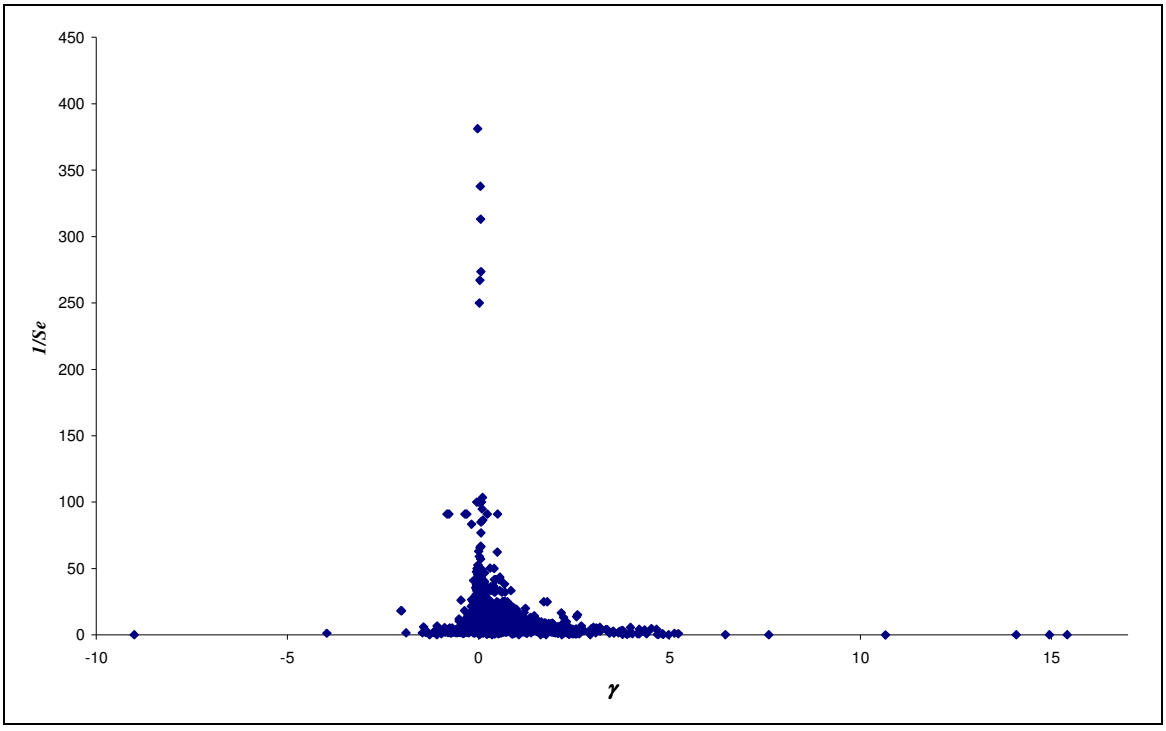
Table 3: ***Meta-Analysis of 1827 estimates of RTAs effect on trade***

	Pooled Estimate	Lower Bound of 95% CI	Upper Bound of 95% CI	p-value for H_0: no effect	test statistic Q (p-value)	H^2	I^2
Fixed-effects	0.100	0.097	0.101	0.00	0.000	47.65	98%
Random-effects	0.500	0.482	0.515	0.00			

The null hypothesis is easily rejected, confirming the existence of a genuine impact of RTAs on bilateral trade. The smaller fixed-effects estimate indicates that RTAs raise trade by 10%, while the random effects estimate indicates an increase up to 65%. Appendix 4 presents the results included in Table 3 for each of the 85 studies. For most of the studies the null hypothesis of no effect is easily rejected at any standard significance level. The fixed and random effects estimators do not differ greatly in magnitude but, due to the heterogeneity characterizing most of the studies, the random effects estimates are to be considered more reliable.

Following Stanley (2005), we look for publication bias in our sample of disparate effects sizes plotting the funnel graph.

Figure 2*: Funnel graph of 1827 individual estimates*



Even though the graph in Figure 2 slightly resembles a funnel, it does not present the symmetry that is crucial to exclude publication bias. Estimates of RTAs effects seem to indicate a positive effect on trade, but Figure 2 clearly shows that the plot is overweighted on the right side. Then publication selection assumes a particular direction.

The six different estimates with the smallest standard errors do not differ significantly from each other. The average of the top six points on the graph, that is the estimates associated with the smallest standard errors, is equal to 0.04, implying a 4.1% increase in trade. Consequently, if research reporting was unbiased, estimates should vary randomly and symmetrically around the value 0.04, whereas the simple average of all 1827 estimates is 0.59, implying a 80% increase in trade.

Table 4 reports the result of the MRA tests. Robust ordinary least squares estimation is used and standard errors are recorded in parenthesis. Both tests confirm the presence of publication bias and the existence of a positive impact. The estimate of β_0 significantly different from 0 confirms the apparent asymmetry of the funnel graph; while the β_1 estimate different from 0 and a positive value for the α_1 estimate, both statistically significant, provide evidence of a genuine empirical effect.

Table 4: ***MRA tests of Effect and Publication Bias***

Variables		**Dependent Variables**	
		1: t	*2:* $ln\|t\|$
β_0:	*intercept*	3.53*	
		(0.16)	
β_1:	1/*Se*	0.03*	-
		(0.01)	
α_1:	*Ln*(*n*)	-	0.25*
			(0.02)
Obs		1827	1642
R-squared		0.01	0.14
S.E. of regression		6.18	1.18

Column 2: studies not reporting the number of observations are excluded

*Standard errors are reported in parenthesis – *: significant at 1 percent.*

After adding all of the explanatory variables discussed in the previous section, we dropped the insignificant variables, one at a time, to yield the results for equation (16) presented in Table 5. The two columns 1 and 2 present the estimated coefficients (the standard errors adjusted for 85 studies/clusters are reported in parentheses) with and without the introduction of a fixed effect for each type of agreement. Results show a significant general RTA effect on trade exceeding 11%. Comparing the two columns it appears that the results are by far and large robust.

The use of the *log of average bilateral trade flows* rather than the average of the logs of the trade flows leads to significantly higher estimates of the RTAs effect. This result confirms and provides a quantitative assessment of the silver medal mistake pointed out by Baldwin (see

section 4.1): the confusion between the log of the average and the average of the logs tends to inflate the gravity estimates by 3 standard errors.

The *time effects* dummy is equal to "1" when time fixed effects are included in the regression: this should control for the global trends existing in the data. In particular, time dummies are expected to offset the bronze medal error implied by the mistaken deflation procedure. The negative sign associated with this variable shows that uncorrected studies tend to overestimate the RTAs impact on trade.

The *country effect* dummy is equal to "1" when the original studies use dummies to characterize trade flows involving a particular country. Since this dummy is used to correct for the "gold medal" mistake pointed out by Baldwin and Taglioni, the positive coefficient suggests that the omitted variable bias leads to a serious underestimation of the RTAs trade impact.

Regarding the typologies of data used, we introduce 2 dummies with self-explanatory names: *cross-section* and *pooled*.[11] Results are negative for both variables confirming that cross-sectional and pooled regression models may be affected by the exclusion or mismeasurement of trading pair–specific variables (Baldwin, 2006). More specifically, our results support the claim by Baier and Bergstrand (2005) that cross-section estimates are downward biased due to the endogeneity problem.

As far as the estimation methods are concerned, the dummy *ols* equal to "1" if estimates are obtained through simple OLS and "0" whether estimates are obtained with other approaches(i.e., instrumental variables, Hausman-Taylor, etc.). We find a positive and significant coefficient for the *ols* dummy. As it was mentioned in the previous section, the OLS-estimator may yield biased and inconsistent estimates due to omitted variables and selection bias. Trade between any pair of countries is likely to be influenced by certain unobserved individual effects, if the unobserved effects are correlated with the explanatory variables, coefficients of the latter may be higher because they incorporate these unobserved effects. On the other hand, the dummy *random effects* is equal to "1" when a panel model is estimated through a random effects approach. If we believe, following Baier and Bergstrand (2005), that there unobserved time-invariant bilateral variables influencing simultaneously the presence of a RTA and the volume of trade, the positive coefficient of this dummy provides an estimate of the upward bias deriving from the assumption of zero correlation between unobservables and RTAs.

Coming to the variables related to each study characteristics, we find a negative and highly significant coefficient for the *agreement* dummy taking the value "1" if the original paper used a variable for each type of agreement. Studies focusing on specific RTAs, then, tend to estimate

[11] To avoid collinearity problems we do not include an additional dummy variable for panel studies.

much lower impacts on trade: apparently, the estimation problems do not cancel out when all the RTAs are lumped together, rather they make the overestimation bias even larger.

The negative coefficient found for the dummy *published* may seem at odds with the picture provided by the funnel graph. However, the negative bias of the published results may be a good news, suggesting that editors do a pretty good job in excluding the highest (and possibly less realistic) results. On the other hand, the dummy *interested* is strongly positive, hinting to the existence of a "psychological bias", since authors primarily interested in estimating the RTA effect tend to report larger results.

As it was mentioned in the previous section, we handle the extreme values in the sample adding a dummy called *outliers*. The estimated coefficient of this variable is clearly positive, since most outliers indicate a positive and very high effect size of RTAs. In any case, the removal of this dummy does not significantly affect the results.

Finally, we find significant and negative coefficients associated with the dummies for period ranges (except for the *1970s*). The effect size is much smaller before 1970, while the most recent studies seem to get higher estimates. Such a result is consistent with the often noted evolution from 'shallow' to 'deep' regional integration agreements, where the latter reduce trade costs through behind-the-border reforms.

Table 5: ***Multivariate Meta-Regression Analysis (MRA) of Common RTAs Effects***

Variables	*Coefficient* (*Robust with Cluster Standard Errors)*	*Coefficient* (*Robust with Cluster Standard Errors)*
Intercept	3.27 (0.43)***	2.73 (0.46)***
1/Se$_i$	0.11 (0.05)**	0.11 (0.06)**
Log of average trade	0.13 (0.06)**	0.15 (0.06)***
Time effects	-0.14 (0.06)**	-0.14 (0.07)**
Country effects	0.35 (0.10)***	0.36 (0.11)***
Random effects	0.14 (0.08)*	0.17 (0.08)**
Cross-section	-0.21 (0.04)***	-0.21 (0.07)***
Pooled	-0.19 (0.04)***	-0.19 (0.05)***
Ols	0.21 (0.04)***	0.24 (0.05)***
Agreement	-0.10 (0.05)**	-
Interested	0.31 (0.07)***	0.30 (0.07)***
Published	-0.10 (0.04)***	-0.15 (0.05)***
Outliers	3.03 (0.26)***	2.53 (0.60)***
Before 1970	-0.35 (0.12)***	-0.29 (0.14)**
1970s	0.04 (0.22)	0.11 (0.24)
1980s	-0.22 (0.09)***	-0.16 (0.09)*
After 1990	-0.20 (0.05)***	-0.17 (0.06)***
Afta	-	0.09 (0.19)
Aifta	-	-0.17 (0.06)***
Anzcer	-	0.24 (0.41)
Bfta	-	1.90 (0.18)***
Cacm	-	-0.04 (0.14)
Can	-	0.36 (0.15)***
Caricom	-	-0.06 (0.10)
Cefta	-	0.12 (0.26)
Ciscu	-	1.57 (0.19)***
Custa	-	-0.69 (0.07)***
Efta	-	-0.12 (0.08)
Eu	-	-0.09 (0.05)*
Lafta	-	0.69 (0.08)***
Laia	-	-0.12 (0.09)
Mercosur	-	0.12 (0.09)
Nafta	-	0.12 (0.30)
Us-Chile	-	-0.67 (0.10)***
Us-Israel	-	0.26 (0.08)***
Obs	1827	1827
No of Clusters	85	85
R-squared	0.25	0.34
Prob > F	0.00	0.00
S.E. of regression	5.40	5.10

****:significant at 1 percent; **: significant at 5 percent; *: significant at 10 percent; All moderator variables are divided by Se$_i$*

- Focus on single RTAs.

46 studies out of 85 estimate the RTAs impact on trade introducing different dummies for each trade agreement, yielding 1338 estimates. Table 6 summarizes the main results obtained for each RTA.

Table 6: ***Descriptive statistics of estimates of single RTAs***

Variable: γ	**Obs**	**Mean**	**Std. Dev.**	**Min**	**Max**
RTAs	489	0.62	0.65	-3.97	4.83
Afta	41	0.81	0.69	-0.07	2.35
Aifta	10	0.06	0.04	0.00	0.10
Anzcer	15	0.87	1.10	-0.16	3.98
Bfta	24	2.96	0.43	2.37	3.77
Cacm	37	1.19	1.02	0.01	4.40
Can	13	1.34	0.55	0.12	2.22
Caricom	37	2.02	1.79	-0.35	5.23
Cefta	57	0.41	0.36	-0.51	1.52
Ciscu	6	2.66	0.60	1.98	3.37
Custa	63	-0.23	0.64	-1.89	2.26
Efta	343	0.23	0.50	-1.38	2.17
Eu	524	0.52	1.47	-9.01	15.41
Lafta	5	0.98	0.92	0.30	2.57
Laia	9	0.53	0.12	0.39	0.82
Mercosur	47	0.72	0.73	0.12	4.35
Nafta	90	0.90	1.06	-1.47	3.89
Us-Chile	5	0.27	0.66	-0.30	1.42
Us-Israel	12	0.82	0.75	-0.08	2.41

The largest number of observations refers to EU, one of the oldest and most studied case of economic integration. Manifestly, the range between minimum and maximum estimates are very large for the most of agreements, showing the large variety of estimates provided in the literature.

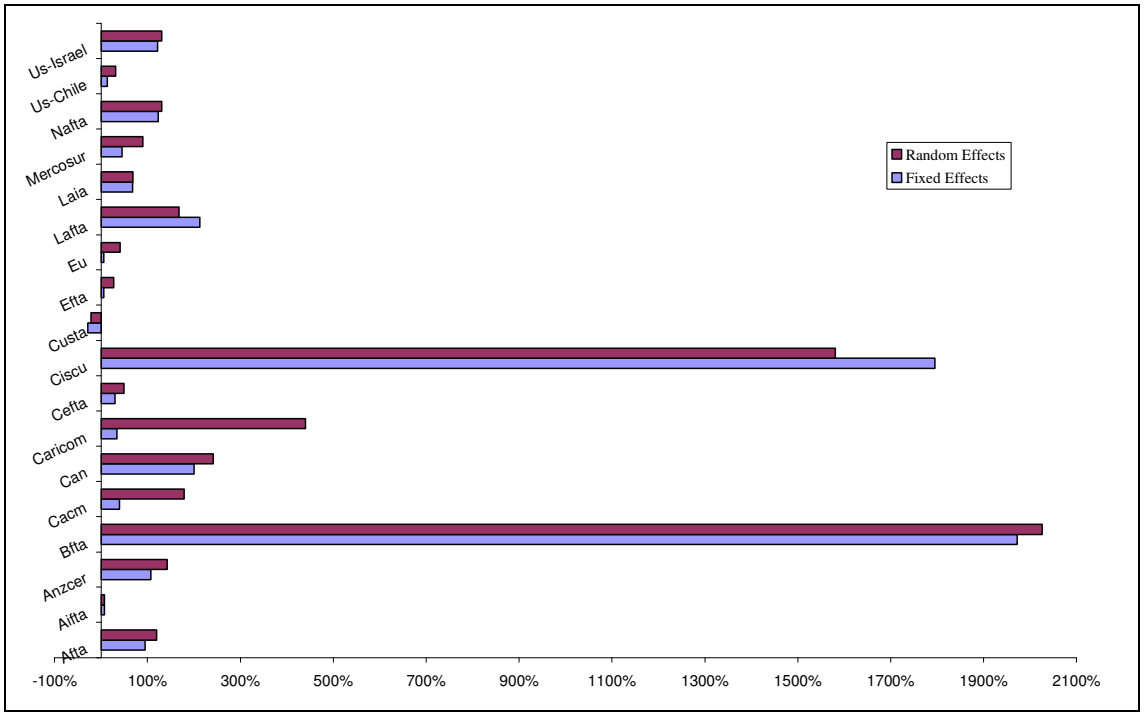
Table 7 presents the results of the MA for the RTAs for which estimates are available. The tests show that random effects estimates would be the most appropriate in most of the cases. Only 4 out of the 18 agreements do not show significant differences between fixed and random effects estimates (in bold in the table), and most of these cases are characterized by a fairly low number of observations.

Table 7: ***Meta-Analysis of estimates of specific RTAs***

RTA		Pooled Estimate	Variation in Trade (%)	Lower Bound of 95% CI	Upper Bound of 95% CI	test Q (p-value)	H^2	I^2	No. of Estimates
Afta	Fixed	0.67	95%	0.63	0.70	0.00	30.92	97%	41
	Random	0.79	120%	0.60	0.99				
Aifta	**Fixed**	**0.07**	**7%**	**0.05**	**0.08**	**0.18**	**1.40**	**29%**	**10**
	Random	**0.07**	**7%**	**0.05**	**0.09**				
Anzcer	Fixed	0.73	107%	0.67	0.78	0.00	117.93	99%	15
	Random	0.88	142%	0.22	1.55				
Bfta	**Fixed**	**3.03**	**1972%**	**2.92**	**3.14**	**0.04**	**1.57**	**36%**	**24**
	Random	**3.06**	**2026%**	**2.91**	**3.21**				
Cacm	Fixed	0.34	40%	0.31	0.37	0.00	30.51	97%	37
	Random	1.03	179%	0.83	1.23				
Can	Fixed	1.10	200%	1.00	1.19	0.00	5.78	83%	13
	Random	1.23	242%	0.97	1.49				
Caricom	Fixed	0.29	34%	0.26	0.32	0.00	53.91	98%	37
	Random	1.69	440%	1.42	1.96				
Cefta	Fixed	0.26	30%	0.24	0.28	0.00	13.95	93%	57
	Random	0.40	49%	0.30	0.50				
Ciscu	Fixed	2.94	1795%	2.69	3.19	0.02	2.61	62%	6
	Random	2.82	1581%	2.38	3.26				
Custa	**Fixed**	**-0.34**	**-29%**	**-0.36**	**-0.32**	**0.00**	**2.13**	**53%**	**63**
	Random	**-0.25**	**-22%**	**-0.36**	**-0.14**				
Efta	Fixed	0.05	6%	0.05	0.06	0.00	18.92	95%	343
	Random	0.24	27%	0.21	0.28				
Eu	Fixed	0.05	6%	0.05	0.06	0.00	59.27	98%	524
	Random	0.35	41%	0.32	0.37				
Lafta	Fixed	1.14	213%	1.07	1.21	0.00	133.70	99%	5
	Random	0.98	168%	0.16	1.81				
Laia	**Fixed**	**0.52**	**68%**	**0.47**	**0.57**	**0.13**	**1.58**	**37%**	**9**
	Random	**0.52**	**69%**	**0.45**	**0.60**				
Mercosur	Fixed	0.37	45%	0.35	0.39	0.00	16.36	94%	47
	Random	0.64	90%	0.55	0.74				
Nafta	Fixed	0.80	123%	0.76	0.85	0.00	17.98	94%	90
	Random	0.84	131%	0.64	1.04				
Us-Chile	Fixed	0.13	14%	-0.04	0.30	0.00	9.94	90%	5
	Random	0.27	31%	-0.31	0.85				
Us-Israel	Fixed	0.80	122%	0.72	0.87	0.00	19.87	95%	12
	Random	0.84	131%	0.47	1.21				

The largest effect is registered for the Baltics-RTA (BFTA): the fixed effects estimate suggests an increase in trade around 2000%! Other agreements presenting exceedingly high estimates are the CISCU (1581%) and the Caribbean Community (400%) (Figure 3) .

Figure 3*: Meta-Analysis of estimates of specific RTAs*



Looking at the most widely studied agreements – EU, EFTA and NAFTA –, the largest impact is for NAFTA (131%), while the European agreements register much lower, but possibly more realistic values: 27% in the case of EFTA, 41% for the EU. It is also worth noting that custom unions – EU, CARICOM, MERCOSUR, CACM, CISCU – does not seem to consistently outperform the free trade areas in terms of trade impact. Indeed, in the meta-analysis regression the coefficient of the CU variable was never significant.

4.3 Probit Significance Equation

In our dataset of 1827 effect sizes, 1134 are significantly different from zero at the level of 5%, and 1048 of these estimates are positive. This is the sample used in the probit estimate (equation 17). The results in terms of the marginal effects at the sample means are shown in Table 8. The value at the mean of the linear combination of the explanatory variables (Z) is 0.22, while the marginal probability of finding a positive and significant impact on trade is 0.4.

Since we use the same set of variables presented in section 4.1, we can compare these results with those presented in table 5. We single out 3 groups of variables: significant variables in both cases with the same sign; significant variables in both cases with opposite signs; significant variables in the probit regression that were dropped from the MRA.

In the first group we find the dummies for the different decades, the *log of the average*, the analysis of specific RTAs , the presence or not of country effects, the use of a random effects model in panel estimations, and the primary focus of the analysis. In these cases, then, the probit

estimates are largely consistent with the evidence provided by the MRA. Firstly, the assessments of older agreements (or first stages of implementation) are less likely to detect a positive impact on trade: using data *before 1970*, for instance, reduces the probability by almost 40 percent. By the same token, the use of data on specific agreements reduces the probability of estimating a positive impact on trade by 20 percent, as it could have been expected given that the estimates provided by these studies are generally lower. On the contrary, confusion between the log of the average and the average of the logs (the "silver medal mistake"), omitted variables bias (*country effects*), panel estimates through random effects, and interest in estimating the RTA impact substantially raise the probability to find a positive and significant effect: a likely consequence of the overestimation highlighted by the MRA.

In the second group, we find that the dummies for the time effects, the data used, the estimation method, and the publication bias. In these cases, the probit estimates indicate a lower (higher) probability to get significant estimates, even if the effect sizes show an upward (downward) bias. Accordingly, studies offsetting the "bronze medal error" (*time effects*) or formally published are more likely to find significant results, even if their estimates tend to be smaller; while the positive sign associated with the *cross-section* and *pooled* dummies suggests that the downward bias indicated by the MRA is mostly due to non significant estimates. On the other hand, the estimation problems related to the OLS decrease the probability of getting significant results, even if these estimates tend to be inflated.

In the third group, we find the methodological dummies related to studies using dynamic techniques (*dynamic*) or dealing with the multilateral trade resistance term (*Anderson-van Wincoop*), and the selection bias and the presence of zero trade flows (*Heckman, Tobit*, *Poisson*). In these cases, even if there is not an evidence of a significant impact on the effect size when we use the largest sample, there seems to be a negative sign associated with the significant estimates. Accordingly, the use of more sophisticated estimation methods increases the probability of getting lower, though, still positive estimates of the RTAs impact on trade.

Table 8: Probit Analysis

Probit Estimation	Mean	β	Mean* β	$f(Z)$	$f(Z)$
Before 1970	0.06	-0.92***	-0.06	0.40	-0.37
1970s	0.07	-0.57***	-0.04	0.40	-0.23
1980s	0.20	-0.82***	-0.16	0.40	-0.33
After 1990	0.43	-0.25***	-0.11	0.40	-0.10
Log of average trade	0.20	0.29**	0.06	0.40	0.12
Anderson-van Wincoop	0.11	0.59***	0.07	0.40	0.23
Time effects	0.12	0.27**	0.03	0.40	0.11
Country effects	0.04	0.45**	0.02	0.40	0.18
Random effects	0.04	0.53***	0.02	0.40	0.21
Pooled	0.31	0.54***	0.17	0.40	0.22
Cross-section	0.44	0.42***	0.18	0.40	0.17
Ols	0.71	-0.44***	-0.31	0.40	-0.18
Heckman	0.02	-0.44*	-0.01	0.40	-0.18
Tobit	0.06	-0.83***	-0.05	0.40	-0.33
Poisson	0.05	-0.68***	-0.04	0.40	-0.27
Dynamic	0.03	-0.54***	-0.02	0.40	-0.21
Agreement	0.73	-0.50***	-0.37	0.40	-0.20
Published	0.40	0.20***	0.08	0.40	0.08
Interested	0.39	0.45***	0.18	0.40	0.18
Intercept	1.00	0.57***	0.57	0.40	0.23
Total			0.22		
No. of Obs	1048				
Wald $\chi^2_{(19)}$	340				
(p-value)	(0.000)				
Pseudo R^2	0.14				

**: significant at 5%; **: significant at 1%.*

5. Conclusion.

RTAs have been widely studied, and the interest on this type of trade liberalization is likely to increase in the next future due to the crisis of the multilateral liberalization process. One way to carry out a comparative study of the empirical results is to simply tabulate authors, country, methodology, and results. However, for policy analysis and a better understanding of the consequences of RTAs, it is useful to complement broad qualitative conclusions with a more precise quantitative research synthesis. This is the purpose of the present paper with respect to one core issue: the impact of these agreement on member countries' bilateral trade flows. In particular, we decided to overcome the main limitations of qualitative reviews, summarizing statistically the whole body of work through meta-regression analysis.

In this paper, we have investigated the result of previous studies analysing the effect of RTAs: the estimated effect varies widely from study to study and sometimes even within the same study. From the methodological point of view, this suggests the opportunity to retain all the available observations in most of our statistical analysis, though considering estimates from the same study as possibly correlated observations. Accordingly, by means of meta-analysis techniques, we statistically summarized 1827 estimates collected from a set of 85 studies.

All combined estimates imply a substantial increase in trade, but they vary a lot depending on the estimation method. In particular, the 'random-effects' estimate entails an increase of 65%. The more modest 'fixed-effects' estimate (10%) cannot be trusted because its basis is undermined by obvious heterogeneity in this literature. However, there is also strong statistical evidence of publication selection, favoring the reporting of significantly positive trade effects: such publication bias causes all simple combined estimates of trade effects, whether fixed- or random-effects, to be exaggerated.

Our analysis also provides a range of additional results helping to explain the wide variation in reported estimates. In this respect, meta-analysis statistical techniques are something more than mere weighted averages of all point estimates. Even if we do not dare to assign "weights" (or "medals") according to which of the studies we deem as good or bad, we do provide a quantitative assessment of the coonsequences due to the publication selection or possibly questionable methodological choices. For example, estimates obtained from cross and pooled data are more likely to find a positive and significant impact, though they report smaller values. The same example is possible for fixed and random effect estimators. On the other hand, studies reporting OLS estimates are less likely to get (statistically speaking) "good results" and provide results that may be upward biased due to misspecifications and omitted variables. Several studies lump different trade agreements together: this has a negative impact on the likelihood of finding significant results, and lead to an underestimation of the impact on trade. Conversely, published papers and studies mainly interested in studying the RTAs' impact are more likely to report significant results that tend to be overestimated.

After filtering out the publication selection and other biases, the meta-analysis confirms a robust, positive RTA effect, equivalent to an increase in trade exceeding 11%. The estimates tend to get larger in recent years, and this could be a consequence of the evolution from 'shallow' to 'deep' trade agreements. Looking at fixed effects for type of trade agreement, we find evidence of a differentiated impact on trade, the majority of coefficients are positive and strongly significant, although they are lower than results obtained by single MA for specific RTAs. Indeed, in many cases the MA estimate of the impact on trade for type of agreement largely exceeds the estimate for all the agreements combined.

The meta-analysis of the trade effects of RTAs provide a combined estimate more plausible than some extreme values reported in the literature. Moreover, our results shed some light on the role played by some research characteristics in explaining the variation in reported estimates. However, our findings should still be considered as provisional, since there remains excess unexplained variation in our meta-regression models.

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APPENDIX 1

Reciprocal Trade Agreements (in chronological order of date of entry into force)

Trade Agreements	Date
European Union (EU):	**1958**
Belgium-Luxembourg, France, Germany, Italy, Netherlands	
Denmark, Ireland, United Kingdom	1973
Greece	1981
Portugal, Spain	1986
Austria, Finland, Sweden	1995
Cyprus, Czech Rep., Estonia, Hungary, Latvia, Lithuania, Malta, Poland,	
Slovak Rep., Slovenia	2004
European Free Trade Association (EFTA):	**1960**
Switzerland, United Kingdom	Until 1973
Norway, Portugal	Until 1986
Sweden	Until 1995
Denmark	1973
Finland	1986-1995
Austria	1995
Latin American Free Trade Agreement/Latin American Integration	
Agreement, (LAFTA/ LAIA):	**1961-1979, 1993**
Argentina, Bolivia, Brazil, Chile, Ecuador, Mexico, Paraguay, Peru, Uruguay,	ineffective 1980-1990 reinitiated
Venezuela	1993
Central American Common Market (CACM):	**1961-1975, 1993**
El Salvador, Guatemala, Honduras, Nicaragua, Costa Rica	1965
Anglo–Irish Free Trade Area Agreement (AIFTAA)	**1965**
Caribbean Community (CARICOM):	**1968**
Jamaica, Trinidad and Tobago, Guyana	1995
Australia -New Zealand Closer Economic Relations (ANZCER)	**1983**
US-Israel	**1985**
US-Canada (CUFTA)	**1989**
Central Europe Free Trade Agreement (CEFTA):	**1993**
Hungary, Poland, Romania	1997
Bulgaria	1998
Baltic FTA(BFTA):	**1993**
Estonia, Latvia, Lithuania	
Commonwealth of Independent States (CIS):	**1993**
Russia, Kazakhstan, Ukraine,	
and Belarus	
NAFTA:	**1995**
Mercado Comun del Sur (Mercosur):	**1991**
Argentina, Brazil, Paraguay, Uruguay	formed in 1991 and FTA in 1995
Association of Southeast Nations ASEAN (AFTA):	**1998**
Indonesia, Philippines, Singapore, Thailand	
US-Chile	**2004**

APPENDIX 2

Econometrics results from the literature

Selected Articles	Trade Agreements*	Sample	Number of Estimates	
			Positive (significant**)	Negative (significant**)
1 *Aiello et al., 2006*	RTAs	Panel of data on trade of agricultural products granted by 8 major OECD countries to exports from LDCs over the period 1995-2003.	6 (3)	3 (0)
2 *Adam et al. 2003*	BFTA, CEFTA, EU	Cross-section data on Central and Eastern Europe total exports from 1992-2003	9 (9)	-
3 *Aitken, 1973*	EEC, EFTA	Cross-sectional trade flow model considering European trade relations over the period 1951-67	17 (8)	17 (0)
4 *Babetskaia-Kukharchuk, Maurel, 2004*	EU	Panel of data on trade of 14 EU countries over the period 1994-2001.	15 (9)	3 (0)
5 *Baier, Bergstrand, 2005*	RTAs	Panel of cross-section time-series data at 5 year intervals from 1960 to 2000 for 96 countries	17 (12)	8 (4)
6 *Bayoumi, Eichengreen, 1995*	EEC, EFTA	Panel of cross-section time-series data at 2 year intervals from 1956 to 1973	12 (4)	-
7 *Bergstrand, 1985*	EEC, EFTA	Cross-section using data for years 1965, 1966, 1975, 1976 for 15 OECD countries	8 (5)	-
8 *Bergstrand,1989*	EEC, EFTA	Cross-section using data for years 1965, 1966, 1975, 1976 for 15 OECD countries	70 (44)	2 (0)
9 *Blomqvist, 2004*	ASEAN	Cross-section using data for over period 1981-99 for developed countries and Asean members.	4 (0)	-
10 *Brada, Mendez, 1985*	RTAs	Cross-section using data for over period 1990-94 for all OECD countries	3 (1)	-
11 *Breuss, Egger, 1999*	EU	Cross-section using data for years 1970, 1973, 1976 for member countries of EEC, EFTA CACM, LAFTA and Adean Community.	3 (3)	-
12 *Broto et al., 2006*	RTAs, AFTA, ANZCER, CACM, CAN, CARICOM, EFTA, EU, MERCOSUR, NAFTA, US-CHILE, US-ISRAEL	Panel dataset includes bilateral trade flows for a total of 205 countries from 1948 to 2005	53 (43)	14 (10)
13 *Bun, Klaassen, 2002*	RTAs, EU	Panel data on bilateral exports between the 15 European Union countries and the G7 countries outside Europe (Canada, Japan and the U.S.) from 1965 through 2001	3 (3)	-
14 *Bun, Klaassen, 2006*	RTAs	Panel data on bilateral exports between the 15 European Union countries and the G7 countries outside Europe (Canada, Japan and the U.S.) from 1965 through 2001	4 (4)	-
15 *Carrère, 2006*	CACM, EU, LAIA	Panel data set including observations from over 130 countries from 1962 to 1996	25 (25)	-
16 *Cernat, 2001*	AFTA, CARICOM, EU, MERCOSUR, NAFTA	Cross-section dataset of more than 100 countries for three individual years: 1994, 1996, and 1998.	16 (12)	4 (0)
17 *Cheng, Tsai, 2005*	CUSFTA, EEC, EFTA, EU, LAFTA, MERCOSUR, NAFTA.	Pooled cross-section over the period 1981-97 for 44 exporting and 57 importing countries	28 (24)	7 (5)
18 *Cheng, Wall, 2004*	ANZCER, EU, MERCOSUR, NAFTA, US-ISRAEL	Panel data including 797 unidirectional country-pairs in each of four years: 1982, 1987, 1992, and 1997	20 (11)	5 (1)
19 *De Benedictis et al., 2005*	EU, EU-CEECs	Panel data on bilateral trade flows between eight CEECs and the EU-23 over period 1993-2003	2 (1)	-
20 *Eaton, Kortum, 1997*	EFTA, EU	Cross-section using 1990 data on trade in manufactures of 19 OECD countries	7 (1)	1 (0)
21 *Egger, 2005*	RTAs	Cross-section data on average 1990–97 bilateral exports of a sample of countries including OECD and non-OECD economies	3 (3)	-
22 *Elliot, Ikemoto, 2004*	ASEAN, EEC, EU, NAFTA	Panel on data of APEC, ASEAN, EU, NAFTA and other 16 countries over period 1982 to 1999	28 (22)	-
23 *Endoh, 2000*	ASEAN, EAEC	Cross-section analysis using a data set of 80 countries for every five-year term from 1960 to 1995.	50 (48)	2 (0)
24 *Faruquee, 2004*	RTAs	Panel data for 22 industrial countries, sample period from 1992-2002	12 (0)	2 (0)
25 *Fazio et al, 2005*	RTAs	Cross-section analysis using a data set of annual observations for 134 countries over 1980-2000	8 (1)	-
26 *Feenstra et al 2001*	RTAs	Five different cross-sections: 1970,1075, 1980, 1985, 1990. Sample of 110 countries considering differentiated and homogeneous goods.	34 (26)	-
27 *Fidrmuc, Fidrmuc, 2003*	BFTA, CEFTA, EC, EC+EFTA, EFTA,	Cross-section analysis using data on bilateral trade from 1990 to 1998 for OECD countries and Central and Eastern European countries.	130 (103)	5 (1)
28 *Frankel, Wei, 1997*	EEC, EFTA	Cross-sectional trade flow model considering 63 countries and data at 5 year intervals from 1960 to 1990	35 (14)	11 (1)
29 *Frankel, Rose, 2000*	RTAs	Panel data set including observations from over 180	4	-

			countries at 5-year intervals from 1970 through 1995	(4)	
30	*Frankel, Stein, Wei, 1995*	EEC, EFTA	Cross-sectional trade flow model considering 63 countries and data at 5 year intervals from 1965 to 1990	25 (10)	11 (0)
31	*Frankel, Stein, Wei, 1997*	EC	Cross-sectional trade flow model considering 63 countries and data at 5 year intervals from 1970 to 1990	2 (2)	
32	*Fratianni, Kang, 2006*	RTAs	Cross-section analysis using data on bilateral trade from 1970 to 1999, at five-years intervals, for 175 countries.	2 (2)	-
33	*Freund, Weinhold, 2004*	RTAs	Cross-section analysis using data on bilateral trade from 1995 to 1999 for 56 countries.	5 (1)	-
34	*Gaulier et al., 2004*	EFTA, EU	Panel of a large number of (group of) countries covering the whole world over the period 1967-2001,	12 (11)	-
35	*Ghosh, Yamarik, 2004*	CACM, CARICOM, EEA, EFTA, EU, LAIA	The data set consists of six annual observations for 186 developing and developed countries. The annual observations are for 1970, 1975, 1980, 1985, 1990, and 1995	5 (4)	1 (0)
36	*Glick, Rose, 2002*	RTAs	Pooled panel, data set of 186 countries from 1948 to 1997.	1 (1)	-
37	*Grünfeld, Moxnes, 2003*	RTAs	Cross-section analysis using data on service exports for 1999 and 2000 of 22 OECD countries.	3 (0)	1 (0)
38	*Hassan, 2001*	EEC, NAFTA	Cross-section analysis using annual data on bilateral trade flow of 27 countries in years 1996 and 1997.	8 (2)	-
39	*Jakab et al., 2001*	CEE, CEFTA, EC+EFTA, MERCOSUR, NAFTA	Cross-section of trade data from 1990 to 1997 for 53 developed and non-developed countries.	8 (8)	10 (10)
40	*Jayasinghe, Sarker, 2004*	NAFTA	Pooled cross-sectional time-series regression for trade of six selected agrifood products from 1985 to 2000 for NAFTA	24 (12)	6 (1)
41	*Katamaya, Melatos, 2006*	RTAs	Panel dataset constructed by Glick and Rose (2002), covering 217 countries from 1948 to 1997.	12 (10)	-
42	*Kenen, 2002*	RTAs	Rose's (2000) data set (113 countries for 1990)	6 (6)	-
43	*Kien, Hashimoto, 2005*	AFTA, EU, MERCOSUR,NAFTA	Panel data on exports flows of 39 countries for the period 1988-2002.	12 (11)	4 (3)
44	*Kimura , Lee, 2004*	RTAs	Cross-section data on bilateral services trade and goods trade between 10OECD countries and other countries (OECD members and non OECD members) for the years 1999 and 2000.	48 (34)	0 (0)
45	*Klein, 2002*	RTAs	Cross-section of annual observations on 165 countries (27 industrial countries and 138 non-industrial countries) from 1948 to 1997	14 (9)	-
46	*Klein, 2005*	RTAs	Cross-section of annual observations on 165 countries (27 industrial countries and 138 non-industrial countries) from 1948 to 1997	6 (6)	1 (0)
47	*Krueger, 1999*	EU, ANZCER	pooled time-series-cross-section regression using data from 1987 to 1997 for members of various PTAs.	2 (1)	-
48	*Lee et al., 2004*	RTAs	Panel data set of 175 countries from 1948 to 1999.	15 (15)	-
49	*Lee, Park, 2005*	RTAs, AFTA, ANZCER, CACM, CAN, CARICOM, EC/EU, EFTA, MERCOSUR, NAFTA, and US-Israel FTA.	Panel data set of 175 countries from 1948 to 1999.	18 (12)	4 (2)
50	*Lennon. 2006*	RTAs	Panel data on bilateral trade in services are drawn from the OECD Statistics on International Trade in Services from 1999 to 2002.	13 (7)	3 (2)
51	*Màrquez-Ramos et al., 2006*	CACM, CAN, CARICOM, EU, MERCOSUR, NAFTA	Data for 65 countries in 1980, 1985, 1990, 1995 and 1999	39 (37)	1 (0)
52	*Martìnez-Galàn et al, 2005*	EU	Cross-section analysis using data on trade of manufactured products between EU25 and Eastern European countries from 1999 to 2002	-	4 (0)
53	*Martìnez-Zarzoso, Horsewood, 2005*	CACM, CARICOM, EU, NAFTA	Sample of 47 countries from 1980 to 1999.	48 (41)	-
54	*Martìnez-Zarzoso, Nowak-Lehmann, 2003*	EU, MERCOSUR	Panel data of a sample of 20 countries, 15 EU countries and 5 Mercosur countries, from 1988 to 1996	28 (27)	-
55	*Mayer, Zignago, 2005*	RTAs, ADEAN, ASEAN, EU, MERCOSUR, NAFTA	Cross-section analysis data for 67 developing and developed countries over the period 1976-1999.	12 (12)	-
56	*Meliz, 2001*	RTAs	Frankel and Rose (2000) database	4 (4)	-
57	*Meliz, 2002*	RTAs	Frankel and Rose (2000) database	3 (3)	-
58	*Micco et at, 2003*	RTAs	Panel data set including information on bilateral trade for 22 developed countries from 1992 to 2002.	22 (9)	21 (2)
59	*Nitsch, 2002*	RTAs	Rose's (2000) data set.	15 (15)	-
60	*Paiva, 2005*	RTAs	Data set covers bilateral trade in agricultural goods for 152	5	-

			countries over the periods 1990–93 and 1999–2002.	(5)	
61	*Pakko, Wall, 2001*	RTAs	Rose's (2000) data set	6 (3)	-
62	*Papazoglou et al., 2006*	EU	This sample consists of 26 countries: 14 EU members (with Belgium and Luxembourg being treated as one country) and the 12 major trading partner countries, for 1992-2003	2 (2)	-
63	*Rauch, 1996*	EEC, EFTA	Cross-section, data on 63 countries for the years 1970, 1980, 1990	42 (6)	30 (9)
64	*Rauch, Trindade, 1999*	EEC,EFTA	Cross-section, data on 63 countries for the years 1970, 1980, 1990	29 (4)	19 (2)
65	*Rose, 2000*	RTAs	Panel data, bilateral observations for five during 1970-90 covering 186 countries	50 (50)	3 (1)
66	*Rose, 2004*	RTAs	Panel data, bilateral observations for five during 1950-2000 covering 175 countries.	6 (6)	-
67	*Rose, 2005a*	RTAs	Panel data, bilateral observations for five during 1950-2000 covering 175 countries.	17 (16)	-
68	*Rose, 2005b*	RTAs	Panel data, bilateral observations for five during 1950-2000 covering 150 countries.	-	4 (2)
69	*Rose, Engel, 2002*	RTAs	Cross-section analysis using a data set of annual observations for 210 countries between 1960 and 1996	4 (4)	-
70	*Rose, van Wincoop, 2001*	RTAs	Panel data on bilateral observations for five during 1970-95 covering 200 countries.	2 (2)	-
71	*Saiki, 2005*	RTAs	Panel of OPEC and OECD countries for the period 1980 and 1997.	4 (4)	
72	*Sanso et al., 1993*	EEC, EFTA	Cross-section of annual observations on trade in 16 OECD countries from 1964 to 1987	27 (11)	1 (0)
73	*Sapir, 2001*	EFTA	Cross-section, annually over the period 1960–1992 on the 240 bilateral trade flows	18 (0)	15 (4)
74	*Siliverstovs, Schumacher, 2006*	CUSTA, EFTA, EU	Panel data over the period 1988 to 1990 for 22 OECD countries	101 (50)	73 (20)
75	*Silva, Tenreyro, 2003*	RTAs	Cross-section analysis of 137 countries in 1990.	6 (6)	-
76	*Silva, Tenreyro, 2005*	RTAs	Cross-section analysis of 137 countries in 1990.	12 (10)	-
77	*Sissoko, 2004*	BFTA, CEFTA	Panel of 36 countries of the European zone with annual data during the period 1988 – 2000.	27 (19)	-
78	*Subramanian, Wei 2003*	RTAs	Panel data set of annual data over the period 1960–1992 on the 240 bilateral trade flows	30 (28)	-
79	*Subramanian, Wei 2005*	RTAs	Panel data set of annual data over the period 1960–1992 on the 240 bilateral trade flows	29 (28)	1 (0)
80	*Tang, 2005*	ANZCER, ASEAN, NAFTA	The data set covers the bilateral trade flows for 21 countries from 1989 to 2000.	10 (8)	-
81	*Tenreyro, 2001*	RTAs	Panel data set of annual observations for over 200 countries from 1978 to 1997.	4 (4)	-
82	*Thom, Walsh, 2002*	AIFTA, EEC	Panel and cross-section analysis for Anglo–Irish trade over the period 1950–1998	12 (7)	8 (4)
83	*Verdeja, 2005*	EFTA	Cross-section data covering 137 countries for the period 1973-2000.	11 (6)	4 (1)
84	*Walsh, 2006*	EU	Panel data covers imports between 27 OECD countries and up to fifty of their trading partners over a three year period (1999-2001).	13 (5)	3 (0)
85	*Yeyati, 2003*	RTAs	Rose's (2000) data set (186 countries for 1995)	10 (10)	-

** RTAs indicates estimates that do not specify the type of agreement.*

*** Statistically significant at the 5% level.*

APPENDIX 3

Papers included in the database

Descriptive Statistics

References	Nb. of Estimates	Ranges		Simple Mean	Standard Error
		Min	*Max*		
Aiello et al., 2006	9	-0.13	0.59	0.17	0.08
Adam et al. 2003	9	0.48	3.69	1.70	0.39
Aitken, 1973	34	-0.21	0.89	0.16	0.06
Babetskaia-Kukharchuk, Maurel, 2004	18	-0.51	3.37	0.98	0.30
Baier, Bergstrand, 2005	25	-3.97	2.51	0.12	0.24
Bayoumi, Eichengreen, 1995	12	0.01	0.21	0.07	0.02
Bergstrand, 1985	8	0.18	0.73	0.47	0.08
Bergstrand,1989	72	-0.11	1.93	0.73	0.06
Blomqvist, 2004	4	0.09	0.20	0.13	0.02
Brada, Mendez, 1985	3	3.77	4.83	4.43	0.33
Breuss, Egger, 1999	3	0.29	0.42	0.38	0.04
Broto et al, 2006	67	-0.51	2.59	0.65	0.10
Bun, Klaassen, 2002	3	0.02	0.08	0.05	0.02
Bun, Klaassen, 2006	4	0.06	0.85	0.41	0.16
Carrère, 2006	25	0.22	0.99	0.58	0.04
Cernat, 2001	20	-0.72	4.41	1.05	0.32
Cheng, Tsai, 2005	35	-0.35	4.35	0.74	0.17
Cheng, Wall, 2005	25	-0.16	3.98	0.59	0.19
De Benedictis et al., 2005	2	0.11	0.14	0.12	0.02
Eaton, Kortum, 1997	8	-0.12	0.54	0.23	0.08
Egger, 2005	3	0.52	1.29	0.78	0.25
Elliot, Ikemoto, 2004	28	0.10	2.35	0.55	0.11
Endoh, 2000	52	-0.07	1.93	0.85	0.07
Faruquee, 2004	14	-0.01	0.01	0.01	0.00
Fazio et al, 2005	8	0.10	0.27	0.17	0.02
Feenstra et al 2001	34	0.18	2.20	1.09	0.09
Fidrmuc, Fidrmuc, 2003	135	-0.15	3.96	0.68	0.08
Frankel, Wei, 1997	46	-0.41	1.15	0.21	0.05
Frankel, Rose, 2000	4	1.16	1.31	1.25	0.03
Frankel, Stein, Wei, 1995	36	-0.32	1.51	0.16	0.06
Frankel, Stein, Wei, 1997	2	0.24	0.31	0.28	0.04
Fratianni, Kang, 2006	2	1.04	1.37	1.21	0.17
Freund, Weinhold, 2004	5	0.02	0.28	0.16	0.04
Gaulier et al, 2004	12	0.16	2.17	1.01	0.17
Ghosh, Yamarik, 2004	6	-0.11	2.22	0.79	0.42
Glick, Rose, 2002	1	0.99	0.99	0.99	.
Grünfeld, Moxnes, 2003	4	-0.14	0.17	0.02	0.06
Hassan, 2001	8	0.22	4.71	2.45	0.63
Jakab et al., 2001	18	-2.03	0.69	-0.17	0.20
Jayasinghe, Sarker, 2004	30	-1.47	3.76	0.83	0.21
Katamaya, Melatos, 2006	12	0.15	0.85	0.44	0.07
Kenen, 2002	6	0.59	2.32	1.12	0.26
Kien, Hashimoto, 2005	16	-0.46	2.23	0.41	0.15
Kimura , Lee, 2004	48	0.19	0.65	0.36	0.02
Klein, 2002	14	0.07	2.35	0.85	0.16
Klein, 2005	7	-0.48	2.52	0.99	0.33
Krueger, 1999	2	0.07	0.50	0.29	0.22
Lee et al., 2004	15	0.42	0.92	0.63	0.04
Lee, Park, 2005	22	-0.35	1.97	0.65	0.13

Lennon, 2006	16	-0.20	0.41	0.07	0.03
Màrquez-Ramos et al., 2006	40	-0.21	5.23	2.03	0.26
Martìnez-Galàn et al, 2005	4	-0.06	0.00	-0.02	0.01
Martìnez-Zarzoso, Horsewood, 2005	48	0.01	2.63	0.77	0.11
Martìnez-Zarzoso, Nowak-Lehmann, 2003	28	0.04	0.65	0.24	0.03
Mayer, Zignago, 2005	12	0.72	2.30	1.78	0.16
Meliz, 2001	4	1.03	1.24	1.16	0.05
Meliz, 2002	3	1.00	1.02	1.01	0.01
Micco et at, 2003	43	-0.30	0.18	0.01	0.01
Nitsch, 2002	15	0.68	1.28	1.07	0.05
Paiva, 2005	5	1.01	1.15	1.10	0.02
Pakko, Wall, 2001	6	0.05	0.91	0.43	0.17
Papazoglou et al., 2006	2	0.29	0.33	0.31	0.02
Rauch, 1996	72	-1.18	1.11	0.03	0.06
Rauch, Trindade, 1999	48	-0.64	0.46	0.07	0.04
Rose, 2000	53	-0.97	1.54	0.79	0.06
Rose, 2004	6	0.94	1.50	1.19	0.07
Rose, 2005a	17	0.07	0.75	0.53	0.05
Rose, 2005b	4	-0.07	-0.02	-0.05	0.01
Rose, Engel, 2002	4	0.75	0.95	0.88	0.04
Rose, van Wincoop, 2001	2	0.46	1.09	0.78	0.32
Saiki, 2005	4	0.73	1.66	1.24	0.22
Sanso et al., 1993	28	-0.05	1.32	0.34	0.07
Sapir, 2001	33	-0.54	0.34	0.01	0.05
Siliverstovs and Schumacher, 2006	174	-1.89	1.77	0.08	0.04
Silva, Tenreyro, 2003	6	0.26	0.79	0.44	0.11
Silva, Tenreyro, 2005	12	0.14	1.29	0.48	0.10
Sissoko, 2004	27	0.21	2.64	1.57	0.15
Subramanian, Wei 2003	30	0.18	1.99	0.89	0.08
Subramanian, Wei 2005	30	-0.13	1.99	0.92	0.09
Tang, 2005	10	0.26	1.83	0.80	0.19
Tenreyro, 2001	4	0.29	0.70	0.53	0.09
Thom, Walsh, 2002	20	-0.10	0.74	0.11	0.06
Verdeja, 2005	15	-1.38	1.90	0.39	0.23
Walsh, 2006	16	-9.01	15.41	4.35	1.69
Yeyati, 2003	10	0.47	1.00	0.58	0.05

APPENDIX 4

Within-Study Meta-Analysis of RTAs effect on trade

Study		Coefficient	$H_0 : \gamma = 0$ (p-value)	test Q (p-value)	H^2	I^2	Heterogeneity
Aiello et al., 2006	Fixed	0.17	0.00	0.00	7.77	87%	High
	Random	0.17	0.05				
Adam et al. 2003	Fixed	0.91	0.00	0.00	45.18	98%	High
	Random	1.61	0.00				
Aitken, 1973	Fixed	0.20	0.00	0.00	1.77	44%	Moderate
	Random	0.18	0.00				
Babetskaia-Kukharchuk, Maurel, 2004	Fixed	0.51	0.00	0.00	25.55	96%	High
	Random	0.94	0.00				
Baier, Bergstrand, 2005	Fixed	0.14	0.00	0.00	25.73	96%	High
	Random	0.16	0.03				
Bayoumi, Eichengreen, 1995	Fixed	0.09	0.00	0.02	2.08	52%	Moderate
	Random	0.08	0.00				
Bergstrand, 1985	Fixed	0.45	0.00	0.01	2.71	63%	Moderate
	Random	0.46	0.00				
Bergstrand,1989	Fixed	0.80	0.00	0.00	3.92	74%	High
	Random	0.76	0.00				
Blomqvist, 2004	Fixed	0.12	0.01	0.88	0.23	0%	Low
	Random	0.12	0.01				
Brada, Mendez, 1985	Fixed	4.34	0.00	0.93	0.07	0%	Low
	Random	4.34	0.00				
Breuss, Egger, 1999	Fixed	0.38	0.00	0.43	0.85	0%	Low
	Random	0.38	0.00				
Broto et al., 2006	Fixed	0.20	0.00	0.00	54.26	98%	High
	Random	0.64	0.00				
Bun, Klaassen, 2002	Fixed	0.05	0.00	0.00	9.00	89%	High
	Random	0.05	0.00				
Bun, Klaassen, 2006	Fixed	0.20	0.00	0.00	263.89	100%	High
	Random	0.41	0.01				
Carrère, 2006	Fixed	0.51	0.00	0.00	13.04	92%	High
	Random	0.57	0.00				
Cernat, 2001	Fixed	0.47	0.00	0.00	17.18	94%	High
	Random	1.04	0.00				
Cheng, Tsai, 2005	Fixed	0.18	0.00	0.00	168.83	99%	High
	Random	0.70	0.00				
Cheng, Wall, 2004	Fixed	0.30	0.00	0.00	54.08	98%	High
	Random	0.60	0.00				
De Benedictis et al., 2005	Fixed	0.12	0.01	0.74	0.11	0%	Low
	Random	0.12	0.01				
Eaton, Kortum, 1997	Fixed	0.20	0.01	0.43	1.00	0%	Low
	Random	0.20	0.01				
Egger, 2005	Fixed	0.87	0.00	0.00	6.18	84%	High
	Random	0.79	0.00				
Elliot, Ikemoto, 2004	Fixed	0.33	0.00	0.00	13.52	93%	High
	Random	0.49	0.00				
Endoh, 2000	Fixed	0.73	0.00	0.00	11.99	92%	High
	Random	0.83	0.00				
Faruquee, 2004	Fixed	0.00	0.89	1.00	0.05	0%	Low
	Random	0.00	0.89				
Fazio et al, 2005	Fixed	0.17	0.00	0.98	0.21	0%	Low
	Random	0.17	0.00				
Feenstra et al 2001	Fixed	1.23	0.00	0.00	12.44	92%	High
	Random	1.14	0.00				
Fidrmuc, Fidrmuc, 2003	Fixed	0.05	0.00	0.00	34.58	97%	High
	Random	0.49	0.00				
Frankel, Wei, 1997	Fixed	0.25	0.00	0.00	2.12	53%	Moderate
	Random	0.23	0.00				
Frankel, Rose, 2000	Fixed	1.25	0.00	0.79	0.35	0%	Low
	Random	1.25	0.00				
Frankel, Stein, Wei, 1995	Fixed	0.45	0.00	0.00	3.43	71%	High
	Random	0.23	0.00				
Frankel, Stein, Wei, 1997	Fixed	0.29	0.00	0.52	0.42	0%	Low
	Random	0.29	0.00				
Fratianni, Kang, 2006	Fixed	1.21	0.00	0.00	9.55	90%	High
	Random	1.21	0.00				
Freund, Weinhold, 2004	Fixed	0.11	0.01	0.30	1.22	18%	Low
	Random	0.12	0.01				
Gaulier et al., 2004	Fixed	1.20	0.00	0.00	81.65	99%	High
	Random	1.02	0.00				

Ghosh, Yamarik, 2004	Fixed Random	0.38 0.74	0.00 0.03	0.00	47.34	98%	High
Glick, Rose, 2002	Fixed Random	0.99 0.99	0.00 0.00	-	-	-	-
Grünfeld, Moxnes, 2003	Fixed Random	0.03 0.03	0.70 0.70	0.56	0.70	0%	Low
Hassan, 2001	Fixed Random	2.61 2.61	0.00 0.00	0.44	0.99	0%	Low
Jakab et al., 2001	Fixed Random	-0.44 -0.18	0.00 0.15	0.00	484.32	100%	High
Jayasinghe, Sarker, 2004	Fixed Random	0.69 0.81	0.00 0.00	0.00	4.71	79%	High
Katamaya, Melatos, 2006	Fixed Random	0.54 0.45	0.00 0.00	0.00	16.15	94%	High
Kenen, 2002	Fixed Random	0.76 0.86	0.00 0.00	0.07	2.05	51%	Moderate
Kien, Hashimoto, 2005	Fixed Random	0.19 0.41	0.00 0.01	0.00	92.81	99%	High
Kimura , Lee, 2004	Fixed Random	0.37 0.37	0.00 0.00	0.44	1.02	2%	Low
Klein, 2002	Fixed Random	0.87 0.87	0.00 0.00	0.01	2.15	54%	Moderate
Klein, 2005	Fixed Random	1.06 1.04	0.00 0.00	0.00	6.12	84%	High
Krueger, 1999	Fixed Random	0.10 0.21	0.13 0.29	0.10	2.65	62%	Moderate
Lee et al., 2004	Fixed Random	0.56 0.62	0.00 0.00	0.00	14.64	93%	High
Lee, Park, 2005	Fixed Random	0.51 0.62	0.00 0.00	0.00	20.63	95%	High
Lennon. 2006	Fixed Random	0.09 0.08	0.00 0.01	0.00	6.16	84%	High
Màrquez-Ramos et al., 2006	Fixed Random	0.89 1.82	0.00 0.00	0.00	29.99	97%	High
Martìnez-Galàn et al, 2005	Fixed Random	-0.02 -0.02	0.81 0.81	1.00	0.02	0%	Low
Martìnez-Zarzoso, Horsewood, 2005	Fixed Random	0.16 0.61	0.00 0.00	0.00	26.42	96%	High
Martìnez-Zarzoso, Nowak-Lehmann, 2003	Fixed Random	0.06 0.18	0.00 0.00	0.00	22.63	96%	High
Mayer, Zignago, 2005	Fixed Random	1.60 1.76	0.00 0.00	0.00	35.81	97%	High
Meliz, 2001	Fixed Random	1.16 1.16	0.00 0.00	0.45	0.88	0%	Low
Meliz, 2002	Fixed Random	1.01 1.01	0.00 0.00	0.99	0.01	0%	Low
Micco et at, 2003	Fixed Random	0.02 0.02	0.00 0.04	0.00	2.57	61%	Moderate
Nitsch, 2002	Fixed Random	1.06 1.07	0.00 0.00	0.08	1.56	36%	Low
Paiva, 2005	Fixed Random	1.10 1.10	0.00 0.00	0.85	0.34	0%	Low
Pakko, Wall, 2001	Fixed Random	0.53 0.43	0.00 0.01	0.00	17.29	94%	High
Papazoglou et al., 2006	Fixed Random	0.30 0.30	0.00 0.00	0.84	0.04	0%	Low
Rauch, 1996	Fixed Random	0.03 0.04	0.35 0.47	0.00	3.15	68%	Moderate
Rauch, Trindade, 1999	Fixed Random	0.10 0.10	0.00 0.01	0.01	1.54	35%	Low
Rose, 2000	Fixed Random	0.10 0.83	0.00 0.00	0.00	85.65	99%	High
Rose, 2004	Fixed Random	1.18 1.18	0.00 0.00	0.13	1.69	41%	Moderate
Rose, 2005a	Fixed Random	0.60 0.54	0.00 0.00	0.00	9.53	90%	High
Rose, 2005b	Fixed Random	-0.04 -0.04	0.00 0.00	0.47	0.85	0%	Low
Rose, Engel, 2002	Fixed Random	0.88 0.88	0.00 0.00	0.79	0.35	0%	Low
Rose, van Wincoop, 2001	Fixed Random	0.83 0.78	0.00 0.01	0.00	16.27	94%	High
Saiki, 2005	Fixed Random	0.97 1.15	0.00 0.00	0.01	4.23	76%	High
Sanso et al., 1993	Fixed	0.32	0.00	0.00	7.17	86%	High

	Random	0.35	0.00				
Sapir, 2001	Fixed Random	-0.02 -0.01	0.68 0.90	0.02	1.55	36%	Low
Siliverstovs, Schumacher, 2006	Fixed Random	0.25 0.13	0.00 0.00	0.00	5.35	81%	High
Silva, Tenreyro, 2003	Fixed Random	0.35 0.42	0.00 0.00	0.00	5.34	81%	High
Silva, Tenreyro, 2005	Fixed Random	0.41 0.46	0.00 0.00	0.00	8.99	89%	High
Sissoko, 2004	Fixed Random	1.48 1.46	0.00 0.00	0.00	1.99	50%	Moderate
Subramanian, Wei 2003	Fixed Random	0.86 0.88	0.00 0.00	0.00	7.42	87%	High
Subramanian, Wei 2005	Fixed Random	0.89 0.92	0.00 0.00	0.00	9.83	90%	High
Tang, 2005	Fixed Random	0.75 0.80	0.00 0.00	0.00	17.95	94%	High
Tenreyro, 2001	Fixed Random	0.53 0.53	0.00 0.00	0.00	5.24	81%	High
Thom, Walsh, 2002	Fixed Random	0.04 0.10	0.00 0.00	0.00	18.40	95%	High
Verdeja, 2005	Fixed Random	0.37 0.45	0.00 0.02	0.00	5.72	83%	High
Walsh, 2006	Fixed Random	0.31 0.47	0.00 0.01	0.01	2.16	54%	Moderate
Yeyati, 2003	Fixed Random	0.58 0.58	0.00 0.00	0.05	1.91	48%	Moderate