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The WTO Trade Effect

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Abstract

This paper reexamines the GATT/WTO membership effect on bilateral trade flows, using nonparametric methods including pair-matching, permutation tests, and a Rosenbaum (2002) sensitivity analysis. Together, these methods provide an estimation framework that is robust to misspecification biases, allows general forms of heterogeneous treatment effects, and addresses potential hidden selection biases. This is in contrast to most conventional parametric studies on this issue. Our results suggest large GATT/WTO trade-promoting effects, robust to various restricted matching criteria, alternative indicators for GATT/WTO involvement, different matching methodologies, non-random incidence of positive trade flows, and inclusion of multilateral resistance terms.

JEL Classification: F13; F14; C14; C21; C23

KEY WORDS: Trade flow; Treatment effect; Matching; Permutation test; Signed-rank test; Sensitivity analysis.

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1. INTRODUCTION

The analysis of the GATT/WTO effect on bilateral trade flows in the empirical trade literature has largely relied on parametric estimation of gravity-type models, where the volume of trade between two countries is hypothesized to vary proportionally with the product of their economic sizes and the factor of proportionality to depend on trade resistance measures (Anderson, 1979; Bergstrand, 1985; Deardorff, 1998; Anderson and van Wincoop, 2003). A pioneering study by Rose (2004) suggests that GATT/WTO-membership dummies fail to reveal any statistically significant and robust influence on the volume of bilateral trade, while more recent studies by Tomz et al. (2007) and Subramanian and Wei (2007) suggest more positive findings when looking at alternative indicators of GATT/WTO involvement or at certain subsets of the sample.

In this paper, we use nonparametric methods to re-evaluate the GATT/WTO membership effect on bilateral trade flows. First, we apply pair-matching methods to obtain point effect estimates. By using the matching method, we avoid potential parametric misspecifications and allow for heterogeneous treatment (i.e. membership) effects, which appear to be an important element of the current application. The matching framework allows the treatment effects to vary with observed covariates, and thus allows more general forms of heterogeneity than Subramanian and Wei (2007). Second, given a panel of bilateral trade data, which likely have a complicated data structure with serial and spatial dependence to render the derivation of asymptotic tests for matching estimators difficult, this paper applies permutation tests which circumvent the above problem. Permutation tests are nonparametric and exact inferences; they are also straightforward to implement in the matching framework. We generalize the test to explicitly allow for heterogeneous treatment effects in constructing the confidence intervals for the mean effect. Third, the paper conducts a nonparametric sensitivity analysis following Rosenbaum (2002) to formally address potential biases due to unobserved self-selection into treatment. We put together these methods in a coherent manner such that they can be easily applied to other treatment effect problems of similar nature.

Applying the nonparametric methods to the Rose (2004) data set, this paper reaches a conclusion that is in stark contrast with Rose (2004): membership in the GATT/WTO has large and significant trade-promoting effects. We explore robustness of this result to various possible critiques.

First, both parametric gravity and nonparametric matching estimators rely on the assumption of 'selection on observables' and assume away non-random selection into treatment based on unobservables. This assumption may fail if there are important omitted variables. The Rosenbaum (2002) sensitivity analysis partly addresses this problem. Alternatively, we also conduct restricted matching, where we further limit the match to observations from the same dyad (where a dyad stand for two trading countries), the same year/period, or the same relative development stage. This eliminates potential biases arising from unobservable heterogeneity across dyads, years, GATT/WTO periods, or countries of different development stages.

Second, Tomz et al. (2007) emphasize the importance of *de facto* participation in the GATT/WTO

by colonies, newly independent nations and provisional members, and find strong GATT/WTO effects on trade when these types of nonmember participation is taken into account. We conduct the same nonparametric analysis using the data set of Tomz et al. (2007) and find even stronger results than those based on the Rose (2004) data set.

Third, we verify the robustness of pair-matching by conducting 'kernel-weighting matching' which allows multiple matches for a subject while assigning greater weights to closer matches. The kernel-weighting matching effect estimates are very similar to pair-matching estimates.

Fourth, by using the data set of Rose (2004) or Tomz et al. (2007), we have based our analysis on observations with positive trade flows. Studies by Helpman et al. (2008) and Felbermayr and Kohler (2007) suggest that the incidence of positive trade flows may not be random. To address possible biases due to non-random incidence of active trading relationships, we apply our nonparametric procedures to a subset of the Rose (2004) data set where a dyad have reported bilateral trade flows before they ever join the GATT/WTO. For these observations, the membership effect on prompting new trading relationships is not relevant and the effect estimates correspond to only the membership effect on trade volumes. We find overall stronger effect estimates based on this refined analysis.

Fifth, relative trade resistance rather than absolute trade resistance is argued by some gravity theories to be more appropriate in explaining bilateral trade flows, cf. Anderson and van Wincoop (2003), and multilateral resistance terms may have to be controlled for. We follow recent studies by Baier and Bergstrand (2009a,b) to approximate the endogenous multilateral resistance terms by observable exogenous trade resistance covariates and to control for time-varying multilateral resistance terms in the matching framework. The strong effects of GATT/WTO still remain.

Finally, we explore an alternative treatment effect concept, difference-in-difference, that is based on weaker identification assumptions and thus is more robust to potential biases due to selection on unobservables. This method compares the difference over time in trade volumes of a treated dyad to that of a comparable untreated dyad. The matching estimates indicate that the GATT/WTO membership effects are negligible in early phases of the treatment but become statistically and economically significant five or six years into the treatment. To complete the analysis, we conduct "placebo" exercises and verify that the time trends of trade flows of matched dyads are comparable in advance of membership, dismissing concerns that the difference-in-difference estimates may be picking up systematic differences in time trends between the treatment and control group due to unobservables not controlled for.

The discrepancy between the findings of the current nonparametric approach and the conventional parametric approach suggests that conventional gravity models may be misspecified. We explore generalizing the gravity model's specifications to reduce the discrepancy. Our limited search suggests that the assumption of homogeneous membership effects could be a major source of misspecification. By allowing the membership dummies to interact with observed covariates (and hence allowing the membership effects to vary with dyad-specific characteristics), we find the

parametric effect estimates to become significant and positive. However, more research into the nature of heterogeneous membership effects seems desirable and we leave this for future research.

The rest of the paper is organized as follows. Section 2 introduces the nonparametric procedures. Section 3 explains the data used. Sections 4 and 5 present our benchmark estimation results and robustness checks. Section 6 explores potential misspecifications of the gravity models. Section 7 concludes.

2. METHODOLOGY

2.1 Mean Effects and Matching

Recall that a 'dyad' indicate two trading countries. An observation unit is a dyad i in a year t; a matched 'pair' are two observation units matched on covariates. Let d_{it} denote the observed treatment status of a dyad i in year t, where $d_{it} = 1$ if the subject it is treated and 0 if untreated. The treatment dummy d_{it} takes on different meanings as the treatment under investigation changes. For example, a dyad-year subject is 'both-in' treated if both countries of the dyad in the year are GATT/WTO members and untreated if both are nonmembers. Define y^1_{it} (y^0_{it}) as the potential *treated* (*untreated*) response; in our application, this corresponds to the potential treated (untreated) bilateral trade volume for the dyad-year subject it. Let $y_{it} \equiv d_{it}y^1_{it} + (1-d_{it})y^0_{it}$ denote the observed response. Finally, let x_{it} denote the observed covariates for the dyad-year subject it that could potentially affect the treatment and response. Label the group of treated observations 'the treatment group' and the group of untreated observations 'the control group'. In the following, we will often omit the subscript it to simplify presentations.

In observational data, treatment d is self-selected. Matching on x helps removing the 'overt selection bias' caused by observed differences across the treatment and control group. By conditioning on x, one can identify the conditional mean effect $E(y^1 - y^0|x)$ with the conditional group mean difference:

$$E(y|d=1,x) - E(y|d=0,x) = E(y^1|d=1,x) - E(y^0|d=0,x) = E(y^1 - y^0|x) \text{ if } (y^0, y^1) \amalg d|x,$$

where $(y^0, y^1) \amalg d|x$ states that both the potential treated and untreated response (y^0, y^1) are independent of d given x. This condition is the identifying 'selection on observables' assumption—that is, the only source of selection bias is via the observed covariates; the selection into treatment is random once x is controlled for. The same assumption is required for parametric regression approaches.

A weaker identifying assumption $y^0 \amalg d|x$ is sufficient if one is only interested in the 'effect on the treated', as under the assumption,

$$\begin{aligned} E(y|d=1,x) - E(y|d=0,x) &= E(y^1|d=1,x) - E(y^0|d=0,x) \\ &= E(y^1|d=1,x) - E(y^0|d=1,x) = E(y^1 - y^0|d=1,x). \end{aligned}$$

Alternatively, the assumption $y^1 \amalg d|x$ is sufficient to identify the 'effect on the untreated' $E(y^1 - y^0|d = 0, x)$. Once the x-conditional effect is found, x can be integrated out to yield a marginal effect. For example, for the effect on the treated, the distribution $F(x|d = 1)$ of $x|d = 1$ is typically used to render

$$E(y^1 - y^0|d = 1) = \int E(y^1 - y^0|d = 1, x)\mathrm{d}F(x|d = 1).$$

This framework of first finding the x-conditional effect (on all, on the treated, or on the untreated) allows for possibly heterogeneous treatment effects across dyad-year subjects that differ in x. The unconditional mean effect then reflects the average of the heterogeneous x-conditional treatment effects weighted by the frequency of x. It is this average effect (on all, on the treated, or on the untreated) that we estimate. This departs from the parametric gravity regression approach, where a homogeneous treatment effect regardless of x is typically assumed.

Matching for the effect on the treated can be carried out as follows. First, a treated subject, say subject it, is selected. Second, control subjects are selected who are the closest to the treated subject it in terms of x, based on the simple scale-normalized distance measure, $(x_{it} - x_{i't'})\Sigma_x^{-1}(x_{it} - x_{i't'})'$, where Σ_x is a diagonal matrix with the sample variances of the covariates in the pooled sample on the diagonal and $i't'$ refers to a control subject. As x in our data includes continuous variables (cf. Section 3), the likelihood of multiple-matching (multiple control subjects being the closest match) is negligible; thus, we restrict our attention to pair-matching (a unique control subject being the closest match). Third, given pair-matching, suppose there are M pairs (where M is the number of treated subjects that successfully find a match), and y_{m1} and y_{m2} are the trade volumes of the two subjects in pair m ordered such that $y_{m1} > y_{m2}$ without loss of generality. Then, defining $s_m = 1$ if the first subject in pair m is treated and -1 otherwise, the effect on the treated can be estimated with

$$D \equiv \frac{1}{M}\sum_{m=1}^{M} s_m(y_{m1} - y_{m2}) \to^p E(y^1 - y^0|d = 1) \quad \text{under } y^0 \amalg d|x, \tag{1}$$

which is the average of the pair-wise differences. For a treated subject, if there is no good matching control, the subject may be passed over; i.e., a 'caliper' c may be set such that a treated subject it with $\min_{i't' \in C} \|x_{it} - x_{i't'}\| > c$ is discarded, where '$i't' \in C$' indicates subjects in the control group. The above matching scheme can be reversed to result in an estimator for the effect on the untreated: a control subject is selected first and then a matching subject from the treatment group later. For the effect on all, D simply includes all (treated and control) subjects who can find a good match.

In addition to the possibility of allowing for heterogeneous treatment effects, another advantage of taking the nonparametric matching approach is to avoid the misspecification bias that may arise in the parametric approach due to misspecifications of the regression functional form. Pair-matching, which matches subjects who differ in their treatment (e.g. membership) status but are otherwise similar in their covariates, allows arbitrary functional forms of the covariates.

Matching is widely used in labor and health economics. See, e.g., Heckman et al. (1997) and

Imbens (2004), and applications in Heckman et al. (1998), Lechner (2000), and Lu et al. (2001). Matching methods have also started to appear in international economics studies such as Persson (2001) of the currency union effect and Baier and Bergstrand (2009b) of the free trade agreement effect. For more discussions on treatment effect and matching in general, see Rosenbaum (2002) and Lee (2005).

2.2 Permutation Test for Matched Pairs

Although matching estimators are popular in practice, their asymptotic properties are not fully understood.[1] In practice, a standard t-statistic or a bootstrap procedure is often used to derive the p-value or the confidence interval (CI). The standard t-statistic is straightforward but theoretical justifications in most cases are not available; bootstrap is computationally demanding and is argued by Abadie and Imbens (2006) to be invalid. In this paper, we propose using permutation tests.

Permutation tests invoke the concept of *exchangeability* that under the null hypothesis H_0 of no effect, potential treated and untreated responses are exchangeable without affecting their joint distribution: $F(y_{it}^0, y_{it}^1|x) = F(y_{it}^1, y_{it}^0|x)$. This implies that under the null, the two potential responses have the same marginal distribution and hence the same mean given x. Thus, we can test the equal mean (i.e. zero mean effect) implication of the null.

It is straightforward to carry out the permutation test described above for matched pairs and test for a zero mean effect under the null. Under the null hypothesis of exchangeability, the two subjects in each matched pair are exchangeable in the labeling of their treatment status (treated or untreated). In each permutation of 'pseudo' treatment assignment, one can calculate the 'pseudo' effect estimate. By obtaining all possible 2^M permutations of the treatment labels in all M pairs, one can calculate the exact p-value of the observed mean effect estimate D by placing it in the "empirical" distribution of the pseudo effect estimates.

When M is large (as in the current application), such that the number of permutations is huge, one can approximate the exact p-value by simulating only a subset (say, 1000) of permutation possibilities from the complete permutation space and comparing the observed effect estimate D against the simulated sample of pseudo effect estimates. Alternatively, one can apply normal approximation. Note that in a permutation, the obtained pseudo effect estimator can be written as $D' \equiv \frac{1}{M}\sum_{m=1}^{M} w_m s_m (y_{m1} - y_{m2})$, where w_m, $m = 1, ..., M$, is a iid random variable such that $P(w_m = 1) = P(w_m = -1) = 0.5$. That is, the treatment labels of the two responses in pair m are exchanged if $w_m = -1$, and no exchange otherwise. We show in the appendix that conditional on the observed data, the exact p-value of D can be approximated by

$$P(D' \geq D) \simeq P\left\{ N(0,1) \geq \frac{D}{\{\sum_{m=1}^{M}(y_{m1} - y_{m2})^2/M^2\}^{1/2}} \right\}, \tag{2}$$

which turns out to use the same t-statistic as the conventional two-sample test. Thus, this display

[1] See, however, exceptions such as Abadie and Imbens (2006) for the case of iid data.

incidentally provides a theoretical justification for the common practice of using the t-statistic to evaluate the significance of matching estimators, although we have derived (2) from an exact inferential approach (i.e. permutation with respect to the treatment labels but conditional on the observed data) and not based on asymptotic distribution theories (i.e. sampling with respect to the data).

In addition to testing the null hypothesis of a zero mean effect, one may also be interested in an interval estimate of the mean effect. We show in the appendix how to obtain the CI for the mean effect by "inverting" the above test (see, e.g., Lehmann and Romano, 2005). It is worth noting that in deriving the CI, we generalize the inverting procedure to explicitly allow for heterogeneous treatment effects.

As indicated above, permutation inference methods have several advantages: (i) they are non-parametric as they do not require distributional assumptions on the response, other than the exchangeability condition, (ii) they are exact inferences despite making no parametric distributional assumptions in small samples, and they are often equivalent to conventional asymptotic inference methods in large samples when normal approximation is used. On the other hand, as permutation tests invoke a stronger concept of no effect (on the distribution), this rules out testing for null hypotheses of no effect that still allow some changes in the distribution. In small samples where normal approximation does not apply, permutation tests may also be computer-intensive. Both disadvantages, however, are not important in the current application.

Permutation tests, in stead of asymptotic tests, are especially convenient in the current application with a panel of bilateral trade data, which possibly have a complicated data structure with serial and spatial dependence, rendering the derivation of asymptotic properties for the matching estimator difficult if not impossible. By relying on exchangeability as the null hypothesis of no effect, the permutation test can accommodate potentially a wide range of data structures. For example, suppose that the joint distribution $F(y_{it}^0, y_{it}^1|x)$ is normal. In this scenario, the exchangeability condition requires only that the treated and untreated responses have the same variance conditional on x. This allows for heteroskedasticity (i.e. variances of responses to vary with x) or correlation across time or observation units.

Permutation tests have a long history in statistics since Fisher (1935) and are widely used in statistics and medicine. Recently, Imbens and Rosenbaum (2005) applied permutation inference to well-known "weak instrument" data in economics to find that only permutation methods provided reliable inference. Ho and Imai (2006) also applied permutation inference to a political science data set. As can be seen in these examples, the application of permutation methods is fairly new in social sciences. See Hollander and Wolfe (1999), Pesarin (2001), Ernst (2004), and Lehmann and Romano (2005) among others, for more on permutation (or randomization) tests in general.

2.3 Signed-Rank Test for Matched Pairs

Instead of the difference in response $s_m(y_{m1} - y_{m2})$, we can apply the permutation inference to the "signed rank" of the difference in response. The advantage is that rank-based tests are more

robust to outliers. In addition, the ensuing Rosenbaum (2002) sensitivity analysis can be applied to the signed-rank test easily. The disadvantage on the other hand is that such rank-based tests are geared more to testing for no effect rather than to estimating the effect itself, which results in a roundabout way of getting the point estimate and CI (as shown in the appendix). Since these estimates can only be derived under the assumption of homogeneous treatment effects, in contrast with those in Sections 2.1 and 2.2, they are of less interest to the current application. However, it is important to note that the signed-rank test itself and the corresponding sensitivity analysis are valid against an alternative of either homogeneous or heterogeneous treatment effects.

Applying the Wilcoxon (1945) signed-rank test to the current context, rank $|y_{m1} - y_{m2}|$, $m = 1, ..., M$, and denote the resulting ranks as $r_1, ..., r_M$, where a larger rank r_m corresponds to a larger absolute difference in response. The signed-rank statistic is then the sum of the ranks of the pairs where the treated subject has the higher response:

$$R \equiv \sum_{m=1}^{M} r_m 1[s_m = 1].$$

The p-value of the R-statistic can be obtained by the pseudo-sample simulation procedure or the normal approximation method as discussed in Section 2.2. In particular, we show in the appendix that when M is large, the normally approximated p-value for R under the null hypothesis of exchangeability is

$$P(R' \geq R) \simeq P\left\{N(0,1) \geq \frac{R - E(R')}{V(R')^{1/2}}\right\}, \tag{3}$$

where R' is the permuted version of R, $E(R') = M(M+1)/4$, and $V(R') = M(M+1)(2M+1)/24$.

2.4 Sensitivity Analysis with Signed-Rank Test

The key identifying assumption for the matching estimator is the 'selection on observables' condition as noted in Section 2.1. The same condition is also required for parametric regression approaches. This condition may fail if there are omitted third variables or unobservables that affect both the treatment d (the decision to join the GATT/WTO) and the response y (the trade flows). In a parametric framework, one may deal with this problem of 'selection on unobservables' using techniques such as Heckman's (1979). In a nonparametric framework as ours, the Rosenbaum (2002) sensitivity analysis provides a convenient way to account for selection on unobservables.

The analysis is structured as follows. Suppose that the treatment d is affected by an unobserved confounder ε. Then, two subjects in a matched pair with the same x but possibly different ε may have different probabilities of taking the treatment. Let the odds ratio of taking the treatment across all pairs be bounded between $1/\Gamma$ and Γ for some constant $\Gamma \geq 1$. For instance, if the first subject's probability of taking the treatment is 0.6 and the second subject's 0.5, the odds ratio is $(0.6/0.4)/(0.5/0.5) = 1.5$.

Given the bounds on the odds ratio, Rosenbaum (2002) shows that one could derive the corresponding bounds on the significance level of many rank-sum statistics under the null hypothesis of

no effect. This places bounds on the significance level that would have been appropriate had ε been observed. The sensitivity analysis for a significance level starts with the scenario of no hidden bias ($\Gamma = 1$). The sensitivity parameter Γ is then increased from 1 to see how the initial conclusion is affected. If it takes a large value of Γ (i.e., a large deviation from 1 in the odds ratio) to eliminate an original finding of a significant effect or to overturn an original finding of no effect, the initial conclusion is deemed robust to unobserved confounders; otherwise, the initial finding is sensitive.

We show in the appendix how to apply the Rosenbaum (2002) sensitivity analysis to the signed-rank statistic and derive the bounds on the significance level (the p-value) of the observed statistic R under the null of no effect. In particular, for a given degree $\Gamma \geq 1$ of departure from the state of no hidden bias, define $p^+ \equiv \frac{\Gamma}{1+\Gamma} \geq 0.5$ and $p^- \equiv \frac{1}{1+\Gamma} \leq 0.5$. The p-value of the observed statistic R is bounded as follows:

$$P(R^+ \geq R) \geq P(R' \geq R) \geq P(R^- \geq R), \tag{4}$$

where $R^+ \equiv \sum_{m=1}^{M} r_m u_m$ with $P(u_m = 1) = p^+$ and $P(u_m = 0) = 1 - p^+$, and likewise for R^-. Note that the means and variances of R^+ and R^- include $E(R')$ and $V(R')$ as a special case when $p^+ = p^- = 1/2$ under no hidden bias.

Specifically, suppose that the H_0-rejection interval is in the upper tail, and the p-value assuming no hidden bias is $P(R' \geq R) = 0.001$, leading to the rejection of H_0 at level $\alpha > 0.001$. By allowing an unobserved confounder to cause the odds ratio to deviate from 1 and up to $(1/\Gamma, \Gamma)$, the correct tail probability is unknown but is bounded above by $P(R^+ \geq R) \simeq P\{N(0,1) \geq \frac{R - E(R^+)}{SD(R^+)}\}$. The upper bound can be obtained for different values of Γ to find the critical value Γ^* at which the upper bound crosses the level α.

The relevant distribution (R^+ or R^-) to use for the sensitivity analysis corresponds to the direction of hidden bias that would undermine an initial finding of a significant treatment effect or reverse an initial finding of no effect. For example, if the finding is a significantly positive effect, we only need to worry about 'positive' selection, where a subject with a higher potential treatment effect is also more likely to be treated; thus, the relevant distribution is R^+ that embodies selection bias in this direction. On the other hand, if the finding is a significantly negative effect, then 'negative' selection where a subject with a lower potential treatment effect is also more likely to be treated can reverse or weaken the original finding; in this case, the sensitivity analysis with R^- is applicable.

As reviewed in the appendix, there exist alternative approaches of sensitivity analysis, but they are typically parametric in nature or not applicable to cases with continuous response variables. In comparison, the Rosenbaum (2002) approach imposes relatively mild assumptions (that the odds ratio of subjects matched on x be bounded between $1/\Gamma$ and Γ) and is straightforward to apply. While most other approaches specify how the unobserved confounder affects both the treatment and response, the Rosenbaum (2002) approach focuses only on how the unobservable may affect the treatment. Thus, the Rosenbaum (2002) approach is likely more robust to parametric misspecifica-

tions (and at the same time, conservative). On the other hand, by leaving the relationship between the unobserved confounder and the response unspecified, this approach cannot in general construct bias-adjusted effect estimates as in parametric approaches (of the sensitivity analysis nature or of the Heckman type). Instead, this approach evaluates how robust the effect estimate obtained under the assumption of no hidden bias is to the unobserved selection problem. This sensitivity analysis ultimately relies on the researcher's judgement of whether the critical value Γ^* at which the initial significance finding reverses is considered large enough. In general, the more important covariates are included in x and the smaller the likelihood of unobserved confounders to make Γ deviate much from 1, the smaller a threshold value may be adopted. Roughly speaking, we will adopt a threshold of 1.5; see Aakvik (2001) and Hujer et al. (2004) for similar stances.

3. DATA DESCRIPTION

We use the Rose (2004) data set (faculty.haas.berkeley.edu/arose/GATTdataStata.zip). Readers are referred to the source for a detailed account of the data. We will also use the Tomz et al. (2007) data set in Section 5.2 below.

We use the same set of covariates as in Rose (2004) to allow comparison with the studies in this literature that mostly use the same set of covariates but follow parametric regression approaches. The covariates x include *ldist* (the log distance of a dyad), *lrgdp* (the log product of a dyad's real GDP's), *lrgdppc* (the log product of a dyad's real GDP's per capita), *comlang* (= 1 if a dyad share a common language), *border* (= 1 if a dyad share a land border), *landl* (= the number of landlocked countries in a dyad), *island* (= the number of island nations in a dyad), *lareap* (the log product of a dyad's land areas), *comcol* (= 1 if a dyad were ever colonies after 1945 with the same colonizer), *curcol* (= 1 if a dyad are in a colonial relationship), *colony* (= 1 if a dyad were ever in a colonial relationship), *comctry* (= 1 if a dyad remained part of the same nation during the sample period), *custrict* (= 1 if a dyad use the same currency), *regional* (= 1 if a dyad belong to the same regional trade agreement), and year dummies for $t = 1948, \ldots, 1999$.

The response variable is *ltrade* (the log average value of a dyad's current real bilateral trade flows). In addition to the GATT/WTO membership effect, we also evaluate the treatment effect of the Generalized System of Preferences (GSP); this bilateral trade preference arrangement was found by Rose (2004) to have stronger trade effects than membership in the GATT/WTO. In particular, the treatment effects of both-in, one-in, and GSP are evaluated and the treatment indicator variable d corresponds to: *bothin* (= 1 if both countries in a dyad are GATT/WTO members, and = 0 if both are nonmembers), *onein* (= 1 if only one country in a dyad is a GATT/WTO member, and = 0 if both are nonmembers), or *gsp* (= 1 if a dyad have a GSP arrangement, and = 0 if not). When the GSP effect is evaluated, the other two dummy variables (*bothin* and *onein*) are used as part of the covariates; when the both-in or one-in effect is evaluated, *gsp* is used as one of the covariates.

The data set includes 234,597 observations on trade flows among 178 IMF trading entities

between 1948 and 1999 (with some "gaps" and missing observations). There are 12,150 distinct dyads and on average about 19 observations for each dyad. Table 1 gives the summary statistics of the covariates across three groups of observations by their joint membership status (both in, one in, or none in). The control (none-in) dyads on average tend to be closer in distance, smaller in economic sizes, poorer, and appear in earlier years. Based on simple logistic regressions, Table 2 shows that most of the observable covariates affect the selection into membership, and their selection effects (in terms of odds) are statistically significant (different from one). For example, dyads that are farther apart from each other, larger in economic sizes, have landlocked nations, have island nations, have colonial ties, use the same currency, and have a GSP relationship are more likely to be GATT/WTO members.

A typical concern about the use of matching methods is the extent of overlapping support of the distribution of observable covariates between the treatment and control group. Figure 1 provides one such visual check often used in the literature, where based on the same logistic regression as above, the propensity score of an observation taking the treatment is estimated and its frequencies tabulated across the treatment and control group. The histograms in Figure 1 show that the supports of the propensity score overlap fairly well between the both-in treated and control group, or between the one-in treated and control group.

4. BENCHMARK RESULTS

The benchmark results based on the Rose (2004) data set are shown in Table 3, labeled 'unrestricted matching'. The number of matched pairs for the effect on the treated (untreated) is indicated by M_1 (M_0). We set the caliper such that only the best 100%, 80%, 60%, or 40% of matched pairs obtained are included in the analysis. With the caliper choice of 60%, for example, the matched pairs with the quadratic distance exceeding the upper 60 percentile of all matched pairs obtained are discarded. The caliper choice of 100% is equivalent to using all matched pairs.

Table 4 describes what kinds of observations are dropped as poor matches by tight calipers. As indicated in Table 1, the control (none-in) dyads are on average smaller in economic sizes, poorer, and appear in earlier years. Thus, Table 4 shows that as the caliper gets tightened, the right tail of the treated (both-in or one-in) dyads are trimmed in the estimation of the effect on the treated and the left tail of the untreated (none-in) dyads are trimmed in the estimation of the effect on the untreated, as they are the ones that cannot find good matches from the other group. For example, as the 80% caliper is set, the both-in dyads that are on average the biggest in economic sizes ($E(lrgdp) = 49.030$), the richest ($E(lrgdppc) = 17.010$), and appear in the most recent years ($E(year) = 1987.8$) are dropped first. As the 60% caliper is set, the next biggest, the next richest, and the next most recent both-in dyads are dropped. The reverse is generally true for the none-in dyads. Some examples are the United States, Germany, and Japan, who have been among the richest countries and joined the GATT in 1948, 1951, and 1955, respectively. Most 'US-Japan' both-in observations are dropped with the 80% caliper and all are dropped with the 60% caliper;

similarly, only 10 out of 49 'US-Germany' both-in observations are kept with the 60% caliper and all are removed with the 40% caliper.

4.1 Both-In Effects

Turn back to Table 3. Note that when the both-in treatment effect is studied, the one-in observations are dropped. Column (i) shows a large mean effect on the treated: membership in the GATT/WTO by both countries on average raises bilateral trade volume by 74% ($= e^{0.553} - 1$) to 277% ($= e^{1.328} - 1$) for dyads who both chose to be in the GATT/WTO. These effects are significantly positive, regardless of the caliper choice, as the corresponding p-values in column (ii) or CI's in (iii) indicate. The point and interval effect estimates in (iv) and (vi) based on the signed-rank statistic are obtained under the stronger assumption of homogeneous treatment effects, as mentioned in Section 2.3 and explained in detail in the appendix, but they appear to be in an order of magnitude similar to those of (i) and (iii) reported above. This could indicate that the ranks are not substantially affected by subtracting a uniform effect from all pairs instead of a hypothetically heterogeneous effect from each pair. Recall that the p-value of the R-statistic in (v) is valid against homogeneous or heterogeneous treatment effects; the results are similar to those of (ii) and clearly reject the null hypothesis of no effect. We note the similarity in the results obtained based on the D-statistic and the R-statistic, and will henceforth focus on the effect estimates of the former that allows for heterogeneous effects.

How robust is the finding of a significant both-in effect to the possibility of unobserved confounders? Since the finding is a positive effect, only positive selection is a concern. Results of the sensitivity analysis indicate that the positive both-in effect is robust to a positive selection (cf. R^+) to the extent that the odds of a treated subject taking the treatment is not more than 2.081 times that of a comparable untreated subject in terms of observed covariates (by the 80% caliper and the two-sided test). The robustness is stronger with a one-sided test (naturally) and with a larger caliper choice, with Γ^* ranging from 1.467 to 2.434. In similar sensitivity analyses, Aakvik (2001), Hujer et al. (2004), Caliendo et al. (2005), Hujer and Thomsen (2006), and Lee and Lee (2009) seem to adopt a threshold around 1.5. By this threshold, the above finding of a positive both-in effect is reasonably robust to hidden selection biases. In addition to the Rosenbaum (2002) sensitivity analysis, we will also conduct further refinements of the matching procedure in Section 5.1 to reduce potential sources of unobserved heterogeneity across the treatment and control group.

Relative to the both-in effect on the treated, the both-in effect on the untreated is smaller and less robust to potential hidden biases. The estimates suggest that bilateral trade volumes would have increased by 20% ($= e^{0.185} - 1$) to 40% ($= e^{0.337} - 1$) if the nonmember dyads were to both join the GATT/WTO. Overall, the mean both-in effect on all trading relationships (mainly driven by the effect on the treated) is positive and significant, with the estimates ranging from 53% ($= e^{0.428} - 1$) to 224% ($= e^{1.175} - 1$). Given that the estimates of the effect on the untreated are more sensitive to hidden biases, the empirical evidence for a positive *potential* both-in effect for nonmember dyads is not as strong as the *realized* both-in effect for member dyads. Similar observations apply to the

one-in treatment analyzed below. Although there are exceptions as various robustness checks are performed below, the evidence for a positive membership/participation effect on the untreated is not strong. Thus, our discussions will henceforth focus on the effect on the treated.

On theoretical grounds, there are several economic models that lead one to expect a positive both-in effect on trade. Among others, the terms-of-trade argument (Johnson, 1953–1954; Bagwell and Staiger, 1999, 2001) suggests that multilateral trade agreements help coordinate countries' trade policies and remove their terms-of-trade incentives to raise trade barriers. The terms-of-trade incentive is shown by Broda et al. (2008) to be an important factor in non-WTO countries' trade policy. The political-commitment argument (Staiger and Tabellini, 1987, 1989, 1999), on the other hand, suggests that multilateral trade agreements help national governments commit themselves to liberalized trade policies, which brings about efficient production and trade structures.

In spite of the above theories, there are several empirical difficulties in using membership to measure the GATT/WTO effect, as noted by many in the literature, cf. Rose (in press). First, tariff reductions and policy liberalizations do not necessarily coincide with the date of accession. Second, some GATT/WTO members may extend their most-favored-nation (MFN) treatment to nonmember trading partners. Third, some countries (particularly developing countries) did not liberalize their trade policies in spite of their membership in the GATT (although this is less the case under the WTO). Fourth, some sectors (e.g. oils and minerals) face little protectionism with or without the GATT/WTO, while some (e.g. agriculture) are highly protected with or without the GATT/WTO. The first two considerations imply that membership is a noisy measure and the estimates will be downward biased, while the last two imply that GATT/WTO effect is heterogeneous with no effect in some cases. The fact that we obtained positive significant effects implies that on average across many trading relationships, the theoretical both-in effect is strong enough to dominate the above factors and to leave an empirically measurable impact.

4.2 One-In and GSP Effects

Unlike the both-in effect where one may expect a positive effect, or a zero effect at worst, *a priori*, the one-in effect can take either sign. On one hand, import diversion by the new member from its nonmember trading partner to other member trading partners may lower the dyad's bilateral trade volumes. On the other hand, in many cases, when a country joins the GATT/WTO, its tariff reductions (and other policy liberalizations) offered to members on a MFN basis are also extended to nonmember trading partners. In this case, imports increase from all sources, including that of nonmember trading partners. Furthermore, when a country gains access to the markets of existing GATT/WTO members with the newly acquired membership, it may increase imports of inputs necessary for the production of exports to these destinations. Some of these additional imports may fall on third nonmember countries. For example, with the accession into WTO, China may increase imports of oil from Iran in its expansion of production and export activities. The figures in Table 3 suggest that the one-in effect on the treated is overall positive; the estimates range from 39% ($= e^{0.326} - 1$) to 115% ($= e^{0.767} - 1$). These one-in effects are smaller than the both-in

effects, but are positive and significant. Thus, the trade-creating effects when one country in a dyad unilaterally joins the GATT/WTO dominate the potential trade-diverting effects.

Relative to the GATT/WTO membership, a preferential GSP scheme is also found to promote bilateral trade, by a factor of 94% ($= e^{0.665} - 1$) to 134% ($= e^{0.851} - 1$) [the upper bound estimate is very close to Rose's (2004) benchmark estimate 136% ($= e^{0.86} - 1$)]. The GSP effect estimates are smaller than the both-in effects and larger than the one-in effects in general. The finding of this ranking of the three treatment effects seems reasonable. Since the GSP is of unilateral trade preferences extended only from a high-income country to its poor trading partners, its likely effect on bilateral trade volumes is *a priori* smaller than if both the rich and the poor countries in a dyad lower their import restrictions against each other, which happens presumably if both join the GATT/WTO. On the other hand, any trade-promoting effect of the one-in membership is, as argued above, indirect and conditional on the spillover of the MFN treatment and on the dyad's initial trade pattern, while the effect of GSP is directly derived from a straightforward reduction of dyad-specific trade resistance. As we shall see, this ranking (both-in effect > GSP effect > one-in effect) holds in general regardless of refinements to the matching procedure or variations in the data used (although the one-in effect is sometimes larger than the GSP effect). Note the relatively large range of both-in effect estimates across calipers, in contrast with the relatively narrow range of GSP effect estimates. Among others, this may reflect heterogeneous both-in effects across trading relationships as discussed above.

It may be also helpful to point out that the positive and stronger trade effect of both-in is shared by a larger number of bilateral trading relationships ($114,750$) than that of GSP ($54,285$). Thus, either on the average or in the aggregate, our estimation results suggest that the realized trade-creating effect of GATT/WTO membership is larger than GSP.

For the GSP effect, we did not report the effect on the untreated, as the GSP does not apply to all kinds of trading relationships. For example, it is not relevant to propose a GSP arrangement between two poor countries. On the other hand, the estimates of the GSP effect on the treated reported above in Table 3 should be taken with a grain of salt, as the conditioning set of covariates do not control for the development stages of the two countries in a dyad. The existence of a GSP arrangement between a dyad and their bilateral trade volumes are both very likely dependent on their relative development stage, which implies potential selection biases. We shall see a refinement to the matching procedure in Section 5.1 to address this concern.

5. ROBUSTNESS CHECK

5.1 Restricted Matching

In this section, we refine the baseline matching procedure to address some potential sources of selection biases that may still remain with x controlled. Although we did the Rosenbaum (2002) analysis to assess the sensitivity of the benchmark results to whatever selection bias may remain, the analysis itself does not remove the bias. In the literature, four potential sources of biases

seem to be of major concern. They are systematic unobservable heterogeneity across dyads, across years, across GATT/WTO negotiating rounds, and across developing and developed countries. We restrict the potential match for a subject to observations that have the opposite treatment status (as in the case of unrestricted matching) and that are also from the same dyad, the same year, the same time period defined according to GATT/WTO negotiating rounds, and the same combination of relative development stage, respectively. By restricting the potential match to the observations of the specified criterion (say, the same dyad), we remove the likely bias arising from systematic unobservable differences (say, across dyads) that influence bilateral trade volumes as well as selection into GATT/WTO or GSP.

5.1.1 Same Dyad. Table 5 summarizes the restricted matching results based on the Rose (2004) data set. Extended results for each set of restricted matching not reported here (such as the point effect estimate and CI based on the R-statistic) are available upon request. The number of matched pairs obtained when matching is restricted within the same dyad shrinks substantially for both-in (19,760 vs 9,510) and one-in treatments (23,463 vs 15,182), as some dyads may not have both treated and untreated observations during the sampling years. For example, the 'US-Japan' dyad have 'one-in' (years 1950–1954) and 'both-in' (years 1955–1999) observations but do not have 'none-in' observations. In cases like this, dyads without qualified control/treated subjects are dropped from the estimation. While the 'within-dyad' estimates suggest overall smaller treatment effects on the treated, the trade-enhancing both-in effect continues to be economically and statistically significant, and larger than either the one-in or GSP effect. The estimates of the both-in effect on the treated range from 114% ($= e^{0.760} - 1$) to 156% ($= e^{0.941} - 1$), those of the one-in effect on the treated range from 37% ($= e^{0.314} - 1$) to 59% ($= e^{0.464} - 1$), and those for GSP from 31% ($= e^{0.271} - 1$, in this case, smaller than the one-in effect) to 64% ($= e^{0.492} - 1$).

5.1.2 Same Year. Alternatively, we restrict matching to observations from the same year; this controls for possible year-specific effects. The 'within-year' results are almost the same as the benchmark results. This indicates that in unrestricted matching, the matched subjects are often from the same year; thus, the estimates in Table 3 pick up mostly cross-sectional variations. This is not surprising, as the set of covariates x in unrestricted matching include year dummies, which encourages matching observations from the same year. A further look into the data (not shown in the table) at every five-year interval $(1950, 1955, \ldots, 1995)$ shows that the positive both-in or one-in effect is not lumpy in a few particular years but is felt throughout the years, except in 1975 and 1995 when there is a dip in the membership effects. On the other hand, the GSP effect is low in 1970 (its early phase) and 1995. The same pattern is observed as the Tomz et al. (2007) data set is used, cf. Section 5.2.

In contrast with the 'within-year' estimates that measure cross-sectional (or 'between') variations, the 'within-dyad' estimates measure time-series (or 'within') variations. Both 'within' and 'between' variations indicate that there are significant gains in trade volumes by joining the GATT/WTO, although the 'within' effects are overall smaller than the 'between' effects. This seems

at odd with the results in Rose (2004), where he finds stronger effects with fixed-effect estimation (which reflects 'within' variations in a panel framework) than with OLS estimation (which reflects both 'within' and 'between' variations). We shall see that this relative ranking is not universal and changes as the Tomz et al. (2007) data set is used.

The difference between the 'within' and 'between' effects may reflect different theoretical effects along the time-series and cross-sectional dimensions. Alternatively, theoretical effects may be the same along these two dimensions, but empirical factors as discussed earlier obscure the theoretical effect to different extents in these two dimensions. For example, if the phenomenon of liberalizations prior to or later than the date of accession is prevalent, the 'within' estimates of the theoretical both-in effect based on the date of accession will be biased downward. The earlier the advance or the longer the phase-in period, the stronger the downward bias of the 'within' effect estimate. On the other hand, the 'between' estimates of the both-in effect based on formal membership are likely affected by the other problems. For example, comparison of two developing member countries that do not make significant trade liberalizations with two other comparable nonmember developing countries implies a zero both-in effect. The stronger the presence of these dyads, the smaller the 'between' estimate of the overall both-in effect. *A priori*, it is difficult to say which of the two effect estimates – 'within' or 'between' – is likely to be larger or smaller. It is also understandable that the ranking of the two effect estimates can reverse with a different definition of involvement in the GATT/WTO, if that changes the date of associated treatment and the status of treatment for a significant number of observations.

5.1.3 Same GATT/WTO Round. Define periods according to the GATT/WTO trade negotiations rounds: 1948 (Before Annecy round), 1949-1951 (Annecy to Torquay round), 1952-1956 (Torquay to Geneva round), 1957-1961 (Geneva to Dillon round), 1962-1967 (Dillon to Kennedy round), 1968-1979 (Kennedy to Tokyo round), 1980-1994 (Tokyo to Uruguay round), and 1995- (After Uruguay round). By restricting matching to observations from the same period, the 'within-period' estimates are almost the same as in unrestricted matching. This is no surprise, given our finding above that matched subjects in unrestricted matching often come from the same year; the criterion of matching within the same period does not impose extra restriction in most cases.

5.1.4 Same Development Stage Combination. The last column 'within-devel.' shows results when matching is restricted to the same development stage combination, where the combinations are: low-income/low-income, low-income/middle-income, low-income/high-income, middle-income/middle-income, middle-income/high-income, and high-income/high-income dyads. The argument for conducting this restricted matching is that a dyad of developed countries are likely to have a trade structure systematically different from a dyad of developed/developing countries (e.g. intra-industry trade versus inter-industry trade) and are not comparable in terms of their potential trade volumes with or without the membership. At the same time, the probabilities of being in the GATT/WTO may vary systematically across development stages. In this case, matching within the same development stage combination removes this source of potential bias. Similar (or

stronger) critique applies to estimating the GSP treatment effect, given that the decision to use GSP is directly dependent on a dyad's relative development stage. As the set of covariates in this exercise also include year dummies, we compare the results with the benchmark or 'within-year' estimates. The 'within-devel.' effects on the treated are smaller overall, although the relative ranking remains the same (both-in effect > GSP effect > one-in effect). The current estimates suggest that membership raises bilateral trade by 48% ($= e^{0.393} - 1$) to 208% ($= e^{1.124} - 1$) for dyads that both chose to be in the GATT/WTO, and by 27% ($= e^{0.242} - 1$) to 92% ($= e^{0.650} - 1$) for dyads where only one country chose to be in the GATT/WTO. In comparison, the GSP is estimated to raise bilateral trade by 51% ($= e^{0.410} - 1$) to 108% ($= e^{0.732} - 1$).

Are the positive membership effects shared evenly among countries of different development stages, or are they concentrated on particular subsets of countries? A further look into the data (not reported in the table) shows that the positive effects are indeed concentrated on dyads of middle-income/middle-income, middle-income/high-income, and high-income/high-income countries. The low-income countries do not benefit much from a membership in the GATT/WTO. Similar lumpy patterns were found in Subramanian and Wei (2007), although we still find a positive average effect while they found no positive average effect. This asymmetry may reflect the two empirical concerns that major export sectors (e.g. agriculture) of low-income countries still face steep protectionism from the rich world with or without the GATT/WTO and that the low-income countries themselves do not significantly liberalize their import sectors despite their membership in the GATT/WTO. Interestingly, the GSP effect also shows some lumpy patterns, where the positive effect is mostly driven by dyads that involve a high-income country. Similar observations apply when the Tomz et al. (2007) data set is used.

How robust are the restricted matching effect estimates? As the matching criterion becomes more stringent so that potential sources of selection biases are minimized, one may accept a lower critical threshold for Γ^* than in unrestricted matching, as the remaining possibility of selection bias is lower. Both 'within-dyad' and 'within-devel.' matching impose effective constraints relative to the benchmark. In the latter case, positive treatment effects on the treated are less robust in general than the benchmark. The tolerance threshold (Γ^*) for positive selection now stands at 1.256, 1.197, and 1.530 for the lower bound estimates of the both-in, one-in, and GSP effect, respectively. The GSP estimate seems rather robust in this matching exercise, when matching within the same relative development stage is argued above to be a desirable restriction for the GSP effect estimation. Thus, we regard this set of GSP estimates as preferred ones. Similar conclusions are reached when the Tomz et al. (2007) data set is used instead.

In contrast, the robustness of the 'within-dyad' estimates of membership effects on the treated strengthens ($\Gamma^* = 2.503$ at the minimum for the both-in effect, and $\Gamma^* = 1.508$ at the minimum for the one-in effect). Thus, it is relatively comfortable for us to accept the 'within' estimates of the both-in and one-in effects, as the tolerance level for hidden selection biases is higher despite the fact that the possibility of remaining selection biases is lower with the extra matching criterion.

5.2 Participation instead of Formal Membership

Tomz et al. (2007) stress the importance of *de facto* participation in the multilateral system by nonmembers such as colonies, newly independent colonies, and provisional members. They share to a large extent the same set of rights and obligations under the agreement as formal members. Tomz et al. (2007) classify these territories as nonmember participants and define participation to include both formal membership and nonmember participation. Without changing the estimation framework of Rose (2004), they find significant participation effects on trade.

We verify if the alternative definition of GATT/WTO involvement changes our conclusions using matching. Table 6 summarizes the results. When the GSP effect is estimated, the participation status of a dyad, instead of their membership status, is used as part of the covariates. Thus, the GSP effect estimates are not exactly the same as those based on the Rose (2004) data set. Tomz et al. (2007) also corrected some coding errors in Rose's data set, in particular, the income status and geography indicator of some territories (Tomz et al., 2007, Foonote 32). This explains the difference in the number of matched pairs obtained for GSP under 'within-devel.' with the alternative data set.

Table 6 indicates that participation effects are overall stronger than membership effects. Both the 'between' estimates ('unrestricted', 'within-year', 'within-period') and the 'within' estimates ('within-dyad') are larger than corresponding estimates based on Rose's data set. They are also more robust to hidden selection biases. In particular, the 'within' estimates are so much larger that they are now larger than the 'between' estimates (cf. Section 5.1). The exception to this pattern – stronger participation effects than membership effects – occurs when matching is restricted within the same relative development stage. In contrast, the estimates of the GSP effect are overall smaller when matching is based on the Tomz et al. (2007) data set than on the Rose (2004) data set, regardless of the matching criteria. However, the difference is not large. This is understandable given that the change in the GATT/WTO indicator from membership to participation affects the GSP estimates only indirectly through conditioning covariates that include many other variables. In all, the current finding of an overall larger participation effect than the membership effect is consistent with the contrasting results found by Tomz et al. (2007) and by Rose (2004).

5.3 Kernel-Weighting Matching

In this section, we verify the robustness of the point effect estimates to kernel-weighting matching using the Rose (2004) data set. In contrast with pair matching which uses only the nearest match, kernel-weighting matching uses multiple potential matches with weights attached defined by the chosen kernel and bandwidth. In particular, we use the normal kernel and define weights for the potential matches $i't'$ of a treated (untreated) subject it as $w_{it,i't'} \equiv \phi(\frac{x_{1,it}-x_{1,i't'}}{SD(x_1)h}) \dots \phi(\frac{x_{P,it}-x_{P,i't'}}{SD(x_P)h})$ where $\phi(\cdot)$ denotes the standard normal density function, P the dimension of the covariate vector x, $SD(x_p)$ the standard deviation of a covariate x_p in the pooled sample, and h the chosen bandwidth. For matching within dyad where the number N_{it} of potential comparison subjects for a subject it is small, we use a larger bandwidth $h = 0.5N_{it}^{-1/(P+4)}$; otherwise, we use a smaller bandwidth

$h = 0.25N_{it}^{-1/(P+4)}$ (the computation hits numerical bounds for smaller bandwidths than this). The kernel-weighting matching estimator is then defined as $\frac{1}{M}\sum_{it}(y_{it} - \sum_{i't'} \tilde{w}_{it,i't'}y_{i't'})$, where $\tilde{w}_{it,i't'} \equiv w_{it,i't'} / \sum_{i't'} w_{it,i't'}$ is the normalized weight. We set calipers in the same fashion as in pair matching, such that subject it that does not have a good match in terms of the scale-normalized distance is discarded. Table 7 summarizes the results. The effect estimates are very similar to those obtained by pair matching, cf. Tables 3 and 5, across types of treatments, calipers, and the matching criteria. We also experiment with larger bandwidths. As the chosen bandwidth is enlarged, the point effect estimates tend to increase. Thus, we may consider the pair matching estimates as overall conservative estimates.

5.4 Non-random Incidence of Positive Trade Flows

By using the data set of Rose (2004) or Tomz et al. (2007), we have based our analysis on observations with positive trade flows. Recent studies of Helpman et al. (2008) and Felbermayr and Kohler (2007) emphasize the importance of incorporating observations with zero trade flows in estimating the gravity equation. In particular, both studies find that GATT/WTO membership has a positive effect on the formation of bilateral trading relationships. This suggests that using only observations with positive trade flows will induce a downward bias in the effect estimate of GATT/WTO membership (and other trade barriers in general) on trade volumes, since a dyad who are not GATT/WTO members but still observed trading with each other are likely to have lower unobserved trade resistance. Both studies find that consideration of this selection bias alone indeed strengthens the gravity equation estimates albeit not considerably.[2]

Given that we found a strong and positive membership effect based on positive trade flows, the above selection argument suggests that incorporating observations with zero trade flows in our analysis will only strengthen the initial finding of a positive effect. Thus, we do not expect our general conclusions to change with the inclusion of zero trade. Both studies by Helpman et al. (2008) and Felbermayr and Kohler (2007) are based on parametric estimations of the trade flow equation, although the former considers parametric as well as nonparametric estimations of the selection equation. To estimate the membership effect and also to address the selection into positive trade flows in a fully nonparametric framework, one can potentially apply the newly proposed methodology of Lee (2008). We leave this considerably more extensive work for future research, and attempt a less ambitious approach here to isolating the GATT/WTO membership effect on trade volumes from its effect on 'trade start' without resorting to a new data set and a full-blown new estimation framework.

Still based on the Rose (2004) data set, we use only observations where a dyad start trading with each other before joining the GATT/WTO. In other words, these dyads have reported bilateral

[2]Helpman et al. (2008) also distinguish the direct partial effect of trade resistance on trade flows from its indirect effect on trade flows through changes in the number of exporters. In this paper, we have not made this distinction. In our view, the larger trade flows due to an increase in the number of exporters should also be considered as part of the benefit of GATT/WTO membership. Thus, the matching estimates presented correspond to the total effect of GATT/WTO membership, including both the direct and indirect effects.

trade flows before either one country of them ever joins the GATT/WTO. Using this sub-sample of observations that trade with or without the GATT/WTO membership, the membership effect on prompting new trading relationships is not present; thus, the effect estimates consist only of the GATT/WTO membership's effect on trade volumes. Table 8 presents the effect estimates for this sub-sample following the same matching procedure as in the benchmark and restricted matchings. We see that this refined analysis reports overall stronger membership effects, and thus in a way the results are consistent with the above selection argument.

5.5 Multilateral Resistance

Relative trade resistance rather than absolute trade resistance is argued by some gravity theories to be more appropriate in explaining bilateral trade flows, cf. Anderson and van Wincoop (2003), and multilateral resistance (MR) terms may have to be included in the list of covariates. As their paper suggested, there are two ways to control for the terms. One is to solve the endogenous MR terms given the parameter values and then to estimate the parametric gravity equation incorporating dyads' MR terms by nonlinear least squares. Both the solution to the endogenous MR terms and the parametric gravity equation rely on certain functional form assumptions and thus are subject to specification errors as noted by the authors themselves, which are exactly what we try to avoid in the current paper by using the matching framework. An alternative suggested by the same authors is to replace the MR terms with country dummies. In a way, we have controlled for dyad-specific and hence country-specific effects when we conduct the matching within the same dyad; the strong effects of GATT/WTO remained. On the other hand, we do not have a good way in the matching framework to control for time-varying country-specific effects as emphasized by some parametric studies, cf. Subramanian and Wei (2007).

Recent studies by Baier and Bergstrand (2009a,b) present some potential methods to approximate the endogenous MR terms by observable exogenous trade resistance covariates and thus the possibilities to address time-varying MR terms in the matching framework. Specifically, in one version of their proposed approximations, the two country-specific MR terms for a dyad are decomposed into a list of MR terms associated with each trade resistance covariate. For example, the MR term for a trade resistance covariate x^r_{kmt} between countries k and m in year t, would be $MRx^r_{kmt} = (1/N)\sum_{m'=1}^{N} x^r_{km't} + (1/N)\sum_{k'=1}^{N} x^r_{k'mt} - (1/N^2)\sum_{k'=1}^{N}\sum_{m'=1}^{N} x^r_{k'm't}$, reflecting the respective average trade resistance of the two countries to all their trading partners, adjusted by a typical country's average resistance to all its trading partners. One can add this list of MR terms to the list of covariates already used in the matching.[3] Specifically, to estimate the both-in treatment effect, we follow the same matching procedure as in the benchmark case but with the modified list of matching covariates that include the same economic size covariates (*lrgdp*, *lrgdppc*, *lareap*), the trade resistance covariates (*ldist*, *comlang*, ..., *regional*, *gsp*) and their corresponding MR terms,

[3] Alternatively, one can construct the relative trade resistance covariate $BVx^r_{kmt} \equiv x^r_{kmt} - MRx^r_{kmt}$ and use it in place of the absolute trade resistance covariate x^r_{kmt} in the matching, as done in Baier and Bergstrand (2009b). We take the former approach, as it imposes less structure.

year dummies, and the MR term of the treatment dummy.[4] Similar adjustments are made to the list of matching covariates for one-in and GSP effect estimations. The results are summarized in Table 9.

When the multilateral resistance terms are controlled for, we see that the strong both-in effects on the treated remain; at the same time, the both-in effects on the untreated strengthen and become more similar in magnitude to those on the treated. Overall, the both-in treatment effect is economically and statistically significant. Based on the lower bound estimate, a treated dyad's bilateral trade flows are higher by 144% ($= e^{0.894} - 1$) than comparable untreated dyads. The one-in treatment effects now become weaker overall with statistically significant but small trade promoting effects.

The results are almost identical when the matching is restricted within the same year or the same period, reflecting again the fact that in the unrestricted matching, most matched observations are cross sections from the same year. When matching is restricted within the same dyad, the both-in and one-in effects are comparable to those in Table 5 without the MR terms controlled for. This suggests that the MR terms do not vary much across years for the same dyad, and hence the extra control does not affect the matching significantly. The trade creating effect is 133% ($= e^{0.845} - 1$) with both countries in the GATT/WTO, and 45% ($= e^{0.371} - 1$) with only one country in the GATT/WTO, based on the lower bound estimates for the effect on the treated. When matching is restricted within the same relative development stage, the both-in effects are also stronger with the MR terms controlled for, with the lower bound estimate suggesting a trade promoting effect of 77% ($= e^{0.569} - 1$). The mean both-in effect again masks a large variation across dyads of different development stages (not reported in the table), with large benefits tending to concentrate on higher income dyads and costs on lower income dyads. The one-in effects are again weaker with the MR terms controlled for, with either economically small or statistically insignificant effects.

The GSP treatment effects are stronger with the MR terms controlled for as in the case of both-in effects, and show a pattern of weaker results when matching is restricted within the same dyad. In the preferred case for the GSP estimation where matching is restricted within the same relative development stage, the lower bound estimate suggests a trade promoting effect of 104% ($= e^{0.712} - 1$).

5.6 Difference in Difference Matching Estimator

In this section, we explore an alternative treatment effect concept, difference-in-difference (DD), that is based on weaker identification assumptions. This method compares the difference over time in trade volumes of a treated dyad to that of a comparable untreated dyad. Consider a time period $[t - b, t + a]$ around the treatment timing t with $a, b > 0$. Using our notations, the DD treatment

[4]Note that we have included the MR term of *bothin* in the list of matching covariates in estimating the both-in treatment effect; thus, the estimated both-in effect corresponds to its partial equilibrium effect and not its potential general equilibrium effect (the estimation of which goes against the typical assumption of matching estimation). In the context of free trade agreements (FTAs) that Baier and Bergstrand (2009b) studied, they argued that the effect of the MR term of the treatment dummy, FTA, was conceptually negligible.

effect estimand is:

$$\begin{aligned} DD &= E(y_{t+a} - y_{t-b}|d=1,x) - E(y_{t+a} - y_{t-b}|d=0,x) \\ &= E(y^1_{t+a} - y^0_{t-b}|d=1,x) - E(y^0_{t+a} - y^0_{t-b}|d=0,x) \\ &= E(y^1_{t+a} - y^0_{t+a}|d=1,x) \end{aligned} \tag{5}$$

if the *same time-effect* condition $E(y^0_{t+a} - y^0_{t-b}|d=1,x) = E(y^0_{t+a} - y^0_{t-b}|d=0,x)$ holds. That is, DD identifies *the treatment effect on the treated at time* $t+a$ if the potential untreated response changes by the same magnitude on average over the time period $[t-b, t+a]$ for comparable treated and untreated dyads. This identifying assumption is weaker than $E(y^0|d=1,x) = E(y^0|d=0,x)$ required for the effect on the treated in the benchmark analysis, cf. Section 2.1, and thus is more robust to hidden biases due to selection on unobservables. For example, the same time-effect condition allows potential systematic unobserved dyadic heterogeneities across the treatment and control group or systematic time trends in trade volumes unrelated to the treatment, as long as the time trends are on average the same for comparable dyads. See Heckman et al. (1997) for DD estimation based on matching, and Imbens and Wooldridge (2009) and references therein for other DD approaches.

To estimate DD, we carry out matching in a similar fashion as described in Section 2.1. In particular, start with a both-in treated dyad. If the dyad was first treated in year t, the pool of potential matches for this dyad are dyads who were both not in the GATT/WTO throughout the period $[t-b, t+a]$. The best match is identified based on the baseline response and the covariates in the pre-treatment year (y_{t-b}, x_{t-b}). The same scale-normalized distance measure is used, with the sample variance of (y_{t-b}, x_{t-b}) calculated based on all observations in year $t-b$. Given the match, the difference over time in trade flows $(y^0_{t+a} - y^0_{t-b})$ of the control dyad is subtracted from the difference over time $(y^1_{t+a} - y^0_{t-b})$ of the treated dyad. Given M pairs of match, DD is estimated by the sample average of the pair-wise differences in differences. The one-in and GSP treatment analysis can be carried out similarly. Note that we have included the baseline response y_{t-b} in the list of matching covariates. This is to control for potential unobservables that may systematically affect trade flows but are not captured by the observables x_{t-b}, and thus to reduce the scope of selection on unobservables.

Some remarks are in order. First, selecting the lead and lag years (a, b) is difficult. One guideline is whether the same time effect condition will hold given the choice of (a, b). As noted earlier, policy changes do not necessarily coincide with the official year of GATT/WTO accession. Some countries may undertake structural changes required for the accession beforehand or economic agents may act on anticipation of the upcoming accession. Thus, trade flows may well have changed before the official accession of the treated dyad, and to satisfy the same time effect condition, a large b may be required. On the other hand, it is quite often true that acceding countries take several years to phase in the agreed-upon trade policy changes, and thus one may expect the treatment effect

to manifest itself only years later. A large a may address this concern. However, choosing too large a window (a, b) may pose two problems: first, the sample size will be significantly reduced as not all dyads have observations in long extended periods; second, with a long window, other factors not controlled for (by the same time effect condition and the matching covariates) may affect the trade flows and contaminate the result. We experiment with several symmetric windows: $a = b = \{1, 2, \ldots, 6\}$. Another remark worth noting is that a dyad typically went from a none-in period to a one-in period and then to a both-in period, if they were ever both-in treated. It is relatively rare for a dyad to simultaneously join the GATT/WTO and to go directly from none-in to both-in. To maintain reasonable sample sizes, we allow both scenarios of pre-treatment status (none-in or one-in) in estimating the both-in treatment effect. Thus, the both-in effect estimate is a mixture of the two effects when the dyad go from one-in to both-in and when the dyad go from none-in to both-in, relative to if they stay none-in throughout the interval. The one-in and GSP effect analysis are spared such complication.

The findings are summarized in Figure 2. The results are similar across different caliper choices. In general, the GATT/WTO membership effects are negligible in early phases of the treatment but become statistically and economically significant five or six years into the treatment. At year six, an average dyad's bilateral trade flows increase roughly by 65% ($= e^{0.5} - 1$). Similar patterns apply to the both-in or one-in treatment. In contrast, the GSP effect is small if not negligible and manifests itself relatively fast following the treatment. The effect remains relatively stable throughout the years, and is statistically insignificant in most cases.

These findings seem to agree with the casual observations and our discussions above regarding the gradual phase-in of policy changes after an official GATT/WTO accession. It may also be reconcilable with the larger benchmark and restricted matching estimates discussed in Tables 3 and 5. In these earlier exercises, we did not control for the vintage of the treated observations; thus, the treatment effect estimate effectively summarizes the effects across all vintages following the treatment for as far as several decades. If the effect is larger, the more aged the treatment is, a larger effect estimate observed in the previous exercises is understandable.

5.6.1 Placebo Exercise. In this section, we conduct "placebo" exercises to verify that the time trends of trade flows of matched dyads are comparable in advance of membership. A finding against differences in pre-trends would help alleviate concerns that the DD estimates may be picking up systematic differences in time trends between the treatment and control group due to unobservables not controlled for in our matching exercise. To do so, we apply the DD estimation procedure to a bogus treatment year $t' = t - d$ that predates the actual year of treatment t (here identified as the first year when either one country in a treated dyad joins the GATT/WTO). As there is no treatment at the bogus treatment year, the DD estimate, instead of estimating the treatment effect, captures the difference in the time trends between comparable treated and untreated dyads in advance of GATT/WTO membership.

As discussed above, countries may undertake policy reforms in advance of membership, and

their trade patterns may well have changed years before the official year of accession. Thus, the period of comparison of the pre-trends has to be set reasonably far into the past, such that it does not overlap with the likely period of transition to the accession. For this, we experiment with $d = \{7, \ldots, 12\}$ and symmetric DD windows $a = b = \{1, \ldots, 6\}$, with $d - a \geq 6$. That is, the period of comparison of the pre-trends will be at least six years before the actual year of treatment. For example, if the bogus treatment year is set 10 years before the actual treatment year, the forward/backward window for DD estimation can range from one to four years.

The results are summarized in Table 10. As can be seen from the table, of the 21 possible periods of comparison (and of the four caliper scenarios for each period), all estimates are not significantly different from zeros, except three estimates that are significantly negative (which does not go against a finding of positive treatment effects). Thus, on the whole, there is no evidence of systematic differences in the time trends in advance of membership between the treatment and control group that are comparable in terms of observables.

6. WHAT COULD BE THE PROBLEMS WITH THE PARAMETRIC GRAVITY ESTIMATES

The results above suggest that the conventional gravity models may be misspecified. In this section, we explore generalizing the parametric gravity model to reduce the discrepancy between the conventional OLS gravity estimates and the current nonparametric matching estimates. Our investigation points to one possible explanation: omission of relevant interaction terms. Interaction terms present two problems in the parametric framework. First, there can be too many when there are many covariates. Second, if the treatment interacts with some covariates as often happens in practice, this makes the effect heterogeneous and difficult to present. This is where nonparametric methods come particularly useful: nonparametric methods deliver findings without the need to search for the correct specification.

We first explore adding quadratic terms of continuous/categorical covariates and interactions of these covariates with all other binary covariates (other than the treatment dummies themselves), to the Rose (2004) default gravity specification. Many of these terms are significant, but the OLS estimates of the membership effects are not affected significantly.

While our matching estimator allows for heterogeneous treatment effects that vary with the observed covariates, the Rose (2004) gravity estimates basically assume homogeneous treatment effects. Subramanian and Wei (2007) allow for heterogeneous effects in the parametric framework but only across certain subsets of samples. To allow for more arbitrary forms of heterogeneous effects in the parametric framework, we explore adding interaction terms of the treatment dummies with all other covariates to the Rose (2004) default specification. The results are summarized in Table 11. When only the *bothin* treatment dummy is allowed to interact with the other covariates, the general finding does not change, although many of the interaction terms are significant. As both the *bothin* and *onein* dummies are allowed to interact with the other covariates, the mean

effects of both membership treatments become significantly positive. Many of the interaction terms are statistically significant, and the default model is rejected in favor of the alternative model. While the estimates for the main gravity covariates (such as distance and GDP) remain stable across specifications, estimates for the other covariates are not, suggesting that the modeling of these augmenting covariates (typically used to control for the degree of trade resistance) is problematic. Basically, parametric effect estimates of these augmenting trade resistance covariates are very sensitive to the model specifications. This may help explain some of the disagreements in the gravity literature regarding the currency union effect (Persson, 2001; Rose, 2001) or the free trade agreement effect (Frankel, 1997; Baier and Bergstrand, 2007).

As the *gsp* dummy is also allowed to interact with the other covariates, the mean effect estimates of the both-in and one-in membership remain significantly positive. The GSP mean effect estimate is, however, rather similar to its marginal effect estimate in the default specification. This suggests that allowing for heterogeneous GSP effects helps increasing the explanatory power of the model but the degree of heterogeneity is not strong, compared with the both-in and one-in effects. This also agrees with the findings of the matching framework above: while the GSP effect estimates are relatively stable across the choice of calipers, the both-in and one-in effect estimates vary a lot, and while the GSP effect estimates are relatively similar across the parametric and nonparametric approaches, the membership effect estimates are very different across the two approaches.

Based on the results in the last column of Table 11, it appears that the both-in and one-in membership effects are intensified by GDP's per capita and the physical areas of the dyad, and are also intensified if the dyad share a common language, were ever in a colonial relationship, or belong to a common currency union. Overall, the explorations above suggest that it is important in practice to recognize the potential nonlinearity in which trade resistance covariates interact with each other in affecting bilateral trade flows.

In the Rose (2004) default specification, the MR terms are not controlled for. We also explored controlling for the MR terms before proceeding with the same experiment as above of adding higher-order or interaction terms. In particular, we follow Subramanian and Wei (2007) and use time-varying country dummies to proxy for the MR terms in the Rose (2004) parametric framework.[5] The findings are similar to those above without the MR terms controlled for. The both-in and one-in effect estimates are not statistically significant by controlling for the MR terms alone. By incorporating the interaction terms of the membership dummies with the other covariates, the effects turn significantly positive. The set of statistically significant interaction terms are similar: e.g., GDP's per capita, a common language, and being ever in a colonial relationship tend to strengthen the membership effects.

In the current application, we stop short of fully explaining away the discrepancy between the

[5]Instead of using the complete Rose (2004) data set, only observations at every five years between 1950 and 1995 are used. This is to keep the number of time-varying country dummies computationally manageable; see Subramanian and Wei (2007) for the same approach. Five variables—*lrgdp*, *lrgdppc*, *landl*, *island*, *lareap*—are dropped from the list of regressors, as their coefficients cannot be precisely estimated with the presence of time-varying country dummies; their higher-order terms or interaction terms with the other covariates can still be included, however.

effect estimates of the parametric and nonparametric approaches, as the dimension of the covariate vector is high and there are many potential functional forms of the covariates. For example, the treatment dummies may also interact with the interaction terms of the other covariates. Nonetheless, our limited search suggests that the assumption of homogeneous treatment effects could be a major source of misspecification. The nonparametric framework we propose in this paper offers a convenient estimation framework to accommodate heterogeneous treatment effects and at the same time circumvents the specification difficulty in a high-dimensional application.

7. CONCLUSION

This paper contributes to the literature on the effects of GATT/WTO membership/participation on actual trade flows. Previous studies of this issue have largely relied on parametric gravity-based trade models. Concerns about parametric misspecifications, the assumption of homogeneous treatment effects and unobserved selection bias are raised by the current paper and addressed by using nonparametric methods. In particular, pair-matching estimator is used to obtain the point effect estimates, permutation tests to derive the inferences, and a sensitivity analysis based on signed-rank tests to evaluate the robustness of the baseline results to unobserved confounders. The last two methods are relatively new in econometrics. We put together these statistical tools in a coherent manner so that they could be easily applied to other treatment effect problems of similar nature.

Our findings suggest that membership in the GATT/WTO has a significant trade-promoting effect for dyads that have both chosen to be members. The effect is larger than bilateral trade preference arrangements, GSP, and larger than when only one country in a dyad is a member. Although the GSP effect appears to be relatively constant across subjects, the both-in and one-in effects display substantial heterogeneities. The finding of a positive both-in effect is quite robust to potential unobserved confounders but the finding of a positive one-in effect is less robust.

The overall conclusion does not change when we restrict the matching to observations from the same dyad (thus, capturing the within effect), the same year (thus, capturing the between effect), the same GATT/WTO period, or the same relative development stage. The overall conclusion does not change either when we use participation status instead of formal membership as the treatment indicator, or when we use kernel-weighting matching instead of pair-matching. The results are also robust to using only observations where a dyad's trading relationship exists before either one country of them ever joins the GATT/WTO (thus, isolating the membership's effect on trade volumes from its effect on the formation of trading relationships), and robust to controlling for time-varying multilateral resistance terms in the matching framework. A final robustness check using the difference-in-difference matching estimator reveals that the significant and positive GATT/WTO effect on trade takes several years after the official accession before manifesting itself.

The contrast between the results of the current paper and of Rose (2004) suggests that conventional gravity models may be misspecified. We show that the omission of interaction terms between membership dummies and functions of the other covariates from the gravity model may

be the major source of misspecification. The nonparametric framework we propose in this paper offers a convenient estimation framework to accommodate heterogeneous treatment effects and at the same time circumvents the specification difficulty in a high-dimensional application.

8. APPENDIX: PERMUTATION TEST FOR MATCHED PAIRS

Recall that $D' \equiv \frac{1}{M}\sum_{m=1}^{M} w_m s_m(y_{m1}-y_{m2})$, where only the permutation variable w_m is random with $P(w_m = 1) = P(w_m = -1) = 0.5$, conditional on the observed data. Hence, $E(D') = 0$ and $V(D') = E(D'^2) = \frac{1}{M^2}\sum_{m=1}^{M} E\{w_m^2 s_m^2 (y_{m1} - y_{m2})^2\} = \frac{1}{M^2}\sum_{m=1}^{M}(y_{m1} - y_{m2})^2$. By applying the central limit theorem to w_m's, the exact p-value of D can be approximated by

$$\begin{aligned} P(D' \geq D) &= P\left\{ \frac{D'}{\{\sum_{m=1}^{M}(y_{m1} - y_{m2})^2/M^2\}^{1/2}} \geq \frac{D}{\{\sum_{m=1}^{M}(y_{m1} - y_{m2})^2/M^2\}^{1/2}} \right\} \\ &\simeq P\left\{ N(0,1) \geq \frac{D}{\{\sum_{m=1}^{M}(y_{m1} - y_{m2})^2/M^2\}^{1/2}} \right\}. \end{aligned}$$

We can obtain the CI for the mean effect by inverting the above test procedure. For instance, suppose that the treatment effect is β_m for pair m. Define the mean effect $\bar{\beta} \equiv \frac{1}{M}\sum_{m=1}^{M}\beta_m$. In this case, the no-effect situation is restored by replacing y_{m1} with $y_{m1} - \beta_m$ when $s_m = 1$ or y_{m2} with $y_{m2} - \beta_m$ when $s_m = -1$:

$$\begin{aligned} D_{\bar{\beta}} \equiv \frac{1}{M}\sum_{m=1}^{M} s_m(y_{m1} - s_m\beta_m - y_{m2}) &= \frac{1}{M}\sum_{m=1}^{M} s_m(y_{m1} - y_{m2}) - \frac{1}{M}\sum_{m=1}^{M}\beta_m \\ &= \frac{1}{M}\sum_{m=1}^{M} s_m(y_{m1} - y_{m2}) - \bar{\beta}, \end{aligned}$$

and the permutation test can be applied. Define accordingly $D'_{\bar{\beta}} \equiv \frac{1}{M}\sum_{m=1}^{M} w_m[s_m(y_{m1}-y_{m2})-\bar{\beta}]$ to observe $E(D'_{\bar{\beta}}) = 0$ and $V(D'_{\bar{\beta}}) = \frac{1}{M^2}\sum_{m=1}^{M}[s_m(y_{m1} - y_{m2}) - \bar{\beta}]^2$. Now conduct level-$\alpha$ tests with

$$\frac{D_{\bar{\beta}}}{\{\sum_{m=1}^{M}[s_m(y_{m1} - y_{m2}) - \bar{\beta}]^2/M^2\}^{1/2}}. \tag{6}$$

The collection of $\bar{\beta}$ values that are not rejected using (6) is the $(1-\alpha)100\%$ CI for $\bar{\beta}$. In the above framework, *we have generalized the procedure to allow for heterogeneous treatment effects, and as such, the CI constructed is for the mean effect $\bar{\beta}$. Clearly, this framework includes homogeneous treatment effects as a special case when $\beta_m = \beta$ for all m.*

9. APPENDIX: SIGNED-RANK TEST FOR MATCHED PAIRS

The permuted version R' for R can be written as $R' \equiv \sum_{m=1}^{M} r_m 1[w_m s_m > 0] = \sum_{m=1}^{M} r_m(1[w_m = 1, s_m = 1] + 1[w_m = -1, s_m = -1])$. Note that r_m's and s_m's are fixed conditional on the data and the only thing random is the permutation variable w_m. Thus, under the H_0 of exchangeability,

$E(R') = \sum_{m=1}^{M} r_m/2 = M(M+1)/4$, and $V(R') = \sum_{m=1}^{M} r_m^2/4 = M(M+1)(2M+1)/24$. Hence, when M is large, the normally approximated p-value for R is

$$P\left\{N(0,1) \geq \frac{R \ - \ M(M+1)/4}{\{M(M+1)(2M+1)/24\}^{1/2}}\right\}.$$

Under the assumption of homogeneous treatment effects, the CI for the effect can be obtained by inverting the signed-rank test procedure. Conduct level-α tests with different values of β using

$$\frac{R_\beta \ - \ M(M+1)/4}{\{M(M+1)(2M+1)/24\}^{1/2}}, \quad \text{where} \quad R_\beta \equiv \sum_{m=1}^{M} r_{m\beta} 1[s_m(y_{m1} - s_m\beta - y_{m2}) > 0] \tag{7}$$

and $r_{m\beta}$ is the rank of $|y_{m1} - s_m\beta - y_{m2}|$, $m = 1, ..., M$. The collection of β values that are not rejected is the $(1-\alpha)100\%$ CI for β. To obtain a point estimate of the treatment effect, we can use the Hodges and Lehmann (1963) estimator, which is the solution of β such that

$$R_\beta = \frac{M(M+1)}{4} \ \{= E(R')\}. \tag{8}$$

Note that when treatment effects are heterogeneous, the pair-wise effect β_m (instead of β) should be subtracted from each pair-wise difference in (7), but in R_β we cannot pull out the pair-wise effects β_m, $m = 1, 2, \ldots, M$, and summarize them by a single number as in $D_{\bar{\beta}}$. Thus, one cannot generalize (7) and (8) to the case of heterogeneous treatment effects.

10. APPENDIX: SENSITIVITY ANALYSIS

Given $p^+ \equiv \frac{\Gamma}{1+\Gamma} \geq 0.5$ and $p^- \equiv \frac{1}{1+\Gamma} \leq 0.5$, define R^+ (R^-) as the sum of M-many independent random variables where the mth variable takes the value r_m with probability p^+ (p^-) and 0 with probability $1-p^+$ ($1-p^-$). Writing R^+ as $\sum_{m=1}^{M} r_m u_m$, where $P(u_m = 1) = p^+$ and $P(u_m = 0) = 1 - p^+$, we get

$$\begin{aligned} E(R^+) &= \sum_{m=1}^{M} r_m E(u_m) = p^+ \sum_{m=1}^{M} r_m = \frac{p^+ M(M+1)}{2}, \\ V(R^+) &= \sum_{m=1}^{M} r_m^2 V(u_m) = p^+(1-p^+) \sum_{m=1}^{M} r_m^2 = \frac{p^+(1-p^+)M(M+1)(2M+1)}{6}. \end{aligned}$$

Doing analogously, we obtain

$$E(R^-) = \frac{p^- M(M+1)}{2} \quad \text{and} \quad V(R^-) = \frac{p^-(1-p^-)M(M+1)(2M+1)}{6}.$$

It follows from Rosenbaum (2002, Proposition 13) that $P(R^+ \geq a) \geq P(R' \geq a) \geq P(R^- \geq a)$ for arbitrary a.

For treatment effect analysis with matching, various sensitivity analyses have appeared in the

statistics literature as reviewed in Rosenbaum (2002), but not many in econometrics. Those that have appeared in the econometrics literature include the parametric/structural regression approach of Imbens (2003) and Altonji et al. (2005). This approach allows for an unobserved confounder to affect both treatment and response, but is heavily dependent on the parametric assumptions about the structural equations of treatment and response.

Ichino et al. (2008) suggested an alternative, simulation-based, approach of sensitivity analysis for matching estimators. This approach also allows for an unobserved confounder to affect both treatment and response, but without relying on any parametric/structural model for the treatment and response. The unobserved confounder is simulated and included in the list of matching covariates to evaluate the sensitivity of point effect estimates. This is feasible only for binary unobserved confounders in the context of binary treatment/response variables, so that the distribution of the unobserved confounder can be characterized by four probability parameters conditional on the treatment/response outcomes.

Gastwirth et al. (1998) extended the Rosenbaum (2002) approach by allowing the unobserved confounder to affect both treatment and response. The approach of Gastwirth et al. (1998) is, however, parametric/structural; it specifies exactly how the unobserved confounder appears in the treatment and response equation. For instance, in the case where both the treatment and response variables are binary, the logit form is obtained, which may not look so objectionable; in other cases, the parametric specification becomes too restrictive. In a sense, the benefit of considering how the unobserved confounder affects the response is obtained at this parametrization cost. Refer to Lee et al. (2007) and Lee and Lee (2009) for applications of this approach. Since a hidden bias results from unobserved confounders affecting both treatment and response, the Rosenbaum (2002) analysis is conservative in the sense that it may be concerned with a hidden bias that does not exist at all if the unobserved confounder does not affect the response. Thus, if we find a result to be robust using the Rosenbaum (2002) approach, its robustness using the Gastwirth et al. (1998) approach is implied. Refer also to Lee (2004) for a nonparametric reduced-form sensitivity analysis.

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Table 1: Rose (2004) data set – descriptive statistics

variables	Both in				One in				None in (control group)			
	mean	SD	25%	75%	mean	SD	25%	75%	mean	SD	25%	75%
ltrade	10.472	3.415	8.344	12.815	9.759	3.253	8.013	11.937	9.246	2.964	8.062	11.124
ldist	8.198	0.797	7.843	8.745	8.188	0.772	7.751	8.749	7.873	0.972	7.216	8.685
lrgdp	48.404	2.681	46.615	50.218	47.582	2.526	45.930	49.265	46.432	2.582	44.968	48.068
lrgdppc	16.234	1.579	15.242	17.358	15.940	1.394	15.036	16.902	15.386	1.344	14.508	16.249
comlang	0.238	0.426	0	0	0.187	0.390	0	0	0.304	0.460	0	1
border	0.027	0.162	0	0	0.026	0.160	0	0	0.072	0.258	0	0
landl	0.251	0.471	0	0	0.241	0.461	0	0	0.246	0.467	0	0
island	0.364	0.548	0	1	0.331	0.535	0	1	0.264	0.503	0	0
lareap	24.145	3.230	22.445	26.314	24.270	3.293	22.466	26.588	24.238	3.480	22.362	26.739
comcol	0.105	0.307	0	0	0.089	0.285	0	0	0.124	0.330	0	0
curcol	0.004	0.062	0	0	0	0	0	0	0.000	0.017	0	0
colony	0.027	0.162	0	0	0.016	0.126	0	0	0.008	0.092	0	0
comctry	0.001	0.024	0	0	0	0	0	0	0	0	0	0
custrict	0.019	0.136	0	0	0.009	0.093	0	0	0.014	0.117	0	0
regional	0.018	0.134	0	0	0.009	0.096	0	0	0.019	0.138	0	0
gsp	0.299	0.458	0	1	0.201	0.400	0	0	0.008	0.090	0	0
year	1984.1	11.5	1976	1994	1978.9	12.4	1970	1989	1973.6	12.7	1963	1983
obs.	114,750				98,810				21,037			

Table 2: Rose (2004) data set – selection on observables

variables	Both in				One in			
	odds	p-value	95% CI		odds	p-value	95% CI	
ldist	1.174	0.000	1.147	1.202	1.230	0.000	1.203	1.257
lrgdp	1.538	0.000	1.521	1.555	1.222	0.000	1.209	1.235
lrgdppc	0.892	0.000	0.878	0.906	0.999	0.907	0.984	1.014
comlang	0.767	0.000	0.735	0.801	0.714	0.000	0.686	0.743
border	0.870	0.002	0.795	0.951	0.848	0.000	0.783	0.918
landl	1.187	0.000	1.142	1.233	1.072	0.000	1.034	1.112
island	1.872	0.000	1.793	1.955	1.448	0.000	1.391	1.508
lareap	0.875	0.000	0.867	0.882	0.947	0.000	0.940	0.955
comcol	1.645	0.000	1.546	1.750	1.293	0.000	1.223	1.368
curcol	12.385	0.000	5.320	28.834	1.678	0.000	1.417	1.988
colony	2.126	0.000	1.775	2.547	—	—	—	—
comctry	—	—	—	—	—	—	—	—
custrict	6.961	0.000	6.031	8.034	1.705	0.000	1.467	1.981
regional	0.762	0.000	0.666	0.873	0.879	0.070	0.764	1.011
gsp	27.698	0.000	23.750	32.303	19.230	0.000	16.487	22.428
obs.	135,720				119,841			

Note: The results are based on logistic regressions with $nonein = 1$ observations as the control group. The odds estimates are equal to exponential transformation of coefficient estimates in logit regressions. All regressions include year dummies. In the both-in regression, *comctry* is dropped as $comctry = 1$ predicts $bothin = 1$ perfectly. In the one-in regression, *curcol* is dropped as $curcol = 1$ predicts $onein = 0$ perfectly and *comctry* is dropped because of collinearity.

Figure 1: Support of covariates for treatment and control group

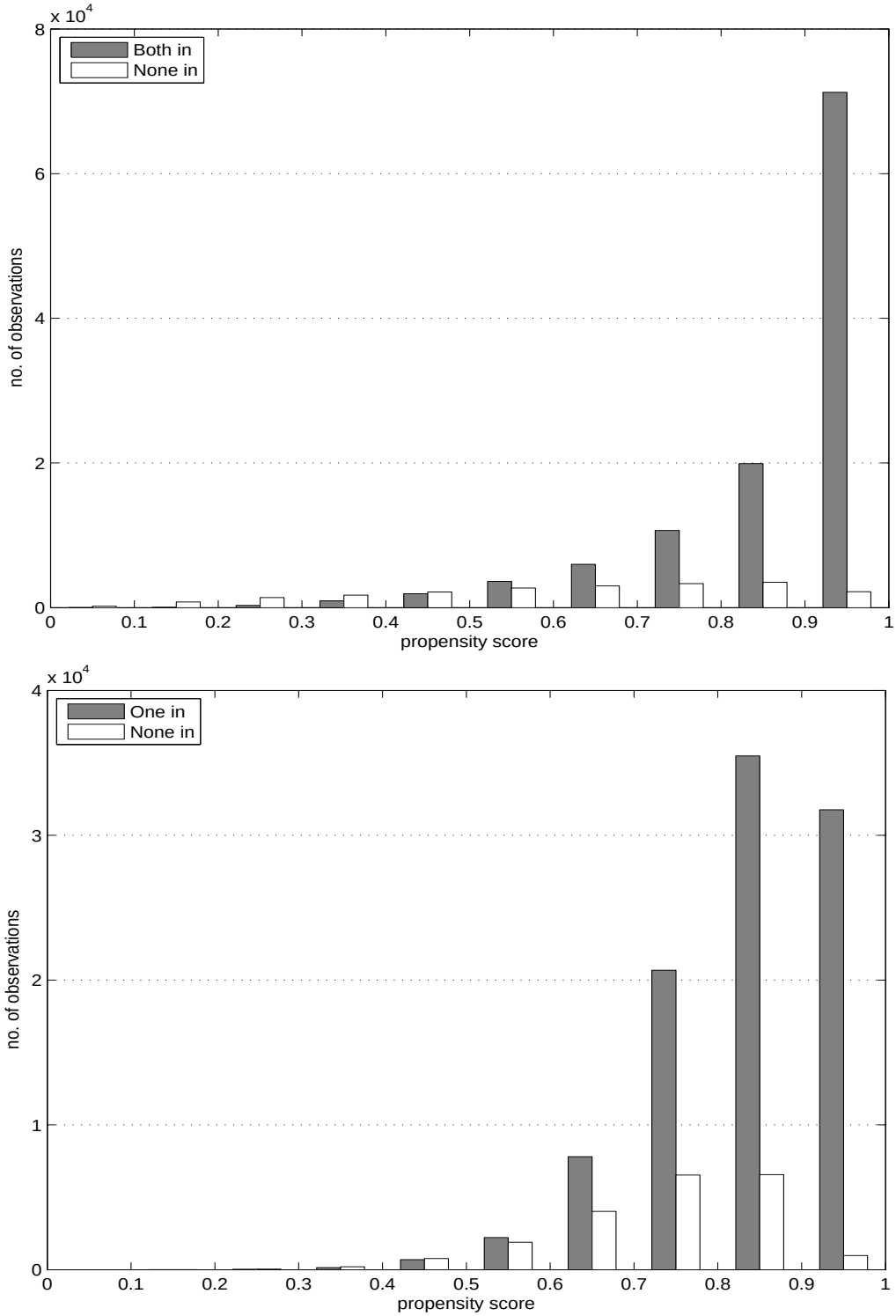


Table 3: Rose (2004) data set – unrestricted matching

	permutation test			signed-rank test			sensitivity analysis			
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	one-sided test		two-sided test	
caliper	effect	p-value	95% CI	effect	p-value	95% CI	Γ^*	as in	Γ^*	as in
Both in GATT/WTO treatment effect										
on the treated ($M_1 = 114,750$)**:**										
100%	1.328	0.000	[1.307, 1.349]	1.332	0.000	[1.312, 1.351]	2.434	R^+	2.428	R^+
80%	1.075	0.000	[1.052, 1.098]	1.075	0.000	[1.053, 1.096]	2.086	R^+	2.081	R^+
60%	0.836	0.000	[0.810, 0.862]	0.835	0.000	[0.810, 0.859]	1.780	R^+	1.775	R^+
40%	0.553	0.000	[0.522, 0.584]	0.535	0.000	[0.507, 0.563]	1.472	R^+	1.467	R^+
on the untreated ($M_0 = 21,037$)**:**										
100%	0.337	0.000	[0.296, 0.379]	0.303	0.000	[0.266, 0.342]	1.250	R^+	1.243	R^+
80%	0.239	0.000	[0.192, 0.286]	0.200	0.000	[0.157, 0.241]	1.144	R^+	1.138	R^+
60%	0.185	0.000	[0.131, 0.239]	0.138	0.000	[0.090, 0.187]	1.084	R^+	1.077	R^+
40%	0.304	0.000	[0.239, 0.368]	0.243	0.000	[0.184, 0.301]	1.177	R^+	1.167	R^+
on all ($M_1 + M_0 = 135,787$)**:**										
100%	1.175	0.000	[1.156, 1.193]	1.161	0.000	[1.143, 1.179]	2.209	R^+	2.205	R^+
80%	0.899	0.000	[0.878, 0.919]	0.883	0.000	[0.863, 0.902]	1.858	R^+	1.854	R^+
60%	0.636	0.000	[0.613, 0.659]	0.619	0.000	[0.597, 0.640]	1.559	R^+	1.555	R^+
40%	0.428	0.000	[0.400, 0.455]	0.399	0.000	[0.374, 0.424]	1.342	R^+	1.338	R^+
One in GATT/WTO treatment effect										
on the treated ($M_1 = 98,810$)**:**										
100%	0.767	0.000	[0.746, 0.789]	0.773	0.000	[0.753, 0.792]	1.759	R^+	1.755	R^+
80%	0.564	0.000	[0.540, 0.588]	0.568	0.000	[0.547, 0.589]	1.525	R^+	1.521	R^+
60%	0.422	0.000	[0.396, 0.449]	0.428	0.000	[0.405, 0.451]	1.397	R^+	1.393	R^+
40%	0.326	0.000	[0.296, 0.357]	0.325	0.000	[0.298, 0.351]	1.294	R^+	1.289	R^+
on the untreated ($M_0 = 21,037$)**:**										
100%	0.030	0.068	[-0.009, 0.069]	0.034	0.022	[0.000, 0.068]	1.006	R^+	1.001	R^+
80%	0.092	0.000	[0.048, 0.135]	0.089	0.000	[0.052, 0.126]	1.057	R^+	1.051	R^+
60%	0.078	0.001	[0.028, 0.129]	0.084	0.000	[0.041, 0.127]	1.046	R^+	1.039	R^+
40%	0.138	0.000	[0.076, 0.201]	0.149	0.000	[0.096, 0.203]	1.102	R^+	1.094	R^+
on all ($M_1 + M_0 = 119,847$)**:**										
100%	0.638	0.000	[0.619, 0.657]	0.632	0.000	[0.615, 0.649]	1.610	R^+	1.607	R^+
80%	0.443	0.000	[0.422, 0.464]	0.437	0.000	[0.418, 0.455]	1.401	R^+	1.397	R^+
60%	0.324	0.000	[0.301, 0.347]	0.321	0.000	[0.301, 0.340]	1.297	R^+	1.293	R^+
40%	0.225	0.000	[0.198, 0.253]	0.220	0.000	[0.197, 0.243]	1.194	R^+	1.190	R^+
GSP treatment effect										
on the treated ($M_1 = 54,285$)**:**										
100%	0.851	0.000	[0.831, 0.871]	0.792	0.000	[0.774, 0.811]	2.277	R^+	2.269	R^+
80%	0.757	0.000	[0.736, 0.778]	0.696	0.000	[0.676, 0.716]	2.125	R^+	2.117	R^+
60%	0.693	0.000	[0.668, 0.717]	0.627	0.000	[0.604, 0.649]	1.998	R^+	1.990	R^+
40%	0.665	0.000	[0.635, 0.696]	0.581	0.000	[0.553, 0.608]	1.879	R^+	1.869	R^+

Note:

1. The pool of potential matches for an observation are restricted to observations with the opposite treatment status; no further restriction is imposed. The number of matched pairs for the effect on the treated (untreated) is indicated by M_1 (M_0).
2. The caliper is set such that only the best 100%, 80%, 60%, or 40% of matched pairs obtained are included in the analysis. For example, with the caliper choice of 60%, the matched pairs with the quadratic distance exceeding the upper 60 percentile of all matched pairs obtained are discarded.
3. In 'permutation test', the results are based on the D-statistic.
4. In 'signed-rank test', the results are based on the R-statistic.
5. We carried out both simulation and normal approximation approaches for calculating the p-values and the CI's, and found almost identical results (which is expected given that the sample size is large). Thus, we report only the results based on normal approximation.
6. In 'sensitivity analysis', the sensitivity analysis is conducted for the significance (p-value) of the signed-rank R-statistic based on the critical level $\alpha = 0.05$ in a one-sided or two-sided test. R^+ or R^- (as a function of the odds ratio Γ) indicates the relevant distribution in calculating the critical bound Γ^* at which the conclusion of the signed-rank test reverses.

Table 4: Rose (2004) data set – means of covariates for removed dyads with tighter calipers

covariates	treated subjects					untreated subjects			
	$[c_{100t}, c_{80t}]$	$[c_{80t}, c_{60t}]$	$[c_{60t}, c_{40t}]$	$[c_{40t}, c_{0t}]$		$[c_{100c}, c_{80c}]$	$[c_{80c}, c_{60c}]$	$[c_{60c}, c_{40c}]$	$[c_{40c}, c_{0c}]$
	Both in				vs.	**None in**			
ldist	8.056	8.257	8.138	8.269		7.121	7.769	7.882	8.298
lrgdp	49.030	48.648	48.361	47.990		44.489	45.946	46.714	47.505
lrgdppc	17.010	16.519	16.351	15.645		15.120	15.536	15.417	15.429
comlang	0.501	0.246	0.249	0.097		0.557	0.428	0.295	0.120
border	0.076	0.021	0.019	0.009		0.212	0.082	0.047	0.009
landl	0.352	0.314	0.236	0.176		0.370	0.307	0.264	0.144
island	0.547	0.469	0.361	0.222		0.489	0.301	0.200	0.165
lareap	23.548	24.028	23.720	24.714		22.150	23.678	24.665	25.348
comcol	0.187	0.130	0.148	0.031		0.354	0.147	0.076	0.022
curcol	0.019	0.000	0.000	0.000		0.000	0.001	0.000	0.000
colony	0.129	0.002	0.001	0.001		0.018	0.015	0.006	0.001
comctry	0.003	0.000	0.000	0.000		0.000	0.000	0.000	0.000
custrict	0.084	0.003	0.003	0.001		0.063	0.001	0.001	0.002
regional	0.085	0.004	0.002	0.000		0.087	0.008	0.002	0.000
gsp	0.576	0.573	0.186	0.080		0.000	0.001	0.009	0.015
year	1987.8	1984.8	1984.2	1981.8		1969.1	1972.8	1974.4	1975.8
	One in				vs.	**None in**			
ldist	8.192	8.113	8.120	8.258		7.245	7.677	7.929	8.258
lrgdp	47.894	47.301	47.498	47.609		44.660	46.049	46.752	47.349
lrgdppc	16.721	16.052	15.858	15.534		15.210	15.430	15.443	15.424
comlang	0.294	0.268	0.163	0.106		0.556	0.420	0.268	0.138
border	0.052	0.042	0.016	0.011		0.222	0.086	0.023	0.014
landl	0.327	0.305	0.214	0.179		0.367	0.294	0.242	0.163
island	0.517	0.510	0.311	0.159		0.482	0.317	0.223	0.149
lareap	23.717	23.480	24.168	24.993		22.511	23.851	24.599	25.114
comcol	0.129	0.195	0.067	0.027		0.387	0.115	0.064	0.027
curcol	0.000	0.000	0.000	0.000		0.001	0.000	0.000	0.000
colony	0.074	0.003	0.002	0.001		0.027	0.012	0.000	0.001
comctry	0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000
custrict	0.038	0.002	0.001	0.001		0.054	0.004	0.008	0.002
regional	0.030	0.008	0.002	0.003		0.047	0.032	0.011	0.004
gsp	0.712	0.139	0.088	0.032		0.000	0.002	0.002	0.018
year	1981.9	1980.8	1979.1	1976.3		1973.2	1972.2	1973.2	1974.6

Note: The symbol $c_{\#t}$ denotes the #% caliper of the treated subjects, and $c_{\#c}$ the #% caliper of the untreated subjects. For example, in the column $[c_{100t}, c_{80t}]$, *ldist* has mean 8.056, which is the mean for the dyads that are removed when the caliper tightens from 100% to 80% for the effect on the treated. In the column $[c_{100c}, c_{80c}]$, the same interpretation holds except that the estimated effect is the effect on the untreated. Parallel interpretations apply to the other specified ranges.

Table 5: Rose (2004) data set – restricted matching effect estimates and sensitivity

	within dyad		within year		within period		within devel.	
caliper	effect	Γ^*	effect	Γ^*	effect	Γ^*	effect	Γ^*
Both in GATT/WTO treatment effect								
on the treated:								
M_1	19,760		114,750		114,750		112,959	
100%	0.941***	3.170	1.329***	2.427	1.331***	2.432	1.124***	2.019
80%	0.760***	2.543	1.075***	2.081	1.075***	2.081	0.778***	1.601
60%	0.833***	2.771	0.836***	1.775	0.836***	1.775	0.541***	1.385
40%	0.796***	2.503	0.553***	1.467	0.553***	1.467	0.393***	1.256
on the untreated:								
M_0	9,510		21,037		21,037		21,013	
100%	1.300***	4.129	0.340***	1.245	0.340***	1.245	0.309***	1.216
80%	1.117***	3.440	0.239***	1.138	0.239***	1.138	0.175***	1.077
60%	0.989***	2.983	0.185***	1.077	0.185***	1.077	0.101***	1.009
40%	0.847***	2.600	0.304***	1.167	0.304***	1.167	0.077**,[b]	1.019$^-$
on all:								
$M_1 + M_0$	29,270		135,787		135,787		133,972	
100%	1.058***	3.496	1.176***	2.205	1.177***	2.208	0.997***	1.880
80%	0.895***	2.911	0.899***	1.854	0.899***	1.854	0.662***	1.504
60%	0.935***	3.002	0.636***	1.555	0.636***	1.555	0.442***	1.309
40%	0.873***	2.679	0.428***	1.338	0.428***	1.338	0.247***	1.143
One in GATT/WTO treatment effect								
on the treated:								
M_1	23,463		98,810		98,810		98,363	
100%	0.464***	1.931	0.761***	1.747	0.762***	1.749	0.650***	1.552
80%	0.403***	1.772	0.564***	1.521	0.564***	1.521	0.476***	1.391
60%	0.371***	1.656	0.422***	1.393	0.422***	1.393	0.342***	1.263
40%	0.314***	1.508	0.326***	1.289	0.326***	1.289	0.242***	1.197
on the untreated:								
M_0	15,182		21,037		21,037		21,013	
100%	0.579***	2.097	0.032[a]	1.004	0.032[a]	1.004	0.049**	1.001
80%	0.463***	1.805	0.092***	1.051	0.092***	1.051	0.063***	1.014
60%	0.386***	1.597	0.078***	1.039	0.078***	1.039	0.034	1.013$^-$
40%	0.317***	1.465	0.138***	1.094	0.138***	1.094	0.062**,[c]	1.004$^-$
on all:								
$M_1 + M_0$	38,645		119,847		119,847		119,376	
100%	0.509***	2.016	0.633***	1.601	0.634***	1.603	0.544***	1.452
80%	0.428***	1.808	0.443***	1.397	0.443***	1.397	0.391***	1.316
60%	0.403***	1.698	0.324***	1.293	0.324***	1.293	0.215***	1.161
40%	0.291***	1.460	0.225***	1.190	0.225***	1.190	0.175***	1.137
GSP treatment effect								
on the treated:								
M_1	52,025		54,285		54,285		53,811	
100%	0.487***	2.570	0.850***	2.267	0.851***	2.269	0.732***	2.011
80%	0.492***	2.494	0.757***	2.117	0.757***	2.117	0.588***	1.807
60%	0.379***	1.937	0.693***	1.990	0.693***	1.990	0.507***	1.699
40%	0.271***	1.528	0.665***	1.869	0.665***	1.869	0.410***	1.530

Note:
The effect estimate refers to the D-statistic. All significance levels refer to a two-sided test. The effect estimate is significant at the 1%, 5%, or 10% significance level if indicated by a superscript of ***, **, or *, respectively. The sensitivity parameter Γ^* is based on a two-sided test at the 5% significance level. The distribution used in calculating the critical bound Γ^* is R^+ unless a superscript $^-$ is indicated following the bound Γ^*, in which case, R^- is used. Other than those indicated below, the significance level of the D-statistic agrees with that of the R-statistic.
a. The D-statistic is not significant at the 10% level, but the R-statistic is significant at the 5% level.
b. The D-statistic is significant at the 5% level, but the R-statistic is not significant.
c. The D-statistic is significant at the 5% level, but the R-statistic is only significant at the 10% level.

Table 6: Tomz et al. (2007) data set – matching effect estimates and sensitivity

	unrestricted		within dyad		within year		within period		within devel.	
caliper	effect	Γ^*	effect	Γ^*	effect	Γ^*	effect	Γ^*	effect	Γ^*
Both participating in GATT/WTO treatment effect										
on the treated:										
M_1	152,986		8,005		152,986		152,986		152,986	
100%	1.418***	2.426	1.554***	7.535	1.427***	2.439	1.426***	2.438	1.065***	2.099
80%	1.260***	2.284	1.513***	6.689	1.260***	2.284	1.260***	2.284	0.710***	1.626
60%	1.089***	2.058	1.285***	4.969	1.089***	2.058	1.089***	2.058	0.515***	1.382
40%	0.762***	1.706	1.361***	5.134	0.762***	1.706	0.762***	1.706	0.461***	1.324
on the untreated:										
M_0	9,703		4,144		9,703		9,703		9,703	
100%	0.396***	1.341	1.743***	8.186	0.396***	1.341	0.396***	1.341	0.164***	1.092
80%	0.343***	1.280	1.561***	6.518	0.343***	1.280	0.343***	1.280	0.169***	1.093
60%	0.361***	1.275	1.334***	4.927	0.361***	1.275	0.361***	1.275	0.168***	1.079
40%	0.404***	1.301	1.060***	3.527	0.404***	1.301	0.404***	1.301	0.200***	1.075
on all:										
$M_1 + M_0$	162,689		12,149		162,689		162,689		162,689	
100%	1.357***	2.351	1.618***	7.950	1.365***	2.363	1.365***	2.362	1.012***	2.028
80%	1.184***	2.189	1.536***	6.684	1.184***	2.189	1.184***	2.189	0.656***	1.571
60%	1.002***	1.956	1.417***	5.872	1.002***	1.956	1.002***	1.956	0.467***	1.341
40%	0.666***	1.618	1.315***	4.992	0.666***	1.618	0.666***	1.618	0.402***	1.284
One participating in GATT/WTO treatment effect										
on the treated:										
M_1	71,908		11,637		71,908		71,908		71,908	
100%	0.818***	1.777	0.852***	2.877	0.822***	1.782	0.820***	1.778	0.464***	1.457
80%	0.631***	1.580	0.716***	2.393	0.631***	1.580	0.631***	1.580	0.278***	1.244
60%	0.444***	1.423	0.738***	2.279	0.444***	1.423	0.444***	1.423	0.290***	1.241
40%	0.304***	1.295	0.546***	1.840	0.304***	1.295	0.304***	1.295	0.172***	1.154
on the untreated:										
M_0	9,703		6,548		9,703		9,703		9,703	
100%	-0.040	1.033^-	0.754***	2.384	-0.040	1.033^-	-0.040	1.033^-	0.089***	1.044
80%	0.006	1.025^-	0.633***	1.993	0.006	1.025^-	0.006	1.025^-	0.099***	1.032
60%	0.028^a	1.005^-	0.473***	1.604	0.028^a	1.005^-	0.028^a	1.005^-	0.028	1.040^-
40%	0.135***	1.097	0.396***	1.456	0.135***	1.097	0.135***	1.097	0.103**	1.010
on all:										
$M_1 + M_0$	81,611		18,185		81,611		81,611		81,611	
100%	0.716***	1.668	0.817***	2.725	0.720***	1.672	0.718***	1.669	0.420***	1.412
80%	0.523***	1.484	0.719***	2.370	0.523***	1.484	0.523***	1.484	0.242***	1.213
60%	0.349***	1.335	0.643***	2.059	0.349***	1.335	0.349***	1.335	0.238***	1.209
40%	0.206***	1.192	0.411***	1.565	0.206***	1.192	0.206***	1.192	0.172***	1.152
GSP treatment effect										
on the treated:										
M_1	54,285		52,025		54,285		54,285		54,285	
100%	0.824***	2.243	0.485***	2.561	0.823***	2.241	0.824***	2.243	0.688***	1.959
80%	0.726***	2.065	0.480***	2.407	0.726***	2.065	0.726***	2.065	0.569***	1.786
60%	0.667***	1.944	0.375***	1.893	0.667***	1.944	0.667***	1.944	0.489***	1.679
40%	0.621***	1.782	0.265***	1.494	0.621***	1.782	0.621***	1.782	0.401***	1.510

Note: The general notes for Table 5 apply to the current table.

a. The D-statistic is not significant at the 10% level, but the R-statistic is significant at the 10% level.

Table 7: Rose (2004) data set – kernel-weighting matching effect estimates

caliper	unrestricted	within dyad	within year	within period	within devel.
Both in GATT/WTO treatment effect					
on the treated:					
100%	1.323	0.929	1.284	1.321	0.962
80%	1.078	0.764	1.076	1.079	0.778
60%	0.840	0.835	0.837	0.841	0.542
40%	0.558	0.799	0.554	0.559	0.396
on the untreated:					
100%	0.323	1.282	0.317	0.324	0.308
80%	0.249	1.105	0.243	0.250	0.179
60%	0.200	0.977	0.193	0.202	0.105
40%	0.303	0.835	0.300	0.304	0.081
on all:					
100%	1.168	1.046	1.131	1.166	0.856
80%	0.902	0.892	0.899	0.903	0.663
60%	0.641	0.932	0.637	0.642	0.444
40%	0.434	0.868	0.429	0.435	0.251
One in GATT/WTO treatment effect					
on the treated:					
100%	0.753	0.484	0.748	0.753	0.604
80%	0.573	0.423	0.571	0.574	0.483
60%	0.436	0.393	0.433	0.436	0.353
40%	0.344	0.342	0.341	0.344	0.260
on the untreated:					
100%	0.037	0.555	0.023	0.038	0.058
80%	0.101	0.439	0.093	0.103	0.068
60%	0.094	0.358	0.083	0.096	0.047
40%	0.131	0.285	0.124	0.133	0.065
on all:					
100%	0.626	0.512	0.619	0.626	0.505
80%	0.452	0.431	0.449	0.452	0.398
60%	0.337	0.404	0.333	0.338	0.226
40%	0.243	0.295	0.238	0.244	0.188
GSP treatment effect					
on the treated:					
100%	0.874	0.491	0.863	0.875	0.744
80%	0.786	0.497	0.773	0.788	0.605
60%	0.731	0.384	0.712	0.733	0.544
40%	0.709	0.277	0.688	0.711	0.456

Table 8: Rose (2004) data set – trading relationship exists before GATT/WTO membership

	unrestricted		within dyad		within year		within period		within devel.	
caliper	effect	Γ^*	effect	Γ^*	effect	Γ^*	effect	Γ^*	effect	Γ^*
Both in GATT/WTO treatment effect										
on the treated:										
M_1	19,760		19,760		19,760		19,760		19,522	
100%	1.599***	2.983	1.032***	3.372	1.606***	2.983	1.607***	2.989	1.302***	2.364
80%	1.447***	2.660	0.836***	2.726	1.447***	2.660	1.447***	2.660	1.157***	2.086
60%	1.149***	2.195	0.886***	2.885	1.149***	2.195	1.149***	2.195	0.909***	1.771
40%	0.861***	1.817	0.821***	2.586	0.861***	1.817	0.861***	1.817	0.639***	1.469
on the untreated:										
M_0	21,037		9,510		20,700		21,027		21,013	
100%	0.891***	1.891	1.220***	3.746	0.922***	1.943	0.948***	1.990	0.604***	1.453
80%	0.791***	1.758	1.037***	3.075	0.788***	1.752	0.792***	1.758	0.617***	1.466
60%	0.675***	1.611	0.912***	2.684	0.668***	1.598	0.675***	1.611	0.523***	1.364
40%	0.639***	1.526	0.754***	2.280	0.637***	1.520	0.639***	1.526	0.446***	1.295
on all:										
$M_1 + M_0$	40,797		29,270		40,460		40,787		40,535	
100%	1.234***	2.387	1.093***	3.531	1.256***	2.426	1.267***	2.454	0.940***	1.859
80%	1.048***	2.092	0.938***	2.991	1.041***	2.080	1.048***	2.092	0.825***	1.703
60%	0.846***	1.834	0.919***	2.899	0.843***	1.828	0.846***	1.834	0.625***	1.479
40%	0.705***	1.636	0.848***	2.574	0.705***	1.635	0.705***	1.636	0.503***	1.371
One in GATT/WTO treatment effect										
on the treated:										
M_1	23,463		23,463		23,463		23,463		23,384	
100%	0.986***	2.060	0.469***	1.935	0.985***	2.058	0.985***	2.059	0.903***	1.879
80%	0.758***	1.743	0.392***	1.753	0.758***	1.743	0.758***	1.743	0.691***	1.609
60%	0.615***	1.590	0.351***	1.653	0.615***	1.590	0.615***	1.590	0.492***	1.384
40%	0.535***	1.491	0.354***	1.621	0.535***	1.491	0.535***	1.491	0.415***	1.320
on the untreated:										
M_0	21,037		15,182		21,027		21,027		21,013	
100%	0.457***	1.378	0.562***	2.054	0.463***	1.389	0.461***	1.385	0.311***	1.205
80%	0.523***	1.479	0.459***	1.788	0.523***	1.479	0.523***	1.479	0.415***	1.311
60%	0.438***	1.397	0.478***	1.856	0.438***	1.398	0.438***	1.398	0.347***	1.246
40%	0.473***	1.450	0.395***	1.651	0.474***	1.451	0.474***	1.451	0.380***	1.314
on all:										
$M_1 + M_0$	44,500		38,645		44,490		44,490		44,397	
100%	0.736***	1.726	0.505***	2.001	0.738***	1.731	0.738***	1.729	0.623***	1.543
80%	0.623***	1.612	0.419***	1.788	0.623***	1.611	0.623***	1.611	0.552***	1.471
60%	0.522***	1.505	0.407***	1.761	0.521***	1.504	0.521***	1.504	0.411***	1.317
40%	0.493***	1.478	0.387***	1.691	0.493***	1.478	0.493***	1.478	0.399***	1.334

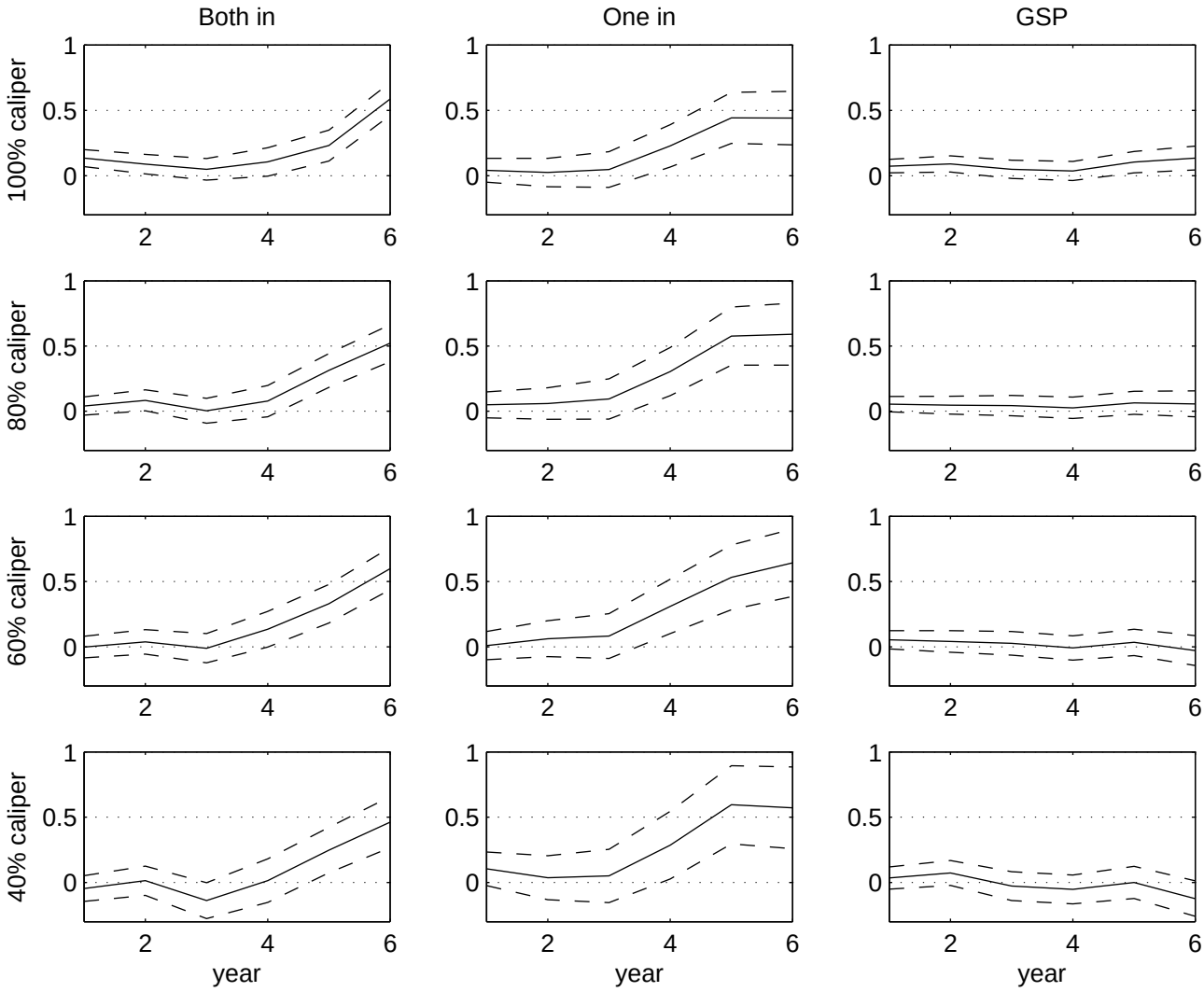
Note: The general notes for Table 5 apply to the current table.

Table 9: Rose (2004) data set – with multilateral resistance terms

	unrestricted		within dyad		within year		within period		within devel.	
caliper	effect	Γ^*	effect	Γ^*	effect	Γ^*	effect	Γ^*	effect	Γ^*
Both in GATT/WTO treatment effect										
on the treated:										
M_1	114,750		19,760		114,750		114,750		112,959	
100%	1.622***	2.616	0.942***	3.170	1.618***	2.605	1.620***	2.609	1.243***	2.041
80%	1.355***	2.273	0.778***	2.594	1.355***	2.273	1.355***	2.273	0.750***	1.543
60%	1.130***	2.023	0.850***	2.858	1.130***	2.023	1.130***	2.023	0.659***	1.452
40%	0.894***	1.798	0.845***	2.624	0.894***	1.798	0.894***	1.798	0.569***	1.375
on the untreated:										
M_0	21,037		9,510		21,037		21,037		21,013	
100%	0.838***	1.850	1.276***	4.089	0.851***	1.866	0.842***	1.854	0.541***	1.454
80%	0.773***	1.749	1.112***	3.457	0.773***	1.749	0.773***	1.749	0.545***	1.436
60%	0.695***	1.696	0.985***	2.988	0.695***	1.696	0.695***	1.696	0.472***	1.357
40%	0.638***	1.660	0.867***	2.637	0.638***	1.660	0.638***	1.660	0.408***	1.317
on all:										
$M_1 + M_0$	135,787		29,270		135,787		135,787		133,972	
100%	1.500***	2.496	1.051***	3.484	1.499***	2.490	1.499***	2.491	1.133***	1.949
80%	1.236***	2.170	0.897***	2.925	1.236***	2.170	1.236***	2.170	0.694***	1.514
60%	1.002***	1.919	0.949***	3.083	1.002***	1.919	1.002***	1.919	0.633***	1.456
40%	0.834***	1.785	0.891***	2.746	0.834***	1.785	0.834***	1.785	0.512***	1.360
One in GATT/WTO treatment effect										
on the treated:										
M_1	98,810		23,463		98,810		98,810		98,363	
100%	0.627***	1.560	0.454***	1.903	0.627***	1.560	0.627***	1.561	0.455***	1.385
80%	0.401***	1.368	0.399***	1.761	0.401***	1.368	0.401***	1.368	0.270***	1.230
60%	0.246***	1.242	0.371***	1.650	0.246***	1.242	0.246***	1.242	0.209***	1.194
40%	0.252***	1.267	0.374***	1.612	0.252***	1.267	0.252***	1.267	0.107***	1.124
on the untreated:										
M_0	21,037		15,182		21,037		21,037		21,013	
100%	0.191***	1.107	0.551***	2.004	0.202***	1.118	0.195***	1.110	0.129***	1.041
80%	0.154***	1.096	0.443***	1.745	0.154***	1.096	0.154***	1.096	0.105***	1.040
60%	0.061***	1.034	0.443***	1.674	0.061***	1.034	0.061***	1.034	0.006	1.037$^-$
40%	0.079***	1.061	0.323***	1.461	0.079***	1.061	0.079***	1.061	-0.034	1.024
on all:										
$M_1 + M_0$	119,847		38,645		119,847		119,847		119,376	
100%	0.550***	1.485	0.492***	1.962	0.552***	1.487	0.551***	1.486	0.397***	1.330
80%	0.331***	1.300	0.414***	1.770	0.331***	1.300	0.331***	1.300	0.247***	1.205
60%	0.232***	1.225	0.412***	1.709	0.232***	1.225	0.232***	1.225	0.186***	1.167
40%	0.208***	1.227	0.355***	1.554	0.208***	1.227	0.208***	1.227	0.072***	1.084
GSP treatment effect										
on the treated:										
M_1	54,285		52,025		54,285		54,285		53,811	
100%	1.044***	2.243	0.485***	2.559	1.043***	2.242	1.044***	2.244	0.954***	2.183
80%	1.060***	2.309	0.494***	2.624	1.060***	2.309	1.060***	2.309	0.948***	2.195
60%	0.954***	2.139	0.456***	2.261	0.954***	2.139	0.954***	2.139	0.762***	1.869
40%	0.872***	2.023	0.325***	1.679	0.872***	2.023	0.872***	2.023	0.712***	1.748

Note: The general notes for Table 5 apply to the current table.

Figure 2: Difference-in-Difference matching estimates



Note:
a. The x-axis indicates the years of lead and lag (a, b) used in the DD estimation; here, symmetric leads and lags are used. The y-axis (not labeled) indicates the treatment effect magnitude.
b. The solid line indicates the treatment effect point estimate. The dashed lines indicate the 95% CI based on the permutation test.
c. The sample size (the number of qualified matched pairs) for each treatment scenario is as follows. Both-in: 3600 (1 year), 3216 (2 years), 2955 (3 years), 2461 (4 years), 2277 (5 years), 1812 (6 years). One-in: 1303 (1 year), 1110 (2 years), 1022 (3 years), 828 (4 years), 736 (5 years), 651 (6 years). GSP: 2231 (1 year), 2184 (2 years), 2031 (3 years), 1976 (4 years), 1913 (5 years), 1859 (6 years). These correspond to the sample size used in the 100% caliper choice.

Table 10: Rose (2004) data set – placebo exercise

		years before the actual treatment year								
		12			11			10		
DD window (years)	caliper	effect	95% CI		effect	95% CI		effect	95% CI	
1	100%	0.040	[-0.107,	0.187]	-0.064	[-0.219,	0.090]	0.052	[-0.088,	0.192]
	80%	-0.003	[-0.171,	0.166]	-0.118	[-0.289,	0.053]	0.037	[-0.115,	0.189]
	60%	-0.025	[-0.224,	0.174]	-0.202	*[-0.390,*	*-0.013]*	0.026	[-0.156,	0.207]
	40%	-0.024	[-0.243,	0.195]	-0.264	*[-0.498,*	*-0.030]*	0.014	[-0.209,	0.238]
2	100%	-0.025	[-0.201,	0.152]	0.010	[-0.161,	0.180]	-0.006	[-0.180,	0.169]
	80%	-0.080	[-0.282,	0.123]	-0.088	[-0.283,	0.106]	-0.006	[-0.202,	0.191]
	60%	-0.038	[-0.276,	0.199]	-0.044	[-0.279,	0.191]	-0.061	[-0.290,	0.167]
	40%	-0.054	[-0.360,	0.253]	-0.050	[-0.337,	0.237]	-0.065	[-0.340,	0.209]
3	100%	0.092	[-0.139,	0.324]	0.145	[-0.057,	0.346]	0.010	[-0.175,	0.196]
	80%	0.196	[-0.070,	0.463]	0.181	[-0.052,	0.415]	-0.044	[-0.249,	0.160]
	60%	0.234	[-0.039,	0.507]	0.067	[-0.182,	0.317]	-0.065	[-0.294,	0.164]
	40%	0.061	[-0.261,	0.383]	0.146	[-0.148,	0.440]	-0.089	[-0.353,	0.175]
4	100%	0.012	[-0.198,	0.222]	-0.027	[-0.239,	0.185]	-0.019	[-0.223,	0.185]
	80%	0.024	[-0.208,	0.257]	0.057	[-0.196,	0.311]	0.039	[-0.190,	0.268]
	60%	-0.007	[-0.277,	0.263]	0.007	[-0.294,	0.308]	0.122	[-0.137,	0.382]
	40%	-0.007	[-0.363,	0.349]	-0.062	[-0.426,	0.301]	0.164	[-0.151,	0.479]
5	100%	-0.166	[-0.411,	0.079]	-0.213	[-0.438,	0.012]			
	80%	-0.116	[-0.391,	0.159]	-0.240	[-0.491,	0.011]			
	60%	-0.121	[-0.434,	0.193]	-0.304	*[-0.606,*	*-0.003]*			
	40%	-0.133	[-0.508,	0.243]	-0.304	[-0.658,	0.049]			
6	100%	-0.138	[-0.425,	0.149]						
	80%	-0.105	[-0.430,	0.220]						
	60%	-0.015	[-0.388,	0.357]						
	40%	-0.230	[-0.651,	0.191]						

		years before the actual treatment year								
		9			8			7		
		effect	95% CI		effect	95% CI		effect	95% CI	
1	100%	0.134	[-0.011,	0.278]	-0.039	[-0.172,	0.093]	0.106	[-0.031,	0.243]
	80%	0.090	[-0.065,	0.244]	-0.035	[-0.169,	0.100]	0.078	[-0.069,	0.225]
	60%	0.061	[-0.115,	0.238]	-0.002	[-0.164,	0.160]	0.036	[-0.135,	0.206]
	40%	0.116	[-0.091,	0.324]	-0.011	[-0.214,	0.192]	0.021	[-0.187,	0.230]
2	100%	-0.072	[-0.232,	0.089]	0.058	[-0.094,	0.209]			
	80%	-0.072	[-0.255,	0.111]	0.058	[-0.097,	0.214]			
	60%	-0.047	[-0.245,	0.150]	-0.004	[-0.174,	0.167]			
	40%	-0.009	[-0.264,	0.246]	-0.003	[-0.213,	0.206]			
3	100%	-0.104	[-0.280,	0.073]						
	80%	-0.077	[-0.271,	0.117]						
	60%	-0.044	[-0.263,	0.175]						
	40%	-0.084	[-0.359,	0.191]						

Note:
1. The estimation proceeds as described in Section 5.6 for DD estimation, but with a bogus treatment year $t' = t - d$ used, where $\{d = 7, \ldots, 12\}$, which predates the actual year of treatment t (here identified as the first year when either one country in a treated dyad joins the GATT/WTO).
2. The DD window refers to the years of lead and lag (a, b) used in the DD estimation, where it is set that $a = b$.
3. The effect refers to the bogus treatment effect on the treated dyad when using the bogus treatment year.

Table 11: parametric gravity estimates with heterogeneous treatment effects

ltrade	Rose default		heter. both-in effect		heter. both-in / one-in effect		heter. both-in / one-in / gsp effect	
ldist	-1.119	(0.022)	-1.112	(0.028)	-1.099	(0.060)	-1.100	(0.060)
lrgdp	0.916	(0.010)	0.900	(0.012)	0.858	(0.027)	0.858	(0.027)
lrgdppc	0.321	(0.014)	0.246	(0.019)	0.045	(0.044)	0.044	(0.044)
comlang	0.313	(0.040)	0.259	(0.053)	0.092	(0.107)	0.091	(0.107)
border	0.526	(0.111)	0.475	(0.122)	0.560	(0.190)	0.558	(0.190)
landl	-0.271	(0.031)	-0.253	(0.041)	-0.174	(0.086)	-0.173	(0.086)
island	0.042	(0.036)	0.043	(0.048)	0.108	(0.116)	0.109	(0.116)
lareap	-0.097	(0.008)	-0.122	(0.010)	-0.171	(0.023)	-0.171	(0.023)
comcol	0.585	(0.067)	0.669	(0.084)	1.080	(0.158)	1.079	(0.158)
curcol	1.075	(0.235)	2.780	(0.356)	4.812	(0.570)	4.810	(0.570)
colony	1.164	(0.117)	1.076	(0.152)	-0.526	(0.210)	-0.522	(0.209)
comctry	-0.016	(1.081)	0.056	(1.035)	0.047	(1.035)	0.333	(1.035)
custrict	1.118	(0.122)	0.624	(0.177)	0.038	(0.325)	0.037	(0.324)
regional	1.199	(0.106)	1.435	(0.154)	0.576	(0.392)	0.573	(0.391)
bothin	-0.042	(0.053)	-4.587	(0.636)	-10.720	(1.102)	-10.260	(1.124)
onein	-0.058	(0.049)	-0.056	(0.048)	-7.606	(1.075)	-7.402	(1.078)
gsp	0.859	(0.032)	1.127	(0.048)	0.556	(0.258)	-2.214	(0.760)
bothin x ldist			-0.017	(0.037)	-0.030	(0.065)	-0.054	(0.066)
bothin x lrgdp			0.029	(0.016)	0.071	(0.029)	0.057	(0.030)
bothin x lrgdppc			0.134	(0.025)	0.335	(0.047)	0.343	(0.048)
bothin x comlang			0.134	(0.067)	0.301	(0.117)	0.248	(0.121)
bothin x border			0.109	(0.197)	0.024	(0.254)	0.027	(0.250)
bothin x landl			-0.048	(0.052)	-0.127	(0.093)	-0.117	(0.096)
bothin x island			-0.035	(0.059)	-0.101	(0.123)	-0.075	(0.124)
bothin x lareap			0.052	(0.013)	0.101	(0.024)	0.114	(0.025)
bothin x comcol			-0.193	(0.114)	-0.606	(0.180)	-0.584	(0.180)
bothin x curcol			-1.890	(0.443)	-3.914	(0.621)	-3.737	(0.637)
bothin x colony			0.088	(0.186)	1.692	(0.253)	1.584	(0.281)
bothin x custrict			0.784	(0.219)	1.374	(0.352)	1.324	(0.352)
bothin x regional			-0.589	(0.193)	0.271	(0.409)	0.237	(0.409)
bothin x gsp			-0.458	(0.054)	0.108	(0.260)	-0.051	(0.281)
onein x ldist					-0.013	(0.063)	-0.030	(0.064)
onein x lrgdp					0.053	(0.029)	0.045	(0.029)
onein x lrgdppc					0.246	(0.047)	0.252	(0.047)
onein x comlang					0.273	(0.116)	0.249	(0.117)
onein x border					-0.077	(0.230)	-0.093	(0.229)
onein x landl					-0.099	(0.091)	-0.094	(0.091)
onein x island					-0.100	(0.119)	-0.083	(0.119)
onein x lareap					0.057	(0.024)	0.065	(0.024)
onein x comcol					-0.580	(0.175)	-0.568	(0.175)
onein x colony					1.708	(0.239)	1.609	(0.261)
onein x custrict					0.674	(0.369)	0.647	(0.374)
onein x regional					1.167	(0.409)	1.079	(0.415)
onein x gsp					0.479	(0.261)	0.362	(0.281)
gsp x ldist							0.180	(0.045)
gsp x lrgdp							0.062	(0.018)
gsp x lrgdppc							-0.018	(0.029)
gsp x comlang							0.188	(0.074)
gsp x border							-1.545	(0.383)
gsp x landl							-0.038	(0.060)
gsp x island							-0.135	(0.064)
gsp x lareap							-0.054	(0.014)
gsp x curcol							-0.585	(0.402)
gsp x colony							0.141	(0.191)
gsp x comctry							-1.421	(1.074)
gsp x custrict							0.169	(0.278)
gsp x regional							0.635	(0.279)
mean bothin effect	-0.042	(0.053)	-0.043	(0.001)	0.272	(0.002)	0.240	(0.002)
mean onein effect	-0.058	(0.049)	-0.056	(0.048)	0.272	(0.002)	0.241	(0.001)
mean gsp effect	0.859	(0.032)	1.127	(0.048)	0.556	(0.258)	0.718	(0.001)
R^2	0.6480		$0.6504^{\dagger}$		$0.6525^{\dagger}$		$0.6530^{\dagger}$	

Note:

1. OLS with year effects (intercepts not reported). Robust standard errors (clustering by dyads) are in the parenthesis. Some interaction terms are dropped due to collinearity.

2. When an effect is heterogeneous, the subject-wise effect equals the main effect plus the interaction effects scaled by the subject's covariates. The mean effect is estimated by the sample average of the subject-wise effects. When an effect is assumed homogeneous, the mean effect estimate records the marginal effect estimate.

3. A superscript † over the R^2 value indicates that the restricted default model (R^2_r) is rejected in favor of the unrestricted model (R^2_u) at the conventional significance levels by the χ^2_q test of $(N-\kappa)(R^2_u - R^2_r)/(1-R^2_u)$, where N is the sample size, κ the number of parameters in the unrestricted model, and q the difference in the numbers of parameters in the restricted and unrestricted models.