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ABSTRACT

On the Distribution of Exchange Rate Regime Treatment Effects on International Trade*

This paper provides evidence of heterogeneous treatment effects on trade from switching among three types of de-facto exchange rate regimes: freely floating, currency bands, and pegs or currency unions. A cottage literature at the interface of macroeconomics and international economics focuses on the consequences of exchange rate regimes for economic outcome such as trade. The majority of contributions points to trade-stimulating average effects of tighter exchange rate tying in general and of currency unions in specific. While there is great variability of the estimated quantitative effects across studies, all of the associated work adopted at least two and most of it all of the following three assumptions: assignment of countries to exchange rate regimes is random, the treatment effect of adopting a currency union is independent of the underlying regime transition, and it is homogeneous and hence fully captured by the average. This paper allows for self-selection into exchange rate regimes conditional on observable characteristics and a given regime state prior to a transition and provides evidence of strong impact heterogeneity on bilateral trade among otherwise observationally equivalent country-pairs.

JEL Classification: C22, C32, F31 and F33
Keywords: endogenous treatment effects, exchange rate regimes and heterogeneous treatment effects

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What is the effect of a common currency on international trade? Answer: Large.	*...the point estimates are positive, but the prediction that a common currency increases trade is qualified by substantial uncertainty.*	*"On the average" has never been a satisfactory statement with which to conclude a study on heterogeneous population.*
ANDREW K. ROSE (2000, P. 9)	TORSTEN PERSSON (2001, P. 446)	MOSHE BUCHINSKY (1994, P. 453)

1 Introduction

Empirical research suggests that tying exchange rates – especially through currency unions – increases bilateral trade flows among country-pairs by mitigating uncertainty,[1] though there is only little consensus about the magnitude of the effect. Most of the evidence on the matter assumes that assignment to exchange rate regimes occurs at random.[2] Also, it is commonly assumed that the response of outcome such as international trade is constant (i.e., homogeneous) within the population of country-pairs[3] limiting attention to average outcomes. To the best of our knowledge, previous work on exchange rate regimes at large did not pay attention to heterogeneous effects *of* when allowing for self-selection *into* different types of currency regimes.

[1]See Rose (2000, 2001), Rose and van Wincoop (2001), Frankel and Rose (2002), Glick and Rose (2002), Levy Yeyati (2002), Rose and Engel (2002), Barro and Tenreyro (2006), Klein and Shambaugh (2006), Egger (2008). Rose and Stanley (2005) provide a meta-analysis of earlier work. Evidence of a positive impact of fixed exchange rates on bilateral trade is consistent with evidence on the detrimental effect of exchange rate risk and volatility on trade flows (see Cushman, 1983, 1988; Brada and Mendez, 1988). Work which cast doubt on significantly positive trade effects of common currency encompasses Klein (1990, 2005) and Persson (2001). The latter work is consistent with two observations: namely that international trade is to a large extent controlled (if not induced) by multinational firms (see Zeile, 2003) and that evidence on exchange rate regimes on foreign direct investment is mixed (see Russ, 2007).

[2]Persson (2001) and Barro and Tenreyro (2006) are two of the few studies considering self-selection into currency unions. While Persson (2001) pursues an approach of matching based on the propensity score in a cross section of country-pairs, Barro and Tenreyro (2006) follow an instrumental variables approach with a panel of country-pairs.

[3]Rose and van Wincoop (2001) is one of the few studies considering implicitly heterogeneous responses of country-pairs' trade in general equilibrium.

By way of contrast, this paper will document significant variability of exchange rate regime treatment effects on bilateral international trade even after controlling for non-random assignment. We relax the assumption of random assignment within the framework of selection on observables. Within that framework, we focus on the question of impact variability on bilateral trade flows after conditioning out both, heterogeneity of country-pairs in terms of observable characteristics determining selection and the regime state prior to a transition.

Data on de-facto exchange rate regimes for 136 countries over the period 1965-2001 provide startling evidence for exchange rate regime impact heterogeneity on bilateral trade. We utilize three regime states (*freely floating*, *currency band*, and *currency peg or union*) based on data collected and provided by Reinhart and Rogoff (2003a,b, 2004) and employ quite strict criteria regarding the similarity between treated and control country-pairs prior to exchange rate regime transitions. Nevertheless, the variability of responses of bilateral exports is huge. Minimum and lowest 0.05^{th} quantile responses in average well-defined sub-populations of the data are negative, irrespective of the treatment and control sequence of transitions. Average maximum and above 0.95^{th} quantile responses are positive, irrespective of the treatment and control sequence of transitions. Moreover, there is no clear-cut pattern regarding average 0.5^{th} quantile (median) responses regarding transitions from tighter to less tight regimes or vice versa. All the mentioned location parameters of the impact distribution are statistically significantly different from zero at high precision. These results are novel and astonishing against the background of earlier evidence on average impacts of currency unions on trade. In fact, they suggest that the qualitatively quite robust conclusions about average treatment effects of, e.g., currency union membership accrues to at least one of two features: high positive extreme values of treatment effects on the treated or high negative extreme values of treatment effects on the untreated in the distribution; or the (by data refuted) assumption of the irrelevance of the exchange rate regime status prior to participating in a currency union.

Suppose one wished to consult risk-averse or risk-neutral policy makers who were interested in stimulating trade by choosing among exchange rate regimes. For this purpose, define risk-averse policy makers as ones who prefer transiting between exchange rate regimes for which the associated effects on bilateral trade were very unlikely (i.e., with low probability mass) negative and risk-neutral ones who considered positive effects on bilateral trade of average (or median) transitions. Based on the evidence in this paper, we would say that there is no clear-cut recommendation to make for risk-neutral policy makers. Risk-averse policy makers face a danger of reducing trade quite significantly no matter what they do. For this interpretation, it is important to recall that the source of variability of the exchange rate regime impacts lies beyond the pattern of observable characteristics. Hence, country-pairs with identical characteristics and exchange rate regime states ex ante may fare very differently from adopting one and the same transition in comparison to an identical counterfactual. Also, recall, that the location parameters of the impact distribution are estimated at very high statistical precision so that the impact variability should not be ascribed to a lack of data. It appears to be a fundamental feature of the data that international goods markets respond at high variability to exchange rate regime transitions in situations that look largely comparable to the econometrician.

The remainder of the paper is organized as follows. The next section describes data on exchange rate regime transitions and bilateral exports in the employed sample of 11,727 country-pairs and 36 years (1965-2001) which makes 160,464 observations. Section 3 discusses the econometric approach utilized for inference about averages and distributional features of treatment impacts in a multiple endogenous treatment effects framework. Section 4 summarizes the results and some robustness checks, and the last section concludes.

2 Design, data, and stylized facts

2.1 Some remarks on the study design

We focus on the impact of exchange rate regime transitions on the annual change of log bilateral exports between a year $t = 0$ prior to the transition and the subsequent year $t = 1$. Since the data come as (unbalanced) panel data, this entails that country-pairs may surface repeatedly. We will generally specify the probability of an exchange rate regime transition as a function of time-invariant and time-varying variables measured at time $t = 0$.[4] Since transitions among exchange rate regimes as well as the change in log bilateral exports refer to year $t = 1$ relative to $t = 0$, and since all specified determinants of such transitions are measured at time $t = 0$, we may generally suppress a specific year index without loss of generality and only use a time index to indicate the reference period of observables.[5]

2.2 Variable construction and data sources

2.2.1 Dependent variables

We employ two sets of dependent variables, namely the difference of log bilateral exports of country i to j from year $t = 0$ to $t = 1$ as the outcome of interest, Δy_{ij}, and a set of multinomial selection indicators reflecting transitions among de-facto exchange rate regimes prevailing between countries i and j in year $t = 1$ relative to $t = 0$.

To construct a measure of the outcome Δy_{ij}, we use unbalanced panel data from the United Nation's Comtrade Database on bilateral exports over the period 1965-2001. The multinomial selection indicators are obtained using the information on de-facto exchange rate regimes provided by Reinhart and Rogoff

[4]However, for inference about the significance of the conditioning variables determining regime transitions, we will take care of repeated country-pair observations by clustering standard errors.

[5]The latter notation will turn out useful for stating some of the assumptions below.

(2003a,b, 2004). We distinguishing between three types of de-facto exchange rate regimes: *freely floating*, *currency bands* (of any width), and *currency pegs or unions*.[6]

2.2.2 Observable variables determining selection into treatment

We use four types of variables to specify the conditioning set determining selection into specific de-facto exchange rate regimes in period $t = 1$. Taking the perspective of a reduced-form specification of the selection model, the inclusion of the variables mentioned in the following is clearly motivated by previous research. Before going into details, and bearing in mind the intrinsic element of subjectiveness with respect to the matter of model specification, we will rely on parsimony.[7] Let us denote the variables employed in the econometric selection equations by upper case letters.

Geography

With bilateral distance between countries i and j, DIST_{ij}, we employ a measure of geography which has been found to be robustly positively correlated with the magnitude of bilateral trade costs and negatively with bilateral trade volume (as well as other outcomes). Since a wider distance between (economic centers of) two countries reduces trade, it should also reduce the marginal benefit from an implementation of trade-facilitating measures (see Baier and Bergstrand, 2004). Hence, to the extent that tighter exchange rate arrangements are implemented for the sake of stimulating trade, we would expect to see currency tying more frequently between proximate rather than distant countries.[8]

Country size

Country size is a very robust determinant of various forms of bilateral outcomes

[6] In principal, the econometric approach adopted here would permit using an even finer classification. Reinhart and Rogoff (2003a,b, 2004) distinguish between two different classification schemes, a fine one with 15 regime classes and a coarse one with 4 classes. However, using a finer classification scheme than the one employed in this paper requires estimation of some of the treatment effects from too few observations so that econometric inference becomes doubtful. Hence, we chose a coarser grid in the interest of efficiency.

[7] The critical reader may view the finally employed specification of the latent process underlying the multinomial exchange rate regime adoption as too restrictive. For instance, one might consider a different functional form for the index or one might include covariates beyond the suggested ones. As will be discussed in section 4.1, we have estimated a set of alternative multinomial choice models. It turns out that the ranking and support regions of the propensity scores are very similar between those models so that estimation results are largely unaffected by the choice among the compared specifications.

[8] While most empirical work employs bilateral distance between two countries in logs, we use it in levels in the main specification. However, we will provide evidence that using one or the other does not impact our results. For instance, research by Eaton and Kortum (2002) and others does not rely on a log-linear distance specification of bilateral trade flows and results for the impact of distance on trade turn out robust to such non-log-linear specifications.

such as trade flows, capital flows, foreign direct investment, and migration flows. Recently, it has been found to affect economic integration agreements at large: goods trade agreements (see Baier and Bergstrand, 2004), investment and tax agreements (Bergstrand and Egger, 2011; Egger and Wamser, 2011), and currency unions (see Barro and Tenreyro, 2007). Following a literature motivating reduced-form specifications of outcome such as bilateral trade and foreign direct investment as well as trade and investment agreements, we employ the log of total real GDP (in U.S. dollars of 2000) of country i and country j together in year $t = 0$ as a measure of bilateral market size. We denote this variable by SIZE_{ij}. Moreover, we employ a similarity index in two countries' GDPs in year $t = 0$, SIMI_{ij}. The latter has been motivated by theoretical work on new trade theory (see Helpman and Krugman, 1985) and was employed successfully in subsequent empirical work to explain bilateral trade (see Helpman, 1987).[9] SIZE_{ij} and SIMI_{ij} together are key fundamental variables explaining the volume of goods trade, the share of goods trade within industries, and the propensity for trade-facilitating instruments to be applied.

Relative factor endowments

Helpman and Krugman (1985) and Helpman (1987) illustrate in a world of both relative-factor-endowment and new-trade-preference-based fundamentals how volumes of trade increase with bigger relative factor endowment differences between two countries when holding SIZE_{ij} and SIMI_{ij} constant. Hence, bigger relative factor endowment differences as a driver of trade volumes should also increase the incentive for two countries to adopt trade-facilitating policy measures. We broadly follow Helpman (1987) to employ a measure of relative factor endowment differences. Our measure approximates differences in capital-labor ratios between countries by the absolute value of the difference in log real per-capita income differences. The measure employed is defined as $\text{RLFAC}_{ij} \equiv |\ln(RGDPPC_i) - \ln(RGDPPC_j))|$ for countries i and j in year $t = 0$, where $RGDPPC_i$ reflects real GDP per-capita of country i in year $t = 0$.

Economic volatility

Finally, economic volatility – measured in terms of prices and/or real per-capita income – prior to the implementation of tighter or less tight exchange rate regimes should have an impact on the propensity of such schemes to be realized (see Barro and Tenreyro, 2007). We employ two measures of volatility in order to capture either aspect. VGDPPC_{ij} measures the volatility of real per-capita GDP and VINFL_{ij} measures the volatility of inflation in year $t = 0$. Either measure is based on the residuals of a log-linear dynamic econometric model, which regresses log real GDP per capita or the inflation rate on its

[9] SIMI_{ij} is defined as $\log\{1 - [RGDP_i/(RGDP_i + RGDP_j)]^2 - [RGDP_j/(RGDP_i + RGDP_j)]^2\} \in [\log(0.5), 0]$, where $RGDP_i$ is country i's real GDP in year $t = 0$.

once- and twice-lagged level, a fixed country effect, and common time effects.[10] Then, in each period $t = 0$ the residuals are used to calculate a country's coefficient of variation over the previous three years. The two measures VGDPPC_{ij} and VINFL_{ij} then reflect the corresponding three-year coefficients of variation, respectively.

2.3 Sample composition and descriptive statistics

Table A.1 in the Appendix provides a list of all 136 countries and the corresponding years covered. Focusing on the countries covered in the study by Reinhart and Rogoff (2003a,b, 2004) and on nonmissing data for fundamental variables determining exchange rate regime transitions (as introduced above) and the change in bilateral exports, we are left with $11,721$ country-pairs and $160,464$ (country-pair-year) observations.[11]

Table 1 contains descriptive statistics for the dependent variable, one-period growth in log bilateral exports Δy_{ij}, and the set of covariates determining exchange rate regimes.

– Table 1 about here –

With respect to the conditioning variables in the selection model, comparing their first and second moments with the analogous robust location measures already reveals that there is non-normality in the data.[12]

2.4 Stylized facts

The effects of exchange rate regime transitions between $t = 0$ and $t = 1$ on the change in log bilateral exports between these periods are at the heart of this paper's interest. Table 2 exhaustively summarizes the empirical transitions in the data.

– Table 2 about here –

Table 2 indicates that there is great persistence. The number of country-pair-year units which have the same exchange rate regime in $t = 1$ as in $t = 0$ across all periods selected is $150,491 + 2,441 + 5,967 = 158,899$. Altogether, we observe $139 + 461 + 291 + 96 + 512 + 66 = 1,565$ changes of exchange rate regimes. Hence, the probability of an arbitrary exchange rate regime change for a randomly chosen country-pair and year in the data is about 0.98%. The most frequent state observed is one with a *freely floating* exchange rate regime

[10] Note that such a dynamic fixed effects model which does not account for the endogeneity of lagged dependent yields virtually unbiased estimates of both the parameters and the residuals with as many as 36 years of data (see Nickell, 1981).

[11] About 10.5% of the country-pair units are observed for the full spell of 37 years. The average number of years per country-pair is about 13.7. About 76.9% of the country-pairs in the data appear at least 13.7 times.

[12] This is also confirmed by a series of formal normality tests. For the sake of brevity, we do not report them explicitly.

between countries i and j. There are more than twice as many pairs of countries that have a *currency peg or union* rather than a *currency band*.

– Table 3 about here –

Table 3 summarizes the average change in log bilateral exports from $t = 0$ to $t = 1$ for all country-pairs in the respective cell of the transition matrix of Table 2. If exchange rate regime transitions were assigned at random, the numbers in Table 3 would reflect consistent estimates of the respective average treatment effects. For instance, the figures in the table suggest that when switching from a *freely floating* exchange rate at $t = 0$ to a *currency peg or union* at $t = 1$, the average country-pair would experience a marginal increase in the growth of bilateral exports by somewhat less than one percentage point. In many cases, tighter arrangements suggest faster growth of bilateral trade on average, but switching from a *currency peg or union* to a *currency band* is a notable exception from that pattern. However, with a non-random adoption of exchange rate regime transitions, simple comparisons of cells in Table 3 may be inconsistent estimates of average treatment effects. Moreover, simple averages might capture only one out of a set of features of the respective treatment impact that are relevant to a policy maker's decision.

– Table 4 about here –

Table 4 summarizes distributional features of the change in log bilateral exports with respect to each of the observed empirical transitions. However, the reported figures present features of the observed marginal outcome distributions. Motivating this by the fundamental problem of causal inference as a problem of missing data or missing information in general. Even under random assignment of exchange rate regime transitions, we can not trivially infer about the respective distributions of treatment impacts. This means that without any further knowledge of where country-pairs observed within one marginal outcome distribution would appear in another one, we cannot even determine whether comparisons of average outcomes across regime transitions actually reveal a feature of the distribution of treatment impacts.

3 Econometric approach to avoid a self-selection bias

3.1 Outline and notation

Let us refer to a generic unit of observation by the index $ij \in \{1, \ldots, N\}$ and to the time period at stake by $t \in \{0, 1\}$. Let us denote the exchange rate regime states of *freely floating*, *currency band*, *currency peg or union* by acronyms F, B, and U, and indicate the exchange rate regime unit ij adopts at time t by s_{ijt}, where $s_{ijt} \in \Omega$ and $\Omega = \{F, B, U\}$. Thus, s_{ijt} may be considered as one unit-specific realization of the trinomial random variable S_t.

Hence, at time $t = 1$ there is a sequence of two trinomial random variables, $\underline{S} = (S_0, S_1)$, and a sequence of specific realizations of $\underline{s}_{ij} = (s_{ij0}, s_{ij1})$ with $\underline{s}_{ij} \in \Omega \times \Omega$. Consequently, $\Omega \times \Omega$ contains the set of all possible transitions among exchange rate regimes between periods $t = 0$ and $t = 1$.[13] Associated with each sequence of exchange rate regimes $\underline{s}_{ij}$, there is a potential outcome for the dependent variable Δy_{ij}. Conditioning on a specific realization s_{ij0} of the random variable S_0 at time $t = 0$, we can distinguish among three potential outcomes in the subsequent period, each of them associated with one specific realization of the random variable S_1 conditional on $S_0 = s_{ij0}$. For a given realization $\underline{s}_{ij}$ of sequence $\underline{S}$, let us denote the corresponding potential outcome by $\Delta y_{ij}^{\underline{s}} = \Delta y_{ij}^{s_0 s_1}$.[14]

As units are only observed in one treatment state at a time, and conditional on the same origin state s_{ij0}, we have to estimate unobserved counterfactual outcomes within regime $\tilde{s}_{ij1}$ at $t = 1$. Let us refer to the counterfactual realization of the random sequence $\underline{S}$ by $\underline{\tilde{s}}_{ij} = (s_{ij0}, \tilde{s}_{ij1})$. And, let us denote the corresponding counterfactual outcome by $\Delta y_{ij}^{\underline{\tilde{s}}} = \Delta y_{ij}^{s_0 \tilde{s}_1}$.

We refer to a generic unit ij in treatment state s_{ij1} by the index $ij = \{1, \ldots, N^{\underline{s}}\}$. To distinguish a generic unit in the respective counterfactual state, we refer to this unit by lm with $lm = \{1, \ldots, N^{\underline{\tilde{s}}}\}$.

Assume the existence of a $K \times 1$ random vector of covariates $\boldsymbol{X}$ with realization $\boldsymbol{x}_{ij1}$ in period $t = 1$ for unit ij, with $ij = 1, \ldots, N$, that is observed along with the realization of the sequence $\underline{S}$. We refer to the support of values $\boldsymbol{x}_{ij1}$ the random vector $\boldsymbol{X}$ can take on by $\boldsymbol{\chi}$. If we refer to specific locations on the support $\boldsymbol{\chi}$, let us do so by denoting the r^{th} location by $\boldsymbol{\chi}_r$ with $\boldsymbol{\chi}_r \subseteq \boldsymbol{\chi}$ and $r \in \{1, \ldots, R\}$.

3.2 Treatment effects on the treated of interest

For pairwise comparisons of potential outcomes associated with two different realizations of S_1 conditional on $S_0 = s_{ij0}$, we point attention at treatment effects on the treated (TTs). Rather than focusing on the first moment (the average) only, we may estimate broader features of the distribution of treatment impacts. As is well known, if units select into treatment based on potential outcomes, pure pairwise comparisons of outcomes $\Delta y_{ij}^{\underline{s}}$ and $\Delta y_{ij}^{\underline{\tilde{s}}}$ will not permit the features of TT to have a causal interpretation. Refereing to the literature on selection on observables, once we assemble country-pairs with sufficiently similar observable characteristics $\boldsymbol{X} = \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$, this may or may not capture impact heterogeneity. This means that once we have solved the selection problem, we may identify impact heterogeneity by the data under a number of additional assumptions which we will outline subsequently. Table 5 gives a summary of the respective sources of variation in TTs.

– Table 5 about here –

[13]This refers to the transitions as reported in Table 2.

[14]For instance, for $s_{ij0} = F$, the set of potential outcomes in $t = 1$ is $\Delta y_{ij}^{FF}, \Delta y_{ij}^{FB}, \Delta y_{ij}^{FU}$, and for $s_{ij0} = B$, the set of potential outcomes in $t = 1$ is $\Delta y_{ij}^{BF}, \Delta y_{ij}^{BB}, \Delta y_{ij}^{BU}$, etc.

3.2.1 Homogeneous TTs

If the expected outcome under a specific sequence of exchange rate regimes were constant over the support $\boldsymbol{\chi}$ of conditioning values $\boldsymbol{X}_{ij} = \boldsymbol{x}_{ij1}$, each marginal outcome distribution were degenerate at its mean, and the TT$= \theta_{ij}^{\underline{s},\tilde{\underline{s}}}(\boldsymbol{X} = \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}) = \theta^{\underline{s},\tilde{\underline{s}}}$ for all $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}$ and all $ij \in \{1, \ldots, N^{\underline{s}}\}$. Consequently, the TT for the pairwise comparison $\underline{s}, \tilde{\underline{s}}$ would be *homogeneous* across units, independent of the realization of $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}$, since the distribution of differences in two compared potential outcomes would be degenerate at the difference of the means from the two marginals.

3.2.2 Heterogeneous TTs by way of heterogeneity in $\boldsymbol{X}$

In a somewhat less restrictive setting than above, one could relax the assumption of homogeneity of TTs by allowing them to vary with $\boldsymbol{x}_{ij1}$. Then, for two arbitrary values $\boldsymbol{x}_{ij1}$ and $\boldsymbol{x}_{ij1}^*$ that $\boldsymbol{X}$ may take on, the corresponding marginal distributions of two compared potential outcomes could be viewed as degenerate at their conditional means obtained over the subset of values $\boldsymbol{\chi}_r$ in $\boldsymbol{\chi}$ as long as both $\boldsymbol{x}_{ij1}$ and $\boldsymbol{x}_{ij1}^*$ lied within $\boldsymbol{\chi}_r$. In other words, a randomly selected country-pair with observable characteristics $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$ or $\boldsymbol{x}_{ij1}^* \in \boldsymbol{\chi}_r$ would still have a *homogeneous TT* conditional on observed characteristics. While this type of heterogeneity of TTs may be of interest depending on the question at stake, we are interested in more fundamental forms of heterogeneity as explained subsequently.

3.2.3 Generally heterogeneous TTs

Empirically, even homogeneity of TTs of exchange rate regime transitions *conditional on observed characteristics* may be inappropriate. Then, for instance, policy makers could not hope to form precise expectations about the associated effects on outcomes such as changes in trade flows or GDP. In that case, even within $\boldsymbol{\chi}_r$ where units have sufficiently similar conditioning values of $\boldsymbol{X}$, the marginal outcome distributions would not become degenerate at the outcome associated with $\boldsymbol{\chi}_r$. Assuming a homogeneous TT would then not be justified at all, and the distribution of TTs for a randomly selected unit with $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$ or $\boldsymbol{x}_{ij1}^* \in \boldsymbol{\chi}_r$ would vary freely. Conceptually, this notion of *generally heterogeneous TTs conditional on observed characteristics* expresses an uncertainty about treatment impacts which calls for an assessment of further features of the distribution of TTs. Then, a randomly selected treated unit within $\boldsymbol{\chi}_r$ is assigned a whole probability distribution of outcomes under either of two considered potential sequences of exchange rate regimes, $\underline{s}, \tilde{\underline{s}}$, requiring a non-trivial mapping of the distribution of $\Delta y_{ij}^{\underline{s}}$ conditional on $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$ into the marginal distribution of $\Delta y_{ij}^{\tilde{\underline{s}}}$ conditional on $\boldsymbol{x}_{ij1}^* \in \boldsymbol{\chi}_r$.

3.3 Assumptions

This subsection outlines the assumptions imposed in order to obtain pairwise identification of marginal outcome distributions for treated units conditional on a vector of observable covariates. Moreover, we impose assumptions in order to identify the related distribution of treatment impacts conditional on observable characteristics. Therefore, the first set of assumptions is such that – for a specific sub-group of units defined by their exchange rate regime in period $t = 0$ – the assignment mechanism of exchange rate regimes in $t = 1$ may be considered as random, once we condition on a scalar function of covariates. The objective is to validly employ propensity score matching in a framework explicitly taking account of the multinomial nature of potential exchange rate regime states at $t = 1$. The subsequent discussion primarily draws on Lechner (2001). After restoring an independent assignment mechanism conditional on observables, we impose a second set of assumptions that allows to recover the conditional distribution of differences in outcomes, or – adopting the terminology of Heckman, Smith, and Clements (1997) – the distribution of impacts for treated units. For this, we adopt the framework developed in Heckman, Smith, and Clements (1997) and Heckman and Smith (1998) to associate outcomes across marginal distributions.[15] Since we are considering a conditional-on-observables version of their framework, our statements also involve assumptions about comparable conditioning values. The empirical analysis in this paper relies on the following set of assumptions.

3.3.1 Pairwise identification of marginal outcome distributions

Even if $\Delta y_{ij}^{\tilde{\underline{s}}}$ depends on $S_1|S_0 = s_{ij0}$, we assume that we may restore independence by conditioning on a vector of observables.

Assumption 1a. Conditional independence

$$\Delta y_{ij}^{\tilde{\underline{s}}} \amalg S_1 | S_1 \in \{s_{ij1}, \tilde{s}_{ij1}\}, S_0 = s_{ij0}, \boldsymbol{X} = \boldsymbol{x}_{ij1},\ \forall \Delta y_{ij}^{\tilde{\underline{s}}},\ \tilde{\underline{s}}_{ij} \in \Omega \times \Omega,\ \tilde{s}_{ij1} \neq s_{ij1}.$$

The previous statement postulates that, conditional on a K-vector of covariates, on the initial exchange rate regime state $S_0 = s_{ij0}$, and on the state in $t = 1$ being either s_{ij1} or $\tilde{s}_{ij1}$, the outcome under the counterfactual sequence $\tilde{\underline{s}}_{ij} = (s_{ij0}, \tilde{s}_{ij1})$ versus the observed sequence $\underline{s}_{ij} = (s_{ij0}, s_{ij1})$ is independent of S_1.[16] This implies that conditioning on observables and the initial state is sufficient to solve the selection problem in period $t = 1$.

To complete this statement, we further impose the following exogeneity condition on the initial exchange rate regime state in $t = 0$:

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[15] Without any further assumptions, it is in general not possible to infer about the distribution of the TTs. Only if the treatment impact is constant (i.e., homogeneous) over all treated units, the respective distribution is degenerate, implying a perfectly positive correlation of outcomes for both marginal distributions. This would simplify the problem of evaluating exchange rate regime effects substantially.

[16] See Proposition 2c in Lechner (2001) or also Lee (2005, pp. 174 f.)

Assumption 1b. Exogeneity of the initial state

$$\Delta y_{ij}^{\tilde{s}} \amalg S_0,\ S_0 = s_{ij0},\ \forall s_{ij0} \in \Omega.$$

Together with Assumption 1a, this assumption states that pairwise comparisons of potential outcomes are identified, but only for groups of units defined by their initial exchange rate regime in period $t = 0$. This implies that the exchange rate regime at $t = 0$ is random at time $t = 1$.[17] Taken together, the two previous statements explicitly take into account the multi-valued nature of the random variable S_1. They impose that the counterfactual outcome is locally independent of the assignment mechanism given the set of covariates determining selection within a specific sub-group of units as defined by a common initial state. Also note that Assumptions 1a and 1b are stated in terms of conditional independence, that is considered as rather excessive if the researcher is only interested in identifying average treatment effects.[18] Since we are interested in the full marginal distribution of the outcome a treated unit would have had, had it been observed in the counterfactual state, conditional independence is required for identification.[19]

By the next assumption, the dimensionality of the conditioning set required to restore independent treatment assignment – as implied by the K-variatness of $\boldsymbol{X}$ – may be substantially reduced. Lechner (2001) shows that the probability to switch to the counterfactual state conditional on either switching to the control state or to the observed state is a valid balancing score of dimension one.[20]

[17]Note that Assumption 1b is a modification of Lechner's (2004) weak dynamic conditional independence assumption (Statement b) and the implications thereof are discussed in his Theorem 1.

[18]In this case the weaker conditional mean independence assumption is sufficient for identification, which is implied by conditional independence.

[19]As outlined in Heckman and Vytlacil (2007), matching identifies the marginal outcome distribution that a treated unit would have had under the respective counterfactual exchange rate regime in the absence of selection bias.

[20]For the case of a binary treatment, the dimensionality-reduction property of the propensity score was first noted by Rosenbaum and Rubin (1983). Generalizing this concept to a multi-valued treatment, Imbens (2000) and Lechner (2001) point out that it must be ensured that the conditioning sets are in fact the same across treated and control observations. Hence, in this case a meaningful dimensionality reduction requires the conditioning set to contain both, the marginal transition probability to switch to the observed regime state as well as the marginal transition probability to switch to the counterfactual state. As pointed out in Imbens (2000), comparing two different potential states in a framework of multi-valued treatments, does not generally lead to a scalar-valued representation of the respective elements in the conditioning set. The intuition behind Lechner's result follows directly from the original proof in Rosenbaum and Rubin (1983). Assume that $b^*(\boldsymbol{X} = \boldsymbol{x}_{ij1}) = [p_{ij}^{\tilde{s}}, p_{ij}^{s}]$ is a valid balancing score (where the p's represent the respective marginal transition probabilities) and $b(\boldsymbol{X} = \boldsymbol{x}_{ij1}) = p_{ij}^{\tilde{s}}/(p_{ij}^{\tilde{s}} + p_{ij}^{s})$, such that $b(\cdot)$ is coarser than $b^*(\cdot)$. In the terminology of Rosenbaum and Rubin (1983), the information set represented by $b(\cdot)$ is the smaller one of the two and the law of iterated expectations implies that $E[b^*(\cdot)|b(\cdot)] = b(\cdot)$, which is a function of dimension one as in the binary treatment case. For a more extensive discussion see Lechner (2001) and also Lee (2005).

A*ssumption 2a. Balancing score*

If Assumptions 1a and 1b hold, then

$$\Delta y_{ij}^{\tilde{s}} \amalg S_1 | b(\boldsymbol{X} = \boldsymbol{x}_{ij1}), S_0 = s_{ij0},\ \forall \Delta y_{ij}^{\tilde{s}},\ \underline{\tilde{s}}_{ij} \in \Omega \times \Omega,\ \tilde{s}_{ij1} \neq s_{ij1}.$$

Hence, if it suffices to condition on $\boldsymbol{X} = \boldsymbol{x}_{ij1}$ to solve the selection problem, it also suffices to adjust for the scalar-valued function $b(\boldsymbol{X} = \boldsymbol{x}_{ij1})$. Taken together, it follows from Assumptions 1a-2a that conditional on the initial exchange rate regime state and the balancing score all marginal counterfactual distributions are pairwise identified. Hence, conditional on $S_0 = s_{ij0}$ we may validly employ matching on $b(\boldsymbol{X} = \boldsymbol{x}_{ij1})$.

In order to operationalize estimation of the balancing score function, we impose two further assumptions.

Assumption 2b. Stationary one-period transition probabilities

$$Pr(S_{\tau+1} | S_\tau = s_{ij\tau}, \boldsymbol{X} = \boldsymbol{x}_{ij,\tau+1}) = Pr(S_\tau | S_{\tau-1} = s_{ij,\tau-1}, \boldsymbol{X} = \boldsymbol{x}_{ij,\tau}),$$
$$\forall \tau \in \{1, \ldots, T-1\},\ \underline{s}_{ij\tau} \in \Omega \times \Omega.$$

Hence, the marginal probability of a unit to be observed in exchange rate regime $S_1 = s_{ij1}$ is a first-order stationary Markov process once we condition on state $S_0 = s_{ij0}$ and the K-vector of covariates $\boldsymbol{X} = \boldsymbol{x}_{ij1}$.[21]

The second statement involves the standard common support requirement i.e.,

Assumption 2c. Common support

$$0 < Pr(S_1 = s_{ij1} | S_0 = s_{ij0}, \boldsymbol{X} = \boldsymbol{x}_{ij1}) < 1,\ \forall \underline{s}_{ij} \in \Omega \times \Omega.$$

This emphasizes that propensity score matching can only solve the selection problem if suitable (comparable) control observations are available.[22]

Altogether, Assumptions 1a - 2b identify all pairwise marginal counterfactual distributions for a treated unit conditional on the initial exchange rate regime state and the balancing score, where the latter may be constructed from the marginal transition probabilities as obtained from a pooled multinomial choice model.

[21] For estimation this implies that we may pool the data for two periods ($t = 0$ and $t = 1$) in order to estimate the balancing score $b(\boldsymbol{X} = \boldsymbol{x}_{ij1})$. This reduces the complexity of the data structure and justifies using the cross-section notation introduced above as is custom with difference-in-difference estimation.

[22] Put differently, a transition from a specific initial state $S_0 = s_{ij0}$ to the subsequent state $S_1 = s_{ij1}$ conditional on covariates $\boldsymbol{X} = \boldsymbol{x}_{ij1}$, may neither be impossible nor may it be guaranteed (i.e., perfect prediction of exchange rate regime states may not be used for identification).

3.3.2 Identification of impact distributions for pairwise TTs

Given the above assumptions, the marginal distribution of the outcome a treated unit would have had under the counterfactual sequence of exchange rate regimes is identified, i.e., $F_{\tilde{\underline{s}}}(\Delta y_{ij}^{\tilde{\underline{s}}}|\underline{S} = \tilde{\underline{s}}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1})) = F_{\tilde{\underline{s}}}(\Delta y_{ij}^{\tilde{\underline{s}}}|\underline{S} = \underline{s}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1}))$. Moreover, $F_{\underline{s}}(\Delta y_{ij}^{\underline{s}}|\underline{S} = \underline{s}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1}))$ is observed from the data. But, we are interested in the conditional distribution of impacts, $F_{\theta}(\Delta y_{ij}^{\underline{s}} - \Delta y_{ij}^{\tilde{\underline{s}}}|\underline{S} = \underline{s}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1}))$, whose identification requires further assumptions.[23]

For instance, we need an assumption that allows us to group treated and untreated units within sub-populations assembling observations with highly similar observable characteristics $\boldsymbol{X}$. In general, for a conditioning set containing continuous random variables, it is difficult to obtain a set of treated and untreated observations that are subject to exactly the same conditioning value.

It is generally desirable to discard observations from the control sample that are very different from the treated observations under consideration in terms of the balancing scores as a metric of comparison. Rosenbaum and Rubin (1984) suggest subclassifying treated and untreated observations by stratification on the balancing score function. In this spirit, Dehejia and Wahba (2002) formalize a procedure for a combined estimation of the terms included in the first-stage estimation of the balancing score and the number of strata on the interval of estimated scores. For the case of a binary treatment, they follow Rosenbaum and Rubin (1984) and subdivide the interval of the estimated propensity score into quintiles. Focusing on t-values as the statistical criterion of interest, Dehejia and Wahba suggest employing a more flexible specification of the linear index in propensity score estimation, whenever mean equivalence does not hold for a specific regressor for all or a substantive amount of propensity score intervals. In this notion, a lack of balancing indicates the need to formulate a more accurate approximation that also captures higher-order effects within the index specification. We propose an alternative approach which enforces balancing to hold ex ante for the set of regressors at hand. An immediate advantage of this procedure is that we may rely on a parsimonious specification of the linear index in the propensity score model, since the number of strata as well as the location of strata bounds – as associated with sub-populations of similar observations – is not preassigned as in Dehejia and Wahba but estimated by exploiting the information contained in the data.

With the procedure proposed here, we have to find some valid approximation that defines a neighborhood $\boldsymbol{\chi}_r$ on the support of observable characteristics determining selection. In the following, we will refer to the associated mapping of $\boldsymbol{\chi}_r$ into the respective support of the balancing score function by $\mathcal{B}(\boldsymbol{\chi}_r)$.

[23] A treated unit can not be assigned a specific location where it would appear in the counterfactual conditional marginal outcome distribution. Conditional on $b(\boldsymbol{X} = \boldsymbol{x}_{ij1})$, a treated unit may be assigned a set of outcomes under the counterfactual exchange rate regime.

Assumption 3. Approximating the conditioning set

$$F_\theta(\theta_{ij}^{\underline{s},\tilde{\underline{s}}}|\underline{S} = \underline{s}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1}))$$

$$\approx F_\theta(\theta_{ij}^{\underline{s},\tilde{\underline{s}}}|\underline{S} = \underline{s}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1}) \in \mathcal{B}(\boldsymbol{\chi}_r) \wedge b(\boldsymbol{X} = \boldsymbol{x}_{ij1}^*) \in \mathcal{B}(\boldsymbol{\chi}_r))$$

with $\mathcal{B}(\boldsymbol{\chi}_r)$ being a set of conditioning values such that $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$ and $\boldsymbol{x}_{ij1}^* \in \boldsymbol{\chi}_r$.

In addition, we define each conditioning set with respect to highly similar conditioning values as

$$\boldsymbol{\chi}_r := \{\boldsymbol{x}_{ij1}, \boldsymbol{x}_{ij1}^* | Pr(\boldsymbol{x}_{ij1} = \boldsymbol{x}_{ij1}^* | H_0) > \alpha\}.$$

Assumption 3 states that, when conditioning on a neighborhood $\boldsymbol{\chi}_r$ instead of conditioning on any $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$ directly, will produce a reasonable approximation of the impact distribution of interest. In addition, we give a definition of the respective conditioning set in terms of the similarity of conditioning values assembled within a specific neighborhood. We will make use of the definition of $\boldsymbol{\chi}_r$ in Assumption 3 to estimate partitions of the K-dimensional support of conditioning values in order to define sub-populations of highly similar units.

In order to test the assumption of homogeneous TTs conditional on observable characteristics, we adopt the following assumption from Heckman, Smith, and Clements (1997), and Heckman and Smith (1998).[24] We postulate that units are perfectly positively ranked in both outcome distributions in the following sense:

Assumption 4a. Fréchet upper bound conditional on the set $\mathcal{B}$

$$F_{\underline{s},\tilde{\underline{s}}}(\Delta y_{ij}^{\underline{s}}, \Delta y_{ij}^{\tilde{\underline{s}}} | \underline{S} = \underline{s}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1}) \in \mathcal{B}(\boldsymbol{\chi}_r) \wedge b(\boldsymbol{X} = \boldsymbol{x}_{ij1}^*) \in \mathcal{B}(\boldsymbol{\chi}_r)) \leq$$

$$\min[F_{\underline{s}}(\Delta y_{ij}^{\underline{s}} | \underline{S} = \underline{s}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1}) \in \mathcal{B}(\boldsymbol{\chi}_r)), F_{\tilde{\underline{s}}}(\Delta y_{ij}^{\tilde{\underline{s}}} | \underline{S} = \underline{s}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1}^*) \in \mathcal{B}(\boldsymbol{\chi}_r))],$$

where the marginal distributions are assumed to be continuous and strictly increasing.

By Assumption 4a – and given that $b(\boldsymbol{X} = \boldsymbol{x}_{ij1})$ and $b(\boldsymbol{X} = \boldsymbol{x}_{ij1}^*)$ are sufficiently similar such that they may be attributed to units assembled within the same sub-population on the support of observable characteristics – we may use the Fréchet upper bound assuming perfect positive dependence in outcomes among the compared marginal distributions to construct a set of lower bounds for the sub-population-specific variances of the TT. This enables us to employ a version of the test suggested by Heckman, Smith, and Clements (1997) to

[24] For a conditional on covariates version of the statement in Heckman, Clements and Smith (1997) see also Abbring and Heckman (2007) or Fan and Park (2010).

check whether these lower bounds are statistically different from zero, or not, possibly providing evidence against the homogeneous TT model.

Note that the dependence structure implied by Assumption 4a is just one possibility to associate units' rankings across marginal outcome distributions. As pointed out in Heckman, Smith, and Clements (1997), there exist many different $F_\theta(\cdot)$ that are consistent with the two marginal outcome distributions. They can be distinguished by specific assumptions on the dependence of the rankings among units as they appear in the marginals. Specifically, if we are interested in estimating the distribution of TTs we will consider the following set of prior beliefs about units' rankings:

Assumption 4b. Priors about units' rankings of marginal outcomes

Let Kendall's τ *reflect the dependence structure among outcomes in the marginal distributions. We will refer to the case where each treated unit can occur in each rank of the counterfactual marginal outcome distribution with an equal probability as a naïve prior, i.e.,* $\tau \sim U[-1;1]$. *We will refer to the case where rankings across marginals are positively related by* $\tau \sim U(0;1]$ *(positive prior), and to the case of a negative association among rankings by* $\tau \sim U[-1;0)$ *(negative prior).*

Hence, once we have identified sub-populations of highly similar units (Assumption 3), Assumption 4b enables us to infer about sub-population-specific distributional characteristics of TTs (besides the average) that are consistent with the respective prior beliefs of how units are located across the marginal outcome distributions under consideration.

3.4 Implementation

This section outlines an estimation procedure based on the above assumptions in the context of our application. In a first step, we estimate a multinomial choice model to obtain the respective balancing score functions for pairwise comparisons of potential outcomes with respect to a conditioning set of scalar dimension. Next, we apply a grid-search procedure to partition the interval of balancing scores into subintervals that are distinct sets assembling sufficiently similar conditioning values that observable characteristics can take on. Then, we employ propensity score matching in order to restore the marginal outcome distributions under the absence of selection bias with respect to each estimated sub-population. We adopt the framework of Heckman, Smith, and Clements (1997) and Heckman and Smith (1998) to test the assumption of sub-population-specific homogeneous TTs. Finally, in order to obtain estimates of interesting features of the impact distributions, we employ sub-sampling from the set of potential outcome permutations that are consistent with the respective marginals and the imposed prior belief about units' rankings in the marginals. We will elaborate on the employed econometric procedures in detail in what follows.

3.4.1 Estimating sub-populations of sufficiently similar observable characteristics

As motivated above, we assemble treated and untreated observations with observable characteristics $\boldsymbol{X} = \boldsymbol{x}_{ij1}$ and $\boldsymbol{X} = \boldsymbol{x}_{lm1}$ such that $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$ and $\boldsymbol{x}_{lm1} \in \boldsymbol{\chi}_r$ ensure sufficient similarity of observable characteristics determining selection into treatment. Specifically, we estimate a set of points in K-coordinates, $(\boldsymbol{x}^r)_{r=1,\ldots,\hat{R}}$, such that two units' observable characteristics being sufficiently close to the point $\boldsymbol{x}^r$ (in each of the K-coordinates) implies that both units belong to the r^{th} sub-population. This involves estimating partitions of the K-dimensional support of observable characteristics, $\boldsymbol{\chi}_1, \ldots, \boldsymbol{\chi}_{\hat{R}}$, that contain treated and untreated observations within a neighborhood of the respective points $\boldsymbol{x}^1, \ldots, \boldsymbol{x}^{\hat{R}}$.[25] We desire the number of sub-populations $\hat{R}$ to be data-driven. For this, we exploit that each $\boldsymbol{\chi}_r$ is associated with an interval $\mathcal{B}(\boldsymbol{\chi}_r)$ on the segment of the real line defined by the lowest and the highest estimated balancing score function on the common support of treated and untreated observations.[26] Specifically, we estimate intervals $\mathcal{B}(\boldsymbol{\chi}_r)$ that assemble treated and untreated observations such that $\mathcal{B}(\boldsymbol{\chi}_r) = \{ij, lm | Pr(x_{k,ij1} = x_{k,lm1} | H_0) > \alpha, \forall k \in K)\}$, i.e., for each coordinate of $\boldsymbol{x}^r$ this defines a neighborhood with respect to the type I error α. In order to estimate all $(\mathcal{B}(\boldsymbol{\chi}_r))_{r=1,\ldots,\hat{R}}$ this requires determining $\hat{R}-1$ grid points such that within each of the $\hat{R}$ intervals statistical similarity among compared units in terms of their covariates can not be rejected for a pre-specified cutoff probability. A grid search procedure iteratively rescales the interval of the respective common support of the balancing score for treated an untreated observations – sorted from the lowest to the highest – until convergence to the largest subinterval is achieved, where $Pr(q_{50}(x_{k,ij1}) = q_{50}(x_{k,lm1}) | H_0) > \alpha, \forall k \in K$. Then the grid search is continued over the remaining interval, and so on.[27]

[25] Note that by definition each observation is uniquely assigned to a single sub-population.

[26] Though not necessarily required to estimate TT, we found the grid search algorithm to behave better if we restrict it to the common support of treated and untreated observations.

[27] Specifically, we determine the region of common support of the estimated balancing score for treated and untreated observations and sort in ascending order from the lowest to the highest value it takes on and choose the value α. We then set $\min[\hat{b}(\boldsymbol{X})] = \lambda_1^-$ and as an initial guess we set $\max[\hat{b}(\boldsymbol{X})] = \tilde{\lambda}_1^+$. We then degressively rescale the length of the testing interval subject to the update equation $\tilde{\lambda}_1^+ = \frac{\max[\hat{b}(\boldsymbol{X})]}{m}$, with m being the next largest integer such that for the second guess about $\hat{\lambda}_1^+$, we have $m = 2$, and for the third guess $m = 3$, and so on. For each regressor $k \in \{1, \ldots, K\}$, we keep on testing $H_0 : q_{50}(x_{ijk}) = q_{50}(x_{lmk})$ until statistical similarity of each regressor can not be rejected with a probability greater than α for a given testing interval. Once we have found the largest sub-interval on the common support of the estimated balancing score for treated and untreated observations, we have estimated $\mathcal{B}(\hat{\chi}_1)$. Hence, we may equate the operating guess about the upper interval bound and the estimated bound, i.e., $\tilde{\lambda}_1^+ = \hat{\lambda}_1^+$. Next, by definition $\hat{\lambda}_2^- = \hat{\lambda}_1^+$ and we set $\tilde{\lambda}_2^+ = \max[\hat{b}(\boldsymbol{X})]$. Again, we rescale the upper bound of the testing interval according to the above update equation and keep on testing the null $H_0 : q_{50}(x_{k,ij1}) = q_{50}(x_{k,lm1})$ until we have found the largest sub-interval where we can not reject statistical similarity with a probability of a type I error greater than α. We repeat the same procedure until all $\hat{R}-1$ grid points and all respective $\mathcal{B}(\hat{\chi}_1), \ldots, \mathcal{B}(\hat{\chi}_{\hat{R}})$ are found. Note that we refer to an interval

Notice that the suggested procedure requires *approximating* the set of outcomes for observationally otherwise highly similar units.[28] Therefore, defining sub-populations on the common support of treated and untreated observations has to meet two main requirements. On the one hand, we have to ensure that there remains a sufficiently large number of treated and untreated observations within each sub-population. On the other hand, the units in each sub-population have to be sufficiently similar. For the data-set at hand, we find that defining sub-populations around a set of points in K-dimensional space as sub-populations of median equivalence performs well.[29] Moreover, this provides a direct association of sub-population-specific impact distributions and the respective strata-specific estimates of average TTs as obtained by propensity score matching. It also provides a natural adoption of the test for homogeneous TTs as proposed by Heckman, Smith, and Clements (1997) conditional on sub-populations of highly similar units.

3.4.2 Propensity score matching algorithms

This section outlines and discusses how we impute the potential sample of outcome observations as they would have been observed in the respective counterfactual state under absence of selection bias. The following discussion presupposes that all assumptions in Section 3.3.1 are satisfied, so that we may apply propensity score matching.

For each treated unit, matching constructs an artificial control observation based on a similarity metric with respect to observable characteristics. By Assumptions 1a-2a, this metric may be constructed from the marginal transition probabilities, where conditioning on a specific initial state is kept implicit. Specifically, we employ kernel matching. This method imputes the counterfactual outcome that unit ij would have had otherwise as a kernel weighted average of control observations. The kernel weight assigned to the lm^{th} unit from the control sample is formed with respect to the distance between the estimated balancing scores of the treated and the respective control observation, where this distance is normalized by a bandwidth parameter. Hence, to

as one where convergence can not be achieved, if there remain less than one treated and/or two untreated observations within this interval. (This refers to the identification requirement of the constant and the variance for local constant regression.) Note that all intervals' but the last one's upper bounds are exclusive.

[28] As noted in Abbring and Heckman (2007), it would principally be possible to integrate out the conditioning variables from the marginal outcome distributions. Yet, this would require further assumptions on the joint distribution of outcomes and the conditioning variables, and would not allow us to recover the conditional distribution of impacts with respect to specific observable characteristics.

[29] Of course, one could think of alternative ways to group the data on conditional outcomes, e.g., grouping the conditioning values on the common support with respect to the nine nearest neighbors would be an obvious alternative. (The number nine is the smallest integer identifying more than 100,000 distinct permutation matrices to associate outcomes across marginal outcome distributions.) We have tried so for blocks of the transition matrix with only a few treated observations and did not find a notable difference in the estimation results between the proposed procedure and this alternative for the data at hand. However, for cells of the transition matrix with many treated observations, this way of grouping induces a *curse of dimensionality*, rendering estimation computationally infeasible.

estimate unit ij's outcome under the counterfactual sequence of exchange rate regimes $\underline{\tilde{s}}_{ij}$ we use

$$\Delta\hat{y}^{\underline{\tilde{s}}}_{ij} = \sum_{lm=1}^{N^{\underline{\tilde{s}},r}} \hat{\omega}_{ij,lm}\Delta y^{\underline{\tilde{s}}}_{lm}, \tag{1}$$

where summation over $lm = 1,\ldots,N^{\underline{\tilde{s}},r}$ refers to the r^{th} sub-population as estimated from the data. The explicit formulation of the weights $\hat{\omega}_{ij,lm}$ is the distinguishing feature between different matching estimators. In the following, we use kernel weights as obtained from local constant matching as well as the weights implied by local linear matching. Moreover, based on the local linear weights we also employ regression-adjusted matching as suggested in Heckman, Ichimura, and Todd (1998).[30] Analytically, all estimators employed represent the solution for the constant in a locally formulated weighted least squares problem, where the local linear representation of the minimization problem also contains a slope parameter on the distance between the estimated balancing scores of the respective treated observation and all respective untreated observations.[31,32]

To estimate the bandwidth as the relative weight that is given to neighbor-

[30]Specifically, we implement this by using a linear projection of the control group outcome on the elements contained in $\boldsymbol{X}$. We then predict the respective residuals for treated and untreated units and use them to replace $\Delta\hat{y}^{\underline{\tilde{s}}}_{ij}$ and $\Delta y^{\underline{\tilde{s}}}_{lm}$ in equation (1). The method of regression-adjusted matching suggested by Heckman, Ichimura and Todd (1998) provides a valuable robustness check on whether treated and control observations differ in a way that is not adequately captured by the other matching algorithms employed.

[31]The local constant estimator is obtained as

$$\arg\min_{a}\left\{\sum_{lm=1}^{N^{\underline{\tilde{s}},r}} \left(\Delta y^{\underline{\tilde{s}}}_{lm} - a\right)^2 K\left(\frac{\hat{b}(\boldsymbol{X}=\boldsymbol{x}_{ij1}) - \hat{b}(\boldsymbol{X}=\boldsymbol{x}_{lm1})}{\hat{h}_r}\right)\right\},$$

and the local linear estimator as

$$\arg\min_{a,c}\left\{\sum_{lm=1}^{N^{\underline{\tilde{s}},r}} \left(\Delta y^{\underline{\tilde{s}}}_{lm} - a - c[\hat{b}(\boldsymbol{X}=\boldsymbol{x}_{ij1}) - \hat{b}(\boldsymbol{X}=\boldsymbol{x}_{lm1})]\right)^2 K\left(\frac{\hat{b}(\boldsymbol{X}=\boldsymbol{x}_{ij1}) - \hat{b}(\boldsymbol{X}=\boldsymbol{x}_{lm1})}{\hat{h}_r}\right)\right\},$$

respectively. Solving each of the two previous expressions for an estimator $\hat{a}$ leads to the respective explicit representations of the kernel weights as denoted by $\hat{\omega}_{ij,lm}$ in expression (1).

[32]For notational convenience, decompose the weights $\hat{\omega}_{ij,lm}$ in expression (1) as the weight given to the lm^{th} control observation relative to all other observations in the respective control sample

$$\hat{\omega}_{ij,lm} = \frac{\hat{\mathrm{w}}_{ij,lm}}{\sum_{lm=1}^{N^{\underline{\tilde{s}},r}} \hat{\mathrm{w}}_{ij,lm}}.$$

Then, for the local constant weights

$$\hat{\mathrm{w}}_{ij,lm} = K\left(\frac{\hat{b}(\boldsymbol{X}=\boldsymbol{x}_{ij1}) - \hat{b}(\boldsymbol{X}=\boldsymbol{x}_{lm1})}{\hat{h}_r}\right),$$

and for the local linear weights

$$\hat{\mathrm{w}}_{ij,lm} = K\left(\frac{\hat{b}(\boldsymbol{X}=\boldsymbol{x}_{ij1}) - \hat{b}(\boldsymbol{X}=\boldsymbol{x}_{lm1})}{\hat{h}_r}\right)\left[L_2 - \left(\hat{b}(\boldsymbol{X}=\boldsymbol{x}_{ij1}) - \hat{b}(\boldsymbol{X}=\boldsymbol{x}_{lm1})\right)L_1\right]$$

ing observations, we follow Silverman's suggestion. In general this approximation also behaves well, if the data do not follow the same distribution as the design density. We employ this estimator with respect to each sub-population as estimated from the data.[33] As we impute the counterfactual outcome with respect to distinct sub-populations, this issue is emphasized by employing a global bandwidth independent of the location of treated observations. Since the number of control observations that are feasible to form a match varies with the comparison sample that is assigned to the r^{th} sub-population of observations, it is more desirable to ensure a flatter kernel if there are only few observations in the comparison sample, and a steeper one in the opposite case. We propose obtaining comparison-sample-specific bandwidth estimates to control for the location of treated observations.

3.4.3 Testing for homogeneous TTs

This section outlines and discusses how we test the null hypothesis of homogeneous TTs for sub-populations of highly similar units. The following discussion presupposes that all assumptions from Section 3.3.1 are satisfied. Moreover, we make use of Assumptions 3 and 4a from Section 3.3.2 to approximate the lower bound for the impact standard deviation with respect to a specific sub-population.

We would like to test whether all units observed for a specific sequence of exchange rate regimes, have the same effect of switching from exchange rate regime $S_0 = s_{ij0}$ to regime $S_1 = s_{ij1}$ versus switching to exchange rate regime $S_1 = \tilde{s}_{ij1}$, once we condition on $b(\boldsymbol{X} = \boldsymbol{x}_{ij1}) \in \mathcal{B}(\boldsymbol{\chi}_r)$ and $b(\boldsymbol{X} = \boldsymbol{x}^*_{ij1}) \in \mathcal{B}(\boldsymbol{\chi}_r)$. I.e., we want to infer whether two distinct treated units within the same sub-population have the same TT or not. In order to construct a set of lower bounds for the impact standard deviations as associated with each sub-population $r = 1, \ldots, \hat{R}$, we employ the procedure suggested by Heckman, Clements, and Smith (1997). They make use of the Fréchet-Hoeffding upper bound providing a lower bound for the impact variance. Hence, for the r^{th} sub-population of units we assume perfect positive dependence among their marginal outcomes in order to map one marginal outcome distribution into the other (see Assumption 4a). From the differences in outcomes as obtained from the Fréchet-Hoeffding upper bound we may construct sub-population-specific estimates of the lower bound for the conditional impact variance. In the following, we denote this lower bound for the r^{th} sub-population by $\mathrm{Var}_\ell(\theta_{ij}^{\underline{s},\tilde{\underline{s}}} | \underline{S} = \underline{s}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1}) \in \mathcal{B}(\boldsymbol{\chi}_r) \wedge b(\boldsymbol{X} = \boldsymbol{x}^*_{ij1}) \in \mathcal{B}(\boldsymbol{\chi}_r))$. In contrast to Heckman, Smith, and Clements (1997), we maintain condition-

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where

$$L_l = \sum_{lm=1}^{N^{\tilde{\underline{s}},r}} K\left(\frac{\hat{b}(\boldsymbol{X} = \boldsymbol{x}_{ij1}) - \hat{b}(\boldsymbol{X} = \boldsymbol{x}_{lm1})}{\hat{h}_r}\right) \left[\hat{b}(\boldsymbol{X} = \boldsymbol{x}_{ij1}) - \hat{b}(\boldsymbol{X} = \boldsymbol{x}_{lm1})\right]^l, \quad l = 1, 2.$$

An explicit formulation of the weights can also be found in Heckman, Ichimura, and Todd (1997).

[33] See Frölich (2005) and Galdo, Smith, and Black (2008) for a discussion of bandwidth selection in nonparametric treatment effects models.

ing on a unit being located within a specific sub-population.[34] For the lower bounds for all sub-populations, we know that $\text{Var}_\ell(\theta_{ij}^{\underline{s},\tilde{\underline{s}}}|\underline{S} = \underline{s}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1}) \in \mathcal{B}(\boldsymbol{\chi}_r) \wedge b(\boldsymbol{X} = \boldsymbol{x}_{ij1}^*) \in \mathcal{B}(\boldsymbol{\chi}_r)) \geq 0$, $\forall r \in \{1, \ldots, R\}$. Hence, we base our test-statistic on the sum of impact standard deviations over all sub-populations and therefore formalize the null hypothesis of homogeneous TTs conditional on observable characteristics as

$$H_0 : \sum_{r=1}^{R} \sqrt{\text{Var}_\ell(\theta_{ij}^{\underline{s},\tilde{\underline{s}}}|\underline{S} = \underline{s}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1}) \in \mathcal{B}(\boldsymbol{\chi}_r) \wedge b(\boldsymbol{X} = \boldsymbol{x}_{ij1}^*) \in \mathcal{B}(\boldsymbol{\chi}_r))} = 0 \quad (2)$$

This null hypothesis states that for each sub-population the lower bound conditional impact standard deviation is consistent with the homogeneous TTs model conditional on observable characteristics determining selection. Hence, non-rejection of the null implies that the distribution of TTs from experiencing exchange rate regime sequence $\underline{s}_{ij}$ versus $\tilde{\underline{s}}_{ij}$ for a unit that actually experienced sequence $\underline{s}_{ij}$ is not rejected to be degenerate at the respective average impact.

As suggested in Heckman, Smith, and Clements (1997), we infer the distribution of this test-statistic under the null from Monte-Carlo simulations. Specifically, under the assumption of homogeneous TTs conditional on observables, we apply stratified sampling from the estimated counterfactual distribution. The strata are given by the intervals on the common support of treated and untreated observations that were estimated to be clusters of observations with identical medians for all covariates in the conditioning set. For each Monte-Carlo replication we calculate the treated outcome under the null by adding a constant to the set of sampled untreated observations and then compute the test-statistic for equation (2). The distribution of those sums over all Monte-Carlo replications approximates the distribution of the test statistic under the null hypothesis of a homogeneous treatment impact on treated units, once we know their observable characteristics.[35] To finally obtain the statistical statement of interest, as in Heckman, Smith, and Clements (1997), we then compute quantiles of the simulated distribution under the null associated with specific values of the probability of a type I error.

3.4.4 Impact distribution estimation

We assume that all assumptions from Section 3.3.1 as well as Assumptions 3 and 4b from Section 3.3.2 are satisfied.

Once we have evidence, that there is substantive variation in the impact on bilateral trade for treated units experiencing exchange rate sequence $\underline{s}_{ij}$

[34] Heckman, Smith, and Clements (1997) have unconditional marginal outcome distributions since they consider a randomized experiment. Beyond that, we might ask whether we have impact heterogeneity by way of heterogeneity in $\boldsymbol{X}$ (see section 3.2.2), or treatment response is generally heterogeneous even conditional on observable differences (see section 3.2.3).

[35] Since this sum is bounded at zero from below we have a one-sided test. For instance, the 95% quantile of the simulated distribution is associated with a probability of a type I error equal to 5%.

versus $\underline{\tilde{s}}_{ij}$, we estimate certain features of $F_\theta(\theta_{ij}^{\underline{s},\underline{\tilde{s}}}|\underline{S} = \underline{s}_{ij}, b(\boldsymbol{X} = \boldsymbol{x}_{ij1}) \in \mathcal{B}(\boldsymbol{\chi}_r) \wedge b(\boldsymbol{X} = \boldsymbol{x}_{ij1}^*) \in \mathcal{B}(\boldsymbol{\chi}_r))$ for all sub-populations identified from the data.[36] Within each sub-population, $r = 1, \ldots, \hat{R}$, we follow Heckman, Smith, and Clements (1997) by employing sub-sampling from the collection of possible impact distributions consistent with the two marginal outcome distributions under consideration.[37] As formulated in Assumption 4b, we perform sub-sampling with respect to the set of all permutations that reflect a certain prior belief about units' rankings in the two marginals. Hence, the empirical analysis is conducted as follows.

For a generic sub-population of similar units, we sort all units in the treatment state, $S_1 = s_{ij1}$, and in the respective counterfactual state, $S_1 = \tilde{s}_{ij1}$, from the highest to the lowest outcome. In this way, we construct $\boldsymbol{Y}^{\underline{s},r} = (\Delta y_{(1)}^{\underline{s},r}, \ldots, \Delta y_{(N^{\underline{s},r})}^{\underline{s},r})'$ for the treatment state, and $\boldsymbol{Y}^{\underline{\tilde{s}},r} = (\Delta y_{(1)}^{\underline{\tilde{s}},r}, \ldots, \Delta y_{(N^{\underline{s},r})}^{\underline{\tilde{s}},r})'$ for the control state, where the subscript refers to an observation's ranking within the respective marginal outcome distribution. Denote the p^{th} draw from the set of possible $N^{\underline{s},r} \times N^{\underline{s},r}$ permutation matrices consistent with a specific prior about Kendall's τ by $\boldsymbol{\Pi}_r^p(\tau)$. Then, we construct the associated sub-population-specific set of treatment impacts as

$$\boldsymbol{\theta}_r^p = \boldsymbol{Y}^{\underline{s},r} - \boldsymbol{\Pi}_r^p(\tau)\boldsymbol{Y}^{\underline{\tilde{s}},r} \tag{3}$$

for each sub-population $r = 1, \ldots, \hat{R}$ and for each sampled permutation $p = 1, \ldots, P$. After having sampled a set of possible permutation matrices over P replications, $\boldsymbol{\Pi}_1^1(\tau), \ldots, \boldsymbol{\Pi}_{\hat{R}}^1(\tau)$ for the first replication until $\boldsymbol{\Pi}_1^P(\tau), \ldots, \boldsymbol{\Pi}_{\hat{R}}^P(\tau)$ for the P^{th} replication, and having constructed the associated sub-population-specific impacts, $\boldsymbol{\theta}_1^1, \ldots, \boldsymbol{\theta}_1^P$ for the first sub-population until $\boldsymbol{\theta}_{\hat{R}}^1, \ldots, \boldsymbol{\theta}_{\hat{R}}^P$ for the $\hat{R}^{\text{th}}$ sub-population, we then aggregate each $\boldsymbol{\theta}_r^1, \ldots, \boldsymbol{\theta}_r^P$ over all replications $p = 1, \ldots, P$ in order to obtain the distribution of TTs for each of the $r = 1, \ldots, \hat{R}$ sub-populations. As we assume a uniform prior about Kendall's τ among units' rankings in the two marginals, we may consider each sampled permutation as equally likely. I.e., for aggregating among the sampled impact distributions we may assign each draw $p = 1, \ldots, P$ an equal probability weight. Finally, from the sub-population-specific impact estimates we may recover a richer set of parameters, besides the mean, describing the impact of transitions among different types of exchange rate regimes on bilateral trade for a randomly drawn treated unit located within a specific sub-population of observable characteristics determining selection into treatment. This involves interesting quantiles that are identified with respect to each sub-population as estimated from the data.[38]

[36] As indicated above, in order to identify at least $100,000$ distinct impact distributions from the data we have to exclude sub-populations with less than nine units from the analysis.

[37] Note that considering all mappings among outcomes for the two marginals is in general not feasible, since the total number of mappings observationally equivalent for two given marginals is equal to the factorial of the number of treated (and therefore imputed counterfactual) observations within the respective sub-population.

[38] Note that it is possible to estimate the full probability density function or cumulative

In order to obtain standard errors for the respective parameter estimates, Heckman, Smith, and Clements (1997) suggest employing the bootstrap. We do so by block sub-sampling from the estimated sub-populations and perform the procedure as described above with respect to each bootstrap replication. Therefore, the empirical standard deviation over the set of bootstrap estimates for the respective location parameter serves as the bootstrap estimate of the associated standard error.

4 Estimation Results

4.1 Dimensionality reduction

Table 6 reports the multinomial choice model estimated in order to construct the required balancing score functions. The results suggest that bilateral distance (DIST_{ij}), bilateral country size (SIZE_{ij}), similarity of country size (SIMI_{ij}), relative factor endowments (RLFAC_{ij}), and volatility of inflation rate differences (VINFL_{ij}), each displays a significant impact for at least one of the marginal transitions listed. Volatility of per-capita income differences (VGDPPC_{ij}) does not enter significantly, since the corresponding variation is highly collinear with VINFL_{ij}.

– Table 6 about here –

As noted previously, the conditioning set of independent variables determining marginal transitions among different types of exchange rate regimes underlying Table 6 is quite parsimonious. In view of the literature, the set of independent variables could be modified or augmented in several straightforward ways. For instance, one might call into question whether using the level of distance rather than the log thereof in Table 6 is justified. Moreover, previous work on exchange rate regime effects on trade employed additional covariates inspired by the literature on gravity models in trade. Third, one might think that certain exchange rate regimes – akin to economic crises – happen at higher or lower frequency in different time intervals. Fourth, exchange rate regimes might respond more sluggishly to shocks in time-varying regressors than assumed in Table 6. Finally, the regressors in Table 6 might be the relevant ones but affect the choices in a less restrictive way than assumed. We addressed all of these issues in alternative models, each of them relying on a different set of conditioning variables entering the selection equation. We compare the respective alternative models by way of how predicted marginal transition probabilities are altered and how the likelihood of the data given the model is affected relative to the base specification reported in Table 6.

distribution function of a TT nonparametrically with respect to each sub-population, or to obtain a nonparametric estimate of the respective probability density function or cumulative distribution function conditional on the set of median characteristics, $\boldsymbol{x}^1, \ldots, \boldsymbol{x}^{\hat{R}}$, defining the sub-populations as explained in Section 3.4.1. However, we do not present these results here for the sake of brevity.

– Table 7 about here –

In Table 7, we consider Pearson's, Spearman's, and Kendall's coefficient of correlation to measure the degree of accordance among the marginal transition probabilities as predicted by the respective alternative specification versus the parsimonious benchmark. A highly positive correlation coefficient indicates that subsequent estimation results are hardly affected by relying on one or the other conditioning set. Next, we report the change in the log-pseudolikelihood as compared to the base specification and the number of regressors employed in the alternative model at the outer right of Table 7. The gain in the log-pseudolikelihood (ΔLL) serves as a measure of how more or less likely the observed data are, given the respective alternative specification relative to the benchmark in Table 6. Allowing a different set of regressors to enter the selection equation without a substantive gain in the log-pseudolikelihood is interpreted as evidence for the redundancy of the information about exchange rate regime switching contained in the alternative conditioning set relative to the one in Table 6.

The figures in Table 7 suggest the following conclusions. First, the change in the log-psuedolikelihood suggests that using the level of bilateral distance is preferable over using the log for the choices at stake. Moreover, using nine different gravity variables to approximate bilateral trade costs does not change the predicted marginal transition probabilities substantially.[39] Third, also the correlations between alternative models employing five-year period specific effects, once-lagged time-varying regressors in addition to the contemporaneous ones (at time $t = 0$), and a polynomial specification of the linear index function underlying the transition probabilities does not lead to substantive changes.

Notice that the grid search algorithm employed to estimate sub-populations of highly similar treated and untreated observations is designed to enforce median equivalence with respect to each of the K covariates included in the first-stage propensity score model. Hence, there is a trade-off between including many covariates in the model and maintaining computational tractability. In the interest of this trade-off, we prefer ceteris paribus a more parsimonious model.[40] Hence, we proceed by utilizing the results based on the model in

[39] We employed the log land area size of country i and j and the log sum of the land areas of i and j as three additional regressors. Furthermore, we included a dummy variable which is unity if countries i and j were members of the same regional trade agreement at time $t = 0$. Also, we included two indicator variables stating whether country i and country j are landlocked or not. Finally, we included indicator variables for a common land border, a common official language, and a colonial relationship in the past between countries i and j.

[40] Among all alternative specifications considered, the highest increase of the log-pseudolikelihood (about 11.14%) was found for the probability to switch to a freely floating regime or a currency band relative to remaining in the initial state regime of a currency peg or union, for the model under letter (b) in Table 7. That model relies on nine additional regressors relative to the one in Table 6. For the log-pseudolikelihood when a freely floating regime is the initial state, the respective increase was about 5.48%, and only 3.36% if currency band was the initial state. The model under letter (c) in Table 6 led to increases in the log-pseudolikelihood of 7.03%, 3.18%, and 8.68%, referring to the initial state regimes freely floating, currency band, and currency peg or union, respectively. For the specification under letter (d) in Table 7, we find relative increases of the log-pseudolikelihood of 0.59%,

Table 6.

4.2 Estimated sub-populations to establish comparability of units

Table 8 summarizes descriptive features of estimated sub-populations of highly similar units (see Section 3.4.1). Table 8 is organized in four columns with column 1 indicating the observed and counterfactual sequences, $\underline{s}_{ij}$ and $\underline{\tilde{s}}_{ij}$, for each pairwise comparison of potential exchange rate regime states considered at time $t = 1$. Column 2 in Table 8 reports the number of estimated intervals on the common support of treated and untreated observations, $\hat{R}$. By design, each of these intervals is associated with a sub-population with at least one treated and two untreated observations to meet the minimal identification requirement for the estimates of the constant and the variance by local constant regression. However, identification of a slope parameter beyond the constant requires an additional untreated observation. This explains why local linear regressions may or may not be based on the same number of observations as local constant regressions. Columns 3 and 4 of Table 8 report the sub-population average number of treated and untreated observations ($\bar{N}^{\underline{s},r}$ and $\bar{N}^{\underline{\tilde{s}},r}$, respectively). In general, to identify a sufficiently large set of distinct random mappings of one marginal outcome distributions into the other, we have to rely on sufficiently big sub-populations in terms of treated units.[41]

– Table 8 about here –

Table 8 suggests that the estimated number of sub-populations, $\hat{R}$, varies substantially over different compared exchange rate regime sequences. For instance, while for $\underline{s}_{ij}$ =FU and $\underline{\tilde{s}}_{ij}$ =F$\tilde{\text{F}}$ we estimate 73 distinct sub-populations, whereas for the tuple {UU,U$\tilde{\text{B}}$} we estimate only 2 distinct sub-populations. In summary, the average number of estimated sub-populations over all exchange rate sequences to be compared is about 15.83, the respective median equals 9, and the mode is 5. Notice that swapping the treatment and control sequence – e.g., comparing {FU,F$\tilde{\text{F}}$} with {FF,F$\tilde{\text{U}}$} – in Table 8 leads to starkly different entries across all columns.[42]

Supplementing the information contained in Table 8, Table A.2 in the Appendix contains descriptive features of the estimated probabilities of a type I error under the null hypothesis of similar medians with respect to each estimated sub-population. For each of the six regressors contained in the conditioning set of Table 6, we report the mean, the associated standard deviation,

2.48%, and 2.87% relative to Table 6, referring to the initial state regimes freely floating, currency band, and currency peg or union, respectively. Finally, for the model under letter (e) in Table 7 we find increases of 4.02%, 3.81%, and 3.73%, again referring to the initial state regimes as listed in the above ordering.

[41] Hence, this may restrict the sub-populations used for subsequent estimations to a subset of those reported in Table 8.

[42] This asymmetry of sub-populations may be interpreted as an indication that units among different observed sequences of exchange rate regimes differ non-randomly.

the minimum, and the maximum of estimated p-values. In order to produce tractable sub-populations, we find a minimum cutoff probability of 0.1 to perform well for the data-set at hand. This ensures that the respective minima in Table A.2 are bounded away from 0.1 from below. This means that a unit may be regarded as belonging to the respective sub-population if in each of the six dimensions its associated vector of characteristics is equal to the vector of median characteristics with a probability of a type I error smaller or equal to the estimated one. Therefore, the cutoff probability of 0.1 may be interpreted as the maximum amount of dissimilarity we are not willing to accept when making this statement about probabilities.

4.3 Imputed counterfactual outcomes and estimated average TTs

We impute each counterfactual outcome under absence of selection bias as it would have been observed for a treated unit, with observed sequence of exchange rate regimes $\underline{s}_{ij}$, had it instead experienced the counterfactual sequence $\underline{\tilde{s}}_{ij}$. Therefore, matching on the balancing score function restores the marginal outcome distribution of the treated under the respective counterfactual exchange rate regime in $t = 1$ conditional on $\boldsymbol{X} = \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$. For this, we employ three different kernel matching algorithms, local linear, local linear regression-adjusted, and local constant matching, respectively.[43] Due to the linearity of the expectations operator, the first moment of the TTs depends only on the marginal outcome distributions. Hence, averaging TTs over the support of estimated sub-populations serves as an estimate of the first (unconditional) moment of $\theta_{ij}^{\underline{s},\underline{\tilde{s}}}(\boldsymbol{X} = \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r)$. Table 9 presents estimation results about average TTs.

– Table 9 about here –

For a given initial state in $t = 0$, Table 9 reports the 18 different average TTs, $\hat{E}_{\boldsymbol{\chi}}[\theta_{ij}^{\underline{s},\underline{\tilde{s}}}(\boldsymbol{X} = \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r)]$, that are identified for pairwise comparisons of potential outcomes in $t = 1$. These are obtained as frequency-weighted averages over estimated sub-populations. Moreover, Table 9 summarizes the number of treated observations, the number of estimated sub-populations included, and the average bandwidth used for the local constant estimator and the respective deviations for the local linear estimators employed.[44,45]

With a few exceptions, the reported averages are of the same sign and magnitude for all of the three estimators employed. Using the local linear estimator

[43] As outlined in Section 3.4.2, local linear and local constant matching differ by the way local regression weights are formed from the differences in the balancing score functions of treated and untreated observations.

[44] See the annotations in the footnotes of Table 9 for details.

[45] As noted in Table 9, we used Silverman's rule of thumb for Gaussian data to determine the bandwidth. However, we followed the literature and used twice and half of that plug-in estimate as alternatives to assess the sensitivity of the results. The findings from those alternative procedures are not qualitatively different from those reported so that we suppress them in the interest of brevity. The results are available from the authors upon request.

and *freely floating* as the initial exchange rate regime state at time $t = 0$, the average TTs for the tuples {FF,F$\tilde{\text{B}}$}, {FF,F$\tilde{\text{U}}$}, and {FU,F$\tilde{\text{B}}$} are significantly different from zero. With the same estimator but a *currency band* at $t = 0$, the average TTs obtained for the tuples {BF,B$\tilde{\text{B}}$}, {BB,B$\tilde{\text{F}}$}, {BB,B$\tilde{\text{U}}$}, {BF,B$\tilde{\text{U}}$}, and {BU,B$\tilde{\text{F}}$} are significantly different from zero. Finally, when considering *currency peg or union* as the exchange rate regime state at time $t = 0$, the local linear estimator results in statistically significant average TTs for the tuples {UU,U$\tilde{\text{F}}$} and {UU,U$\tilde{\text{B}}$}. Considering the obtained pattern of estimates results in interesting conclusions: six of the significant average TTs suggest that tighter exchange rate regimes stimulate trade relative to less tight ones, while four of the significant average TTs support the opposite. By any means, the pattern of those effects is not clear-cut.

4.4 Testing the homogeneous TTs hypothesis

As mentioned above, the average TTs in Table 9 are calculated from 18 marginal outcome distributions of treated and imputed control sequences, respectively. Figures A.1.a-A.1.c in the Appendix contain scatter plots of the raw data distributions for the respective observed and imputed outcomes over the full set of estimated sub-populations as reported in Table 8 (using black color for treated and red color for the imputed control sequences). These figures illustrate clearly that there is substantive heterogeneity in the distributions of potential outcomes within different sub-populations for all compared sequences. Yet, while these figures are indicative of the heterogeneity of TTs in the data, whether homogeneity of the TTs has to be rejected can be assessed more formally.

– Table 10 about here –

Following the procedure outlined in Section 3.4.3, Table 10 summarizes the results for the test of homogeneous TTs conditional on observable characteristics determining selection. We infer the distribution of the test-statistic under the null hypothesis of a zero impact standard deviation from Monte-Carlo simulations such that the respective quantiles of the simulated distribution may be associated with the respective probabilities of a type I error.[46] Tables A.3.a-A.3.c in the Appendix contain the respective simulated probabilities of a type I error. We base estimation on 100,000 Monte-Carlo replications where each draw is obtained by performing stratified sampling from the imputed counterfactual distributions. Each block is defined by the respective estimated sub-population and the relative sample size is about one-third of the full sample.

The findings reported in Table 10 confirm substantive heterogeneity of TTs. This result is largely insensitive to the propensity score matching algorithm employed to impute counterfactual outcomes. Of the 54 test statistics reported in Table 10, only six do not reject the null hypothesis of homogeneity of TTs. Hence, there is strong support of heterogeneous TTs across the board. We will

[46]For details see Section 3.4.3 or Appendix E in Heckman, Smith, and Clements (1997).

base subsequent results on the local linear estimator, which rejects the null hypothesis in 15 out of 18 cases.

As indicated above, operationalizing the procedure suggested by Heckman, Smith, and Clements (1997) within our setting relies on an approximation of the conditioning set. The source of this approximation is the representation of each unit within the sub-population by the median characteristics rather than conditioning on the respective characteristics directly. Accordingly, one might argue that part of the identified heterogeneity in TTs may be artificial due to this approximation. We aim at addressing this problem as follows. We also computed the test based on transformed outcomes, obtained as the residuals from a sub-population-specific linear projection of the observed treated and the estimated counterfactual outcome, respectively, on deviations from the common sub-population-specific vector of median characteristics.[47] Table A.4 in the Appendix contains the respective results and suggests that exchange rate regime impact heterogeneity as documented in Table 10 is a fundamental feature in the data on bilateral exports that does not flow from the aforementioned approximation.

4.5 Estimated features of the distribution of exchange rate regime TTs on bilateral exports

Tables 11.a-11.c present estimation results for certain distributional features of the TTs for the different exchange rate regime transitions considered. While Table 11.a contains results based on a naïve prior about the rankings of units in each marginal outcome distribution as introduced in Section 3.3.2, Tables 11.b and 11.c report the ones based on positive and negative priors, respectively. As outlined in Section 3.3.2, the priors are reflected in Kendall's τ as a measure of rank correlations across marginals consistent with one's a priori belief.[48] All estimation results are based on a sample of $P = 100,000$ distinct random permutations. The reported bootstrap standard errors are based on 30 bootstrap replications with each sub-sampled block equal to about one-third of the original data.

Before going into details, it is worth noting that the results do not appear to be prior-driven and are closely comparable across the different assumptions employed. This is a desirable feature, in particular, since the mapping of marginal outcomes in the treatment state into marginal outcomes in the respective control state entails an imputation of missing information.

[47] For each sub-population r, we decompose $\Delta y^{\underline{s}}_{ij}$ and $\Delta \hat{y}^{\tilde{\underline{s}}}_{ij}$ into the component explained by the deviation of each element in the conditioning set from the respective sub-population-median and the remaining residual variation. Hence, for each sub-population we obtain the estimated linear projection residuals from the regression of $\Delta \boldsymbol{y}^{\underline{s},r} = (\Delta y^{\underline{s},r}_1, \ldots, \Delta y^{\underline{s},r}_{N^{\underline{s}}})$ and $\Delta \hat{\boldsymbol{y}}^{\tilde{\underline{s}},r} = (\Delta \hat{y}^{\tilde{\underline{s}},r}_1, \ldots, \Delta \hat{y}^{\tilde{\underline{s}},r}_{N^{\tilde{\underline{s}}}})$ on $\boldsymbol{Z}^r = (\boldsymbol{z}^r_1, \ldots, \boldsymbol{z}^r_K)$, with $\boldsymbol{z}^r_k = \boldsymbol{x}^r_{k,ij1} - q_{50}(\boldsymbol{x}^r_{k,ij1})$ where $\boldsymbol{x}^r_{k,ij1}$ the $N^{\underline{s},r} \times 1$ vector of observations on the k^{th} regressor in the r^{th} sub-population. Hence, we obtain $\Delta \hat{\boldsymbol{u}}^{\underline{s},r} = \Delta \boldsymbol{y}^{\underline{s},r} \boldsymbol{M}^r$ and $\Delta \hat{\boldsymbol{u}}^{\tilde{\underline{s}},r} = \Delta \hat{\boldsymbol{y}}^{\tilde{\underline{s}},r} \boldsymbol{M}^r$, with $\boldsymbol{M}^r = \boldsymbol{I}_{N^{\underline{s}},r} - \boldsymbol{Z}^r (\boldsymbol{Z}^{r\prime} \boldsymbol{Z}^r)^{-1} \boldsymbol{Z}^{r\prime}$.

[48] See Assumption 4b for details.

– Tables 11.a-11.c about here –

In Tables 11.a-11.c we present estimates of expected location parameters of the distribution of TTs as well as the expected share of treated units to experience a positive effect. The reported expectations are estimated over the support of estimated sub-populations of country-pairs with highly similar observable characteristics determining selection.[49] Therefore, the reported distributional features represent frequency-weighted averages obtained over the set of estimated sub-populations. Note that for each sub-population in the data, the specific average TT of an exchange rate regime transition in Table 9 corresponds to the same distribution as the sub-population-specific location parameters underlying the weighted averages in Tables 11.a-11.c.[50] As in Table 9, each frequency weight is obtained as the estimated number of units observed within a specific sub-population relative to all units observed within a specific treatment state.

In the subsequent discussion, we will focus on the results based on a naïve prior with $\tau \sim U[-1; 1]$ as in Table 11.a. From a bird's eye view, the results in Table 11.a complement the finings about average TTs in a striking way. First of all, for each and every one of the estimated TT distributions the 5% quantile value ($\hat{E}_\chi(0.05^{\text{th}}$ quantile)) is negative and significantly different from zero, while the 95% quantile ($\hat{E}_\chi(0.95^{\text{th}}$ quantile)) is positive and significantly different from zero. Similar conclusions apply even when considering the 25% and 95% quantiles instead. Based on this evidence, we would not advise a very risk-averse policy maker to, for instance, abandon a less tight exchange rate regime in favor of a tighter one. In fact, we would not recommend any action to such a policy maker given any exchange rate regime state of origin.

Apart from the average TTs in Table 9, the risk-neutral policy maker might seek advice from the median TT ($\hat{E}_\chi(0.5^{\text{th}}$ quantile). In that regard, 10 out of the 18 estimated median effects in Table 11.a are negative and the rest is positive (each effect being statistically significantly different from zero).

Table 11.a is organized in three vertical blocks (as Tables 11.b and 11.c). The one at the top refers to sequences with a *freely floating* (F) exchange rate regime at $t = 0$. The block at the center refers to sequences with a *currency band* (B) at $t = 0$. The block at the bottom indicates sequences with a *currency peg or union* (U) at $t = 0$.

Accordingly, there are three sequences in the top block where units that actually switched from a freer to a tighter arrangement are compared with

[49] The reported standard errors are estimated assuming independent observations across sub-populations.

[50] It appears useful to emphasize the following. Aggregating average TTs over the support of sub-populations leads to an estimate of the unconditional average TT. In contrast, averaging other location parameters such as the quantiles in Tables 11.a-11.c over the support of sub-populations leads to an estimate of the averaged location parameter of the conditional distribution but not the location parameter for the unconditional distribution of the TTs. Therefore, the results in Tables 11.a-11.c should be interpreted as to refer to the average sub-population. By that token, a significant negative 5% quantile together with a significant positive 95% quantile in Table 11.a are not inconsistent with a statistically significant (negative or positive) average TT in Table 9.

ones that did not ({FB,F$\tilde{\text{F}}$} and {FU,F$\tilde{\text{F}}$}) or, at least, not to the same degree ({FU,F$\tilde{\text{B}}$}). Conversely, there are three pairs in that block which did not at all or not to the same extent switch to a freer regime as the control units. If tighter regimes were at least at the median better for trade than less tight ones, we should observe a positive sign in the first, second, and sixth column and negative signs in the other columns. Four out of the six signs are not aligned with this. If anything, we would say that the block at the top of Table 11.a would support the view that leaving the regime of a freely floating exchange rate will likely be detrimental to trade and not recommend it to risk-neutral policy makers who would like to stimulate exports.

We can look at the block at the center from a similar perspective. However, since the initial state is a *currency band*, a *currency peg or union* is the only option to apply a tighter regime than that by design. If a tighter regime would stimulate trade relative to freer ones at the median, we would expect a negative sign in the first, third, fourth, and fifth column, and a positive one in the second and the sixth column. This is the case for four out of six median TTs. Hence, at the median in the row labeled $\hat{E}_\chi(0.5^{\text{th}}$ quantile), we might recommend adopting a tighter or avoiding a freer regime than a currency band to risk-neutral policy makers wishing to stimulate exports, if a currency band were in place in the outset.

Pegs or currency unions are the exchange rate regime at $t = 0$ for all TTs with observed or counterfactual transitions at the bottom of Table 11.a. From that origin state, an exchange rate regime can only become freer than in the outset. The question is how such a transition affects bilateral trade. Suppose again that a risk-neutral policy maker would aim at stimulating trade with the presumption that tighter exchange rate regimes are better for exports than freer ones. Such a policy maker would expect a negative sign in the first, second, and fifth column and a positive one in the third, fourth, and last column in the row labeled $\hat{E}_\chi(0.5^{\text{th}}$ quantile). Those expectations are met in only two out of six cases.

Together, the 18 TTs evaluated at the median do not suggest a clear-cut absorbing exchange rate state for risk-neutral policy makers who aim at stimulating trade: on average such individuals would prefer maintaining a floating regime when starting out with one; they would prefer to leave a currency band more likely in favor of a tighter regime; but they would on average not want to stay in such a regime then not stick to the latter. Across the board, there is no indication that tighter exchange rate regimes induce better expected trade outcomes than less tight ones.

Sufficiently risk-loving policy makers would feel comfortable in any exchange rate regime, since already the 75% quantile has an expected value that is positive. And sufficiently risk-averse policy makers would feel uncomfortable with almost everything they face since the 25% quantile has an expected value that is almost always negative (except for one out of 18 possibilities).

5 Conclusions

The empirical analysis of exchange rate regimes and their effects on bilateral trade flows entertained vivid attention over the last decade. Research on the matter focused on (exogenous or endogenous) treatment effects of regime states or changes thereof under two assumptions: first, that average treatment effects on trade were sufficient to consider and, second, that it did not matter which exchange rate regime countries (or country-pairs) were in prior to a transition. Allowing for selection on observables, the analysis in this paper was conducted in a framework of pairwise comparisons of multiple heterogeneous treatment effects among three possible exchange rate regime transitions for a given initial state. Therefore, this paper set out to allow endogenous exchange rate regime effects to be fundamentally heterogeneous along two lines: first, within sub-populations of highly similar units in terms of observables and with the same treatment; second, across exchange rate regime states prior the transition but with identical states after transiting.

This led to startling insights from this paper in comparison to earlier work. According to our findings, there is enormous variability in the expected treatment effects within a given type of exchange rate regime transition that can not be attributed to characteristics observable to the econometrician. This is true even when focusing on narrowly defined comparison groups of countries or country-pairs. Evaluating distributional features at average sample characteristics of country-pairs, expected location parameters indicate that the treatment impacts always have negative and positive support. However, for an average country-pair there is always a positive probability of increasing or decreasing bilateral trade. The respective parameters can all be estimated at high statistical precision so that the result does not flow from a lack of data.

With an eye on economic policy, one could interpret these findings as follows. Risk-averse policy makers would not find any type of exchange rate regime state or transition desirable. Most importantly, for risk-neutral policy makers it is not clear whether tighter or freer regimes would benefit trade more likely at the median, independent of the state of origin. However, there is also tremendous variability in median treatment effects for transiting to the same regime across regime states of origin. For instance, risk-neutral policy makers from countries with a currency band could be advised to adopt a currency peg or union on average. Neither could such policy makers in countries with a freely floating regime be advised to move to a tighter regime, nor could ones in countries with a currency peg or union be recommended to maintain it in general.

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Tables

Table 1: Data summary

	Δy_{ij}	DIST_{ij}	SIZE_{ij}	SIMI_{ij}	RLFAC_{ij}	VGDPPC_{ij}	VINFL_{ij}
Mean	0.072	7226.521	48.342	−2.352	8.618	0.003	−0.005
Median	0.075	6947.899	48.311	−1.843	8.985	0.007	0.103
Standard deviation	0.960	4527.260	2.804	1.656	1.280	0.321	1.137
Interquartile range	0.578	6716.922	3.630	2.261	1.598	0.092	1.170
Minimum	−10.365	10.479	36.157	−10.043	−3.547	−9.408	−11.125
Maximum	11.760	19772.340	59.100	−0.693	10.681	6.755	4.845

Note: The total number of observations is 160,464, collected as an unbalanced panel from 1965 - 2001 containing observations on 11,721 county-pairs. About 10.5% of country-pairs are observed for the full spell of 37 years, whereas the average number of years observed for each country-pair is about 13.7. Finally, about 76.9% of the observations in the data set correspond to at least 13.7 years of data.

Table 2: Empirical transition matrix among different types of exchange rate regimes

		$t = 1$		
		Freely floating	*Currency band*	*Currency peg or union*
	Freely floating	150,491 *93.7849%*	139 *0.0866%*	461 *0.2873%*
$t = 0$	*Currency band*	291 *0.1813%*	2,441 *1.5212%*	96 *0.0598%*
	Currency peg or union	512 *0.3191%*	66 *0.0411%*	5,967 *3.7186%*

Note: Italic numbers are percentage sample proportions relative to the total number of 160,464 country-pair observations.

Table 3: Average change in log-bilateral exports in different exchange rate regime transitions

		$t = 1$		
		Freely floating	*Currency band*	*Currency peg or union*
	Freely floating	0.0729*** *7.5593%*	0.0618 *6.1892%*	0.0734** *7.5292%*
$t = 0$	*Currency band*	0.1713*** *18.4964%*	0.0777*** *8.0576%*	0.1318** *13.5371%*
	Currency peg or union	0.0028 *0.2098%*	0.0796 *7.8794%*	0.0476*** *4.8754%*

1. Reported significance levels are based on bootstrapped standard errors with 100 bootstrap replications.
2. Italic numbers are growth rates in percent which correspond to the respective coefficients.

Table 4: Distributional features of Δy_{ij} for different exchange rate regime transitions

	FF	FB	FU	BF	BB	BU	UF	UB	UU
Minimum	−10.3651	−4.6444	−3.9512	−2.4454	−6.6050	−1.3863	−6.2653	−3.0684	−7.5167
0.05$^{\text{th}}$ quantile	−1.3067	−1.1560	−0.9345	−0.3672	−0.6762	−0.4786	−1.2071	−0.9510	−1.0003
0.25$^{\text{th}}$ quantile	−0.2198	−0.0505	−0.1382	−0.0713	−0.0505	−0.0771	−0.1892	−0.0534	−0.1179
0.50$^{\text{th}}$ quantile	0.0758	0.0718	0.0717	0.0774	0.0863	0.0731	0.0432	0.0719	0.0574
0.75$^{\text{th}}$ quantile	0.3796	0.2221	0.2467	0.2877	0.2416	0.2888	0.2257	0.2300	0.2373
0.95$^{\text{th}}$ quantile	1.4308	0.7860	1.1180	1.0105	0.7413	0.7149	1.0604	0.9303	1.0462
Maximum	11.7604	3.1246	6.9654	5.2592	4.6900	4.1190	7.3778	2.8811	5.5086

Note: The above figures refer to the data cells as defined by the empirical transitions given in Table 2.

Table 5: Potential sources of treatment effect variation

(a) Homogeneous TTs

Implication:	Holds for:
$\theta_{ij}^{\underline{s},\underline{\tilde{s}}}(\boldsymbol{X} = \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}) = \theta^{\underline{s},\underline{\tilde{s}}}$	$\forall \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}$, $\forall ij \in \{1, \ldots, N^{\underline{s}}\}$

(b) Heterogeneous TTs by way of heterogeneity in $\boldsymbol{X}$

Implication:	Holds for:
$\theta_{ij}^{\underline{s},\underline{\tilde{s}}}(\boldsymbol{X} = \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}) = \theta^{\underline{s},\underline{\tilde{s}}}(\boldsymbol{X} = \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r)$	$\forall \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$ and $\forall \boldsymbol{x}^*_{ij1} \in \boldsymbol{\chi}_r$, $\forall ij \in \{1, \ldots, N^{\underline{s}}\}$

Note:

I.e., $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$ and $\boldsymbol{x}^*_{ij1} \in \boldsymbol{\chi}_r$ implies that $\theta_{ij}^{\underline{s},\underline{\tilde{s}}}(\boldsymbol{X} = \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r) = \theta_{ij}^{\underline{s},\underline{\tilde{s}}}(\boldsymbol{X} = \boldsymbol{x}^*_{ij1} \in \boldsymbol{\chi}_r)$. Hence, it follows that $\theta^{\underline{s},\underline{\tilde{s}}}(\boldsymbol{X} = \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r)$ does not vary over ij once we condition on $\boldsymbol{\chi}_r$.

(c) Generally heterogeneous TTs

Implication:	Holds for:
$\theta_{ij}^{\underline{s},\underline{\tilde{s}}}(\boldsymbol{X} = \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}) = \theta_{ij}^{\underline{s},\underline{\tilde{s}}}(\boldsymbol{X} = \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r)$	$\forall \boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$, $\forall ij \in \{1, \ldots, N^{\underline{s}}\}$

Table 6: Multinomial logistic estimation of marginal transition probabilities $Pr(S_1 = s_{ij1}|S_0 = s_{ij0}, \boldsymbol{X} = \boldsymbol{x}_{ij1})$

	S_1 = B|S_0 =F	S_1 = U|S_0 =F	S_1 = F|S_0 =B	S_1 = U|S_0 =B	S_1 = F|S_0 =U	S_1 = B|S_0 =U
DIST_{ij}	-0.0002^{***}	-0.0001^{***}	-0.0001^{***}	0.0000	-0.0001^{***}	0.0001^{***}
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
SIZE_{ij}	0.3532^{***}	-0.0931^{***}	0.0155	-0.2609^{***}	0.1014^{***}	0.1528^{***}
	(0.0456)	(0.0242)	(0.0323)	(0.0511)	(0.0145)	(0.0437)
SIMI_{ij}	-0.3464^{***}	-0.1356^{***}	0.3666^{***}	-0.0088	0.1543^{***}	-0.0974
	(0.0579)	(0.0307)	(0.0395)	(0.0548)	(0.0319)	(0.0662)
RLFAC_{ij}	0.1041	-0.1642^{***}	0.5172^{***}	0.5110^{***}	0.5156^{***}	-0.1026
	(0.0913)	(0.0350)	(0.0949)	(0.1434)	(0.0624)	(0.0717)
VGDPPC_{ij}	-0.2913	0.1250	-0.2074	-0.4124	-0.0568	-0.0852
	(0.4149)	(0.1093)	(0.2883)	(0.2608)	(0.3147)	(0.1472)
VINFL_{ij}	0.1306^{**}	-0.1015^{***}	0.4643^{***}	0.3523^{**}	0.0635	0.0580
	(0.0650)	(0.0386)	(0.0987)	(0.1584)	(0.0472)	(0.0989)
constant	-25.2707^{***}	0.0818	-6.1215^{***}	5.0504^{***}	-11.1945^{***}	-12.0560^{***}
	(2.7057)	(1.1418)	(1.1758)	(1.8961)	(0.7769)	(2.4205)
Log-pseudolikelihood		-4078.4598		-1214.7624		-2021.2251
Wald $\chi^2(12)$		200.8168^{***}		272.1213^{***}		187.3052^{***}
Pseudo R^2		0.0383		0.0973		0.0641

1. Cluster-robust standard errors are reported in parentheses; significance of coefficient estimates at 1%, 5%, and 10% is indicated by ***, by **, and *, respectively.
2. The direction of the effect with respect to the regressors is interpretable relative to the base category stayers i.e., $s_{ij1} = s_{ij0}$.

Table 7: Alternative specifications

Alternative Specification including	Pearson	Spearman	Kendall	ΔLL	Regressors
(a) log of distance (instead of levels)	0.9680	0.9809	0.8988	-0.23%	6
(b) additional trade variables	0.6755	0.7160	0.5441	6.66%	15
(c) period specific effects	0.6855	0.6979	0.5408	6.30%	12
(d) lag 1 of time-varying regressors	0.8397	0.8961	0.7358	1.98%	12
(e) square and interaction terms	0.7500	0.7315	0.5781	3.85%	27

1. The first three columns report measures of average correlation of predicted marginal transition probabilities as obtained after the model as reported in Table 6 and those obtained from the various alternative specifications as listed right above.
2. The fourth column reports the average relative gain in the log-pseudolikelihood of the respective alternative specification with respect to the likelihood of the model in Table 6 as ΔLL.
3. A more elaborate description of the respective alternative specifications is given in the text.

Table 8: Estimated sub-populations (as assembled by sufficiently similar characteristics determining selection)

$\underline{s}_{ij}, \underline{\tilde{s}}_{ij}$	$\hat{R}$	$\bar{N}^{\underline{s},r}$	$\bar{N}^{\underline{\tilde{s}},r}$
FB,F$\tilde{\text{F}}$	23	5.87	6542.70
		(8.48)	(18388.77)
FU,F$\tilde{\text{F}}$	73	6.23	2061.52
		(13.53)	(4999.46)
FF,F$\tilde{\text{B}}$	5	29955.80	25.80
		(47807.55)	(21.61)
FF,F$\tilde{\text{U}}$	31	3872.39	12.65
		(9852.42)	(26.35)
FB,F$\tilde{\text{U}}$	10	13.40	41.80
		(26.46)	(36.47)
FU,F$\tilde{\text{B}}$	5	74.20	26.40
		(43.81)	(39.66)
BF,B$\tilde{\text{B}}$	10	29.10	244.10
		(20.57)	(207.01)
BU,B$\tilde{\text{B}}$	10	9.40	243.30
		(10.75)	(255.29)
BB,B$\tilde{\text{F}}$	8	285.88	35.63
		(176.59)	(50.34)
BB,B$\tilde{\text{U}}$	5	440.80	17.80
		(411.35)	(12.52)
BF,B$\tilde{\text{U}}$	8	19.75	9.38
		(31.21)	(8.30)
BU,B$\tilde{\text{F}}$	10	7.90	24.00
		(8.36)	(24.85)
UF,U$\tilde{\text{U}}$	36	14.03	165.53
		(23.08)	(255.10)
UB,U$\tilde{\text{U}}$	4	16.50	1491.75
		(19.00)	(2617.27)
UU,U$\tilde{\text{F}}$	35	163.80	14.11
		(204.14)	(16.12)
UU,U$\tilde{\text{B}}$	2	2983.50	33.00
		(2380.83)	(9.90)
UF,U$\tilde{\text{B}}$	5	64.40	12.00
		(100.36)	(17.56)
UB,U$\tilde{\text{F}}$	9	7.11	39.44
		(11.74)	(76.45)

1. The reported number of sub-populations $\hat{R}$ refers to those intervals where the grid search algorithm employed converged to a subinterval with at least one treated ($N^{\underline{s},r}$) and at least two untreated ($N^{\underline{\tilde{s}},r}$) observations to ensure the identification requirement for sub-population-specific local constant regression estimates of the mean treatment parameter on the treated. The two columns, $\bar{N}^{\underline{s},r}$ and $\bar{N}^{\underline{\tilde{s}},r}$, contain the average number of treated and untreated observations over the estimated sub-populations $1, \ldots, \hat{R}$.
2. The numbers reported in parentheses are the respective standard errors.
3. The minimum cutoff probability for non-rejection of the null of equivalent medians for the six independent variables contained in $\boldsymbol{X}$ is set to $\alpha = 0.1$.
4. Note there is Table A.2 in the Appendix where we report the estimated probabilities of a type I error for all regressors contained in the model.

Table 9: Estimated average treatment effects on the treated

	$\hat{E}_{\boldsymbol{\chi}}[\theta_{ij}^{s,\tilde{s}}(\boldsymbol{X}=\boldsymbol{x}_{ij1}\in\boldsymbol{\chi}_r)]$ when initial state exchange rate regime is *freely floating*					
	FB,F$\tilde{\text{F}}$	FU,F$\tilde{\text{F}}$	FF,F$\tilde{\text{B}}$	FF,F$\tilde{\text{U}}$	FB,F$\tilde{\text{U}}$	FU,F$\tilde{\text{B}}$
Local linear matching	0.0621	0.0160	0.0243	−0.2210	0.0285	−0.7102
	(0.0624)	(0.0429)	(0.0025)	(0.0029)	(0.0554)	(0.0296)
Local linear regression adjusted matching	0.0626	0.0166	−0.0231	−0.1997	0.0187	−0.7470
	(0.0626)	(0.0428)	(0.0025)	(0.0029)	(0.0560)	(0.0300)
Local constant matching	0.0623	0.0152	0.0554	−0.0852	0.0415	0.0849
	(0.0624)	(0.0430)	(0.0028)	(0.0029)	(0.0559)	(0.0403)
$N^{\underline{s}}$ local constant estimator	98	290	149779	120038	116	371
(± local linear estimators)	(±0)	(±0)	(±0)	(-3983)	(± 0)	(-22)
$\hat{R}$ local constant estimator	5	11	5	31	3	5
(± local linear estimators)	(±0)	(±0)	(±0)	(-8)	(±0)	(-1)
Avg. $\hat{h}_r$ local constant estimator	0.00008	0.00003	0.00053	0.00017	0.04580	0.04280
(± local linear estimators)	(±0.00000)	(±0.00000)	(±0.00000)	(+0.00001)	(±0.00000)	(+0.00204)
	$\hat{E}_{\boldsymbol{\chi}}[\theta_{ij}^{s,\tilde{s}}(\boldsymbol{X}=\boldsymbol{x}_{ij1}\in\boldsymbol{\chi}_r)]$ when initial state exchange rate regime is *currency band*					
	BF,B$\tilde{\text{B}}$	BU,B$\tilde{\text{B}}$	BB,B$\tilde{\text{F}}$	BB,B$\tilde{\text{U}}$	BF,B$\tilde{\text{U}}$	BU,B$\tilde{\text{F}}$
Local linear matching	0.1192	0.0670	−0.1163	−0.0278	0.2155	0.6606
	(0.0337)	(0.0680)	(0.0131)	(0.0137)	(0.0963)	(0.0903)
Local linear regression adjusted matching	0.1233	0.0633	−0.1656	−0.0557	0.1785	0.3903
	(0.0334)	(0.0674)	(0.0135)	(0.0142)	(0.0981)	(0.0885)
Local constant matching	0.1198	0.0722	−0.2227	−0.0755	0.2105	−0.2213
	(0.0343)	(0.0686)	(0.0142)	(0.0135)	(0.0480)	(0.0952)
$N^{\underline{s}}$ local constant estimator	281	80	2287	2202	148	58
(± local linear estimators)	(±0)	(±0)	(±0)	(-127)	(-94)	(±0)
$\hat{R}$ local constant estimator	8	4	8	5	4	3
(± local linear estimators)	(±0)	(±0)	(±0)	(-1)	(-1)	(±0)
Avg. $\hat{h}_r$ local constant estimator	0.00701	0.00225	0.01602	0.00512	0.02089	0.01460
(± local linear estimators)	(±0.00000)	(±0.00000)	(±0.00000)	(+0.00010)	(−0.00598)	(±0.00000)
	$\hat{E}_{\boldsymbol{\chi}}[\theta_{ij}^{s,\tilde{s}}(\boldsymbol{X}=\boldsymbol{x}_{ij1}\in\boldsymbol{\chi}_r)]$ when initial state exchange rate regime is *currency peg or union*					
	UF,U$\tilde{\text{U}}$	UB,U$\tilde{\text{U}}$	UU,U$\tilde{\text{F}}$	UU,U$\tilde{\text{B}}$	UF,U$\tilde{\text{B}}$	UB,U$\tilde{\text{F}}$
Local linear matching	−0.0638	0.0213	−0.1190	0.0086	0.0781	−0.0971
	(0.0417)	(0.0907)	(0.0098)	(0.0000)	(0.0625)	(0.1061)
Local linear regression adjusted matching	−0.0656	0.0222	−0.0820	−0.0823	0.2281	−0.0711
	(0.0417)	(0.0907)	(0.0099)	(0.0103)	(0.0648)	(0.1104)
Local constant matching	−0.0635	0.0254	0.0186	−0.0263	−0.0030	−0.0897
	(0.0418)	(0.0908)	(0.0124)	(0.0104)	(0.0589)	(0.1080)
$N^{\underline{s}}$ local constant estimator	441	62	5713	5967	315	38
(± local linear estimators)	(±0)	(±0)	(-809)	(±0)	(-58)	(±0)
$\hat{R}$ local constant estimator	13	2	33	2	4	1
(± local linear estimators)	(±0)	(±0)	(-10)	(±0)	(-2)	(±0)
Avg. $\hat{h}_r$ local constant estimator	0.00197	0.00115	0.00211	0.00434	0.02209	0.00936
(± local linear estimators)	(±0.00000)	(±0.00000)	(+0.00006)	(±0.00000)	(+0.00564)	(±0.00000)

1. The above results were obtained including only those sub-populations with ≥ 9 nine treated observations.
2. As suggested in Rosenbaum and Rubin (1984), the $\hat{E}_{\boldsymbol{\chi}}[\theta_{ij}^{s,\tilde{s}}(\boldsymbol{X}=\boldsymbol{x}_{ij1}\in\boldsymbol{\chi}_r)]$ were obtained as frequency-weighted averages over the respective sub-populations.
3. Sub-population-specific standard errors are obtained as suggested in Lechner (2001), and the respective estimates for the aggregate parameters were approximated assuming independent observations across the $\boldsymbol{\chi}_1,\ldots,\boldsymbol{\chi}_{\hat{R}}$ with $\hat{R}$ the number of estimated sub-populations. As applying large sample results may not be justified for each sub-population, we replace Lechner's unconditional variance estimate for the untreated outcome by the conditional variance estimate as obtained by the respective local polynomial estimator.
4. Sub-population-specific bandwidth estimates ($\hat{h}_r$) were obtained using Silverman's rule of thumb for Gaussian data. However, we report averages of the respective estimates, obtained over all sub-populations used for estimation.
5. $N^{\underline{s}}$ denotes the number of treated observations.

Table 10: Test for heterogeneous treatment effects on the treated conditional on observable characteristics

	Initial state exchange rate regime is *freely floating*					
	FB,F$\tilde{\text{F}}$	FU,F$\tilde{\text{F}}$	FF,F$\tilde{\text{B}}$	FF,F$\tilde{\text{U}}$	FB,F$\tilde{\text{U}}$	FU,F$\tilde{\text{B}}$
Local linear matching	9.4049***	49.5721***	4.4181	34.4276***	5.5411***	16.6446***
Local linear regression adjusted matching	9.4124***	49.5707***	4.4431	33.7631***	5.6126***	17.3357***
Local constant matching	9.5378***	46.4653***	2.4120***	23.6864***	5.7171***	0.9303***
	Initial state exchange rate regime is *currency band*					
	BF,B$\tilde{\text{B}}$	BU,B$\tilde{\text{B}}$	BB,B$\tilde{\text{F}}$	BB,B$\tilde{\text{U}}$	BF,B$\tilde{\text{U}}$	BU,B$\tilde{\text{F}}$
Local linear matching	2.6526***	1.6587***	6.7090***	1.9042***	1.0178***	23.8445
Local linear regression adjusted matching	2.5800***	1.6079***	6.1811***	1.9491***	0.9651***	12.0922
Local constant matching	2.7156***	1.8128***	3.0729***	2.4131***	1.1730***	1.5424***
	Initial state exchange rate regime is *currency peg or union*					
	UF,U$\tilde{\text{U}}$	UB,U$\tilde{\text{U}}$	UU,U$\tilde{\text{F}}$	UU,U$\tilde{\text{B}}$	UF,U$\tilde{\text{B}}$	UB,U$\tilde{\text{F}}$
Local linear matching	77.4847***	1.0464***	3.9792***	0.6598***	1.6382***	11.3008
Local linear regression adjusted matching	76.9971***	1.0446***	4.0012***	0.7250***	1.6671***	11.5574
Local constant matching	80.8891***	1.0545***	11.1642***	0.6787***	4.4354***	2.5381***

1. Under the null hypothesis of homogeneous treatment effects, once we know $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$, the lower bound for the standard deviation of the distribution of treatment impacts should not be significantly different from zero for each of the estimated sub-populations. Hence, the reported significance indicates whether those estimates are statistically different from zero, and therefore rejection of the null indicates heterogeneous treatment effects on the treated conditional on $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$.

2. The above reported numbers are the sub-population sums over the estimates for the lower bound of the standard deviation for the distribution of the treatment effect on the treated with respect to the compared outcomes. For each sub-population, estimation results were obtained as suggested by the procedure in Heckman, Smith, and Clements (1997).

3. As suggested in Heckman, Smith, and Clements (1997), we estimate Monte-Carlo cutoff values for rejection of the null stated in expression (2) in section 3.4.3. The estimated values are given in the appendix.

Table 11.a: Estimated features of the impact distributions aggregated over sub-populations for naïve prior about country-pairs' rankings in either marginal outcome distribution $\tau \sim U[-1;1]$

	Initial state exchange rate regime is *freely floating*					
	FB,F$\tilde{\text{F}}$	FU,F$\tilde{\text{F}}$	FF,F$\tilde{\text{B}}$	FF,F$\tilde{\text{U}}$	FB,F$\tilde{\text{U}}$	FU,F$\tilde{\text{B}}$
$\hat{E}_{\boldsymbol{\chi}}$(Minimum)	−1.2492	−1.6947	−3.5072	−4.6446	−1.3575	−2.8993
	(0.0000)	(0.0000)	(0.0005)	(0.0007)	(0.0001)	(0.0044)
$\hat{E}_{\boldsymbol{\chi}}$(0.05$^{\text{th}}$ quantile)	−0.8722	−0.9702	−0.9027	−1.6871	−0.6321	−1.5068
	(0.0000)	(0.0000)	(0.0004)	(0.0005)	(0.0001)	(0.0027)
$\hat{E}_{\boldsymbol{\chi}}$(0.25$^{\text{th}}$ quantile)	−0.0914	−0.2273	−0.1606	−0.5163	−0.1072	−0.8973
	(0.0000)	(0.0000)	(0.0004)	(0.0005)	(0.0001)	(0.0031)
$\hat{E}_{\boldsymbol{\chi}}$(0.5$^{\text{th}}$ quantile)	0.0384	−0.0052	0.0843	−0.1685	0.0402	−0.7156
	(0.0000)	(0.0000)	(0.0005)	(0.0005)	(0.0001)	(0.0020)
$\hat{E}_{\boldsymbol{\chi}}$(0.75$^{\text{th}}$ quantile)	0.1887	0.2082	0.3111	0.1505	0.1583	−0.5036
	(0.0000)	(0.0000)	(0.0005)	(0.0006)	(0.0001)	(0.0038)
$\hat{E}_{\boldsymbol{\chi}}$(0.95$^{\text{th}}$ quantile)	0.8699	1.1355	1.0203	1.2745	0.7473	0.1895
	(0.0000)	(0.0000)	(0.0003)	(0.0006)	(0.0001)	(0.0025)
$\hat{E}_{\boldsymbol{\chi}}$(Maximum)	1.9532	1.8586	3.3615	3.9300	2.6963	0.9681
	(0.0000)	(0.0000)	(0.0004)	(0.0008)	(0.0001)	(0.0046)
$\hat{E}_{\boldsymbol{\chi}}$(Share impact > 0)	0.5816	0.4759	0.5592	0.4593	0.5948	0.2541
	(0.0000)	(0.0000)	(0.0007)	(0.0004)	(0.0000)	(0.0022)
	Initial state exchange rate regime is *currency band*					
	BF,B$\tilde{\text{B}}$	BU,B$\tilde{\text{B}}$	BB,B$\tilde{\text{F}}$	BB,B$\tilde{\text{U}}$	BF,B$\tilde{\text{U}}$	BU,B$\tilde{\text{F}}$
$\hat{E}_{\boldsymbol{\chi}}$(Minimum)	−0.9765	−0.9502	−4.9291	−5.6597	−0.5887	−0.7009
	(0.0001)	(0.0001)	(0.0007)	(0.0004)	(0.0004)	(0.0049)
$\hat{E}_{\boldsymbol{\chi}}$(0.05$^{\text{th}}$ quantile)	−0.3977	−0.5492	−0.9066	−0.8274	−0.5411	−0.2819
	(0.0001)	(0.0001)	(0.0005)	(0.0003)	(0.0004)	(0.0033)
$\hat{E}_{\boldsymbol{\chi}}$(0.25$^{\text{th}}$ quantile)	−0.1063	−0.1524	−0.2482	−0.1755	−0.1285	0.4514
	(0.0001)	(0.0001)	(0.0003)	(0.0002)	(0.0003)	(0.0029)
$\hat{E}_{\boldsymbol{\chi}}$(0.5$^{\text{th}}$ quantile)	0.0430	0.0054	−0.1013	−0.0226	0.0208	0.6267
	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0004)	(0.0042)
$\hat{E}_{\boldsymbol{\chi}}$(0.75$^{\text{th}}$ quantile)	0.2251	0.1847	0.0536	0.1449	0.3598	0.7976
	(0.0001)	(0.0001)	(0.0004)	(0.0002)	(0.0004)	(0.0036)
$\hat{E}_{\boldsymbol{\chi}}$(0.95$^{\text{th}}$ quantile)	0.9951	1.1228	0.5805	0.6947	1.7747	1.9422
	(0.0001)	(0.0001)	(0.0005)	(0.0003)	(0.0004)	(0.0029)
$\hat{E}_{\boldsymbol{\chi}}$(Maximum)	2.3163	1.8529	3.5421	3.6894	2.1864	2.5050
	(0.0001)	(0.0001)	(0.0008)	(0.0004)	(0.0005)	(0.0037)
$\hat{E}_{\boldsymbol{\chi}}$(Share impact > 0)	0.5591	0.5179	0.3966	0.5004	0.6111	0.4483
	(0.0011)	(0.0063)	(0.0003)	(0.0006)	(0.0000)	(0.0000)
	Initial state exchange rate regime is *currency peg or union*					
	UF,U$\tilde{\text{U}}$	UB,U$\tilde{\text{U}}$	UU,U$\tilde{\text{F}}$	UU,U$\tilde{\text{B}}$	UF,U$\tilde{\text{B}}$	UB,U$\tilde{\text{F}}$
$\hat{E}_{\boldsymbol{\chi}}$(Minimum)	−3.2090	−2.7015	−3.8112	−7.2109	−5.9430	−3.1372
	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0005)	(0.0002)
$\hat{E}_{\boldsymbol{\chi}}$(0.05$^{\text{th}}$ quantile)	−1.2274	−1.1246	−0.9715	−1.1234	−0.8998	−1.1491
	(0.0001)	(0.0001)	(0.0004)	(0.0003)	(0.0004)	(0.0002)
$\hat{E}_{\boldsymbol{\chi}}$(0.25$^{\text{th}}$ quantile)	−0.2553	−0.1047	−0.3090	−0.2345	−0.1962	−0.1471
	(0.0001)	(0.0001)	(0.0003)	(0.0002)	(0.0004)	(0.0003)
$\hat{E}_{\boldsymbol{\chi}}$(0.5$^{\text{th}}$ quantile)	−0.0229	−0.0015	−0.1116	−0.0522	0.0593	−0.0569
	(0.0001)	(0.0000)	(0.0003)	(0.0001)	(0.0005)	(0.0002)
$\hat{E}_{\boldsymbol{\chi}}$(0.75$^{\text{th}}$ quantile)	0.2064	0.1623	0.0835	0.1352	0.2996	0.0763
	(0.0000)	(0.0001)	(0.0003)	(0.0002)	(0.0004)	(0.0003)
$\hat{E}_{\boldsymbol{\chi}}$(0.95$^{\text{th}}$ quantile)	0.9327	1.1793	0.7615	0.9122	1.1398	0.8489
	(0.0001)	(0.0001)	(0.0003)	(0.0003)	(0.0005)	(0.0002)
$\hat{E}_{\boldsymbol{\chi}}$(Maximum)	2.2894	1.5796	3.3497	4.7963	7.1196	0.9811
	(0.0001)	(0.0001)	(0.0005)	(0.0003)	(0.0005)	(0.0003)
$\hat{E}_{\boldsymbol{\chi}}$(Share impact > 0)	0.4559	0.4839	0.4082	0.4291	0.5720	0.3947
	(0.0013)	(0.0000)	(0.0003)	(0.0002)	(0.0000)	(0.0000)

1. Estimation results are based on drawing 100,000 permutations for each sub-population of county-pairs. Bootstrap estimates of standard errors are reported in parenthesis. They are based on 30 bootstrap replications with a sample size of about one-third of the full sample.
2. The reported above figures are based on frequency weighted averages of the respective parameters as obtained for each estimated sub-population of country-pairs. Aggregation across sub-populations is performed assuming independent observations across them.
3. For each sub-population estimation results were obtained as suggested by the procedure in Heckman, Clements, and Smith (1997) applied to the blocks defined by the respective sub-populations. However, we perform sub-sampling by directly mapping each sampled observation into the respective counterfactual distribution, except for {FF,F$\tilde{\text{B}}$} and {FF,F$\tilde{\text{U}}$}, where we follow Heckman, Clements, and Smith (1997) and rather base estimation on mapping percentile means, due to computational infeasibility of the direct approach.

Table 11.b: Estimated features of the impact distributions aggregated over sub-populations for positive prior about country-pairs' rankings in either marginal outcome distribution $\tau \sim U(0;1]$

	Initial state exchange rate regime is *freely floating*					
	FB,F$\tilde{\text{F}}$	FU,F$\tilde{\text{F}}$	FF,F$\tilde{\text{B}}$	FF,F$\tilde{\text{U}}$	FB,F$\tilde{\text{U}}$	FU,F$\tilde{\text{B}}$
$\hat{E}_{\boldsymbol{\chi}}$(Minimum)	−1.2430	−1.6851	−3.4840	−4.5608	−1.3449	−2.7039
	(0.0000)	(0.0000)	(0.0004)	(0.0007)	(0.0001)	(0.0035)
$\hat{E}_{\boldsymbol{\chi}}$($0.05^{\text{th}}$ quantile)	−0.8662	−0.9610	−0.8820	−1.6109	−0.6202	−1.3309
	(0.0000)	(0.0000)	(0.0004)	(0.0005)	(0.0001)	(0.0025)
$\hat{E}_{\boldsymbol{\chi}}$($0.25^{\text{th}}$ quantile)	−0.0881	−0.2222	−0.1509	−0.4742	−0.1006	−0.8172
	(0.0000)	(0.0000)	(0.0003)	(0.0005)	(0.0001)	(0.0016)
$\hat{E}_{\boldsymbol{\chi}}$($0.5^{\text{th}}$ quantile)	0.0385	−0.0052	0.0808	−0.1685	0.0402	−0.6983
	(0.0000)	(0.0000)	(0.0006)	(0.0004)	(0.0001)	(0.0009)
$\hat{E}_{\boldsymbol{\chi}}$($0.75^{\text{th}}$ quantile)	0.1855	0.2031	0.2938	0.1080	0.1517	−0.5997
	(0.0000)	(0.0000)	(0.0004)	(0.0005)	(0.0001)	(0.0014)
$\hat{E}_{\boldsymbol{\chi}}$($0.95^{\text{th}}$ quantile)	0.8639	1.1263	0.9921	1.1986	0.7354	0.0109
	(0.0000)	(0.0000)	(0.0004)	(0.0005)	(0.0001)	(0.0026)
$\hat{E}_{\boldsymbol{\chi}}$(Maximum)	1.9470	1.8490	3.3310	3.8465	2.6837	0.7712
	(0.0000)	(0.0000)	(0.0005)	(0.0008)	(0.0001)	(0.0044)
$\hat{E}_{\boldsymbol{\chi}}$(Share impact > 0)	0.5816	0.4759	0.5600	0.4591	0.6063	0.2428
	(0.0000)	(0.0000)	(0.0011)	(0.0003)	(0.0041)	(0.0014)
	Initial state exchange rate regime is *currency band*					
	BF,B$\tilde{\text{B}}$	BU,B$\tilde{\text{B}}$	BB,B$\tilde{\text{F}}$	BB,B$\tilde{\text{U}}$	BF,B$\tilde{\text{U}}$	BU,B$\tilde{\text{F}}$
$\hat{E}_{\boldsymbol{\chi}}$(Minimum)	−0.9610	−0.9257	−5.6495	−5.6495	−0.5195	−0.5186
	(0.0001)	(0.0001)	(0.0007)	(0.0004)	(0.0003)	(0.0026)
$\hat{E}_{\boldsymbol{\chi}}$($0.05^{\text{th}}$ quantile)	−0.3833	−0.5256	−0.8829	−0.8183	−0.4739	−0.1843
	(0.0001)	(0.0001)	(0.0005)	(0.0003)	(0.0003)	(0.0021)
$\hat{E}_{\boldsymbol{\chi}}$($0.25^{\text{th}}$ quantile)	−0.0984	−0.1396	−0.2366	−0.1703	−0.0948	0.3634
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0003)	(0.0027)
$\hat{E}_{\boldsymbol{\chi}}$($0.5^{\text{th}}$ quantile)	0.0431	0.0064	−0.1011	−0.0227	0.0209	0.6260
	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0004)	(0.0047)
$\hat{E}_{\boldsymbol{\chi}}$($0.75^{\text{th}}$ quantile)	0.2171	0.1719	0.0428	0.1399	0.3227	0.8487
	(0.0001)	(0.0001)	(0.0003)	(0.0002)	(0.0003)	(0.0038)
$\hat{E}_{\boldsymbol{\chi}}$($0.95^{\text{th}}$ quantile)	0.9806	1.0992	0.5533	0.6854	1.7066	1.9647
	(0.0001)	(0.0001)	(0.0005)	(0.0003)	(0.0003)	(0.0036)
$\hat{E}_{\boldsymbol{\chi}}$(Maximum)	2.3008	1.8285	3.5121	3.6792	2.1165	2.5169
	(0.0001)	(0.0001)	(0.0006)	(0.0005)	(0.0004)	(0.0050)
$\hat{E}_{\boldsymbol{\chi}}$(Share impact > 0)	0.5650	0.5125	0.3958	0.5026	0.6296	0.4483
	(0.0015)	(0.0000)	(0.0003)	(0.0005)	(0.0000)	(0.0000)
	Initial state exchange rate regime is *currency peg or union*					
	UF,U$\tilde{\text{U}}$	UB,U$\tilde{\text{U}}$	UU,U$\tilde{\text{F}}$	UU,U$\tilde{\text{B}}$	UF,U$\tilde{\text{B}}$	UB,U$\tilde{\text{F}}$
$\hat{E}_{\boldsymbol{\chi}}$(Minimum)	−3.1927	−2.6929	−3.7856	−7.2078	−5.9184	−3.1193
	(0.0001)	(0.0001)	(0.0005)	(0.0003)	(0.0005)	(0.0002)
$\hat{E}_{\boldsymbol{\chi}}$($0.5^{\text{th}}$ quantile)	−1.2121	−1.1166	−0.9485	−1.1205	−0.8769	−1.1327
	(0.0001)	(0.0001)	(0.0004)	(0.0002)	(0.0004)	(0.0002)
$\hat{E}_{\boldsymbol{\chi}}$($0.25^{\text{th}}$ quantile)	−0.2468	−0.1002	−0.2962	−0.2331	−0.1838	−0.1384
	(0.0001)	(0.0001)	(0.0003)	(0.0002)	(0.0005)	(0.0003)
$\hat{E}_{\boldsymbol{\chi}}$($0.5^{\text{th}}$ quantile)	−0.0229	−0.0016	−0.1116	−0.0521	0.0593	−0.0570
	(0.0001)	(0.0001)	(0.0003)	(0.0001)	(0.0005)	(0.0002)
$\hat{E}_{\boldsymbol{\chi}}$($0.75^{\text{th}}$ quantile)	0.1982	0.1579	0.0707	0.1337	0.2870	0.0673
	(0.0000)	(0.0001)	(0.0003)	(0.0002)	(0.0004)	(0.0002)
$\hat{E}_{\boldsymbol{\chi}}$($0.95^{\text{th}}$ quantile)	0.9174	1.1713	0.7383	0.9093	1.1171	0.8326
	(0.0001)	(0.0001)	(0.0003)	(0.0003)	(0.0004)	(0.0002)
$\hat{E}_{\boldsymbol{\chi}}$(Maximum)	2.2732	1.5710	3.3243	4.7932	7.0949	0.9634
	(0.0001)	(0.0001)	(0.0005)	(0.0004)	(0.0005)	(0.0003)
$\hat{E}_{\boldsymbol{\chi}}$(Share impact > 0)	0.4537	0.4839	0.4071	0.4289	0.5720	0.3947
	(0.0006)	(0.0000)	(0.0004)	(0.0003)	(0.0010)	(0.0000)

1. Estimation results are based on drawing 100,000 permutations for each sub-population of county-pairs. Bootstrap estimates of standard errors are reported in parenthesis. They are based on 30 bootstrap replications with a sample size of about one-third of the full sample.
2. The reported above figures are based on frequency weighted averages of the respective parameters as obtained for each estimated sub-population of country-pairs. Aggregation across sub-populations is performed assuming independent observations across them.
3. For each sub-population estimation results were obtained as suggested by the procedure in Heckman, Clements, and Smith (1997) applied to the blocks defined by the respective sub-populations. However, we perform sub-sampling by directly mapping each sampled observation into the respective counterfactual distribution, except for {FF,F$\tilde{\text{B}}$} and {FF,F$\tilde{\text{U}}$}, where we follow Heckman, Clements, and Smith (1997) and rather base estimation on mapping percentile means, due to computational infeasibility of the direct approach.

Table 11.c: Estimated features of the impact distributions aggregated over sub-populations for negative prior about country-pairs' rankings in either marginal outcome distribution $\tau \sim U[-1;0)$

	Initial state exchange rate regime is *freely floating*					
	FB,F$\tilde{\text{F}}$	FU,F$\tilde{\text{F}}$	FF,F$\tilde{\text{B}}$	FF,F$\tilde{\text{U}}$	FB,F$\tilde{\text{U}}$	FU,F$\tilde{\text{B}}$
$\hat{E}_{\boldsymbol{\chi}}$(Minimum)	−1.2555	−1.7044	−3.5315	−4.7278	−1.3701	−3.0959
	(0.0000)	(0.0000)	(0.0005)	(0.0007)	(0.0001)	(0.0038)
$\hat{E}_{\boldsymbol{\chi}}$($0.05^{\text{th}}$ quantile)	−0.8782	−0.9795	−0.9247	−1.7632	−0.6440	−1.6852
	(0.0000)	(0.0000)	(0.0004)	(0.0006)	(0.0001)	(0.0026)
$\hat{E}_{\boldsymbol{\chi}}$($0.25^{\text{th}}$ quantile)	−0.0947	−0.2324	−0.1721	−0.5586	−0.1137	−0.9946
	(0.0000)	(0.0000)	(0.0005)	(0.0005)	(0.0001)	(0.0039)
$\hat{E}_{\boldsymbol{\chi}}$($0.5^{\text{th}}$ quantile)	0.0384	−0.0052	0.0859	−0.1685	0.0402	−0.7152
	(0.0000)	(0.0000)	(0.0005)	(0.0006)	(0.0001)	(0.0027)
$\hat{E}_{\boldsymbol{\chi}}$($0.75^{\text{th}}$ quantile)	0.1920	0.2133	0.3256	0.1926	0.1649	−0.4048
	(0.0000)	(0.0000)	(0.0004)	(0.0005)	(0.0001)	(0.0031)
$\hat{E}_{\boldsymbol{\chi}}$($0.95^{\text{th}}$ quantile)	0.8759	1.1447	1.0454	1.3510	0.7592	0.3668
	(0.0000)	(0.0000)	(0.0004)	(0.0005)	(0.0001)	(0.0023)
$\hat{E}_{\boldsymbol{\chi}}$(Maximum)	1.9596	1.8682	3.3888	4.0135	2.7089	1.1624
	(0.0000)	(0.0000)	(0.0004)	(0.0008)	(0.0001)	(0.0037)
$\hat{E}_{\boldsymbol{\chi}}$(Share impact > 0)	0.5728	0.4724	0.5551	0.4611	0.5862	0.2783
	(0.0035)	(0.0000)	(0.0012)	(0.0003)	(0.0000)	(0.0010)
	Initial state exchange rate regime is *currency band*					
	BF,B$\tilde{\text{B}}$	BU,B$\tilde{\text{B}}$	BB,B$\tilde{\text{F}}$	BB,B$\tilde{\text{U}}$	BF,B$\tilde{\text{U}}$	BU,B$\tilde{\text{F}}$
$\hat{E}_{\boldsymbol{\chi}}$(Minimum)	−0.9920	−0.9746	−4.9594	−5.6700	−0.6589	−1.1190
	(0.0001)	(0.0001)	(0.0006)	(0.0005)	(0.0004)	(0.0030)
$\hat{E}_{\boldsymbol{\chi}}$($0.05^{\text{th}}$ quantile)	−0.4121	−0.5728	−0.9342	−0.8366	−0.6093	−0.6811
	(0.0001)	(0.0001)	(0.0005)	(0.0003)	(0.0004)	(0.0024)
$\hat{E}_{\boldsymbol{\chi}}$($0.25^{\text{th}}$ quantile)	−0.1143	−0.1654	−0.2636	−0.1806	−0.1655	0.2312
	(0.0001)	(0.0001)	(0.0004)	(0.0002)	(0.0003)	(0.0030)
$\hat{E}_{\boldsymbol{\chi}}$($0.5^{\text{th}}$ quantile)	0.0430	0.0054	−0.1012	−0.0227	0.0208	0.6287
	(0.0001)	(0.0001)	(0.0003)	(0.0003)	(0.0004)	(0.0059)
$\hat{E}_{\boldsymbol{\chi}}$($0.75^{\text{th}}$ quantile)	0.2331	0.1977	0.0690	0.1501	0.3970	1.0181
	(0.0001)	(0.0001)	(0.0005)	(0.0003)	(0.0003)	(0.0033)
$\hat{E}_{\boldsymbol{\chi}}$($0.95^{\text{th}}$ quantile)	1.0095	1.1464	0.6082	0.7038	1.8420	2.3419
	(0.0001)	(0.0001)	(0.0005)	(0.0002)	(0.0003)	(0.0031)
$\hat{E}_{\boldsymbol{\chi}}$(Maximum)	2.3318	1.8774	3.5727	3.6995	2.2557	2.9234
	(0.0001)	(0.0001)	(0.0006)	(0.0004)	(0.0003)	(0.0052)
$\hat{E}_{\boldsymbol{\chi}}$(Share impact > 0)	0.5552	0.5250	0.3955	0.4996	0.6111	0.4787
	(0.0000)	(0.0000)	(0.0004)	(0.0005)	(0.0000)	(0.0074)
	Initial state exchange rate regime is *currency peg or union*					
	UF,U$\tilde{\text{U}}$	UB,U$\tilde{\text{U}}$	UU,U$\tilde{\text{F}}$	UU,U$\tilde{\text{B}}$	UF,U$\tilde{\text{B}}$	UB,U$\tilde{\text{F}}$
$\hat{E}_{\boldsymbol{\chi}}$(Minimum)	−3.2252	−2.7102	−3.8366	−7.2139	−5.9677	−3.1549
	(0.0001)	(0.0001)	(0.0005)	(0.0003)	(0.0005)	(0.0003)
$\hat{E}_{\boldsymbol{\chi}}$($0.05^{\text{th}}$ quantile)	−1.2427	−1.1327	−0.9947	−1.1262	−0.9225	−1.1655
	(0.0001)	(0.0001)	(0.0003)	(0.0003)	(0.0004)	(0.0002)
$\hat{E}_{\boldsymbol{\chi}}$($0.25^{\text{th}}$ quantile)	−0.2637	−0.1091	−0.3219	−0.2362	−0.2088	−0.1563
	(0.0001)	(0.0001)	(0.0003)	(0.0002)	(0.0004)	(0.0003)
$\hat{E}_{\boldsymbol{\chi}}$($0.5^{\text{th}}$ quantile)	−0.0230	−0.0015	−0.1116	−0.0522	0.0591	−0.0569
	(0.0001)	(0.0001)	(0.0003)	(0.0001)	(0.0006)	(0.0002)
$\hat{E}_{\boldsymbol{\chi}}$($0.75^{\text{th}}$ quantile)	0.2148	0.1667	0.0963	0.1368	0.3123	0.0854
	(0.0001)	(0.0001)	(0.0003)	(0.0002)	(0.0004)	(0.0003)
$\hat{E}_{\boldsymbol{\chi}}$($0.95^{\text{th}}$ quantile)	0.9480	1.1873	0.7845	0.9150	1.1625	0.8653
	(0.0001)	(0.0001)	(0.0003)	(0.0003)	(0.0004)	(0.0002)
$\hat{E}_{\boldsymbol{\chi}}$(Maximum)	2.3056	1.5882	3.3751	4.7994	7.1441	0.9990
	(0.0001)	(0.0001)	(0.0005)	(0.0004)	(0.0005)	(0.0003)
$\hat{E}_{\boldsymbol{\chi}}$(Share impact > 0)	0.4626	0.4839	0.4113	0.4293	0.5693	0.3947
	(0.0000)	(0.0000)	(0.0003)	(0.0002)	(0.0018)	(0.0000)

1. Estimation results are based on drawing 100,000 permutations for each sub-population of county-pairs. Bootstrap estimates of standard errors are reported in parenthesis. They are based on 30 bootstrap replications with a sample size of about one-third of the full sample.
2. The reported above figures are based on frequency weighted averages of the respective parameters as obtained for each estimated sub-population of country-pairs. Aggregation across sub-populations is performed assuming independent observations across them.
3. For each sub-population estimation results were obtained as suggested by the procedure in Heckman, Clements, and Smith (1997) applied to the blocks defined by the respective sub-populations. However, we perform sub-sampling by directly mapping each sampled observation into the respective counterfactual distribution, except for {FF,F$\tilde{\text{B}}$} and {FF,F$\tilde{\text{U}}$}, where we follow Heckman, Clements, and Smith (1997) and rather base estimation on mapping percentile means, due to computational infeasibility of the direct approach.

Appendix

Table A.1: List of all 136 countries with information on years included in the estimation sample

	NATO code	Country name	Years in panel		
			earliest	*latest*	*number*
1	AGO	Angola	1995	1997	3
2	ALB	Albania	1996	2001	6
3	ARG	Argentina	1965	2001	37
4	ARM	Armenia	1998	2001	4
5	AUS	Australia	1965	2001	37
6	AUT	Austria	1965	2001	37
7	AZE	Azerbaijan	1998	2001	4
8	BDI	Burundi	1971	2001	31
9	BEL	Belgium	1999	2001	3
10	BEN	Benin	1997	2001	5
11	BFA	Burkina Faso	1967	2001	35
12	BGD	Bangladesh	1991	2001	11
13	BGR	Bulgaria	1995	2001	7
14	BLR	Belarus	1997	2001	5
15	BLZ	Belize	1985	2001	17
16	BOL	Bolivia	1965	2001	37
17	BRA	Brazil	1985	2001	17
18	BRB	Barbados	1972	2001	30
19	CAF	Central African Republic	1985	1999	15
20	CAN	Canada	1965	2001	37
21	CHE	Switzerland	1965	2001	37
22	CHL	Chile	1965	2001	37
23	CHN	China	1991	2001	11
24	CIV	Cte d'Ivoire	1965	2001	37
25	CMR	Cameroon	1973	2001	29
26	COG	Congo	1990	1998	9
27	COL	Colombia	1965	2001	37
28	CPV	Cape Verde	1988	2001	14
29	CRI	Costa Rica	1965	2001	37
30	CYP	Cyprus	1965	1997	33
31	CZE	Czech Republic	1998	2001	4
32	DEU	Germany	1996	2001	6
33	DMA	Dominica	1985	1997	13
34	DNK	Denmark	1965	2001	37
35	DOM	Dominican Republic	1965	2001	37
36	DZA	Algeria	1974	2001	28
37	ECU	Ecuador	1965	2001	37
38	EGY	Egypt	1965	2001	37
39	ESP	Spain	1965	2001	37
40	EST	Estonia	1997	2001	5
41	ETH	Ethiopia	1970	2001	32
42	FIN	Finland	1965	2001	37
43	FJI	Fiji	1974	2000	27
44	FRA	France	1965	2001	37
45	GAB	Gabon	1967	2001	35
46	GBR	UK and Northern Ireland	1965	2001	37
47	GEO	Georgia	2000	2001	2
48	GHA	Ghana	1969	2001	33
49	GMB	Gambia	1968	2001	34
50	GNB	Guinea-Bissau	1992	2001	10
51	GRC	Greece	1965	2001	37
52	GRD	Grenada	1981	2001	21
53	GTM	Guatemala	1965	2001	37
54	GUY	Guyana	1999	2000	2
55	HKG	China, Hong Kong	1986	2001	16
56	HND	Honduras	1965	2001	37
57	HRV	Croatia	1999	2001	3
58	HTI	Haiti	1971	1999	29
59	HUN	Hungary	1977	2001	25
60	IDN	Indonesia	1969	2001	33
61	IND	India	1965	2001	37
62	IRL	Ireland	1965	2001	37
63	IRN	Iran	1966	2001	22
64	ISL	Iceland	1965	2001	37
65	ISR	Israel	1965	2001	37

(Table continued on following page)

(Table A.1 continued)

	NATO code	Country name	Years in panel		
			earliest	*latest*	*number*
66	ITA	Italy	1965	2001	37
67	JAM	Jamaica	1965	2001	37
68	JOR	Jordan	1974	2001	28
69	JPN	Japan	1965	2001	37
70	KAZ	Kazakhstan	1998	2001	4
71	KEN	Kenya	1965	2001	37
72	KGZ	Kyrgyzstan	2000	2001	2
73	KHM	Cambodia	1999	2000	2
74	KNA	Saint Kitts and Nevis	1984	2001	18
75	KOR	Republic of Korea	1971	2001	31
76	LCA	Saint Lucia	1984	2001	18
77	LKA	Liechtenstein	1965	2001	37
78	LTU	Lithuania	1997	2001	5
79	LUX	Luxembourg	1999	2001	3
80	LVA	Latvia	1996	2001	6
81	MAR	Morocco	1965	2001	37
82	MDA	Republic of Moldova	1999	2001	3
83	MDG	Madagascar	1969	2001	33
84	MEX	Mexico	1965	2001	37
85	MKD	Macedonia	1998	2001	4
86	MLI	Mali	1993	2001	9
87	MLT	Malta	1998	1999	2
88	MOZ	Mozambique	1992	2001	10
89	MRT	Mauritania	1990	2000	11
90	MUS	Mauritius	1970	2001	32
91	MWI	Malawi	1985	2001	17
92	MYS	Malaysia	1969	2001	33
93	NER	Niger	1968	2001	34
94	NGA	Nigeria	1965	2001	34
95	NIC	Nicaragua	1977	2001	25
96	NLD	Netherlands	1965	2001	37
97	NOR	Norway	1965	2001	37
98	NPL	Nepal	1983	2001	19
99	NZL	New Zealand	1965	2001	37
100	PAK	Pakistan	1965	2001	37
101	PAN	Panama	1965	2001	37
102	PER	Peru	1965	2001	37
103	PHL	Philippines	1965	2001	37
104	PNG	Papua New Guinea	1976	2000	25
105	POL	Poland	1987	2001	15
106	PRT	Portugal	1965	2001	37
107	PRY	Paraguay	1965	2001	37
108	ROM	Romania	1995	2001	7
109	RUS	Russian Federation	1997	2001	5
110	RWA	Rwanda	1971	2001	27
111	SEN	Senegal	1972	2001	30
112	SGP	Singapore	1970	1997	28
113	SLE	Sierra Leone	1965	1997	33
114	SLV	El Salvador	1965	2001	37
115	SVK	Slovakia	1998	2001	4
116	SVN	Slovenia	1997	2001	5
117	SWE	Sweden	1965	2001	37
118	SYC	Seychelles	1975	2001	27
119	SYR	Syrian Arab Republic	1965	2001	37
120	TCD	Chad	1988	2001	14
121	TGO	Togo	1971	2001	31
122	THA	Thailand	1965	2001	37
123	TTO	Trinidad and Tobago	1965	2001	37
124	TUN	Tunisia	1988	2001	14
125	TUR	Turkey	1970	2001	32
126	TZA	United Republic of Tanzania	1990	2001	12
127	UGA	Uganda	1985	2001	17
128	UKR	Ukraine	1997	2001	5
129	URY	Uruguay	1965	2001	37
130	USA	United States of America	1965	2001	37
131	VCT	Saint Vincent and Grenadines	1981	2001	21
132	VEN	Venezuela	1965	2001	34
133	ZAF	South Africa	1965	2001	37
134	ZAR	Congo	1968	1998	28
135	ZMB	Zambia	1990	2001	12
136	ZWE	Zimbabwe	1983	2001	19

1. The above table lists countries by their NATO three-letter code.
2. The last three columns summarize some features of years included in the sample. Since we are considering a differences-in-differences cross section, note that the earliest year refers to the earliest $t = 0$ and the latest year to the latest $t = 1$.

Table A.2: Type I errors as associated with estimated sub-populations (assembled by sufficiently similar characteristics determining selection)

$\underline{s}_{ij}, \tilde{\underline{s}}_{ij}$	$\hat{R}$		$Pr(q_{50}(x_{k,ij1}) = q_{50}(x_{k,lm1})\|H_0) > \alpha$ over sub-populations $\boldsymbol{\chi}_1, \ldots, \boldsymbol{\chi}_{\hat{R}}$					
			DIST$_{ij}$	SIZE$_{ij}$	SIMI$_{ij}$	RLFAC$_{ij}$	VGDPPC$_{ij}$	VINFL$_{ij}$
FB,F$\tilde{F}$	23	Mean	0.2669	0.2558	0.2773	0.2508	0.2700	0.2650
		Standard deviation	0.1657	0.1714	0.1937	0.1684	0.1877	0.1612
		Minimum	0.1025	0.1025	0.1080	0.1024	0.1063	0.1084
		Maximum	0.7372	0.8182	0.7951	0.8182	0.8182	0.7388
FU,F$\tilde{F}$	73	Mean	0.2949	0.2742	0.2363	0.2730	0.2963	0.2561
		Standard deviation	0.2488	0.2460	0.2008	0.2476	0.2647	0.2033
		Minimum	0.1039	0.1002	0.1002	0.1002	0.1025	0.1010
		Maximum	1.0000	1.0000	0.9986	0.9995	1.0000	1.0000
FF,F$\tilde{B}$	5	Mean	0.3240	0.2203	0.4374	0.1385	0.3943	0.4614
		Standard deviation	0.2149	0.2327	0.2205	0.0427	0.2251	0.2061
		Minimum	0.1028	0.1024	0.1568	0.1014	0.1299	0.1568
		Maximum	0.6152	0.6360	0.7059	0.2040	0.6387	0.7050
FF,F$\tilde{U}$	31	Mean	0.5129	0.4306	0.2939	0.3766	0.3988	0.3860
		Standard deviation	0.3348	0.3217	0.2521	0.3002	0.3123	0.2961
		Minimum	0.1065	0.1018	0.1006	0.1006	0.1025	0.1290
		Maximum	0.9986	0.9977	0.9986	1.0000	0.9993	1.0000
FB,F$\tilde{U}$	10	Mean	0.4944	0.3836	0.3678	0.2480	0.4548	0.3811
		Standard deviation	0.3536	0.2485	0.2726	0.1975	0.3161	0.3644
		Minimum	0.1213	0.1042	0.1042	0.1082	0.1042	0.1150
		Maximum	0.9411	0.9372	0.8111	0.7098	0.9411	0.9411
FU,F$\tilde{B}$	5	Mean	0.6476	0.7350	0.5998	0.3929	0.4688	0.2968
		Standard deviation	0.2881	0.3020	0.4216	0.2719	0.3294	0.2456
		Minimum	0.1991	0.2511	0.1004	0.1742	0.2106	0.1083
		Maximum	1.0000	1.0000	1.0000	0.7761	1.0000	0.6764
BF,B$\tilde{B}$	10	Mean	0.6308	0.4711	0.4761	0.4533	0.4434	0.3979
		Standard deviation	0.2614	0.2282	0.2374	0.3044	0.2800	0.2934
		Minimum	0.1127	0.1473	0.1295	0.1225	0.1076	0.1184
		Maximum	0.9619	0.7720	0.8154	0.9188	0.7986	1.0000
BU,B$\tilde{B}$	10	Mean	0.1773	0.3432	0.3537	0.3111	0.2791	0.2886
		Standard deviation	0.0693	0.2538	0.2468	0.1705	0.1380	0.1305
		Minimum	0.1004	0.1042	0.1595	0.1042	0.1310	0.1310
		Maximum	0.3173	0.8807	0.9479	0.6373	0.5054	0.5054
BB,B$\tilde{F}$	8	Mean	0.4938	0.4817	0.3817	0.4131	0.4529	0.4486
		Standard deviation	0.2746	0.2496	0.1985	0.2681	0.3583	0.2510
		Minimum	0.1686	0.1559	0.1511	0.1452	0.1274	0.1078
		Maximum	0.9682	0.8674	0.6229	0.8674	0.9938	0.8105
BB,B$\tilde{U}$	5	Mean	0.4658	0.2464	0.6215	0.5107	0.4808	0.7015
		Standard deviation	0.2281	0.2234	0.3647	0.4382	0.3206	0.2748
		Minimum	0.1510	0.1105	0.2193	0.1134	0.1556	0.3341
		Maximum	0.7018	0.6415	0.9866	0.9866	0.9740	0.9913
BF,B$\tilde{U}$	8	Mean	0.4576	0.4163	0.5181	0.5219	0.4226	0.2975
		Standard deviation	0.3111	0.3606	0.3630	0.3974	0.2713	0.2620
		Minimum	0.1025	0.1025	0.1025	0.1025	0.1025	0.1025
		Maximum	1.0000	1.0000	1.0000	1.0000	0.9762	0.8350
BU,B$\tilde{F}$	10	Mean	0.4554	0.3585	0.4184	0.3275	0.3186	0.2405
		Standard deviation	0.3380	0.3001	0.3437	0.2552	0.2456	0.1647
		Minimum	0.1025	0.1025	0.1025	0.1025	0.1255	0.1025
		Maximum	1.0000	1.0000	1.0000	1.0000	0.9617	0.6101
UF,U$\tilde{U}$	36	Mean	0.2941	0.2976	0.4575	0.3430	0.4401	0.4256
		Standard deviation	0.2278	0.2752	0.3307	0.2542	0.3243	0.2796
		Minimum	0.1025	0.1025	0.1025	0.1021	0.1025	0.1025
		Maximum	0.9510	0.9896	1.0000	0.9866	1.0000	0.9906
UB,U$\tilde{U}$	4	Mean	0.4986	0.2586	0.4345	0.2133	0.4877	0.3502
		Standard deviation	0.2188	0.2641	0.3168	0.1017	0.4223	0.4182
		Minimum	0.1931	0.1204	0.1456	0.1352	0.1148	0.1210
		Maximum	0.7108	0.6546	0.7576	0.3558	0.9990	0.9771
UU,U$\tilde{F}$	35	Mean	0.3689	0.2601	0.5044	0.3249	0.4952	0.4463
		Standard deviation	0.2685	0.2124	0.3076	0.2440	0.3160	0.2987
		Minimum	0.1015	0.1026	0.1087	0.1103	0.1213	0.1103
		Maximum	0.9304	0.9708	1.0000	0.9590	1.0000	1.0000
UU,U$\tilde{B}$	2	Mean	0.3660	0.4992	0.5934	0.3684	0.5950	0.5504
		Standard deviation	0.0673	0.2556	0.5737	0.0639	0.1200	0.6358
		Minimum	0.3185	0.3185	0.1878	0.3232	0.5101	0.1009
		Maximum	0.4136	0.6799	0.9991	0.4136	0.6799	1.0000
UF,U$\tilde{B}$	5	Mean	0.3804	0.3449	0.5306	0.3003	0.4033	0.4718
		Standard deviation	0.3757	0.2274	0.3897	0.3399	0.3551	0.4554
		Minimum	0.1365	0.1410	0.1365	0.1368	0.1365	0.1365
		Maximum	1.0000	0.6197	1.0000	0.9079	1.0000	1.0000
UB,U$\tilde{F}$	9	Mean	0.3423	0.3640	0.3880	0.3694	0.4194	0.4466
		Standard deviation	0.3754	0.2947	0.3015	0.3101	0.3354	0.3532
		Minimum	0.1038	0.1200	0.1056	0.1056	0.1348	0.1348
		Maximum	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000

1. Note that the above table is supplementary to Table 8.
2. The reported number of sub-populations $\hat{R}$ refers to those intervals where the grid search algorithm employed converged to a subinterval with at least one treated and at least two untreated observations to ensure the identification requirement for sub-population-specific local constant regression estimates of the mean treatment parameter on the treated. The minimum cutoff probability is set to $\alpha = 0.1$.

Table A.3.a: Monte-Carlo cutoff probabilities of a type I error under homogeneous treatment effects model conditional on observable characteristics according to Table 10 (local linear estimator)

	Initial state exchange rate regime is *freely floating*					
P-value	FB,F$\tilde{F}$	FU,F$\tilde{F}$	FF,F$\tilde{B}$	FF,F$\tilde{U}$	FB,F$\tilde{U}$	FU,F$\tilde{B}$
0.5	0.003588	0.088780	1.745033	7.087390	0.022410	1.685495
0.4	0.004309	0.144315	1.911719	15.673199	0.028710	2.080119
0.3	0.007785	0.172653	2.473584	15.840049	0.035864	2.613189
0.2	0.009854	0.211893	4.306368	20.862979	0.042652	3.382499
0.15	0.010408	0.230870	4.540957	21.018914	0.047554	3.946179
0.1	0.011078	0.250529	6.263743	22.061207	0.060944	4.727414
0.05	0.023859	0.272455	6.439683	22.310780	0.072659	6.065987
0.01	0.026175	0.299986	8.172912	27.318576	0.096814	9.024355
0.001	0.027351	0.324312	8.187630	27.569707	0.116209	12.913621
0.0001	0.027926	0.343707	8.195470	27.830175	0.132764	15.453836
	Initial state exchange rate regime is *currency band*					
P-value	BF,B$\tilde{B}$	BU,B$\tilde{B}$	BB,B$\tilde{F}$	BB,B$\tilde{U}$	BF,B$\tilde{U}$	BU,B$\tilde{F}$
0.5	0.017642	0.053711	0.886459	0.033915	0.194762	13.616267
0.4	0.023799	0.061991	1.050672	0.126169	0.236127	14.335145
0.3	0.027589	0.070035	1.252895	0.149808	0.263848	19.851686
0.2	0.030885	0.081569	1.529680	0.177528	0.303403	26.171081
0.15	0.032785	0.090530	1.720614	0.192171	0.337662	27.836446
0.1	0.035130	0.160745	1.983591	0.208517	0.377798	31.158695
0.05	0.038784	0.182366	2.419126	0.233524	0.468652	33.125687
0.01	0.047495	0.205122	3.361928	0.273186	0.605699	41.571677
0.001	0.056536	0.221639	4.554662	0.313049	0.672234	44.311811
0.0001	0.061888	0.231844	5.510080	0.365844	0.715352	46.003185
	Initial state exchange rate regime is *currency peg or union*					
P-value	UF,U$\tilde{U}$	UB,U$\tilde{U}$	UU,U$\tilde{F}$	UU,U$\tilde{B}$	UF,U$\tilde{B}$	UB,U$\tilde{F}$
0.5	0.161905	0.000633	0.251999	0.000240	0.145377	1.894255
0.4	0.183952	0.000724	0.316621	0.000276	0.248689	15.825827
0.3	0.212000	0.000829	0.373657	0.000318	0.351624	16.462965
0.2	0.255887	0.000965	0.426936	0.000373	0.422037	17.094595
0.15	0.286277	0.001057	0.457470	0.000410	0.459277	17.401181
0.1	0.322771	0.001183	0.494627	0.000461	0.496720	17.847468
0.05	0.371431	0.001385	0.548335	0.000540	0.546789	18.233908
0.01	0.464020	0.001841	0.653986	0.000682	0.633481	19.208048
0.001	0.586643	0.002373	0.789634	0.000858	0.729961	19.970333
0.0001	0.658769	0.002736	0.900501	0.001042	0.794562	20.163348

1. For each estimated sub-population, the reported above Monte-Carlo cutoff values were estimated as suggested by Heckman, Smith, and Clements (1997). (They give a detailed description in their Appendix E.)

2. The given estimates are based on 100,000 random samples drawn from the blocks of imputed control observations as defined by the respective sub-populations. Hence, for each block the size of each random sample is about one third the size of the actual sample size. As in analogy to Heckman, Smith, and Clements (1997), also treated observations are generated by sampling from the imputed control sample and then adding a constant in order to simulate the distribution of the test-statistic under the null hypothesis.

3. Hence, for each Monte-Carlo draw the lower bound for the sub-population-specific standard deviation is calculated (assuming perfect positive dependence among marginals conditional on observable characteristics).

4. Finally, the respective quantiles of the simulated distribution of the test-statistic under the null are assigned the corresponding probabilities as reported in the above table.

Table A.3.b: Monte-Carlo cutoff probabilities of a type I error under homogeneous treatment effects model conditional on observable characteristics according to Table 10 (local linear regression adjusted estimator)

	Initial state exchange rate regime is *freely floating*					
P-value	FB,F$\tilde{F}$	FU,F$\tilde{F}$	FF,F$\tilde{B}$	FF,F$\tilde{U}$	FB,F$\tilde{U}$	FU,F$\tilde{B}$
0.5	0.003249	0.089344	1.744333	7.445375	0.022415	1.742154
0.4	0.003821	0.143735	1.910408	16.013042	0.028029	2.157002
0.3	0.007584	0.172988	2.471141	16.174916	0.035301	2.717412
0.2	0.009433	0.211269	4.294380	21.281581	0.042336	3.513874
0.15	0.009822	0.228961	4.533644	21.433316	0.047327	4.098593
0.1	0.010929	0.248568	6.253418	22.501497	0.058592	4.928729
0.05	0.021787	0.271635	6.429297	22.745029	0.070225	6.350460
0.01	0.024634	0.300211	8.160419	27.849089	0.094288	9.445191
0.001	0.025401	0.325216	8.176291	28.092911	0.110311	13.236050
0.0001	0.025892	0.343356	8.185038	28.405210	0.121168	16.790024
	Initial state exchange rate regime is *currency band*					
P-value	BF,B$\tilde{B}$	BU,B$\tilde{B}$	BB,B$\tilde{F}$	BB,B$\tilde{U}$	BF,B$\tilde{U}$	BU,B$\tilde{F}$
0.5	0.018349	0.067016	1.065828	0.035047	0.142281	6.188985
0.4	0.022019	0.076207	1.302415	0.149666	0.163338	6.831437
0.3	0.025161	0.084535	1.606460	0.173358	0.189195	8.807162
0.2	0.028312	0.097123	1.957509	0.194380	0.220492	11.969711
0.15	0.030210	0.107188	2.183693	0.206682	0.245895	12.875629
0.1	0.032635	0.220257	2.492642	0.221216	0.292678	13.986477
0.05	0.036357	0.242293	3.001361	0.242649	0.373395	15.401100
0.01	0.045624	0.267892	4.028799	0.284503	0.464557	19.124707
0.001	0.054525	0.287514	5.133432	0.326049	0.548122	21.346663
0.0001	0.059238	0.297785	6.257850	0.367674	0.614857	22.939759
	Initial state exchange rate regime is *currency peg or union*					
P-value	UF,U$\tilde{U}$	UB,U$\tilde{U}$	UU,U$\tilde{F}$	UU,U$\tilde{B}$	UF,U$\tilde{B}$	UB,U$\tilde{F}$
0.5	0.164033	0.000642	0.241045	0.000271	0.165802	1.433355
0.4	0.187218	0.000741	0.292670	0.000317	0.210747	15.642348
0.3	0.216158	0.000857	0.338450	0.000366	0.273589	16.604562
0.2	0.261545	0.001013	0.382870	0.000433	0.359517	16.921856
0.15	0.292160	0.001120	0.409342	0.000480	0.420033	17.256995
0.1	0.328559	0.001271	0.440785	0.000551	0.491771	17.891317
0.05	0.378451	0.001519	0.485911	0.000666	0.573272	18.159828
0.01	0.469380	0.002070	0.569899	0.000838	0.679153	19.171004
0.001	0.597057	0.002705	0.670463	0.001001	0.785365	19.588519
0.0001	0.657588	0.003237	0.757224	0.001159	0.873714	19.697337

1. For each estimated sub-population, the reported above Monte-Carlo cutoff values were estimated as suggested by Heckman, Smith, and Clements (1997). (They give a detailed description in their Appendix E.)

2. The given estimates are based on 100,000 random samples drawn from the blocks of imputed control observations as defined by the respective sub-populations. Hence, for each block the size of each random sample is about one third the size of the actual sample size. As in analogy to Heckman, Smith, and Clements (1997), also treated observations are generated by sampling from the imputed control sample and then adding a constant in order to simulate the distribution of the test-statistic under the null hypothesis.

3. Hence, for each Monte-Carlo draw the lower bound for the sub-population-specific standard deviation is calculated (assuming perfect positive dependence among marginals conditional on observable characteristics).

4. Finally, the respective quantiles of the simulated distribution of the test-statistic under the null are assigned the corresponding probabilities as reported in the above table.

Table A.3.c: Monte-Carlo cutoff probabilities of a type I error under homogeneous treatment effects model conditional on observable characteristics according to Table 10 (local constant estimator)

Initial state exchange rate regime is *freely floating*

P-value	FB,F$\tilde{\text{F}}$	FU,F$\tilde{\text{F}}$	FF,F$\tilde{\text{B}}$	FF,F$\tilde{\text{U}}$	FB,F$\tilde{\text{U}}$	FU,F$\tilde{\text{B}}$
0.5	0.001266	0.031926	0.000703	0.182605	0.011356	0.146495
0.4	0.001483	0.036003	0.000813	0.214611	0.014044	0.174132
0.3	0.001707	0.041120	0.000955	0.259694	0.022909	0.213248
0.2	0.001958	0.050409	0.001156	0.333166	0.028078	0.252635
0.15	0.002114	0.055808	0.001304	0.383729	0.031054	0.277329
0.1	0.002332	0.064193	0.001514	0.452177	0.034160	0.312717
0.05	0.004261	0.072999	0.001879	0.565123	0.039519	0.370234
0.01	0.005491	0.085332	0.002654	0.761456	0.055437	0.489905
0.001	0.006106	0.093992	0.003659	0.959717	0.066798	0.636520
0.0001	0.006455	0.100132	0.004380	1.090795	0.070772	0.746074

Initial state exchange rate regime is *currency band*

P-value	BF,B$\tilde{\text{B}}$	BU,B$\tilde{\text{B}}$	BB,B$\tilde{\text{F}}$	BB,B$\tilde{\text{U}}$	BF,B$\tilde{\text{U}}$	BU,B$\tilde{\text{F}}$
0.5	0.014692	0.021406	1.003787	0.018034	0.184550	0.070136
0.4	0.023977	0.025303	1.028886	0.021364	0.214208	0.131800
0.3	0.027759	0.029767	1.055923	0.025345	0.247276	0.141551
0.2	0.030855	0.034060	1.562160	0.030472	0.296751	0.150032
0.15	0.032625	0.037103	1.683084	0.033951	0.322900	0.155004
0.1	0.034748	0.041006	1.734107	0.038788	0.360853	0.161022
0.05	0.037983	0.046259	1.859979	0.046098	0.453426	0.170198
0.01	0.043868	0.056383	2.093900	0.059342	0.590190	0.189097
0.001	0.049221	0.068143	2.117615	0.074689	0.677024	0.212424
0.0001	0.053433	0.076358	2.123738	0.087303	0.744728	0.232674

Initial state exchange rate regime is *currency peg or union*

P-value	UF,U$\tilde{\text{U}}$	UB,U$\tilde{\text{U}}$	UU,U$\tilde{\text{F}}$	UU,U$\tilde{\text{B}}$	UF,U$\tilde{\text{B}}$	UB,U$\tilde{\text{F}}$
0.5	0.129411	0.000481	0.241705	0.000158	0.043357	0.325104
0.4	0.147112	0.000547	0.259594	0.000185	0.048719	0.858443
0.3	0.168008	0.000625	0.281029	0.000218	0.054747	0.936748
0.2	0.192361	0.000728	0.309075	0.000262	0.062118	1.011051
0.15	0.206124	0.000797	0.328636	0.000292	0.066596	1.043352
0.1	0.226563	0.000889	0.355161	0.000335	0.072331	1.093898
0.05	0.257220	0.001039	0.399977	0.000406	0.080855	1.167196
0.01	0.312166	0.001375	0.504750	0.000566	0.098231	1.301958
0.001	0.364009	0.001813	0.650699	0.000775	0.119293	1.446719
0.0001	0.394360	0.002097	0.807999	0.001000	0.137826	1.480472

1. For each estimated sub-population, the reported above Monte-Carlo cutoff values were estimated as suggested by Heckman, Smith, and Clements (1997). (They give a detailed description in their Appendix E.)

2. The given estimates are based on 100,000 random samples drawn from the blocks of imputed control observations as defined by the respective sub-populations. Hence, for each block the size of each random sample is about one third the size of the actual sample size. As in analogy to Heckman, Smith, and Clements (1997), also treated observations are generated by sampling from the imputed control sample and then adding a constant in order to simulate the distribution of the test-statistic under the null hypothesis.

3. Hence, for each Monte-Carlo draw the lower bound for the sub-population-specific standard deviation is calculated (assuming perfect positive dependence among marginals conditional on observable characteristics).

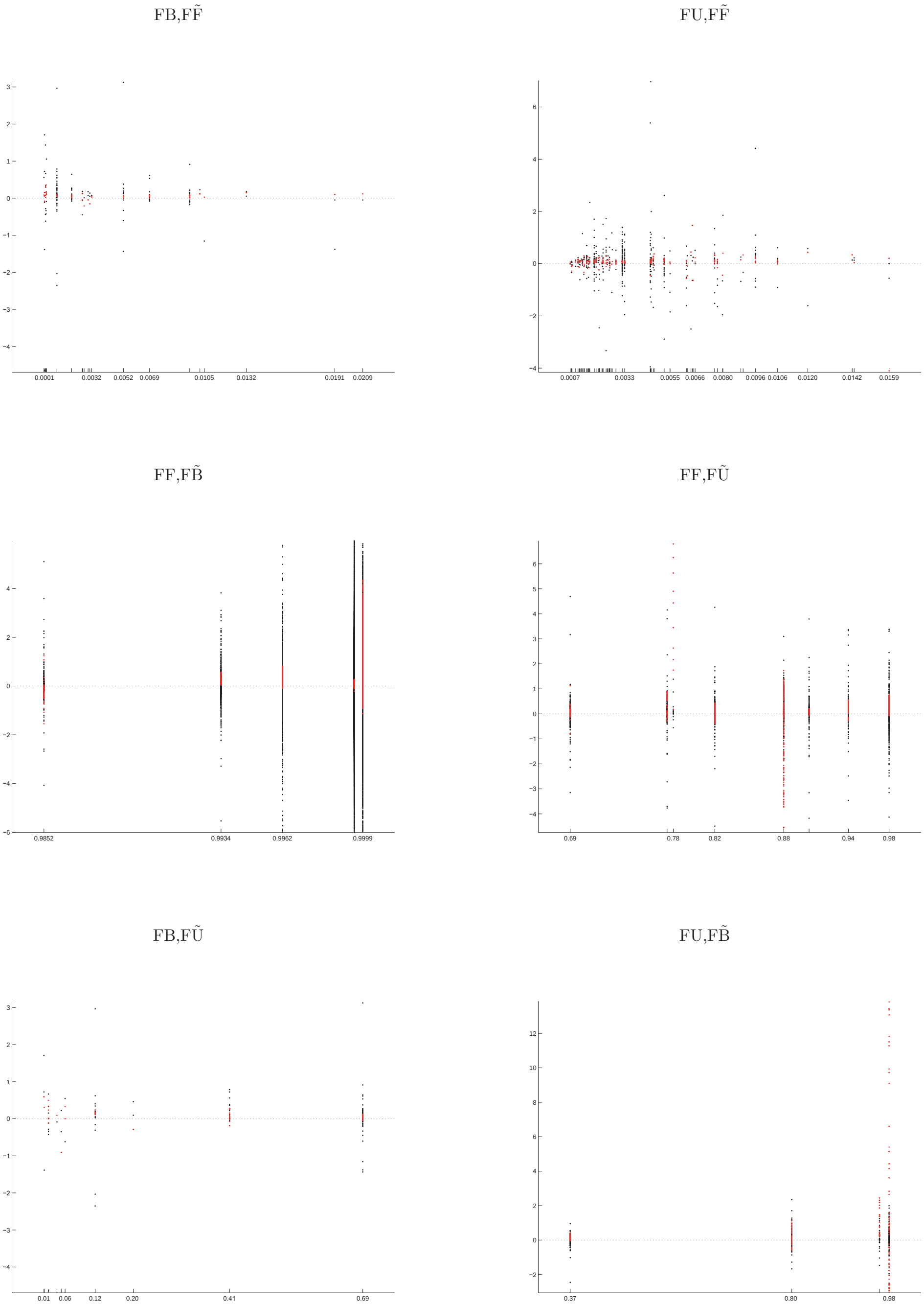
4. Finally, the respective quantiles of the simulated distribution of the test-statistic under the null are assigned the corresponding probabilities as reported in the above table.

Table A.4: Test for heterogeneous treatment effects on the treated conditional on observable characteristics (robust version)

	Initial state exchange rate regime is *freely floating*					
	FB,F$\tilde{F}$	FU,F$\tilde{F}$	FF,F$\tilde{B}$	FF,F$\tilde{U}$	FB,F$\tilde{U}$	FU,F$\tilde{B}$
Local linear matching	4.4325***	38.4077***	4.4076	27.7566***	1.5273***	9.1459***
Local linear regression adjusted matching	4.4631***	38.5102***	4.4413	27.7425***	1.5377***	9.3761***
Local constant matching	4.4344***	38.4245***	2.2350***	22.2173***	1.5421***	0.7752***
	Initial state exchange rate regime is *currency band*					
	BF,B$\tilde{B}$	BU,B$\tilde{B}$	BB,B$\tilde{F}$	BB,B$\tilde{U}$	BF,B$\tilde{U}$	BU,B$\tilde{F}$
Local linear matching	1.8370***	0.7083***	5.4662***	1.6320***	0.5034***	7.2517***
Local linear regression adjusted matching	1.8473***	0.7321***	5.3076***	1.7380***	0.4194***	3.5912***
Local constant matching	1.8562***	0.7402***	2.4357***	2.1051***	0.6794***	0.4728***
	Initial state exchange rate regime is *currency peg or union*					
	UF,U$\tilde{U}$	UB,U$\tilde{U}$	UU,U$\tilde{F}$	UU,U$\tilde{B}$	UF,U$\tilde{B}$	UB,U$\tilde{F}$
Local linear matching	6.0222***	0.9058***	3.5101***	0.6584***	1.0009***	0.3468***
Local linear regression adjusted matching	6.0481***	0.9293***	3.5892***	0.7079***	1.1792***	0.3690***
Local constant matching	6.1773***	0.9180***	9.4416***	0.7192***	2.2471***	0.3587***

1. Under the null hypothesis of homogeneous treatment effects, once we know $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$, the lower bound for the standard deviation of the distribution of treatment impacts should not be significantly different from zero for each of the estimated sub-populations. Hence, the reported significance indicates whether those estimates are statistically different from zero, and therefore rejection of the null indicates heterogeneous treatment effects on the treated conditional on $\boldsymbol{x}_{ij1} \in \boldsymbol{\chi}_r$.
2. The above reported numbers are the sub-population sums over the estimates for the lower bound of the standard deviation for the distribution of the treatment effect on the treated with respect to the compared outcomes. Estimation results were obtained as suggested by the procedure in Heckman, Smith, and Clements (1997) accommodated for the need to control for within-sub-population differences in the conditioning sets. Hence, we base computation of the test statistic on the variation in the estimated treatment effects on the treated net off the variation explained by deviations from the vector of sub-population medians. Hence, the above results are based on the therefore obtained residual variation.
3. As suggested in Heckman, Smith, and Clements (1997), we estimate Monte-Carlo cutoff values for rejection of the null stated in expression (2) in Section 3.4.3. The estimated values are given in the Appendix.

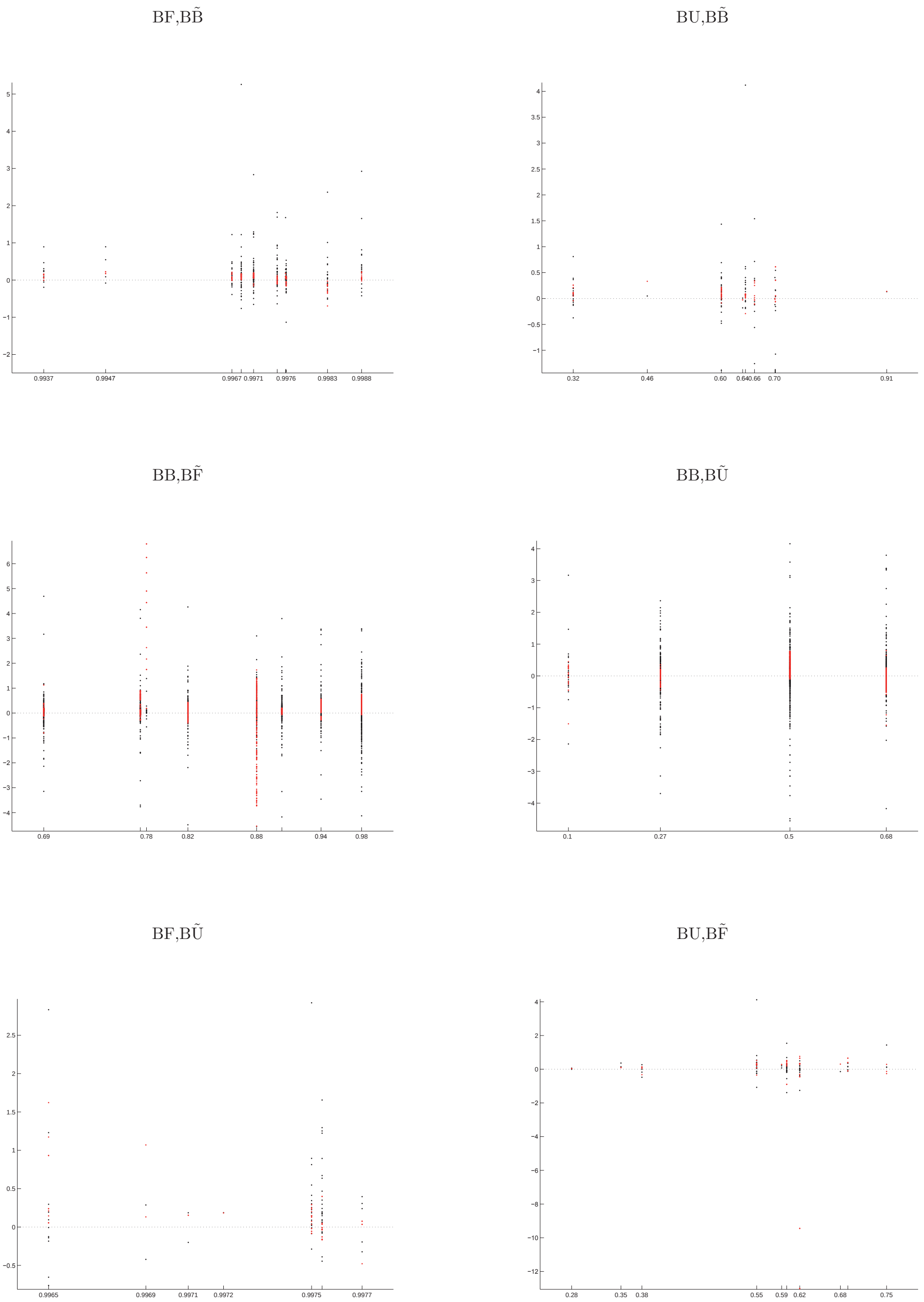
Figure A.1.a: Sub-population-specific potential outcomes when initial state exchange rate regime is *freely floating*



1. Observed sequence $\underline{s}_{ij}$ in black and imputed control observations for sequence $\underline{\tilde{s}}_{ij}$ in red; control observations imputed by local linear propensity score matching.

2. The abscissa plots the conditional probabilities to receive treatment evaluated at the vector of median characteristics as associated with the respective sub-population as summarized within Table 8.

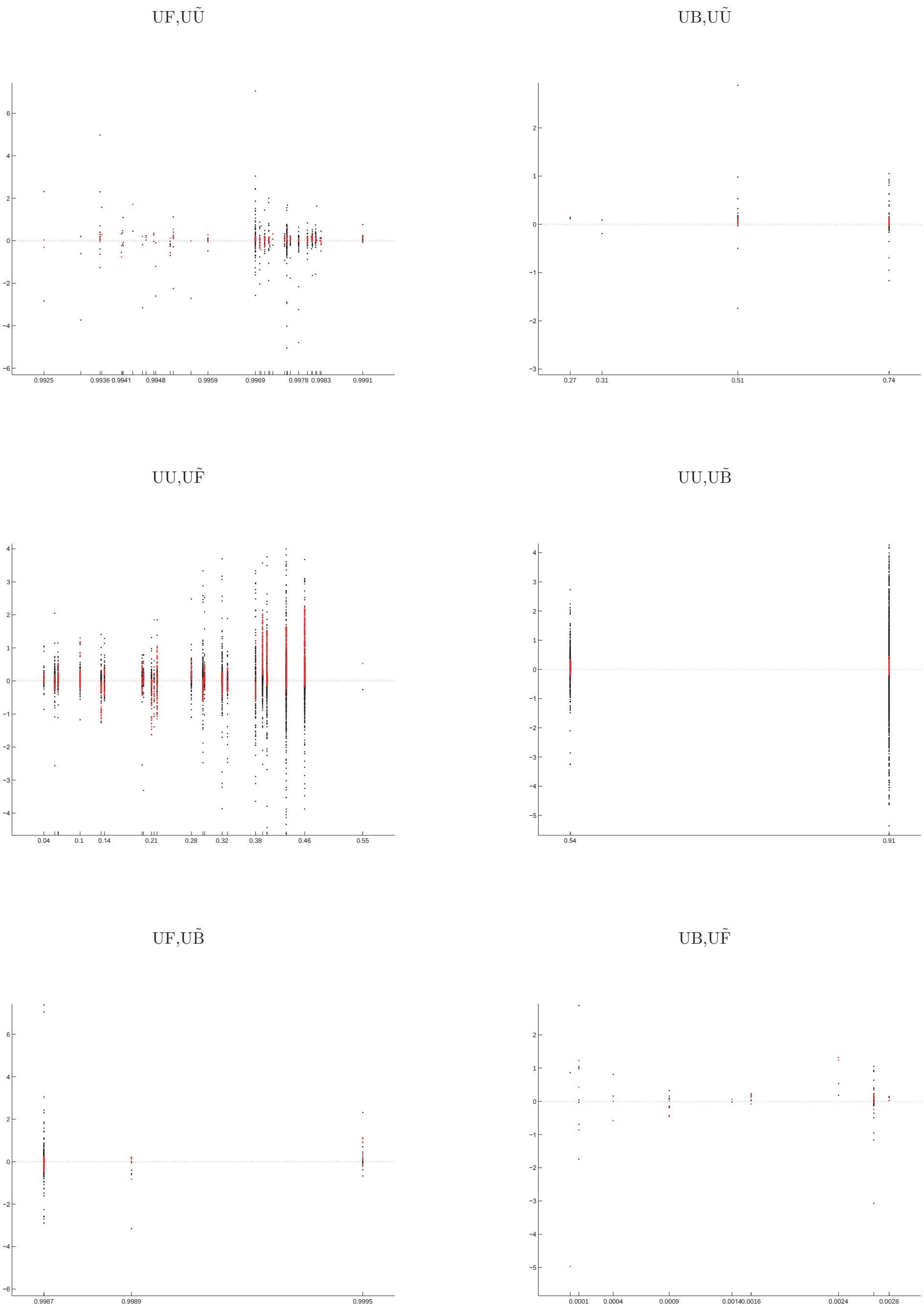
Figure A.1.b: Sub-population-specific potential outcomes when initial state exchange rate regime is *currency band*



1. Observed sequence $\underline{s}_{ij}$ in black and imputed control observations for sequence $\tilde{\underline{s}}_{ij}$ in red; control observations imputed by local linear propensity score matching.

2. The abscissa plots the conditional probabilities to receive treatment evaluated at the vector of median characteristics as associated with the respective sub-population as summarized within Table 8.

Figure A.1.c: Sub-population-specific potential outcomes when initial state exchange rate regime is *currency peg or union*



1. Observed sequence $\underline{s}_{ij}$ in black and imputed control observations for sequence $\underline{\tilde{s}}_{ij}$ in red; control observations imputed by local linear propensity score matching.

2. The abscissa plots the conditional probabilities to receive treatment evaluated at the vector of median characteristics as associated with the respective sub-population as summarized within Table 8.