The Euro Effect on Trade is not as Large as Commonly Thought

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Abstract

Existing studies on the impact of the euro on goods trade report increments between 5% and 40%. These estimates are based on standard panel gravity models for the level of trade. We show that the residuals from these models exhibit upwards trends over time for the euro countries, and that this leads to an upward bias in the estimated euro effect. To correct for that, we extend the standard model by including a time trend that may have different effects across country-pairs. This results in an estimated euro impact of only 3%.

Key words: currency union, dynamic OLS, EMU, gravity model, panel data, robust standard errors, time trend.
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1 Introduction

Since the introduction of the euro in 1999, several studies have estimated the impact of the euro on bilateral goods trade within the euro zone. Micco, Stein and Ordoñez (2003) estimate an increase between 5 and 20 percent, Flam and Nordström (2003) report estimates between 8% and 15%, Barr, Breedon and Miles (2003) calculate 29% and Bun and Klaassen (2002) report an increase of 38% for the trade effect of the euro.[1] This suggests that the euro effect is positive and lies in the range of 5% to 40%.

These estimates are useful for evaluating the benefits of the euro for existing euro zone countries (European Commission, 2003). They have also affected the debate in non-euro countries on whether to join the euro (HM Treasury, 2003, for the U.K.). Also for the new European Union (EU) members the potential trade benefits of adopting the euro are relevant.

Because of this policy relevance, it is important to verify the robustness of the current euro estimates: do they really represent the impact of the euro, or are they driven by something else? The models that are typically used are all quite similar in the sense that they essentially explain trade by income and a euro dummy (which is one if the countries involved have the euro), while correcting for some other factors. It might be that some variables omitted from this common modelling approach have led to bias in all estimated trade benefits given earlier.

An indication of such omitted variables bias follows from the variation in the estimates given earlier in relation to the number of time periods in the sample. Micco et al. (2003) and Flam and Nordström (2003) find the lowest estimates using data over about 1992-2002, Barr et al. (2003) derive the middle estimate from data over 1978-2002, and Bun and Klaassen (2002) report the largest one from data over 1965-2001. Therefore, the longer the data period, the higher the euro estimate. This is difficult to explain from an economic point of view. Because in longer samples the time series characteristics of the variables involved usually have more impact, we suspect the euro estimate to be biased by some misspecification of the time series characteristics of trade.

In this study we investigate a particularly important time series characteristic, namely the trends in trade flows over time. It is well known from the time series literature that trend misspecification can lead to substantial bias. We examine whether euro estimates are biased upwards, because the euro dummy (which is one only at the end of the sample) picks up increasing trends in trade that are actually caused by omitted variables.

[1] More precisely, by the euro effect on trade we mean the trade effect of entering stage three of the Economic and Monetary Union (EMU).

To some extent omitted trending variables bias is already avoided in the studies mentioned earlier. After all, they include an income regressor and other trending variables to explain the trend in trade. Moreover, they use panel data so that they can include time effects to correct for any residual trend common to all bilateral trade flows.

However, trending behavior of trade flows may also be affected by variables not included in the specification and trends may vary across country-pairs, both due to country-specific and country-pair specific factors. Examples of such factors can be derived from the generalized gravity equation of Bergstrand (1989), which is particularly interesting because the models in the existing euro studies are closely related to this model. Although Bergstrand's model is a one-period model, let us imagine that it holds several periods after each other. Bergstrand derives that trade depends on productivity parameters, capital/labor ratios, transport costs, tariffs, among other things. The first two factors are nation specific, and they tend to increase over time, leading to country-specific trends in trade. Transport costs depend on distance and the goods composition of trade, which are both different across country-pairs. Because transport costs have decreased over time, the transport cost term in the gravity model is one source of country-pair specific trend growth in trade. The tariff term in the model is another source, because trade liberalization usually occurs gradually and at different speeds across country-pairs. Because such variables, and potentially many others, such as telecommunications costs and the trade costs mentioned by Anderson and van Wincoop (2004), are not included in existing models used to estimate the euro impact, it is unlikely that the standard trend corrections mentioned earlier are sufficient to completely avoid omitted trending variables bias.

To correct for this bias, one could include proxies for the variables just mentioned. However, these may be difficult to construct, and it is unlikely that one can find proxies to capture all omitted trending variables. The way we correct for this is based on the fact that a major part of their signal is the deterministic time trend, or drift term. Hence, in our panel model we add the time variable t and, to account for both country and country-pair trending variables, we allow it to have heterogeneous coefficients across country pairs. This extension is novel in the gravity literature, but has already been used elsewhere (Cornwell, Schmidt and Sickles, 1990, and Mark and Sul, 2003, among others).

The set up of the paper is as follows. In the next section we discuss an empirical panel gravity model including country-pair specific time trends. In Section 3 the estimation results are presented, both for a dataset involving euro data (to study whether existing euro estimates are biased upwards) and for the Glick and Rose (2002) sample, which has data on non-euro currency unions (to study the robustness of the typical finding that such currency unions increase bilateral trade a lot, namely by about 90%). In Section 4 we examine the robustness of the main findings. The final section concludes.

2 A gravity model with country-pair specific time trends

2.1 Model and estimation

The panel gravity model occurs in several variants. Rose (2000) explains bilateral trade (exports plus imports) by national income of both countries, their incomes per capita, free trade area and currency union dummies, and time-invariant variables such as distance. Glick and Rose (2002) extend the model by using fixed country-pair specific intercepts to correct for all time-invariant trade determinants, and in a robustness check they also include fixed time effects to account for all country-pair invariant variables. Micco et al. (2003) and Barr et al. (2003) use a similar model to examine the currency union in Europe.

The model of Bun and Klaassen (2002), also used by Flam and Nordström (2003), is somewhat different from that of Glick and Rose (2002), as it takes exports instead of trade as dependent variable. Moreover, in contrast to the other studies, Bun and Klaassen (2002) account for the dynamics in trade data by including lagged dependent and explanatory variables besides contemporaneous values.

These latter two differences are not important for the point we want to make in the current study, because our conclusions turn out to be essentially the same for models with trade or exports as dependent variable and for static or dynamic panel data models (see Section 4.1). For the sake of comparison, the benchmark model in the present study is therefore based on the static panel gravity model for trade used by Glick and Rose (2002). We now describe that model in more detail and generalize it by introducing the country-pair specific time trends.

The dependent variable is $TRADE_{ijt}$, the logarithm of real bilateral trade between countries i and j in year t, where real bilateral trade is the sum of nominal bilateral exports and imports, both in U.S. dollars, divided by the U.S. producer price index.[2] The first explanatory variable is the log of the product of the countries' real GDP, both expressed in U.S. output; it is denoted by GDP_{ijt}. We also include the log of GDP per

[2] This is the common approach in the currency union literature. This focus on the U.S. is not important for our main conclusion, because Section 4.1 shows that using real exports measured in exporter's output as dependent variable leads to a similar conclusion.

capita ($GDPCAP_{ijt}$), which is GDP_{ijt} minus the log of the product of the countries' population sizes. To measure the euro effect, we follow existing studies by including $EURO_{ijt}$, a dummy that is one if i and j have the euro in year t (hence it can only be one from 1999 onwards). Thus, we model the euro impact as a permanent break in the level of trade for the euro country pairs (in Section 4.1 we show that our results are robust to somewhat more sophisticated approaches). To correct for trade increases from free trade area arrangements, we include a dummy FTA_{ijt} that is one in case the countries have free trade with each other. Finally, we account for the effects of all possible time-invariant determinants of trade (such as distance) by a fixed "individual" effect η_{ij} for country-pair ij, and we use a fixed time effect λ_t to correct for the impact of all possible country-pair invariant trade determinants (such as the state of the world economy). These are all quite standard elements and definitions in panel gravity models nowadays, and we follow these choices to ensure that our results can be easily compared to the existing literature.

Apart from the aforementioned set of regressors, there may be many other trade determinants. A particularly important group of variables may be the group of trending trade determinants other than GDP_{ijt} and $GDPCAP_{ijt}$, because trends are strong signals so that leaving them out of the model may have substantial effects on the results. A subset of all trending determinants, the country-pair invariant ones, are accounted for by the time effects λ_t. To extend the standard model, we thus concentrate on the country and country-pair specific ones, such as factor productivity, capital/labor ratios, transportation costs and tariffs. To simplify terminology, we will refer to all such variables as "country-pair" variables.

To approximate the impact of all country-pair specific omitted trending variables, we focus on one of their main characteristics, the time trend t, as motivated in the introduction. We therefore ignore other potentially relevant characteristics, such as stochastic trends. Incorporating such refinements would go beyond the purpose of this study. The country-pair dependence of the trend effects is represented by τ_{ij}. These effects are considered to be fixed (instead of random), just as η_{ij} and λ_t.

This results in

$$\begin{aligned} TRADE_{ijt} &= \beta_1 GDP_{ijt} + \beta_2 GDPCAP_{ijt} + \delta_1 EURO_{ijt} + \delta_2 FTA_{ijt} \\ &\quad + \eta_{ij} + \tau_{ij} \cdot t + \lambda_t + \varepsilon_{ijt}, \end{aligned} \tag{1}$$

where ε_{ijt} is allowed to be heteroskedastic (across country-pairs and time), serially correlated and cross-sectionally correlated (both contemporaneous and lagged). We assume that ε_{ijt} is stationary and treat all regressors as strictly exogenous with respect to ε_{ijt} (Sections 4.3 and 4.4 show that these assumptions are not important for the main conclusion of our study).

The parameter of interest is δ_1, which represents the impact of the euro on trade between euro member states. The difference in their trade before and after the introduction of the euro is used to identify δ_1.

We estimate model (1) by least-squares after transforming away the nuisance effects η_{ij}, $\tau_{ij} \cdot t$ and λ_t. This is an LSDV (least-squares dummy variables) type approach. Note that the standard within transformation to wipe out η_{ij}, which subtracts country-pair specific means over time from each variable, does not work here, because that will not remove $\tau_{ij} \cdot t$. To nevertheless wipe out $\tau_{ij} \cdot t$, we use the fact that the within transformation is actually a projection of all variables on the null-space of the matrix of dummy variables corresponding to all η_{ij}; see Wansbeek and Kapteyn (1989). We apply this projection argument to our model, so that we project all variables in model (1) on the null-space of the matrix of dummy/time variables corresponding to all η_{ij}, $\tau_{ij} \cdot t$ and λ_t. This transforms away all fixed effects.

To compute standard errors that are robust to heteroskedasticity as well as serial and cross-sectional correlation, we follow Driscoll and Kraay (1998) in combination with Newey and West (1987, 1994).[3]

2.2 Comparison with existing approaches

The model commonly used to estimate the euro effect is the special case of (1) where $\tau_{ij} = 0$ for all country-pairs. If there happen to be no omitted trending regressors in reality (the true value of τ_{ij} is 0), the estimated δ_1 in the general model will be equal to that of the standard model on average (although the standard error will be larger). Hence, the fact that we leave τ_{ij} unrestricted does not cause a bias of the estimated euro effect.

Another difference with existing models concerns the moments of the innovations ε_{ijt}. One usually allows for arbitrary heteroskedasticity of ε_{ijt} across country-pairs and time. However, because of, for instance, entrance and exit barriers to trade due to sunk costs and habit formation among consumers, past trade presumably has an impact on current trade that is not captured by the regressors and effects in model (1);

[3] In essence, the method takes the sample moment conditions on which the least-squares procedure is based, averages them across country-pairs so that a single time series results, and computes a heteroskedasticity and autocorrelation consistent variance matrix for that series (we take the Newey and West (1987) algorithm with the Newey and West (1994) optimal lag selection rule, which results in one lag). This gives a robust estimate of the long run variance of the moment conditions. As usual, pre- and postmultiplication by the sample second moment of the regressor vector (Hessian) then gives the total estimated limiting variance, which delivers the standard errors.

see Bun and Klaassen (2002) for empirical evidence. Hence, ε_{ijt} is probably serially correlated. Moreover, ε_{ijt} may be cross-sectionally correlated, because regional trade shocks affect several trade flows jointly and nation-specific shocks potentially affect trade flows with all trading partners, for example. We therefore allow for both serial correlation and cross-sectional correlation of ε_{ijt} in addition to the usual correction for heteroskedasticity.

Finally, model (1) is related to Baltagi, Egger and Pfaffermayr (2003). In a model for exports from country i to j, they add fixed effects indexed by it and jt, say, $\xi_{it} + \mu_{jt}$, so that each country has a separate parameter for each time period when it is an exporter and another set of parameters when it is an importer. Because we have summed exports and imports into trade, μ_{jt} cannot be distinguished from ξ_{jt}, so their approach in our context means having $\xi_{it} + \xi_{jt}$. This is very flexible in the it and jt dimensions of the panel, because the effects correct for all possible nation-specific variables (such as institutional characteristics, factor endowments, government policy, and cultural aspects) and these are allowed to move unrestrictedly over time. In the cross-sectional (ij) dimension, however, our approach is more flexible, because it allows the trade development over time to be driven by other than purely national factors, such as the transportation cost and tariff variables mentioned in the introduction. Because we want to study the effect of omitted trends, we want to account for such trends in the ij dimension. As linear trends usually capture the major part of the time development of trending variables, we thus prefer our full flexibility in the ij dimension at the cost of imposing linearity for the trend instead of allowing for unrestricted time variation at the cost of restricting the ij dimension. Our linearity assumption is supported by the fact that the euro estimates remain essentially the same when allowing for quadratic trends (see Section 4.1).

3 The importance of accounting for time trends for the euro estimate

This section describes the data and then estimates model (1), both under $\tau_{ij} = 0$ (in Section 3.2) and with τ_{ij} unrestricted (in Section 3.3). By comparing both estimates for the euro dummy $EURO_{ijt}$ we get an idea of the trend robustness of the estimated euro effect, which is the main purpose of the study.

Table 1: Numbers of observations across country pairs

Country pair	Period Pre-euro	Period Euro	Total
Two euro countries	1,780	200	1,980
Euro and non-euro countries	2,816	352	3,168
Two non-euro countries	896	112	1,008
Total	5,492	664	6,156

A country is named "euro country" if it has the euro in 2002, so including Greece, which entered the euro zone in 2001.

3.1 Data

We have data on all bilateral combinations of 19 countries, namely all EU countries prior to the May 2004 expansion, Norway, Switzerland, Canada, Japan and the U.S., where Belgium and Luxembourg are taken together because trade data are only available at the Belgium-Luxembourg Economic Union (BLEU) level. This gives $N = 171$ country-pairs. We have annual data from 1967 through 2002 (including four years of the euro), so that there are $T = 36$ time periods. The panel is balanced, so that we have 6,156 observations. Table 1 provides more details. It implies that identification of the euro effect δ_1 is driven by the behavior of 1,780 versus 200 observations.

Data for $TRADE_{ijt}$ come from the IMF Direction of Trade Statistics (DOTS) in combination with the U.S. producer price index from the OECD Main Economic Indicators. Data on GDP_{ijt} are from the OECD Economic Outlook. Population data used to construct $GDPCAP_{ijt}$ are from the U.S. Bureau of the Census website. The FTA_{ijt} dummy is based on the trade agreement chronology given in Bun and Klaassen (2002).

For comparison, we also use two other samples. The first one is the 1992-2002 subset of the data just described. This approximates the Micco et al. (2003) dataset. The main difference is that they also use data on Australia, Iceland and New Zealand, but that is not expected to affect the results much. The second sample is the Glick and Rose (2002) dataset. It is an unbalanced panel of $N = 11,178$ country-pairs from 1948 through 1997, resulting in 219,558 observations. This sample includes many different currency unions, mostly involving small and poor countries, but not the euro area. Hence, the $EURO_{ijt}$ dummy in (1) is substituted by CU_{ijt}, which is one if the trading partners have a currency union in year t. Although the focus of the study is on the euro, the results from the Glick and Rose sample give some insight into the robustness of our conclusions.

3.2 Estimation without country-pair specific time trends

The estimates for model (1) using the three samples are shown in Table 2. The columns headed by "No trends" contain the results under the restriction $\tau_{ij} = 0$, so that they are the estimates one would obtain using the standard panel gravity model. Note that this restricted model still allows for some trend, as it includes time effects λ_t, but this trend is restricted to be common to all country-pairs. The estimated euro and general currency union effects are 0.41, 0.16 and 0.62, which are similar to the ones reported in Bun and Klaassen (2002), Micco et al. (2003) and Glick and Rose (2002), respectively.[4] Because $TRADE_{ijt}$ is the logarithm of trade, these estimates correspond to a relative change of trade itself of ($\exp(\delta_1) - 1 =$) 51%, 18% and 86%, respectively.

Table 2 reports two types of standard errors. The first one, in braces, represents the common approach in the gravity literature of allowing for conditional and cross-sectional heteroskedasticity. However, for reasons discussed in Section 2.2, the residuals will presumably also exhibit serial correlation and are correlated across country pairs, implying that the common standard errors are invalid. The second type of standard errors, in brackets, is robust to heteroskedasticity as well as serial and cross-sectional correlation.

The usefulness of the additional robustness is demonstrated by Table 2. The common standard errors turn out to be roughly three times smaller than the robust ones. A more detailed analysis reveals that this is caused by both neglected serial and neglected cross-sectional correlation. Nevertheless, even with robust standard errors, the euro and currency union estimates in the model without heterogeneous trends are all significant at the 5% level (the level we use throughout the study).

Instead of moving directly to the estimates of the unrestricted model, we first analyze the standard model in more detail in the remaining part of this section. The purpose is to obtain some preliminary insights into the relevance of our suggestion that omitted upward trending trade determinants in combination with a euro dummy that is only one at the end of the sample may lead to an upward bias in the estimated euro

[4] The difference between 0.41 and the long-run estimate of 0.33 in our (2002) paper is caused by the fact that that paper uses a model for exports instead of trade, takes account of dynamics, and has a slightly smaller dataset.
Even though we use the data underlying the Glick and Rose (2002) paper, our 0.62 differs slightly from their 0.59 (which they obtain when using year controls, see their Table 5). The reason is that we have left out their current colony variable. This simplification does not alter the main pattern of results in the present paper.

Table 2: Estimation results for trade model (1)

		OWN DATA				GLICK-ROSE DATA	
		Whole period 1967-2002		Micco et al. period 1992-2002		Whole period 1948-1997	
		No trends	Trends	No trends	Trends	No trends	Trends
$EURO_{ijt}/CU_{ijt}$	δ_1	0.410	0.032	0.164	0.018	0.622	0.223
(currency union)		{0.028}	{0.014}	{0.013}	{0.013}	{0.043}	{0.055}
		(0.075)	(0.016)	(0.032)	(0.016)	(0.079)	(0.052)
FTA_{ijt}	δ_2	0.41	0.06	–	–	0.85	0.32
(free trade area)		{0.02}	{0.01}			{0.03}	{0.03}
		(0.09)	(0.03)			(0.13)	(0.08)
GDP_{ijt}	β_1	1.41	0.70	1.99	0.12	0.46	0.86
(product GDP)		{0.10}	{0.15}	{0.31}	{0.95}	{0.02}	{0.05}
		(0.39)	(0.36)	(0.63)	(0.77)	(0.08)	(0.10)
$GDPCAP_{ijt}$	β_2	−0.68	−0.23	−1.51	0.25	0.53	−0.13
(product GDP capita)		{0.09}	{0.15}	{0.33}	{0.96}	{0.02}	{0.05}
		(0.37)	(0.35)	(0.69)	(0.76)	(0.10)	(0.10)
#observations		6,156	6,156	1,881	1,881	219,558	219,558
#fixed effects		206	376	181	351	11,227	21,304

White standard errors in braces (robust for arbitrary heteroskedasticity over time and country-pairs), and Driscoll-Kraay-Newey-West standard errors in parentheses (robust for arbitrary heteroskedasticity over time and country-pairs, serial correlation and lagged and contemporaneous cross-sectional correlation; see footnote 3).
"No trends" denotes model (1) under $\tau_{ij} = 0$ and "Trends" is the model with τ_{ij} unrestricted. CU_{ijt} indicates that the Glick and Rose (2002) data are about currency unions other than the euro area. The FTA_{ijt} effect cannot be estimated with the 1992-2002 subsample, because it is constant for that period for each country-pair. The number of fixed effects is computed after removing unidentified parameters.

effect.

We first consider the sample length T. If the euro effect is biased upwards by omitted trends in trade, then one would expect a larger estimate from a long sample than from a short sample, because the signal coming from trends is stronger for longer samples. Indeed, the estimate of 0.41 based on the complete sample exceeds the 0.16 from the much smaller subsample starting in 1992.

When we use a more gradual reduction of the sample period by taking as starting years 1970, 1980, and 1990, then the estimated euro effects become 0.38, 0.25, and 0.18, respectively. Similarly, we can reduce the Glick and Rose dataset. Because most currency unions are in the first part of the sample, however, we move the ending instead of starting years. If the ending years are 1990, 1980, 1970 and 1960, then the estimated currency union effects are 0.61, 0.49, 0.39, and -0.13. Hence, the shorter the sample, the smaller the estimate. This is difficult to justify from an economic point of view. However, it corroborates that trends matter for the magnitude of euro (or general currency union) estimates.

The standard model does correct for trends in some respect. After all, it includes time effects λ_t to account for omitted trending variables that are common to all country-pairs. If the omitted trends in trade were all driven by this general trend, then there would be no reason for bias in the euro estimate. The sample length dependence just discussed thus suggests that cross-sectional variation in the omitted trending variables is relevant.

If there exist omitted country-pair specific trending trade determinants, then one expects to see varying trends in the country-pair residual series from a model that does not account for that. Hence, we study the residuals from model (1) estimated under $\tau_{ij} = 0$. Plotting the residuals by country-pair over time reveals that there are indeed time trends left in the residuals and that these vary across country-pairs. This is confirmed by country-pair specific regressions of the residuals on the time variable t. They yield t-values for the time variable that are smaller than -2 in 42% and larger than 2 in 33% of the cases for the whole sample (34% and 29%, respectively, for the post-1992 subsample, and 29% and 30% for the Glick and Rose data).

The mere existence of omitted country-pair trends does not necessarily result in an upward bias of the euro effect. Only if the trends are upwards for the euro countries, our argumentation could explain an upward bias in the euro effect. To check whether the euro dummy is misused to help capture upward trends, we reestimate model (1) under $\tau_{ij} = 0$ but without the euro dummy. The residuals are plotted in the left graph of panel A of Figure 1, where the solid line plots the residual series averaged across the 55 country pairs involving two euro countries, and the dotted line refers to the other 116 country pairs. There is a clear upward residual trend for the euro country pairs, and the diverging trends across the two groups of country pairs lead to a large difference in the residuals at the end of the sample period. We then take the residuals of the model including the euro dummy. The right graph of panel A shows that the euro dummy is used to explain the difference in the residuals at the end of the sample, so that the estimated euro effect is indeed driven by the diverging trends.

A more detailed analysis reveals that 40 of the 55 euro country-pairs have an upward estimated trend in the residuals (43 for the subsample). Removing these 40 pairs and estimating model (1) under $\tau_{ij} = 0$ with the euro dummy reduces the euro effects from 0.41 to 0.06 (0.16 to 0.01 for the subsample). This again suggests that the many upward

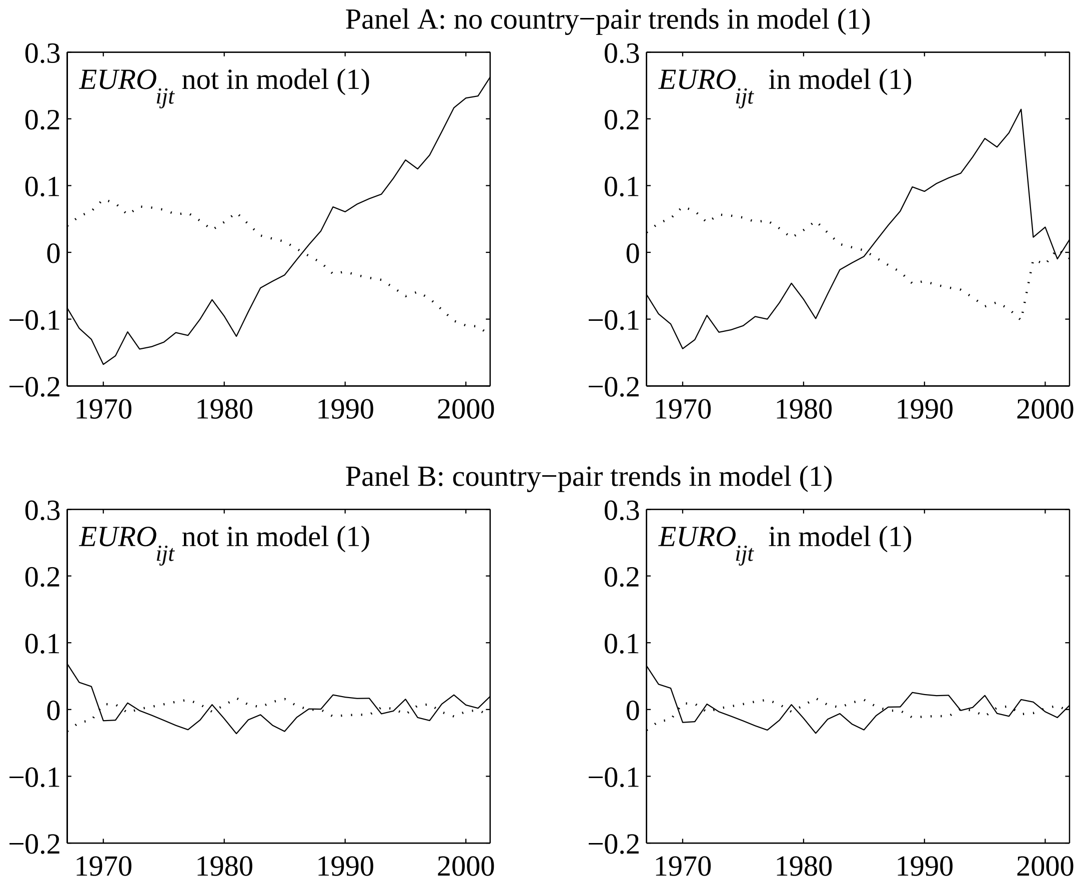

Figure 1: Residuals from model (1) averaged across 55 euro country pairs (solid lines) and 116 other pairs (dotted lines)

sloping omitted trending variables for the euro countries cause an upward bias in the estimated euro effect.

We redo this analysis for the Glick and Rose sample. In contrast to our sample, currency unions in that sample sometimes break down during the sample period and sometimes (though less frequently) are formed. Under the aforementioned claim that upward residual trends combined with currency union formation lead to an upward bias, downward residual trends combined with currency union dissolution also lead to an upward bias. Hence, we treat both combinations as one group. Of the 131 country-pairs that have changes in the currency union dummy, 85 belong to that group.[5] Removing them from the sample and reestimating the standard model with the currency union dummy reduces the estimate from 0.62 to -0.55, so almost the opposite. This confirms

[5] In the underlying regressions we have only used country-pairs with 10 observations or more to have at least some degrees of freedom.

the suggestion about the trend relevance from our own data.

3.3 Estimation with country-pair specific time trends

The results from the previous section indicate that it pays to correct for country-pair specific omitted trending variables. As motivated in Section 2, we do this by including country-pair specific trends $\tau_{ij} \cdot t$. The columns headed by "Trends" in Table 2 present the results.

The euro and currency union effects on trade for the three datasets under consideration become 0.03, 0.02 and 0.22. We see that the existing euro estimates obtained from the standard gravity specification (so without $\tau_{ij} \cdot t$) are substantially different: 51% versus 3% and 18% versus 2% for the two samples. Given the small standard errors, the estimates are rather precise and are around the insignificant/significant bound. In addition, the currency union estimate based on the Glick and Rose (2002) dataset changes from 86% to 25%. However, the currency union effect is still considerable and, in our opinion, the magnitude of the new estimate is more realistic than the existing one from an economic point of view.

Another sign of the relevance of including the trends follows from varying the time dimension T of the sample. Recall from the previous section that without trends a gradual reduction of T changes the euro estimate. With trends and starting the sample in 1970, 1980, and 1990, gives euro estimates of 0.01, 0.01, and 0.03, respectively. For the Glick and Rose dataset we again move the ending years from 1990 to 1980, 1970, and 1960. The estimates are 0.16, 0.25, 0.16, and 0.26. Hence, the estimates with unrestricted τ_{ij} no longer depend on T in a systematic way.

Not surprisingly, also the country-pair residual series no longer have a trend as they had before; compare panel B of Figure 1 to panel A. Hence, the euro dummy can no longer be misused to explain a difference in residuals resulting from trend divergence.

Based on the results so far, we argue that upward trends in omitted trade determinants have caused a substantial upward bias in the existing euro estimates, and that the magnitudes of those estimates are to a large extent driven by the lengths of the sample periods considered. When we add country-pair trends to the standard model, the estimate changes from 51% to 3%, so the euro effect is not as large as one would conclude from the literature so far. Hence, it is important to account for time trends when estimating the effect of the euro on trade. This is the main claim of the study, which is confirmed by the more recent study of Berger and Nitsch (2005).

Finally, we briefly discuss the effects of allowing for τ_{ij} on the other estimates in Table 2. The estimated effect of FTA_{ijt} has become substantially lower. This is presumably explained by the fact that trade integration between two countries often takes up a major part of the time series available for the country-pair and gradually increases over time, so that projecting out $\tau_{ij} \cdot t$ also removes these trade integration effects to a great extent. For instance, European integration has existed over the whole sample period and gradually deepened, so that its trade enhancing effects may be captured more by $\tau_{ij} \cdot t$ than by the dummy variable FTA_{ijt}. Nevertheless, FTA_{ijt} still seems to have some positive effect.

The estimates for GDP_{ijt} and $GDPCAP_{ijt}$ have become more homogeneous across the three samples. This is presumably due to the fact that adding time trends relieves the included trending regressors from the burden of capturing the trend of omitted variables as well, so that the true income and income per capita elasticities are more cleanly detectable. Moreover, Table 2 shows that using 36 years of data yields more precise estimates of the GDP_{ijt} and $GDPCAP_{ijt}$ impacts than taking 11 years, as expected.

4 Sensitivity analysis

We now examine the robustness of the euro and currency union estimates presented in Section 3.3, namely 0.03, 0.02, and 0.22. Section 4.1 examines whether the estimates change if we generalize model (1), so as to verify that this specification is sufficient for appropriate estimation of the euro impact. Section 4.2 examines alternatives for our country-pair trend specification. Section 4.3 discusses the effect of explicitly accounting for the nonstationarity and cointegration features of the data. Finally, Section 4.4 discusses potential endogeneity issues.

4.1 Generalizing the model

Although the model allows for heterogeneous fixed effects and time trends, the parameters for the economic variables are assumed to be homogeneous. Allowing them to be heterogeneous as well, and estimating the model for each of the 55 euro country-pairs separately, gives an average euro effect of 0.02 for the whole sample, 0.02 for the post-1992 sample, and 0.26 for the Glick and Rose data (Table 3, row 1). The averages are similar to the estimates from model (1), which are replicated in row 0 of the table.

We have also assumed homogeneity over time, in particular for the euro effect. One might argue that the euro benefits gradually increase over time. Perhaps there were already advantages in the process towards the euro because in the middle of the nineties it was already known that a number of countries would presumably qualify for the common currency. The simple term $\delta_1 \cdot EURO_{ijt}$ in model (1) does not allow for such time variation. Therefore, we now let the euro effect depend on time by interacting three sets of dummy variables, similar to the approach in Micco et al. (2003). Let 1[.] denote the indicator function that is one if the condition inside brackets is valid, define $YEAR_t$ as the year number at time t, and let $EURO^*_{ij}$ be one if i and j have the euro in 2002 (so it is already positive in all years before the euro era) and zero otherwise. Then we substitute $\delta_1 \cdot EURO_{ijt}$ in (1) by $\sum_{\tau=1993}^{2002} \delta_\tau \cdot 1[YEAR_t = \tau] \cdot EURO^*_{ij} \cdot 1[YEAR_t \geq 1993]$. It yields different euro estimates $\delta_{1993}, \ldots, \delta_{2002}$ from 1993 onwards. For the model with $\tau_{ij} = 0$, they are 0.25, 0.28, 0.32, 0.31, 0.34, 0.39, 0.44, 0.46, 0.47, 0.51, with standard errors of 0.06. The euro effect thus seems to increase over time. This corresponds to the claims of Micco et al. (2003) and Flam and Nordström (2003). However, these estimates may also be biased due to omitted trending variables, just like the existing estimates for δ_1 are biased. Indeed, leaving τ_{ij} unrestricted yields 0.00, 0.01, 0.04, -0.00, -0.01, 0.03, 0.05, 0.03, 0.02, 0.05, with standard errors of about 0.03. Improving the model specification thus removes the gradual increase in the euro benefits. Interestingly, the last five estimates tend to be higher than the first four, and the simple $\delta_1 \cdot EURO_{ijt}$ term with the estimate of 0.03 is apparently quite appropriate.

Using the 1992-2002 sample, we also see a gradual increase in the euro estimates if $\tau_{ij} = 0$ is imposed. Allowing for unrestricted τ_{ij} leads to identification problems. The reason is as follows. If one wants to test whether the euro has led to a gradual increase in trade from 1993 through 2002, then one needs a reference path of trade for that whole period that indicates how large trade would have been if there had been no euro. This reference path depends on τ_{ij}. However, there is not enough data before 1993 to estimate it, so that from the 1992-2002 sample one cannot identify which part of the realized trade increase is caused by the euro and which part is simply normal growth in trade. One needs a sufficiently long period before the period of analysis for proper identification. This is another motivation for our choice for using data starting in the sixties.

The next robustness check of the euro and currency union estimates deals with the idea that the euro period may have coincided with a slowdown in intra-euro-area trade resulting from other factors. We do control for many of such potential factors, but our correction is necessarily imperfect. Suppose that factors for which we do not control would have caused a trade reduction of 10% if there had been no euro. Then, our estimate of 3% would actually represent a trade-enhancing effect of the euro of 13%. Though it is impossible to completely correct for such factors, one can obtain an approximate correction by assuming that the trade impact of such factors is the same

Table 3: Sensitivity of euro estimate to generalizations of model (1)

Model generalization	OWN DATA 1967-2002	OWN DATA 1992-2002	GLICK-ROSE 1948-1997
0: Baseline	0.032 (0.016)	0.018 (0.016)	0.223 (0.052)
1: Country-pair specific regression parameters	0.024 (0.078)	0.023 (0.058)	0.257 (0.736)
2: Year-specific euro effects	[-0.006, 0.053] ([0.024, 0.042])	–	–
3: Year-specific EU effects added	0.047 (0.018)	0.040 (0.030)	–
4: Quadratic country-pair trends added	−0.013 (0.014)	−0.002 (0.015)	0.239 (0.056)
5: Unrestricted country trends $\left(\xi_{it} + \xi_{jt}\right)$ added	0.050 (0.012)	0.058 (0.022)	0.101 (0.053)
6: Dynamic model (long-run effects)	0.015 (0.039)	−0.001 (0.014)	0.167 (0.086)
7: Export as dependent variable	0.012 (0.019)	−0.004 (0.016)	–

Driscoll-Kraay-Newey-West standard errors in parentheses; see note to Table 2.
1: The numbers are averages of the estimates and standard errors resulting from country-pair specific estimations of (1) without time effects. For the Glick-Rose data we only use currency union country-pairs with at least 10 observations. 2: Intervals give range of estimates and standard errors; for the post-1992 subsample there are identification problems: see main text of Section 4.1. 6: The dynamic model adds two lags of $TRADE_{ijt}, GDP_{ijt}, GDPCAP_{ijt}$ as regressors to (1); as there is no residual serial correlation now, standard errors are based on zero lags. 7: This variant explains $EXPORT_{ijt}$ as a function of $GDP_{jt}, GDPCAP_{jt}, EURO_{ijt}, FTA_{ijt}$, the bilateral real exchange rate RER_{ijt}, and the fixed effects of (1). There is no estimate for the Glick-Rose data set, because it does not include data on uni-directional trade flows.

for country-pairs that have the euro and country-pairs that do not have the euro but are in the European Union. We want to allow for unrestricted intra-EU specific trade developments from 1993 onwards (taking 1999 instead of 1993 yields similar results). Therefore, we add $\sum_{\tau=1993}^{2002} \alpha_\tau \cdot 1[YEAR_t = \tau] \cdot EU^*_{ij} \cdot 1[YEAR_t \geq 1993]$, where EU^*_{ij} is one if i and j are in the EU in 2002, similar to the definition of $EURO^*_{ij}$ before. As row 3 in Table 3 shows, the euro estimates become 0.05 and 0.04 for the long and short samples, respectively, so similar to the baseline.

Next, we examine the linearity of the country-pair time trends. One may argue that the results underestimate the true euro effect, because of a slowdown in the growth of international trade (for instance, after the first half of the seventies), so that the euro parameter may now capture the fact that trade is below the linear trend at the end of the sample. However, a general slowdown in the growth is captured by the time effects λ_t, and adding quadratic country-pair specific trends to the model (as in Cornwell et al., 1990) hardly affects the estimates (they become -0.01, -0.00 and 0.24 for the three samples).

One can also generalize the trends in the country instead of country-pair direction, along the lines of Baltagi et al. (2003), as discussed in Section 2.2. They suggest including fixed effects to control for all possible individual country time-varying variables, where the triple (ijt) dimensionality of the trade panel allows them to let these fixed effects move unrestrictedly over time. This in fact generalizes the year effect λ_t into $\xi_{it} + \xi_{jt}$. We add this to model (1), which results in an encompassing combination of the Baltagi et al. and our approaches. As row 5 of Table 3 shows, the estimated euro effects become 0.05 for the whole sample and 0.06 for the post-1992 sample. Despite the substantial number of additional parameters (612 for the complete sample), the estimates are in line with the ones from model (1) reported in Table 2. For the Glick and Rose sample we obtain a currency union estimate of 0.10. This is somewhat lower than the baseline, so that for the Glick-Rose data nation-specific factors matter. Interestingly, the currency union estimate is not statistically different from the euro estimates.

Next, we extend (1) by adding lags of $TRADE_{ijt}, GDP_{ijt}$, and $GDPCAP_{ijt}$ as regressors to capture the dynamic nature of trade more directly instead of indirectly through the adjustment of standard errors. Two lags turn out to be enough, and we have no evidence that $EURO_{ijt}$ and FTA_{ijt} need lags. As before, we estimate the model by LSDV. It is well known that in dynamic panel models this estimator is biased and only consistent when the number of time periods is large. Because we use 36 years of data and inspecting the simulation results by Judson and Owen (1999), we expect finite sample bias to be acceptably small in our case. The estimates are 0.02 and -0.00, and for the Glick-Rose sample we get 0.17 (see row 6 of Table 3). These are in line with the baseline estimates.

Finally, we use real exports instead of trade as dependent variable (following Bun and Klaassen, 2002), where real exports are measured in exporter's output. The estimated euro effects become 0.01 and -0.00. They are similar to our baseline estimates, although they tend to be somewhat lower.

In summary, we conclude that several model generalizations leave the estimated euro effect essentially unchanged, so that model (1) seems general enough for appropriate estimation of the euro impact on trade.

4.2 Alternative trend specifications

We have suggested using unrestricted country-pair specific linear trends to make the estimated euro effect more robust. However, there may exist alternative trend specifications with which one can attain the same goal. This section considers such alternatives.

More specifically, we use two alternative approaches. First, we examine simplifications of our approach to examine whether model (1) is too general, as we currently use many (171) parameters τ_{ij}. Even though the standard error for the euro estimate is quite small, one may try to obtain more precise estimates by economizing on the number of trend parameters. Such simplifying restrictions may also provide insights into the factors that are really crucial for avoiding bias in the euro estimate. Second, we compare our specification with the trend approach of Baltagi et al. (2003), which uses $\xi_{it} + \xi_{jt}$ instead of $\tau_{ij} \cdot t$.

To organize the results, we consider all alternatives as restrictions upon a single general model:

$$\begin{aligned} TRADE_{ijt} &= \beta_1 GDP_{ijt} + \beta_2 GDPCAP_{ijt} + \delta_1 EURO_{ijt} + \delta_2 FTA_{ijt} \\ &\quad + \eta_{ij} + \tau_{ij} \cdot t + \xi_{it} + \xi_{jt} + \varepsilon_{ijt}, \end{aligned} \tag{2}$$

which encompasses the Baltagi et al. (2003) model and our specification (1). For instance, the latter imposes $\xi_{it} = \lambda_t$ (for all i). We check the validity of each restriction by comparing the restricted estimates to those from a more general variant of (2). We confine ourselves to the complete sample; the results for the post-1992 period and the Glick-Rose sample are similar.

The first column of Table 4 gives the estimates for the encompassing model (2), which we take as the benchmark. (These are exactly equal to those underlying row 5 of Table 3.)

For ease of comparison, the next two columns replicate the estimates of model (1) in Table 2 with and without trends. Note that the latter model only has $\eta_{ij} + \lambda_t$ as effects. As discussed earlier, the euro estimate is biased upwards in that case. Because all restricted versions of $\eta_{ij} + \lambda_t$, such as $\widetilde{\eta}_i + \widetilde{\eta}_j$, give similarly high estimated euro impacts, the third column represents all those incomplete specifications.

The next three columns concern specifications with various restrictions on τ_{ij}. Apart from that, all variants restrict $\xi_{it} = \lambda_t$ in model (2). First, we analyze whether we can replace the country-pair specific trends by time-varying effects of distance, i.e. imposing $\tau_{ij} = \alpha DIST_{ij}$ where $DIST_{ij}$ is the logarithm of distance obtained from the Glick-Rose data set. We find the counterintuitive result of slightly significantly increasing (in absolute value) distance coefficients over time. However, this is consistent

Table 4: Sensitivity of estimates to trend specifications in (2)

		$\tau_{ij}t+\xi_{it}+\xi_{jt}$	$\tau_{ij}t$	–	$DIST_{ij}t$	$EURO^*_{ij}t$	$(\widetilde{\tau}_i+\widetilde{\tau}_j)\,t$	$\xi_{it}+\xi_{jt}$
$EURO_{ijt}$	δ_1	0.050	0.032	0.410	0.399	0.119	0.063	0.055
(currency union)		(0.012)	(0.016)	(0.075)	(0.055)	(0.032)	(0.022)	(0.020)
FTA_{ijt}	δ_2	0.10	0.06	0.41	0.39	0.31	0.23	0.33
(free trade area)		(0.02)	(0.03)	(0.09)	(0.08)	(0.02)	(0.06)	(0.06)
GDP_{ijt}	β_1	–	0.70	1.41	1.61	1.60	0.60	–
(product GDP)			(0.36)	(0.39)	(0.35)	(0.10)	(0.41)	
$GDPCAP_{ijt}$	β_2	–	−0.23	−0.68	−0.87	−0.89	−0.10	–
(product GDP capita)			(0.35)	(0.37)	(0.33)	(0.09)	(0.39)	
#fixed effects		988	376	206	206	206	224	836

All trend specifications include year effects λ_t. Driscoll-Kraay-Newey-West standard errors in parentheses; see note to Table 2.

with earlier empirical evidence reporting stable distance coefficients with no tendency to decline over time (Leamer and Levinsohn, 1995) or increasing distance coefficients (Brun et al., 2005). It may indicate that distance does not completely capture transportation costs. Anyway, the pattern of the remaining estimates corresponds closely with that of the model restricting $\tau_{ij} = 0$. In particular, the euro effect is 0.40. The residuals again show trends and we conclude that this parsimonious specification of the trends is not sufficient.

The second restriction on τ_{ij} is motivated by Figure 1. It suggests that the euro estimate is biased because of faster unexplained trade growth over the whole period for country-pairs that now have the euro compared to other country-pairs, for instance due to increased economic cooperation irrespective of the euro. We model this by allowing for a difference in trend between the group of euro country-pairs and the other pairs: $\tau_{ij} = \alpha EURO^*_{ij}$, where $EURO^*_{ij}$ has been defined in the previous section. The estimated α is indeed positive and the euro effect moves from 0.41 to 0.12. However, it is still significantly higher than the estimated euro impact from the benchmark model. We see that allowing for more flexibility regarding trends affects the euro estimate substantially. However, we conclude that the single group-specific trend variable is not sufficiently general.[6] Nevertheless, it could be that after the introduction of additional

[6] Micco et al. (2003) use a related variable called $EUTrend_{ijt}$, which is t if both i and j belong to the EU at time t and 0 otherwise. Adding this variable to our specification yields a euro estimate of 0.26. Again, the estimate is lower than in case of no country-pair trend correction, but it is significantly higher than the estimate from the more general benchmark model. Hence, also $EUTrend_{ijt}$ does not

group-specific trends the trend variation becomes sufficiently captured. In any case, the general approach of using a trend for each country-pair separately provides a useful reference point.[7]

Third, we let the trends be country instead of country-pair specific. That is, we restrict $\tau_{ij} = \widetilde{\tau}_i + \widetilde{\tau}_j$, where $\widetilde{\tau}_i$ is the trend coefficient for country i irrespective of the partner country. This reduces the number of trend effects from 171 to 19. Column four presents an estimate of 0.06. This does not differ significantly from the benchmark case. Hence, the trending behavior of country-specific omitted regressors is the most important reason for the bias of the euro effect in models with no trends.

This result motivates another direction for testing the robustness of our euro estimate. The Baltagi et al. (2003) approach controls for all country variables irrespective of their development over time. That is, their approach generalizes the linear country trends, $(\widetilde{\tau}_i + \widetilde{\tau}_j) \cdot t$, into unrestricted country trends $\xi_{it} + \xi_{jt}$. It restricts $\tau_{ij} = 0$. Table 4 demonstrates that the euro estimate is similar to the one resulting under linearity, despite the 612 additional parameters. Therefore, linearity of the country-specific trends is fine. An additional advantage of the linearity restriction is that the effects of variables consisting of country-specific components only, such as GDP_{ijt} (the sum of the log of real GDP of both countries), can be estimated, whereas the presence of $\xi_{it} + \xi_{jt}$ makes them unidentified.

The obvious question is then why to use country-pair specific instead of country-specific trends. First, trade data are country-pair oriented by nature, so it seems natural to start with country-pair instead of country trends, particularly in a model that has already country-pair intercepts η_{ij}. At least, allowing for country-pair trends provides a way to check whether the restriction $\tau_{ij} = \widetilde{\tau}_i + \widetilde{\tau}_j$ is valid.

A second reason for using country-pair trends comes from a comparison of estimates across trend specifications for the free trade area dummy FTA_{ijt}. Table 4 shows that the most general specification (2) yields an estimated FTA effect of 0.10. Imposing homogeneity on the country-specific effects ($\xi_{it} = \lambda_t$), as our model does, makes no substantial difference: 0.06. However, excluding the country-pair trends $\tau_{ij} \cdot t$ changes the

completely avoid the upward bias.

[7] Micco et al. (2003) and Flam and Nordström (2002) have added another dummy to the model to capture trade diversion effects of the euro, that is, a shift in a euro member's trade with a non-member to a member. That dummy is one if exactly one of the trading partners has the euro. If there is trade diversion, this dummy has a negative effect. However, contrary to the authors' expectations, they get a positive estimate. Since the dummy is only one at the end of the sample, it may be biased upwards for the same reason as the euro dummy estimate. Indeed, if we add the trade diversion dummy to our model, then it has a positive impact if no country-pair specific trends are allowed (0.20 for complete sample, 0.08 for post-1992 sample), but a zero impact once trends are in the model (0.02 and -0.01, respectively). Hence, the inclusion of trends solves the surprisingly positive trade diversion estimate.

FTA estimate into 0.23 and 0.33 for the linear and unrestricted country trends, respectively. Apparently, correction for country-pair variation of omitted trending variables is important.

We conclude that country-pair trends are sufficient to obtain a robust euro estimate, and that they are necessary for a robust estimate of the FTA dummy. Moreover, they make it possible to estimate the impacts of nation-specific factors such as GDP, in contrast to models with nation-year-specific fixed effects.

4.3 Panel cointegration estimation

The analysis up to now has ignored potential unit-root nonstationarity features of the variables in model (1). This is the standard approach in the gravity literature. Because $TRADE_{ijt}$, GDP_{ijt} and $GDPCAP_{ijt}$ are presumably nonstationary, we have thus essentially implicitly approximated the distribution of the estimator by the asymptotic distribution for an infinite cross-section dimension N but a finite time dimension T. This may be appropriate for the 1992-2002 sample, because there T is rather small. However, we have also seen in Sections 3.3 and 4.1 that it is advisable to use the complete sample, and for that sample (more than 30 years) the approximation for infinite T may be better. In that case, nonstationarity issues are relevant for inference. For instance, if the three variables are nonstationary and cointegrated, which seems quite plausible from an economic point of view, the limiting variance of the least squares estimator of the cointegrating vector depends on the long run covariance between changes in the regressors (ΔGDP_{ijt} and $\Delta GDPCAP_{ijt}$) and the error term ε_{ijt}, which invalidates standard inference (see Mark and Sul, 2003). Even though we are ultimately interested in the euro estimate and not in cointegrating vector estimation, problems regarding the latter may carry over to the euro estimate. Therefore, this section investigates the nonstationarity and whether it affects the estimated euro effect.

We first test for unit roots and cointegration in $TRADE_{ijt}$, GDP_{ijt} and $GDPCAP_{ijt}$.[8] The panel unit root tests of Harris and Tzavalis (1999), testing the null hypothesis of a unit root, and of Hadri (2000), testing the null of stationarity, both indicate nonstationarity (only in the case of $TRADE_{ijt}$ there are conflicting outcomes). The panel cointegration tests of Pedroni (1999) reveal cointegration.

To solve the resulting least squares inference problems mentioned earlier, one can use fully modified OLS (FMOLS) or dynamic OLS (DOLS) techniques for panel data (see Kao and Chiang, 2000). Because various authors report satisfactory results from DOLS (Stock and Watson, 1993, Maddala and Kim (1998, p.184), we follow that approach. In

[8] The tests are computed using the package NPT 1.3 of Chiang and Kao (2002).

particular, we follow Mark and Sul (2003) who allow for a similar specification as model (1). They show that the panel DOLS estimator is asymptotically normally distributed, so that standard inference can be made.

The way we use DOLS to estimate the euro effect consists of two steps. First, we use DOLS to estimate the cointegrating vector. That is, we correct for the covariance between changes in the nonstationary regressors and the error term ε_{ijt} by including two leads and two lags of these changes directly in the regression equation using heterogeneous coefficients (see the note to Table 5 for a motivation for the number of leads and lags). Thus we add $\sum_{s=-2}^{2} \gamma_{ijs1} \Delta GDP_{ij,t-s} + \gamma_{ijs2} \Delta GDPCAP_{ij,t-s}$ to the right-hand-side of model (1) (while removing the stationary euro and FTA dummies). Panel DOLS estimation then gives estimates for the cointegrating vector parameters β_1 and β_2 with standard errors, which we base on the Newey and West (1987, 1994) long-run variance estimator.

In the second estimation step we substitute the estimates for β_1 and β_2 into model (1) and estimate the impacts δ_1 and δ_2 of $EURO_{ijt}$ and FTA_{ijt}. Because the equilibrium error is stationary, this step is a stationary panel regression, so one can use standard inference. As before, we use the Driscoll-Kraay-Newey-West approach to obtain robust standard errors.[9]

The estimation results are in Table 5. The conclusions drawn in the previous section remain valid. In particular, the euro effect is again substantially different when heterogeneous trends are included, and in the model with trends it is again about 3%.

4.4 Endogeneity

So far, we have assumed that the general disturbance term ε_{ijt} in model (1) is uncorrelated with the regressors. This may be problematic. First, countries that trade a lot with each other may experience high economic growth, may be more likely to adopt a common currency, enter into a free trade agreement, and so on. Hence there may be a causality from ε_{ijt} via $TRADE_{ijt}$ to the regressors, that is, simultaneity. Second, omitted variables may be correlated with included regressors. Both sources of endogeneity can lead to biased estimates if the estimation method does not account for it. This problem has yet to be effectively addressed in the gravity literature, though some authors have contributed to solving the issue (Rose, 2000, among others). This sec-

[9] We follow a two-step instead of single-step procedure, because adding the leads and lags removes observations at the beginning and end of the sample and (because the euro dummy is only one at the end of the sample) that would lead to a severe loss of euro observations in a single-step approach. In the second step of our regression, there are no leads and lags, so that no euro observations are lost. Because the estimator of β_1 and β_2 is superconsistent, the asymptotic distribution of the estimator for the euro and FTA effects is not affected by the two-step nature of the approach.

Table 5: Cointegration based estimation results for trade model (1)

		OWN DATA				GLICK-ROSE DATA	
		Whole period 1967-2002		Micco et al. period 1992-2002		Whole period 1948-1997	
		No trends	Trends	No trends	Trends	No trends	Trends
$EURO_{ijt}/CU_{ijt}$	δ_1	0.374	0.034	0.153	0.015	0.586	0.171
(currency union)		(0.064)	(0.018)	(0.031)	(0.016)	(0.079)	(0.055)
FTA_{ijt}	δ_2	0.38	0.05	–	–	0.86	0.31
(free trade area)		(0.08)	(0.04)			(0.16)	(0.06)
GDP_{ijt}	β_1	0.59	0.94	2.03	0.92	0.56	0.94
(product GDP)		(0.18)	(0.26)	(0.44)	(2.17)	(0.03)	(0.08)
$GDPCAP_{ijt}$	β_2	0.20	−0.49	−1.78	−0.68	0.61	0.31
(product GDP capita)		(0.16)	(0.26)	(0.47)	(2.17)	(0.03)	(0.08)

See the notes to Table 2. However, the estimates and standard errors for GDP_{ijt} and $GDPCAP_{ijt}$ are now based on the Mark and Sul (2003) panel dynamic OLS estimator using the Newey and West (1987, 1994) method for long run variance estimation.
DOLS requires choosing the number of leads and lags. We have used all combinations between (0,0), (3,0), (0,3), and (3,3), but the eventual euro estimate is very robust. Therefore, we only present results for two leads and two lags, following the choice by Mark and Sul (2003). For the 1992-2002 data the DOLS endogeneity correction is restricted to be homogeneous across country-pairs and uses only one lag and one lead to maintain enough degrees of freedom.

tion provides an alternative approach, based on cointegration and instrumental variable estimation.

Regarding the regressors GDP_{ijt} and $GDPCAP_{ijt}$ we already corrected for endogeneity in the panel cointegration model given above. More in particular, using the DOLS procedure we specified that ε_{ijt} is correlated with some leads and lags of the changes in the nonstationary regressors GDP_{ijt} and $GDPCAP_{ijt}$. Because we established that $TRADE_{ijt}, GDP_{ijt}$, and $GDPCAP_{ijt}$ are cointegrated, the super consistency and limiting distribution of the DOLS estimators of β_1 and β_2 are robust to potential endogeneity of GDP_{ijt} and $GDPCAP_{ijt}$.

This argument does not hold for the stationary regressors $EURO_{ijt}$ and FTA_{ijt}. To correct for endogeneity we estimate by Instrumental Variables (IV) the second estimation step of the procedure of the previous section using one-period lagged values $EURO_{ij,t-1}$ and $FTA_{ij,t-1}$ as instruments. A complication is that the disturbance term of the second step exhibits autocorrelation, so that such instruments are not valid. To remove the residual autocorrelation we include two lags of the dependent variable, i.e. the equilibrium error, as regressors. This makes the instruments valid. Relying on large

T asymptotic theory, as in Section 4.3, the resulting dynamic panel data model can be consistently estimated by the proposed IV procedure.

The IV euro estimates are in Table 6. We include also the corresponding OLS (LSDV) estimates, which are inconsistent in case of endogeneity. As can be seen, IV and OLS estimates do not differ much, particularly for the complete sample. Hence we conclude that endogeneity is of limited importance. Furthermore, the estimates corroborate those in Table 2, so that the conclusions drawn in Section 3 remain valid. In particular, the euro effect is again substantially different after the inclusion of the heterogeneous trends, and in the model with trends it is now about 1.5%.

There are several explanations for the minor relevance of endogeneity here. The economic reasons for exchange rate stabilization and integration policies, such as euro and FTA membership, mainly depend on the importance of trade between countries i and j for their economies. This is only partly determined by the absolute trade level, i.e. the dependent variable in the model, so that the effect of ε_{ijt} on the regressors is not so direct.

In addition, much of the potential endogeneity of the $EURO_{ijt}$ and FTA_{ijt} regressors is absorbed by the fixed unobserved heterogeneity effects, i.e. the country-pair specific constants and time trends and the year effects. To explain this, assume that euro and FTA membership are determined by the importance of trade between countries i and j for their economies. This depends on (1) the importance of bilateral trade relative to multilateral trade, and (2) the importance of multilateral trade for the economies involved. Firstly, bilateral trade relative to multilateral trade (as measured by trade shares) is largely determined by gravity type of variables (such as distance), and cultural and institutional characteristics. These variables are more or less constant over time, so that the regressors $EURO_{ijt}$ and FTA_{ijt} are presumably more correlated with the country-pair specific effect η_{ij} than with ε_{ijt}. The fact that bilateral trade for some country pairs has nevertheless grown faster than for others is captured by $\tau_{ij}t$. Secondly, the importance of multilateral trade has increased over time for all countries, reflecting globalization of the international economy, so that the regressors are more affected by the time effect λ_t than by ε_{ijt}.

Finally, monetary and trade policies are also affected by cultural and political circumstances, which are most likely exogenous of nature. This is particularly true for the introduction of the euro, because a major reason was to get closer to a political union in Europe.

Table 6: IV based euro estimates for trade model (1)

	OWN DATA				GLICK-ROSE DATA	
	Whole period 1967-2002		Micco et al. period 1992-2002		Whole period 1948-1997	
	No trends	Trends	No trends	Trends	No trends	Trends
OLS	0.369	0.014	0.115	0.034	0.583	0.133
	(0.094)	(0.039)	(0.038)	(0.016)	(0.095)	(0.084)
IV	0.379	0.019	0.138	0.081	0.547	0.016
	(0.070)	(0.052)	(0.053)	(0.064)	(0.106)	(0.095)

See the notes to Table 2. The estimates concern long-run effects. As there is no residual serial correlation now, standard errors are based on zero lags.

5 Conclusion

This study has revisited the question whether the euro has increased trade. Existing estimates show trade benefits between 5% and 40% and the magnitude of the euro effect positively depends on the length of the sample used. Using data on 171 industrial country-pairs over 1967-2002 we have first replicated these findings with a commonly used panel gravity model.

The residuals from that model exhibit trends that vary across country-pairs. Most of the euro country-pairs have upward trends, and we have shown that the euro dummy, which is one only at the end of the sample, captures part of these upward residual trends. This leads to an upward bias in the estimated euro effect.

To avoid such omitted trending variables bias, we have proposed extending the standard model by country-pair specific time trends $\tau_{ij} \cdot t$. The estimated euro effect then becomes 3%, which is on the significant/insignificant bound. The estimate is robust to various model generalizations and is no longer driven by the length of the sample period.

Hence our main conclusions are that omitted trending variables have biased existing euro estimates and that the magnitude of that bias depends on the length of the sample. Including country-pair time trends avoids both and shows that the euro effect is not as large as one would conclude from the literature so far.

In addition, we have improved on existing standard error computations by making the standard errors not only robust to heteroskedasticity, but also to serial and cross-sectional correlation. We have accounted for the nonstationarity and cointegration features in the data by using panel dynamic OLS estimation. Finally, we have contributed to the solution of endogeneity bias in the gravity literature by combining cointegration with instrumental variables estimation.

Our finding that it is important to account for country-pair specific time trends may be relevant for other applications of the panel gravity model as well. For instance, the current study has shown that generalizing the model in this direction changes the estimated benefit of non-euro currency unions from 86% to 25% using the Glick and Rose (2002) sample. Moreover, including trends may be relevant for research on the effect of trade integration or the benefits of accession to the EU for the Eastern European countries, as well as for studies using general non-trade panel models for trending data. These issues are left for future research.

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