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 shrinks the estimated euro impact to 3%.

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

JEL classification: C23; F15; F33.

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 regress a bilateral trade variable observed over many years and country-pairs on an income variable 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 of the sample, 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 paper we investigate a particularly important time series characteristic, namely the trends in trade flows over time, because we know from the time series literature that misspecification of the trend can lead to substantial bias. We examine whether the 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

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[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).

caused by omitted variables.

To some extent omitted trending variables bias is already avoided in the papers mentioned earlier. After all, those papers have some income regressor and other trending variables to explain the trend in trade, and their use of panel data allows for 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 regression and trends may vary across country-pairs. A few sources of country-pair specific omitted trending variables can be derived from the generalized gravity equation of Bergstrand (1989), which is particularly interesting because the models in the existing euro papers are related to his model. Although Bergstrand's model is a one-period model, let us imagine that it holds several periods after each other. One trade determinant is transport costs. These depend on the country-pair 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, are not included in the 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 the omitted trending variables is based on the fact that a major part of their signal is the deterministic time trend, or drift term. In such a case, the time series literature would strongly recommend including time as a regressor. Hence, in our panel model we add the time variable t and we allow it to have heterogeneous impacts across country-pairs; $\tau_{ij} \cdot t$ for country-pair ij. 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 add the country-pair specific time trends to the empirical panel gravity model. 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

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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 (that is, exports plus imports) from the contemporaneous national incomes 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 papers, Bun and Klaassen (2002) account for the dynamics in trade data by including lagged dependent and explanatory variables besides contemporaneous ones.

Both differences are not important for the point we want to make in the current paper, 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. For the sake of comparison, the benchmark model in the present paper 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. 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 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

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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-pair specific ones, such as transportation costs and tariffs.

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 paper. 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

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\begin{aligned}
TRADE_{ijt} \;=\; & \beta_1 GDP_{ijt} + \beta_2 GDPCAP_{ijt} + \delta_1 EURO_{ijt} + \delta_2 FTA_{ijt} \\
& + \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). The parameter of interest is δ_1, which represents the impact of the euro on trade between euro member states.

We estimate the model 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

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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).[2]

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 downward 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); see Bun and Klaassen (2002) for 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. This paper therefore allows for serial correlation and cross-sectional correlation of ε_{ijt} (besides the usual allowance for heteroskedasticity).

Finally, the model 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, ξ_{it} and μ_{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. 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

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[2]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). 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.

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 and because linear trends usually capture the major part of the time development of trending variables, we 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. This choice is supported by the fact that our euro estimates remain essentially the same when generalizing the linearity by allowing for quadratic trends.

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 3.1) and with τ_{ij} unrestricted (in Section 3.2). By comparing both estimates for $EURO_{ijt}$ we get an idea of the trend robustness of the estimated euro effect, which is the main purpose of the paper.

We have data on all bilateral combinations of 19 countries, namely all EU countries, 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. The data are yearly and run from 1967 through 2002 (they thus include four years of the euro), so that there are $T = 36$ time periods. The panel is balanced, so that we have 6,156 observations. Because we have 11 euro countries, there are $1,980$ observations between euro countries, $3,168$ between one euro and one non-euro country, and $1,008$ observations between non-euro countries.

The 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 database. Population data used to construct $GDPCAP_{ijt}$ are from the U.S. Bureau of the Census. 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 paper is on the euro, the results from the Glick and Rose sample give some insight into the robustness of our conclusions.

3.1   Estimation without country-pair specific time trends

The estimates for model (1) using the three samples are depicted in Table 1. The columns headed by "No trends" contain the results under the restriction $\tau_{ij} = 0$, so that they are the results one would obtain using the standard panel gravity model. Note that this restricted model still allows for some trend, because it includes time effects λ_t, although 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.[3]   Because $TRADE_{ijt}$ is the logarithm of trade, these estimates correspond to a relative change of trade itself of $(\exp(0.41) - 1 =)$ 51%, 18% and 86%, respectively.

Table 1 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 probably inconsistent. 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 1. 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

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[3]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 1: 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
$EURO_{ijt}/CU_{ijt}$ (currency union)	δ_1	0.410 {0.028} (0.075)	0.032 {0.014} (0.016)	0.164 {0.013} (0.032)	0.018 {0.013} (0.016)	0.622 {0.043} (0.079)	0.223 {0.055} (0.052)
FTA_{ijt} (free trade area)	δ_2	0.41 {0.02} (0.09)	0.06 {0.01} (0.03)	–	–	0.85 {0.03} (0.13)	0.32 {0.03} (0.08)
GDP_{ijt} (product GDP)	β_1	1.41 {0.10} (0.39)	0.70 {0.15} (0.36)	1.99 {0.31} (0.63)	0.12 {0.95} (0.77)	0.46 {0.02} (0.08)	0.86 {0.05} (0.10)
$GDPCAP_{ijt}$ (product GDP capita)	β_2	−0.68 {0.09} (0.37)	−0.23 {0.15} (0.35)	−1.51 {0.33} (0.69)	0.25 {0.96} (0.76)	0.53 {0.02} (0.10)	−0.13 {0.05} (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 2).
"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.

significant at the 5% level (the level we use throughout the paper).

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 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 shrink to 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 shrink to 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 play a role for the 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 a 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 in the residuals and that these trends vary across country-pairs. This is confirmed by country-pair specific regressions of the residuals on the time variable t, because 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. Of the 55 country-pairs that have joined the euro, 40 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 suggests that the many upward 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,

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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.[4] 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 the suggestion about the trend relevance from our own data.

3.2 Estimation with country-pair specific time trends

The results from the previous section indicate that it is worthwhile 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 1 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 greatly reduced: from 51% to 3% and from 18% to 2% for the two samples. Given the small standard errors, the estimates are rather precise and are around the insignificant/significant bound. The currency union estimate based on the Glick and Rose (2002) dataset decreases from 86% to 25%. Again, a large reduction. However, the currency union effect is still there and, in our opinion, the magnitude of the new estimate is more realistic than the existing one from an economic point of view.

Besides the reduction of the bias of the euro and currency union estimates, it is remarkable to see that accounting for trends also improves their accuracy (smaller standard errors), despite the large number of additional parameters τ_{ij}. The usual price of generalizing the model, that is, higher standard errors, is thus absent here. The reason is presumably that the time trends explain a substantial part of the trade dynamics and cross-sectional dependence, so that the standard errors no longer have to incorporate that.

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 reduces the euro estimate. With trends and starting the sample

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

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.

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 shrinks 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 paper.

Finally, we briefly discuss the effects of allowing for τ_{ij} on the other estimates in Table 1. 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 effect are more cleanly detectable. Moreover, Table 1 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 3.2, namely 0.03, 0.02, and 0.22. Section 4.1 focusses on deviations from the model equation (1), whereas Section 4.2 discusses the effect of explicitly accounting for the nonstationarity and cointegration features of the data.

4.1 Model specification

Although the model allows for heterogeneous effects, the parameters for the economic variables are assumed to be homogeneous. Allowing them to be heterogeneous as well, and estimating the model for each country-pair separately, gives a mean euro effect of 0.03 for the whole sample, 0.02 for the post-1992 sample, and 0.26 for the Glick and Rose data (using only country-pairs with at least 10 observations). The means are similar to the estimates from model (1), so that the homogeneity assumption in that model is not the reason for the small euro effects.

We have also assumed homogeneity over time, in particular for the euro effect. One might argue that the euro benefits gradually increase over time, and perhaps there were already advantages in the process towards the euro because in the middle of the nineties it was already clear 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 substituting $\delta_1 \cdot EURO_{ijt}$ by $\delta_t \cdot EURO_{ij}^*$, where $EURO_{ij}^*$ is 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, and δ_t is zero before 1993; this approach is similar to Micco et al. (2003). It yields different euro estimates from 1993 through 2002. 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 (although one might opt for a start in 1998 instead of 1999).

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 were 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 of Section 3.2 concerns 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 early 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).

Model (1) contains $N = 171$ parameters τ_{ij}. Even though the standard error for the euro estimate is quite small, one may try to obtain even more precise estimates by economizing on the number of trend parameters while still avoiding the upward bias. We discuss two approaches. The first one concentrates on the problem that the euro estimate is biased upwards 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. To capture this, we remove $\tau_{ij} \cdot t$ and allow for a difference in trend between the group of country-pairs that have the euro now and the other pairs. Therefore, we include an additional regressor $EURO^*_{ij} \cdot t$, where $EURO^*_{ij}$ has been defined earlier. The estimated effect of this regressor is indeed positive and its inclusion reduces the euro effect from 0.41 to 0.12 for the complete sample, and from 0.16 to 0.04 for the sample starting in 1992. We again see that allowing for more flexibility regarding trends affects the euro estimate substantially. However, the results presented earlier show that adding $\tau_{ij} \cdot t$ reduces the euro effect further, and to a significant extent. Hence, the single group-specific trend variable is not sufficient.[5] Nevertheless, it could be that after the introduction of additional 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.[6]

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[5] 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 for the complete sample and 0.11 for the post-1992 sample. As expected, this variable helps reduce the euro effect somewhat, but also this trend variable is not sufficient.

[6] 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

A second approach to reduce the number of parameters is to let the trends be country instead of country-pair specific. That is, instead of leaving τ_{ij} unrestricted, write it as $\tau_{ij} = \overline{\tau}_i + \overline{\tau}_j$, where $\overline{\tau}_i$ is the trend coefficient for country i irrespective of the partner country. This reduces the number of trend effects from 171 to 19. Adding the restricted time trends to the standard model reduces the estimated euro effects substantially: from 0.41 to 0.06 for the complete sample and from 0.16 to 0.03 for the sample beginning in 1992. But, they are still somewhat higher that the estimates from the general model (1), and the standard errors for the restricted model are larger. As in Section 3.2, we see that the most general model also gives the most accurate estimates. Therefore, using country instead of country-pair specific trends is not completely satisfactory.

It is also interesting to combine our approach with that of Baltagi, Egger and Pfaffermayr (2003), as discussed in Section 2.2. They suggest to include 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 variables move unrestrictedly over time. Because they use data on both exports and imports instead of only the sum, they extend the model by separate exporter and importer effects, say, $\xi_{it} + \mu_{jt}$. Because we have summed exports and imports into trade, μ_{jt} cannot be distinguished from ξ_{jt}, so we mimic their approach by adding $\xi_{it} + \xi_{jt}$ to model (1). The estimated euro effects become 0.05 (with a standard error of 0.03) for the whole sample and 0.06 (0.03) for the post-1992 sample. Despite the substantial number of additional parameters (648 for the complete sample), the estimates are in line with the ones from (1) reported in Table 1, although somewhat higher.[7]

Finally, let us use exports instead of trade as dependent variable (following Bun and Klaassen, 2002). The estimated euro effects become 0.00 (with a standard error of 0.02) and 0.00 (0.02). Again, they are similar to our baseline estimates, although they tend to be somewhat lower.

4.2 Model estimation: panel cointegration results

The results so far have 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 es-

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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.

[7]This is supported by the results for the Glick and Rose sample. There the Baltagi et al. (2003) approach gives a currency union estimate of 0.10 (with a standard error of 0.05). It is positive and not statistically different from the euro estimates.

sentially 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 3.2 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 $GDPCAP_{ijt}$ (using the package NPT 1.3 of Chiang and Kao, 2002). 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 particular, we follow Mark and Sul (2003) who allow for a similar specification as (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 leads and lags of these changes directly in the regression equation using heterogeneous coefficients. 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).[8] Panel DOLS estimation then gives estimates for the cointegrating vector parameters β_1 and

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[8] 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).

Table 2: 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
$EURO_{ijt}/CU_{ijt}$ (currency union)	δ_1	0.374 (0.064)	0.034 (0.018)	0.153 (0.031)	0.015 (0.016)	0.586 (0.079)	0.171 (0.055)
FTA_{ijt} (free trade area)	δ_2	0.38 (0.08)	0.05 (0.04)	–	–	0.86 (0.16)	0.31 (0.06)
GDP_{ijt} (product GDP)	β_1	0.59 (0.18)	0.94 (0.26)	2.03 (0.44)	0.92 (2.17)	0.56 (0.03)	0.94 (0.08)
$GDPCAP_{ijt}$ (product GDP capita)	β_2	0.20 (0.16)	−0.49 (0.26)	−1.78 (0.47)	−0.68 (2.17)	0.61 (0.03)	0.31 (0.08)

See the notes to Table 1. 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. 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.

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 2. The conclusions drawn in the previous section remain valid. In particular, the euro effect is again substantially reduced by the inclusion of the heterogeneous trends, and in the model with trends it is again about 3%.

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[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.

5   Conclusion

This paper 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, tries to capture 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 drops to 3%, which is on the significant/insignificant bound. The estimate is robust to various model perturbations 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 benefits upwards and that the magnitude of that bias depends on the length of the sample. Including country-pair time trends helps avoid both and shows that the euro effect is not as large as one would conclude from the literature so far.

We have also found that accounting for trends is not only useful for reducing estimation bias, but also for improving the estimation precision. To measure the precision, 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. A final novelty is that we have accounted for the nonstationarity and cointegration in the data by using panel dynamic OLS 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 paper has shown that generalizing the model in this direction reduces 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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