Honey, the Currency Union Effect on Trade Hasn't Blown Up

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1. THE PITCH

TWO years ago I wrote a paper (Rose, 2000) that estimated the effect of currency union on trade using an empirical 'gravity' model of bilateral trade. I was concerned with estimating β in the linear model:

$$\ln(T_{ijt}) = \beta CU_{ijt} + \gamma Z_{ijt} + \epsilon_{ijt}$$

where I follow Nitsch in defining T_{ijt} as the value of real bilateral trade between 'countries' i and j at time t, CU is a dummy variable which is unity if i and j share the same currency at time t, Z is a vector of controls given by an augmented gravity model, γ are the associate nuisance coefficients, and ϵ is a (hopefully) well-behaved residual. My chief finding was that β was large and robust; my point estimate was $\beta = 1.2$, implying that currency union is associated with a tripling (since $\exp(1.2) \approx 3$) of trade.

In his paper in this issue, Volker Nitsch provides three pieces of value-added. First he finds and corrects a number of data errors in my original data set. Second, he shows that adding extra controls to my empirical setup, disaggregating by currency unions, and adjusting for missing data affects the results. He concludes that the effect of currency union on trade is smaller than my original estimates, has not been reliably estimated, and may be zero.

2. OOPS!

I welcome and appreciate the corrections to my data set that Nitsch has made and apologise for my mistakes. While I could debate the coding of language and preferential trade agreements, there is no need to: the corrections make little impact, as Nitsch himself states. Lowering the impact of currency

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union on trade from a factor of three to a factor of 2.9 (for Table 1) or 2.5 (for Table 2) seems trifling. The reason that changing the regressors has little effect was noted in my original paper: the currency union dummy is not very highly correlated with the controls. Even with all of Nitsch's corrections and extensions, the highest correlation between the currency dummy and the other controls is only 0.25.

3. QUIBBLING

Nitsch makes a number of other telling points. To list just a few, he warns that: (a) there are few observations for country-pairs in currency unions, (b) trade and control data are missing for a number of observations, including currency union pairs, and (c) any extrapolation to other countries (such as EMU) is dangerous since pre-EMU currency unions tend to involve small and/or poor countries. While I agree with all these points, his evidence has not persuaded me. The reason? I discussed all these points in my original paper. Again: Nitsch advocates the use of country-specific fixed effects to overcome the problem of missing data. In my original paper, I used Tobit (to deal with missing trade data), and Heckit (to deal with missing regressors). I also used country-specific fixed effects in my paper with van Wincoop (2001), published shortly before Nitsch's working paper. But high-tech econometric machinery did not overcome the resiliently positive currency union effect on trade for me, and I will now show that it has not done so for Nitsch either.

4. HIDING IN THE OPEN

In my original research on this topic (Rose, 2000), I found the estimated effect of currency union on trade to be large. Embarrassingly and implausibly large. Thus I stated:

> Without taking the precise estimates too literally, it seems clear that trade is substantially higher for countries that use the same currency, holding other things equal (p. 17)

and repeated:

> One need not take my precise point estimates too literally … (p. 32)

Indeed, footnote 31 subtly expressed my sentiments:

> Even if my estimate is overstated by a factor of five, growth of trade inside EMU would still be large.

Yet taking my estimate literally and reducing it by a small amount seems to be exactly the objective of much of Nitsch's analysis.

Even then, it is not clear that he succeeds. He states in the abstract and introduction that simple manipulations of my data set and the regression specification reduce the estimated currency union effect on trade by about one-half. Really? Nitsch provides two sets of corrections in his Tables 1 and 2 and ten estimates of the currency union effect on trade (= two corrections × five years). His median estimate of β, the currency union coefficient in a gravity equation, is 0.97; the mean is 0.98. So data corrections alone do not reduce the trade effect by one-half. In his Tables 3–5, he provides 73 more estimates using different sets of controls and estimators. The median estimate in these tables is 1.22, and the mean is 1.61. These are *higher* than the estimate of mine that he is criticising. Thus, Nitsch's estimates seem higher than he portrays. More importantly, they just seem high.[1]

I agree with Nitsch that the effect of currency union on trade was certainly not reliably and convincingly estimated by me. I knew and stated that at the time; to repeat, the effect seemed too darn large. But reducing my tripling effect a little even by a half is too small to make the result much more plausible.

5. SPREADING THE MEAT TOO THIN

The biggest econometric issue with my original paper was balancing the small number of currency union observations with the potentially large number of nuisance controls. Only one per cent of the country-pairs in the sample involves currency unions (some 267 of the 22,948 observations used in the estimation). That is, there are a large number of observations, but only a small number are relevant. And the currency union observations are far from independent across either time or country-pairs. So Occam's razor certainly applies here; one does not want to add extra regressors (*Z*'s in the terminology above) without good reason. That is why I chose to pool observations over time, and take advantage of all the currency union pairings simultaneously, while also controlling for the (nine) other nuisance factors that account for most of the variation in trade across countries. I continue to think the model worked well, in that the parsimonious model explained 63 per cent of the data with a root mean squared error of only 2.02 per cent. I noted that:

> few of the effects vary much across time, so pooling the data simply improves the precision of the coefficient estimates (p. 17)

and that:

> most of the coefficient variation (over time) is of negligible economic interest, though it is significant on purely statistical grounds.

[1] Nitsch's estimates are not only economically large, but they are usually statistically significant. Despite his profligate use of nuisance coefficients, three-quarters of the *t*-statistics for β exceed 2; the median *t*-ratio is 3.2 and the mean is 7.6.

Nitsch casually adds regressors and thus burns degrees of freedom throughout his paper. He does this by allowing coefficients to vary across (1) years (throughout), (2) controls (such as language, colonial heritage and so forth in Table 3), and (3) currency unions (in Table 4). For instance, in place of the 11 slope coefficients in my default specification, he estimates 132 in Table 3.[2] Where I estimate one β coefficient, Nitsch estimates 29 in each panel of Table 4. Since we share the same 267 currency union observations, Nitsch estimates one currency union effect for every nine currency union observations. I find this profligate use of coefficients worrying in the face of scarce data. Perhaps more importantly, I also think it is unnecessary. For instance, the hypothesis that the currency union coefficient β is constant across time in Table 2 (which uses Nitsch's corrected and extended data set) cannot be rejected at any reasonable significance level (the F-test for equal β slopes across years is $F(4,22888) = 0.24$).[3] And time-variation in controls only delivers a tiny improvement in the fit of the same equation; the fit falls from R^2 of 0.635 and RMSE of 2.015 with coefficients which vary year by year only to $R^2 = 0.633$ and RMSE of 2.018 with the imposition of constant coefficients over time. But imposing constancy does improve the precision of the all-important currency union coefficient and simplify presentation of results.

6. AS THE CURTAIN FALLS

Since my original paper, I have estimated the currency union effect on trade in three different ways. First, with van Wincoop I pursued a more structural approach. Second, I used 'difference in difference' techniques to account for selection and non-linearity issues. Third, and most convincingly, I have used (Glick and Rose (2001)) country-pair fixed effect estimators on a large panel that covers 217 countries from 1948 through 1997. The latter are important estimates, since all pair-specific time-invariant effects (including, e.g., language and dependency ties, but also unobservables) are controlled in these estimates. *Essentially all of these estimates are economically large.*

Nitsch has tabulated 83 more estimates of the currency union effect on trade. Of these, half imply that currency union at least triples trade; 90 per cent of his estimates imply a trade expansion effect of more than 25 per cent; and almost all are statistically significant. Thus I do not believe that he has beaten the effect down to a size that is considered by most to be *prima facie* plausible.

[2] Of course, we both lose additional degrees of freedom to estimate intercepts and residual variance.
[3] Imposing that (reasonable) restriction delivers an estimate of $\beta = 0.93$ (with a robust t-statistic of 0.13), even allowing for time-variation in the other nuisance controls.

I do not believe Nitsch's own estimates show that he has shrunk the currency union effect by one-half. But even if he had, he would have succeeded only in the small, not the large. If I had originally estimated that currency union leads trade to rise by 'only' 100 per cent, I still think that Nitsch would have been provoked to write his paper.

The mystery remains.

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