

E-Mail exchange between Andrew Rose and Michael Pakko and Howard Wall regarding currency unions and trade.

Pakko to Rose, June 8, 2001

Dear Professor Rose:

Attached is a paper on which my colleague, Howard Wall, and I have been working. Howard tells me that you were quite helpful as he was putting together the data from your web site.

The paper presents a skeptic's view of your prominent results on the trade-creating effects of currency unions. We apply a fixed-effects approach to estimating a gravity equation, and find that with that specification the trade effects shrink to insignificance.

I'm sure you won't find the result surprising. Having seen your response to Torsten Persson's comment, we are aware that you consider the fixed-effects approach to be untenable in this case. However, we have tried to make a case supporting that specification, and our objective is simply to present our results as a cautionary note: the data may support your remarkable findings ... then again, maybe they don't.

We plan to have our paper distributed as an article in the Federal Reserve Bank of St Louis Review. It is presently in the process of our internal peer-review process, and is likely to undergo some changes before publication. We would welcome and comments or suggestions you might have on the current draft.

Sincerely,
Michael R. Pakko

Rose to Pakko, June 11, 2001

My thanks for your paper, which I read through quickly.

I guess the only thing that I don't understand is the role of the two different country-specific fixed effects. Are they really necessary for any real results? It seems somewhat odd to insist on them while maintaining the constant slopes on the income terms (I have a paper on that with Feenstra and Markusen). I'd add at least a footnote describing the differences (if any) between assuming $b_{ij}=b_{ji}$ and not.

I should say that I don't really buy your stuff, because of my more recent paper with Glick. In that we do the standard fixed effects estimator on both annual and 5-year data, and find pretty strong results. I'll check our results at even lower frequencies. But if they stand up, it comes down to: a) your use of two sets of fixed effects vs our use of one set, or b) the size of the data set. If it's b) as I suspect, then I remain unpersuaded that one can really find out anything much from a data set with such little time-series variation. Does that sound reasonable?

Rose to Pakko, forwarded to Wall, June 11, 2001

I attach some output I did which estimates fixed-effect regressions on my new broad data set that I developed with Glick. Whether one does the estimation at 5-,10-,or 20-year intervals, the fixed effect estimator stays positive, significant and large.

I think this means there are only two differences between us. First, it could be the result of your using the small '70-'90 data set. Second, it could be your use of the second set of fixed effects. Perhaps you could run your regression on our new data set, which is available on the web? Alternatively, you could re-run your regressions with a single set of fixed effects? Either way, it seems best to get to the bottom of the discrepancy before publication.(See attached file: pg2b.log)(See attached file: pg2b1.log)(See attached file: ATT86733.txt)

Rose to Pakko, June 12, 2001

Dear Mr. Pakko,

It turns out that the data set I collected for my work with Glick has exports and imports separately (actually two measures for each, since exporting and importing countries collect different values). Accordingly, we can estimate separate gravity equations for exports and imports using fixed effects. I report these in the attached program. I do this for annual data, and also data at 5-,10-, and 20-yr intervals.

My interpretation of the results is as follows. First, the CU effect is always large, positive and significant in both economic and statistical terms. Second, the CU effect is pretty similar between export and import equations. Third, the other gravity effects (e.g., GDP) often do differ between export and import equations.

So my take is that: a) your focus on differential intercepts but not slopes for exports and imports may be mis-placed; b) the large data set simply has a lot more to say than the smaller one; and c) a substantial CU effect still seems to be very present.



pg2b2.log



ATT90512.txt

What's your view?

Pakko to Rose, June 12, 2001

Dear Professor Rose:

We greatly appreciate the attention you have given to our paper. Your comments have certainly served to sharpen my thinking on the issues.

I find myself in the challenging position of intermediating something of a debate between you

and my co-author. Howard is as adamant about promoting and defending his methodological approach to gravity model estimation (the Cheng and Wall reference in our paper) as you are about promoting and defending the findings of your estimate of the trade-creating effects of currency unions.

I've sifted through the results that you have sent, as well as some new estimates that Howard recently sent me (he's presently visiting the Bank of Japan, so we're communicating long-distance). I think there is a considerable convergence of views on some of the important issues.

First, the question of β_{ij} and β_{ji} turns out to be a red herring. The emphasis that we gave the issue in our paper is based on a misreading of your earlier work (the working-paper version, I believe). We need to clarify the presentation of the model in our paper. It turns out that the fixed effects that we look at are precisely the same as in your paper with Reuven Glick: There is no restriction that β_{ij} not equal β_{ji} because they are the same by definition.

The estimation results that I have attached represent some of our own experimentation with the Glick/Rose data set. It is clear that the new, larger data set provides for much more robust estimates, and that this is due to the more important role of time-series variation. The attached estimates use data at 10 year intervals, for two particular subsets. In the first subset, using 1955, 65, 75, 85 and 95, the coefficient on "custrict" is positive and significant for both the pooled cross-section and fixed-effects specifications (although the coefficient is smaller under the fixed-effects approach). The second subset of data considers only 1975, 85 and 95. In this case, the coefficient estimated in the fixed-effects model -- while still positive -- is no longer significant. It seems to me that these findings highlight the importance of a number of changes in currency-union status that are present in the early years of the data.

Personally, my take on all of this is that our work highlights the fact that your earlier results (using the original data set) are largely cross-sectional. When subjected to a methodology that emphasizes time-series variation, the results don't hold up. This seems to be about the same point that emerges from the exchange of views between you and Torsten Persson.

However, the new, larger data set contains more variation along the time dimension, and therefore provides more robust estimates of common-currency effects -- particularly when the relevant question is "what happens to trade when two (or more) countries adopt a common currency?" The second set of estimates attached to this note suggests a possible stipulation: much of the robustness of the results depends on changes in currency-union status in the earlier years of the sample period. One might question whether estimates from the earlier part of the sample period, which are from the context of a much different world trade and exchange rate regime than we have today, are relevant for countries currently considering the adoption of common currencies.

I'm not sure how this will all work out in our paper. My sense is that while we may have differences of opinion and interpretation about methodology, we have something of a convergence of opinion about the fundamental economic issue: there does seem to be evidence that greater trade volume is associated with common-currency status. However we end up modifying our paper, it would seem appropriate to give greater deference to the Glick/Rose findings on that

fundamental issue.

Of course, I'll be happy to keep you apprised.

Sincerely,
Michael R. Pakko

Rose to Pakko, June 13, 2001

Thanks for the fast reply.

I think your response is quite reasonable, and don't have (at least on first reading) any substantial disagreements. It's good to be able to come to convergence so quickly!

Let me know if I can help further --

Wall to Rose, June 13, 2001

Thanks for your response.

I have been looking into the differences between our fixed effects results. First, we erred in our paper where we said that we had two sets of fixed effects for each trading pair. We don't. Thus, the different results have something to do with the differences in the data sets.

The first thing that struck me was that the post-1970 currency union effect is just different from its pre-1970 effect. However, I did a quick regression using the Rose-Glick data and the years 1950, 60, 70, 80, and 90 and found the post-1970 effect to be even larger than for the entire sample period.

After looking more closely at the two data sets, though, I think I have found a more likely reason. It appears that the currency union dummy in the Rose data is very different from the one in the Rose-Glick data. For example, consider the data for 1975.

In the Rose-Glick data set there are 85 observations of a currency union ($cu=1$). Of these 85, only 37 (44%) of the country pairs also appear in the Rose data set. Of these 37, only 22 (59%) also indicated a currency union in the Rose data set ($cu=1$), for the other 41% the dummy indicated that there was no currency union ($cu=0$).

In the Rose data set, there are 52 observations of a currency union ($cu=1$). Of these 52, only 26 (50%) of the country pairs also appear in the Rose-Glick data set. Of these 26, only 21 (81%) were also indicated as a currency union in the Rose-Glick data set ($custrict=1$).

This points to two reasons for the different sets of results:

(1) The two data sets look at very different sets of countries that share a currency: Half or fewer

of the country pairs for which a currency union is indicated even appear in the other data set.
(2) Even for the country pairs that appear in both data sets, there is a good chance that the two data sets do not agree on whether the two countries were in a currency union.

Either one of these reasons on its own could explain the different results. The first one is probably unresolvable because the data may simply not exist to fill in the gaps. The second one may be resolvable if the currency union dummy was consistent across the two data sets. Perhaps a broader definition of a currency union was used to construct the the Rose-Glick data set.

For your reference, I have attached an Excel file of the pairs of countries that were indicated as being in a currency union by one or both of the data sets in 1975.

Regards
Howard Wall

Rose to Wall, June 13, 2001

Interesting. Let me ponder and see if it's anything that I should be concerned with. I'm not surprised that some disparities, but the size is large.

Wall to Rose, September 13, 2001

Below are the abstract and a link to the published version of my paper with Mike Pakko that estimates the trade-creating effects of currency unions. It appears in our Review.

Regards
Howard Wall

Rose to Wall, September 13, 2001

I am surprised and upset by the news that your paper was published.

When you sent us a draft of your paper on June 11, we responded positively, constructively and quickly. I sent a number of e-mails and output from programs which I ran specially to resolve our issues. Within two days we'd converged at least somewhat. (In fact Reuven and I also discovered the reason for the discrepancies between the two data sets as a result of the exchanges.) However none of this is reflected in the published version of your paper. Further, Pakko stated on June 13 "Of course, I'll be happy to keep you apprised." Now you have published your paper in your home journal without any chance of a response by us and you have manifestly not kept us apprised. Given our response, I consider your action to be professionally discourteous and unproductive.

I'm not sure what can be done, since I assume your Review does not publish responses, and it would not appear in the same issue anyway.

Wall to Rose, September 14, 2001

I just spoke to Mike Pakko, and we are very confused by your reaction. We apologize if there was any perceived miscommunication, but my recollection is that we responded to every one of your comments and suggestions, and that we had pretty much converged.

In his e-mail of June 12th, Mike described to you what we were going to do, which led to the following response from you:

"I think your response is quite reasonable, and don't have (at least on first reading) any substantial disagreements. It's good to be able to come to convergence so quickly! Let me know if I can help further."

On June 13th I sent you my findings about the differences between the two data sets. To this you responded:

"Interesting. Let me ponder and see if it's anything that I should be concerned with. I'm not surprised that some disparities, but the size is large."

Given these exchanges, I cannot figure out how our actions can be characterized as "professionally discourteous and unproductive." We sent you the paper after it had already been through the review process, and made changes in response to your comments and suggestions. You raised no objections to our main findings. We added a section stating quite clearly that Rose and Glick (2001) yields statistically significant and large positive effects with fixed effects estimation and with low frequency data. We even resolved the question of why your two "equally reasonable" datasets yield such different results using exactly the same estimation methods.

Because of this, I can't see how you can say that "none" of our exchange "was reflected in the published version." It is much more accurate to say that *nearly all* of the exchange was reflected in the published version. Indeed, I am having difficulty finding any of it that was not reflected in the published version.

In my opinion, all of this was extremely courteous and constructive in that the result of our exchange is that we lay out why Rose and Glick is subject to none of the empirical problems we raise. I read our results as positioning Rose and Glick to be the definitive study: the whole basis of our paper is that the fixed effects method is the appropriate one for estimating gravity models. An explanation in Rose and Glick laying out why the currency union dummy in the new dataset was better than the old one would settle the whole issue.

I am also confused by your claim that we "manifestly did not keep (you) apprised." Mike's message of the 12th laid out what our views and plans were, and you did not disagree with them (see quote above). The only change to those plans was laid out in my e-mail of the 13th which described how the differences in findings were due to the significant differences in the currency dummies and not to the longer time period. You raised no objections at all to this point (see second quote above).

Again, I apologize if there was some miscommunication between us, but we strongly disagree with your characterization of our actions. We feel that, quite the contrary, they were well above the level of courteousness and productivity normally displayed in the profession.

Rose to Wall, September 14, 2001

Well I guess we just disagree. A lot.

My reading (and that of my co-author) is that very little of our exchanges was in your paper. For instance, Pakko concluded in his e-mail of June 13 that "greater trade volume is associated with common-currency status." That's the opposite of the conclusion of your paper. So I certainly don't see the convergence of views reflected in the paper.

Every other time a critique of my work has been submitted for publication, I've been given the right to respond simultaneously in print. But not here. And I certainly expected it or something like it (the next revision? notice that the paper was accepted for publication?) from the "I'll be happy to keep you apprised" with which Pakko ended.

You may read your paper as saying that Rose and Glick is the definitive study, but I don't, and I bet no one else will either. You discuss it, then dismiss it as showing the sensitivity of the approach and the problems with non-overlapping currency unions. More importantly, you choose not to use its superior data set, even though you it would have reversed your conclusions (at least in the large).

Nevertheless, I will take you at your word that the miscommunication was unintentional. I'll post a reply on my website and be satisfied with that.