# *Why Do Estimates of the EMU Effect On Trade Vary so Much?*

**Andrew K. Rose***

***Comments Welcome***

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**Abstract**

Larger data sets, with more countries and a longer span of time, exhibit systematically larger effects of European monetary union on trade. I establish this stylized fact with meta-analysis and confirm it by estimating a plain-vanilla gravity model. I then explain this finding by examining systematic biases in "multilateral resistance to trade" manifest in time-varying country fixed effects; bias grows as the sample is truncated by dropping small poor countries.

**Keywords**: gravity, exports, span, country, meta, common, currency, monetary, union, panel.

**JEL Classification Numbers**: F14, F33

**Contact:**
Haas School of Business, Berkeley CA 94720-1900
Tel: +1 (510) 642-6609
Fax: +1 (510) 642-4700
Email: arose@haas.berkeley.edu
URL: http://faculty.haas.berkeley.edu/arose

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* Rocca Professor of International Business at Berkeley-Haas, ABFER Senior Fellow, CEPR Research Fellow, and NBER Research Associate. This paper draws on and grew out of related work with Reuven Glick and Tom Stanley, whom I thank for earlier interactions. For hospitality during the course of this research I thank the National University of Singapore, which I visited as Wee Cho Yaw fellow. For helpful comments I thank: Fred Bergsten, Chad Bown, Bill Cline, Caroline Freund, Simon Johnson, Adam Posen, Robert Reed, and participants at the MAER colloquium and the Peterson Institute for International Economics, and especially Tom Stanley. A current version of this paper, relevant data, and output are available at my website.

## 1. Motivation

This paper asks the question "Why do estimates of the EMU effect on trade vary so much?" The downsides of monetary union are clear, particularly in Europe; monetary union (which I use interchangeably with "currency union") prevents its members from pursuing nationalistic monetary policies to offset idiosyncratic business cycles and their effects. This loss of a cyclic insurance mechanism is potentially offset, at least in part, by any stimulus that monetary union gives to trade. So the latter effect – that of currency union on trade – is of concern to policy-makers. It is also a topic of academic interest; my 2000 *Economic Policy* paper met with a tsunami of skepticism. As I show below, there are literally dozens of estimates of the effect of EMU on trade. They vary enormously; in this paper, I ask why.

I focus on EMU (Economic and Monetary Union to pedants; European Monetary Union to most) because it is by far the most important monetary union. EMU is also intrinsically different from other currency unions in that it involves countries that are a) large and b) rich in a c) multilateral currency union whose monetary policy d) entails inflation targeting (the ECCA fixes to the US$; the CFA zones fix to the Euro …). It is also one of the most recent currency unions, so if data span affects the estimates, this is a more important problem for EMU than for most currency unions.

## 2. Meta-Analysis

I begin with a meta-analysis of the literature. Rose and Stanley (2005) and Havranek (2010) have previously provided meta-analyses, but there has been considerable work since the latter. To the best of my knowledge, (as of May 2016) there are 45 papers circulating that estimate the effect of EMU on either bilateral trade or exports (over twenty of these are currently unpublished). Roughly speaking, each study estimates this coefficient of interest (sometimes known as the "study effect"), which I denote γ, from a version of the equation:

$$\ln(Y_{ijt}) = \gamma EMU_{ijt} + \beta_Z Z_{ijt} + \{\lambda_{it}\} + \{\psi_{jt}\} + \{\xi_i\} + \{\phi_j\} + \{\tau_t\} + \{\delta_{ij}\} + \varepsilon_{ijt} \quad (1)$$

where:

- $Y_{ijt}$ denotes either bilateral exports from country i to country j at time t, or trade between i and j (the average of i's exports to j and i's imports from j),
- EMU is unity if i and j use the euro at time t and 0 otherwise,
- $\beta$ is a vector of nuisance coefficients,
- Z is a vector of controls,
- $\{\lambda_{it}\}$ is a complete set of time-varying exporter/country dummy variables,
- $\{\psi_{jt}\}$ is a complete set of time-varying importer/country dummy variables,
- $\{\xi_i\}$ is a complete set of time-invariant exporter/country dummy variables,
- $\{\phi_{jt}\}$ is a complete set of time-invariant importer/country dummy variables,
- $\{\tau_t\}$ is a complete set of time dummy variables,
- $\{\delta_{ij}\}$ is a complete set of time-invariant country-pair dyadic dummy variables, and
- $\varepsilon_{ij}$ represents the myriad other influences, assumed to be well behaved.

From each of these 45 studies I collect the authors' preferred estimate of γ, the partial effect of EMU on trade/exports, along with its standard error and other features of the study.[1,2]

Figure 1 is a conventional Forest plot that visually summarizes the literature. The (45) papers are listed at the extreme left, in chronological order. Separate columns also list when the data used in the study begin and end, as well as how many countries are included. Three other features are also tabulated on the left: a) whether the study examines bilateral exports or trade; b) whether it includes dyadic {δ} fixed effects; and c) whether it includes time-varying country {λ} and {ψ} fixed effects. The study effect – γ, the effect of EMU on trade/exports – is tabulated at the right, along with a 95% confidence interval and a weight for random effects analysis (more on this below). Six of the study effects are negative, and many seem positive but small (31 are positive but below .2).

What does the literature collectively say about the size of the EMU effect on trade? A fixed effect estimator is tabulated in the top row of Table 1; this is based on the idea that there is a single underlying effect of EMU. The estimate is small but still economically substantive, since it implies that EMU stimulates trade/exports by (exp(.085)-1≈) 8.9%; the effect is also statistically different from zero at standard confidence levels. However, this estimator is based on the assumption that there is no heterogeneity between studies. This hypothesis is eminently testable and easily rejected, as shown in the extreme right column. Accordingly, I place more trust in a random effects estimator (the vertical line of Figure 1), which conceptually allows each of the 45 studies to have different treatment effects around a common mean. This estimate, tabulated in the second row of Table 1 is even larger, indicating a statistically and economically significant EMU trade effect of (exp(.116)-1≈) 12.3%. When I examine a variety of different sub-sets of the data to focus on studies that include particular methodological features – those that are of particular interest to me for reasons given below – the point estimates remain high, though the confidence intervals inevitably widen.

The hypothesis that EMU has no effect on trade/exports does not seem consistent with the literature. But do the (45) estimates give an accurate picture of the literature? Perhaps not; certain estimates may be systematically under-reported because of publication bias. Even though many of my studies are unpublished, more might never have made it onto the internet. To get a handle on this, I use the conventional tool of funnel plots. Consider the top-left graph of Figure 2, which is a funnel-plot of the 45 estimates of γ on the x-axis against their precision (the inverse of within-study standard error) on the y-axis. There are two striking features of this funnel plot. First, there is evidence that the estimates are skewed to the right of the random-effects estimate (marked with a solid vertical line). However, this finding is weak; the Egger et al test for bias is significant only at the .12 significance level. Second, many of the estimates fall outside the 95% confidence interval, the area below the two dashed lines. Both findings are also true of other sub-sets of the data, as shown by the other funnel-plots in Figure 2. While there seems to be little evidence of publication bias, the dispersion of the 45 estimates of γ is worrying.

Why do the estimates of γ vary so much? Figure 3 provides some clues; it contains four graphs which compare γ (always portrayed on the y-axis, and labelled on the right) to the sample size, measured (on the x-axis) in four different ways. At the top-left of Figure 3, γ (the estimated effect of EMU on trade/exports) is compared to the (natural) logarithm of the total number of observations used to estimate γ. For convenience, a histogram of log observations is provided (labelled with the left scale), as is a least-squares line. The majority of studies have few observations; the median study has only 5300 observations and even the $90^{th}$ percentile has fewer than 30,000. This might seem reasonable; after all, EMU has currently only admitted nineteen countries over its eighteen year history, making for a maximum of (19*18*18=) 6156 annual observations. However, it seems that the larger the number of observations used to estimate γ, the higher it is. Interestingly, the critical thing about the time-dimension is *not* the number of years of EMU in the sample, as is shown in the flat distribution at the top-right of Figure 3. Rather, the span of years included in the study seems to be important, as shown in the bottom-left graph. The median study includes only seventeen years of recent data, but including older data is systematically associated with higher estimates. Finally, and most strikingly, there is a correlation between the size of γ and the number of countries included in the study, as shown in the lower-right graph. Most studies include relatively few countries in their samples; the median number is 22, while even the $90^{th}$ percentile is less than 50. This seems natural; most studies focus on rich large countries comparable to those in EMU and omit smaller and poorer ones.[3] Still, the few studies that include large numbers of countries are associated with higher estimates of the EMU effect on trade/exports.

To summarize, it seems that longer wider spans of data over both time and countries are systematically associated with higher estimates of the effect of EMU on trade/exports. Expressed alternately, the point estimate of the EMU effect seems to rise with the number of observations/years/countries, even if these extra observations are not directly relevant to the phenomenon of interest, EMU. This seem curious.

The evidence in Figure 3 is striking but not completely persuasive. The simple correlations are bivariate, unweighted, and implicit. Accordingly, I turn to meta-regression analysis to investigate the linkages more rigorously. I ask why the estimated effect of EMU on trade/exports, γ, varies across the (45) studies.

Table 2 provides six columns, each corresponding to a different meta-regression. The first, tabulated at the extreme left, includes regressors for six different features of the studies, along with an intercept. Consistent with the ocular evidence of Figure 3, both the (log of the) number of countries and the (log) span of years are positively related to the estimate of the effect of EMU; both coefficients are significantly different from zero at conventional statistical levels. Given these two influences, the (log) number of observations has no extra effect. I also test for the relevance of three features of the study methodology: whether time-varying country fixed effects are included, whether exports are used as the regressand, and whether country-pair dyadic fixed effects are included. None of the latter three effects is significantly associated with variation in the estimates of $\gamma$. This is true when they are considered either one by one or collectively along with log-observations; the P-value for the joint F-test is tabulated in the second-last row of the table. Succinctly, the meta-regression estimates from the far-left column of Table 2 suggests that including more countries and more years raises the estimated effect of EMU on trade/exports, while little more systematically affects $\gamma$.

The remaining columns of Table 2 show that this result is insensitive. First, I present two columns that employ different weighting schemes (instead of precision, I use both the inverse of the number of observations and the inverse of a study's Google Scholar citations). I then show that the three econometric features (choice of regressand and two sets of fixed effects) have little effect on the regression when dropped; they are also collectively and individually insignificant when the sample size regressors are omitted. Finally, when the both the country- and time-dimension of the sample are included, the number of observations remains insignificant.

The meta-regression analysis points to the conclusion that the estimated effect of EMU on trade/exports rise systematically as the number of countries and/or years rises. While this is consistent with the evidence of Figure 3, I am reluctant to over-interpret these results. The meta-regressions do not fit particularly well, with adjusted-$R^2$s less than .3. More importantly for me, many of the results rely on equations with serious theoretical problems (Baldwin and Taglioni, 2007); only seven employ my preferred specification with exports as the regressand,

and include both time-varying exporter/importer and time-invariant dyadic fixed effects (more on this shortly). Most of the latter cover a relatively small span of both countries (the median is 22) and years (the median is 20). The few relevant estimates thus cannot provide much evidence of the relationship between γ estimates and the span of countries and time. Given the dearth of relevant estimates, I check the meta-results with data below, mostly to ensure that I'm not taking too much of a leap of faith.

**3. Empirical Confirmation**

I now confirm the observations made in my meta-analysis, using the technique and data set of Glick and Rose (2016). I do this in part because of the paucity of studies that use my preferred methodology; I will also use my estimates to confirm my interpretation for the variation in study effects. I present my methodology and data set briefly; further details are available in Glick and Rose (2016), and at my website.

My primary objective is to establish the stylized fact that the effect of EMU on trade rises systematically with the span of the data, either across countries or across time. Given my narrow interest, my methodology is straightforward and intended to remain in the background.

<u>Methodology</u>

I use "theory-consistent estimation" of the gravity equation, closely following the suggestions in the recent survey by Head and Mayer (2014). I rely on the "LSDV" (<u>L</u>east <u>S</u>quares with time-varying country <u>D</u>ummy <u>V</u>ariables) technique, which they show works well in many situations.[4] In particular, I estimate:

$$\ln(X_{ijt}) = \gamma EMU_{ijt} + \beta_{CU}CU_{ijt} + \beta_Z Z_{ijt} + \{\lambda_{it}\} + \{\psi_{jt}\} + \{\delta_{ij}\} + \varepsilon_{ijt} \quad (2)$$

where:

- $X_{ijt}$ denotes the nominal value of bilateral exports from i to j at time t, and
- CU is unity if i and j use the same non-euro currency at time t and 0 otherwise.

I estimate this with least squares, using the technique of Guimarães and Portugal (2010).

Including the dyadic (country-pair-) fixed effects is important, since they account for any interactions between countries that affect their trade, so long as these factors do not vary over time. There are many such influences: some are geographic (e.g., distance, sea-access, land borders); others are cultural or historical (common language, legal institutions, colonial history). I follow Glick and Rose (2016) and always include these fixed effects. Thus (2) can only estimate the effect of pair-specific phenomena which are time-varying, like the currency union effect on exports. This model also partials out all country-specific "monadic" phenomena for both exporters and importers, either constant or varying over time; more on this below.

I use the *Direction of Trade* data set assembled by the International Monetary Fund (IMF). This covers bilateral trade between over 200 IMF country codes; not all are countries in the conventional sense of the word. The annual data set spans 1948 through 2013 (with gaps). Bilateral trade on FOB exports and CIF imports is recorded in U.S. dollars; I create an average value of the nominal value of bilateral exports between a pair of countries i and j by averaging i's exports to j and j's imports from i. To this data set, I add a number of other variables, including population and real GDP (in constant dollars) from *World Development Indicators*, supplemented where necessary by the Penn World Table Mark 7.1, and the IMF's *International Financial Statistics*. The CIA's *World Factbook* provides colonial history; the World Trade Organization's website provides data on regional trade agreements.

The focus of my attention are the bilateral dummy variables, unity if the pair of countries was involved in EMU or some other currency union and zero otherwise. By "currency union" I mean that money was essentially interchangeable between the two countries at a 1:1 par for an extended period of time, so that it was trivial to compare prices; hard fixes (such as those of Hong Kong or Denmark) do not qualify. Glick and Rose (2016) construct this series from the IMF's *Schedule of Par Values* and issues of the IMF's *Annual Report on Exchange Rate Arrangements and Exchange Restrictions*, supplemented with information from of *The Statesman's Yearbook.*

All this has solid theoretical underpinnings while being deliberately plain-vanilla from a methodological viewpoint. This, my preferred methodology, enables me to focus on the key aspect of the empirics, namely the relationship between the sample size and the coefficient of interest.[5]

Estimates of the Effect of EMU on Exports

Estimates of (1) using data for all countries, are tabulated in Table 3. There are seven columns, each corresponding to a sample that begins in 1948 and ends in a different year (listed in the top row). The coefficient of interest remains γ, presented in the second row, the partial effect of EMU on (log-) exports. When the sample ends in 2001 (two years after EMU begins and before the physical introduction of the Euro), the coefficient is small in both economic and statistical senses. However, as I add data in two-year increments, the effect continues to grow in economic size and statistical precision. My data ends in 2013; by that point, the point estimate of .43 corresponds to a substantive economic impact of (exp(.43)-1≈) 54%, with a robust t-statistic of over 20, despite the presence of over 50,000 fixed effects. By way of contrast, the other (nuisance) coefficients seem both sensible and stable; the model fits the data well.

Table 3 uses the entire population of available countries. In Table 4, I tabulate estimates of γ when exactly the same empirical model is estimated, but over the set of observations for rich countries, using the World Bank definition of $12,736 for an upper income country. I also tabulate the analogues when the sample includes all actual or eventual EU countries; the estimates that use the entire sample are also included for easy comparison.[6] As in Table 3, more data, either across countries or time, typically delivers a larger estimate of γ; the number of observations is tabulated below estimates of γ. It is particularly striking how the estimate of γ varies with the number of countries; it also moves, though less consistently, with the span of years.

This point is easy to convey graphically in Figure 4, which summarizes the empirics of Table 4. This graph presents the EMU export estimates along with a plus/minus two standard error confidence interval. There are two sources of variation in the samples portrayed in the coefficients of the figure: country span, and time span. The top line connects estimates of the export effect of EMU that use data for all the (more than 200) countries for which I have data.[7] The middle line is similar, but includes only data for upper income countries (as determined by the World Bank), that is, those with real GDP per capita of at least $12,736.[8] While I have data on some 34,000 country-pairs with *some* trade data, there are only around 4,000 pairs of rich countries, essentially an order of magnitude lower. The bottom line is an analogue for the approximately 800 country-pairs are ever inside the EU (European Union, which now includes some 28 countries, 19 of which are in EMU).

There are two interesting things about the estimates in Figure 4. First, the estimates that use data for *all* the countries in the world are consistently higher (and usually significantly higher, at standard confidence levels) than the estimates derived only from rich countries. The latter, in turn, are consistently higher than those that only use EU data. The EMU estimates that employ data from the entire world are positive and economically significant, the upper-income analogues are usually insignificantly different from zero, and the EU data delivers significantly *negative* point estimates of the EMU effect on trade, despite the latter's understandable focus on the data most obviously relevant to EMU.

Second, the estimates tend to rise as the time-span of the sample is expanded, typically (but not always) significantly so. Again, the point estimate of the EMU export effect rises with the number of observations, though in this case the extra observations seem highly relevant to the phenomenon of interest.[9]

All this is reassuringly consistent with the meta-analysis above, and essentially fills in gaps missing in the literature. It points to three conclusions. First, throwing away data easily allows one to estimate a small and/or negative export effect of EMU. Second, increasing the span of time by adding more observations that are relevant to EMU seems to increase the estimated EMU export effect. Finally, increasing the span of countries by adding observations for countries that seem irrelevant to EMU also seems to increase the EMU export effect. While the first two observations are intuitive and appealing, the last seems odd.

Exploring and Interpreting the Fixed Effects

The evidence of both the meta-analysis and empirical work seems clear. The point estimate of the EMU export effect systematically rises as the cross-section of countries expands to include more countries in the sample, even though these extra countries appear to be unconnected to EMU. I now explore this curious result.

Consider the time-varying exporter and importer fixed effects. They are conventionally interpreted as representing multilateral indices of "trade resistance" following Anderson and van Wincoop (2003).[10] As such, they are important in the analysis which follows since trade between a pair of countries depends on bilateral trade barriers relative to multilateral ones (average trade barriers with all trade partners). Trade between a pair of countries can be low either because the pair of countries have either relatively high bilateral or low multilateral trade barriers.

Omitting these fixed effects altogether would be a mistake, given the current state of economic knowledge; Baldwin and Taglioni (2007) refer to this omission as a "gold medal" mistake, while Head and Mayer (2014) refer to the "multilateral resistance/fixed effects revolution." But mis-estimating them may also have serious consequences. Simply dropping small and poor countries may lead to significant selection bias if those countries have systematically different trade resistance, or if the trade resistance of large countries is reflected in trade with smaller countries.[11] I now explore the possibility that truncating the sample by systematically dropping observations in a non-random way leads to relevant specification error.

At first blush, selection bias does not seem to be a purely theoretical problem. One can directly examine the fixed effects from estimates of (2), that is $\{\lambda_{it}\}$ and $\{\psi_{jt}\}$.[12] Consider the estimates tabulated at the extreme right of Table 3, which use data for *all* (211) countries over the *entire* (1948-2013) period of time; I will denote the relevant fixed effects for exporters {$\lambda_{it}$(Full)} and importers {$\psi_{jt}$(Full)}. It turns out that the countries with high fixed (country-time) effects and multilateral trade resistance are large relatively closed countries like the United States and China; those with big negative fixed effects are small countries like Lesotho and Palau. Moreover, the estimated country-time fixed effects, {$\lambda_{it}$(Full)} and {$\psi_{jt}$(Full)}, differ systematically for different types of currency unions. For EMU observations, the average estimated fixed effect (multilateral trade resistance) is large and *positive*, whether for exporter or importer, as tabulated in the middle panel of Table 3. For monetary unions other than EMU, the estimated fixed effects are, on average, large and *negative*. As can be seen from the bottom line of Table 3, a large fraction – over 95% – of the latter observations are dropped when the sample is truncated from using all available observations to employing only those from rich countries.

Table 4 compares estimates of multilateral resistance, that is {$\lambda_{it}$} and {$\psi_{jt}$}, estimated with different samples of data. Consider the extreme right-column, which uses the entire sample of data over time. Using the (>877k) observations for all (>200) countries, $\gamma$(Full) is estimated to be .43 as shown in the top row; one also estimates {$\lambda_{it}$(Full)} and {$\psi_{jt}$(Full)}. For this sample, both the exporter and importer multilateral resistance terms are systematically higher for rich countries than for the non-rich countries that are typically dropped when the sample is truncated, as shown by the averages tabulated in the third panel of Table 4. When the sample is restricted to the (>75k) observations from the rich country sub-set, $\gamma$(Rich) falls to .11, but typical estimates of {$\lambda_{it}$(Rich)} and {$\psi_{jt}$(Rich)} also fall. The fifth panel of Table 4 shows that {$\lambda_{it}$(Full)} and {$\psi_{jt}$(Full)} are systematically higher than {$\lambda_{it}$(Rich)} and {$\psi_{jt}$(Rich)} *even if one restricts attention to the countries in EMU*. The differences are systematic and positive; estimated multilateral resistance falls on average for both exporters and importers, *including those inside EMU*, as one throws away data from poorer countries.[13] For instance, the 2013 estimate of {$\lambda_{it}$(Full)} for France, an EMU member, is 3.70, while {$\lambda_{it}$(Rich)} is 2.94, and {$\lambda_{it}$(EU)} is 2.83; the analogues for {$\psi_{jt}$} are 3.59, 3.03, and 2.33.

Dropping small and poor countries seems to induce bias in the estimates of multilateral resistance. This bias helps explain why estimates of γ change systematically with the breadth of the data sample; the estimated trade effect is systematically biased downward when small/poor countries are omitted from the sample and the fixed effects change accordingly. The reason is clear from Anderson and van Wincoop (2003, p 176); multilateral trade resistance depends positively on trade barriers with *all* trading partners. So dropping small and/or poor countries, which are likely to have systematically different trade resistance, leads to biased estimates of multilateral trade resistance. And, as Anderson and van Wincoop note, higher multilateral resistance leads to more trade. So downward-biased estimates of multilateral resistance biases γ down. Since multilateral trade resistance is a function of all bilateral trade barriers, all trade partners should be included for the most accurate estimates.

## 4. Conclusion

### Summary

Why do estimates of the EMU effect on trade vary so much? To summarize my findings, including more observations – either over time, or especially by country – seems to increase the estimated effect of EMU on trade. In this short paper, I have first established this stylized fact with both meta-analysis and empirics, and then sought to interpret it. The dependency of the estimated EMU export effect on the number of countries is both more striking in the data and less intuitive than the dependency on time. My explanation is that truncating the sample by omitting countries that are small or poor biases downward the estimates of the country-time fixed effects, conventionally interpreted as indices multilateral trade resistance. This leads in turn to a downward bias in the estimated partial effect of EMU on exports. Succinctly, one can shrink the estimated EMU export effect by inappropriately dropping observations. In general, there is little reason to drop data without necessity; in this particular case, it seems important to include as much data as possible when estimating the EMU trade effect. If I include all the data, my estimate of the export-enhancing effect of EMU is around .43, or 54%, an estimate close to the meta-estimate of 47% that Tom Stanley and I produced in our 2005 survey.

The Future Agenda

In this paper, I have presented a resolution to the question posed in the title; estimates of the EMU trade effect vary because researchers choose different samples of countries and years to estimate this effect. Including observations for all available countries and periods of time delivers both the most theoretically sensible and empirically largest effects. This hypothesis seems grossly consistent with the facts.

Before I have real confidence in my explanation why the estimated effect of EMU on trade varies, this works need to be verified more broadly by others. Placebo tests would build confidence. The meta-analysis could be more complete, for instance, exploiting all estimates from the literature, not simply the single "best" from each study. It would also be interesting to examine other study effects or coefficients of interest, particularly the export effects of other (non-EMU) currency unions and regional trade agreements (the EU would be of particular interest). A Monte Carlo study on the effects of truncation bias would be particularly helpful (this may not be trivial, given the large number of fixed effects). An explicit model of the selection bias implicit in the data truncation would also be of value, so long as the estimation is done within the confines of the LSDV model above (again, this may not easy be for the same reason). It would also be good to verify this result with Poisson pseudo-maximum likelihood to account for zeros and missing observations a la Santos Silva and Tenreyro (2006); this is also currently infeasible (at least for me) for purely computational reasons.[14] Much remains to be done.

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**Table 1: Meta-Estimates of the EMU Effect on Trade/Exports**

| Estimator | Sample | Point Estimate | 95% Confidence Interval | | P-value, no Heterogeneity |
|---|---|---|---|---|---|
| | | | Lower | Upper | |
| Fixed | All (45) | .085 | .078 | .091 | .000 |
| Random | All (45) | .116 | .084 | .147 | .000 |
| Random | Export (27) | .140 | .092 | .189 | .000 |
| Random | Dyadic (35) | .126 | .088 | .164 | .000 |
| Random | Monadic (9) | .132 | -.027 | .291 | .000 |
| Random | Preferred (7) | .151 | -.033 | .336 | .000 |

Notes: "Dyadic" denotes country-pair time-invariant fixed effects; "Monadic" denotes time-varying country fixed effects; "Preferred" denotes export regressand, with both dyadic and time-varying country fixed effects.

**Table 2: Meta Regression Analysis of the EMU Effect on Trade/Exports**

| Weight | Std. Err. | $Obs^{-1}$ | $GSCites^{-1}$ | Std. Err. | Std. Err. | Std. Err. |
|---|---|---|---|---|---|---|
| Log Countries | .16<br>(.06) | .15<br>(.05) | .20<br>(.05) | .15<br>(.06) | .11<br>(.04) | |
| Log Years | .14<br>(.05) | .13<br>(.05) | .09<br>(.05) | .11<br>(.05) | .09<br>(.04) | |
| Log Observations | -.05<br>(.04) | -.03<br>(.03) | .01<br>(.04) | -.03<br>(.03) | | |
| Time-Varying Country FE | -.03<br>(.06) | -.04<br>(.07) | -.07<br>(.07) | | | -.00<br>(.07) |
| Export Regressand | .07<br>(.06) | .04<br>(.06) | .05<br>(.06) | | | .05<br>(.06) |
| Dyadic FE | .03<br>(.06) | .02<br>(.06) | -.01<br>(.07) | | | .05<br>(.07) |
| Intercept | -.45<br>(.18) | -.51<br>(.20) | -.87<br>(.29) | -.44<br>(.17) | -.51<br>(.15) | .05<br>(.07) |
| P(value) | .63 | .81 | .78 | | | |
| Adjusted $R^2$ | .26 | .27 | .47 | .27 | .29 | -.04 |

Meta-regression coefficients (weighted by precision unless otherwise noted); standard errors in parentheses. 45 observations. "Obs" denotes number of observations; "GSCites" denotes number of Google Scholar citations. P(value) denotes p-value for joint null hypothesis that effects of log observations, export regressand, and both time-varying and dyadic fixed effects are all zero.

## Table 3: Gravity Estimates for Bilateral Exports, all countries

| Sample ends: | 2001 | 2003 | 2005 | 2007 | 2009 | 2011 | 2013 |
|---|---|---|---|---|---|---|---|
| **γ: European Monetary and Economic Union (EMU)** | .08<br>(.05) | .12<br>(.04) | .17<br>(.03) | .19<br>(.03) | .25<br>(.02) | .36<br>(.02) | .43<br>(.02) |
| **All Non-EMU Currency Unions** | .29<br>(.03) | .29<br>(.03) | .29<br>(.03) | .29<br>(.03) | .29<br>(.03) | .30<br>(.03) | .30<br>(.03) |
| **Regional Trade Agreement** | .35<br>(.01) | .36<br>(.01) | .38<br>(.01) | .41<br>(.01) | .42<br>(.01) | .40<br>(.01) | .39<br>(.01) |
| **Currently Colony** | .19<br>(.03) | .21<br>(.03) | .22<br>(.03) | .23<br>(.03) | .24<br>(.03) | .26<br>(.03) | .27<br>(.03) |
| **Country·Time Fixed Effects** | 17,441 | 18,244 | 19,046 | 19,850 | 20,652 | 21,454 | 22,256 |
| **Dyadic Fixed Effects** | 28,720 | 29,628 | 30,391 | 30,975 | 31,394 | 31,758 | 32,005 |
| **Observations** | 597,565 | 642,571 | 688,519 | 735,025 | 782,047 | 829,708 | 877,736 |
| **$R^2$** | .86 | .86 | .86 | .86 | .86 | .86 | .86 |
| **RMSE** | 1.30 | 1.32 | 1.34 | 1.36 | 1.38 | 1.40 | 1.42 |
| **Average of Exporter/Importer-Year Fixed Effects, $\{\lambda_{it}\}$, $\{\psi_{jt}\}$** | | | | | | | |
| **Exporter, $\lambda_{it}$ EMU Obs.** | 1.51 | 1.56 | 1.65 | 1.64 | 1.59 | 1.51 | 1.45 |
| **Importer, $\psi_{jt}$ EMU Obs.** | 2.02 | 2.02 | 2.06 | 1.94 | 1.71 | 1.53 | 1.36 |
| **Exporter, $\lambda_{it}$ Non-EMU CU Obs.** | -.95 | -.96 | -.99 | -1.02 | -1.05 | -1.09 | -1.22 |
| **Importer, $\psi_{jt}$ Non-EMU CU Obs.** | -1.15 | -1.16 | -1.17 | -1.17 | -1.17 | -1.17 | -1.17 |
| **Effect of shrinking sample from all to only rich countries on non-EMU CU observations** | | | | | | | |
| **Observations Dropped (%)** | 97.2 | 97.1 | 97.0 | 96.9 | 96.9 | 96.8 | 96.8 |

Regressand: log of bilateral exports. Exporter-year, importer-year, and dyadic fixed effects included not reported. Robust standard errors recorded in parentheses. Annual observations for >200 countries, 1948-year tabulated.

## Table 4: Gravity Estimates for Bilateral Exports, different country samples

| Sample ends: | 2001 | 2003 | 2005 | 2007 | 2009 | 2011 | 2013 |
|---|---|---|---|---|---|---|---|
| **EMU Coefficient, Full Sample (γ)** | .08<br>(.05) | .12<br>(.04) | .17<br>(.03) | .19<br>(.03) | .25<br>(.02) | .36<br>(.02) | .43<br>(.02) |
| **EMU Coefficient, Rich Countries (γ)** | .00<br>(.05) | -.01<br>(.04) | -.02<br>(.04) | -.04<br>(.03) | .00<br>(.03) | .07<br>(.03) | .11<br>(.03) |
| **EMU Coefficient, EU Countries (γ)** | -.33<br>(.06) | -.36<br>(.05) | -.32<br>(.04) | -.28<br>(.04) | -.26<br>(.07) | -.25<br>(.03) | -.24<br>(.03) |
| **Observations** | | | | | | | |
| **Full Sample** | 597,565 | 642,571 | 688,519 | 735,025 | 782,047 | 829,708 | 877,736 |
| **Rich Countries** | 42,673 | 46,851 | 51,824 | 57,317 | 62,764 | 68,428 | 75,096 |
| **EU Countries** | 22,887 | 24,341 | 25,788 | 27,350 | 28,891 | 30,434 | 31,982 |
| **Average of Exporter/Importer-Year Fixed Effects, {$\lambda_{it}$}, {$\psi_{jt}$} for rich-country Observations** | | | | | | | |
| **Exporter, $\lambda_{it(}$(Full)** | .94 | .94 | .94 | .95 | .97 | .98 | 1.00 |
| **Importer, $\psi_{jt}$(Full)** | 1.18 | 1.17 | 1.16 | 1.15 | 1.14 | 1.12 | 1.10 |
| **Average of Exporter/Importer-Year Fixed Effects, {$\lambda_{it}$}, {$\psi_{jt}$} for other non-rich Observations** | | | | | | | |
| **Exporter, $\lambda_{it}$(Full)** | -.07 | -.07 | -.08 | -.08 | -.08 | -.09 | -.09 |
| **Importer, $\psi_{jt}$(Full)** | -.09 | -.09 | -.10 | -.10 | -.10 | -.10 | -.10 |
| **Average difference between Full and Rich sample estimates for EMU observations** | | | | | | | |
| **[$\lambda_{it}$(Full) - $\lambda_{it}$(Rich)]** | .65 | .64 | .64 | .62 | .66 | .67 | .67 |
| **[$\psi_{jt}$(Full) - $\psi_{jt}$(Rich)]** | 1.10 | 1.06 | .99 | .95 | .86 | .77 | .68 |

Regressand: log of bilateral exports. Controls included but not recorded: non-EMU currency union; regional trade agreement, colonial relationship, exporter-year, importer-year, and dyadic fixed effects included not reported. Robust standard errors recorded in parentheses. Annual observations, 1948-year tabulated. Full sample includes >200; rich countries have real GDP per capita ≥$12,736; EU countries are all (28) eventual members of the European Union.

**Figure 1: Forest plot of 45 literature estimates of the EMU effect on trade/exports**

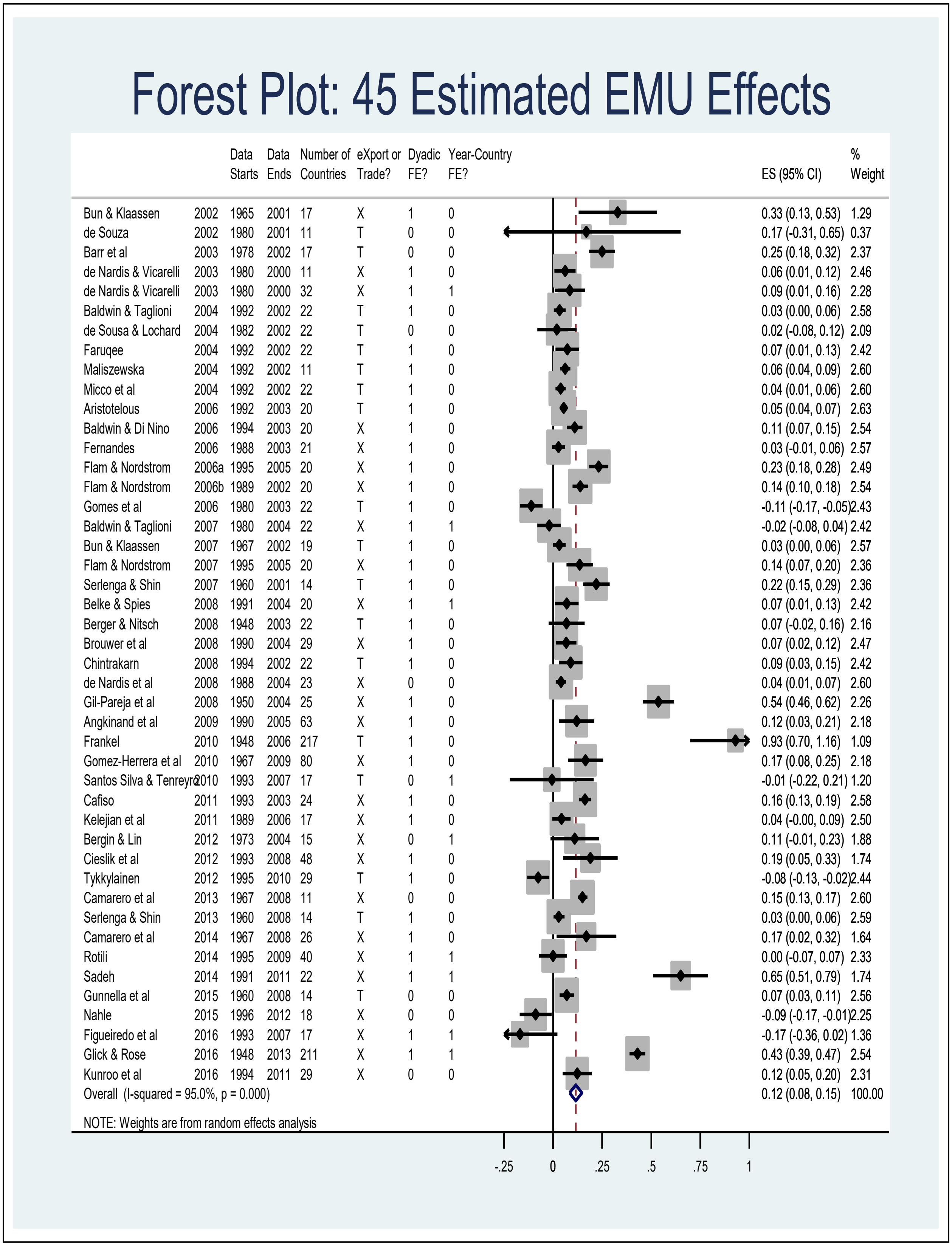


**Figure 2: Funnel plots of literature estimates of EMU effect on trade/exports**

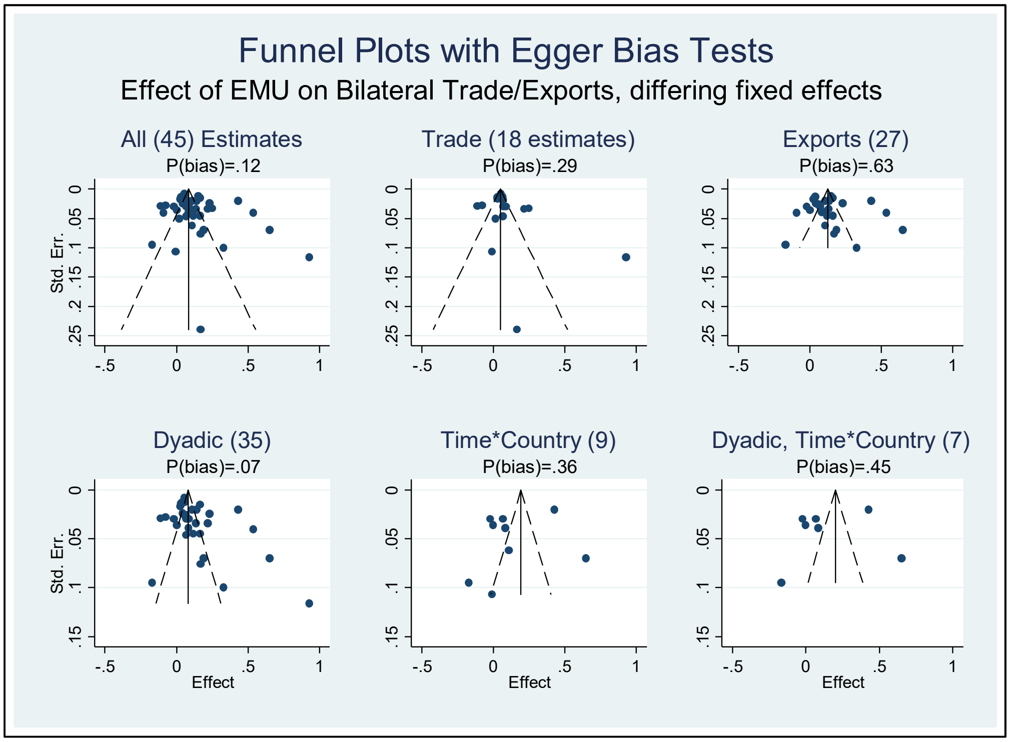



**Figure 3: Relationship between estimates of EMU effect and sample size**

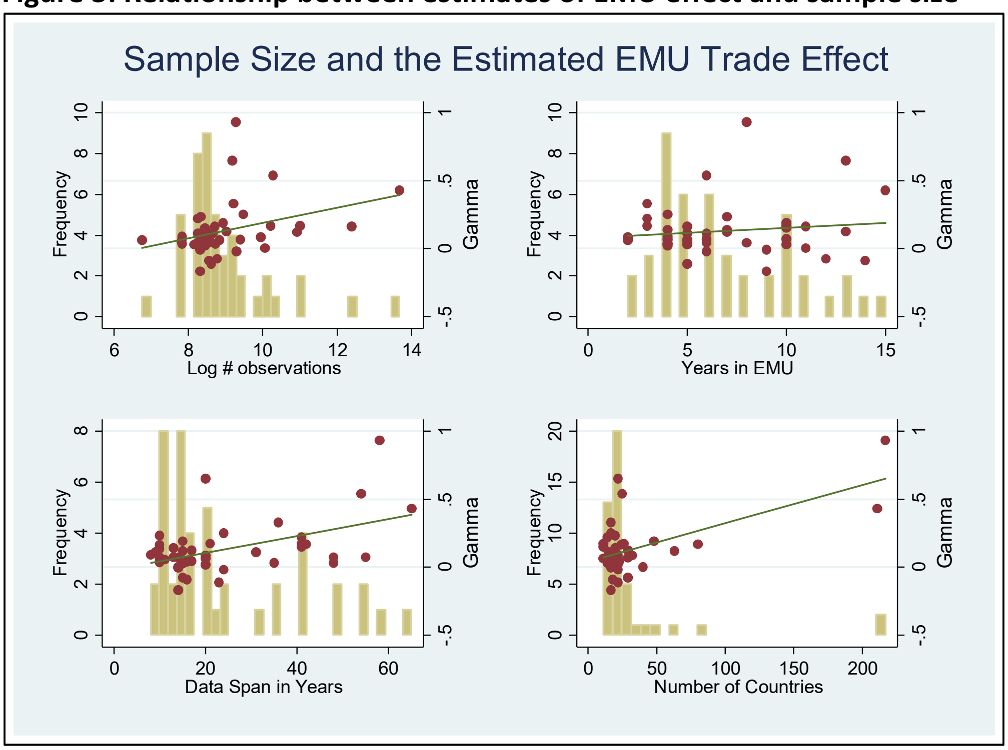
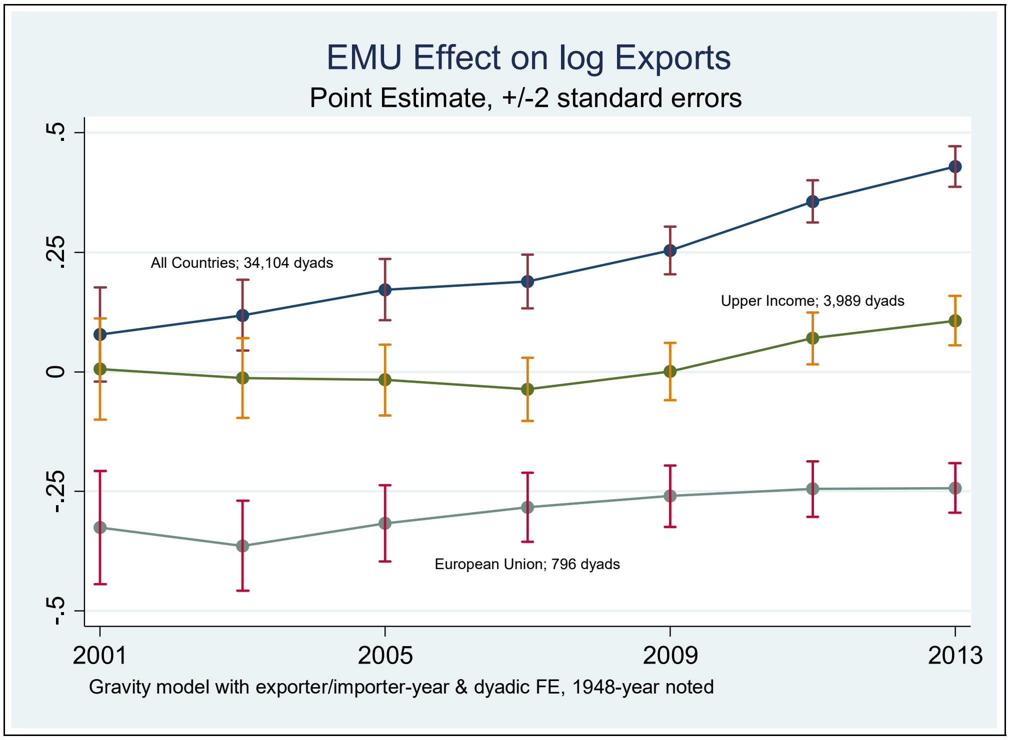


**Figure 4: Estimates of the EMU effect on trade with varying samples**



## Endnotes

[1] Head and Mayer (2014) among others note the potential importance of general equilibrium effects.

[2] This short meta-analysis uses a single "best" estimate from each of the 45 studies, rather than exploiting *all* the estimates from the studies. A more detailed meta-analysis would take advantage of the latter, while adhering to the reporting guidelines proposed by MAER-Net (http://onlinelibrary.wiley.com/doi/10.1111/joes.12008/full).

[3] I used a large number of small and poor countries in my initial work on currency unions and trade because my work was conducted in 1999, before there was any EMU data. At that point, *all* currency unions included only small and poor countries.

[4] This technique allows me to address concerns about multilateral resistance and other general equilibrium effects; more on this below.

[5] I feel especially comfortable doing this since the meta-regression analysis gives me no reason to believe that my preferred methodology affects my estimates of the EMU trade effect.

[6] These are the estimates graphed in Figure 1.

[7] In all cases, the sample begins in 1948; only the end-point varies.

[8] http://data.worldbank.org/about/country-and-lending-groups

[9] Some of this idea is informally expressed by Frankel (2010), who asked a question similar to mine, but provided his answer as a residual explanation after dismissing alternative explanations (lags, non-comparability of samples by country size, and endogeneity). For instance, Frankel writes "… the finding of statistical significance arose only when Rose put together a large enough data set for it to show up … There appears to much useful information from including all 60 years of available data in addition to including developing countries in the entire sample, rather than restricting ourselves to post-1992 observations of European or rich countries … What we find instead is a surprising new result: results reported here suggest that the discrepancy might stem from sample size. …" See also Cîndea and Cîndea (2012).

[10] Other early references include Feenstra (2004) and Redding and Venables (2004). Head and Mayer (2014) provide a tabulated summary of different structural economic interpretations.

[11] If the small poor countries that are typically excluded are disproportionately likely to be involved in currency unions, the effect of currency union on trade may become inestimable for lack of data, at least for currency unions other than EMU.

[12] This would not be sensible in a standard panel data setting, where the fixed effects themselves are estimated inconsistently because of the Neyman-Scott "incidental parameters" problem; inconsistency results as the time span remains fixed and the cross-section (and hence the number of fixed effects) grows asymptotically. In dynamic panel models, the "Nickell-bias" may be large with a small time dimension. Still, a subtlety is implicit in the Forest plot of Figure 1, namely the unusual nature of the panel data sets in this literature. Sixteen of the studies have larger time-series than cross-sectional dimensions, and all the studies have essentially comparable dimensions across years and countries. This differs from the typical panel, as does the fact that the time dimension is non-trivial (the shortest panel has nine observations). Perhaps most importantly, in this literature observations grow over time rather than in the cross-section. All this means that the incidental parameters problem may be of limited relevance in this context; Guimarães and Portugal (2010) provide more discussion and references. Still, there may be some remaining problems; country-year fixed effects in the LSDV model grow with country span, and there may be sampling bias that could be explored with a Monte Carlo exercise; caution is appropriate.

[13] Table 4 reports averages of [$\lambda_{it}$(Full) - $\lambda_{it}$(Rich)] and [$\psi_{jt}$(Full) - $\psi_{jt}$(Rich)] for two sets of observations: Rich countries and EMU observations; these are all positive. To me, this seems to be the most obvious pair of differences to compare over the most obvious span of countries. One could also compare Full to EU and Rich to EU estimates for different sets of observations; with almost no exceptions, these are also large and positive.

[14] That is, there are two types of truncation bias. The more common is the latter, associated with dropping observations with zero or missing trade, i.e., selecting on the basis of values of the regressand. This differs from the former issue, where the sample is truncated on the basis of values of the regressors, particularly country size and income.