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A META-ANALYSIS OF THE EFFECT OF COMMON CURRENCIES ON INTERNATIONAL TRADE

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ABSTRACT

Thirty-four recent studies have investigated the effect of currency union on trade, resulting in 754 point estimates of the effect. This paper is a quantitative attempt to summarize the current state of debate; meta-analysis is used to combine the disparate estimates. The chief findings are that: a) the hypothesis that there is no effect of currency union on trade can be rejected at standard significance levels; b) the combined estimate implies that a bilateral currency union increase trade by between 30% and 90%; and c) the estimates are heterogeneous and not consistently tied to most features of the studies.

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This short paper reviews the recent literature that estimates the effect of common currencies on trade. Meta-analysis is used to provide a quantitative summary of the literature.

The next section briefly reviews the literature qualitatively. Section 2 is the heart of the paper; it provides the quantitative meta-analysis that studies the preferred point estimates of the thirty-four different studies collectively. Section 3 reviews the (over seven hundred) different point estimates tabulated in the literature, while a section on publication bias follows. The paper ends with a short conclusion.

1: A Short History of the Literature

In the summer of 1999, I began to circulate a paper that estimated the effect of currency union on trade; *Economic Policy* subsequently published this paper in 2000. This paper exploited a panel of cross-country data covering bilateral trade between 186 "countries" (really different trading partners) at five-year intervals between 1970 and 1990. Since most of the variation is across pairs of countries rather than time, I used a conventional "gravity" model of trade to account for factors that drive trade (other than monetary arrangements). This equation has now become the standard vehicle for the literature, and takes the form:

$$T_{ijt} = ß_1 D_{ij} + ß_2 (Y_i Y_j)_t + \Sigma_k ß_k Z_{ijt} + \Sigma_t d_t T_t + \gamma CU_{ijt} + u_{ijt},$$

where: T_{ijt} denotes the natural logarithm of trade between countries i and j at time t, {ß} is a set of nuisance coefficients, D_{ij} denotes the log of distance between i and j, Y denotes the log of real GDP, Z denotes other controls for bilateral trade, CU_{ijt} is a dummy variable that is one if countries i and j are in a currency union at t and zero otherwise, and u is a well-behaved disturbance term. The coefficient of interest is γ, which represents the partial effect of currency union on trade, *ceteris paribus.*

In the original study, the trade data was drawn from the *World Trade Data Bank* ("WTDB"), which contains data for a large number of country-pairs (thereby effectively rendering the analysis cross-sectional), though with many missing observations. In this data set, only a small number of the observations are currency unions; further, countries in currency unions tend to be either small or poor (or both).

The surprising and interesting finding was that currency union seemed to have a strong and robust effect on trade. Even using the standard linear gravity model that accounts for most variation in trade patterns, my point estimate was that the coefficient for a currency union dummy variable (which is unity when a pair of countries share a common currency and zero otherwise) has a point estimate of around =1.21. This implies that members of currency unions traded over three times as much as otherwise similar pairs of countries *ceteris paribus*, since exp(1.21)>3. While there was no benchmark from the literature, this estimate seemed implausibly large to me (and many others).[1] Almost all the subsequent research in this area has been motivated by the belief that currency union cannot reasonably be expected to triple trade.

There have been a number of different types of critique. Some are econometric. For instance, Thom and Walsh (2002) argue that broad panel studies are irrelevant to many questions of interest, since most currency unions historically have involved countries that are either small or poor. They adopt a case study approach, focusing on the

1979 dissolution of Ireland's sterling link; Glick and Rose (2002) provide related evidence.

Others have stressed the importance of relying on time-series rather than cross-sectional variation. The time-series approach has the advantage of addressing the relevant policy issue ("What happens to trade when a currency union is created or dissolved?" rather than "Is trade between members of currency unions larger than trade between countries with sovereign currencies?"). This can be done most obviously by using country-pair specific "dyadic fixed effects" with panel data. This is difficult to do sensibly using the WTDB because there is such little time-series variation in currency union membership after 1970 as recognized in my original paper and by e.g., Persson (2001); nevertheless, see Pakko and Wall (2001). However, Glick and Rose (2002) exploit the almost 150 cases of currency union exit and entry they find when the panel analysis is extended back to 1948 using the IMF's *Direction of Trade* data set. See also Fidrmuc and Fidrmuc (2003).

Much of the obsession with the time-series approach (and indeed with the whole area) is concerned with the potential trade effect of Economic and Monetary Union in Europe (EMU). When the area started, the Euro had not been physically introduced. But EMU technically began in 1999, and there is even some trade data since the euro began to circulate in 2002. This more recent data has driven the work of a variety of scholars, including: Barr et al (2003), Bun and Klaassen (2002), de Nardis and Vicarelli (2002), de Souza (2002), and Flam and Nordström, Micco et al (2003). While much of this work might seem premature given the paucity of data from the EMU era, it addresses an issue of compelling policy interest, especially given the debates over EMU-entry of Sweden and the UK.

In my original paper, I stressed that only about 1% of the sample involves pairs of countries in currency unions. Persson (2001) argues that this makes standard regression techniques inappropriate since currency unions are not created randomly, and advocates the use of matching techniques; see also Rose (2001), Tenreyro (2001), and Kenen (2002).

Nitsch (2002a, 2002b) is concerned with aggregation bias, and argues that combining different currency unions masks heterogeneous results. Along the same lines, Levy Yeyati (2003) divides currency unions into multilateral and unilateral currency unions (as did Fatás and Rose, 2001), while Melitz (2001) splits currency unions into those that are also members of either a political union or regional trade area, and others that are neither; see also Klein (2002). Saiki (2002) dis-aggregates total trade into exports and imports.

Tenreyro (2001) argues that sampling the data every fifth year (as I did in my original paper) is dangerous, since trade between members of currency unions may not be large enough to be consistently positive. She advocates averaging trade data over time, and argues that this reduces the (otherwise biased) effect of currency union on trade. While this may be true with the *WTDB* data set employed by Tenreyro, it seems not to be true of the *DoT* data set, where no bias is apparent (see my website for details).

Rather than focusing on post-WWII data, some have extended the data set back to the classical gold standard era. Flandreau and Maurel (2001) and López-Córdova and Meissner (2003) use data sets that include monetary unions from the pre-WWI period.

Estevadeoral, Frantz, and Taylor (2003) estimate a lower bound on the currency union effect by using membership in the gold standard; the inclusion of their estimates imparts a slight downward bias to the meta-analysis below.

A number of researchers have followed my original paper in worrying about reverse causality, including Alesina, Barro and Tenreyro (2003), Bomberger (2002) Flandreau and Maurel (2001), López-Córdova and Meissner (2003), Smith (2002), and Tenreyro (2001); see also Nitsch (2002c).[2] It is possible to also to take a more structural approach as I do in my work with van Wincoop (2001), which also takes account of country-specific effects.

Finally, some research takes a big effect of currency union on trade as given, and seeks to determine the implications of this estimate for e.g., output (Frankel and Rose, 2002) or business cycle co-ordination (Flandreau and Maurel, 2001). Other aspects of the behavior of currency union members are examined by Rose and Engel (2002) and Fatás and Rose (2001). Indeed, in their critique of Rose (2004), Subramanian and Wei (2003) are not directly concerned with currency unions at all; they simply include it as another quantifiable cause of trade.

In all, a number of papers have provided estimates of the effect of currency union on international trade. Obviously many these estimates are highly dependent; they sometimes rely on the same data set, techniques, or authors. Still, there seem to be enough studies to warrant at least a preliminary meta-analysis.

2: Meta-Analysis Across Studies

Meta-analysis is a set of quantitative techniques for evaluating and combining empirical results from different studies. Essentially one treats different point estimates of a given coefficient as individual observations. One can then use this vector of estimates to: estimate the underlying coefficient of interest, test the hypothesis that the coefficient is zero, and link the estimates to features of the underlying studies. Since there are currently a number of studies that have provided estimates of γ, the effect of currency union on trade, meta-analysis seems an appropriate way to summarize the current state of the literature. Stanley (2001) provides an excellent recent review and further references.

One begins meta-analysis by collecting as many estimates of a common effect as possible. To my knowledge, there are thirty-four papers that provide estimates of the effect of currency union on bilateral trade, which are denoted γ. I tabulate these in the appendix, along with the associated estimates of γ (and its standard error) that seems to be most preferred or representative (if a preferred estimate is not available). While I have strong views about the value of some of these estimates (or lack thereof), I weigh each estimate equally, simply because there is no easily defensible alternative weighting scheme.

The most basic piece of meta-analysis is a test of the null hypothesis $\gamma=0$ when the thirty-four point estimates (and their standard errors) are pooled across studies. This classic test is due originally to Fisher (1932) and uses the p-values from each of the (34) underlying γ estimates. Under the null hypothesis that each of the p-values is independently and randomly drawn from a normal [0,1] distribution, minus twice the sum of the logs of the p-values is drawn from a chi-square. The hypothesis can be rejected at any standard significance level, since under the null hypothesis; the test-statistic of 1272 is drawn from chi-squared(68).[3]

I tabulate meta-estimates of the currency effect on trade in Table 1. I provide both "fixed effect" and "random effect" meta-estimates that are common in the area. The former are based on the assumption that a single fixed effect underlies every study, so that, in principle, if every study were infinitely large, every study would yield an identical result. This is the same as assuming there is no heterogeneity across studies. By way of contrast, the random effects estimator assumes that the studies are estimating different treatment effects, drawn from a distribution whose mean is of interest.[4]

Manifestly, there is considerable heterogeneity; the fixed and random effect estimators are not similar in magnitude. However, both estimates are both economically substantial; the smaller fixed effect estimate of γ indicate that currency union raises trade by 33% (as ln(.29)-1=.33), while the random effect estimate indicates that the effect is more like 90%. Also, none of these conclusions change if my six studies are dropped; the test-statistic rejects the hypothesis of no effect, as under the null of no effect, 721 is drawn from chi-squared(54).

There is little indication that any single study is especially influential in driving these results. If the studies are omitted from the meta-analysis one by one, one finds the following (fixed-effect) point estimates for γ (tabulated along with a 95% confidence interval):

While I tried to choose the preferred/representative estimates to match the intentions of the authors, I did … choose them. An alternative way to proceed is to use a more mechanical procedure to choose the underlying estimates of γ for the meta-analysis.

This is easy, since each of the underlying studies provides a number of individual γ estimates. Thus, an alternative I now deploy is to use the median estimates of γ from the underlying studies to construct an alternative set of γ estimates (and associated standard errors) suitable for meta-analysis. I also use the estimates at the 25^{th}, 10^{th}, and 5^{th} percentiles.[5] Table 3 repeats the meta-analysis using these four alternative data sets. The default "preferred" estimates from table 1 are tabulated at the top to facilitate comparison.

The pooled meta-estimate of γ falls as one moves away from the median estimate towards estimates that are lower within individual studies (by design). (It is also interesting – and reassuring – to note that the median estimates are higher than my preferred estimates!) But it is interesting to note that even using the γ estimates taken from the 5^{th}-percentile of each underlying study, the hypothesis of no effect of currency union on trade can be rejected at conventional significance level. Further, all the effects are economically substantive. The lower bound for the lowest estimate is .10, implying an effect of currency union on trade of over ten percent.

One might then ask which design features of the individual studies account for the differences across individual estimates of γ. It would be fun and interesting to explain the variation in γ estimates across studies with a large number of study characteristics. Unfortunately, given the paucity of studies, it does not seem wise to use multivariate meta-regression techniques very intensively. Nevertheless, I report in Table 4a the results of a series of bivariate meta-regressions. Each row tabulates the intercept and slope coefficient from a different bivariate regression, where the regressand is the set of thirty-four γ estimates, and the independent variable is listed at the left of the table. I then combine the most statistically significant variables together in some multivariate meta-analysis in Table 4b.

There are three interesting positive results in Table 4. First, there is not a positive relation between the number of observations and γ. The fact that there is no positive (let alone significant) relation between the sample size and the estimates of γ raises a seriously worrying question as to whether the underlying empirical phenomenon is authentic (Stanley, 2001). Second, papers that I have co-authored have consistently higher point estimates of γ. Finally, papers that are focused on the Euro consistently find a lower effect of currency union on trade. That may occur because there is little data yet on the EMU era, or because the effect is indeed small. Time will tell.

To summarize: the meta-analysis indicates three strong, and one weak finding. First, the hypothesis that there is no effect of currency union on trade can be rejected at standard significance levels when the results from the individual studies are pooled. Second, the pooled effect is not just positive but economically significant, consistent with the hypothesis that currency union raises trade by an economically significant amount. Third, studies that I have co-authored find a higher effect of currency union, while studies that focus on the Euro find a lower effect. Finally the preferred estimates of γ from individual studies are not closely linked to most characteristics of the studies.

3: Different Estimates of g and its Significance within Individual Studies

Most of the thirty-four studies provide many different estimates of γ. For instance, my original paper provided over fifty estimates of γ as a result of sensitivity analysis. In all, there are currently 754 estimates of γ (but fewer associated t-statistics for the hypothesis of an insignificant γ, since these were not always provided). Simply averaging across the 754 estimates of γ produces a mean of .86; the average t-ratio is 5.3.[6]

I provide a number of histograms of the various γ estimates and their t-statistics in Figure 1. First, I provide histograms of all point estimates and their t-ratios at the top-left of the figure. That turns out to be rather uninformative given the presence of outliers; accordingly in the middle-left I provide analogues where γ is restricted to lie in (-2, 2). At the bottom, I provide another pair of analogous graphs, restricting the range of γ further to (0, 1.2). At the right of the figure I cut these data yet another way, and provide histograms of γ (constrained to be between -2 and 2) estimated by myself and others.

What does the graphic show? The vast majority of the point estimates of γ are positive; only 60 of the 754 (<8%) are negative. Many are also economically large; 325 (43%) exceed .69 in magnitude, a number that implies that currency union is associated with a doubling of trade, while 517 (69%) exceed .22, implying that currency union increases trade by 25%.

It is clear that many of the estimates are also statistically significant. The median t-statistic is 4.2; over three-quarters (77% = 479/626 estimates with t-ratios) exceed 2. My t-ratios tend to be larger than those of others, but seventy percent (=337/482) of the t-statistics of others are at least two (the median is 3.6).

Finally, one can also combine the different estimates that exist within the thirty-four studies, on a paper-by-paper basis. Table 5 provides thirty-four rows (one for each study), which perform meta-analysis within the individual study to arrive at both fixed- and random-effect estimates of γ. I also tabulate the p-values for the z-statistics which test the null hypothesis Ho: γ=0. The number of estimates provided by each study is tabulated, as is the p-value for a test of heterogeneity across the estimates (a low value indicates the presence of heterogeneity).

Table 5 clearly shows heterogeneity across γ estimates. While only one is significantly negative (de Souza, fixed effects) and most are significantly positive, they vary considerably.

4: Publication Bias

The analysis above does not rely on published articles; indeed, fourteen of the thirty-four studies are circulating as working papers. Still, authors may be reluctant even to circulate work if they have certain results (e.g., results which corroborate the disputed currency union effect – or the reverse). Or, researchers with small sample size may pre-test their results extensively to corroborate their preferred hypotheses. Thus, it is interesting to test for publication bias in this area.

I begin with "funnel plots" of gamma against its standard error, a standard tool for publication bias. One is searching for signs of asymmetry, indicating that studies with equal precision disproportionately find either small or large results. Asymmetry at the top of the graph, (where studies with high precision – low standard errors – are plotted) may give especially compelling evidence of publication bias.

The preferred estimates from the 34 studies are graphed in a funnel plot at the top left corner of Figure 2, and there is indeed evidence of asymmetry to the right of the plot, consistent with publication bias. This is less apparent in the funnel plot immediately below, which uses the number of observations on the ordinate instead of the inverse of the standard error, a different measure of precision. A different graphical detail, the

"Galbraith plot" of the t-ratio for γ against its standard error, is in the lower left corner of the figure. A regression line, constrained to go through the origin, is portrayed along with a 95% confidence interval, with slope equal to the overall gamma effect. The position of each study on the x-axis indicates the weight allocated to it in meta-analysis. In the absence of heterogeneity, one would expect most studies to lie within the confidence interval. That is, there is again strong evidence of heterogeneity.

The other graphics in Figure 2 are funnel plots for different measures of gamma. At the top of the middle column, I use the within-study median estimates of γ, and below I use the estimates of γ from the 25^{th} and 10^{th} percentiles within study (discussed above and analyzed in Table 3). At the top of the right column, I provide a funnel plot of the 5^{th} Percentile estimates. The median and 25^{th} percentile estimates indicate asymmetry to the right. The middle right funnel plot portrays all 626 estimates (those with standard errors). Since this is dominated by a few outliers, in the bottom right I constrain γ to the range of (-2, 2). Again there is evidence of asymmetry to the right.

It seems there is visual evidence of publication bias from the funnel plots. This can be tested more rigorously with statistical tests which are analogues to various aspects of the visual funnel plots. Begg and Mazumdar (1994) test for publication bias by checking if gamma estimates are correlated with their variances. Egger et al (1997) focus on the intercept in a regression of gamma on its precision (the inverse of the standard error), and interpret significance as evidence of publication bias. In practice, the Begg-Mazumdar test delivers a tiny correlation which is insignificant at over the .9 level. This indication of publication bias is corroborated by the Egger et al test, which indicates significant bias (the intercept has a t-statistic over 4, significant at all conventional levels). It is interesting to note that this is true even if my studies are dropped; both the Begg- Mazumdar and Egger et al tests indicate publication bias. Further, if one replaces the preferred estimates with the within-study median estimates (or those from the other percentiles used above), the indications of publication bias persist. Finally, both tests indicate publication bias if one uses all the estimates of gamma from within the individual studies.

There is strong evidence of publication bias. This could occur for a number of reasons. The effect of currency union on trade is an intensely political issue, especially in Europe, the political preferences of researchers may well be affecting reported analysis.

5: Conclusion

I do not wish to overstate the results of a meta-analysis like this. Thirty-four studies sounds like a lot. But, the studies are dependent and not all of equal interest, two features that I have ignored above. The different estimates of this effect are heterogeneous both across and within studies, and cannot be linked to study features such as the sample size. There is persuasive evidence of publication bias. Thus it would be unreasonable for anyone to have too much confidence in the meta-analytic estimate of the effect of currency union on trade.

That said, a quantitative survey of the literature shows reasonably strong evidence that currency union has a positive effect on trade. When the estimates are examined collectively, this effect is large in terms of both economic and statistical significance, implying that currency union seems typically to be associated with a significant increase of trade, ranging from over 30% up to 90%.

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	Pooled Estimate of g	Lower Bound of 95% CI	Upper Bound of 95% CI	P-value for test of no effect
Fixed	.29	.27	.31	.00
Random	.64	.51	.77	.00
Fixed, without Rose	.22	.19	.24	.00
Random, without Rose	.53	.40	.66	.00

Table 1: Meta-Analysis of Currency Union Effect on Trade (γ)

Study Omitted:	Coefficient	95% CI, lower	95% CI, upper
Rose	.28	.26	.30
Engel-Rose	.29	.26	.31
Frankel-Rose	.28	.26	.30
Rose-van Wincoop	.28	.26	.31
Glick-Rose	.27	.25	.29
Persson	.29	.26	.31
Rose	.26	.24	.29
Honohan	.29	.26	.31
Nitsch	.29	.26	.31
Pakko-Wall	.29	.27	.31
Walsh-Thom	.29	.27	.31
Melitz	.29	.26	.31
Lopez-Cordova and Meissner	.29	.26	.31
Tenreyro	.29	.26	.31
Levy Yeyati	.29	.26	.31
Nitsch	.29	.26	.31
Flandreau and Maurel	.26	.24	.29
Klein	.29	.26	.31
Estevadeoral, Frantz, and Taylor	.29	.27	.31
Alesina, Barro and Tenreyro	.29	.26	.31
Smith	.29	.26	.31
Bomberger	.30	.28	.32
Melitz	.28	.26	.30
Saiki	.29	.26	.31
Micco, Stein, Ordonez	.34	.31	.36
Kenen	.29	.26	.31
Bun and Klaassen	.29	.26	.31
de Souza	.29	.27	.31
de Sousa and Lochard	.28	.26	.30
Flam and Nordström	.35	.33	.38
Barr, Breedon and Miles	.29	.27	.32
de Nardis and Vicarelli	.30	.28	.33
Rose	.28	.26	.30
Subramanian-Wei	.28	.26	.30
Combined	.29	.27	.31

Table 2: Sensitivity of Meta-Analysis of g to Individual Studies (Fixed Effects)

		Pooled γ Estimate	Lower Bound, 95% CI	Upper Bound, 95% CI	P-value for Ho: no effect
“Preferred”	**Fixed**	.27	.25	.29	.00
“Preferred”	**Random**	.64	.51	.76	.00
Median	**Fixed**	.34	.31	.38	.00
Median	**Random**	.82	.62	1.01	.00
25th-Percentile	**Fixed**	.18	.15	.20	.00
25th-Percentile	**Random**	.52	.38	.67	.00
10th-Percentile	**Fixed**	.12	.10	.14	.00
10th-Percentile	**Random**	.37	.24	.51	.00
5th-Percentile	**Fixed**	.11	.10	.13	.00
5th-Percentile	**Random**	.38	.27	.49	.00

Table 3: Sensitivity of Meta-Analysis of γ to Choice of “Preferred” Estimate

Study Characteristic	**Slope Coefficient** (|z-statistic|)	**Intercept** (|z-statistic|)
Number of Observations in study	8.0 e-7 (.9)	.60 (6.7)
Focus on EMU Observations	-.55 (3.9)	.79 (10.6)
Short-Run Focus	-.42 (2.6)	.74 (9.3)
Standard Error of γ	.98 (1.4)	.49 (3.8)
Dummy for Rose as Author	.46 (2.8)	.54 (6.9)
Dummy for mainly cross-section or panel study	.46 (2.2)	.25 (1.2)
Number of Countries in study	.001 (1.6)	.46 (3.5)
Number of Years in study	.002 (0.4)	.59 (3.9)
Dummy for post-WWII study	-.10 (0.4)	.74 (3.0)

Table 4a: Meta-Analysis: Bivariate Determination of g Across Studies

Study Characteristic	**Slope Coefficient** (|z-statistic|)	**Slope Coefficient** (|z-statistic|)
Focus on EMU Observations	-.57 (2.8)	-.50 (4.0)
Dummy for Rose as Author	.40 (2.6)	.39 (2.8)
Dummy for mainly cross-section or panel study	.19 (1.0)	
Short-Run Focus	.14 (0.7)	
Intercept	.50 (2.9)	.69 (9.0)

Table 4b: Meta-Analysis: Multivariate Determination of g Across Studies

Study		Coefficients	Coeff=0 (p-value)	No. of Estimates	Heterogeneity (p-value)
Rose	Fixed	1.289	0.000	52	0.00
	Random	1.311	0.000		
Engel-Rose	Fixed	1.350	0.000	5	0.78
	Random	1.350	0.000		
Frankel-Rose	Fixed	1.631	0.000	5	0.02
	Random	1.634	0.000		
Rose-van Wincoop	Fixed	0.230	0.000	18	0.00
	Random	0.649	0.000		
Glick-Rose	Fixed	0.697	0.000	37	0.00
	Random	0.772	0.000		
Persson	Fixed	0.647	0.000	6	0.11
	Random	0.586	0.000		
Rose	Fixed	0.824	0.000	17	0.00
	Random	1.060	0.000		
Honohan	Fixed	0.352	0.000	12	0.00
	Random	0.356	0.052		
Nitsch	Fixed	3.003	0.000	83	0.00
	Random	1.551	0.000		
Pakko-Wall	Fixed	0.874	0.000	6	0.00
	Random	0.332	0.350		
Walsh-Thom	Fixed	-0.008	0.574	7	0.00
	Random	0.020	0.542		
Melitz	Fixed	1.888	0.000	6	0.00
	Random	1.906	0.000		
Lopez-Cordova and Meissner	Fixed	0.723	0.000	47	0.38
	Random	0.722	0.000		
Silvana Tenreyro	Fixed	0.803	0.000	4	0.03
	Random	0.714	0.000		
Levy Yeyati	Fixed	1.014	0.000	19	0.02
	Random	1.055	0.000		
Nitsch	Fixed	0.464	0.000	8	0.00
	Random	0.429	0.009		
Flandreau and Maurel	Fixed	0.941	0.000	8	0.00
	Random	0.903	0.000		
Klein	Fixed	0.090	0.013	25	0.00
	Random	0.370	0.047		
Estevadeoral, Frantz, and Taylor	Fixed	0.433	0.000	18	0.01
	Random	0.450	0.000		
Alesina, Barro and Tenreyro	Fixed	1.159	0.000	8	0.00
	Random	1.649	0.000		
Smith	Fixed	1.007	0.000	17	0.00
	Random	1.118	0.000		
Bomberger	Fixed	0.205	0.000	6	0.00
	Random	0.315	0.006		
Melitz	Fixed	1.312	0.000	13	0.99
	Random	1.312	0.000		
Saiki	Fixed	1.162	0.000	16	0.00
	Random	0.520	0.008		
Micco, Stein, Ordonez	Fixed	0.098	0.000	54	0.00
	Random	0.130	0.000		

Kenen	Fixed Random	1.081 0.988	0.000 0.000	10	0.01
Bun and Klaassen	Fixed Random	0.330 0.330	0.000 0.001	1	n/a
de Souza	Fixed Random	-0.143 -0.018	0.000 0.714	30	0.00
de Sousa and Lochard	Fixed Random	1.706 1.698	0.000 0.000	14	0.00
Flam and Nordström	Fixed Random	0.150 0.149	0.000 0.000	49	0.00
Barr, Breedon and Miles	Fixed Random	0.234 0.234	0.000 0.000	2	0.44
de Nardis and Vicarelli	Fixed Random	0.090 0.090	0.000 0.001	2	0.90
Rose	Fixed Random	0.905 0.988	0.000 0.000	10	0.00
Subramanian-Wei	Fixed Random	1.142 1.142	0.000 0.000	11	1.0

Table 5: Within-Study meta-estimation of γ

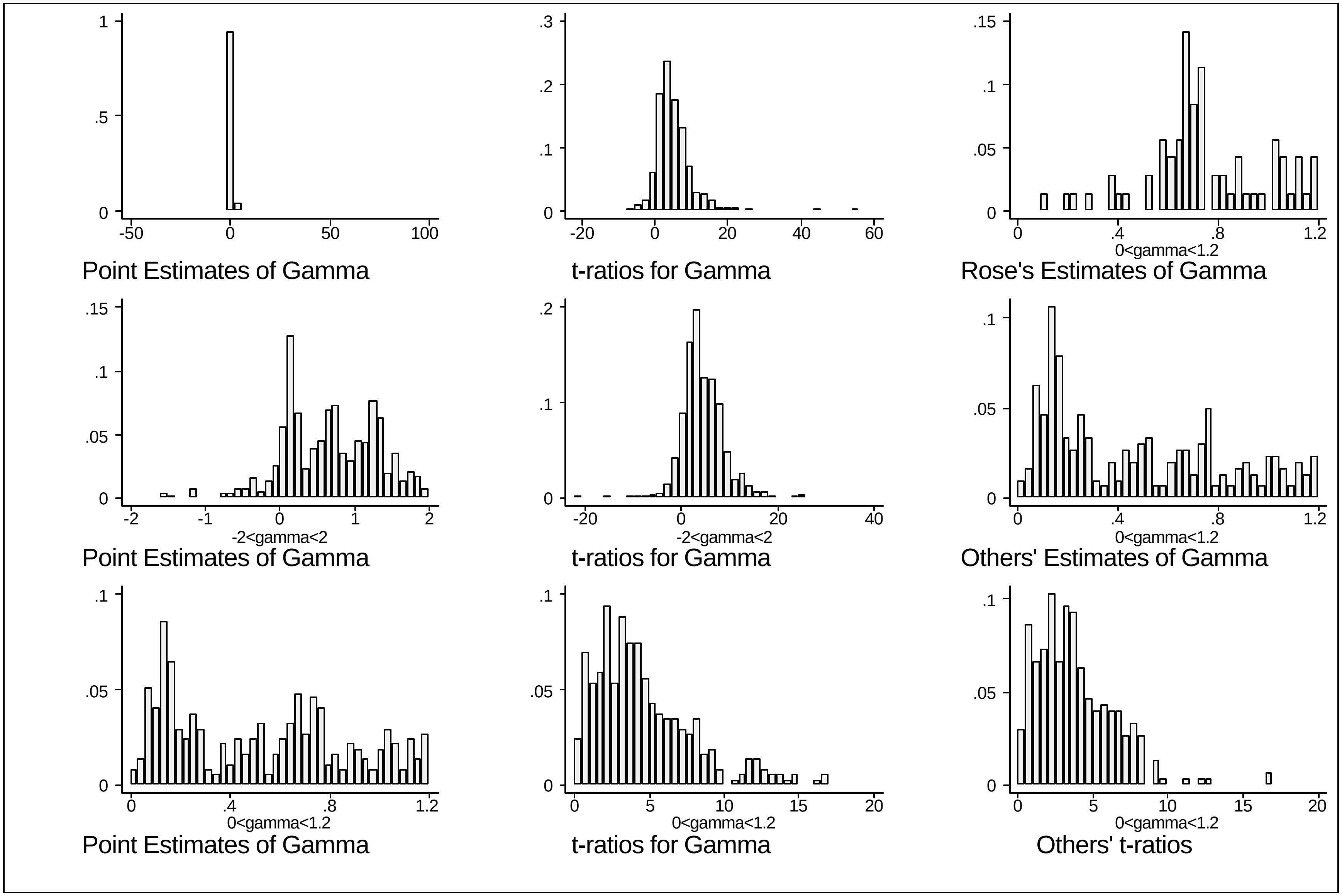

Figure 1: The Estimated Effect of Currency Union on Trade

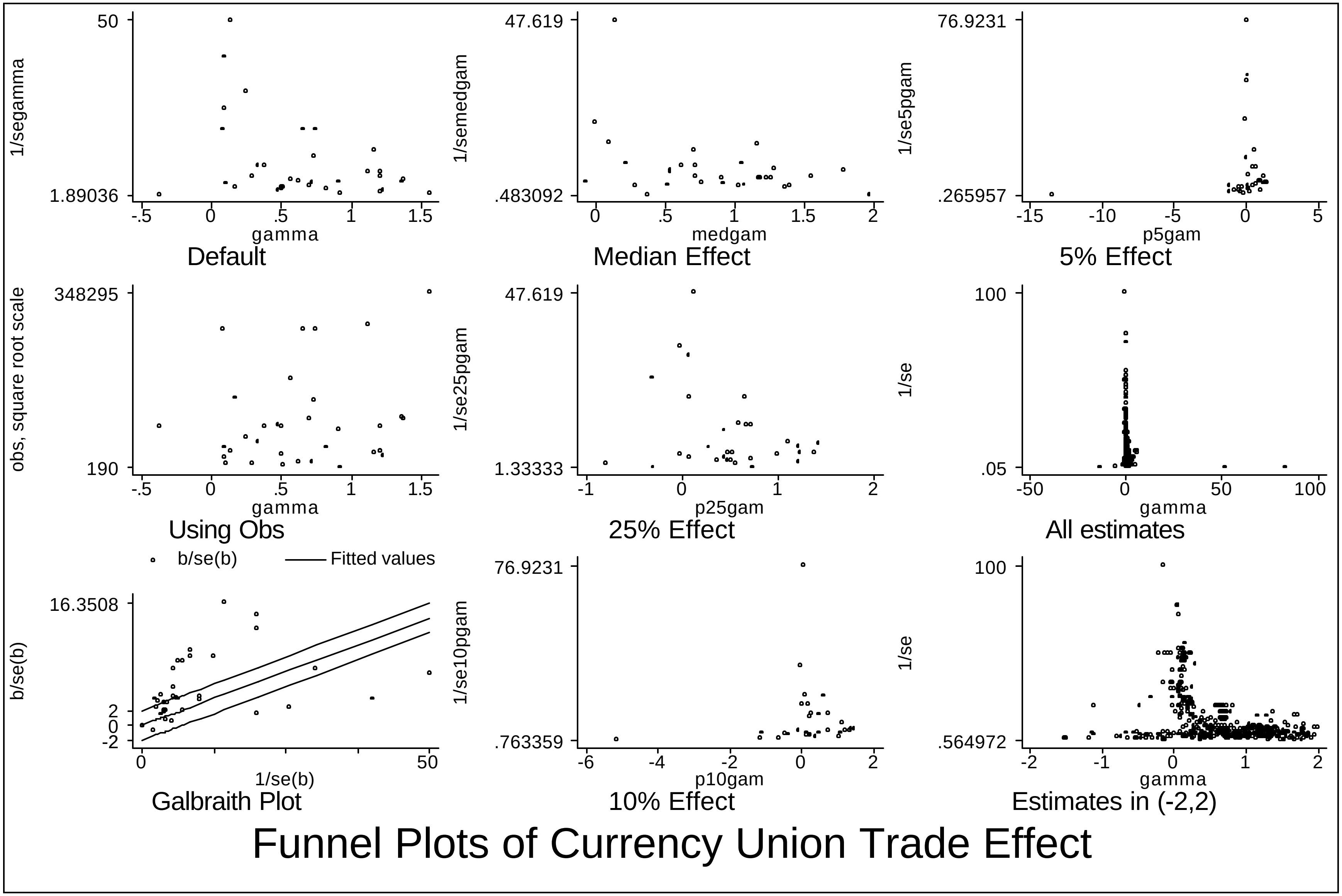

Figure 2: Funnel Plots for Publication Bias

Appendix: Estimates of the Effect of Currency Union on Trade

Author	Year	γ	s.e. of γ
Rose	2000	1.21	0.14
Engel-Rose	2002	1.21	0.37
Frankel-Rose	2002	1.36	0.18
Rose-van Wincoop	2001	0.91	0.18
Glick-Rose	2002	0.65	0.05
Persson	2001	0.506	0.257
Rose	2001	0.74	0.05
Honohan	2001	0.921	0.4
Nitsch	2002b	0.82	0.27
Pakko and Wall	2001	-0.378	0.529
Walsh and Thom	2002	0.098	0.2
Melitz	2001	0.7	0.23
López-Córdova and Meissner	2003	0.716	0.186
Tenreyro	2001	0.471	0.316
Levy Yeyati	2003	0.5	0.25
Nitsch	2002a	0.62	0.17
Flandreau and Maurel	2001	1.16	0.07
Klein	2002	0.50	0.27
Estevadeoral, Frantz, and Taylor	2003	0.293	0.145
Alesina, Barro and Tenreyro	2003	1.56	0.44
Smith	2002	0.38	0.1
Bomberger	2002	0.08	0.05
Melitz	2002	1.38	0.16
Saiki	2002	0.56	0.16
Micco, Stein, Ordonez	2003	0.089	0.025
Kenen	2002	1.2219	0.305
Bun and Klaassen	2002	0.33	0.1
de Souza	2002	0.17	0.24
de Sousa and Lochard	2003	1.21	0.12
Flam and Nordström	2003	0.139	0.02
Barr, Breedon and Miles	2003	0.25	0.033
de Nardis and Vicarelli	2003	0.061	0.027
Rose	2004	1.12	0.12
Subramanian-Wei	2003	0.732	0.08

Estimates of γ and standard error from ln(Trade) = γCurrencyUnion + controls + error

Endnotes

[1] Actually, make that many many others.

[2] This also seems to be true of Ritschl and Wolf (2003), though the author is not able to grasp fully the subtlety of their paper which seems to use fixed exchange rates interchangeably with currency unions.

[3] Edgington's (1972) small sample correction leads to the same conclusion.

[4] http://www.cochrane-net.org/openlearning/HTML/mod13.htm. To elaborate: the fixed effect assumption is that differences across studies are only due to within-study variation. By way of contrast, random effects models consider both between-study and within-study variability and assume that the studies are a random sample from the universe of all possible studies; http://www.poems.msu.edu/InfoMastery/Overviews/steps_in_a_metaanalysis.htm. See also http://www.pitt.edu/~super1/lecture/lec1171/index.htm.

[5] Thus, my initial study contains 52 estimates of γ. The median of these is 1.285 (with standard error of .13). The 25th percentile estimate is 1.1 (.14); the 10th percentile is 1.09 (.26); and the 5th percentile estimate is .96 (.15). If there is an even number of estimates in the underlying study, I choose the higher estimate when e.g., the median lies between two estimates. Three studies – Bun and Klaassen (2002), Barr et al (2003), and de Nardis and Vicarelli (2003) do not contain enough point estimates to allow them to be included in this exercise.

[6] For the 626 estimates with standard errors, the average estimate of γ is 1.00, again with an average t-ratio of 5.3.