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 this effect. This paper uses meta-analysis to combine, explain, and to summarize these disparate estimates of common currency trade effects. The hypothesis that there is no effect of currency union on trade is easily and robustly rejected at standard significance levels. Combining these estimates implies that a currency union increases bilateral trade by between 30% and 90%. Although there is evidence of publication selection, there is also evidence of a genuine positive trade effect beyond publication bias.

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1. Introduction

The economic effects of monetary institutions and policy have always been a central area of economic interest and research. Yet, the recent Economic and Monetary Union of Europe (EMU) has focused much attention on the potential consequences of common currencies (*e.g.*, the Euro). Economists widely believe that monetary unions lower inflation and promote trade. Still, many are surprised that the magnitude of the observed trade effect is so large. Although estimates vary greatly, studies often find that currency union doubles, or even triples, bilateral trade.

It is the purpose of this review to use meta-analysis to summarize, investigate, and more accurately estimate the common-currency trade effect. Meta-analysis can improve the assessment of this important economic parameter by combining all of the estimates, investigating the sensitivity of the overall estimate to variations in underlying assumptions, identifying and filtering out publication bias, and by explaining variations among reported estimates through meta-regression analysis. Our meta-analysis confirms a robust, economically important, positive trade effect from monetary union.

2. A Short History of the Literature

The current interest in the trade effect of common currencies began with Rose (2000). This paper exploits a panel of cross-country data covering bilateral trade between 186 different trading partners at five-year intervals between 1970 and 1990. Since most of the variation is across pairs of countries rather than time, this research uses 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 assessing trade effects. It takes the form:

$$T_{ijt} = ß_1 D_{ij} + ß_2 (Y_i Y_j)_t + \Sigma_k ß_k Z_{ijt} + \Sigma_t d_t T_t + \gamma CU_{ijt} + u_{ijt}, \qquad (1)$$

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 is the log of real GDP, Z represents 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, zero otherwise, and u is a wellbehaved 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"). The WTDB contains data from a large number of country-pairs, thereby effectively rendering the analysis cross-sectional. 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 is that currency union seemed to have a very large effect on trade. Even after using the standard linear gravity model that accounts for most variation in trade patterns, the coefficient for a currency-union dummy variable has a point estimate of around 1.2 (Rose 2000). This estimate implies that members of currency unions traded over three times as much as otherwise similar pairs of countries *ceteris paribus*— $e^{1.2}>3$. While there was no previous benchmark in the literature, this estimate seemed implausibly large to nearly everyone. 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.

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. However, because there is little time-series variation in currency union membership after 1970 in the WTDB, this approach is difficult to accomplish successfully (Rose 2000, Persson 2001, Pakko and Wall 2001). Nonetheless, Glick and Rose (2002) exploit the almost 150 cases of currency union exit and entry that they find when the panel analysis is extended back to 1948 using the IMF's *Direction of Trade* (DoT) data set—also see 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 this area of research began, the Euro had not been physically introduced. There now exist some data since the euro began to circulate in 2002, and the EMU technically began in 1999 in any case. 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 in Sweden and the UK.

Only about 1% of the sample involves pairs of countries in currency unions (Rose, 2000). 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 similar lines, Levy Yeyati (2003) divides currency unions into multilateral and unilateral currency unions, as did Fatás and Rose (2001). 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—also see Klein (2002). Saiki (2002) dis-aggregates total trade into exports and imports.

Tenreyro (2001) argues that sampling the data every fifth year, as did Rose (2000), is dangerous. Trade between members of currency unions may not be large enough to give consistently positive results. She advocates averaging trade data over time and argues that this reduces the (otherwise biased) effect of currency union on trade. While this might be a problem with the *WTDB* data set employed by Tenreyro, it seems not to be an issue with the *DoT* data set, where no bias is apparent.

Rather than focusing on post-WWII data, some have extended the data set back to the 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 Rose (2000) 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).[1] It is also possible to take a more structural approach that accounts for country-specific effects (Rose and van Wincoop 2001).

Finally, some research takes the large effect of currency union on trade as given and seeks to determine the implications of this estimate (Frankel and Rose 2002, 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 substantial number of papers have provided estimates of the effect of currency union on international trade with wide differences in the reported estimates. Yet, some estimates are highly dependent, being generated by the same data, methods or authors. Nonetheless, there are enough studies to warrant at least a provisional meta-analysis.

3: Meta-Analysis across Studies

Meta-analysis is a set of quantitative techniques for evaluating and combining empirical results from different studies. Essentially, different point estimates of a given coefficient may be treated as individual observations. Once compiled, this vector of estimates may be used to test the hypothesis that the coefficient is zero, link the estimates to features of the underlying studies, and to better estimate the underlying coefficient of interest, Because there are a sufficient number of studies that have provided estimates of the effect of currency union on trade, meta-analysis seems an appropriate way to summarize the current state of the literature. See Stanley (2001) for a recent review with further references and also the other papers in this *Special Issue on Meta-Analysis.*

One begins meta-analysis by collecting as many estimates of a common effect as possible. To our knowledge, there are currently thirty-four papers (many unpublished) that provide estimates of the effect of currency union on bilateral trade, denoted here as γ. These studies are reported in the Appendix along with estimates of γ that seem to be most preferred or representative. While we have strong views about the quality of some of these estimates, each estimate is weighted equally; alternative weighting schemes might be regarded as suspect.

The central concern of meta-analysis is to test the null hypothesis that γ=0 when the findings from this entire area of research are combined. The classic test comes from Fisher (1932) and uses the p-values from each of the (34) underlying γ estimates. Under the null hypothesis of no effect, no publication selection and independence, the statistic, minus twice the sum of the logarithms of the p-values, is distributed approximately as a chi-square with 2n (=68) degrees of freedom. For the common currency research literature, this hypothesis is easily rejected at any standard significance level ($\chi^2_{(68)}$=1272).[2]

However, Fisher's test for overall effect is inappropriate for this, and perhaps all, areas of economic research. The problem is that the underlying assumptions for Fisher's test are quite strict and unlikely to be satisfied by empirical economics. Most problematic is its null hypothesis that the value being estimated by *all* studies is exactly zero. In economics, where studies use different data and estimation methods, some studies will inevitably be estimating a nonzero, biased magnitude even when there is no true effect. Unfortunately, it takes only one biased study to make the null hypothesis of the Fisher's test false, and for the calculated test statistic to become significant (Stanley and Jarrell, 1998). Other problems include the assumption of independence across studies, and the presumption of homogeneity. As reported below, the common currency literature exhibits clear signs of heterogeneity. "A finding of significance therefore does not mean that the average effect is statistically significant (and certainly not that it is somehow practically important), but only that there is some unexpected variation among the research findings" (Stanley and Jarrell 1998, p.952). Although consistent with a significant positive trade effect, Fisher's test should be given little weight. Other tests for overall effect are needed.

Table 1 presents combined meta-estimates of the currency effect on trade. Both 'fixed-effects' and 'random-effects' estimates are presented.[3] The former are based on the assumption that a single, 'true' effect underlies every study. Thus in principle, if every study were infinitely large, they would yield identical results. This is the same as assuming there is no heterogeneity across studies, which, as we show below, is easy rejected by the reported common currency results. By way of contrast, the random-effects estimator allows studies to have different treatment effects, but it is their mean that is of primary research interest.[4]

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

	Pooled Estimate of g	Lower Bound of 95% CI	Upper Bound of 95% CI
Fixed	.29	.27	.31
Random	.64	.51	.77
Fixed, without Rose	.22	.19	.24
Random, without Rose	.53	.40	.66

Manifestly, there is considerable heterogeneity. The fixed- and random-effects estimators differ greatly in magnitude, and their confidence intervals do not overlap. However, the magnitudes of all estimates are economically substantial. The smaller fixed-effects estimate of γ indicates that currency union raises trade by 33%, ln(.29)-1= .33, while the random-effects estimate indicates that this average effect is closer to 90%. Note that all confidence bounds exceed zero, which serves as a less presumptuous test for a positive trade effect.

None of these conclusions change if Rose's six studies are dropped, and there is little indication that any single study is especially influential in driving these results. Table 2 reports the fixed-effects estimates for γ when studies are omitted from the meta-analysis one by one. Again, all confidence bounds are positive.

While we tried to choose the preferred/representative estimates to match the intentions of the authors, we did choose them. An alternative way to proceed is to use a more objective statistical procedure to choose the underlying estimates of γ from each study. To insure the robustness of our findings, Table 3 reports fixed- and random-effects estimates based upon a study's median estimate of g and its 10^{th} percentile.[5] Here too, all of the pooled estimates and their confidence bounds are positive, confirming the positive trade effect of currency union. Of course, the pooled meta-estimate of γ falls as one moves away from the median estimate towards the lower percentiles within individual studies. However, it is also worth mentioning that the median estimates are *higher* than the preferred estimates that we selected. 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.

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

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

However, all of these more modest assessments of the common currency effect *(i.e.,* fixed-effects estimates) assume that there is a single, common mean, leaving only random sampling errors to explain the observed variation. Typically, this assumption of homogeneity is tested by: $Q = \Sigma(g_i - g_w)^2/v_i$, where g_i is the i^{th} estimate of γ, g_w is the weighted average, v_i is the variance of the i^{th} estimate of γ, and $1/v_i$ is the weight used for g_w. Under the null hypothesis of homogeneity, Q is distributed as a χ^2 with degrees of freedom equal to the number of studies minus one. Here, Q=778.8, which is significant at any level. As discussed below, heterogeneity is present not only across studies but also within most of the individual studies (Table 6). This excess variation needs to be explained or somehow accommodated. The larger, random-effects estimator reported above is one way to accommodate heterogeneity. Meta-regression analysis is another. The next section uses meta-regression analysis (MRA) to explain the excess variation among estimates of common currency effects and to address potential contamination from publication bias.

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

		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
10^{th}-Percentile	**Fixed**	.12	.10	.14	.00
10^{th}-Percentile	**Random**	.37	.24	.51	.00

4. Publication Selection and Meta-Regression Analysis

Estimates of common currency effects seem to overwhelmingly indicate a positive effect on trade. Nonetheless, it is possible that these strong findings may be the artifact of selection for statistical significance (i.e., publication bias). Publication selection occurs when researchers, referees, or editors have a preference for statistically significant results. Insignificant findings tend to be suppressed, left to languish in the researcher's 'file drawer.'[6] The problem with such selection is that it will tend to exaggerate the magnitude of the empirical effect in question, potentially making negligible effects appear important.

The common currency literature is so strong and one-sided that is unlikely to have been produced by publication selection alone. Nonetheless, the magnitude of the effect may be greatly inflated though publication selection. Thus, it is important to investigate publication selection and, if possible, to correct the estimate of gamma accordingly.

Funnel graphs are the conventional methods to identify publication selection. A funnel graph is a scatter diagram of precision (1/standard error) vs. estimated effect. In the absence of publication selection, the diagram should resemble an inverted funnel—wide at the bottom for small-sample studies, narrowing as it rises. Most importantly, the funnel graph should be also symmetric. Asymmetry is the mark of publication bias.

Figure 1: Funnel Graph of 34 Studies

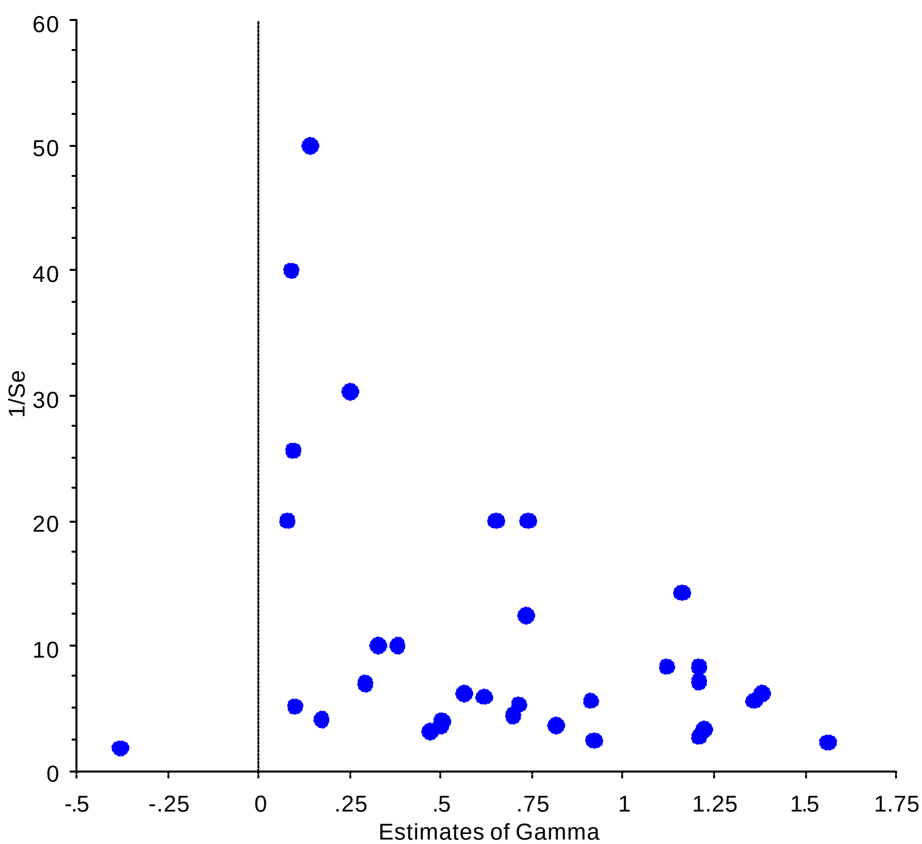
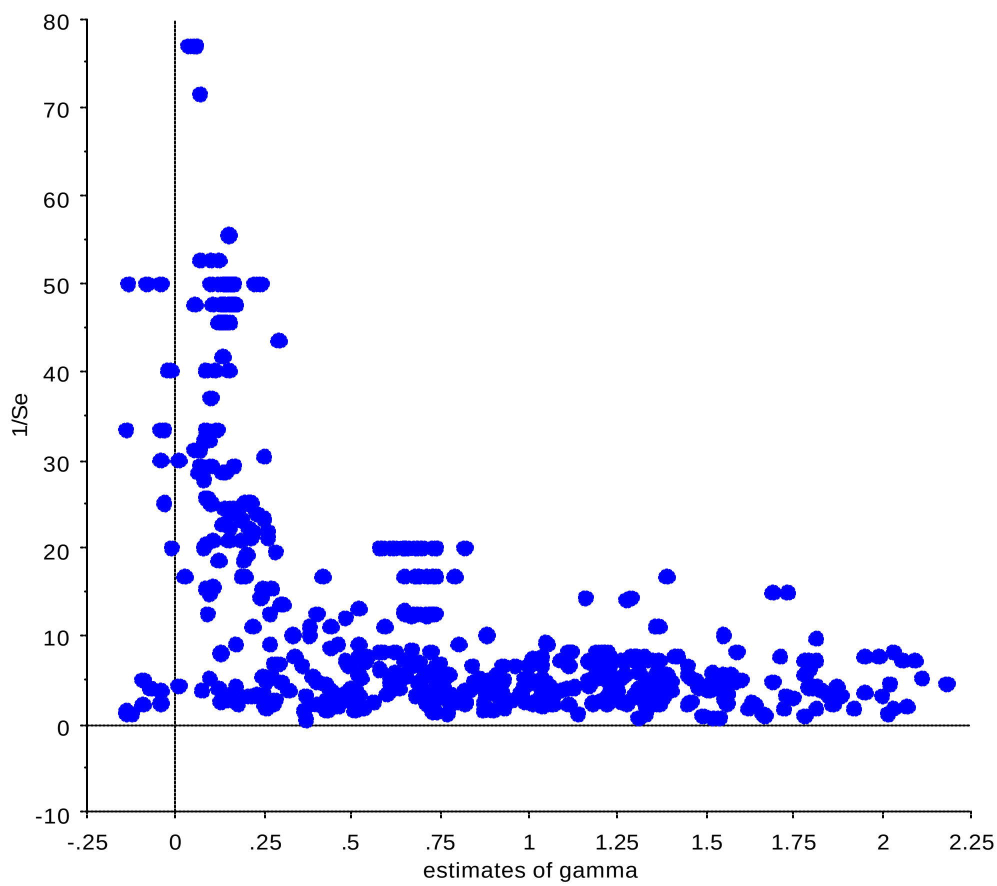


Figure 1 graphs each study's preferred estimate of gamma against precision. However, it does not resemble a funnel, but rather the right half of one. This asymmetry becomes even clearer when all estimates are depicted—Figure 2. Because a few very large outliers, both positive and negative, distort the scale drastically, the top and bottom 5% of the estimates are omitted from Figure 2. Clearly both diagrams lack symmetry. In contrast, see Stanley (2005) in this volume for an example of an apparently symmetric funnel graph. Furthermore, the peak of both graphs, which should roughly represent the 'true effect,' appears positive, though rather small.

Figure 2: Funnel Graph of 678 Individual Estimates



To corroborate this pictographic identification of publication bias, we use a meta-regression analysis (MRA) of the t-value vs. precision (Egger et al., 1997).

$$effect_i = b_1 + b_0 Se_i + e_i \qquad (2)$$

The reasoning behind this model of publication selection begins with the recognition that researchers will be forced to select larger effects when the standard error is also large. Large studies with smaller standard errors will not need to search as hard or as long for the required significant effect. Accounting for likely heteroskedasticity leads to the weighted least squares (WLS) version of equation (2),

$$t_i = b_0 + b_1(1/Se_i) + e_i \qquad (3)$$

In the absence of publication selection, b_0 will be zero. Without selection, the magnitude of the reported effect will be independent of its standard error. Table 4 column 1 estimates b_0 to be 3.85, which is significantly positive (t=5.36; p<.0001), confirming the apparent asymmetry of the funnel graphs.

As discussed in some detail elsewhere in this volume (Stanley 2005), precision's regression coefficient also serves as a test of genuine empirical effect beyond publication bias. Again, we have (moderate) corroboration of an authentically positive common currency effect (t=1.97; one-tail p<.05). As suggested by Monte Carlo simulations (Stanley 2004), it is prudent to confirm this positive precision-effect test with another MRA test for genuine effect. Column 2 of Table 4 reports the result of a meta-significance test (MST), which corroborates the existence of a genuine empirical effect (t=1.73; one-tail p<.05).[7]

Table 4: MRA Tests of Effect and Publication Bias

	Dependent Variables	
Moderator Variables:	*Column 1* *t*	*2 :* *ln|t|*
Intercept	3.85 (5.36)*	.152 (.23)
1/Se	.113 (1.97)	—
ln(n)	—	.123(1.73)
n	34	34
R^2	.093	.078
RMSE	4.02	.84

*t-values are reported in parenthesis and are calculated from heteroskedasticity-consistent standard errors.

Both of these MRA tests find evidence of an authentic effect, and the average effect is also statistically significant. Thus, the effect is better estimated by combining two biased estimates of the currency union effect. Monte Carlo simulations also show that the midpoint of precision's MRA coefficient and the simple average of observed effects greatly reduces bias and MSE when there is a genuine effect. Here, the corrected estimate of the common currency effect is .385, implying that trade is raised 47% by a common currency. These simulations also show that conventionally constructed confidence intervals are valid when a literature first passes this battery of tests for effect. Thus, a 95% confidence interval for gamma is (.184, .586); that is, trade is increased between twenty and eighty percent. Again, even after accommodating publication bias, an economically significant trade effect of monetary union is apparent.

With or without publication bias, fixed- or random-effects, there seems to be a strong trade effect from currency unions. Nonetheless, there remains much heterogeneity in this area of research, and Table 4 explains little of it. Hence, we turn next to multivariate MRAs.

Table 5 columns 1 and 2 report MRAs for both meta-significance and precision-effect models that result from an OLS 'general to specific' approach (Davidson et al. 1978). "The strength of general to specific modeling is that model construction proceeds from a very general model in a more structured, ordered (and statistically valid) fashion, and in this way avoids the worst of data mining" (Charemza and Deadman 1997, p. 78). We coded: *countries* (the number of countries included in the data used to estimate gamma), *Euro* (1 if the study concerned the EMU), *panel* (1 if panel data is used), *postwar* (1 if data is post-WWII), *Rose* (1 if Rose is the author or coauthor of the study), *shortrun* (1 if the study is short run in nature), and *years* (the number of years spanned by the data). After first adding all of these variables together with either *lnn* or *1/Se*, insignificant variables were removed, one at a time, to yield the results found in columns 1 and 2 of Table 5.

Note first the commonality of these multivariate MRAs. Both show signs of an authentic common currency effect (t={2.74 & 2.59}; one-tail p<.01), and both find that moderator variables: *Euro*, *countries* and *Rose* are statistically significant. Studies that focus on the EMU find marginally smaller common currency effects (t=-3.43; p<.01);

papers authored by Rose report larger effects (t=3.25; p<.01); and the larger the number of countries used, the smaller the estimated effect (t=-3.29; p<.01). The difference between these multivariate MRAs is that the precision-effect MRA also finds that the number of years spanned by the data has a positive effect on the estimated gamma (t=2.70; p<.05), while the meta-significance MRA reveals that panel studies report higher effects, *ceteris paribus* (t=2.49; p<.05). Regardless of which model of empirical effect and publication selection we choose, there is broad agreement about the existence of a genuine effect beyond publication bias and about which research characteristics explain much of the variation found across studies.

Table 5: Explanatory MRAs of Common Currency Effects

	Dependent Variables		
Moderator Variables:	*Column 1: t*	*2: ln|t|*	*3: t*
Intercept	2.07 (2.69)	-.749 (-1.15)	2.63 (3.84)
1/Se	.384 (2.59)	—	.718 (4.08)
ln(n)	—	.256 (2.74)	—
*Euro**	-.386 (-3.43)	-.645 (-2.05)	—
*countries**	-.00236 (-3.29)	-.00808(-3.43)	—
*Rose**	.474 (3.25)	.824 (2.81)	.516 (5.39)
*years**	.00929 (2.70)	—	—
panel	—	.806 (2.49)	—
*postwar**	—	—	-.636 (-3.71)
n	34	34	34
R^2	.677	.545	.604
RMSE	2.56	.631	2.75

*These moderator variables are further divided by Se in the WLS MRAs reported in columns 1 and 2.

To all appearances, these MRAs do a good job in explaining the large variation found in this research. In particular, the precision-effect model, column 1, explains two-thirds of the variation (i.e., R^2=67.7%) in reported common currency effects. Explaining 68% of an empirical literature is quite good compared to the typical economic MRA.

However, this coefficient of determination is an incorrect reflection of this MRAs ability to explain variation in effect because column 1 uses WLS with t-values as the dependent variable. After using these WLS estimates to predict common currency effects and comparing them to the observed effects, the resulting adjusted R^2 becomes somewhat lower, 51.7%.

Worse still, there remains excess unexplained variation. Heterogeneity's Q may also be calculated by the residuals sum of squares of the MRA of precision vs. t-values, Q={184.1 & 226.2; p<.0001), (Higgins and Thompson 2002, p. 1547). Thus, there is something missing in this explanation of the common currency literature, and unfortunately what you don't know *can* hurt you (*e.g.*, omitted-variable bias).

Another problem for the precision-effect MRA is that we can find evidence that the model reported in column 1 is mis-specified. After running a battery of misspecification tests, we find a significant value for Ramsey's generic specification RESET test ($F_{(2,26)}$=9.85; p<.001). To address this specification problem, column 3 of Table 5 reports a multivariate precision-effect MRA that passes a battery of specification tests: Breusch-Godfrey LM test for serial correlation $\chi^2_{(3)}$=4.27 (p>.05), White's test of heteroskedasticity $\chi^2_{(23)}$=5.86 (p>.05), and RESET $F_{(2,26)}$=1.36 (p>05). This acceptable precision-effect MRA, column 3, replaces the moderator variables: *Euro, countries* and *years* with *postwar*. Studies using postwar data find smaller trade effects (t=-3.71; p<.01). It should also be noted that the meta-significance MRA, column 2, passes all these misspecification tests: Breusch-Godfrey LM test for serial correlation $\chi^2_{(3)}$=2.68 (p>.05), White's test of heteroskedasticity $\chi^2_{(23)}$=7.05 (p>.05), and RESET $F_{(2,28)}$=.71 (p>05).

If we were to use the multivariate MRA that passes this battery of specification tests (column 3 Table 5) to estimate the common currency effect, we get .082 (or 8.5%) for the postwar period and .718 (or 105%) of the earlier period. To correct for likely bias, these estimates should be combined with the simple observed average. Doing so lowers our previously reported estimate, .385, by .019 postwar and greatly increases it for the earlier period to .721 (or 106%).

To summarize, this meta-analysis has several strong findings and one weak one. First, the hypothesis that there is no trade effect from currency union is robustly rejected when individual studies are pooled. Second, the pooled effect is not only positive but economically significant, whether or not it is adjusted for publication bias. Third, there is evidence of publication bias and of a genuine positive trade effect, beyond publication selection. Fourth, as expected, a number of research characteristics are found to have a significant effect on the reported common currency effect. In particular, studies based upon a greater number of countries tend to report smaller effects, and those that concern the EMU also contain smaller effects, while studies authored or coauthored by Rose tend to find larger common currency effects. The only unenthusiastic finding is that there remains excess unexplained variation in our meta-regression models of common currency effects. Thus, the full story of this area of research has yet to be told.

5. Different Estimates of γ and its Significance within Individual Studies

The thirty-four studies contain many estimates of γ, 754 in all. Simply averaging across the 754 estimates produces a mean of .86 and an average t-ratio of 5.3.[8] The median estimate is .53, implying a 70% increase in trade, and fifty percent of the estimates have a t-value of 4.22 or larger.

The vast majority (92%) of the point estimates of γ is positive, and many are economically large. 325 (43%) exceed .69 in magnitude, a number that implies that currency union doubles trade. 218 (29%) find that currency union triples bilateral trade; 411 (55%) exceed .40, implying that trade increases by 50%.

Finally, one can also combine the different estimates that exist within the thirty-four studies, on a paper-by-paper basis. Table 6 reports both fixed- and random-effects estimates of γ for each of the thirty-four studies along with the p-values associated with the test of Ho: $\gamma=0$. Thirty-one of these studies contain significantly positive trade effects when all of their reported estimates are combined, while three studies' overall effect is not statistically different than zero. Even within the individual studies there is a great deal of heterogeneity, see the last column of Table 6. Studies which show heterogeneity cannot be summarized by the fixed-effects estimate. Again, this unexplained heterogeneity constitutes the only limitation to the positive findings about positive trade effects.

Table 6: Within-Study Meta-Analysis of γ

Study		Coefficients	Ho: g=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		
	Fixed	1.007	0.000	17	0.00
Smith	Random	1.118	0.000		
	Fixed	0.205	0.000	6	0.00
Bomberger	Random	0.315	0.006		
	Fixed	1.312	0.000	13	0.99
Melitz	Random	1.312	0.000		
	Fixed	1.162	0.000	16	0.00
Saiki	Random	0.520	0.008		
	Fixed	0.098	0.000	54	0.00
Micco, Stein, Ordonez	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

6. Conclusion

In spite of its youth, there now exists a rich empirical literature on the trade consequences of currency unions. A meta-analysis confirms a robust, positive effect on trade, which remains statistically significant and economically important even after filtering out likely publication selection. Although the combined estimates vary from roughly 30% to 90%, depending on the exact meta-methods used, they all imply a substantial rise in trade. In particular, the 'random-effects' estimate entail an increase of 90% in bilateral trade, or between 41% and 116% with 95% confidence. The more modest (but still large), 'fixed-effects' estimates (33%) cannot be trusted because its basis is undermined by obvious heterogeneity in this research literature.

There is also strong statistical evidence of publication selection ($t=5.36$; $p<.05$), favoring the reporting of significantly positive trade effects. Such publication bias causes all simple combined estimates of trade effects, whether fixed- or random-effects, to be exaggerated. Correcting for publication bias reduces the trade effect of currency union to 47%, with a 20% to 80% confidence interval at the 95% level. After correcting for likely publication bias, the magnitude of the trade effect remains economically important, and is more plausible than the doubling or tripling of trade often reported.

Meta-analysis does have an important limitation that should be mentioned. If there is a common, systematic bias across the entire literature, meta-analysis has no way to distinguish it from an authentic empirical effect. If, for example, studies tend to be biased in a particular direction due to a common mis-specification, the meta-analysis estimates include the average of this systematic bias.

A number of research characteristics help to explain the wide variation in reported estimates. In particular, the EMU exhibits a significant smaller trade effect as do postwar currency unions. Although more than half of the variation in reported estimates can be explained by obvious research characteristics, excess variation remains. This excess variation brings another cautionary note to our otherwise clear statistical results. With these limitations mind, the findings of this meta-analysis of the trade effects of currency union are strong, if provisional.

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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

Endnotes

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[1] See Nitsch (2002c). This also seems to be true of Ritschl and Wolf (2003).

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

[3] 'Fixed' and 'random-' effects models refer to differing assumptions about the heterogeneity of 'true' effects, not differing assumption about the variation across time and region in panel studies as these terms are used in the econometric literature.

[4] 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 (Sutton et al., 2000).

[5] If there is an even number of estimates in the underlying study, we choose the higher estimate. 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] The 'file drawer problem' is another name for publication selection and its bias.

[7] *ln(n)* is the logarithm of the sample size. Meta-significance tests are inherently one-tailed—H_0: $\alpha_1<0$.

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