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.

Keywords. Currency; Euro; Meta-regression analysis; Monetary; Publication bias; Union

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.

The purpose of this review was 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

*The data sets, key output, and past version of the paper are available at Rose's website.

variations in underlying assumptions, identifying and filtering out publication bias, and by explaining variations among reported estimates through meta-regression analysis (MRA). 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). His paper exploits a panel of cross-country data covering bilateral trade between 186 different trading partners at 5-year intervals between 1970 and 1990. Since most of the variation is across pairs of countries rather than time, Rose (2000) uses a conventional 'gravity' model of trade to account for factors that drive trade (other than monetary arrangements). This resulting equation has now become the standard vehicle for assessing trade effects.

$$T_{ijt} = \beta_1 D_{ij} + \beta_2 (Y_i Y_j)_t + \sum_k \beta_k Z_{ijt} + \sum_t \delta_t T_t + \gamma \mathrm{CU}_{ijt} + u_{ijt}, \qquad (1)$$

where T_{ijt} denotes the natural logarithm of trade between countries i and j at time t, β a set of nuisance coefficients, D_{ij} the log of distance between i and j, Y the log of real GDP, Z other controls for bilateral trade, CU_{ijt} a dummy variable that is 1 if countries i and j are in a currency union at t, 0 otherwise, and u 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 are 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; Pakko and Wall, 2001; Persson, 2001). Nonetheless, Glick and Rose (2002) exploit 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 (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 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. These more recent data have 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), Flam and Nordström, and 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 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, b) 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 (see also Klein, 2002). Saiki (2002) disaggregates 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-World War II (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 *et al.* (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 (Flandreau and Maurel, 2001; Tenreyro, 2001; Bomberger, 2002; Smith, 2002; Alesina *et al.*, 2003; López-Córdova and Meissner, 2003).[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 (Flandreau and Maurel, 2001; Frankel and Rose, 2002). 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 0, link the estimates to features of the underlying studies, and to estimate the coefficient of interest more accurately. 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 (for a recent review, see Stanley (2001) and also the companion papers in this Special Issue).

One begins meta-analysis by collecting as many estimates of a common effect as possible. To our knowledge, there are currently 34 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 $\gamma = 0$ 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 $\left(\chi^2_{(68)} = 1272 \right)$.

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 0. In economics, where studies use different data and estimation methods, some studies will inevitably be estimating a non-zero, 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.[2] 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 easily 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.[3]

Manifestly, there is considerable heterogeneity. The fixed- and random-effects estimators differ greatly in magnitude, and their confidence intervals do not overlap. The magnitudes of all estimates are economically substantial. The smaller fixed-effects estimate of γ indicates that currency union raises trade by 33% $[\ln(0.29) - 1 = 0.33]$, while the random-effects estimate indicates that this average effect is closer to 90%. However, both fixed- and random-effects estimates are considerably smaller than the simple mean of all 754 estimates, 0.86, implying a 136% increase in trade. Note that all confidence bounds exceed zero, which serves as a less presumptuous test for a positive trade effect.

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

	Pooled estimate of γ	Lower bound of 95% CI	Upper bound of 95% CI
Fixed	0.29	0.27	0.31
Random	0.64	0.51	0.77
Fixed, without Rose	0.22	0.19	0.24
Random, without Rose	0.53	0.40	0.66

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 on a study's median estimate of γ and its 10th percentile.[4] Here too, all of the pooled estimates and their confidence bounds are positive, confirming the positive trade effect of currency union. Of course, the

Table 2. Sensitivity of Meta-Analysis of γ to Individual Studies (Fixed Effects).

Study omitted	Coefficient	95% CI, lower	95% CI, upper
Rose	0.28	0.26	0.30
Rose and Engel	0.29	0.26	0.31
Frankel and Rose	0.28	0.26	0.30
Rose and van Wincoop	0.28	0.26	0.31
Glick and Rose	0.27	0.25	0.29
Persson	0.29	0.26	0.31
Rose	0.26	0.24	0.29
Honohan	0.29	0.26	0.31
Nitsch	0.29	0.26	0.31
Pakko and Wall	0.29	0.27	0.31
Thom and Walsh	0.29	0.27	0.31
Melitz	0.29	0.26	0.31
Lopez-Cordova and Meissner	0.29	0.26	0.31
Tenreyro	0.29	0.26	0.31
Levy Yeyati	0.29	0.26	0.31
Nitsch	0.29	0.26	0.31
Flandreau and Maurel	0.26	0.24	0.29
Klein	0.29	0.26	0.31
Estevadeoral *et al.*	0.29	0.27	0.31
Alesina *et al.*	0.29	0.26	0.31
Smith	0.29	0.26	0.31
Bomberger	0.30	0.28	0.32
Melitz	0.28	0.26	0.30
Saiki	0.29	0.26	0.31
Micco *et al.*	0.34	0.31	0.36
Kenen	0.29	0.26	0.31
Bun and Klaassen	0.29	0.26	0.31
De Souza	0.29	0.27	0.31
De Sousa and Lochard	0.28	0.26	0.30
Flam and Nordström	0.35	0.33	0.38
Barr *et al.*	0.29	0.27	0.32
De Nardis and Vicarelli	0.30	0.28	0.33
Rose	0.28	0.26	0.30
Subramanian and Wei	0.28	0.26	0.30

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 H_0: no effect
Preferred				
Fixed	0.27	0.25	0.29	0.00
Random	0.64	0.51	0.76	0.00
Median				
Fixed	0.34	0.31	0.38	0.00
Random	0.82	0.62	1.01	0.00
10th percentile				
Fixed	0.12	0.10	0.14	0.00
Random	0.37	0.24	0.51	0.00

pooled meta-estimate of γ falls as one moves away from the median estimate toward the lower percentiles within individual studies, which make the 10th percentile a very conservative estimate. It is also worth mentioning that the median estimates are higher than the preferred estimates that we selected, causing these reported estimates (Table 1) to be conservative. Further, all the effects are economically substantive. The lower bound for the lowest estimate is 0.10, implying an effect of currency union on trade of over 10%.

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 ith estimate of γ, g_w the weighted average, v_i the variance of the ith estimate of γ, and v_i 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. MRA is another. The next section uses MRA to explain the excess variation among estimates of common currency effects and to address potential contamination from publication bias.

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'.[5] 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 it is unlikely to have been produced by publication selection alone. Nonetheless, the magnitude of the effect may be greatly inflated through publication selection. Thus, it is important to investigate publication selection and, if possible, to correct the estimate of γ accordingly.

Funnel graphs are the conventional methods to identify publication selection. A funnel graph is a scatter diagram of precision [1/standard error (SE)] versus 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 also be symmetric. Asymmetry is the mark of publication bias.

Figure 1 graphs each study's preferred estimate of γ 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, drawn from Doucouliagos and Laroche (2003), of an
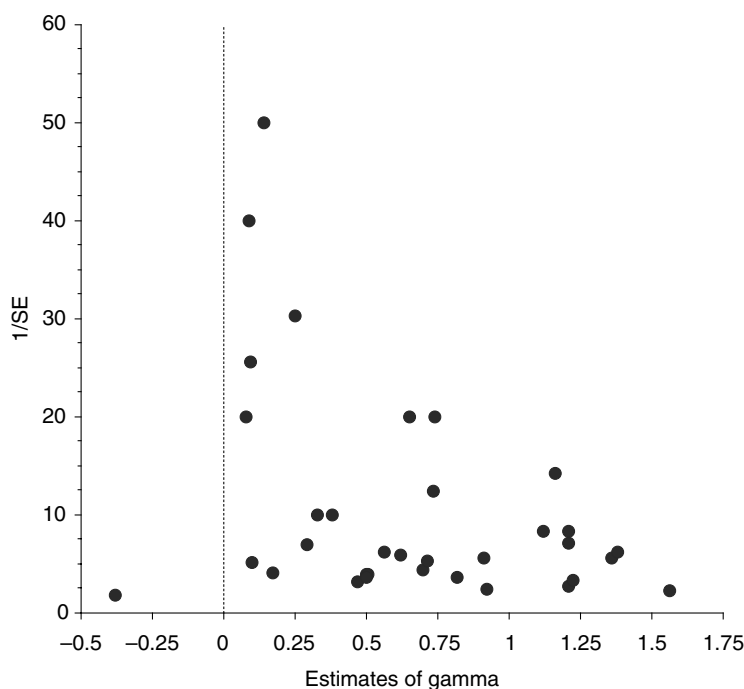

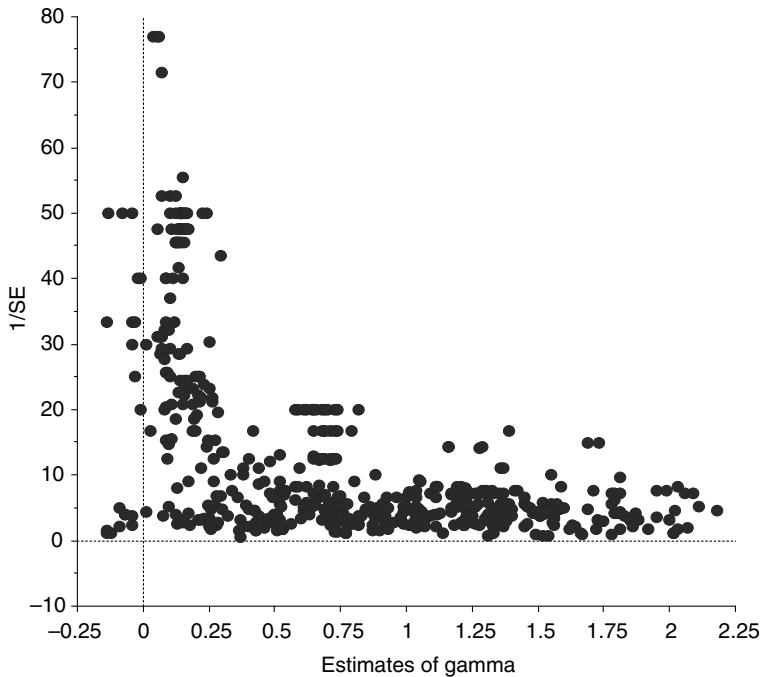**Figure 1.** Funnel graph of 34 studies.

Figure 2. Funnel graph of 678 individual estimates.

apparently symmetric funnel graph. Furthermore, the peak of both graphs, which should roughly represent the 'true effect', appears positive, though rather small.

To corroborate this pictographic identification of publication bias, we use an MRA of the t-value versus precision (Egger *et al.*, 1997). First, publication bias is typically modelled as given below (Card and Krueger, 1995; Ashenfelter *et al.*, 1999; Görg and Strobl, 2001):

$$\text{Effect}_i = \beta_1 + \beta_0 \text{SE}_i + \varepsilon_i. \tag{2}$$

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),

$$t_i = \beta_0 + \beta_1 (1/\text{SE}_i) + e_i. \tag{3}$$

In the absence of publication selection, β_0 will be zero. Without selection, the magnitude of the reported effect will be independent of its standard error. Column 1 of Table 4 estimates β_0 to be 3.85, which is significantly positive ($t = 5.36$; $p < 0.0001$), confirming the apparent asymmetry of the funnel graphs.

Table 4. Meta-Regression Analysis Tests of Effect and Publication Bias.

	Dependent variables	
Moderator variables	Column 1 t	Column 2 $\ln\lvert t \rvert$
Intercept	3.85 (5.36)*	0.152 (0.23)
1/SE	0.113 (1.97)	–
$\ln(n)$	–	0.123 (1.73)
n	34	34
R^2	0.093	0.078
RMSE	4.02	0.84

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

As discussed in 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 $p < 0.05$). 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. Stanley (2001) proposes using the relationship between a study's standardized effect (e.g. its t-value) and its sample size (or degrees of freedom) to identify authentic empirical effect from the artifacts of publication bias. Classical statistics implies that the magnitude of the standardized test statistics varies directly with degrees of freedom only if there is in fact an overall genuine empirical effect. Such statistical power traces form an MRA test (i.e. meta-significance testing, MST) and evaluate whether an area of economic research contains a genuine empirical effect (Stanley, 2005). Column 2 of Table 4 reports the result of an MST, which corroborates the existence of a genuine empirical effect ($t = 1.73$; one-tail $p < 0.05$).[6]

Both of these MRA tests find evidence of an authentic effect, and the average effect is also statistically significant. In such cases, 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 reduce bias and (mean squared error) MSE when there is a genuine effect (Stanley, 2004). Here, the corrected estimate of the common currency effect is 0.385; that is, currency unions raise trade by 47%. This corrected estimate falls comfortably between the previously reported fixed- and random-effects estimates (Table 1), yet outside both of their confidence bounds. The fixed-effects estimate is known to be invalid in this application, due to the clear heterogeneity among currency union research results. Compared to the random-effects estimate (90%), this corrected estimate (47%) is just over half and roughly one-third of the unadjusted, simple average (136%). Correcting or accommodating publication bias consistently lowers the estimated trade effect.

Monte Carlo simulations also show that conventionally constructed confidence intervals around this corrected estimate are valid when a literature first passes the

above battery of tests (Stanley, 2004). A 95% confidence interval for γ, after correcting for publication bias, is 0.184–0.586; that is, trade is increased between 20 and 80%. Note how the confidence intervals for different estimates do not overlap (an exception occurs between the fixed-effects and the corrected estimates). Again, after accommodating publication bias, an economically significant trade effect of monetary union remains.

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 MRA.

Columns 1 and 2 of Table 5 report MRA 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). After adding all of the variables given in Table 5 together with either $\ln(n)$ or $1/SE$, insignificant variables were removed, one at a time, to yield the results found in columns 1 and 2.

Note the commonality of these multivariate MRA. Both show signs of an authentic common currency effect ($t = 2.74$ and 2.59; one-tail $p < 0.01$), 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 ($t = -3.43$; $p < 0.01$); papers authored by Rose report larger

Table 5. Explanatory Meta-Regression Analysis (MRA) of Common Currency Effects.

Moderator variables	Dependent variables		
	Column 1 t	Column 2 $\ln\|t\|$	Column 3 t
Intercept	2.07 (2.69)	−0.749 (−1.15)	2.63 (3.84)
1/SE	0.384 (2.59)	–	0.718 (4.08)
ln(n)	–	0.256 (2.74)	–
Euro*	−0.386 (−3.43)	−0.645 (−2.05)	–
Countries*	−0.00236 (−3.29)	−0.00808 (−3.43)	–
Rose*	0.474 (3.25)	0.824 (2.81)	0.516 (5.39)
Years*	0.00929 (2.70)	–	–
Panel	–	0.806 (2.49)	–
Postwar*	–	–	−0.636 (−3.71)
n	34	34	34
R^2	0.677	0.545	0.604
RMSE	2.56	0.631	2.75

*These moderator variables are further divided by SE in the WLS MRAs reported in columns 1 and 3.
Notes: Countries – the number of countries included in the data used to estimate γ; Euro – 1 if the study concerned the EMU; Panel – 1 if the panel data are used; Postwar – 1 if the data are post-WWII; Rose – 1 if Rose is the author or coauthor of the study.

effects ($t = 3.25$; $p < 0.01$); and the larger the number of countries used, the smaller the estimated effect ($t = -3.29$; $p < 0.01$). The difference between these multivariate MRA is that the precision-effect MRA also finds that the number of years spanned by the data has a positive effect on the estimated γ ($t = 2.70$; $p < 0.05$), while the meta-significance MRA reveals that panel studies report higher effects, *ceteris paribus* ($t = 2.49$; $p < 0.05$). 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.

To all appearances, these MRA 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 MRA 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 versus *t*-values ($Q = 184.1$ and 226.2, respectively; $p < 0.0001$) (Higgins and Thompson, 2002, p. 1547). Thus, there is something missing in this explanation of the common currency literature, and unfortunately what you do not 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 misspecified. After running a battery of misspecification tests, we find a significant value for Ramsey's generic specification RESET test [$F_{(2,26)} = 9.85$; $p < 0.001$]. To address this specification problem, column 3 of Table 5 reports a multivariate precision-effect MRA that passes the same battery of specification tests: Breusch – Godfrey LM test for serial correlation $\chi^2_{(3)} = 4.27$ ($p > 0.05$), White's test of heteroskedasticity $\chi^2_{(23)} = 5.86$ ($p > 0.05$), and RESET $F_{(2,26)} = 1.36$ ($p > 0.05$). 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 ($t = -3.71$; $p < 0.01$). It should also be noted that the meta-significance MRA, column 2, passes all these misspecification tests: Breusch – Godfrey LM test for serial correlation $\chi^2_{(3)} = 2.68$ ($p > 0.05$), White's test of heteroskedasticity $\chi^2_{(23)} = 7.05$ ($p > 0.05$), and RESET $F_{(2,28)} = 0.71$ ($p > 0.05$).

If we were to use the multivariate MRA that passes this battery of specification tests (column 3 of Table 5) to estimate the common currency effect, we get 0.082 (or 8.5%) for the postwar period and 0.718 (or 105%) for 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, 0.385, by 0.019 postwar and greatly increases it for the earlier period to 0.721 (or 106%).

To summarize, this meta-analysis has several strong findings and 1 weak. First, the hypothesis that there is no trade effect from currency union is robustly rejected when individual studies are pooled. Steps, which progressively accommodate publication bias, successively lower the trade effect – from 136% (simple mean) to 90% (random-effects) to 47% (corrected estimate). 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 on 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.[7]

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

The 34 studies contain many estimates of γ, 754 in all. Simply averaging across the 754 estimates produces a mean of 0.86 and an average t-ratio of 5.3.[8] The median estimate is 0.53, implying a 70% increase in trade, and 50% of the estimates have a t-value of 4.22 or larger.

The vast majority (92%) of the point estimates of γ are positive, and many are economically large. 325 (43%) exceed 0.69 in magnitude, a number that implies that currency union doubles trade. 218 (29%) find that currency union triples bilateral trade; 411 (55%) exceed 0.40, implying that trade increases by 50%.

Finally, one can also combine the different estimates that exist within the 34 studies, on a paper-by-paper basis. Table 6 reports both fixed- and random-effects estimates of γ for each of the 34 studies along with the p-values associated with the test of H_0: $\gamma = 0$. 31 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.

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

Table 6. Within-Study Meta-Analysis of γ.

Study	Coefficients	H_0: $\gamma = 0$ (p-value)	Number of estimates	Heterogeneity (p-value)
Rose				
Fixed	1.289	0.000	52	0.00
Random	1.311	0.000		
Rose and Engel				
Fixed	1.350	0.000	5	0.78
Random	1.350	0.000		
Frankel and Rose				
Fixed	1.631	0.000	5	0.02
Random	1.634	0.000		
Rose and van Wincoop				
Fixed	0.230	0.000	18	0.00
Random	0.649	0.000		
Glick and 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 and Wall				
Fixed	0.874	0.000	6	0.00
Random	0.332	0.350		
Thom and Walsh				
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 *et al.*				
Fixed	0.433	0.000	18	0.01
Random	0.450	0.000		
Alesina *et al.*				
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 *et al.*				
Fixed	0.098	0.000	54	0.00
Random	0.130	0.000		
Kenen				
Fixed	1.081	0.000	10	0.01
Random	0.988	0.000		
Bun and Klaassen				
Fixed	0.330	0.000	1	n/a
Random	0.330	0.001		
De Souza				
Fixed	−0.143	0.000	30	0.00
Random	−0.018	0.714		
De Sousa and Lochard				
Fixed	1.706	0.000	14	0.00
Random	1.698	0.000		
Flam and Nordström				
Fixed	0.150	0.000	49	0.00
Random	0.149	0.000		
Barr *et al.*				
Fixed	0.234	0.000	2	0.44
Random	0.234	0.000		
De Nardis and Vicarelli				
Fixed	0.090	0.000	2	0.90
Random	0.090	0.001		
Rose				
Fixed	0.905	0.000	10	0.00
Random	0.988	0.000		
Subramanian and Wei				
Fixed	1.142	0.000	11	1.0
Random	1.142	0.000		

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 cannot be trusted because its basis is undermined by obvious heterogeneity in this research literature.

There is also strong statistical evidence of publication selection ($t = 5.36$; $p < 0.05$), 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 20–80% 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 misspecification, 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 significantly 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 in mind, the findings of this meta-analysis of the trade effects of currency union are strong, if only provisional.

Acknowledgements

Notes

1. See Nitsch (2002c). This also seems to be true of Ritschl and Wolf (2003).
2. '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.
3. To elaborate: the fixed-effect assumption is that differences across studies are only due to within- 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).
4. 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.
5. The 'file drawer problem' is another name for publication selection and its bias.

6. $ln(n)$ is the logarithm of the sample size. Meta-significance tests are inherently one-tailed – H_0: $\alpha_1 < 0$.

7. See Abreu *et al.* (2005) for the 'mixed' MRA approach to this type of heterogeneity.

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.

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Appendix. Estimates of the effect of currency union on trade.

Author	γ	s.e. of γ
Rose (2000)	1.21	0.14
Rose and Engel (2002)	1.21	0.37
Frankel and Rose (2002)	1.36	0.18
Rose-van Wincoop (2001)	0.91	0.18
Glick and 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 *et al.* (2003)	0.293	0.145
Alesina *et al.* (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 *et al.* (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 *et al.* (2003)	0.25	0.033
De Nardis and Vicarelli (2003)	0.061	0.027
Rose (2004)	1.12	0.12
Subramanian and Wei (2003)	0.732	0.08