Session I:
Trade integration

Discussant:

Jeffrey A. Frankel
Harvard University

Comments on Richard Baldwin's, "The Euro's Trade Effects,"
for
What effects is EMU having on the euro area and its member countries?
European Central Bank, Frankfurt, 16 June, 2005.

Jeffrey A. Frankel, Harpel Professor
Kennedy School of Government, Harvard University

Richard Baldwin's survey of the evidence on how the volume of trade is affected by currency unions in general and the euro in particular is excellent and extensive. As he himself might say, it is not only exhaustive but even exhausting.   Those of us who have written short reviews of this subject pale by comparison.[1]   I can communicate to him in private a few places where he might consider a little cutting.   But I take it that a comprehensive and detailed survey of the literature is what was desired, and Richard is to be congratulated for delivering it – especially as the prose is very clear and very lively, and he does not hesitate to let us know his evaluation is of each contribution.

Baldwin correctly places Andrew Rose's research at the center of this survey. Indeed it could be titled The Rose Effect and the Reaction to It.   This is appropriate. I consider Rose's 2000 *Economic Policy* paper, "One Money, One Market…"to be the most influential international economics paper of the last ten years.

Baldwin goes into details of the intellectual history of this new sub-field.   He remarks that Rose's "original contribution was to add a common currency dummy to the list [of variables in the gravity model of bilateral trade] – hard to imagine that no one had thought of it before 2000" (2.2.3).   I think I can answer the question "why did nobody think of it before?"   Some of us had earlier added bilateral exchange rate variability to the gravity model, to see whether a reduction in variability encouraged trade between pairs of countries: Thursby and Thursby (1987), DeGrauwe (1988), Brada and Mendez (1988), and Frankel and Wei (1993, 1994, 1995, 1997).   The first data sets were limited to European or OECD countries, but they were progressively broadened. Mine generally used a set of 63 countries.   A big advantage of these gravity models, whether used to evaluate FTAs, or exchange rate regimes, or other issues, is that a large amount of data could be applied: 63x62=3906 observations of bilateral exports per year (or half as many, in the case – which Richard abhors -- where bilateral exports and imports were aggregated together).   We found some evidence that exchange rate variability had a small negative effect on trade.   The evidence was limited, and tended to diminish in the 1980s. But it was stronger than had been found in the traditional time series studies of

---

[1] E.g., Begg, et al (2003), Frankel (2003, 2004).

non-bilateral trade by industrialized countries. [2]   I have always said that having the much larger data set, and the wider variety of countries, is what made the difference.

This fits in with a larger long-held belief of mine that lots of the important questions in macroeconomics and international economics can only be satisfactorily answered with large data sets – either long historical time series, or wide cross-sections, or panel studies that combine the two.   Many of our colleagues have wasted a lot of time applying ever-more refined techniques to the same 30 annual observations that are insufficient to yield sharp estimates or a powerful test no matter how sophisticated the techniques.   (Tests of PPP are one good example.[3])

I haven't yet answered why none of us thought to add to our gravity equation a dummy variable for a currency union.   That question is easy to answer: there were few currency unions, if any, in the data set.   Why then did we not extend the data set to include all the smaller countries and dependencies, as Rose did in 2000, to include enough cases of currency unions?   Two answers.   First were the technical limitations.   The source was the Direction of Trade Statistics books of the IMF.   I don't think comprehensive bilateral trade data was available for most of these smaller countries and regions.   Furthermore this is another case where it is easy, today, to forget how different computer technology was as recently as the early 1990s.   Often I had to construct the data set by having a research assistant type in all 3906 observations by hand.   And that was just one year of observations on one variable, bilateral trade.   To do 200x199 (= 39,800) observations would not then have been practical.

The other reason is that there was not much interest until the last ten years in the specific question of currency unions, as distinct from fixed exchange rates more generally.   Most writings did not even make the distinction.   Rather, the battle line was drawn between fixed and floating. A majority of economists were skeptical that exchange rate variability had a substantial effect on trade. (One of their major arguments was that importers and exporters could hedge on the foreign market, which neglected that there was a price for hedging.[4]   Another was that exchange rate variability under floating was merely the symptom of fundamental causes that if suppressed under a regime of fixed rates would just show up somewhere else – specifically in nominal prices.)   I don't think anybody –even those of who took the effect of exchange rate risk and transactions costs more seriously -- was thinking that a currency union would have a substantially different effect than reducing exchange rate variability to zero through a conventional peg.

The 1990s brought far more interest in what are now called "hard pegs," for a variety of reasons: the realization that conventional pegs were all changed sooner or later [e.g., the devaluation of the CFA Franc countries against the franc in 1994], the

---

[2] Surveys of this earlier time series literature were included in Edison and Melvin (1990) and Goldstein (1995).
[3] E.g., Frankel (1990).
[4]   The problem with the theoretical argument is that forward and futures markets (1) do not exist for most countries and for most longer-term horizons, (2) come with transactions costs when they do exist, and (3) come also with risk premiums, which can drive a wedge between the forward rate and the expected future spot rate

resurrection of currency boards, especially Argentina in 1991, the much discussed option of dollarization (e.g., Ecuador in 2000) and most of all, of course, the achievement of the common currency in euroland in 1999.   While monetary theorists continued to focus on the touted dynamic consistency of hard pegs as the key difference, rather than anything to do with effects on trade, the door was nevertheless open to consider hard pegs as a separate valid topic.

Andy Rose deserves every bit as much credit as Baldwin gives him, for adopting the larger data set, and asking the currency union question, which was made possible by the inclusion of plenty of currency union members, in his 2000 paper.   Even if he had not included the currency union dummy, this paper would still have been important, because he had bilateral exchange rate variability in there, and it was highly significant statistically [which I, again, attribute to the much larger set of bilateral observations]. But the attention-grabber, of course, was that the currency union dummy had a far larger, and highly significant effect – the famous tripling estimate -- above and beyond the effect of bilateral variability per se.

I have relatively little to say about the possible microeconomic explanation for such a finding, which Baldwin discusses at the end.   I like the story about fragmentationi of stages of production in Baldwin-Taglioni (2004).   I do have a few thoughts of my own, as to why a common currency could have a discretely larger effect on trade than simply reducing exchange rate volatility to zero.   My understanding is that when the EU went beyond an FTA with zero tariffs internally, to a full customs union in which the trucks no longer had to stop at the border and fill out forms, that this provided a big boost to trade. I think there is a big boost to simplicity and convenience from abolishing the distinction between currencies or customs areas completely [which goes beyond reducing the measured price costs to zero], and from an institutional commitment that the new regime is permanent.   Or consider the even closer analogy of the fundamental reason for the existence of money.   A money economy is more efficient than a barter economy, because of convenience and transactions costs.   It would be very inefficient if each individual had his or her own money ("IOUs"), and had to evaluate those of others every time they wanted to do business.   The same is true across countries, qualitatively, across countries.

Andy also deserves a lot of credit for two more things, two of his usual habits in all his work.   First, he tried out many extensions and robustness checks – often anticipating ex ante the corresponding critiques from discussants or others [perhaps to a greater extent than Baldwin allows], rather than merely reacting ex post to their trenchant insights.   Second, he posted the data and regressions on his website, making it easy for others to replicate his results and try variations.   This completed the process whereby a gravity regression that used to take a year of hard data work, from beginning to end, can now be done in a matter of seconds.

Rose's remarkable tripling estimate has by now been replicated in various forms many times. But no sooner had he written his paper than the brigade to "shrink the Rose effect" (Baldwin's phrase) – or to make it disappear altogether -- descended en masse.

These papers often read to me as "guilty until proven innocent." Until I got toward the end of Baldwin's paper, I had suspected that he might be a member of this group. I am pleased to see that his bottom line is that there is a Rose effect, but that it is probably substantially smaller than a tripling. That is fine with me. If Rose had come up with a 30% effect on trade from the beginning, everyone would have considered that large and important.

But I intend to offer a stronger defense of the Rose findings [stronger not only than Baldwin's bottom line, but perhaps stronger than Rose himself claims]. If one got the impression from Baldwin's paper that Rose has tended to slant the case in favor of large and significant estimates and to downplay qualifications, I think this would be incorrect. To the contrary, he has sometimes acted embarrassed and apologetic about the magnitude of his estimates, and when possible has chosen to emphasize the smaller ones.

It is entirely understandable that a threefold effect would be greeted with a lot of skepticism, as this is a very large number. There are four grounds for skepticism, as I classify them. First, one cannot infer from cross-section evidence what would be the effect in real time of countries adopting a common currency. Second, the statistical association between currency links and trade links might not be the result of causation running from currencies to trade but might arise instead because both sorts of links are caused by a third factor, such as colonial history, remaining political links, complementarity of endowments, or accidents of history. I would also include here many of the other allegations of missing variables, misspecification of the equation, or endogeneity of the currency decision. Third, the estimated effect on trade [and on income, in subsequent work] simply seems too big to be believable. Fourth, Rose's evidence came entirely from countries that were either small (e.g., Ireland, Panama) or *very* small (e.g., Kiribati, Greenland, Mayotte), and so it was not clear that the estimates could be extended to larger countries.

While each of these four arguments has some validity, to each there is a better response than one might expect.

Times series dimension

First, regarding the time dimension, subsequent research on time-series data finds that a substantial share of the tripling that Rose had estimated from the cross-section data (which is presumably the long-run effect) shows up within a few decades of a change. Using a 1948–97 sample that includes a number of countries that left currency unions during that period, Glick and Rose (2002) find that trade among the members was twice as high in the currency union period as afterward. This suggests that roughly two-thirds of the tripling effect may be reached within three decades of a change in regime. (This reasoning assumes symmetry with respect to entry into and exit from currency unions.)

Baldwin thinks of this doubling as an estimate of the long-run effect. (He also views it as a likely overestimate, due to the usual claims of omitted variables and simultaneity, which could well be right, but which we will get to next.) Regardless, even 30 years may not be the long run effect. The effect may keep rising for a long time. We know that other gravity influences leave an effect on bilateral trade many decades after the cause has been removed. One piece of evidence is the slow speed of adjustment

estimated in models with lags.[5] Another is the effect that colonial relationships have even decades after independence, and even after controlling for continuing linguistic, political, or other links.   Consider as an illustration a trivia question: what is Congo's largest trading partner?   Not one of its neighbors, nor a large country, as the simple gravity model would lead you to expect; it is Belgium, the old colonial master, with whom ties were abruptly severed 35 years ago.[6] Even when the original reason for a high level of bilateral trade has disappeared, the stock of capital that firms have invested in the form of marketing and distribution networks, brand-name loyalty among customers, and so forth, lives on for many years thereafter. The word *hysteresis* is sometimes applied to this phenomenon, suggesting that the effect is considered to be permanent.   I think Baldwin may be right that there is an analogy with his early work on hysteresis in exchange rate pass-through. Why should not the same long-lagged effects not also be true of currencies? Panama has been on the US dollar for over a hundred years.

Furthermore it is likely that currency unions, like FTAs, can start to have substantial effects on trade patterns even before they have formally gone into effect. This is the pattern in the data.[7]   The most obvious interpretation is that once the negotiations (which typically have been going on for many years) are far enough along that the union appears likely to take place, businessmen move quickly to try to establish a position in what is expected to be a large new market opportunity, perhaps to get a "first mover advantage" in the case of markets not destined for perfect competition. Baldwin (section 5.1) makes much of the striking fact that the estimated Rose effect appears suddenly in 1998, even though EMU did not take effect until January 1999.   He regards this as suspicious. Even allowing perceptions to set the date rather than waiting for 1999, he says "right up to March 1998, skeptics doubted that monetary union would be a reality." I am the one who lives far from Europe, so he probably knows better.   But I have statistics from financial markets that identify June 1997 as the breakpoint in perceptions. On June 15, 1997, implied probabilities of joining Germany in EMU in 1999 were 100% for Belgium and France and over 70% for Finland, Spain and Portugal (calculations from JP Morgan based on spreads in the interest rate swap market).   A similar statistic from Goldman Sachs on the probability of EMU taking place on January 1, 1999, shot up above 75% after the Stability Pact was agreed in June 1997.   So I find it plausible that businesses had started reacting in a measurable way by 1998.

Omitted variables, misspecification, and endogeneity

The second objection concerns the possible influence of third factors. Rose did a thorough job of controlling for common languages, colonial history, and remaining political links.[8] The large estimated effect of a common currency remains. It seems very possible that there remain other third factors (e.g., accidents of history) that influence

---

[5] Eichengreen and Irwin (1998) .

[6] Kleiman (1976) finds that about one-quarter of the (2- to 4-fold) bias of colonial times remains for countries that have been independent for two decades. Wang and Winters (1991) and Hamilton and Winters (1992) find significant effects for UK Ex-colonial relationships (though not French) as late as 1984-86.

[7] E.g., Frankel (1997).

[8] While it is admirable how many factors Rose controls for, I agree with Baldwin and also Melitz (2001) in regarding as a "nuisance" Rose's persistent habit of calling these "nuisance parameters."   These coefficients are of interest in their own right, and also help gauge the persuasiveness of the overall model.

both currency choices and trade links. Nevertheless, Rose's various extensions of the original research—these robustness tests together with the time-series results—reduce some of the force of this critique.

Many of the critiques of the Rose results, after pointing out a problem of omitted variables or endogeneity or one of the other legitimate problems, offer an alleged way to address it and then report that the currency union effect disappears.[9]   My own view is that many of these responses in effect throw out most of the data in the name of addressing the (correctly identified) issues of endogeneity or country size. Or they do something similar: put in a large number of dummy variables or fixed effects, in some cases one for every pair of countries. This approach seems these days to be considered not just good econometric practice, but essential; we are told definitively that we are not allowed so much as a peek at evil studies that neglect to do this. But my view is that since the finding of statistical significance arose only when Rose put together a large enough data set for it to show up, there is little information gained in reducing the data set sharply and then noticing the loss in statistical significance.   The statistical power lies in the cross-country variation. Throw that out, and you are left with little.

As Baldwin relates (2.5.2), Rose (2001) himself tried adding pair fixed effects to his original data set, and found that the currency union dummy lost all significance, while pointing out that it is hard to see how it could have been otherwise, since all the action is in the bilateral cross-section. Ditto Pakko and Wall (2001). Klein (2002), who deliberately focuses on US bilateral data alone, is one of many examples of throwing out enough data until the results become insignificant. Persson (2001) is another, despite the virtues of the matching estimator.   When Rose tries the matching estimator on a larger data set, he found a significant (though smaller) effect (2.6.3).

Then there are the uni-observational case studies. Thom and Walsh (2002), which focuses on Ireland's abandonment of pound sterling in 1979, is also an example. In fact, a highly downward trend in the UK's share of Irish trade accelerates at just about the right time. But when the authors try to control for Ireland's accession to the EC, no significance remains. This is what one would expect, with so few degrees of freedom.   I think it is misleading to summarize the paper as "finds no evidence from time series or panel regressions that the change of exchange rate regime had a significant effect on Anglo-Irish trade. (Baldwin in 2.5.1). The evidence is about as supportive as could be expected, given how close together were the EC accession and the currency change. [10] The example of the Czech-Slovak breakup of 1993, which had a substantial negative effect on bilateral trade,[11] is viewed as more supportive of the Rose effect, apparently because a customs union was retained. But we know that political borders such as the one that divided the new Czech and Slovak Republics at the same time have effects at least as large as conventional trade barriers.

Glick and Rose (2002) put together a huge data set covering the entire postwar period, which includes enough additional examples like the breaking of the Irish-pound link and

_____

[9] See Rose (2001) for a reply to one, and his Web site
(http://faculty.haas.berkeley.edu/arose/RecRes.htm#CUTrade) for more.
[10] In fairness, this is similar to how the authors summarize their own "finding." Otherwise, I do like the paper, which appeared in the same issue as Glick and Rose (2002); I commissioned/edited it.   The case examined is potentially one of the more important ones, as Ireland is one of the largest countries in the sample of countries that entered or left a currency union in the postwar period.
[11] Frankel (1997, 121-122) and Fidrmuc and Fidrmuc (2001).

Czech-Slovak link to get statistical significance out of the time series dimension. Indeed, they are able to do so even when including pair-specific dummies, thereby giving up the power in the cross-section variation. It is indeed true that the beauty of fixed effects is that they take account of time-invariant facts, observed or unobserved; so Glick and Rose's still significant results are very persuasive This should have been enough to satisfy the hard-line gold-medal skeptics. As usual, the authors try lots of robustness checks. I don't agree with the admonishment (e.g., Tenreyo, 2004) that they should try all the robustness checks together, rather than one by one. One-by-one is the way to keep the volume of output manageable. Furthermore I don't see as interesting an algorithm that checks whether trying every possible permutation can eventually produce some equation in which the currency union coefficient loses significance.

The omitted variable that is probably of greatest concern to the critics comes from the influential Anderson-VanWincoop paper, and is variously called "remoteness," "multilateral resistance term," or what Baldwin wants to call the "relative prices matter" term. Baldwin is as fanatic on this point as VanWincoop: anyone who omits the relevant terms should not be received in polite society. As it happens, I am one of those who long ago included remoteness in some of my gravity estimates (though not all). I devoted two pages to the subject in Frankel (1997, 143-144), and noted that it sometimes makes a difference to the results.[12] I even gave the same intuitive example that Baldwin gives, of how the remoteness of Australia and New Zealand makes each of them somewhat dependent on the other for trade.[13]

I think the Anderson (1979) and Anderson-VanWincoop (2001) model is an important contribution, both as theoretical foundations for the gravity model and in offering an argument that some of the border effects may have been quantitatively over-estimated. But its insistence on the role of trade-diversion may be too doctrinaire. If I understand correctly the aspect of the Anderson-van Wincoop theory that leads to numerical estimates of the effects of borders and currencies that are sharply reduced (though still significant) is the property that the elimination of borders or currency differences entails substantial diversion of trade from the rest of the world to within the union. But it seems to me that such trade-diversion from currency unions is not in the data, by and large.[14]

Baldwin, after citing the gold medal to signal the primacy of the missing-variable critique, cites the silver medal for what he sees as a serious misspecification problem: aggregating exports and imports together. I won't take issue with his declaration that the

---

[12] The resistance to Canadian-U.S. trade is An example where it makes a difference; Wei (1996) found that controlling for remoteness helped knock down the home country bias from around 10 to around 3. Another may by the finding of a huge apparent effect of Pacific Islanders adopting the Australian dollar, in Nitsch (2001).

[13] Also it is just barely possible that I am responsible for coining "remoteness," in defining it explicitly as a term representing distance from everybody, expected to have a positive sign on bilateral trade, when estimated alongside bilateral distance, expected to have a negative sign. But I don't know.

[14] For example, the UK does not appear to have lost trade to euroland as a result of the euro. E.g., Begg, et al (2003), Frankel and Rose (2002), Frankel (2003), and Micco, Stein and Ordoñez (2003). (Baldwin also mentions trade-diversion in 4.1.1, perhaps making an improper analogy: going from trade theory, where trade barriers are distortions that deliver some revenue to somebody, to monetary theory, where it could be argued that currency barriers are dead-weight losses more akin to transport costs, whose elimination can potentially benefit all, even non-member countries.)

correct specification is to use estimate exports and imports separately, except to make two notes. Firstly, one advantage of adding them together is that exchange rate competitiveness effects (which worry him elsewhere) roughly wash out this way.   And secondly, many of us have tried it both ways – aggregating exports and imports together, or not – and have found that it usually does not make much difference.

   Rose (2004) performed a "meta-analysis," combining estimates from 34 other studies, which yielded a central tendency of 30% to 90% for the currency union effect. Baldwin is dismissive, as others have been. He knows which of the studies are good and which are bad (these are the ones that win the Olympic medals[15]), and wants only to count the good ones.   The problem with this is that other authors have other opinions as to what is good and what is bad.   I don't suggest that the meta-analysis should be viewed as the optimal estimator that pools all available information, as it might be in the medical research field. For one thing, these studies mostly use the same data set over and over again. But I don't buy that the researchers have all sought to mimic the same results "as a way of showing that the author's data set is sound in that it can reproduce the mistaken estimates in Rose (2000)" (2.8.1). To the contrary, I think a majority of them have from Day One been looking for a way to overthrow the new reigning champion. One might view the meta-analysis as a sort of poll of the estimates of the experts who have worked in the field -- not one of Rose's most important contributions to the debate, perhaps, but not to be ignored either.

The high magnitude of the estimates
   The third objection is the surprisingly large magnitude of the estimates.   In this regard, it is important to consider something else that we have learned in recent years: there is a strong home-country bias in trade, which is especially surprising in light of all one hears about globalization. Many studies have found that people trade with their fellow citizens far more easily than with those living in other countries. This finding emerges whether one looks at the volume of trade flows between locations or at the ability of arbitrage to keep prices in line across locations. It holds even when one controls for the effects of distance; trade barriers; and linguistic, social, and historical differences. It holds even between the United States and Canada. The best-known finding is that Canadian provinces are 20 times more prone to trade with each other than with U.S. states (McCallum, 1995). This estimate was cut very roughly in half after the Canada-U.S. FTA went into effect (Helliwell, 1998) and was cut further when a few more factors were controlled for (Wei, 1996). Nevertheless, a substantial bias—roughly threefold—remains even in this case. The home-country bias must certainly be higher for most other country pairs. Using the same gravity methodology, Nitsch (1990, 1991) finds that intranational trade within European countries is about seven times as high as trade with EU partner countries of similar size and similar distance away.

   Similarly, studies of the ability of arbitrage to narrow price differentials find that crossing the U.S.-Canada border discourages trade more than does traveling the entire length of Canada (Engel and Rogers, 1996) and that the barrier is even greater for other pairs of countries (Engel and Rogers, 1998; Parsley and Wei, 2001a, 2001b). What can explain these remarkable findings of home bias in both quantity data and price data? The difference in currencies is not an implausible explanation, given the paucity of alternative

---

[15] This paper doesn't just mix metaphors; it puts them through the blender.

candidates. This point -- that one needs to take the existence of home bias as the benchmark, not the presumption of perfect integration -- seems to be [one of the few things] missing from the Baldwin survey.

I have characterized the "get Rose" literature as an attitude of "guilty unless proven innocent:" put in enough dummy variables or fixed effects, or throw out enough data, until the estimates lose their significance. It might seem that to place the burden of proof on the proponents of a currency union effect follows naturally from the surprising magnitude and is consistent with the usual econometrics approach of requiring high significance levels – 5% or 1 % -- before asserting that a relationship is supported. But given the existence of home country bias, and the paucity of other candidates to explain it, I don't see why the burden of proof should be entirely on one side.

The estimated currency union coefficient is in magnitude and explanatory power roughly the same as the FTA variable, ranking behind the colonial relationship, and ahead of common language and the residual political union effect.[16] Baldwin takes it as self-evident that FTAs and customs unions have a big effect on trade. Indeed, he cites approvingly the assertion of Berger and Nitsch (2005) that it is implausible, even crazy, on the face of it to think that the trade effect of the euro could be as large as the trade effect of the EU. But this sort of finding is in fact common. If he and other critics of the currency union literature were to apply the same tough standards to both customs unions and currency unions, I think he might find that the estimated magnitudes, significance levels, *and necessary methodological qualifications* are comparable.

Further thoughts on small countries

Regarding the applicability of the results to large countries, we will not know for sure until the EMU experiment is older. It would seem plausible that very small geographical units (the Kiribatis) are so dependent on international trade—due either to inadequate scale of the domestic market or to insufficiently diversified factors of production—that strategies such as currency unions or free trade areas would have a larger payoff for them than for larger, more self-sufficient, economies. But there are two counterarguments. First, Rose tested whether there are any nonlinearities among his currency union sample (e.g., any difference between the effects among units that are merely small and those that are very small). He found no significant difference. Second, I don't see the evidence that the home-country bias departs from linearity, regardless of the size of the country, as being as overwhelmingly strong as Persson and Baldwin seem to (though I must admit that my priors have shifted a bit and are further shiftable). That is, if two small units join together, thereby doubling the size of the economy, the ratio of trade to gross domestic product (GDP) falls—that is, home-country bias increases—roughly as much (by 0.2, in log form) as when two large units join together.[17] To the extent that currencies explain this, the effect does not seem to be limited to small countries.

---

[16] This claim is confirmed by Rose and van Wincoop (2001), who estimate that half the typical border barrier is due to different sovereign monies.

[17] One would expect that below some size threshold, a unit like New Caledonia is so small – lacking a large enough internal market to sustain any scale economies and lacking a diversity of endowments – that it is highly dependent on trade to survive, and that currency unions and FTAs are alternatives to political unions to boost that trade. But, thinking ahead to estimates of the effects on growth, the long-run effect of increased trade on income seems to be the same for large and small countries (Frankel and Rose, 2002).

I agree that reverse causality of the decision to form a currency union is a big problem (endogeneity), which could bias the standard Rose results. It is certainly true that small countries are more likely to adopt currency unions – after all, trade shares are one of the classic optimum currency area criteria. But Germany and California are not small units, and they have joined currency unions. Baldwin mentions my attempt to address the endogeneity of exchange rate variability by using variability of money supplies as an IV for exchange rate variability (Frankel and Wei). He doesn't say what he thinks of this idea. Probably he saw that I was going to be the other discussant at the ECB conference, and so decided it would be prudent to withhold his true feelings. Basically I agree with him that "all instruments will be bad instruments" (2.7.2). But we are hard up for an instrumental variable to correct for the endogeneity of the exchange rate regime decision. The only clearly better proposal for an exogenous means of identification that I have encountered are the rare discrete experiments like EMU itself (and even here, of course, there is an element of endogeneity with respect to trade patterns).

One aspect of selection bias can go in Rose's direction: bilateral flows that are not recorded do not always reflect the absence of integration, but sometimes reflect a level of integration that is so high that the customs authorities can't measure all the cross-border flows (e.g., Belgium-Luxembourg).

Experience with the euro

Finally, we now have four to six years of data since the EMU went into effect in January 1999. Econometricians have updated the gravity estimates to see what can be learned from the record so far. Micco, Stein, and Ordoñez (2003) find that for pairs of the 12 countries that joined the EMU, trade has increased significantly. The estimated effect is about 15 percent beyond what could be explained by growth and other factors—actually a range of 6 to 26 percent, depending on the use of country and year dummies, with a larger set of 22 industrialized countries. The preferred Micco, Stein, and Ordoñez (2002) estimates of "differences in differences" reveal that between 1992 and 2001 the boost to intra-EMU trade was about 18 to 35 percent, depending on whether one uses country-pair dummies, or conditions on the standard gravity variables. These magnitudes are less than in the Rose studies. As the authors quite reasonably point out, however, the effects are not only statistically significant but also economically important, especially considering that the sample covers only the first four years of the EMU, a period in which the euro did not even circulate. Recall the arguments above that it might take many decades to reach the peak effects on bilateral trade.

Other evidence confirms the finding. Bun and Klaassen (2002, p.1) also update gravity estimates and find that "the euro has significantly increased trade, with an effect of 4% in the first year" and a long-run effect projected to be about 40 percent. Berger and Nitsch (2005) and De Nardis and Vicarelli (2003) report similarly positive results. Baldwin's favorite paper methodologically is Flam and Nordstrom (2003); but he is bothered by the implication of their estimated effects in that the "euro must be making it easy, cheaper and/or safer to sell to EuroZone nations." Well, why not? Multinationals now find it more convenient to sell into one and all, because they share a common "currency platform." A firm that might not have found it worth its while to export solely to, say, Ireland, now finds it can reach a much larger market; the marginal costs of

expanding into additional countries are lower than they were in the days of multiple currencies.

Studies with price data have tended to be more mixed, but some confirm that the euro is facilitating arbitrage among the markets of member countries.[18] It seems that the trade effects of monetary union are not, after all, limited to small countries.

---

[18] Looking at price data across pairs of European cities, Rogers (2001, 2002) finds evidence of convergence in the 1990s. In the European auto market, Goldberg, Koujianou, and Verboven (2001) find gradual convergence over the period 1970–2000. Goldberg and Verboven (2004) nail down EMU, per se, as a significant determinant of this convergence.

References

Anderson, James, 1979, "The theoretical foundation for the gravity equation," *American Economic Review* 69, 106-116.

Anderson, James and Eric van Wincoop, 2001, "Gravity with gravitas: a solution to the border puzzle," NBER Working Paper 8079, *American Economic Review*.

Baldwin, R. and D. Taglioni, 2004 "OCA".

Begg, David, et al, 2003, The Consequences of Saying No, *An Independent Report into the Economic Consequences of the UK Saying No to the Euro*, Commission on the UK Outside the Euro, London.

Brada, Josef C., 1993, "Regional Integration in Eastern Europe: Prospects for Integration Within the Region and with the European Community," in *New Dimensions in Regional Integration,* ed. by Jaime De Melo and Arvind Panagariya (Cambridge, UK**:** Cambridge University Press).

Bun, Maurice, and Franc Klaassen, 2002, "Has the Euro Increased Trade?" (unpublished; Amsterdam: University of Amsterdam). Tinbergen Institute Discussion Paper, TI 2002-108/2. Available via the Internet: http://www.eabcn.org/research/documents/bun_klaasen02.pdf

De Nardis, Sergio and Claudio Vicarelli (2003) "Currency Unions and Trade: The Special Case of EMU", *World Review of Economics*, 139 (4): 625-649.

Edison, Hali, and Michael Melvin, 1990, "The Determinants and Implications of the Choice of an Exchange Rate System," in *Monetary Policy for a Volatile Global Economy,* ed. by William Haraf and Thomas Willett (Washington: American Enterprise Institute).

Eichengreen, Barry, and Douglas Irwin. 1998, "The Role of History in Bilateral Trade Flows," in *The Regionalization of the World Economy*, ed. by Jeffrey Frankel (Chicago: University of Chicago Press).

Engel, Charles, and John Rogers, 1996, "How Wide Is the Border?" *American Economic Review*, Vol. 86 (December), pp. 1112–25.

———, 1998, "Regional Patterns in the Law of One Price: The Roles of Geography versus Currencies," in *The Regionalization of the World Economy*, ed. by Jeffrey Frankel (Chicago: University of Chicago Press).

Flam, Harry, and Hakan Nordstrom, 2003, "Trade Volume Effects of the Euro: Aggregate and Sector Estimates," Institute for International Economic Studies, Stockholm.

Frankel, Jeffrey, 1990, "Zen and the Art of Modern Macroeconomics: The Search for Perfect Nothingness," in Monetary Policy For a Volatile Global Economy, W. Haraf and T. Willett, eds., American Enterprise Institute, Washington, D.C..

--------- 1997, *Regional Trading Blocs in the World Trading System* (Washington: Institute for International Economics).

———, 2003, "The UK Decision re EMU: Implications of Currency Blocs for Trade and Business Cycle Correlations," in *Submissions on EMU from Leading Academics* (London; H.M. Treasury).

_____ "Real Convergence and Euro Adoption in Central and Eastern Europe: Trade and Business Cycle Correlations as Endogenous Criteria for Joining EMU," *Conference on*

Euro Adoption in the Accession Countries – Opportunities and Challenges, Czech National Bank, Prague, Feb. 2-3, 2004. KSG working paper RWP04-039. Forthcoming, International Monetary Fund, edited by Susan Schadler.

Frankel, Jeffrey, and Andrew Rose, 2002, "An Estimate of the Effect of Common Currencies on Trade and Income," *Quarterly Journal of Economics*, Vol. 117 (May), 437-466.

Frankel, Jeffrey, and Shang-Jin Wei, "Trade Blocs and Currency Blocs" NBER Working Paper 4335 and Global Finance Lecture, University of Birmingham, UK, November 1992. In The Monetary Future of Europe, Guillermo de la Deheza, et al, eds., Centre for Economic Policy Research: London, 1993.

___ "Is There a Currency Bloc in the Pacific," in Exchange Rates, International Trade and the Balance of Payments, edited by Adrian Blundell-Wignall, Reserve Bank of Australia, Sydney, 1993: 275-308.

___ "Yen Bloc or Dollar Bloc? Exchange Rate Policies of the East Asian Economies" (with Shang-Jin Wei), in Macroeconomic Linkages: Savings, Exchange Rates, and Capital Flows, *NBER - East Asia Seminar on Economics, Volume 3*, Takatoshi Ito and Anne Krueger, editors, University of Chicago Press, 1994.

____ 1995a, "Emerging Currency Blocs," in *The International Monetary System: Its Institutions and Its Future*, ed. by Hans Genberg (Berlin: Springer).

———, 1995b, "European Integration and the Regionalization of World Trade and Currencies: The Economics and the Politics," in *Monetary and Fiscal Policy in an Integrated Europe*, ed. by Barry Eichengreen, Jeffry Frieden, and Jurgen von Hagen (Heidelberg: Springer-Verlag).

___ 1997 "Regionalization of World Trade and Currencies: Economics and Politics" (with Shang-Jin Wei), in The Regionalization of the World Economy, J. Frankel, editor, University of Chicago Press, Chicago.

Glick, Reuven, and Andrew Rose, 2002, "Does a Currency Union Affect Trade? The Time Series Evidence," *European Economic Review*, Vol. 46 (June), pp. 1125–51.

Goldstein, Morris, 1995, *The Exchange Rate System and the IMF: A Modest Agenda,* Policy Analyses in International Economics No. 39 (Washington: Institute for International Economics).

[Hamilton, Carl, and L.Alan Winters. 1992. "Opening Up International Trade in Eastern Europe," Economic Policy (April).]

Helliwell, John, 1998, *How Much Do National Borders Matter?* (Washington: Brookings Institution Press).

[Kleiman, Ephraim, 1976, "Trade and the Decline of Colonialism," The Economic Journal 86, September, 459-480.]

Klein, Michael, 2002, "Dollarization and Trade." NBER working paper 8879, April.

McCallum, John, 1995, "National Borders Matter: Canada-U.S. Regional Trade Patterns," *American Economic Review*, 85 (June), pp. 615–23.

McKinnon, Ronald, 1963, "Optimum Currency Areas," *American Economic Review*, Vol. 53 (September), pp. 717–24.

Melitz, Jacques, 2001, "Geography, Trade and Currency Union," Univ.of Strathclyde, July, and CEPR Discussion Paper, No. 2987.

Micco, Alejandro, Ernesto Stein, and Guillermo Ordoñez, 2002, *Should the UK Join EMU?* (Washington: Inter-American Development Bank).

———, 2003, "The Currency Union Effect on Trade: Early Evidence from EMU," *Economic Policy*, Vol. 18 (October), pp. 315–43.

Nitsch, Volker, 1991, *National Borders and International Trade: Evidence from the European Union* (Berlin: Bankgesellschaft).

Nitsch, Volker, 2000, "National Borders and International Trade: Evidence from the European Union "*Canadian Journal of Economics*, November 2000, Volume 33, 1091-1105 .

Nitsch, Volker, 2001, "Honey, I Shrunk the Currency Union Effect on Trade," Bankgesellshaft Berlin, May.

Pakko, Michael R. and Howard J. Wall (2001) "Reconsidering the Trade-Creating Effects of a Currency Union", *Federal Reserve Board of St. Louis Review*, 83-5, 37-45.

Parsley, David, and Shang-Jin Wei, 2001, "Explaining the Border Effect: The Role of Exchange Rate Variability, Shipping Costs, and Geography," *Journal of International Economics*, Vol. 55 (October), 87-105.

———, 2001, "Limiting Currency Volatility to Stimulate Goods Market Integration: A Price Based Approach," NBER Working Paper No. 8468 (Cambridge, Massachusetts: National Bureau of Economic Research).

Persson, Torsten (2001) "Currency Unions and Trade: How Large is the Treatment Effect?", *Economic Policy* 33, 435-448.

Rose, Andrew, 2000, "One Money, One Market: Estimating The Effect of Common Currencies on Trade," *Economic Policy*, Vol. 15 (April), 7-45**.**

———, 2001, "Currency Unions and Trade: The Effect Is Large," *Economic Policy*, (October) 33, 449-461.

_____, 2004, "A Meta-Analysis of the Effect of Common Currencies on International Trade," NBER WP no. 10373, March.

Rose, Andrew K. and E. van Wincoop (2001) "National Money as a Barrier to Trade: The Real Case for Monetary Union", *American Economic Review* 91-2, 386-390.

Tenreyro, Sylvana, 2001, "On the Causes and Consequences of Currency Unions," Harvard University.

Thom, Rodney, and Brendan Walsh, 2002, "The Effect of a Common Currency on Trade: Ireland Before and After the Sterling Link," *European Economic Review*, 46, no. 6, June, p. 1111-1123.

Wang, Zhen Kun, and L.Alan Winters, 1991, "The Trading Potential of Eastern Europe," Centre for Economic Policy Research Discussion Paper No. 610, November, London, UK.