

Richard Baldwin, "The Euro's Trade Effects"

This instructive, highly readable monograph has two fundamental aspects. First, it is by far the most in-depth and comprehensive analytical survey of the Rose (and rosy) literature on the impact of EMU on bilateral trade among the members. Second, it contains an effort to explain this impact. Notably, Baldwin argues that small as the impact of EMU on bilateral trade is, it is too large to result from usual thinking and requires special explication.

No one who followed the Rose discussion ever doubted that the author's initial assessment that EMU might triple or quadruple trade among the members was exaggerated. Still, based on all of the subsequent evidence, many of us felt that the effect was larger than could ever have been guessed beforehand: probably well over 20%. Few of Baldwin's readers will come away still cherishing this belief. According to Baldwin's "bottom line," the true numbers are about 5 to 10%. Most important in destroying confidence in big numbers, I believe, is Baldwin's telling argument that one does not need to choose between the major criticisms of Rose's initial assessment. All of them are correct. There is the issue of omitted variables. In addition, there is that of reverse causality. Further, there is the problem of model misspecification (which Baldwin expresses with particular force and clarity). As a matter of fact, I find the part of the monograph requiring most development to be the support for the 5 to 10% figures themselves following the important section on Carousel, fraud, ROOs and PECS where Baldwin casts doubts upon them. There must be better reason to adhere to those numbers than businessmen's confidence that "of course, EMU increased trade." In my following remarks, I will turn a blind eye to this problem (reparable, I believe).

I have three main points to make about the 5 to 10 % figures, and then a few lesser remarks to add concerning Baldwin's criticisms of earlier contributors. In the first place, there is no mystery about a 5-10% impact of EMU on trade. That is roughly what earlier empirical work together with the evidence of the European Commission in *One Market One Money* would have led us to guess had Rose never written. If those are the right figures, then there is no bloom to the Rose at all. Baldwin had no cause to search around for new theoretical foundations. Second, the alternative, more traditional explanation I support is equally consistent with the de-

tailed data that Baldwin marshals in favor of his more novel reasoning. Third, based on the two empirical studies on which Baldwin mainly rests his 5-10% assessment – the Micco-Stein-Ordoñez (2003) and Flam-Nordstrom (2003) ones – the true figure is closer to 15%. As for my lesser points, I would like to defend Rose (and most of his followers including myself) against Baldwin’s criticisms for converting all the series into constant dollars and for focusing on total trade rather than either exports or imports. In addition, in any choice between import and export data, if one is needed, it is also not clear to me that the export data is preferable.

### I. *The size of the Rose effect*

a. With regard to the 5-10% figures, Baldwin’s micro-founded gravity equation for bilateral trade is a good start. Suppose we add Anderson and van Wincoop’s (2003) simplification  $\tau_{od} = \tau_{do}$ , saying that the ratio of trade costs to output price is the same for goods moving in either direction (if only because this makes everything more transparent). Consequently, in terms of Baldwin’s notation, we get the well-known specification:

$$V_{od} = \left( \frac{\tau_{od}}{P_o P_d} \right)^{1-\sigma} \frac{E_o E_d}{E_W} \quad P_i = \left[ \sum_k (\beta_k P_k \tau_{ki})^{1-\sigma} \right]^{1/(1-\sigma)} \quad i = d, o \in k$$

$E_W$  refers to world spending on current output,  $\beta_k$  is the ratio of country  $k$  output to world output,  $P_o$  and  $P_d$  are the Dixit-Stiglitz utility-based price indices. As distinct from Baldwin, my stress will be on  $\sigma$ . According to the equation, the very sign of the impact of trade costs  $\tau_{od}$  on trade  $V_{od}$  depends on  $\sigma$ , the intra-temporal elasticity of substitution between goods  $o$  and  $d$ . Trade would fall with a fall in trade costs if  $\sigma$  were zero, it would stay constant with unitary elasticity, and it only rises because  $\sigma$  is greater than one. Usual estimates of  $\sigma$  are of the order of 6 to 8. Obstfeld-Rogoff (2000) use 6 and Anderson-van Wincoop (2004) use 8. Consequently, a fall in relative prices of 1% will suffice to yield a 5 to 7% rise in bilateral trade between  $o$  and  $d$  and therefore to bring us into Baldwin’s 5-10% range. A fall in relative prices of 1% is easy to defend.

In its well-known report *One Market One Money*, the European Commission (1990) calculates that eliminating costs of conversion of currencies and costs of cover for exchange risk will reduce costs by .25 to .5 of 1 percent of total output in the EU. The report also stresses

additional gains that would come from the elimination of “in-house costs” associated with multiple units of account. These are reductions in costs connected with record keeping, accounting and decision-making. The previous figures of .25 to .5 of 1% are therefore lows, and those lows are relative to GDP. In order to relate the figures to trade we must multiply by 3 to 4, since trade in the average EU member is about a quarter to a third of GDP. Assume then a fall in trade costs of about 2% relative to trade. Next, it is necessary to correct this figure downward in light of the fact that EMU must lower the product  $P_o P_d$  as well. (Here I am avoiding the “gold medal” mistake.) For example, the increase in bilateral trade between France and Italy will be mitigated by the fact that both countries’ trade costs will also fall relative to Germany, the Benelux and Spain. The EMU embraces roughly 50% of the trade of the members. Therefore, based on the earlier 2% figure, the adoption of monetary union will lower  $P_o$  and  $P_d$ , respectively, by about 1%. The net result will be a fall in  $\tau_{od}/P_o P_d$  of roughly 1% – the figure we were looking for. So much then for “the dog that didn’t bark” or the fact EMU did not produce a sharp break in equations for relative prices. It did not need to do so in order to account for an effect of 5 to 10% on  $V_{od}$ .

*b.* Based on this line of support for the 5-10% figure, there is also no difficulty explaining the detailed evidence Baldwin summons. The impact of EMU on trade was particularly high for the DM-bloc, consisting of Germany, the Netherlands and Austria, and low for Portugal and Greece. Baldwin draws from Marc Melitz (2003) for a new and sophisticated explanation for this evidence. According to Melitz’s model, even a minor change in costs, coming from reductions in exchange rate uncertainty, could lead more efficient firms to move significantly into the export of differentiated goods. However, the same evidence is entirely consistent with the simpler and more traditional explanation I propose. The elasticity of substitution between goods produced by countries that are already closely integrated through trade will be particularly high. This can only mean more trade between Germany and the Netherlands, for example, than between Germany and Greece. (Of course, the rise in German trade with Greece may nevertheless raise welfare more than the rise in its trade with the Netherlands since it refers more largely to greater heterogeneity in trade as opposed to greater variety: but that’s a dif-

ferent story.) As further evidence on behalf of his theory, Baldwin also cites the relatively strong impact of EMU on trade in the industries producing differentiated goods. But that too fits neatly into my simpler interpretation.  $\rho$  must be higher between varieties than between totally different goods.

*c.* Finally, as regards numbers, according to Baldwin's favorite study, by Flam and Nordstrom, the figure is higher than 5-10% – at least if we base ourselves on these two authors' widest control group consisting of 11 rich countries. In this case, their results show 15% more trade from EMU. To justify his lower 5-10% figures, Baldwin relies on the 8% result that Flam and Nordstrom get when they narrow their control group to include only the 3 EU members outside the euro zone – their “cleanest definition of the control group,” he thinks. However, it is easy to argue that the 15% figure is better.

The EMU members could get 15% more trade among themselves as opposed to everyone else on average, while they only get 8% more trade among themselves as compared with the UK, Denmark and Sweden. This could be because EMU also increased their trade with the UK, Denmark and Sweden by 7%. In fact, this last result fits nicely with the evidence from Micco-Stein-Ordoñez – the other study on which Baldwin relies highly – and it would be easy to explain. In principle, outsiders ought to reap some monetary benefits of fewer moneys and fewer units of account from EMU, and the UK, Denmark and Sweden should do so more than most countries on average since they trade far more with the EMU members than the average non-member does. To illustrate, consider the situation of Canada if there were 50 different state monies in the U.S. Evidently Canadians benefit greatly from reductions in transaction costs and units of account from a single U.S. currency, and they do so more than the average country outside North America. Generally, introducing a common currency should be seen as a graded reduction in trade barriers applying mostly to the members but extending to everybody else to a degree depending on how much trade they do with two or more of the members. The 15% figure is then preferable.

## II. *The criticisms of the tests in the Rose literature*

Baldwin offers some interesting criticisms of the empirical use of the gravity model in the Rose literature. But I take exception to a couple of them. In his engaging and inoffensive manner, he sprinkles Olympic medals around in the Rose garden to indicate bad herbs. There is no question about the “gold medal” mistake. But what about the “bronze” and the “silver”?

*a.* The “bronze medal” mistake refers to the practice of converting trade and output figures into real values, as Rose and many of his followers do. Baldwin observes that the model applies to nominal values not real ones. But he disregards the fact that Rose does not simply convert nominal values into constant dollars. Rose also uses output values from the Penn World Table. Those values are then PPP-adjusted and their use therefore may reflect a desire to avoid distortions that come from converting undervalued currencies into dollars. Using undervalued currencies for conversion can mean grossly undervaluing non-traded relative to traded output. Think of Russian wages relative to Russian oil following the disintegration of the Soviet Union. Interestingly, therefore, the resort to real values by Rose and others can be seen as an effort to avoid precisely the error that Baldwin attributes to them: that of introducing distortions in relative values of traded and non-traded goods.<sup>1</sup>

In general, the gravity model says nothing about multiple currencies. It follows easily under barter. The only necessity is a common unit of account. Consequently, when taking the theory to the data, we face the problem of dealing with the presence of multiple monies. Relying on nominal exchange rates for converting into a single unit becomes sheer necessity. But nominal exchange rates are also high frequency variables whose movements can produce serious distortions of relative prices of traded and non-traded goods, as the previous example of Russia was meant to signify. Hence, it may be right to supplement the conversions with PPP-adjustments. Of course, that is not necessarily the case. If exchange rates are well aligned,

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<sup>1</sup> In addition, Baldwin takes a strong stand about the sign of the probable distortion. According to him, in theory, countries belonging to monetary unions ought to be unusually open, and therefore should exhibit exceptionally low values of traded goods relative to non-traded goods. The logic is plain (see section 2.3.3); but one can argue for the exactly opposite result based on Balassa-Samuelson. Rose’s many studies concern monetary unions consisting mostly of tiny principalities with low wages. Consequently, those countries could feature unusually low relative prices of non-traded goods relative to traded goods – precisely the opposite outcome.

sticking to the nominal values may be best. But the principle of always sticking to the nominal values looks doubtful at best. All things considered, perhaps the best thing to do is to try both conversion based on nominal exchange rates alone and the use of PPP-adjusted real values and then to choose the alternative that yields the closest approximation to the theoretical implication of the gravity model of a unitary elasticity of trade,  $V_{od}$ , with respect to output,  $Y_o Y_d$ .

*b.* The distance between France and the UK is the same as the distance between the UK and France. The absence of a common language and a common money between the two countries is also identical from either side. According to the gravity model, the bilateral trade between the two depends on their respective outputs. But the model also says that the relevant variable is the product of the outputs, and this variable too is identical on both sides. Moreover, Anderson and van Wincoop (2003) show that under all the usual assumptions of the model, if we also suppose that trade costs both ways are the same ( $\tau_{od} = \tau_{do}$ ), bilateral trade between all countries must balance. In consequence, many researchers take the view that the model, in its simplest form, says nothing about trade imbalances and applies strictly to total bilateral trade, as measured by the average (or the total) of the movement both ways. Evidently, on this interpretation, the proper variable to consider when converting into logs is the log of the average rather than the average of the logs of trade one way and the other [compare Baldwin's section 2.3.4]. In Baldwin's eyes, this is all wrong; it is the "silver medal" mistake.

There is no question that it would be better to explain the movement of goods in both directions, as Baldwin enjoins us to do. But suppose that we try. In that case, do we not simply need a better model or a more sophisticated version of the one we have? Can it really suffice merely to keep the identical model, use separate figures for the movement of trade both ways, and simply add a relative price term concerning the exchange rate? In fact, Baldwin refers approvingly to a working paper by Helpman, (Marc) Melitz and Rubinstein (2004) that adds productivity differences between firms in different countries in order to explain imbalances in bilateral trade. But is that enough? Think of bilateral trade between China and the U.S. If we employ the usual gravity variables to explain Chinese exports to the U.S. and U.S. exports to

China, then merely add an exchange rate and an indicator of productivity differences in order to take account of trade imbalances, will we not still make wild mistakes in predicting the flow of trade one way or the other or both? Must we not somehow take into account the U.S. willingness to borrow and the Chinese willingness to lend? Generally, it would seem that in order to cope with bilateral trade imbalances we must recognize differences in desired intertemporal substitution between countries and deviate from the assumption that all countries wish to maintain balanced trade in the aggregate. In sum, it is not evident that the way ahead is already well paved.

My main point, in these last remarks, however, is that the usual tendency to apply the gravity model to aggregate bilateral trade data may merely reflect a desire not to take the model further than its simplified structure will allow.

*c.* If exports both ways are to enter, a final issue is which data to use: those for the shipments or the arrivals? Baldwin recommends the shipments. He has a good argument with respect to the EU: the destination principle applying to VAT makes export figures more reliable since the exporters want to be reimbursed for the VAT they pay at home. But the situation concerning VAT in the EU is not universal. Tax considerations often lead to the opposite result: more honest reporting of expenses (or imports) than receipts (or exports). In fact, many consider underreporting of exports to be big in explaining the massive trade deficit on a world level. In addition, the gravity model is simply a demand equation in Baldwin's neat formulation. If so, it is difficult to see how export figures can be ideal. What matters to the buyer are clearly the landed goods rather than those shipped. According to numerous presentations, some of the goods even "melt" along the way. Finally, if we look at export flows in both directions, there is also an issue of identification concerning the sign of the exchange rate. Based on these many difficulties, are shipments really to be generally preferred to arrivals in testing the gravity model?

### III. *Coda*

Let me close by returning to my earlier assessment that Baldwin's piece is "must" reading for any one interested in the Rose debate. His paper has permanently affected my own views. Fireworks may continue. So much is at stake. But with this essay, the War of the Roses may be passing into history at last.

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