John F. Kennedy School of Government
Harvard University
Faculty Research Working Papers Series

An Estimate of the Effect of Common Currencies on Trade and Income

Jeffrey A. Frankel and Andrew K. Rose

April 2001

RWP01-013

An Estimate of the Effect of Common Currencies on Trade and Income *

April 12, 2001

Jeffrey A. Frankel
Kennedy School of Government
Harvard University, 79 JFK Street
Cambridge MA 02138-5801
Tel: (617) 496-3834
Fax: (617) 496-5747
jeffrey_frankel@harvard.edu
http://www.ksg.harvard.edu/fs/jfrankel

Andrew K. Rose
Haas School of Business
University of California
Berkeley, CA 94720-1900
Tel: (510) 642-6609
Fax: (510) 642-4700
arose@haas.berkeley.edu
http://haas.berkeley.edu/~arose

Abstract

Gravity-based cross-sectional evidence indicates that currency unions and currency boards stimulate trade; cross-sectional evidence indicates that trade stimulates income. This paper estimates the effect that common-currency regimes have, via trade, on income per capita. We use economic and geographic data for over 200 countries to quantify the implications of common currencies for trade and income, pursuing a two-stage approach. Our estimates at the first stage suggest that belonging to a currency union more than triples trade with the other members of the zone. Moreover, there is no evidence of trade-diversion. Thus currency unions raise overall trade. Currency boards have similar effects. Our estimates at the second stage suggest that every one percent increase in trade (relative to GDP) raises income per capita by at least one third of a percent over twenty years. We combine the two estimates to quantify the effect of common currencies on output. Our results support the hypothesis that the beneficial effects of such regimes on economic performance come through the promotion of trade, rather than through a commitment to non-inflationary monetary policy, or other macroeconomic influences.

JEL Classification Numbers: F11, F33, O40

Keywords: dollarization, growth, cross-section, empirical, output, union, board, monetary.

* Jeffrey Frankel is Harpel Professor and Director of the NBER program in International Finance and Macroeconomics. Andrew Rose is Rocca Professor, Research Associate of the NBER, and Research Fellow of the CEPR.

The data sets and a current version of the paper are available at Rose's web site. For catching computational errors, we thank Jacques Melitz and Dani Rodrik; for comments we thank: Alberto Alesina, Alan Deardorff, John Helliwell, Tim Kehoe, Dani Rodrik, Torsten Persson, Romain Wacziarg, two helpful anonymous referees, and seminar participants at the Bundesbank, Columbia, the European Central Bank, Goethe University, Harvard, the Hoover Institute, LACEA Rio, Michigan, NBER IFM program, Notre Dame, the Swiss National Bank, and Universidad Torcuato Di Tella. This is a condensed and updated version of NBER WP #7857.

1. Introduction: Why should Common Currencies affect Income?

Proponents of currency unions and currency boards tout them as the ultimate credible commitment to non-inflationary monetary policy. Among the benefits frequently cited are enhanced central bank credibility, superior inflation performance, and deeper capital markets, all of which tend to raise productivity and hence output. This paper confirms that currency unions and boards improve the performance of an economy. However the channel we focus on is the substantial stimulus to trade that a common currency gives to its members, rather than the macroeconomic and financial influences conventionally emphasized.

The paper provides an estimate of the effect that currency union/board has on the long-run level and rate of growth of real income, in a cross-section of countries. We proceed by investigating two relationships: the hypothesis that currency union/board stimulates trade among its constituent units, and the hypothesis that trade in turn stimulates output.

The paper is wholly empirical and presents two chief results. First, common currencies promote bilateral trade. They also promote overall openness (i.e., the ratio of trade to GDP); there is no evidence that trade created among members of a currency union comes at the expense of a diversion of their trade away from non-members. Secondly, by raising overall trade, currency unions also raise income. We also present one negative result, since we test and find no support for the common argument that currency unions improve income through other channels, e.g., by enhancing the central bank's credibility and/or stabilizing the macroeconomy.

If the benefits of currency union resulted from monetary stability, the composition of a currency union would not matter, so long as the anchor currency is strong and stable. In our view, however, geography is highly relevant to the makeup of common currency areas. Countries tend naturally to trade more with large neighbors; thus the benefits to adopting the

currency of a large neighbor will exceed the benefits to adopting the currency of a country that is smaller or more distant, other things being equal. We demonstrate this effect by estimating the benefits for a large cross-section of countries of adopting either the dollar or the euro.

In section 2 below, we estimate the effect of currency unions and currency boards on trade. Section 3 provides estimates of the effects of trade on income, taking into account the likely endogeneity of trade. It also provides tests for alternative effects of currency union on income. Section 4 estimates the effect of "dollarizing" or adopting the euro for individual countries. After some sensitivity analysis and caveats, the paper ends with a few brief conclusions.

2. The Effects of Currency Union on Trade

A popular argument against floating currencies – albeit one that most academic economists have been skeptical of – is that exchange rate variability creates uncertainty that discourages international trade and investment. Fixing the exchange rate eliminates this risk, and so encourages trade. Adopting a neighbor's currency as one's own is a credible commitment to exchange rate stability and has the extra advantage of eliminating transactions costs; both effects may promote trade. The objective of this section of the paper is to provide a quantitative estimate of the effect of currency union in promoting trade.[1]

One reason that academic economists have tended to downplay this argument is that much exchange rate risk can be hedged at low cost, through the use of forward contracts and derivatives. Another reason is that there have been quite a few empirical studies of the effect of exchange rate volatility on both trade and investment; most find small or negligible effects.[2] It is

difficult to estimate a relationship between exchange rate variability and trade using time-series data.

By way of contrast, cross-sectional approaches that use the "gravity" model have found a negative effect of bilateral exchange rate variability on bilateral trade in the 1960s and 1970s.[3] Rose (2000) finds sharper and more persistent effects and also shows that belonging to a common currency area has a large impact, multiplying trade by an estimated factor of over three. Evidently there is a discrete large benefit from eliminating transactions costs and the possibility of future rate changes, which goes beyond the benefit of simply reducing exchange rate variation to zero.

In the bare-bones gravity model, trade between a pair of countries is modeled as an increasing function of their sizes and a decreasing function of the distance between the two countries. The model is one of the more successful empirical models in economics: typically a reasonable proportion of the variation in trade is explained with a model where the coefficients are economically sensible, and well determined statistically. Among many possible references, Frankel (1997) provides a thorough review of the model. Since we are interested in estimating the effect of currency union on trade (and hence output), the gravity model is a natural vehicle to use.

Gravity Estimates

Table 1 reports the results of a number of different specifications of the gravity equation, augmented by different sets of controls. We are most interested in the coefficients on two dummy variables. The "Currency Union" variable is unity if the two countries belonged to a common currency area (such as Panama and the United States), and zero otherwise. "Currency

Board" is unity if one of the countries uses the currency of the other in a currency board arrangement (such as Hong Kong and the United States). All specifications include the standard gravity regressors; we also include a variety of other controls to demonstrate the robustness of our results. The panel data set includes observations from almost 8,000 country-pair observations (from over 180 countries and territories) at five-year intervals from 1970 through 1995. Slightly over 1% of the observations involve trade between members of currency unions or currency board arrangements. The data set is described in more detail in the first appendix.

We estimate our equations with OLS. The standard errors reported are robust to clustered heterogeneity, and year-controls are included in the regression but not reported.

The models of Table 1 fit well, explaining over sixty percent of the variation in the data. The coefficients for the traditional gravity determinants are highly statistically significant, and economically sensible. The estimated coefficient on log distance is slightly over -1, indicating that trade between a pair of countries falls by about one percent for every one percent increase in the distance between them. The coefficient on size (log real GDP) is slightly less than unity; trade rises with size but (holding constant for income per capita) somewhat less than proportionately. That is, large countries are more self-sufficient. Income per capita has its own estimated effect. The coefficient, around 0.5, indicates that rich countries trade proportionately more than the poor.

Two countries that speak the same language trade more by a multiplicative factor of around 1.8 (=exp(.6)), as do countries that share a common land border. Belonging to a regional trading bloc also increases trade, as does a common colonial heritage and a historical link to a mother country. Landlocked and geographically large countries trade less. Areas in political union (such as France and its overseas departments) also trade more.

The focus here is on the currency union and currency board coefficients at the top of the table. The coefficients for each are positive, significant and large. The currency union and board coefficients are also similar in size; we cannot reject the hypothesis that they are equal.[4] While we can think of no compelling theoretical reason why the currency board effect should be precisely the same as that of currency unions, the empirical evidence seems strong; accordingly we impose that restriction from now on. As we add more controls, the coefficients fall from over 2 to 1.36. This estimate still implies that when two units share a common currency or are in a currency board relationship, trade is multiplied by a factor of over three (exp(1.36)=3.9), similar to the estimate in Rose (2000). Inspection of year-specific effects shows a small tendency for the coefficient to rise over time, between the 1970s and the 1990s.

A three-fold effect strikes those new to this literature as large, and indeed it is. But it is more plausible when one recalls the findings, for example, of McCallum (1995) and Helliwell (1998), that Canadian provinces are twelve to twenty times more inclined to trade with each other than with US states, after holding constant for distance and size. The latter finding has received much attention because it cannot be easily explained by geographic, linguistic, or trade policy variables. High on the list of possible reasons why integration is so much higher between provinces within a federation such as Canada than between countries is the fact that the provinces share a common currency. [5]

This massive "home bias" towards domestic trade also characterizes our data set.[6] It seems eminently plausible that some part of home bias is explained by the fact that trade across international borders usually entails trade between different monies. Our equations in effect show that the unexplained part of home bias can be reduced by measuring attributes that are shared by different areas both within and across countries, such as common language, trade

policy and so forth. They show that the currency union variable ranks in explanatory power roughly equal with the FTA variable, behind the colonial relationship, and ahead of common language and the residual political union effect. This claim is confirmed by Rose and van Wincoop (2001), who estimate that half the typical border barrier is due to different sovereign monies.

To check for the possibility that the stimulus to trade among members of a currency union comes at the expense of diversion of trade with non-members, we added a dummy variable that is unity when precisely one of the members of the pair belongs to a currency union or board. It turns out to show up with a statistically significant *positive* coefficient. Thus the evidence points toward trade creation rather than trade diversion. We corroborate this point in the working paper version using aggregate (rather than bilateral) trade data.

3. The Effect of Trade on Income

In this section of the paper, we estimate the effect of trade on output.

Classical trade theory gives us good reason to think that trade has a positive effect on the level of real income. New trade theory has made the field more realistic by introducing roles for increasing returns to scale, trade in imperfect substitutes, and endogenous technology.[7] Some new trade theory also implies that open economies have higher long-run growth rates, rather than just higher income levels, since interaction with foreigners spurs innovation by speeding up the absorption of new ideas.

Quite a few empirical studies of growth rates across countries find that the ratio of exports to GDP, or some other measure of openness, is a significant determinant of growth.[8] A typical specification begins with the determinants of output suggested by neoclassical growth

theory, and adds a variable for exports as a share of GDP.[9] In such empirical work, openness typically seems to have a positive and significant effect on the growth rate.

Interpreting a significant correlation between trade and growth as implying causality from the former to the latter is potentially problematic however, because of the serious problem of simultaneity bias. A number of studies have tangled with the challenge posed by simultaneity. Many studies have sought to identify measures of trade policy, hoping that they are exogenous.[10] Even side from the serious difficulty of measuring trade policy, a fundamental conceptual problem of simultaneity remains (e.g., Sala-i-Martin, 1991). Free-market trade policies may be unimportant to growth but correlated with free-market domestic policies that are hard to measure. In this case, openness will be correlated with growth, even though trade does not cause growth. There have also been other attempts to solve the problem with mixed results. Some have applied Granger-causality tests to the problem; others have attempted to use a simultaneous equation approach. As so often in macro-econometrics, however, the simultaneity problem has remained largely intractable.

What is needed is a good instrumental variable, which is exogenous yet highly correlated with trade. The gravity model offers a solution. Such variables as distance, populations, common borders, and common languages are plausibly exogenous.[11] Yet these variables are highly correlated with trade, and thus make good instrumental variables. We use an intuitive two-step implementation of this idea. In the first stage, we estimate bilateral trade equations using the exogenous regressors in a gravity model. We then aggregate (the exponential of) fitted trade across a country's trading partners to create a prediction of its overall trade. In the second stage we use this predicted trade variable as an instrument for actual trade in an output equation.

If trade still appears to be a significant determinant of output with instrumental variable estimates, then the effect of trade on output is plausibly causal.

This procedure has been implemented in Frankel and Romer (1999), who find that the effect of trade on output actually increases after correcting for simultaneity.[12] Irwin and Tervio (2000) have used the same technique on eight observations spread between 1913 and 1990, and have found that the trade variable has a highly significant effect on income (except for two inter-war years), with a magnitude comparable to that of Frankel and Romer.

The growth equation

The convergence hypothesis in the growth literature dictates that income at the end of a period depends on income at the beginning of the period, with a tendency to regress gradually toward some long-run steady state. Convergence is conditional if it is present only after conditioning on variables such as factor accumulation.[13]

While we consider a number of variants, our approach is based on an equation for which Mankiw, Romer, and Weil (1992) provide theoretical and empirical support. Our basic specification is:

$$\ln(Y/Pop)_{90,i} = \alpha([X+M]/Y)_{90,i} + \beta_0 + \beta_1 \ln(Pop)_i + \phi Z_i$$
$$+ \gamma \ln(Y/Pop)_{70,i} + \delta_1 (I/Y)_i + \delta_2 n_i + \delta_3 School1_i + \delta_4 School2_i + u_i$$

where: the dependent variable is the natural logarithm of GDP (Y) divided by total population (Pop) at the end of 1990, measured in real PPP-adjusted dollars for country i; aggregate exports, aggregate imports, and gross investment are denoted "X", "M" and "I" respectively; the growth

rate of population is denoted "n"; "School1" and "School2" are estimates of human capital investment based, respectively, on primary and secondary schooling enrollment rates; "Z" denotes other controls; Greek letters denote coefficients; and "u" denotes the residual impact of other, hopefully orthogonal influences. Variables other than GDP per capita and openness are computed as averages over the sample period. Following the norm in the growth literature, we measure openness as the ratio of trade to output. The coefficient of interest to us is α, the effect of openness on output.

We call "controls" the variables that derive from neoclassical growth theory and appear on the second line of the equation: initial income, investment, human capital and population growth. Frankel and Romer (1999) and Irwin and Tervio (2000) adopt a more stripped-down specification by omitting these controls, following Hall and Jones (1999). They simply regress output per capita against openness and measures of country size, reasoning that the factor accumulation variables might be endogenous. Including these controls in the output equation might result in a downward-biased estimate of α, if some of the effect of openness arrives via factor accumulation. Of course, inappropriately excluding these variables would also produce biased results and could be expected improperly to attribute too large an effect to trade. Consequently we estimate equation (1) both with and without controls and try to be conservative in our interpretation. Our own preference is for the specification that includes the controls, in part because it is likely to avoid a possible upward bias in the openness coefficient.

OLS results

We begin by estimating our output equation with OLS to replicate the common finding that there is a statistical association between trade and income. In Table 2, we report OLS estimates of the impact of trade on output both with and without factor accumulation controls.

The key estimate in the income equation, the coefficient of openness, is positive, statistically significant, and economically large, whether we include controls (in which case the coefficient is .33) or not (.79). Population has a positive and statistically significant influence whether we include controls or not, confirming that larger countries are better able to take advantage of scale economies and/or resource diversity.

As already noted, the openness variable may be standing in for factor accumulation variables or other national characteristics and initial conditions less easily measured. We want to hold constant for variables such as investment, knowing that we run the risk of then failing to give credit to openness for some effect on income that comes via factor accumulation. When initial GDP, along with other standard growth controls, is included, its coefficient is a highly significant 0.71, representing a plausible degree of conditional convergence.

The key effect of interest is the coefficient on trade, which is a significant 0.33 in the OLS version. This says that, holding constant for 1970 income, income in 1990 was 1/3 per cent higher for every 1.0 percentage point increase in the trade/GDP ratio. As expected, this effect is less than when we did not control for initial income. When multiplied by 3.45 (=1/(1-.71)) to convert to an estimated effect on long-run income, the effect on output is 1.14 per cent for every 1.0 percentage point increase in openness. Parenthetically, the effects of investment and both schooling variables are statistically significant and reasonable; population growth has

the hypothesized negative sign, but as in earlier work is the one neoclassical growth determinant that is not statistically significant.[14]

Instrumental Variable Results

The next step is to estimate the corresponding output equation estimates using IV estimation to account for the possible endogeneity of openness. The instrumental variables we choose come from a simple gravity model which uses as controls: the log of distance, the log of partner country population, the log of area, and dummy variables for common language, common land border, and landlocked status. (The first stage is presented at the bottom of Table 2.) After estimating the gravity model, we aggregate the exponent of the fitted values across bilateral trading partners to arrive at an estimate of total trade for a given country. The correlation between actual trade shares and our generated instrument is a reassuringly high value of .72.[15]

The estimate of interest to us is α, the coefficient on openness. When initial income and other controls are not included, the coefficient is estimated to be a statistically significant 1.61. When we include controls, the effect of trade on output is .43. The implied steady state impact is 1.6 (=.43/(1-.73)), similar to the estimate without the controls. These effects are economically and statistically significant. Both IV estimates are higher than their OLS analogues (the opposite of what simultaneity bias leads one to expect). However, the IV estimates are insignificantly different from the OLS results; Hausman tests do not reject the hypothesis of equal slopes.[16] Thus, we see little evidence of reverse causality running from income to openness.

Table 2 also shows that adding the log of land area as another measure of country size does not destroy the finding of a large effect of openness on output. Land area only enters

positively in the version without controls, where its coefficient is insignificantly different from zero. The presence of land raises the openness coefficient in the version without controls, and lowers it in the version with controls; both changes are insignificant.

To summarize, we have found that openness seems to have a positive effect on real income per capita. Our results seem robust and both economically and statistically significant. To be conservative, we use .33 in our calculations below (our OLS estimate of the effect of trade on output over a twenty-year period), though we keep in mind that this is lower than both our IV and (especially) our steady-state effects.[17] It is important to be conservative since we are acting on the assumption that policy- and naturally-induced trade openness have similar effects on income.[18]

Does Currency Union Affect Income *Other* Than Via Trade?

Thus far we have assumed that currency unions affect income through their effect on openness. But might currency unions have a direct growth effect? In most of the literature on currency unions, the advantage that is emphasized is not the convenience to importers and exporters of abolishing currency distinctions (though Alesina and Barro (2000) provide an elegant model which incorporates this effect). Rather the emphasis is on the credibility benefits derived when the central bank "ties its hands" with a rigid institutional commitment to monetary stability.[19] Many of these models imply that the choice of an anchor currency for a small country to adopt doesn't matter, so long as it is a strong and stable currency (preferably one which experiences similar business cycles). In this view, there is no clear advantage in choosing the currency of a country that is a natural trading partner. In our trade-based approach, on the other hand, it should make a big difference *with which country* one forms a currency union.

We look for possible non-trade effects by including measures of currency union directly in the income equation. We do this in a number of different ways that are designed to isolate the enhancement of trade (or other economic interactions with major partners).

First, we add to the output equation in Table 2 a dummy variable that is unity if the country was a member of a common currency area in 1990, and zero otherwise. The results in Table 2 indicate that the effect of these regimes is *negative*; significantly so when we omit controls, insignificantly different from zero if we include controls. We also add the country's average inflation rate to these regressions, but the results in Table 2 are not substantively changed. (Adding inflation instead of the currency union/board variable leads to similar conclusions.) Apparently currency union in and of itself does not raise income, as one would expect if it improved monetary credibility and stability.

Our hypothesis is that the failure to find evidence that currency unions or boards *per se* have a positive effect on real income stems from the fact that a simple dummy variable does not take account of how many partner countries are in the currency union and how important they are to the domestic country. A currency union with the United States should do more for most countries than a currency union with New Zealand. Thus in Table 3, we include the inner product of bilateral currency union membership with different measures of the importance of the bilateral partners. The importance of the bilateral partners in a currency union can be measured by the key determinants of bilateral trade such as size and proximity.

The results in Table 3 show that a country does not derive an income advantage from belonging to a currency union (or board) *per se*. The coefficient estimate is even negative, and appears statistically different from zero. The emphasis here is on the importance of a country's currency union partners. We begin by entering the inner product of bilateral currency union

membership and the real GDP of the bilateral trading partner. That is, we add $\sum_j CU_{ij}Y_j$ where CU_{ij} is unity if countries i and j were in a currency union or common currency area, and zero otherwise; and Y_j denotes the real GDP of country j. A high value of this inner product indicates that country i is in a currency union with large countries; we expect this to benefit the trade and hence output of country i accordingly.

The inner product does indeed have an economically and statistically significant positive effect on income when we do not include controls. Since our gravity estimates indicate that trade depends not only on partner output, but also on the reciprocal of distance, we also try the aggregate ratio of union partners' output to distance, i.e., $\sum_j CU_{ij}(Y_j / Dist_{ij})$ where $Dist_{ij}$ is the natural logarithm of the distance between countries i and j.[20] Again the coefficient is large, positive and significant in the case without controls. When we include controls, all the inner product coefficients are still positive, although none is significant.

If the currency union dummy had worked positively in the growth equation regardless of the trade-inducing nature of the union partner, it would have suggested that the benefits come through the central bank credibility route. Our evidence instead supports the notion that the currency union effect on income comes through the trade route. Our evidence could be described as mixed, since we find no significant results when we include our controls. Nevertheless, there is little support here for the notion that belonging to a currency union *per se* is good for growth regardless of the partner. Some evidence indicates that it seems to matter whether the currency union includes important trade partners, and none of our evidence is inconsistent with this conclusion.[21]

4. The Effects of Currency Unions on Income

In this section we try to put together the estimates of the two stages – the effect of currency union on trade, and the effect of trade on output – to estimate the effect of currency unions on output.

One way to proceed would be to estimate the effect of currency union on an average country's trade, and the effect of this additional trade on an average country's output. While we pursue this tack in the working paper version and find that the average effect of a currency union on output is about 4%, we do not consider this to be the calculation of greatest interest. The effect of currency union on openness depends on which other countries are in the currency union. The boost to trade (and therefore output) will be stronger if the partner is one with whom one trades, because it is large, nearby, or because of other links (e.g., linguistic or historical). Lithuania will presumably boost its total trade and output more by adopting the euro than by adopting the New Zealand dollar.

Table 4 provides the answers, to two questions of interest: "What is the estimated effect on trade and output of adopting the dollar as the legal currency?" and "What would be the predicted effect of adopting the euro?" We do not distinguish between currency union and a currency board arrangement, since both seem to have similar effects in the data. We exploit our bilateral gravity estimate, which predicts that a currency union or board boosts trade roughly three-fold with other countries that use the currency in question.[22]

The first column in Table 4 reports the country's 1995 openness ratio, trade as a percentage of GDP. The second and third columns show the shares of trade that the country conducted, with the dollar zone (the United States and countries which use the dollar such as Panama) and EMU respectively.[23] The next columns show what the effect would be if the

country in question were to join the dollar or euro zones. In the table, we use our estimate that the formation of a currency union causes trade to triple among the currency union members. The fourth column shows what the overall openness ratio would rise to with dollarization; the fifth is the analogue for euro-adoption. The next two columns report the predicted effect on income per capita. In this case we use our conservative estimate, that the effect on real GDP per capita is one third of a percent for every 1.0 percentage points in openness. Finally, for the countries where the largest trading partner is neither the dollar nor the euro zone, we tabulate the largest trading partner, and the percentage of trade it accounts for. Unweighted averages are provided at the bottom of the table to provide a quick summary. The reader is welcome to substitute his or her own preferred estimate for either the tripling effect that we assume currency union has on trade, or the .33 effect that we assume each percentage point of openness (in terms of GDP) has on real output per capita.

While one should view these estimates as illustrative, they are not without interest. We estimate, for example, that Albania would benefit far more from the trade effects of adopting the euro (an estimated eventual 23 per cent boost to real income per capita) than from "dollarizing" (an estimated 1 per cent boost). Because Albania's natural trading partners are in Europe, a tripling of its trade with the euro block does far more for its overall trade than does "dollarization".

5. Robustness

Canada's proximity and naturally high level of trade with the United States mean that adopting the US dollar would boost trade and output far more for Canada than adopting the euro. Still, for Canada and a number of other countries, the effects estimated in Table 4 seem

implausibly large. Our data set consists primarily of only small and/or poor countries, because these are the only ones that were in currency unions and currency boards (prior to the launch of EMU in 1999). Perhaps our estimates are inapplicable to larger countries?

Intuition tells us that income may depend non-linearly on size and trade. Perhaps a country needs access to a market that is of at least a certain threshold size, after which the benefits of economies of scale are no longer so large. Allowing for such non-linearities might help produce estimates more relevant to larger countries, and would be of intrinsic interest for small territories and countries that contemplate entering or leaving currency unions or political unions.

We have tried a simple test for non-linearity in our estimated relationships: a threshold effect regarding the size of the market. One suspects that such tiny units as Gibraltar, Gaza, and Guam, are not economically viable on their own, and are highly dependent on international trade. If a country makes it past a certain threshold in size, perhaps it is no longer so dependent on trade? However, when we split the sample in half according to the size of the population, we found that openness was large and significant for both large countries and small; indeed the coefficients are larger for larger countries. Also, adding a quadratic term for openness does not remove the finding of a large positive impact of trade on output.

We also examined the trade equation, and found that currency unions were no more beneficial to bilateral trade in very small countries than in somewhat larger ones. Specifically, when we dropped very small countries (e.g., those more than either one or two standard deviations below the average size), the currency union coefficient remained highly significant and positive. The results are also robust to omitting observations where the product of the sizes (defined as either population or GDP) is especially small, or where the difference is especially

large. Thus, we have so far found no evidence that the relationships are sensitive with respect to size.

Dani Rodrik (2000) and Rodriguez and Rodrik (2000) have criticized the methodology we use in linking openness to output. In particular, they argue that our methodology is potentially sensitive to: a) the inclusion of geographic controls (such as distance from the equator, tropical landmass, and continental dummies), b) outliers, and c) institutional characteristics. In appendix 2 below, we show that the vast majority of our results are insensitive to such criticisms.

Summary of Qualifications

While our results appear in many ways quite strong, we must register three important qualifications.

It is natural to suspect that the decision to adopt a common currency is endogenous, in other words, that the observed correlation between currency links and trade links could come about because both are determined by some third factor. In the trade equations of Table 1, we have controlled for factors such as common language, colonial history, political union, and so on, and the currency effect remains almost as strong as ever. Nevertheless, it is possible that an element of endogeneity remains in the currency union/board variable. While we do not believe that endogeneity is driving our results[24], we know of no way to eliminate the possibility that simultaneity bias results in an over-estimate of the effect of currency unions on trade.

Secondly, we have not yet provided any evidence regarding time lags of the effects of currency unions on trade patterns. Thus we do not know how long it may take to attain the large effects that we estimate in cross-section data.[25]

Finally, to repeat, we have found no reason to believe that our results stem solely from the small countries in our sample. Still, if currency unions among large rich countries behave completely differently from unions among small or poor countries, we have no way of knowing it from our data.

6. Conclusions

Scale is important to an economy, whether it is attained by the intrinsic size of the political unit, by political union with a larger country, or by international trade. Currency unions and boards seem to provide a significant stimulus to trade, and thereby to economic performance. But it matters with whom one enters a currency union. The literature on exchange rate regimes with its focus on central bank credibility implies that the crucial requirement for a currency partner is that the currency be stable in value. Our results suggest that the currency should belong to a country or countries that are natural trading partners, by virtue of size, proximity, and/or other linkages.

Using a large data set of economic and geographic variables for over 200 countries and dependencies, we have tried to quantify the implications of currency unions for trade and output using a two-stage approach. Our results at each stage have been significant statistically and economically. Our estimates at the first stage suggest that a currency union more than triples trade with the partners in question. Furthermore, there is no evidence of diversion of trade away from non-members. Thus the currency union boosts total trade. Our estimates at the second stage suggest that every one percent increase in trade (relative to GDP) raises income per capita by at least one third of a percent over a twenty year period, and possibly by much more over the long run. We put the two estimates together to estimate the effect of a currency union on output.

Our results suggest that a country like Poland, which conducts half its trade with the euro zone, could eventually boost income per capita by a fifth by joining EMU. While we are aware that some of our estimates may be implausibly large, we hope they shift the terms of the debate on common currencies towards a more serious consideration of the somewhat neglected trade benefit.

These results are subject to many caveats. We don't yet know how quickly countries reap the trade-boosting effects of currency unions. We can't be sure that the same effects we have estimated for a collection of mostly small and/or poor countries can be extended to large, rich countries (such as those in EMU). Despite our attempts to hold constant for a number of factors, we don't know if the currency union/board variable might still be appropriating some of the influence of cultural or historical links that we have yet to measure.

It is also possible that some of the output effect comes through other geographic interactions that also run along gravity lines. Still, we find it reassuring that the currency union has a positive effect on income when included directly in the income equation, if and only if it is weighted by the importance of trading partners. This suggests that the benefit does not come from monetary stability. And we have found no evidence that currency union *per se* has a positive significant effect on output.

Finally we should make it clear that we have not concerned ourselves with most arguments for or against currency unions – for example that the loss of monetary independence makes it impossible to respond to idiosyncratic shocks. We have simply quantified one potential benefit of currency unions that we consider to have been under-examined in the literature but potentially large.

References

Alesina, Alberto, Enrico Spolaore and Romain Wacziarg, 1999, "Economic Integration and Political Disintegration," Dec. Revised version of NBER Working Paper no. 6163, September 1997. Forthcoming, *American Economic Review*.

Alesina, Alberto and Robert Barro, 2000, "Currency Unions" NBER Working Paper 7927.

Anderson, James E. and Eric van Wincoop (2001) "Gravity with Gravitas: A Solution to the Border Puzzle," NBER Working Paper 8079.

Barro, Robert, 1991, "Economic Growth in a Cross Section of Countries," *Quarterly Journal of Economics*, May.

Barro, Robert, and Xavier Sala-i-Martin, 1992, "Convergence," *Journal of Political Economy.*

Dollar, David, 1992, "Outward-oriented Developing Economies Really Do Grow More Rapidly: Evidence from 95 LDCs: 1976-1985," *Economic Development and Cultural Change* 40, 523-544.

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,* W. Haraf and T. Willett, eds., American Enterprise Institute, Washington, D.C..

Edwards, Sebastian, 1993a, "Openness, Trade Liberalization and Growth in Developing Countries," *Journal of Economic Literature*, Sept. 31, no. 3, 1358-93.

Edwards, Sebastian, 1993b, "Trade Policy, Exchange Rates, and Growth," in R. Dornbusch and S. Edwards, eds., *Reform, Recovery and Growth: Latin America and the Middle East*, Chicago, University of Chicago Press, 13-49, 1995.

Fischer, Stanley, 1991, "Growth, Macroeconomics and Development," on O. Blanchard and S. Fischer, eds., *NBER Macroeconomics Annual 1991*, MIT Press, 329-64.

Fischer, Stanley, 1993, "The Role of Macroeconomic Factors in Growth," *Journal of Monetary Economics*, Jan. 32, no. 3, 485-512.

Frankel, Jeffrey, 1997, *Regional Trading Blocs*, Institute for International Economics, Washington DC.

Frankel, Jeffrey, and David Romer, 1999, "Does Trade Cause Growth?" *American Economic Review* 89, no. 3, June 1999, 379-399.

Frankel, Jeffrey, and Shang-Jin Wei, 1995, "Emerging Currency Blocs," in *The International Monetary System: Its Institutions and its Future*, edited by Hans Genberg, Springer, Berlin, 111-143.

Ghosh, Atish, Ann-Marie Gulde, and Holger Wolf, 1997, " Currency Boards - The Better Peg?", IMF Working Paper.

Grossman, Gene, and Elhanan Helpman, 1991a, *Innovation and Growth in the Global Economy*, MIT Press: Cambridge.

Grossman, Gene, and Elhanan Helpman, 1991b, "Trade, Knowledge Spillovers, and Growth," *European Economic Review* 35, no. 2-3, April, 517-526.

Hall, Robert and Charles Jones, 1999, "Why Do Some Countries Produce So Much More Output per Worker than Others?" *Quarterly Journal of Economics*, Feb., 114, no.1, 83-116.

Helpman, Elhanan. 1988, "Growth, Technological Progress, and Trade," *National Bureau of Economic Research* Reprint no. 1145.

Helpman, Elhanan and Paul Krugman. 1985. *Market Structure and Foreign Trade*, Cambridge, MIT Press.

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

Irwin, Douglas, and Marko Tervio, 2000, "Does Trade Raise Income? Evidence from the Twentieth Century," NBER Working Paper 7745.

Mankiw, N. Gregory, David Romer and David Weil, 1992, "A Contribution to the Empirics of Economic Growth," *Quarterly Journal of Economics* 107, 407-437.

McCallum, John, 1995, "National Borders Matter: Canada-U.S. Regional Trade Patterns," *American Economic Review* 85, no.3, June, 615-623.

Rodríguez, Francisco, and Dani Rodrik (2000) "Trade Policy and Economic Growth: A Skeptic's Guide to the Cross-National Evidence" forthcoming *NBER Macroeconomics Annual*.

Rodrik, Dani, 1994, "Getting Interventions Right: How South Korea and Taiwan Grew Rich," *20th Panel Meeting of Economic Policy*; NBER Working Paper No. 4964, December.

Rodrik, Dani (2000) "Comment on Frankel and Rose" unpublished.

Romer, Paul (1989) "What Determines the Rate of Growth and Technical Change?" *World Bank Working Paper* No. 279.

Rose, Andrew (2000) "One Money, One Market? The Effect of Common Currencies on International Trade," *Economic Policy*.

Rose, Andrew and Eric van Wincoop (2001) “National Money as a Barrier to Trade: The Real Case for Currency Union” forthcoming.

Sala-i-Martin, Xavier, 1991, "Comment," *NBER Macroeconomics Annual* 6, 368-378.

Summers, Robert, and Alan Heston, 1988, "A New Set of International Comparison of Real Product and Price Levels: Estimates for 130 countries, 1950-85," *Review of Income and Wealth* 34, March, 1-26.

Table 1: The Effect of Currency Unions and Boards on Trade in the Gravity Model

Currency Union	2.11 (.19)	1.78 (.18)	1.38 (.19)		
Currency Board	2.08 (.52)	1.45 (.32)	.93 (.29)		
Currency Union or Board				1.36 (.18)	1.55 (.18)
Log Distance	-1.22 (.02)	-1.11 (.03)	-1.06 (.03)	-1.06 (.03)	-1.08 (.03)
Log Product Real GDP	.78 (.01)	.95 (.01)	.94 (.01)	.94 (.01)	.96 (.01)
Log Product Real GDP/capita	.66 (.02)	.47 (.02)	.48 (.02)	.48 (.02)	.45 (.02)
Common Land Border		.61 (.13)	.63 (.12)	.63 (.12)	.63 (.13)
Number land-locked (0, 1 or 2)		-.36 (.04)	-.32 (.04)	-.32 (.04)	-.30 (.04)
Log of Product of Land Area		-.17 (.01)	-.15 (.01)	-.15 (.01)	-.15 (.01)
Common Language		.83 (.06)	.56 (.06)	.56 (.06)	.54 (.06)
Common Colonizer			.40 (.08)	.40 (.08)	.36 (.08)
Ex-Colony/Colonizer			1.95 (.13)	1.95 (.13)	1.77 (.13)
Political Union			.96 (.37)	.97 (.36)	1.05 (.37)
Common FTA			1.07 (.10)	1.07 (.10)	1.06 (.10)
CU or CB/Non-CU and Non-CB					.34 (.04)
R^2	.61	.63	.64	.64	.64
RMSE	2.05	2.00	1.97	1.97	1.97

Regressand is log of bilateral trade in real American dollars.
Number of Observations = 31,226.
Year-specific fixed effects not reported.
Robust standard errors recorded in parentheses.

Table 2: The Effect of Openness on GDP/capita

	OLS	IV	IV	IV	IV	OLS	IV	IV	IV	IV
Openness	.79 (.18)	1.61 (.52)	1.96 (.61)	1.59 (.48)	1.70 (.89)	.33 (.07)	.43 (.10)	.27 (.11)	.43 (.10)	.35 (.13)
Log Population	.14 (.06)	.23 (.08)	.18 (.11)	.18 (.08)	.19 (.12)	.07 (.02)	.08 (.02)	.10 (.03)	.08 (.02)	.07 (.03)
Log Area			.11 (.10)					-.05 (.03)		
Currency Union or Board				-.86 (.22)	-.76 (.29)				-.00 (.09)	-.05 (.11)
Mean Inflation*					-.02 (.05)					-.03 (.02)
Log '70 Real GDP/capita						.71 (.05)	.73 (.06)	.74 (.05)	.73 (.06)	.73 (.06)
Investment Ratio						.016 (.006)	.013 (.006)	.017 (.006)	.013 (.006)	.016 (.007)
Population Growth Rate						-.06 (.05)	-.05 (.06)	-.04 (.06)	-.05 (.06)	-.03 (.06)
Primary Schooling						.002 (.002)	.001 (.002)	.002 (.002)	.001 (.002)	.002 (.002)
Secondary Schooling						.007 (.002)	.008 (.003)	.006 (.002)	.007 (.003)	.007 (.003)
Number of Observations	115	110	109	110	100	106	102	102	102	96
R^2	.11			.07		.94	.94	.94	.94	.94
RMSE	1.02	1.08	1.14	1.04	1.11	.28	.28	.28	.28	.28

Regressand is log of Real GDP/capita in 1990, PWT.
Intercepts not reported. Robust standard errors recorded in parentheses.
* Coefficients and standard errors for mean inflation multiplied by 100.

Instrumental Variable (First Stage) Generation

$$(Trade_{ij}/GDP_i) = \underset{(.05)}{-.94}\log(distance_{ij}) + \underset{(.02)}{.82}\log(pop_j) + \underset{(.11)}{.53}\,ComLang_{ij}$$

$$+ \underset{(.21)}{.64}\,ComBorder_{ij} - \underset{(.01)}{.27}\log(Area_i Area_j) - \underset{(.08)}{.47}\,\#Landlocked_{ij}$$

Equation estimated for 1990. R^2 = .28; Number of Observations = 4052.
Robust standard errors in parentheses; intercept not recorded.

Correlation between trade ratio and generated IV = .72

Table 3: The Effect of Currency Unions on GDP/capita

Controls?	**No**	**No**	**No**	**Yes**	**Yes**	**Yes**
Currency Union Or Board	-.79 (.27)	-1.35 (.21)	-1.23 (.24)	.03 (.11)	-.07 (.10)	.01 (.12)
Inner Product of Currency Union/Board and Real GDP*		1.2 (.35)			.25 (.27)	
Inner Product of Currency Union/Board and (Real GDP/Distance)**			2.3 (.88)			.01 (.3)
Test for Joint Significance of both CU/CB terms (p-value)		.00	.00		.50	.99
Number of Observations	115	108	108	106	102	102
R^2	.07	.18	.15	.92	.92	.92
RMSE	1.04	.99	1.01	.32	.32	.33

Regressand is log of Real GDP/capita in 1990, PWT.
OLS. Controls, and intercepts not reported. Robust standard errors recorded in parentheses.
* Coefficient and standard error multiplied by e^{10}
** Coefficient and standard error multiplied by e^{7}

Table 4: Estimated Effects of "Dollarization" and Euro-adoption on Trade and Output

	I --- Actual Data --------- I			--- Estimated Impacts ---------- I				
		Trade with:		Trade Effect		GDP Effect		
	Openness	$ zone	€ zone	Adopt $	Adopt €	Adopt $	Adopt €	Largest non-$/€
	% GDP	%	%	% GDP	% GDP	% GDP	% GDP	Partner (% trd)
Albania	47	3	75	50	118	1	23	
Algeria	59	13	62	74	132	5	24	
Angola	110	50	34	220	185	36	25	
Australia	42	14	12	54	52	4	3	Japan (19)
Bangladesh	37	20	21	52	53	5	5	
Belize	103	44	8	194	119	30	5	
Brazil	15	23	24	22	22	2	2	
Burundi	33	7	75	38	83	2	16	
Cambodia	80	2	8	83	93	1	4	Singapore (33)
Canada	73	76	5	184	80	37	2	
Chile	55	21	17	78	74	8	6	
China	40	13	11	50	49	3	3	Hong Kong (30)
Colombia	36	38	18	63	49	9	4	
Congo Dem. Rep.	59	13	57	74	126	5	22	
Costa Rica	86	53	17	177	115	30	10	
Denmark	64	4	49	69	127	2	21	
Djibouti	99	3	38	105	174	2	25	
Dominican Rep.	63	76	7	159	72	32	3	
Ecuador	58	45	16	110	77	17	6	
Egypt	53	18	38	72	93	6	13	
El Salvador	59	50	14	118	76	19	5	
Eq'l Guinea	154	21	45	219	293	21	46	
Ethiopia	38	12	53	47	78	3	13	
Fiji	115	10	4	138	124	8	3	Australia (34)
Gambia The	132	2	51	137	267	2	44	
Ghana	60	12	40	74	108	5	16	
Guatemala	45	44	10	85	54	13	3	
Guinea	48	15	56	62	102	5	18	
Guyana	211	28	12	329	262	39	17	
Haiti	36	67	13	84	45	16	3	
Honduras	91	52	17	186	122	31	10	
Hungary	76	4	71	82	184	2	36	
Iceland	67	11	34	82	113	5	15	
India	28	17	29	38	44	3	5	
Indonesia	52	13	15	66	68	4	5	Japan (25)
Iran	36	1	40	37	65	0	10	
Israel	69	25	38	104	121	11	17	
Jamaica	136	53	9	280	160	48	8	
Japan	17	27	12	26	21	3	1	
Jordan	126	7	24	144	186	6	20	
Kenya	73	6	35	82	124	3	17	
Korea Rep.	67	22	11	96	82	10	5	
Kuwait	104	19	24	144	154	13	16	
Lao PDR	61	2	16	63	81	1	6	Thailand (53)
Lebanon	77	12	51	95	156	6	26	
Madagascar	55	4	55	59	116	1	20	
Malawi	73	8	26	85	111	4	13	South Africa (31)
Malaysia	194	17	10	260	233	22	13	Singapore (24)
Mauritania	112	5	60	123	246	4	44	
Mauritius	122	1	36	124	210	1	29	
Mexico	59	79	6	152	66	31	2	
Mongolia	106	8	17	123	142	6	12	China (30)
Morocco	61	5	59	67	133	2	24	

Mozambique	88	7	22	100	127	4	13	South Africa (47)
Myanmar	3	3	5	3	3	0	0	Singapore (24)
Nepal	59	12	22	73	85	5	9	
New Zealand	59	13	11	74	72	5	4	Australia (23)
Nicaragua	91	38	16	160	120	23	10	
Nigeria	30	35	34	51	50	7	7	
Norway	70	6	43	78	130	3	20	
Pakistan	36	12	20	45	50	3	5	
Pap. N. Guinea	103	3	11	109	126	2	7	Australia (36)
Paraguay	48	19	8	66	56	6	3	Brazil (32)
Peru	28	24	18	41	38	4	3	
Philippines	81	24	10	120	97	13	5	
Poland	50	3	60	53	110	1	20	
Romania	59	4	62	64	132	2	24	
Rwanda	36	23	50	53	72	5	12	
Saudi Arabia	72	20	21	101	102	10	10	
Seychelles	104	0	16	104	137	0	11	Yemen (20)
Sierra Leone	39	14	54	50	81	4	14	
Singapore	356	16	10	470	427	38	23	
South Africa	50	10	29	60	79	3	10	
Sri Lanka	82	17	16	110	108	9	9	
Sweden	76	8	48	88	149	4	24	
Switzerland	66	8	61	77	147	3	27	
Tanzania	63	6	26	71	96	2	11	
Thailand	90	14	13	115	113	8	8	Japan (24)
Trin. & Tob.	97	42	9	178	114	27	6	
Tunisia	93	3	75	99	233	2	46	
Turkey	44	9	46	52	84	3	13	
Uganda	32	4	58	35	69	1	12	
United Kingdom	58	12	53	72	119	5	20	
Uruguay	38	9	18	45	52	2	5	Brazil (29)
Venezuela	48	50	10	96	58	16	3	
Vietnam	83	4	16	90	110	2	9	Japan (19)
Yemen Rep.	73	7	13	83	92	3	6	Korea (14)
Zambia	91	5	17	100	122	3	10	South Africa (22)
Zimbabwe	91	4	21	98	129	2	13	South Africa (36)
Average	74	19	29	101	114	9	13	

1995 trade data. Assumptions: currency union triples trade; .33 effect of openness on GDP.

Appendix 1: A Description of the Data Sets

We employ two data sets in this paper. The first is used to estimate the bilateral gravity models of trade, and thereby to model the effect of currency union on trade. The second is used to estimate the impact of trade on output.

The first (trade) data set consists of 41,678 bilateral trade observations spanning six different years (1970, 1975, 1980, 1985, 1990, and 1995). We are missing observations for some of the regressors so the usable sample is smaller for most purposes. All 186 countries, dependencies, territories, overseas departments, colonies, and so forth for which the United Nations Statistical Office collects international trade data are included in the data set. For convenience, we refer to all of these geographical units as "countries." The trade data are taken from the *World Trade Database*, a consistent recompilation of the UN trade data presented in Feenstra, Lipsey and Bowen (1997), augmented with data from the UN's *International Trade Statistics Yearbook*. This data set is estimated to cover at least 98% of all trade. The nominal trade values (recorded in thousands of American dollars) have been deflated by the American GDP chain price index. In this data set, there are 406 country-pair observations where there is trade between two members of a currency union, and 20 observations where one country uses the currency of another in a currency board arrangement. The currency unions and boards are tabulated below.

Table A1: Currency Unions in the Bilateral Trade Data Set

Australia
Kiribati
Nauru
Tuvalu

Denmark
Faroe Islands (part of Denmark)
Greenland (part of Denmark)

ECCA
Anguilla (territory of UK)
Antigua and Barbuda
Dominica
Grenada
Montserrat (territory of UK)
St. Kitts and Nevis
St. Lucia
St. Vincent and the Grenadines

France
French Guiana (overseas department)
French Polynesia (overseas territory)
Guadeloupe (OD)
Martinique (OD)
Mayotte (territorial collectivity)
New Caledonia (OT)
Reunion (OD)
Saint Pierre and Miquelon (TC)

New Zealand
Cook Islands (self-governing)
Niue (self-governing)

CFA
Benin
Burkina Faso
Cameroon
Central African Republic
Chad
Comoros
(Republic of) Congo
Cote d'Ivoire
Gabon
Guinea-Bissau
Mali (post '84)
Niger
Senegal
Togo

UK
Falkland Islands (territory)
Gibraltar (territory)
Saint Helena (territory)
Ireland (pre '79)

USA
US Virgin Islands (territory)
British Virgin Islands (territory of UK)
Turks & Caicos Isl. (territory of UK)
Bahamas
Bermuda (colony of UK)
Liberia
Panama

Currency Boards in the Bilateral Trade Data Set

UK	US
Bahrain (pre '74)	Argentina (post '90)
Fiji (pre '76)	Cayman Islands (post '71)
Gambia (pre '72)	Djibouti
Oman (pre '75)	Hong Kong (pre '75, post '82)
Qatar (pre '74)	
Yemen (pre '72)	

We use the *Penn World Table* (PWT) 5.6 for population and real GDP per capita data, filled in with data from the World Bank *World Development Indicators* (taken from the 1998 WDI CD-ROM) where the former is missing (e.g., for 1995, where the Penn World Table data set is unavailable). For location (used to calculate Great Circle distance and contiguity), official language, colonial background, and other such information, we use information taken from the CIA's web site. A number of regional free trade agreements are included in the FTA dummy: the EEC/EC; the Canada-US FTA; EFTA; the Australia/New Zealand closer economic relationship; the Israeli/US FTA; ASEAN; CACM; PATCRA; CARICOM; SPARTECA; and the Cartagena Agreement, using information at the WTO's web site.

The second macroeconomic data set consists of annual observations for 210 "countries" between 1960 and 1996 extracted from the 1998 World Bank *World Development Indicators* (WDI) CD-ROM, merged with data from the Penn World Table (PWT) Mark 5.6. For most purposes, the sample starts in 1970 and ends in 1990 or 1992. A maximum of 7,803 observations is available (not all countries exist for the entire data sample). For both the WDI and the PWT data sets, we use all available observations (by which we mean the comprehensive set of years, countries, territories, colonies and other entities covered). There are numerous missing observations for variables of interest. The data set has been checked and corrected for mistakes. In this data set, there are 1,891 observations for countries that were members of a currency union.

Appendix 2: A Response to Rodrik's Geography Critique

The idea of using instrumental variables from the gravity model to isolate the effect of openness on growth (Frankel and Romer, 1999) has generally been well received. Nevertheless, the approach has been criticized. In their wide-ranging critique of the empirical literature on trade and growth, Rodriguez and Rodrik (2000) argue that the geographically constructed instrumental variable in Frankel and Romer (1999) or Irwin and Tervio (2000) might be incorrectly appropriating the direct influences of geography on income, such as the harmful effect of tropical diseases. They argue that the results of Frankel and Romer change if the output equation controls for any one of three geographical variables: (1) distance from the equator (which Hall and Jones (1999) suggest belongs in the equation), (2) percentage of land area inside the tropics (proposed by Radelet, et al. 1997), and (3) dummy variables for the three historically poor continents. Clearly this critique must be taken seriously insofar as it affects our work just as seriously as that of Frankel and Romer.

We implement these suggestions in Table A2. Distance from the equator appears with a positive coefficient, as expected, though it is only significant when we do not include extra income controls. The tropical variable has a negative and significant coefficient, as expected. Dummy variables for Latin America, East Asia, and sub-Saharan Africa appear significantly in our equation without controls. Thus, in some sense, the intuition of Rodriguez and Rodrik is confirmed. Still, the key question concerns the implications of these controls for the openness variable. *In every case*, regardless whether the other controls are included or not, *the openness variable retains most of its magnitude and all of its statistical significance* in the presence of each of the three Rodriguez-Rodrik modifications. The t-statistics are 3 to 4.

Rodrik (2000), a comment on an earlier draft of this paper, takes up the trail of criticism. He controls for size, and finds our significant positive effect of openness on income. But he then proposes deleting Hong Kong and Singapore from the sample, and including an additional explanatory variable, "institutions". Again, he claims that these adjustments will cause the significant positive effect of openness to go away.

Excluding a few countries that are outliers in terms of openness does not have a firm rationale, but is nevertheless a useful check for robustness and one we pursued in our original paper. Below we follow Rodrik in excluding just Hong Kong and Singapore, as reported in Table A3. The coefficient on openness remains significant – and in fact turns out to be higher in estimated magnitude – when the city-states are excluded. This is true regardless of whether the equation is estimated by OLS or IV, and whether or not initial income and the other controls are included.

Rodrik's argument for including a measure of the quality of institutions is that these are independent determinants of productivity and that openness may be spuriously appropriating their effect. We implement his suggestion in Table A4 by including measures of corruption, law and order, and bureaucracy for 1990 taken from *International Country Risk Guide* (which Rodrik kindly gave us). The three different measures are scaled from 1 (worst) through 6 (best); we have also followed Rodrik in calculating a rescaled average of the measures. The measures are statistically significant when included in the income equation without initial income and the other controls, and are especially so when entered in the form of a rescaled combined single variable. None are significant when included along side initial income and the other factor accumulation variables. However, the key question is the implication for the openness coefficient, which remains positive with a t-statistic around 3, under each of these specifications.

Finally, Rodrik has suggested including at the same time: distance from equator, selected continental dummies; and institutional measures. We pursue this set of combinations in Table A5. While the openness coefficient remains positive, its size and statistical significance is somewhat diminished, particularly in the case when controls are omitted. This debate will undoubtedly continue. No doubt if one throws enough variables into the equation, at some point the openness effect will indeed be impacted severely enough to disappear . But our sensitivity analysis currently indicates that it is relatively robust.

Table A2: Sensitivity Analysis: The Role of Geography

Controls?	No	No	No	No	Yes	Yes	Yes	Yes
Openness	1.61 (.52)	1.28 (.27)	1.13 (.22)	1.23 (.33)	.43 (.10)	.43 (.10)	.45 (.10)	.36 (.12)
Log Distance from Equator		.58 (.09)				.01 (.04)		
Tropical Dummy			-1.62 (.15)				-.18 (.09)	
Latin Dummy				-.50 (.20)				-.15 (.10)
East Asian Dummy				-1.14 (.30)				.08 (.19)
Sub-Saharan Dummy				-1.60 (.19)				-.18 (.11)
Number of Observations	110	110	106	110	102	102	101	102
R^2		.35	.55	.49	.94	.94	.94	.94
RMSE	1.08	.88	.75	.78	.28	.28	.28	.28

IV estimation.
Regressand is log of Real GDP/capita in 1990, PWT.
Intercepts not reported. Robust standard errors recorded in parentheses.

Table A3: Sensitivity Analysis: The Role of Outliers

		Drop S, HK		Drop S, HK		Drop S, HK		Drop S, HK
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Controls?	No	No	No	No	Yes	Yes	Yes	Yes
Openness	.79 (.18)	1.04 (.31)	1.61 (.52)	4.1 (1.1)	.33 (.07)	.34 (.11)	.43 (.10)	.53 (.28)
Number of Observations	115	113	110	108	106	104	102	100
R^2	.11	.08			.94	.94	.94	.94
RMSE	1.02	1.02	1.08	1.30	.28	.28	.28	.30

Regressand is log of Real GDP/capita in 1990, PWT.
Intercepts not reported. Robust standard errors recorded in parentheses.
Sample is same as Table 2 but drops Singapore and Hong Kong in selected columns.

Table A4: Sensitivity Analysis: The Role of Institutions

Controls?	No	No	No	Yes	Yes	Yes
Openness	1.61 (.52)	.69 (.24)	.68 (.23)	.43 (.10)	.39 (.12)	.38 (.10)
Corruption (1/6)		.10 (.09)			-.06 (.04)	
Law And Order (1/6)		.18 (.07)			.02 (.04)	
Bureaucracy (1/6)		.23 (.08)			.07 (.03)	
Rescaled Combined Institutions (0/1)			3.11 (.23)			.22 (.23)
Number of Observations	110	91	91	102	89	89
R^2		.56	.56	.94	.94	.94
RMSE	1.08	.72	.71	.28	.27	.28

IV estimation.
Regressand is log of Real GDP/capita in 1990, PWT.
Intercepts not reported. Robust standard errors recorded in parentheses.

Table A5: Sensitivity Analysis: Combinations

Controls?	No	No	Yes	Yes
Openness	1.61 (.52)	.27 (.17)	.43 (.11)	.22 (.11)
Rescaled Combined Institutions (0/1)		2.57 (.20)		.56 (.22)
Log Distance from Equator		-.02 (.09)		-.02 (.04)
Latin Dummy		-.17 (.17)		-.23 (.10)
East Asian Dummy		-.20 (.30)		.12 (.17)
Sub-Saharan Dummy		-1.37 (.16)		-.49 (.11)
Number of Observations	110	91	102	89
R^2		.84	.94	.95
RMSE	1.08	.45	.28	.25

IV estimation.
Regressand is log of Real GDP/capita in 1990, PWT.
Intercepts not reported. Robust standard errors recorded in parentheses.

Endnotes

1 For brevity, we sometimes refer to "common currencies" or "currency unions" below, instead of "currency unions or boards."
2 A survey of the literature is available in Edison and Melvin (1990).
3 Frankel and Wei (1995).
4 Our focus is on the currency union effect, in part because there were so few currency board arrangements during the bilateral trade sample. In 1990, only the Cayman Islands, Djibouti and Hong Kong used currency boards.
5 But see Anderson and van Wincoop (2001), who provide a critique.
6 We can reproduce McCallum's estimate in our data set by dropping from the gravity equation all variables that tend to go with nationhood -- currency, language, trade policy -- leaving only the political union variable. When we do so, the home bias effect is estimated at McCallum's level of twenty (the estimated coefficient on political union is 3, and exp(3.0)=20).
7 E.g., Grossman and Helpman (1991a, 1991b), Helpman (1988), and Helpman and Krugman (1985).
8 Among the many examples are: Romer (1989), Dollar (1992), and Edwards (1993a). Edwards (1993b) and Rodrik (1993) survey the literature.
9 For example, Edwards (1993, pp.9-11) regresses the rate of growth of total factor productivity on two measures of openness (total trade as a percent of GDP, and total tariff revenue as a percentage of trade) along with some other variables, and finds that "in every regression the proxies for trade distortions and openness are highly significant."
10 E.g., Fischer (1991, 1993), Dollar (1992), Easterly (1993), Edwards (1993), Sachs and Warner (1995) and Harrison (1996). Rodriguez and Rodrik (2000) offer a critique of much of this work and conclude that the issue remains unsettled.
11 From the viewpoint of a small individual country, the outputs of its trading partners are exogenous as well. For a study like this that seeks to explain output for a cross-section of countries, one does not wish to treat the output of trading partners as exogenous, even if the domestic country is small.
12 Frankel and Romer use samples ranging from 98 to 150 countries and find that the coefficient on openness in an output equation goes from 0.8 in an OLS regression to over 2.0 with instrumental variable estimates, and remains statistically significant.
13 Barro (1991), Barro and Sala-i-Martin (1992), and Mankiw, Romer, and Weil (1992).
14 Population enters with a coefficient of around 0.1. This suggests that the smallest country in the sample derives a per capita income disadvantage relative to the mean country of around 49 per cent [=0.1(8.6-3.7)]. Size matters.
15 Since we only generate and use a single instrument, we have no over-identifying restrictions to test. Nevertheless, we can "disaggregate" our instrument into its underlying gravity components. The first stage, which we use to generate our instrument, comprises six underlying variables: distance, population, common language dummy, common land border, area, and landlocked status. We used each of these, one by one, to generate an instrument from a single gravity determinant, resulting in six "generated instruments". Then we conducted our exclusion tests by excluding from the income equation (and testing for the significance of) the difference between this generated instrument and our baseline instrument of Table 2; this variable measures the independent contribution from the gravity determinant. We use the other five generated instruments to then instrument for openness. Using this methodology delivers twelve exclusion tests: two for each of the six gravity determinants, depending on whether we include or exclude controls from the income equation. With the exception of population in the version without controls, the t-statistics are insignificantly different from zero at conventional significance levels, suggesting that the contribution of the gravity determinants to the instrument can in fact be excluded from the output equation. To summarize succinctly, the implicit over-identifying restrictions do not seem grossly violated. We thank an anonymous referee for this suggestion.
16 Dis-aggregating the first stage of our procedure (which generates the instrument from the gravity equation) allows us to test and corroborate the hypothesis that the instrumental variables can be excluded from the income equation at reasonable significance levels. Further details are available upon request.
17 This estimate is close to that of Frankel and Romer (1999).
18 When we include both actual and gravity-predicted trade in our output equation, we find no evidence inconsistent with the hypothesis that policy-induced openness has a positive effect on income similar to that of gravity-predicted openness. Details are available upon request.

19 Ghosh, Gulde and Wolf (1997) find that currency board countries on average have higher growth than other countries: a difference of 1.8 percent per annum, whether in an equation that conditions on such other variables as initial income, investment, and human capital, or in unconditional averages.

20 More exactly, we use the average of this variable between 1970 and 1990.

21 Our estimates of these effects are probably biased downwards, since we have no data on many small rich members of currency unions (e.g., Brunei, Luxembourg, Liechtenstein, Monaco), for which no trade data exist. We also note in passing that our framework does not rule out the idea that other economic interactions along geographic lines, such as investment, communication, and movement of people, could constitute part of the growth effect, rather than exclusively trade.

22 We exclude current currency union members from the table, except for the recent dollarizers.

23 This paper was written before Greece was admitted to EMU, so we use the eleven original members of EMU.

24 Historical and instrumental variable evidence provided in Rose (2000) suggests that endogeneity of the currency decision may not be an important issue in practice.

25 Historical evidence regarding the formation and dissolution of federations might offer a clue regarding lags. Within five years of the re-unification of East and West Germany in 1989, intra-German trade concentration increased four-fold. Similarly, after the break-ups of the Austro-Hungarian Empire, the Federation of Malaya, the Soviet Union, and Czechoslovakia, trade patterns among the constituent parts in each case shifted away from one another within a few years. In each case, the introduction of different currencies may explain part of the effect, though it is conflated by other events. In each case the impact after five years seems to have been far less than a twenty-fold home bias effect.