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The endogeneity of the optimum currency-area criteria

Jeffrey A. Frankel* and Andrew K. Rose **

Summary

■ A country's suitability for entry into a currency union depends on several economic conditions. These include, *inter alia*, the intensity of trade with other potential members of the currency union, and the extent to which domestic business cycles are correlated with those of the other countries. But international trade patterns and international business-cycle correlations are endogenous. This paper develops and investigates the relationship between the two phenomena. Using 30 years of data for 20 industrialized countries, we uncover a strong and striking empirical finding: countries with closer trade links tend to have more tightly correlated business cycles.■

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The endogeneity of the optimum currency-area criteria

Jeffrey A. Frankel and Andrew K. Rose*

Countries considering whether to enter the proposed European Economic and Monetary Union (EMU) are currently weighing the potential benefits of joining the currency union against the inevitable costs. Joining a currency union brings benefits, such as fewer of the transactions costs associated with trading goods and services among countries with different monies. Countries with close international trade links would potentially benefit greatly from a common currency and are more likely to be members of an *optimum currency area* (OCA). So the nature and extent of international trade is one criterion for EMU entry or more generally, membership in an OCA.

But joining the EMU brings costs. One cited frequently is foregoing the possibility of dampening business-cycle fluctuations through counter-cyclic monetary policy.[1] Countries with idiosyncratic business cycles give up a potentially important stabilizing tool if they join a currency union. So another criterion for EMU entry is the nature and extent of domestic business cycles; *symmetric* countries are more likely to be members of an OCA. Countries with tight international trade ties and symmetric business cycles are more likely to join and gain from the EMU, *ceteris paribus*. This paper analyzes these issues.

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[1] Such costs may be low if labor is mobile among the members of the currency union, or if an effective international or intertemporal system of fiscal transfers exists. We do not focus explicitly on these issues below.

These topics have been closely studied by economists. Estimates of the transactions costs that might be saved by the EMU were summarized by the European Commission (1990). Several economists, including Bayoumi and Eichengreen (1993a, 1993b, 1994, 1996), analyzed the business cycles and shocks affecting different potential EMU members—to be able to quantify the potential importance of national monetary policy. This paper intends to link the two issues to make a simple point. We argue that a naïve examination of historical data gives a misleading picture of a country's suitability for entry into a currency union because both criteria are *endogenous*.[2]

Entry into a currency union may significantly raise international trade linkages and thus the benefits foregone by not joining a currency union. More importantly, tighter international trade ties may significantly affect the nature of national business cycles. Countries that enter a currency union are likely to experience dramatically different business cycles. In part, this will necessarily reflect changes in monetary policy; but it may be partly a result of closer international trade with other members of the union. From a theoretical viewpoint, closer international trade could result in either tighter or looser correlations of national business cycles. Cycles could, in principle, become more idiosyncratic; closer trade ties could result in countries becoming more specialized in the goods in which they have comparative advantage. The countries might then be more sensitive to industry-specific shocks, which result in more idiosyncratic business cycles. But if shocks that are common across countries dominate—or intra-industry trade accounts for most trade—then business cycles may become *more* similar across countries when countries trade more. We believe the latter case to be more realistic, but consider the question to be open.

We test our view empirically, using a panel of bilateral trade and business-cycle data that span 20 industrialized countries during 30 years. The empirical results are strong and clear cut. They indicate that closer international trade links result in more closely correlated business cycles across countries.

In Section 1, we provide a theoretical framework for our analysis, which draws heavily on the large literature on optimum currency ar-

[2] The European Commission (1990) recognized both of these phenomena For example, on p. 11 it states "...Elimination of exchange-rate uncertainty and transactions costs ... are sure to yield gains in efficiency ... EMU will reduce the incidence of country-specific shocks."

eas (OCAs). Then we discuss the literature briefly and present our empirical methodology and dataset. Section 3 contains our actual empirical results and Section 5 contains a brief conclusion.

1. Theoretical framework

Since Mundell (1961) first developed the concept of an optimum currency area, a vast amount of literature has developed in the area. This literature includes classic contributions by McKinnon (1963) and Kenen (1969); recent surveys are available in Tavlas (1992) and Bayoumi and Eichengreen (1996). Much of this literature focuses on four interrelationships among the members of a potential OCA. They are the:

- Extent of trade
- Similarity of the shocks and cycles
- Degree of labor mobility
- System of fiscal transfers and risk-sharing

The greater the linkages among the countries using any of the four criteria, the more suitable a common currency.

Given the theoretical consensus in the area, it is natural that the OCA criteria have been applied extensively. For example, when most researchers judge the suitability of different European countries for the EMU, they examine the four criteria (or some subset) using European data, frequently using the U.S. as a benchmark for comparison.

This paper argues that such a procedure may be untenable, because the OCA criteria are *jointly endogenous*. For example, the suitability of European countries for the EMU cannot be judged on the basis of historical data because the structure of these economies is likely to change if the EMU exists. As such, this paper is simply an application of the well-known *Lucas Critique* (Lucas, 1976). Without denying the importance of the latter criteria, we focus on the first two OCA criteria.

1.1. The OCA paradigm

Countries that are highly integrated with each other, regarding international trade in goods and services, are more likely to constitute an optimum currency area. Openness is one criterion for membership in an OCA because greater trade leads to greater savings in the transactions costs and risks associated with different currencies. And the high marginal propensity to import associated with an open economy reduces output variability and the need for domestic monetary policy, because openness acts as an automatic stabilizer.

Of course, openness is not the only criterion for membership in a common currency area. Ever since Mundell (1961), it has been appreciated that the more highly correlated the business cycles are across member countries, the more appropriate a common currency. Only countries whose business cycles are imperfectly synchronized with others' could benefit from the potential stabilization afforded by a national monetary policy.[3]

In Figure 1, we graph the extent of trade among members of a potential common currency area, against the correlation of their incomes. The OCA line slopes downward: the advantages of adopting a common currency depend positively on trade integration and the degree to which business cycles are correlated internationally. Points high up and to the right represent groupings of countries that should share a common currency; those down and to the left represent countries that should float individually.[4]

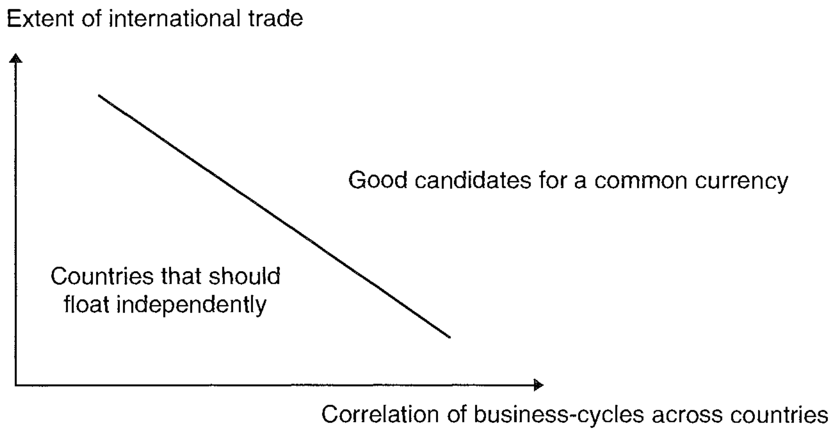
Can the degree of integration among potential members of a common currency area be considered independently of income correlation? Surely not, because *the correlation of business cycles across countries depends on trade integration.*

Our hypothesis is that this relationship is positive: the more one country trades with others, the more highly correlated will be their business cycles. This is certainly the relationship pictured by the European Commission (1990). But it is not universally accepted. Authors such as Eichengreen (1992), Kenen (1969), and Krugman (1993) have pointed out that as trade becomes more highly integrated, countries specialize more in production. The increased degree of specialization may be expected to *reduce* the international correlation of incomes, given sufficiently large supply shocks.

[3] We assume that monetary policy cannot permanently affect either a country's real income level or growth rate; hence our focus on business cycles.

[4] Of course, a high degree of labor mobility among the member countries and/or the existence of a system of fiscal transfers also make monetary independence less necessary.

Figure 1. The extent of trade among members of a potential common currency area, against the correlation of their incomes



1.2. Further analysis

Ideally, we would use a general equilibrium model of international trade to derive testable hypotheses. Such a model must involve barriers to trade (either *natural*, such as transportation costs, or *created*, such as tariffs and non-tariff barriers), because our objective is to gauge the impact of reduced trade barriers on the international co-movements of business cycles. Because of the latter point, this model, unlike most models of international trade, must be stochastic with roles for industry-specific and aggregate shocks. Further, it must involve intersectoral trade (to be able to accommodate specialization) and intra-industry trade (because the effects on the latter of opening trade are thought to be large and different from those of inter-industry trade). Creating such a model from scratch is clearly beyond the limited scope of this (chiefly empirical) paper.

Cross-country covariance in aggregate output can schematically be thought of as depending on:

- Similarity in industry composition in the face of sector-specific (but global) shocks
- Extent of country-specific shocks

As noted by Eichengreen, Kenen, and Krugman, increased trade results in greater specialization if most trade is inter-industry. Because countries tend to produce and export goods in which they have a comparative advantage, greater specialization tends to lead to lower cross-country covariances in aggregate output because sector-specific shocks have a differential impact across countries. Of course, if much trade is *within* rather than among industries, such specialization effects may be small. The latter sort of trade—intra-industry—has attracted much attention of late and is commonly considered to account for a substantial share of international trade.

The covariance of the country-specific aggregate shocks may also be affected by increased integration. There are several potentially important channels. The spillover of aggregate demand shocks will tend to raise the covariance because, for example, an expansionary fiscal policy in one country tends to raise demand for foreign and domestic output. But this may not be the only channel. The presence of greater trade integration may induce a more rapid spread of productivity shocks, raising the covariance. And government-induced policy shocks may become more coordinated in the presence of increased integration.

It seems that closer international integration will tend to raise the covariance of country-specific demand shocks and aggregate productivity shocks. This tends to increase the international coherence of business cycles. But integration tends to raise the degree of industrial specialization, leading to more asynchronous business cycles. The importance of this effect depends on the degree of specialization induced by integration, which may not be large if most trade is intra-industry rather than inter-industry. And the net effect on business-cycle coherence depends on the relative variances of aggregate and industry-specific shocks. If the former are larger than the latter—as in, for example, Stockman (1988)—then we would expect closer trade integration to result in more synchronized business cycles.

Casual empiricism leads us to the view that integration results in more highly correlated national business cycles. But the alternative Eichengreen-Kenen-Krugman view is defensible on theoretical grounds. The matter can only be resolved empirically. We now turn to that task.

2. Related results from the literature

Cohen and Wyplosz (1989) examined the correlation of output growth rates for Germany and France; Weber (1991) did so for other members of the European Community. Stockman (1988) decomposed cross-country growth rates of industrial production for European countries into industry-specific and country-specific components. Bayoumi and Eichengreen (1993a, 1993b, 1993c, 1994) argue that these studies conflate information on the incidence of disturbances and on the economies' responses. Accordingly, Bayoumi and Eichengreen use structural vector auto-regressions to distinguish underlying aggregate demand and aggregate supply disturbances from the subsequent dynamic response. They use the results to find plausible groupings of countries for monetary union.

De Grauwe and Vanhaverbeke (1993) find that *asymmetric* or idiosyncratic shocks tend to be more prevalent at the regional level within a country than at the national level within Europe. This seems to support the view that increasing integration may result in more idiosyncratic activity. But De Grauwe and Vanhaverbeke use the *standard deviation of the difference* in percentage changes in income between the two regions instead of the *correlation* of percentage changes in income between two regions. This is a less useful measure of income links. There is every reason to think that the variance of income at the regional level is much higher than the variance of income at the national level. Because national income is the sum of regional income, some local variation is bound to wash out despite the presence of potentially high interregional correlations.[5]

Close in spirit to our view is a recent paper by Artis and Zhang (1995), which finds that most European countries' incomes were more highly correlated with the U.S. during 1961-79, but (with the exception of the UK) have become more highly correlated with Germany since joining the ERM.[6]

[5] If regional variances are larger than national variances, simple algebra can show that the variance of regional differences can appear larger than the variance of national differences, even though regional incomes are in fact more highly correlated than national variances. The reason is that the correlation coefficient is defined as the covariance divided by the product of the square root of the respective variances.

[6] Of course this may be the result of the loss of monetary independence, rather than of increased trade.

All this work is subject to the previously mentioned *Lucas Critique.* Perhaps more importantly, none of this work attempts to endogenize international business-cycle correlations.

3. Empirical methodology

This section presents some empirical evidence on the relationship between bilateral income correlations and bilateral trade intensity. The evidence is consistent with a strong positive effect of trade intensity on income correlations.

3.1. Measuring bilateral trade intensity and business-cycle correlations

Our empirical analysis relies on two key variables: bilateral trade intensity; and bilateral correlations of real economic activity. We discuss these in turn.[7]

We are interested in the bilateral intensity of international trade between two countries, i and j at a point in time t. We use three different proxies for bilateral trade intensity. The first uses export data exclusively; the second uses only imports, and the final (and preferred) measure uses exports and imports:

$$wx_{ijt} = X_{ijt}/(X_{i,t} + X_{j,t})$$
$$wm_{ijt} = M_{ijt}/(M_{i,t} + M_{j,t})$$
$$wt_{ijt} = (X_{ijt} + M_{ijt})/(X_{i,t} + X_{j,t} + M_{i,t} + M_{j,t})$$

where: X_{ijt} denotes total nominal exports from country i to country j during period t; $X_{i,t}$ denotes total global exports from country i; and M denotes imports. (In practice we take natural logarithms of all three ratios.) We think of higher values of, for example, wt_{ijt} as indicating greater trade intensity between countries i and j.

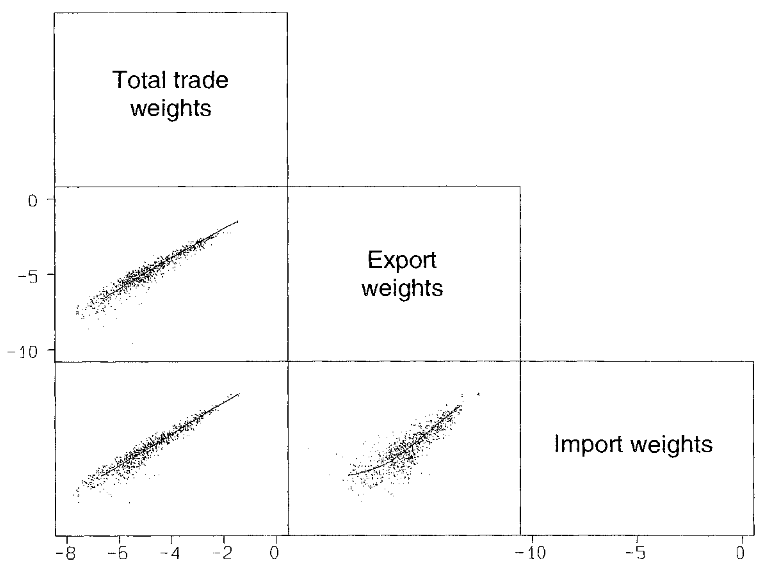
The bilateral trade data are taken from the International Monetary Fund's (IMF) *Direction of Trade* dataset. The data are annual and cover 21 industrial countries from 1959 through 1993.[8]

[7] The STATA dataset is available at http://haas.berkeley.edu/~arose

[8] The countries are: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Norway, Netherlands, New Zealand, Portugal, Spain, Sweden, Switzerland, the UK, and the U.S. In future work, we hope to include developing countries.

A variety of problems are associated with bilateral trade data (for example, $X_{ijt} \neq M_{jit}$). Our data measure actual trade rather than potential trade that would exist if conditions were slightly different. And from a theoretical viewpoint, it is unclear which set of weights is optimal; some countries may have specialized exports or imports. So we conduct our tests with all three measures of trade intensity. Reassuringly, our answers seem insensitive to the exact way that we measure trade intensity. This is unsurprising, because the three different measures are highly positively intercorrelated. Figure 2 provides scatter plots of each measure of trade intensity graphed against the others. Non-parametric data smoothers are provided to connect the dots.

Figure 2. Scatter-plots of each trade-intensity measure graphed against the others.



Different (logs) trade intensity measures.

Our other important variable is the bilateral correlation between real activity in country i and country j at time t. Again, it is difficult to figure out the optimal empirical analogue to the theoretical concept. So we use a variety of different proxies.

We use four different measures of real economic activity: the first pair taken from the IMF's *International Financial Statistics*; the other two from the OECD's *Main Economic Indicators*. In particular, we use: real GDP (typically IFS line 99); an index of industrial production (line 66); total employment (OECD mnemonic *et*); and the unemployment rate (*unr*). All the data are quarterly, covering (with gaps) the same sample of countries and years as the trade data.

We transform our variables in two different ways: by taking natural logarithms of each variable except the unemployment rate and by detrending the variables so as to focus on business-cycle fluctuations.

Given the importance of different detrending procedures and the lack of consensus about optimal detrending techniques, we use four different detrending methodologies, where we:[9]

1. Take simple fourth-differences of the (logs of the) variables (that is, we subtract the fourth lag of, for example, real GDP from the current value), multiplying by 100 (so that the resulting variable can be interpreted as a growth rate).
2. Detrend the variables by examining the residual from a regression of the variable on a linear time trend, a quadratic time trend, and three quarterly dummies.
3. Detrend the variables using the well-known Hodrick-Prescott (HP) filter (using the traditional smoothing parameter of 1600).
4. Apply the HP filter to the residual of a regression of the variable on a constant and quarterly dummies.

After appropriately transforming our variables, we can compute bilateral correlations for real activity. These correlations are estimated (for a given concept of real economic activity), *between* two countries *over* a given time span. Thus, for example, we estimate the correlation between real GDP detrended with the HP filter for two countries, i and j, over the first part of our sample period.

We begin by splitting our sample into four equally sized parts: the beginning of the sample through 1967Q3 (1967, third quarter); 1967Q4 through 1976Q2; 1976Q3 through 1985Q1; and 1985Q2

[9] We have also constructed a fifth transformation of our dependent variable. This is similar to our second variant in that we detrend the variables by examining the residual from a regression of the variable on a set of controls. But we add a control, which is meant to account for the dependency of the economy to imported oil-price shocks. In particular, we take the real oil price (the oil price in dollars per barrel, divided by the CPI for industrial countries), and multiply it by net exports of fuel, expressed as a percentage of nominal GDP. This variable, meant to measure the degree of dependency on imported oil, is then added to our other control variables including linear and quadratic time trends, and quarterly dummies.

through the end of the sample. Because we have 21 countries, we are thus left with a sample size of 840 observations; 210 bilateral country-pair correlations (=(21x20)/2), with four observations (over different periods) per country pair.

While our three measures of trade intensity are quite similar to one another, the same is not as true of our 16 measures of business-cycle correlations. The measure of economic activity (GDP/industrial production and so on) and the detrending technique matter (though all 16 measures of bilateral activity correlation are positively correlated with each other).

Figure 3 graphs different business-cycle correlations against each other, holding the detrending method constant (at fourth-differencing) but allowing the underlying *activity measure* to vary.

Figure 3. Different business-cycle correlations against each other

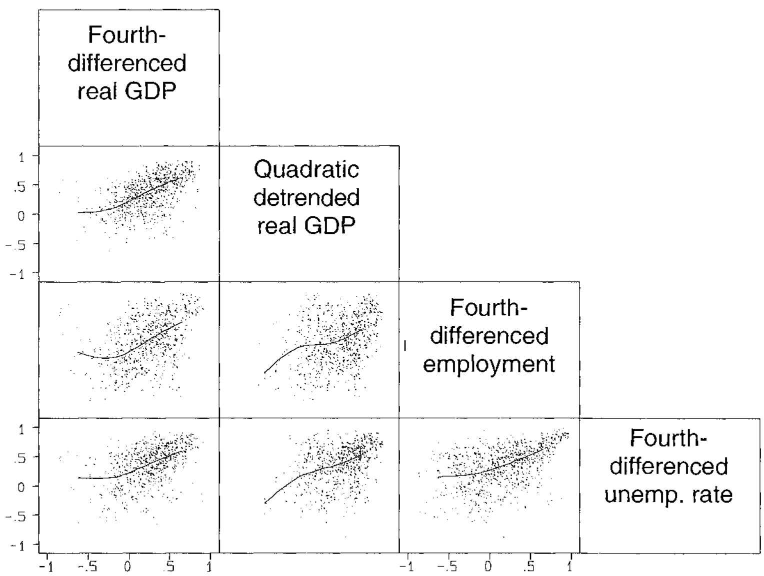
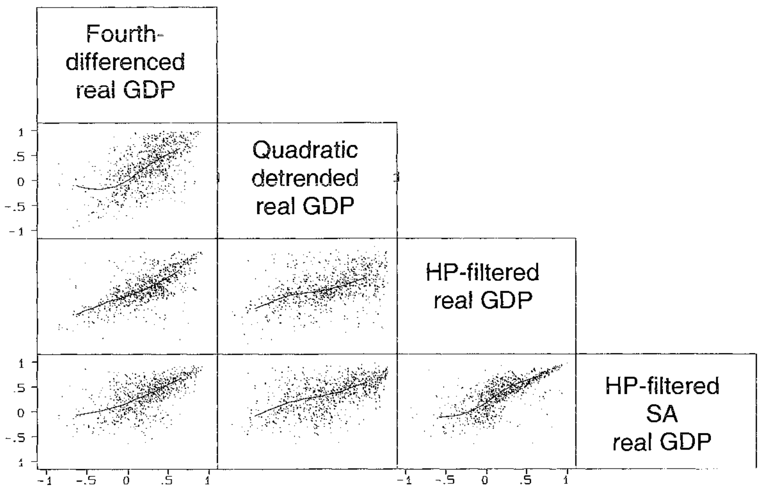


Figure 4 is an analogue that varies the *detrending technique* but only portrays real GDP. Because the international business-cycle correlations are so imperfectly related to one another, we extensively check the sensitivity of our results.

Figure 4. Analogue that varies the *detrending technique* but only portrays real GDP



Real GDP detrended in different ways.

3.2. Econometric methodology

The regressions we estimate take the form:

$$\text{Corr}(v,s)_{i,j,\tau} = \alpha + \beta \text{Trade}(w)_{i,j,\tau} + \varepsilon_{i,j,\tau}.$$

$\text{Corr}(v,s)_{i,j,\tau}$ denotes the correlation between *country i* and *country j* over *time span* τ for *activity concept v* (corresponding to: real GDP (denoted y); industrial production (i); employment (e); or the unemployment rate (u)), *detrended with method s* (corresponding to: fourth-differencing (d); quadratic detrending (t); HP filtering (h); HP filtering on the SA residual (s); or quadratic detrending with the oil control (o)). $\text{Trade}(w)_{i,j,\tau}$ denotes the natural logarithm of the average bilateral trade intensity between country i and country j over time span τ using *trade intensity concept w* (corresponding to: export weights (x); import weights (m); or total trade weights (t)). Finally, $\varepsilon_{i,j,\tau}$ represents the myriad influences on bilateral real activity correlations above and beyond the influences of international trade, and α and β are the regression coefficients to be estimated.

We have 16 versions of the regressand (as we consider four activity concepts and four detrending methods) and three versions of the regressor (because we have three sets of trade weights). We estimate all 48 versions of our regression to check results for robustness.

The object of interest to us is the slope coefficient β. We are interested in the sign and the size of the coefficient. The *sign* of the slope tells us whether the Eichengreen-Krugman specialization effect dominates (in which case we would expect a negative β because more intense trading relations would be expected to lead to more idiosyncratic business cycles and hence a lower correlations of economic activity) or the expected traditional effect prevails (in which case β would be expected to be positive). The *size* of the coefficient allows us to quantify the economic importance of this effect.

A simple OLS regression of bilateral activity income correlations on trade intensity may be inappropriate. Countries will probably deliberately link their currencies to those of some of their most important trading partners, to capture gains associated with greater exchange-rate stability. In doing so, they lose the ability to set monetary policy independently of those neighbors. The fact that their monetary policy will be closely tied to that of their neighbors could result in an observed positive association between trade links and income links. So the association could be the *result* of countries' application of the OCA criterion, rather than an aspect of economic structure that is invariant to exchange-rate regimes.

To identify the effect of bilateral trade patterns on income correlations in such circumstances, we need exogenous determinants of bilateral trade patterns. These can be used as instrumental variables to produce consistent estimates of β. Our preferred set of instrumental variables includes the most basic variables of the well-known *gravity model* of trade: distance and dummy variables for common border or language.[10]

Parenthetically, estimation of the standard error for β is potentially complicated. Our observations may not be independent; for example, the French-Belgian observation for the first quarter of the sample may depend on either the French-Belgian observation for the second quarter, or the French-Dutch observation for the first quarter (or both). We ignore such potential dependencies in computing our

[10] Instrumental variable estimation is also appropriate because the regressors are measured with error.

covariance matrices and instead try simply not to take their precise size too seriously (it turns out there is no need to do so).[11]

4. Empirical results

We start our analysis with simple ordinary least squares. Consistent with our priors, estimation with instrumental variables delivers the same message even more strongly.

4.1. OLS work

Table 1 tabulates ordinary least squares (OLS) estimates of β. Three columns show the estimates (along with their standard errors), which correspond to the three different measures of bilateral trade intensity. For each measure, the rows display 20 estimates (four measures of economic activity, each detrended in four different ways and the oil-adjusted measure).

The estimates indicate that a closer trade linkage between two countries is strongly and consistently associated with more tightly correlated economic activity between the two countries. The size of this effect depends on the exact measure of economic activity (as expected) but does not depend very sensitively on the exact method of detrending the data or the measure of bilateral trade intensity. Parenthetically, the adjustment for the oil price reduces the size of the coefficients slightly, although they remain positive and significant.

We checked these results in several different ways, and they seem to be robust. For example, a consistently positive estimate of β appears whether or not the trade intensity measure is transformed by natural logarithms and whether or not the observations are weighted by country size. More importantly, the results do not seem very sensitive to the exact sample chosen.

[11] The dataset reveals few signs of such dependency. White covariance matrices are very similar to traditional ones; non-parametric tests for dependencies across periods reveal no trends; boot strapping our standard errors results in very similar standard error estimates. Parenthetically, our IV standard errors should be consistent in the presence of generated regressors.

Table 1. OLS estimates of β

(Effect of trade intensity on income correlation)

Activity	Detrending	Total trade weights	Import weights	Export weights
GDP	Differencing	7.1 (.88)	6.2 (.79)	6.7 (.85)
Ind. prod.	Differencing	6.9 (.95)	5.5 (.83)	6.9 (.95)
Employ.	Differencing	5.7 (1.1)	4.8 (1.0)	5.3 (1.1)
Unemp.	Differencing	3.3 (.97)	2.5 (.87)	3.1 (.95)
GDP	Quadratic	7.2 (1.1)	6.3 (.99)	6.4 (1.1)
Ind. prod.	Quadratic	8.3 (1.2)	7.2 (1.0)	7.6 (1.2)
Employ.	Quadratic	6.2 (1.4)	6.1 (1.3)	4.8 (1.5)
Unemp.	Quadratic	7.0 (1.4)	6.1 (1.3)	6.4 (1.5)
GDP	HP-filter	5.7 (.92)	4.2 (.85)	5.9 (.88)
Ind. prod.	HP-filter	5.6 (1.0)	4.5 (.88)	5.5 (1.0)
Employ.	HP-filter	6.6 (1.1)	5.7 (.99)	6.2 (1.0)
Unemp.	HP-filter	3.4 (1.1)	2.6 (.95)	3.2 (1.0)
GDP	HP-SA	4.8 (.84)	3.9 (.78)	4.7 (.81)
Ind. prod.	HP-SA	4.9 (.94)	3.9 (.81)	4.8 (.94)
Employ.	HP-SA	6.5 (1.0)	5.7 (.92)	5.9 (.94)
Unemp.	HP-SA	3.2 (1.0)	2.4 (.94)	5.9 (.98)
GDP	Oil adjusted	4.7 (1.2)	3.8 (1.1)	4.7 (1.2)
Ind. prod.	Oil adjusted	6.3 (1.3)	5.3 (1.1)	5.9 (1.3)
Employ.	Oil adjusted	7.9 (1.5)	6.5 (1.4)	7.6 (1.4)
Unemp.	Oil adjusted	4.7 (1.5)	4.3 (1.3)	4.1 (1.4)

Notes: OLS estimate of β (multiplied by 100) from

$$\text{Corr}(v,s)_{i,j,\tau} = \alpha + \beta \text{Trade}(w)_{i,j,\tau} + \varepsilon_{i,j,\tau}.$$

Huber-White heteroskedasticity consistent standard errors in parentheses. Intercepts not reported. Bilateral quarterly data from 21 industrialized countries, 1959 through 1993 split into four subperiods. Maximum sample size = 840.

The data from the last quarter of the sample show more evidence of a strongly positive estimate of β than does that from the first quarter, but the exact choice of countries does not matter. We have also tested for the importance of non-linearities in the relationship between trade intensity and activity correlations by estimating the equation with a non-parametric data smoother (similar to locally weighted regression but without neighborhood weighting); the non-linear effects are typically statistically insignificant and the strong positive effect of trade intensity on business-cycle correlations is not affected. Adding either time-specific or country-specific *fixed-effect* controls (or both) also does not affect the sign or statistical significance of β. Finally, we split our dataset into two subperiods across time (instead of four) and re-estimated our equations. The resulting point estimates of β remain quite similar to those in Table 1.

4.2. Instrumental variable estimation

The issue of simultaneous causation is potentially serious. For this reason, we take instrumental variable (IV) estimates of β more seriously than our OLS estimates. We use three instrumental variables:

1. The natural logarithm of the distance between the business centers of the relevant pair of countries
2. A dummy variable for geographic adjacency
3. A dummy variable that indicates if the pair of countries share a common language

Each of these variables is expected to be correlated with bilateral trade intensity but can reasonably be expected to be unaffected by other conditions that affect the bilateral correlation of economic activity.

Table 2 shows direct evidence on the first-stage linear projections of (the natural logarithm of) bilateral period-average trade intensity on our three favored instrumental variables.

Distance (more precisely, the natural log thereof) is strongly negatively associated with trade intensity, as predicted by standard *gravity* models of international trade. Countries that share either a common border or a common language also have significantly more trade than others. The first-stage equations appear to fit relatively well.

Table 2 also includes a minor perturbation to our standard first-stage equation, namely the *default equation* augmented by a variable that registers membership in a regional trade agreement. There are two relevant agreements; the:

- U.S./Canada FTA and NAFTA, its successor
- EEC/EC[12]

[12] We compute this variable by taking a pair-specific indicator variable (for example, unity for UK/France in 1975, zero for U.S./Japan in 1975) and computing subperiod averages over time (for example, the subperiod for the last quarter of the sample is non-zero for all EC-Spanish observations, but the observations are not unity because Spain was not in the EC for the entire subsample; earlier Spanish observations are all zero).

Table 2. First-stage estimates

(Determinants of bilateral trade)

	Total trade weights	Total trade weights	Import weights	Import weights	Export weights	Export weights
Log of distance	-.45 (.03)	-.40 (.03)	-.52 (.03)	-.48 (.04)	-.43 (.04)	-.37 (.04)
Adjacency dummy	1.03 (.14)	1.01 (.14)	.83 (.14)	.81 (.14)	1.21 (.16)	1.19 (.16)
Common language	.51 (.11)	.51 (.11)	.58 (.11)	.58 (.11)	.48 (.13)	.48 (.13)
Regional trade member		.44 (.11)		.35 (.12)		.54 (.13)
N	840	840	840	840	840	840
RMSE	.98	.97	1.01	1.01	1.14	1.13
R^2	.39	.40	.40	.40	.33	.34

Notes: OLS estimates from

$$\text{Trade}(w)_{i,j,\tau} = \varphi_{P1} + \varphi_1 \text{Log(Distance)}_{i,j} + \varphi_2 \text{Adjacent}_{i,j} + \varphi_3 \text{Language}_{i,j} + \varphi_4 \text{Regional}_{i,j,\tau} + \nu_{i,j,\tau}.$$

Standard errors in parentheses. Intercepts not reported. Bilateral quarterly data from 21 industrialized countries, 1959 through 1993 split into four subperiods. Maximum sample size = 840.

Membership in a regional trading agreement is strongly associated with more intense international trade in an economic and statistical sense. Entry into a regional trade agreement appears to raise bilateral trade intensity by almost 50 percent. While the variable seems almost orthogonal to our three default instrumental variables, we do not use it as one of our default instrumental variables because it is potentially associated with tighter income correlations directly (for example, through exchange-rate arrangements; there is a high correlation between EC and EMS membership). Happily, our β estimates are insensitive to inclusion or exclusion of the extra instrumental variable.

Table 3, which is a direct analogue to Table 1, tabulates instrumental variable estimates of β (estimated with our three default instrumental variables). As expected, the results are consistent with the OLS results in Table 1, but they are somewhat stronger in economic and statistical significance. The effect of greater intensity of international trade on the correlation of economic activity remains strongly positive and statistically significant but is larger than the simple OLS estimates indicate (though we try not to interpret the t-statistics too literally, given the potential problems of cross-sectional or intertemporal dependency). The oil-adjusted results are now slightly larger than the other coefficients.

Table 3. IV estimates of β

(Effect of trade intensity on income correlation)

Activity	Detrending	Total trade weights	Import weights	Export weights
GDP	Differencing	10.3 (1.5)	10.2 (1.4)	9.7 (1.4)
Ind. prod.	Differencing	10.1 (1.5)	9.8 (1.5)	9.8 (1.5)
Employ.	Differencing	8.6 (1.8)	8.4 (1.8)	8.2 (1.8)
Unemp.	Differencing	7.8 (1.6)	7.6 (1.6)	7.5 (1.6)
GDP	Quadratic	11.3 (1.9)	11.1 (1.9)	10.7 (1.8)
Ind. prod.	Quadratic	9.3 (2.1)	9.0 (2.0)	9.0 (2.0)
Employ.	Quadratic	8.6 (2.5)	8.6 (2.4)	7.9 (2.4)
Unemp.	Quadratic	10.8 (2.4)	10.5 (2.4)	10.6 (2.3)
GDP	HP-filter	8.6 (1.5)	8.4 (1.5)	8.2 (1.4)
Ind. prod.	HP-filter	9.8 (1.7)	9.4 (1.6)	9.4 (1.6)
Employ.	HP-filter	10.1 (1.8)	9.8 (1.8)	9.7 (1.8)
Unemp.	HP-filter	7.8 (1.7)	7.5 (1.7)	7.6 (1.6)
GDP	HP-SA	7.3 (1.5)	7.2 (1.4)	6.9 (1.4)
Ind. prod.	HP-SA	9.1 (1.5)	8.7 (1.5)	8.8 (1.5)
Employ.	HP-SA	8.6 (1.7)	8.4 (1.7)	8.2 (1.7)
Unemp.	HP-SA	8.1 (1.7)	7.8 (1.7)	7.8 (1.6)
GDP	Oil adjusted	14.3 (2.0)	13.9 (2.0)	13.8 (1.9)
Ind. prod.	Oil adjusted	14.0 (2.2)	13.5 (2.1)	13.6 (2.1)
Employ.	Oil adjusted	13.7 (2.4)	13.4 (2.4)	12.9 (2.3)
Unemp.	Oil adjusted	8.4 (2.4)	8.1 (2.4)	8.3 (2.3)

Notes: IV estimate of β (multiplied by 100) from

$$\mathrm{Corr}(v,s)_{i,j,\tau} = \alpha + \beta \mathrm{Trade}(w)_{i,j,\tau} + \varepsilon_{i,j,\tau}.$$

Instrumental variables for trade intensity are: 1) log of distance; 2) dummy variable for common border; and 3) dummy variable for common language.

Standard errors in parentheses. Intercepts not reported. Bilateral quarterly data from 21 industrialized countries, 1959 through 1993 split into four subperiods. Maximum sample size = 840.

As with the OLS results, our IV estimates of β are robust to a wide range of perturbations to our basic econometric methodology.

We performed all the experiments mentioned in conjunction with Table 1 without disturbing our central results. We also changed the list of instrumental variables in several different ways without changing our results. For example, adding dummy variables for membership in GATT or regional trade arrangements as extra instrumental variables does not change our results, as does adding country population and output.

4.3 More sensitivity analysis

We augmented our relationship by adding a dummy variable that is unity if the two countries shared a bilateral, fixed exchange rate throughout the sample. This is an important test. The Bayoumi-Eichengreen view is that the high correlation among European incomes is a result not of trade links but of Europeans' decision to relinquish monetary independence *vis-à-vis* their neighbors. If this is correct, putting the exchange-regime variable explicitly on the right-hand side should show the effect, and the apparent effect of the trade and geography variables should disappear. Instead, the addition of this exchange-rate variable does not significantly alter β. Table 4, which is an analogue of Table 3, shows the actual estimates (with the same instrumental variables) when the equation is augmented by an indicator variable, which is unity if the pair of countries maintained a mutually fixed exchange rate during the relevant sample period.

For simplicity, only the results with total trade weights are reported. The positive β coefficient still appears quite strong; indeed its sign and magnitude is essentially unchanged from Table 3. In contrast, the effect of a fixed exchange-rate regime *per se* is not well determined. The coefficients vary in sign and magnitude depending on the exact measure of economic activity and detrending method used to compute the bilateral activity correlation.[13]

We also performed a more direct check for the importance of oil-price shocks by augmenting our relationship with a variable meant to measure the degree of dependency on imported oil. This variable (the same used to adjust the oil-adjusted regressands tabulated in Tables 1 and 3) is the product of the real oil price (the oil price in dollars per barrel, divided by the CPI for industrial countries) and net fuel exports, expressed as a percentage of nominal GDP. We add this vari-

[13] Results are not changed substantively if the actual bilateral exchange-rate volatility is substituted for our indicator variable.

able to our default regression and estimate the coefficients with instrumental variables. Table 5 shows the results. There are two sets of columns. The second is a minor perturbation, in that the extra regressor is the *percentage change* of the real oil price multiplied by exports of fuel. Again, as in Table 4, the same instrumental variables as in Table 3 are used, and for simplicity, only the results with total trade weights are reported

Table 4. IV estimates of β and γ (effect of fixed-rate regime)

(Total trade weights)

Activity	Detrending	β	γ
GDP	Differencing	11.5 (1.5)	-13.0 (2.9)
Ind. prod.	Differencing	10.7 (1.6)	-5.1 (2.9)
Employ.	Differencing	8.9 (1.9)	-2.7 (3.6)
Unemp.	Differencing	7.3 (1.7)	5.1 (3.2)
GDP	Quadratic	12.6 (2.0)	-15.2 (3.7)
Ind. prod.	Quadratic	11.3 (2.2)	-17.2 (3.9)
Employ.	Quadratic	9.4 (2.6)	-6.8 (4.9)
Unemp.	Quadratic	12.1 (2.5)	-13.2 (4.8)
GDP	HP-filter	8.6 (1.6)	.0 (3.0)
Ind. prod.	HP-filter	10.8 (1.7)	-8.7 (3.1)
Employ.	HP-filter	10.4 (1.9)	-1.7 (3.6)
Unemp.	HP-filter	7.7 (1.8)	1.1 (3.4)
GDP	HP-SA	6.5 (1.5)	10.8 (2.8)
Ind. prod.	HP-SA	9.9 (1.6)	-7.1 (2.9)
Employ.	HP-SA	8.6 (1.8)	.5 (3.4)
Unemp.	HP-SA	7.6 (1.8)	4.7 (3.3)

Notes: IV estimates of β and γ (multiplied by 100) from

$$Corr(v,s)_{i,j,\tau} = \alpha + \beta Trade(w)_{i,j,\tau} + \gamma FIX_{i,j,\tau} + \varepsilon_{i,j,\tau},$$

where $FIX_{i,j,\tau}$ is the (period average of a) dummy variable, which is unity if i and j had a mutually fixed exchange rate during the period.

Instrumental variables for trade intensity are: 1) log of distance; 2) dummy variable for common border; and 3) dummy variable for common language. Standard errors in parentheses. Intercepts not reported. Bilateral quarterly data from 21 industrialized countries, 1959 through 1993 split into four subperiods. Maximum sample size = 840.

The positive β coefficient still appears quite strong; its sign and magnitude is essentially unchanged from Tables 3 and 4. In contrast, the effect of oil-price dependency is not firmly established. The coefficients vary in sign and magnitude when the oil-price level is used.

When the percentage change of the oil price is used, the oil price regressor has a consistently positive (though not always significant) coefficient. But the durable sign and significance of β is unaffected.

Table 5. IV estimates of β and δ (effect of oil-price shock)

(Total trade weights)

		Price of oil		*Change in oil price*	
Activity	**Detrending**	β	δ	β	δ
GDP	Differencing	10.3 (1.5)	.4 (.5)	9.8 (1.4)	6.2 (1.1)
Ind. prod.	Differencing	10.1 (1.5)	.8 (.5)	9.0 (1.4)	9.3 (1.1)
Employ.	Differencing	8.6 (1.8)	-.8 (.6)	8.4 (1.8)	2.5 (1.4)
Unemp.	Differencing	7.9 (1.6)	-2.5 (.6)	7.3 (1.6)	5.5 (1.2)
GDP	Quadratic	11.2 (1.9)	2.5 (.7)	10.9 (1.9)	4.6 (1.5)
Ind. prod.	Quadratic	9.2 (2.1)	6.5 (1.8)	8.6 (2.1)	6.2 (1.6)
Employ.	Quadratic	8.6 (2.5)	-.6 (.8)	8.5 (2.5)	.7 (1.9)
Unemp.	Quadratic	10.8 (2.4)	.5 (.8)	10.3 (2.4)	6.2 (1.9)
GDP	HP-filter	8.6 (1.5)	.2 (.5)	8.2 (1.5)	4.6 (1.2)
Ind. prod.	HP-filter	9.7 (1.6)	4.3 (1.5)	8.7 (1.6)	9.2 (1.2)
Employ.	HP-filter	10.1 (1.8)	-1.0 (.6)	9.8 (1.8)	3.3 (1.4)
Unemp.	HP-filter	7.8 (1.7)	-.5 (.6)	7.3 (1.7)	5.6 (1.3)
GDP	HP-SA	7.3 (1.4)	-.4 (.5)	6.9 (1.4)	4.6 (1.1)
Ind. prod.	HP-SA	9.0 (1.5)	4.2 (1.3)	8.2 (1.4)	7.3 (1.1)
Employ.	HP-SA	8.6 (1.7)	-1.3 (.6)	8.4 (1.7)	1.9 (1.3)
Unemp.	HP-SA	8.1 (1.7)	-.6 (.6)	7.6 (1.7)	5.1 (1.3)

Notes: IV estimates of β and γ (multiplied by 100) from

$$Corr(v,s)_{i,j,\tau} = \alpha + \beta Trade(w)_{i,j,\tau} + \delta(POIL^*\{[(XFuel\text{-}MFuel)/Y]_i[(XFuel\text{-}MFuel)/Y]_j\})_\tau + \varepsilon_{i,j,\tau},$$

where $(POIL^*\{[(XFuel\text{-}MFuel)/Y]_i[(XFuel\text{-}MFuel)/Y]_j\})_\tau$ is the (period average of) the product of the nominal price of oil (in \$/bbl, deflated by the global CPI), net fuel exports normalized by nominal GDP in country i, and the latter variable for country j.

Instrumental variables for trade intensity are: 1) log of distance; 2) dummy variable for common border; and 3) dummy variable for common language. Standard errors in parentheses. Intercepts not reported. Bilateral quarterly data from 21 industrialized countries, 1959 through 1993 split into four subperiods. Maximum sample size = 840.

5. Conclusion

This paper considers the relationship between two of the criteria used to determine whether a country is a member of an optimum currency area. From a theoretical viewpoint, the effect of increased trade integration on the cross-country correlation of business-cycle activity is ambiguous. Reduced trade barriers can result in increased industrial specialization by country and therefore more asynchronous business cycles resulting from industry-specific shocks. But increased integration may result in more highly correlated business cycles because of demand shocks or intra-industry trade.

This theoretical ambiguity does not characterize the data. Using a panel of 30 years of data from 20 industrialized countries, we find a strong positive relationship between the degree of bilateral trade intensity and the cross-country bilateral correlation of business-cycle activity. Greater integration historically has resulted in more highly synchronized cycles.

The endogenous nature of the relationship between various OCA criteria is a straightforward application of the celebrated *Lucas Critique*. Still, it has considerable relevance for the current debate on economic and monetary union in Europe. For example, some countries may appear, on the basis of historical data, to be poor candidates for EMU entry. But EMU entry *per se*, for whatever reason, may provide a substantial impetus for trade expansion; this in turn may result in more highly correlated business cycles. That is, a country is more likely to satisfy the criteria for entry into a currency union *ex post* than *ex ante*.

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