Fluctuations, Bilateral Trade and the Exchange Rate Regime[*]

Jean Imbs[†]
University of Lausanne &
New York University

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Abstract

In a recent paper, Frankel and Rose (1998) documented endogenous effects of a monetary union, whereby costs and benefits of the union evolve after its implementation. This paper questions their findings on three grounds. First, their main result that trading partners display relatively more synchronized cycles is not robust to the presence of fixed effects, or variables omitted from their estimation liable to generate both intense trade and synchronized cycles. Second, the cost of giving up independent monetary policy is usually evaluated on the basis of the extent of co-fluctuations between business cycles. We bring into focus which measure of the cycle ought to be used for that purpose. In particular, such measure should in our opinion reflect how synchronized cycles would be in the absence of independent monetary policy. Third, documenting the assumption that fixed exchange rate regimes translate into more bilateral trade has proved elusive. We show that using a bilateral rather than cross-country approach brings little improvement on that front.

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[†] University of Lausanne, HEC-DEEP, 1015 Lausanne Switzerland. Jean.Imbs@hec.unil.ch

1 Introduction

The bene…ts of a monetary union are usually associated with the intensity of trade between prospective members, and its costs originate in the inability to counteract asymmetric ‡uctuations through independent monetary policy[1]. This paper argues that both business cycles synchronization and trade intensity react endogenously to the implementation of a currency union, and thus that the costs and bene…ts of the union evolve after it is decided. Frankel and Rose (1998) - FR henceforth- showed empirically that trade partners display more conformity in their cycles and inferred that a monetary union should have ex-post e¤ects likely to diminish endogenously the very cost of its implementation. The paper criticizes this claim on three grounds: …rst, the Frankel and Rose result is not robust to the inclusion of country-pair speci…c …xed e¤ects. Second, insofar as they embed the …netuning e¤ects of -partly- independent monetary policies, international correlations of output or employment provide poor measures of how asymmetric ‡uctuations would be in the absence of monetary policy. Third, even if trade fosters cycle synchronization, it remains to be seen whether a currency union will stimulate exchanges, a frequent assumption with scant empirical support at best. The method proposed here generates some evidence - though weak- that countries agreeing on a …xed exchange rate regime trade signi…cantly more with each other.

To a large extent, theory of the international business cycle has focused on the so-called quantity puzzle, as coined by Backus, Kehoe and Kydland (1994), whereby outputs are negatively correlated as resources travel where the rates of return are highest. This has given rise to a variety of sophisticated attempts to correct this defect, involving for instance nominal rigidities, multi-sectoral models or shocks at the international level[2]. However, none of these improvements can explicitly help predict cross-sectional variation in the extent of cycle synchronization[3]. Thus, the empirical researcher must remain agnostic as to what variables ought to be included when attempting to explain why two countries co-‡uctuate relatively more. Figure 1

[1] The cost of losing monetary independence can be mitigated if labor is su¢ ciently mobile between the member countries, or if an international system of …scal transfers exists. These aspects are assumed away in the paper.

[2] See for instance Kollmann (1998), Hornstein and Praschnick (1997) or Canova (1993).

[3] In our opinion, this is an important and unexplored area of research in the international business cycle literature. See Imbs (1998) for a more thorough analysis of why countries co-‡uctuate.

reproduces the main result in FR, namely that trade partners display more synchronized cycles: the relation is indeed positive, with a correlation coe¢-cient equal to 0.21. However, there is no theoretical reason to limit oneself to trade in explaining GDP correlations. For instance, geographical, historical or institutional factors could play a signi…cant role, thus casting doubt on the claim in Frankel and Rose. For instance in a context of multiple equilibria, the use of a common language, or access to common media could play a coordinating role in selecting an equilibrium. This is illustrated in Figure 2, that plots GDP correlations against bilateral distance: the correlation does appear negative, at -0.20. The well-known fact that trade partners are often close to each other is reproduced in Figure 3: thus, the apparent relation between trade and cycle synchronization may actually be spurious insofar as it may stem from the fact that geographically close countries tend to both trade with each other and display synchronized business cycles. In fact, Imbs (1998b) shows that bilateral trade is actually a surprisingly poor predictor of co-‡uctuations. The paper elaborates on the …xed-e¤ect argument that underpins this criticism.

If one wants to evaluate the cost of giving up independent monetary policy, one must estimate the degree of conformity between cycles abstracting from the e¤ects of monetary policy -a measure of "structural synchronization"- a task best ful…lled using bilateral correlations of Total Factor Productivity (TFP), or, under some additional assumptions, correlations between labor productivity. Indeed, output or employment correlations surely contain the e¤ects of a policy that the purpose of the exercise is to assume away. TFP correlations, on the other hand, are supposedly purged from the e¤ects of monetary policy, and potentially from those of …scal policy as well. Thus, TFP correlations provide a conservative estimate of the costs of a monetary union, in that they tend to over-estimate them, whereas output correlations under-estimate them[4]. If trade is to alleviate some of the cost of giving up independent monetary policy, it ought to matter positively for TFP correlations -which would happen for instance if technology were transmitted through trade. It is of course often argued that TFP as measured by the Solow Residual is nothing but a noisy measure of output, thus casting doubt on the meaning of bilateral correlations of TFP. Figure 4 plots TFP correlations against bilateral trade, and the unconditional correlation is actually positive though less signi…cant that in the output case, at 0.15. Furthermore,

[4] Imbs (1998a) shows the importance of choosing the right measure: while European outputs have become more synchronized betwen 1970 and 1993, European TFPs have on the contrary become more idiosyncratic.

it is customary to assume the e¤ects of monetary policy to be short-lived, and thus to be for the most part captured by ‡uctuations in the labor input. Accordingly, we supplement the previous evidence by computing bilateral correlations between labor productivities, and argue that they provide an estimate of "structural synchronization", less questionable than TFP in that it does not fall victim to the issue of measurement of the capital input. Figure 5 plots labor productivity against bilateral trade, with a positive correlation equal to 0.10. The paper presents multi-variate estimations meant to check those unconditional correlations for robustness.

No consensus has emerged from the many papers seeking to establish both theoretically and empirically the e¤ects of the exchange rate regime on trade. Nevertheless, the assumption that …xing the exchange rate stimulates international exchanges is made in a host of papers -starting with FR- and many economists still view a stable currency as favorable to trade. A set of recent papers has argued theoretically that the level of trade is not necessarily higher under …xed exchange rate regimes. For instance, Ricci (1998) presents a model where a ‡oating exchange rate acts as an insurance mechanism. The e¤ects of domestic demand shocks are mitigated by movements in the exchange rate, and risk-averse …rms will trade more in a regime of ‡oat. Bacchetta and vanWincoop (1998) develop a general equilibrium model with monopolistic competition, where the relationship between trade and exchange rate regime depends on the form of the utility function, calibration of risk aversion as well as the nature of stochastic developments. Gagnon (1993) constructs a model meant to exaggerate the negative e¤ect of exchange rate variability, for which he provides a statistically insigni…cant - yet economically signi…cant- upper bound. Unfortunately, perhaps precisely because the e¤ects are statistically insigni…cant, empirical analyses of the issue have so far been unable to settle the debate, for most of them provide at best weak evidence in one way or the other[5]. There is to our knowledge no paper that attempts to measure the e¤ect of bilateral regime on the intensity of bilateral trade, rather resorting to more classical cross-country arguments. This paper proposes to …ll that gap, unfortunately to no decisive avail.

Ex-post e¤ects of a currency union are therefore ambiguous. Supposing that it may stimulate trade, a currency union becomes endogenously more desirable. On the other hand however, perhaps precisely because it

[5] See for instance the survey by Cote (1994).

stimulates exchanges and thus fosters international specialization, a currency union makes it possible for aggregate shocks to become more country-speci…c, and thus makes it dearer to forego independent monetary policy. While this paper shows that aggregate data cannot be used to refute the latter e¤ect, a detailed analysis using sectoral data ought to quantify its importance.

Aside from the already cited paper by Frankel and Rose (1998), evidence on ex-post e¤ects of a currency union is limited. Canova and Dellas (1993) showed that the signi…cance of trade in the transmission of economic disturbances across countries is not robust to the choice of the detrending method, and thus did already cast doubt on the Frankel and Rose result. The present paper goes however further, …rst by showing that the significance of trade is not robust even when one sticks to a single detrending method, and by addressing the question of the e¤ect of trade on structural correlations. Artis and Zhang (1995) show that business cycles in European countries have become more similar to the German cycle than to the US's since the inception of the European Monetary System, and thus argue that successful …xed exchange rate regimes lead to conformity in the business cycles of the participating countries. The present paper reproduces their result, but shows that it is not general.

The paper is organized as follows: next section describes the empirical method used, as well as the way standard econometric issues in this literature are confronted. Section 3 presents the results, arguing in particular for the importance of …xed e¤ects in the FR …ndings. Section 4 develops our evidence on the e¤ects of trade on "structural" synchronization of cycles, and the e¤ects of the exchange rate regime on trade intensity. Section 5 concludes.

2 Methodology

2.1 Empirical Strategy

The strategy is similar to FR. Measures ρ of cycle synchronization are obtained by computing output bilateral correlations for 21 OECD countries[6], which generates 210 observations. The sample period 1970:1 - 1993:4 is divided into sub-periods $\tau = 3; 4$, which multiplies further the number of observations and gives a panel structure to the dataset. These are computed using the same data as in FR. Moreover, a measure of the "structural" extent of co-fluctuations requires the construction of Solow Residuals for the 21 countries in the sample. The necessary series on quarterly national aggregates comes either from the IMF's International Financial Statistics or from the OECD's Quarterly National Accounts[7]

Data on bilateral trade intensity come from the IMF's Direction of Trade dataset. As in FR, annual data from 1970 to 1993 is used to compute an indicator of bilateral trade intensity, WT:

$$WT_{i;j;\tau} = (X_{i;j;\tau} + M_{i;j;\tau}) = (X_{i;\tau} + X_{j;\tau} + M_{i;\tau} + M_{j;\tau})$$

where $X_{i;j;\tau}$ denotes total nominal exports from country i to country j cumulated over period τ, $X_{i;\tau}$ denotes total global exports from country i over period τ, and M denotes imports. As in FR, the argument is that higher values of, say, $wt_{i;j;\tau}$ are indicative of greater trade intensity between countries i and j. There is a trade-off in choosing τ: the number of observations increases with the number of periods, but the accuracy of the estimated correlations diminishes with it. We choose to run our estimations for $\tau = 3; 4$ and periods of equal length[8].

[6] Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Norway, the Netherlands, New Zealand, Portugal, Spain, Sweden, Switzerland, the UK and the US.

[7] Output is assumed to be derived from a Cobb-Douglas production function taking capital and labor stocks as inputs. We assume constant returns to scale, and thus take aggregate payments to labor averaged over time as the empirical counterpart to the labor elasticity of output. Capital series are constructed using the permanent inventory method.

[8] This is clearly arbitrary. However, insofar as the paper brings focus on the importance of effects that remain constant over time, the choice of sub-periods is probably not substantial to the conclusion.

2.2 Econometric Issues

Consider the equation to estimate

$$\rho_\tau = \alpha_0 + \alpha_1 WT_\tau + \alpha_2 X_\tau + \varepsilon_\tau \tag{1}$$

where ρ_τ denotes GDP correlations over τ and X_τ contains other variables whose effect ought to be controlled for. The absence of indices i or j reflects the fact that all pairs of countries are stacked and included in the estimation. While it is not clear on what ground, FR seem to pay particular attention to the issue of reverse causality in (1), arguing that "countries are likely to link their currencies deliberately to those of their most important trading partners" and "exchange rate stability could cause both high trade and co-ordinated business cycles". In our opinion, the issue is more one of omitted variable, as this paper more generally contends -hence the importance of what variables are included in X_τ. Furthermore, seminal work on international business cycles predicts that countries with synchronized cycles would if anything trade relatively less[9], thus making it difficult to see how a positive α_1 could stem from reverse causality. However, for the sake of comparison we shall attempt to correct for this hypothetical bias. FR do it by making use of the now well-documented fact that geography is a powerful determinant of bilateral trade[10], a common justification to use geographic considerations, as summarized by the so-called gravity variables, as instruments for trade in two-stage least-square regressions[11]. Unfortunately, as we argued earlier and elsewhere[12] there are no a priori reasons why geography can be excluded from the right-hand side of (1), thus making it difficult to resort to the same instruments as when investigating the growth effects of trade. In particular, we know already from Figure 2 that the distance variable belongs in X_τ, and thus cannot be used as an instrument for trade, as FR do. Omitting the distance variable -or others, for that matter- from X_τ is at the core of our criticism of FR.

We attempt to correct for the putative reverse causality bias in two ways. First, as it turns out, some gravity variables can actually be excluded from the right-hand side of (1). We use them as instruments. Second, we take

[9] This is for instance the case in Backus, Kehoe and Kydland (1995).

[10] See for instance Deardorff (1984), Frankel, Stein and Wei (1995) or Frankel (1996).

[11] See for instance Frankel and Romer (1996). There, they argue that at least some components of trade corresponding to geographic considerations are unlikely to be correlated with the left-hand side variable, in their case output growth.

[12] See Imbs (1998b).

advantage of the panel structure of the data and use lagged values of the right–hand side variables, at least six years older when ¿ = 4. We therefore estimate:

$$½_{¿} = ®_0 + ®_1 WT_{¿ ¡ 1} + ®_2 X_{¿ ¡ 1} + "_{¿} \quad (1')$$

Next section shows that correcting for endogeneity bias modi…es very little the estimates of $®_1$.

More importantly, omitting the distance variable from $X_{¿}$ generates a potentially important omitted variable bias, whereby positive estimates of $®_1$ are spuriously high. One can indeed think of a wide range of variables potentially associated with synchonized business cycles, that could also cause intense bilateral trade. Examples include geographical, institutional or historical considerations, such as proximity, the presence of a mountain range, a history of war or membership to international agreements. In Imbs (1998b) we take that issue seriously, and investigate in detail what causes cross-sectional variation in GDP correlations. Su¢ce it to say for the present purpose that $X_{¿}$ potentially contains a large number of variables, whose omission is liable to make $®_1$ very positive. A rough way to correct for this bias is to assume $X_{¿} = X$, i.e. omitted variables remain constant over time[13]. Under that -fairly undemanding- assumption, omitted variable bias can be accounted for using a …xed-e¤ect estimation, where …xed e¤ects are speci…c to each country-pair. In other words, estimate

$$½_{¿} ¡ ½_{¿ ¡ 1} = ®_1 (WT_{¿ ¡ 1} ¡ WT_{¿ ¡ 2}) + ´_{¿} \quad (1'')$$

where the …rst-di¤erences on the right-hand side are lagged, so as to still account for potential reverse causality bias. None of the FR results resist to estimation of (1”). We proceed with a detailed discussion of the results.

3 Accounting for Fixed E¤ects

The body of the text presents results using GDP correlations, split into three sub-periods. In section 4, we con…rm our …ndings carry through using bilateral correlations of employment and industrial production instead, and

[13] In the absence of a proper theory directing the choice of variables, the more subtle alternative to include all the relevant variables on the right-hand side and check whether the estimated coe¢cients remain stable, is unpleasantly arbitrary

four sub-periods. Table 1 develops FR point. Whether trade enters in level or in logarithm, and after controlling for some geographic considerations and monetary arrangements[14], trade partners are more synchronized. Furthermore -careful- instrumental variables estimation[15] in specification (iv) does not alter the claim. Nor does estimation with lagged right-hand side variables in specification (v): overall, given the relatively similar magnitude of point estimates in (iii)-(v), it is safe to claim reverse causality is not an important issue in estimating (1). We want to argue that the same is not true of fixed effects.

To be meaningful, estimation of (1") requires sufficiently low serial correlation in both ρ_τ and WT_τ, so that they keep some information even in first-differences. Figures 6 and 7 make a first pass at ensuring this is the case for GDP correlations: the plots of ρ_τ against $\rho_{\tau-1}$ for $\tau = 2; 3$ show reasonably little serial correlation, with coefficients of correlation of respectively 0.18 and 0.24. Such is however not the case for the trade variable, that displays significant serial correlation as evident from Figures 8 and 9. This is why estimations involving WT_τ and $WT_{\tau-1}$ yield quite similar results in Table 1, however without serious consequence since there is not much a reverse causality bias to control for. This is to be expected given the fact that trade intensity is largely determined by time-invariant factors, precisely those to be accounted for to the extent that they impact GDP correlations. Given discrepancies in the serial correlations of the two variables differenced in the fixed effect estimation, it is quite possible that too much information is thrown away and variables in (1") are close to white noise. As long as theory does not help the empirical researcher select what variables are omitted from (1), this is unfortunately the only -rudimentary- way to go[16].

Estimation of (1") is presented in Table 2. The coefficient under investigation asks whether more trade intensity impacts subsequently the extent of cycle synchronization. The answer in Table 2 is a resounding no, which stands in stark contrast with previous results. Estimations in first differences

[14] The ER Regime dummy variable equals one if the regime is fixed, and is averaged over the period τ. Its inclusion is motivated by FR argument that the relation between trade and co-fluctuations could spuriously stem from monetary arrangements.

[15] Instruments are Adjacency and Language, which can be excluded from equation (1), as shown in specification (iii).

[16] The next section actually presents results where a fixed effects estimation with the same variables generates significant positive point estimates, thus making it dubious that first-differences be merely noise.

have however the undesirable feature that they tend to magnify measurement error, a problematic fact given the dependent variable. Bilateral GDP correlations are indeed measured with error: specification (ii) in Table 2 restricts therefore the data to those correlations ρ_τ that are significant at the 10% level at least in all three sub-periods, in an attempt to minimize the inaccuracy in (i). Results show no improvement. Thus, it is quite possible that the channel from trade to cycle synchronization be indirect, and the claim that a monetary union will lead to more conformity in business cycles by stimulating exchanges might well be based on spurious grounds.

4 Structural Co-Fluctuations and the Effect of Exchange Rate Regime

4.1 Trade and Structural Fluctuations

Even if there were evidence of a positive impact of bilateral trade on output correlations, it would not have anything to say about how synchronized output fluctuations would be -or would have been, for that matter- in the absence of independent monetary policy. It would merely tell us fluctuations of aggregate activity become more similar in trading countries, for reasons that remain undetermined. In particular, trade could be affecting TFP correlations, or the synchronization in technological developments between trading partners, as well as movements in inputs driven by other factors than technology. The former effect exists irrespective of the monetary regime, the latter could be arising from country-specific monetary developments and thus disappear in a currency union. Results obtained when replacing output correlations with TFP correlations are presented in the left panel of Table 3. Interestingly, as apparent from specifications (i)-(iii), trade partners seem to experience similar technological developments, as measured by TFP, and this even after controlling for other geographic variables[17]. However, the result does not resist to the use of lagged independent variables, as evident from (iv). In light of the very high serial correlation in the trade variable, at the very least this casts doubt on the robustness of results in (i)-(iii).

The right panel in Table 3 confirms those results using measures of labor productivity instead. Insofar as the effects of monetary policy are presum-

[17] If not spurious, this result can be related to Coe and Helpman (1995), where it is shown that the degree of openness matters in the diffusion of technological developments.

ably short-lived, they are likely to be reflected by movements in the labor input, and thus filtered from labor productivity. On the other hand, labor productivity fluctuates with cyclical movements in the capital stock, which presumably correspond to developments of a non-monetary nature. It may therefore provide a reasonably good account of how cycles would look in the absence of monetary policy. As before however, the impact of trade disappears when other factors are accounted for.

4.2 Effects of the Exchange Rate Regime

Providing decisive evidence about the effects of the exchange rate regime on trade is one of the most elusive endeavor in empirical international economics. The method here provides however a potentially fruitful approach, as it focuses on bilateral variables rather than mere cross-country evidence. Thus we want to estimate

$$WT_\tau = \beta_0 + \beta_1 \rho_\tau + \beta_2 ER_\tau + \beta_3 X_\tau + \varepsilon_\tau \tag{2}$$

There is undoubtedly a serious issue of endogeneity for the right-hand side variables in (2). Given the evidence in this paper, β_1 probably suffers less from it than β_2; as FR themselves argue, there is every reason to believe that it is mostly between trade partners that fixed exchange rate regimes are agreed on, thus resulting in a high value for β_2 but with causality going the reverse way. Finding legitimate instruments is however difficult because of the presence of a trade variable on the left-hand side of (2), thus forbidding the use of any so-called gravity variable, that will belong in X_τ. As before the issue is bypassed by using lagged values of the right-hand side variables. In particular, serial correlation in the exchange rate regime variable is reassuringly low, as apparent from Figure 10. Given the substantial differences between ER_τ and $ER_{\tau-1}$, it can be argued on reasonably safe grounds that causality does not go from WT_τ to $ER_{\tau-1}$. Table 4 presents estimates for (2) both with contemporaneous and lagged values of the independent variables. Regressions (i) and (ii) show a significant effect of the exchange rate regime, even after controlling for geographic variables. Regressions (iii) and (iv) confirm the finding with lagged values of ρ_τ and ER_τ, though with substantially smaller point estimates. We interpret this as evidence that β_2 is indeed plagued by a reverse causality bias in (2), but the effect of exchange rate arrangements on trade still prevails after accounting for it.

However, estimates in specifications (iii) and (iv) potentially fall victim to the same fixed effect criticism developed in the previous section. In particular, some variables omitted from X_τ could affect simultaneously WT_τ, ρ_τ and ER_τ. This would for instance be the case if geographic, historical or political considerations, say, have an impact on exchange rate arrangements as well as on WT_τ and ρ_τ as argued previously[18]. Specification (v) runs (2) in first differences for all observations available on WT_τ, ρ_τ and ER_τ. No coefficient comes out significantly, but as argued earlier, fixed effects estimations tend to exaggerate measurement error. Specification (vi) therefore limits the sample to pairs of countries for which ρ_τ is significant at the 10% level at least. Quite strikingly, for these pairs the exchange rate regime has a significant and positive effect on trade. This is an interesting result in at least two ways: first it provides some evidence in favor of real effect of the monetary regime, second and more importantly, it confers more credibility to the other regressions estimated in first differences in this paper: even though WT_τ is very persistent, it remains meaningful even in growth rates.

4.3 Sensitivity Analysis

In this section, we check that the previous results are robust along two margins: first we split the sample in four instead of three sub-periods. Second, we use different measures of the cycle, namely industrial production and employment. In Table 5, regressions (i) and (ii) show that the fixed effects argument carries through: if more trade results apparently in more synchronized cycles, it is mostly due to omitted time invariant variables. Regressions (iii) and (iv) confirm that trade has no effect on "structural" cycle synchronization, as measured by TFP correlations. Finally, specifications (v) and (vi) cast doubt on the robustness of the finding developed in the last section, that monetary arrangements have real effects on trade. In particular, even for a subsample of significant GDP correlations, the exchange rate regime does not impact the level of bilateral trade. Finally, Table 6 reproduces fixed effects estimations with bilateral correlations of indutrial production and employment on the left-hand side. Bilateral trade never plays a significant role.

[18] Thus, it is for instance striking that Switzerland should remain outside of EMU, when every single one of its neighbors committed to it. Political considerations of neutrality, to a first approximation constant over time, probably play a role in explaining why.

5 Conclusion

There are three reasons why the claim that bilateral trade results in synchronized business cycles ought to be qualified. First, both variables may very well respond simultaneously to other factors, potentially time invariant because of a geographic or institutional nature, but omitted from the estimations underlying the argument. Second, is it really the extent of synchronization as would prevail in the absence of independent monetary policy that reacts to bilateral trade? If not, it is difficult to interpret this evidence in the context of a monetary union. Finally, despite the omnipresence of the hypothesis, it is far from clear that a fixed exchange rate regime will stimulate exchanges. In this paper, we provide evidence that these three qualifications should be taken seriously. From the perspective of ex-posts effects of a currency union, the picture that emerges is mixed: while a fixed regime might indeed result in more trade, and thus increase the desirability of a single currency, the intensity of bilateral trade, in turn, will not impact in any way the extent of cycle synchronization. From the perspective of empirical international economics, the fact that time invariant factors may explain an important part of the cross-sectional variation in GDP correlations calls for a detailed study of the determinants of co-fluctuations.

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Figure 1: GDP Correlations and Bilateral Trade

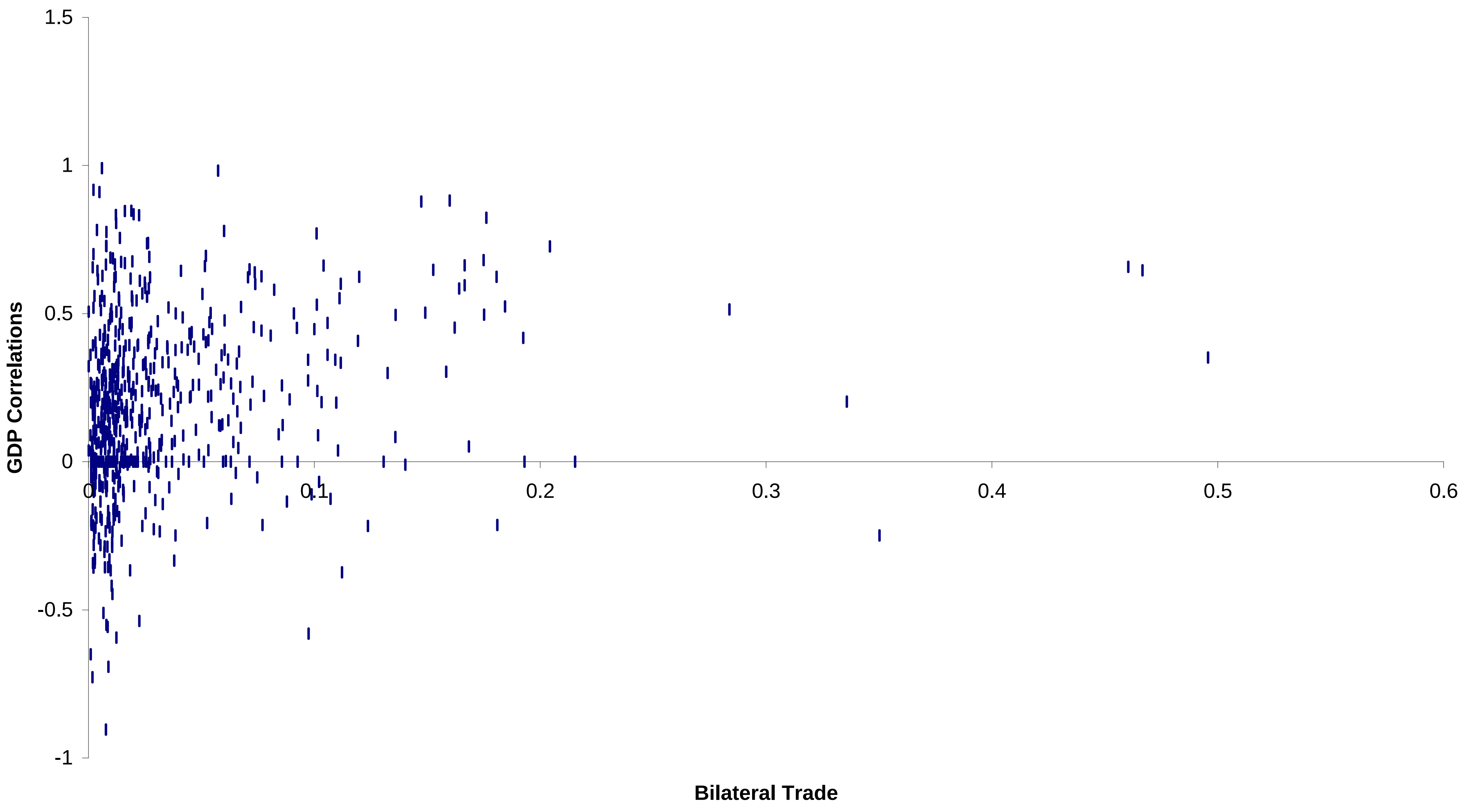

Figure 2: GDP Correlations and Distance

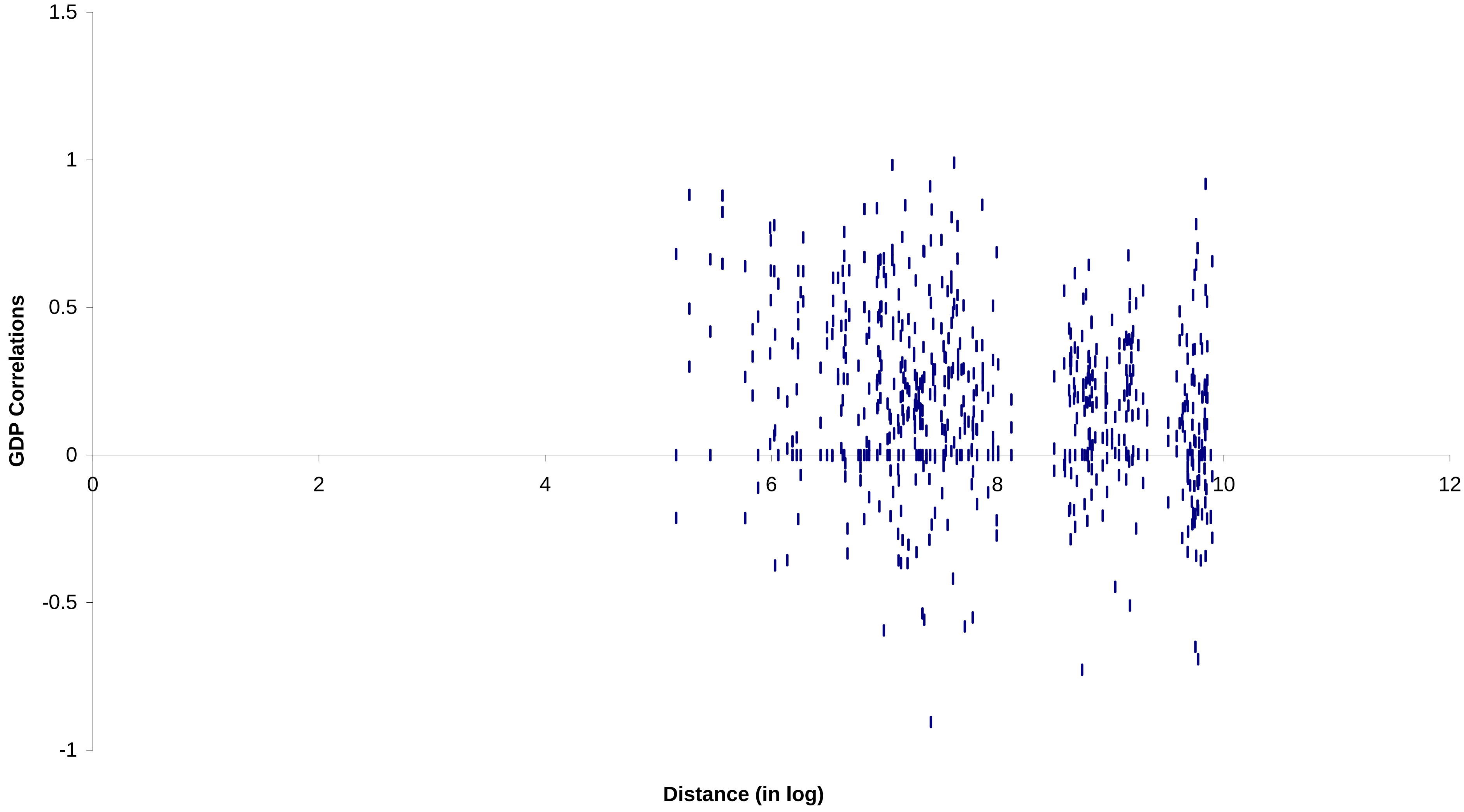

Figure 3: Bilateral Trade and Distance

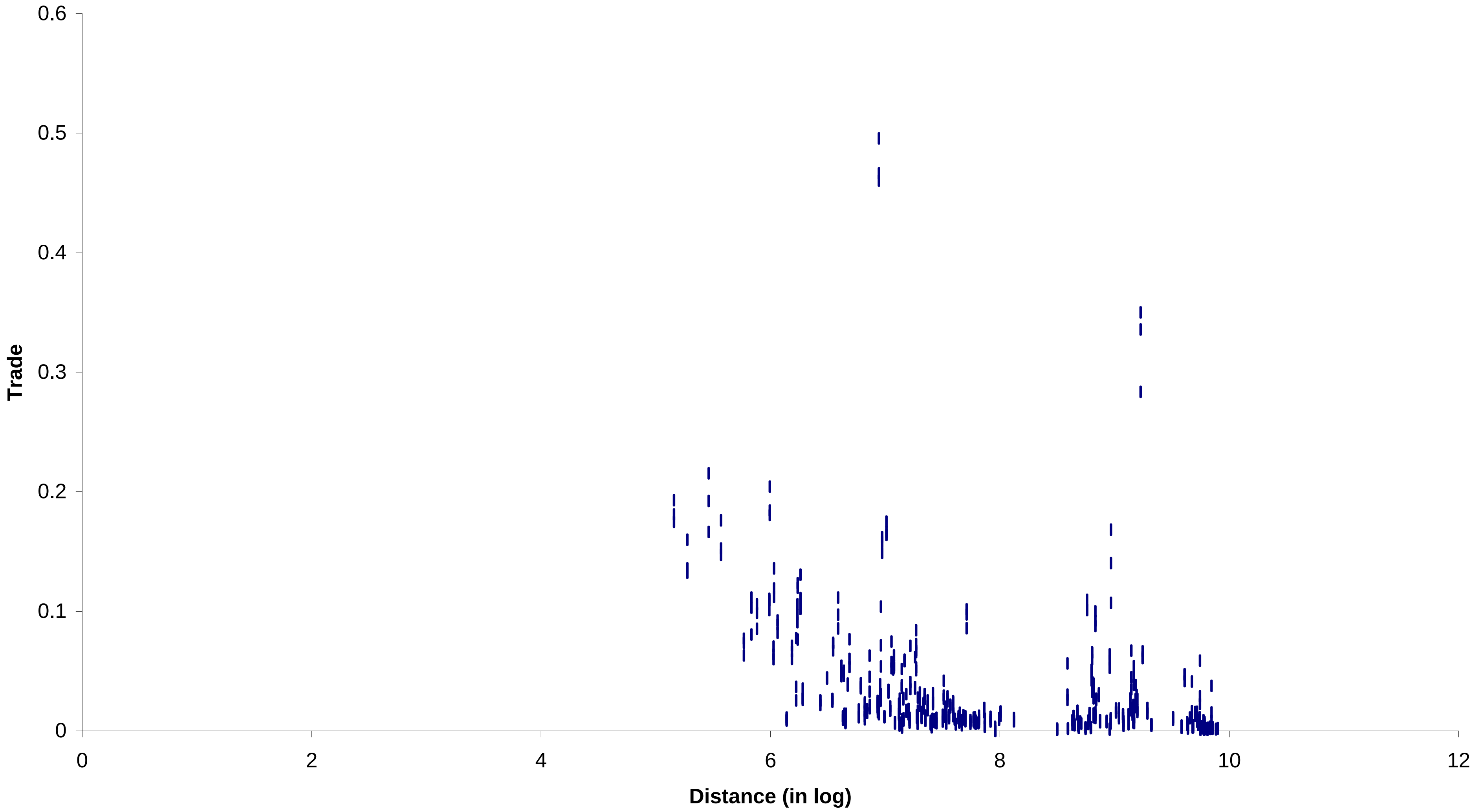

Figure 4: TFP Correlations and Trade

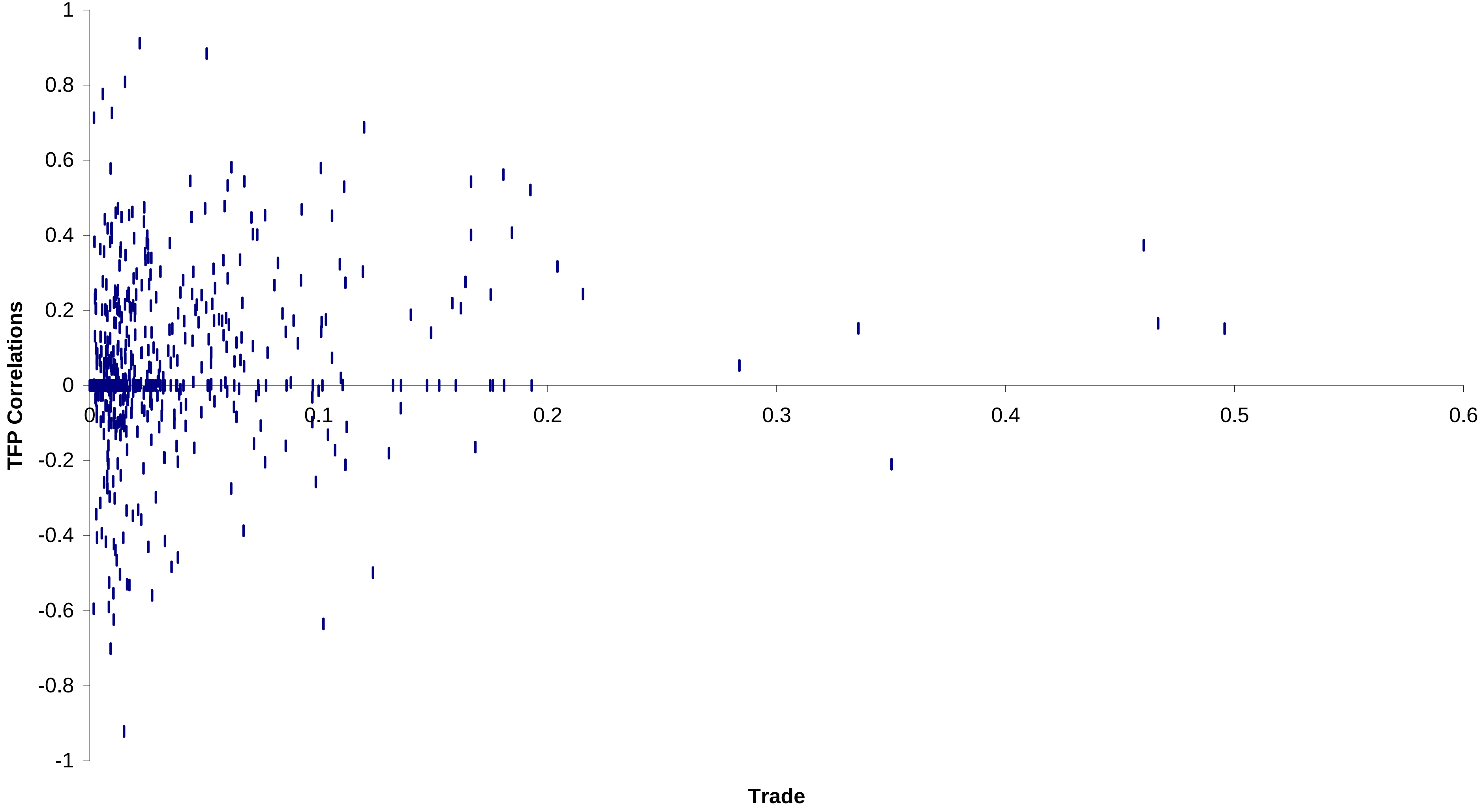

Figure 5: Labor Productivity Correlations and Trade

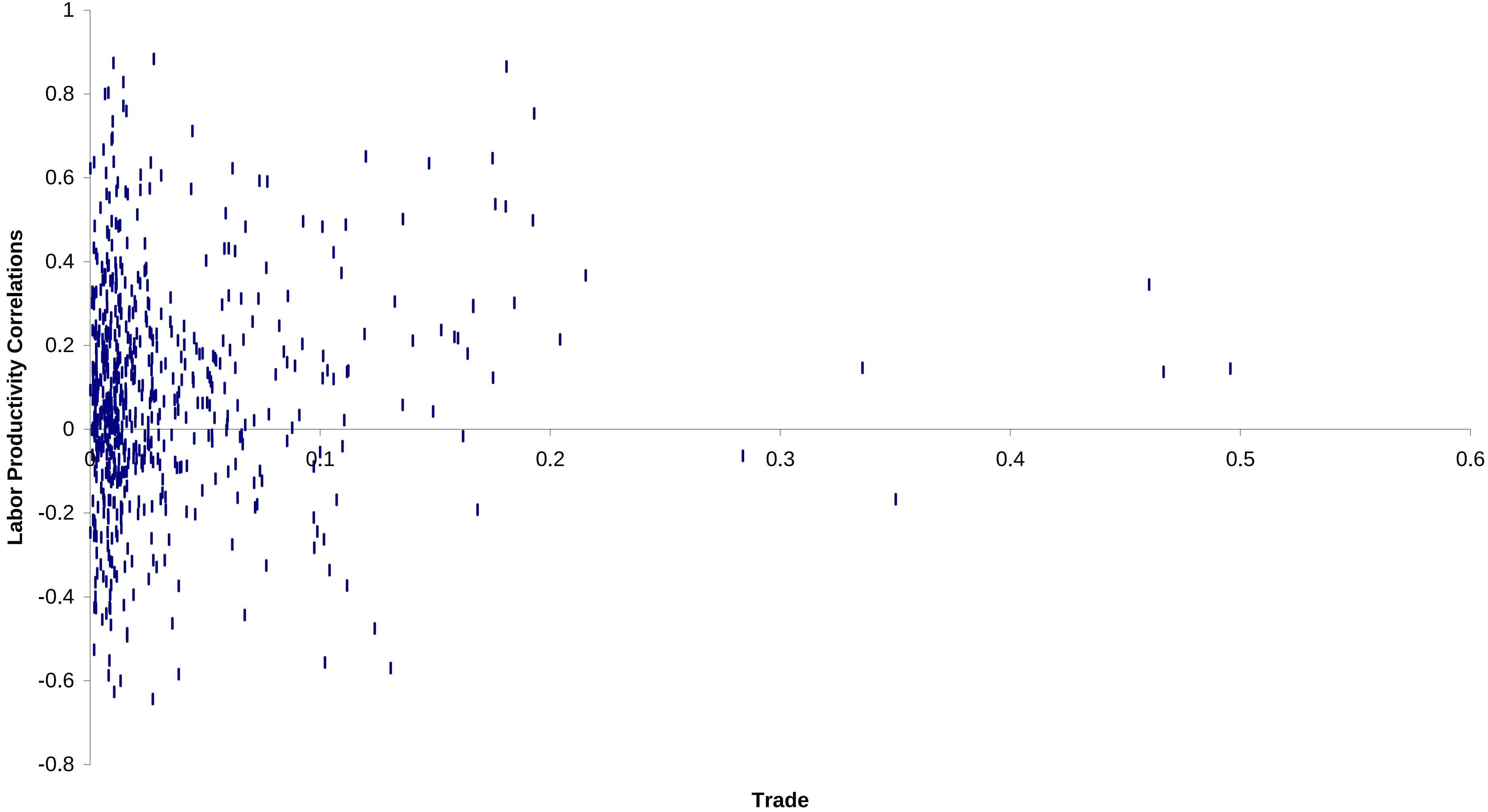

Figure 6: Serial Correlation in GDP Correlations

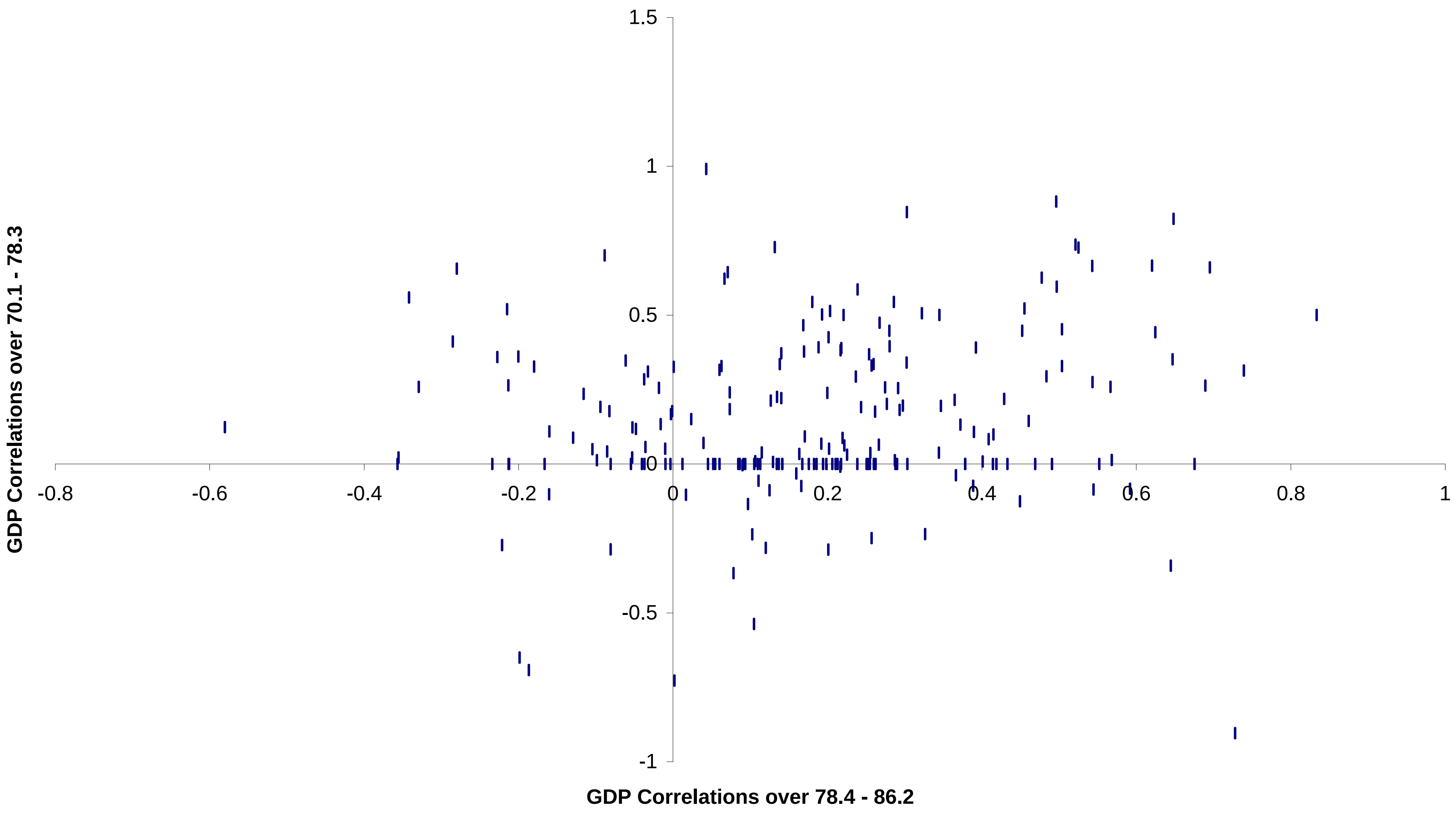

Figure 7: Serial Correlation in GDP Correlations

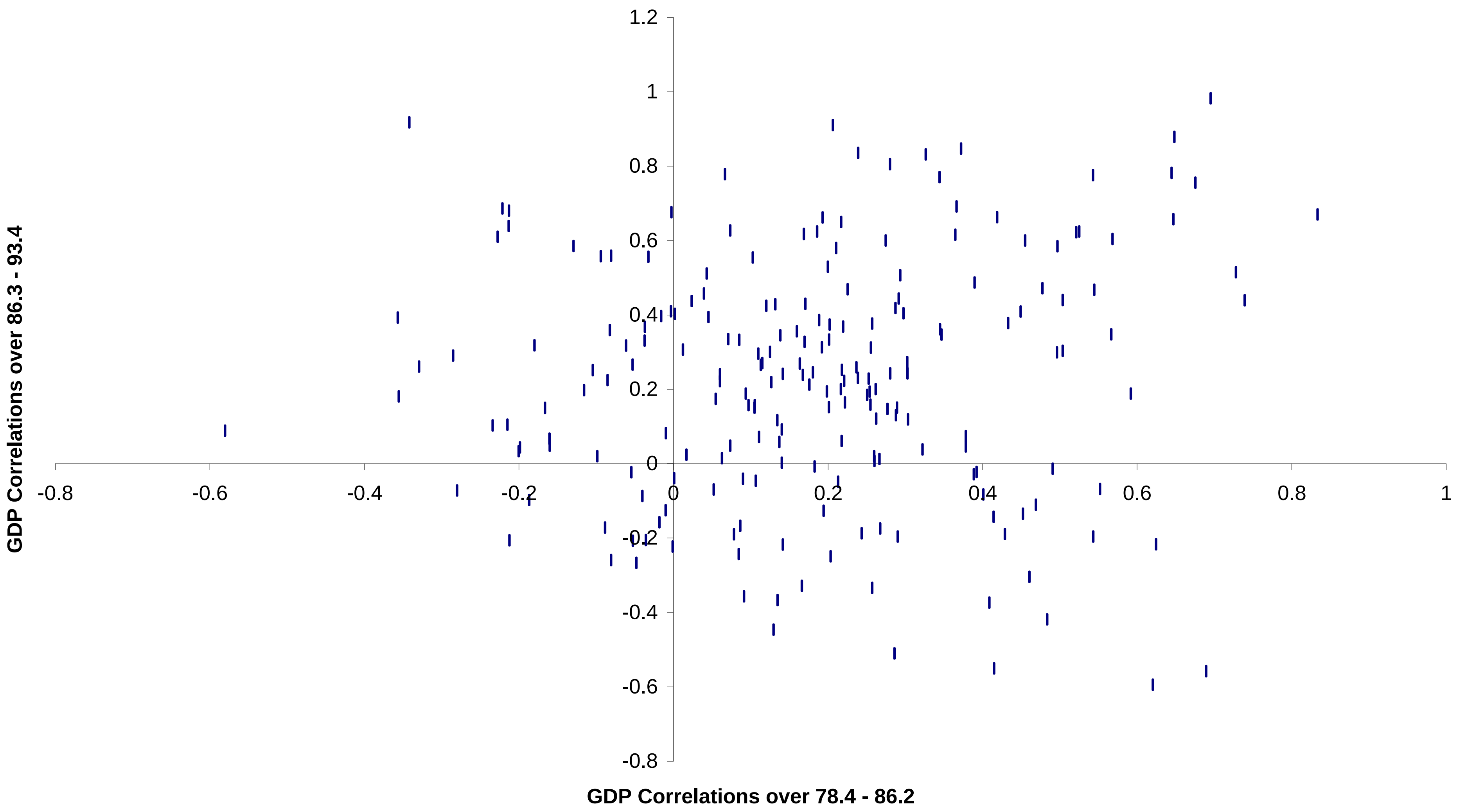

Figure 8: Serial Correlation in the Trade Variable

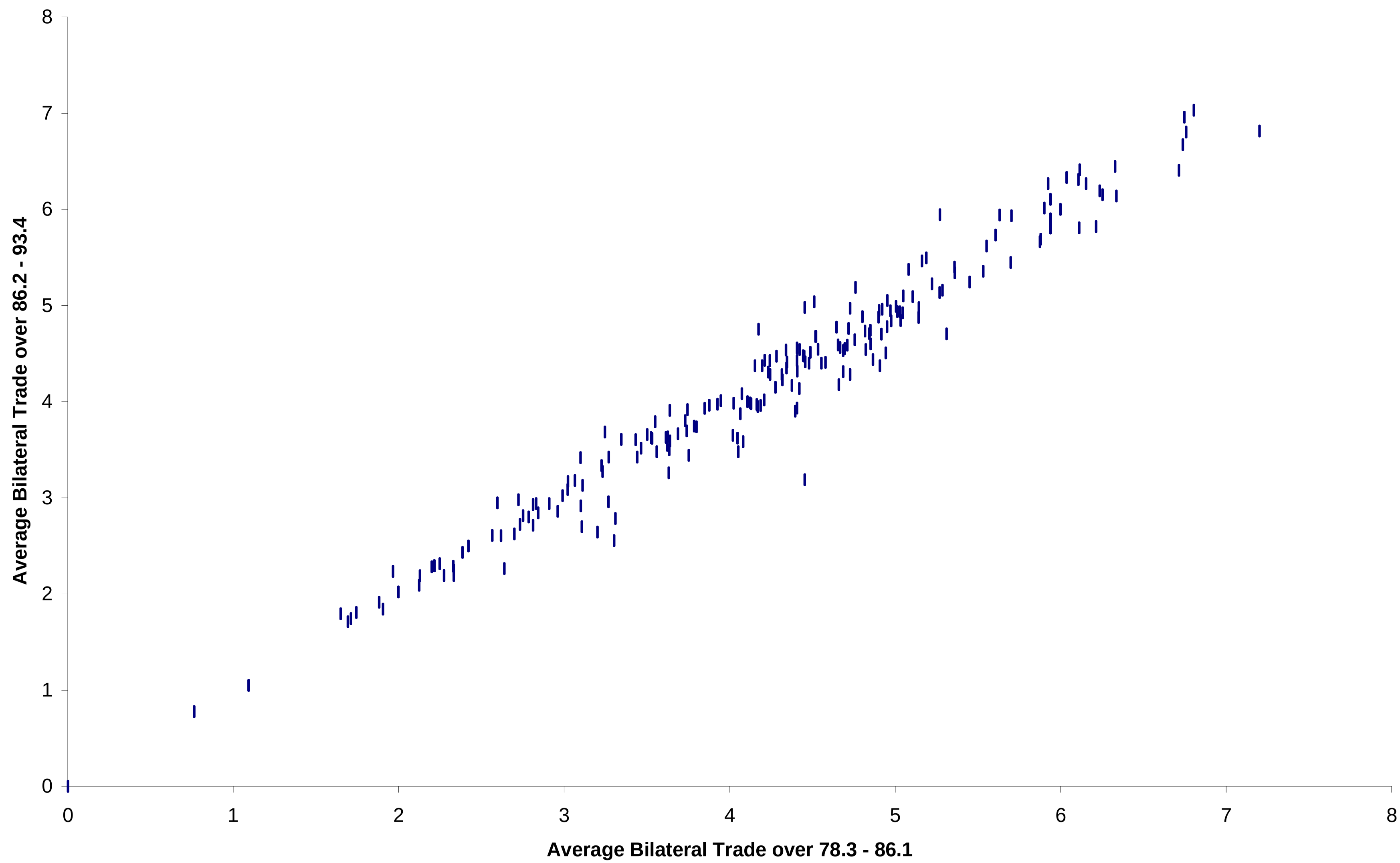

Figure 9: Serial Correlation in the Trade Variable

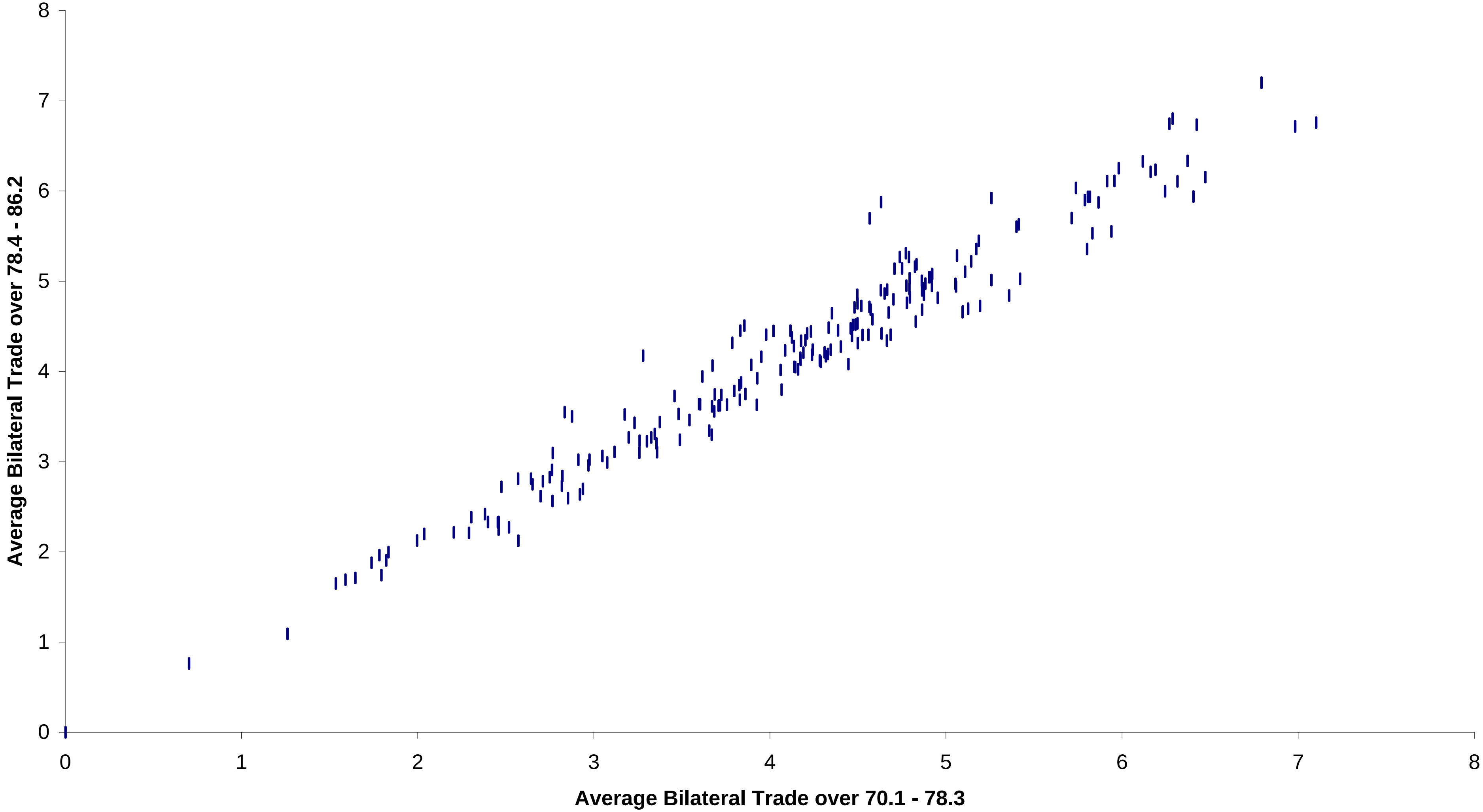

Figure 10: Serial Correlation in the Exchange Rate Variable

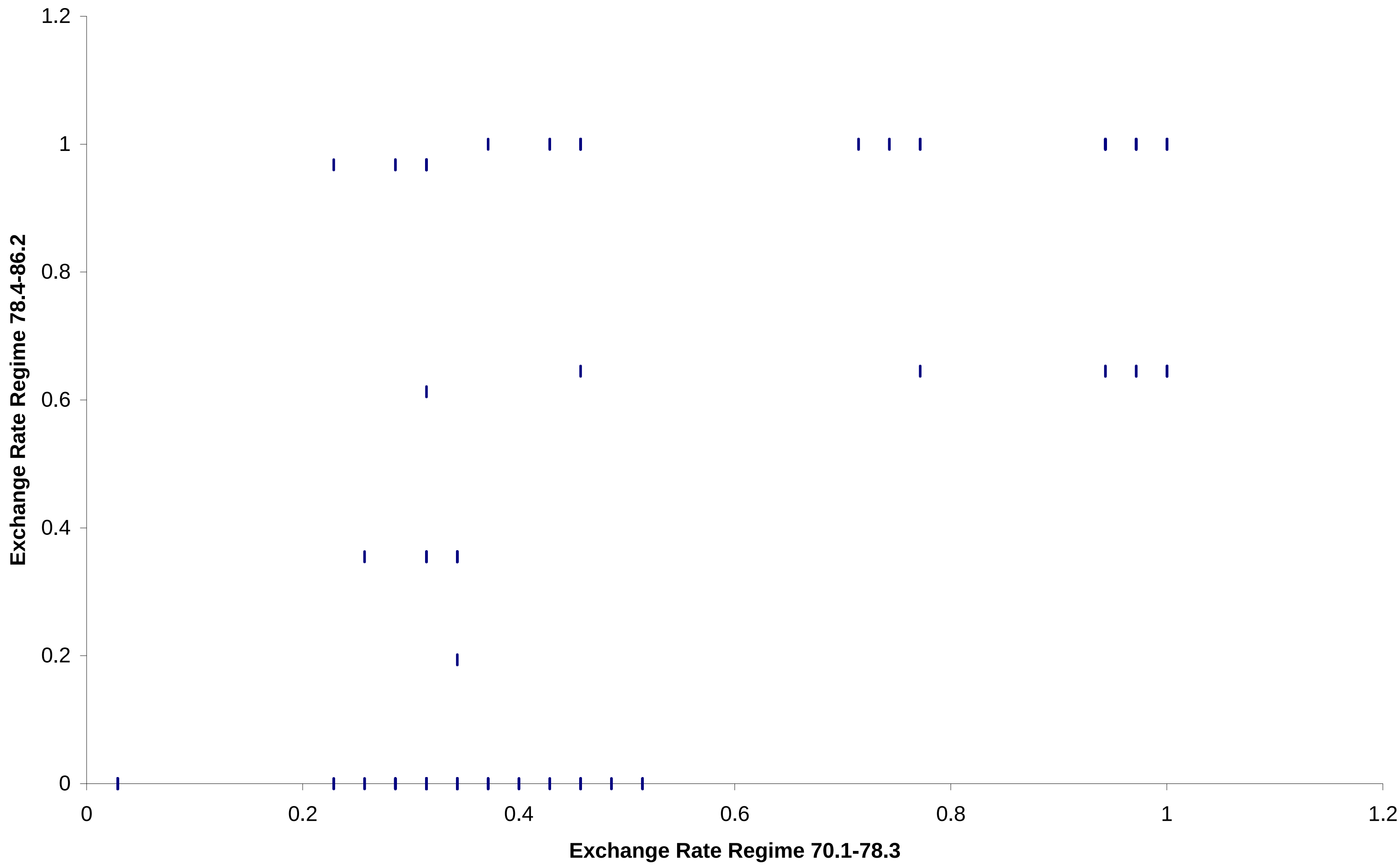

Table 1: OLS and Reverse Causality Bias

	(i)	(ii)	(iii)	(iv)-IV	(v)-LAG
Constant	0.163**	0.481**	0.382**	0.634**	0.418**
	[11.09]	[11.33]	[6.99]	[7.38]	[8.17]
Trade	1.197**				
	[4.48]				
ln(Trade)		0.067 **	0.040**	0.103**	0.039**
		[9.71]	[3.16]	[5.04]	[3.01]
ER Regime			0.027		
			[0.87]		
Distance			-6.16×10^{-6}**		-8.75×10^{-6}**
			[2.57]		[3.33]
Adjacency			0.069		
			[1.36]		
Language			0.060		
			[1.56]		
N Obs	628	628	628	628	418
R-Square	0.047	0.078	0.099	0.054	0.092

Dependent variable is GDP correlation. Huber-White t-statistics reported between brackets. Distance is the kilometric distance between main cities, Adjacency takes value one if countries have a common border, and Language takes value one if countries share the same language. (iv) runs IV using adjacency and language as instruments. (v) runs the estimation using "lagged" values of the RHS variables.

Table 2: Fixed Effects Estimations

	(i)	(ii)
Growth in Trade	0.081	0.235
	[0.79]	[0.43]
N Obs	208	28
R-Square	0.003	0.010

Dependent variable is growth in GDP correlations, between 78.4-86.2 and 86.3-93.4. Regression (i) uses the log of bilateral trade growth between 70.1-78.3 and 78.4-86.2. Regression (ii) uses only GDP correlations significant at the 10% level in all three sub-periods.

Table 3: Structural Effects of Trade

	Total Factor Productivity				Labor Productivity	
	(i)	(ii)	(iii)	(iv)-LAG	(v)	(vi)
Constant	0.222**	0.202**	0.161**	0.117*	0.173**	0.097**
	[4.77]	[4.07]	[2.94]	[2.01]	[4.63]	[2.45]
ln(Trade)	0.040**	0.035**	0.027*	0.014	0.021*	0.004
	[3.36]	[2.60]	[2.01]	[1.09]	[2.54]	[0.44]
ER Regime		0.028				0.091**
		[0.76]				[3.01]
Distance		-1.41×10^{-6}				-4.35×10^{-6}*
		[0.55]				[2.27]
Adjacency			0.084	0.132*		
			[1.64]	[2.29]		
Language			0.017	-0.041		
			[0.47]	[0.94]		
N Obs	408	408	408	272	627	627
R-Square	0.029	0.031	0.038	0.053	0.010	0.035

Table 4: Effects of the Exchange Rate Regime

	OLS		Lagged RHS		First Differences	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Constant	-4.58**	-4.09**	-4.57**	-4.07**		
	[64.04]	[54.71]	[48.53]	[41.13]		
GDP Correlations	1.09**	0.459**	1.39**	0.627**		
	[6.01]	[3.14]	[5.59]	[3.11]		
Growth in GDP Correlations					0.001	0.083
					[0.03]	[1.82]
ER Regime	0.656**	0.456**	0.416**	0.350*		
	[5.28]	[4.07]	[2.67]	[2.48]		
Growth in ER Regime					-0.023	0.183*
					[0.50]	[2.47]
Distance		-7.85×10^{-5}**		-8.23×10^{-5}**		
		[11.53]		[9.35]		
Adjacency		1.30**		1.27**		
		[10.11]		[7.82]		
Language		0.220		.0186		
		[1.43]		[0.98]		
N Obs	570	570	361	361	152	24
R-Square	0.118	0.409	0.113	0.406	0.001	0.27

Dependent variable is bilateral trade. Regression (vi) is estimated using the sub-sample of GDP correlations significant at the 10% level.

Table 5: Sensitivity Analysis – 4 periods

	GDP – First Differences		Lagged TFP		Trade – First Differences	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Constant			0.146**	0.125*		
			[3.16]	[2.50]		
Trade Growth	-0.012	-0.392				
	[0.13]	[0.86]				
ln(Trade)			0.021	0.018		
			[1.83]	[1.59]		
ER Regime			0.035			
			[1.06]			
Growth in ER Regime					0.023	0.018
					[0.61]	[0.22]
Growth in GDP Correlations					0.001	-0.054
					[0.05]	[0.81]
Distance			-3.12×10^{-6}			
			[1.39]			
Adjacency				0.096*		
				[2.10]		
Language				-0.044		
				[1.15]		
N Obs	418	38	408	408	342	38
R-Square	0.000	0.017	0.028	0.036	0.001	0.029

Specifications (i) and (ii) check that effects of trade on GDP correlations do not resist fixed effect estimation, even when only significant GDP correlations are included, as in (ii). Specifications (iii) and (iv) check that trade has no effect on correlations of TFP. Specifications (v) and (vi) check the effects of ER regime on trade in first differences.

Table 6: Sensitivity Analysis – Alternative Measures of the Cycle

	Industrial Production			Employment		
	(i)	(ii)-LAG	(iii)-FE	(iv)	(v)-LAG	(vi)-FE
Constant	0.412**	0.358**		0.180**	0.172**	
	[10.20]	[9.51]		[4.17]	[3.64]	
Trade Growth			0.103			0.059
			[1.59]			[0.64]
ln(Trade)	0.063**	0.056**		0.023*	0.030**	
	[6.83]	[6.09]		[2.37]	[3.15]	
ER Regime	-0.016			0.090**	0.182**	
	[0.72]			[2.72]	[4.56]	
Growth in ER Regime						
Growth in GDP Correlations						
Distance	4.43×10^{-6}*	7.01×10^{-7}		-1.37×10^{-7}		
	[2.37]	[0.43]		[0.68]		
Adjacency	0.025			0.025		
	[0.67]			[0.58]		
Language	0.039			0.062	0.015	
	[1.18]			[2.11]	[0.43]	
N Obs	627	418	209	627	418	209
R-Square	0.106	0.113	0.012	0.047	0.094	0.002