Estimating the impact of currency unions on trade using a dynamic gravity framework

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Using a Dynamic Gravity Framework

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Abstract

This paper revisits the early time series estimates of currency unions on trade from an historical perspective using a dynamic gravity equation and by conducting in-depth case studies of currency union breakups. The early large estimates were driven by omitted variables, as many currency union exits were coterminous with warfare, communist takeovers, coup d'etats, genocide, bloody wars of independence, various other geopolitical travesties, or were predated by trade collapses. Static gravity estimates are found to be sensitive to controlling for these omitted variables, while a dynamic gravity specification implies that currency unions do not increase trade.

JEL Classification: F15, F33, F54

Keywords: Currency Unions, Trade, Dynamic Gravity, Decolonization

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A key policy decision for many nations is the question of whether or not to join a currency union (CU). Hence, it is not surprising that a large body of research in International Macro in recent years has revolved around the impact of CUs on trade.[1] What is surprising, however, is the magnitude of the measured increase, as Rose (2000), Glick and Rose (2002), Barro and Tenreyro (2007), and Alesina, Barro, and Tenreyro (2002) have found that CUs increase trade on a 3-fold, 2-fold, 7-fold and 14-fold basis, with at least another 70 papers finding a large and significant impact. A recent example in this literature is Melitz, Helpman, and Rubinstein (2008), which confirmed that the impact of CUs on trade is surprisingly large, and robust to various bias-correcting estimation methods.[2] Given the importance of trade for development, these findings imply that the impact on trade should be an important consideration in assessing the welfare benefits of joining, or leaving, a currency union. And so a technocrat on the European periphery – such as in Greece – could be forgiven for fearing that leaving the Euro would have have a large negative effect on trade and thus welfare.

Yet, since the Euro's inception, it has become clear that the larger estimates of the Rose Effect—named for the discoverer of the puzzling, enormous apparent effect of CUs on trade—did not seem to explain European trade patterns. Neither Berger and Nitsch (2008) nor Santos Silva and Tenreyro (2009) found any effect of the Euro on trade, while Havranek's (2010) meta-analysis found that the mean impact of the Euro on trade was 3.8% versus over 60% for earlier non-Euro episodes. Moreover, Havranek found systematic evidence of publication bias for the Euro studies, while several more recent studies have found little-to-no effect.[3]

One possibility is that the differing results may reflect the declining importance of currency unions in an era of ATMs, internet banking, credit cards, and electronic transactions.[4] While technological innovations in moving money internationally may well have reduced the trade impact of currency unions, the evidence presented here suggests that the earlier large estimates are not robust to altering estimation techniques or controlling for various omitted variables. Many scholars, including Rose himself,[5] have expressed doubt about the earlier large estimates of the CU effect, and there have also been many insightful critiques of the early estimates of common currencies on trade. These include studies by Persson (2001), Nitsch (2005) on currency union entries, Berger and Nitsch (2005), Bomberger (2003), Bun and Klaassen (2007), and Baranga (2009) which succeed in reducing the size of the impact, increasing the error bounds, or eliminating the effect altogether for select subsamples or for later time periods (*i.e.*, Nitsch [2005] for entries, Klein [2005] for dollarization episodes, and Baranga [2009] arrives at a small positive point estimate with an IV for a later time period than the Glick-Rose dataset). Yet, to-date there has yet to be a full accounting for why the early time series estimates of CUs on trade were so deceptively large for the entire Glick and Rose – henceforth GR – (2002) dataset.

The gravity literature has generally employed "static" gravity models to ascertain the impact of currency unions on trade flows using panel data sets, although trade scholars have long known that dynamics are important in trade. This paper follows Bomberger (2003), Nitsch (2005), Bun and Klaassen (2007) and Qureshi and Tsangarides (2011) in applying a dynamic approach, and adds to this literature by showing that dynamics can explain the time series impact of currency unions on trade for the entire GR (2002) sample.

This paper also differentiates itself from other CU papers considering dynamics by drawing upon the intuition of many economists that there are other omitted variables, as monetary unions are generally meant to be forever and so are unlikely to come apart for trivial reasons. In Thom and Walsh's (2002) case study of the UK-Ireland CU dissolution, the authors mention that all five of Portugal's former colonies ended their common currencies after a coup d'etat in Portugal and wars of independence followed by civil wars and communist takeovers, and that several of France's colonies also dissolved their currencies at the denouement of bitter struggles for independence. For these examples, the CU dissolution was unlikely to have been the main reason trade subsequently declined, yet including country-pair time trends, the method Bun and Klassen (2007) employed, would be a necessary but not sufficiently appropriate control, which may have lead them to find that currency unions increase trade by a still sizeable 25%, and precisely estimated.

Hence, instead of limiting the sample like other researchers have done in order to reduce the magnitude of the impact, I collected more data, researching each of the CU entrances and exits in search of omitted variables. The multifarious causes of CU breakups include warfare, communist takeovers, coup d'etats, geopolitical strife, ethnic cleansing, anti-foreigner rioting, bloody wars of independence, genocide, financial crises, currency crises, severe recessions, and, in addition, CU breakups are strongly correlated with unavailable trade or GDP data and often follow on the heels of equally suspicious trade collapses. And factors such as the gradual trade decline associated with the history of decolonization yielded an interesting insight – that the true gravity relationship is dynamic.

Of the 97 non-colonial country pairs with currency union entrances and exits in the GR (2002) sample, only 73 are left after removing pairs with missing data immediately before or after a currency union entrance or dissolution, and only 55 remain after removing those in which currency unions dissolved at the same time as war broke out or one of the countries in the pair experienced a cataclysmic geopolitical event, such as ethnic cleansing of those who share a common currency. Seventeen of the these dissolutions followed dramatic trade declines, and most of the rest followed recessions (bilateral GDP had fallen in 42 out of 55 cases), yet on average were not followed by trade declines. And there are arguably few or even no clearcut examples where the timing of a trade decline supports the hypothesis that a currency union dissolution caused a sizable decline in trade, while there are dozens of clear counterexamples.

The conclusion is not, however, that currency unions do not tend to promote trade.

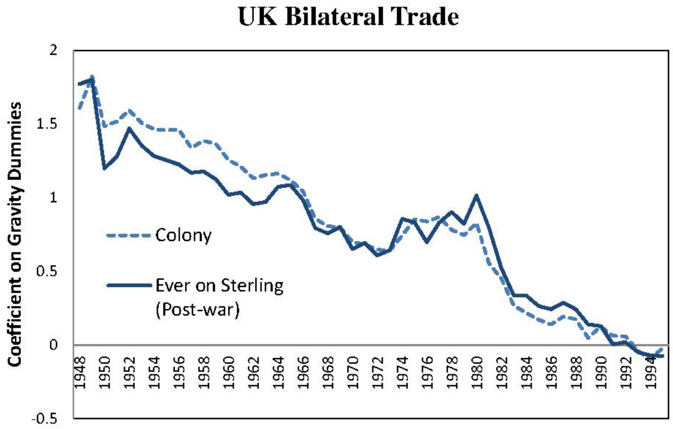
There is a good case to be made that common currencies can moderately reduce the nuisance of international trade and travel, and logic would follow that this would be likely to increase trade – yet it also seems unlikely that currency would be a deciding factor for where to go on holiday or materially alter the number of Volkswagens sold in France. I estimate an impact of CUs on trade close to zero, yet with relatively imprecise error bounds. While other studies have found estimates which lack statistical significance, these studies were either for earlier or later time periods, or divided the GR (2002) sample in some way. For example, Bomberger (2003) examined decolonization, Klein (2002) investigated dollarization, Nitsch (2005) looked at currency union entrances, and Pakko and Wall (2001) predated GR's (2002) time series estimates of currency unions on trade and used a smaller dataset. To the best of my knowledge, this is the first paper which demonstrates that properly accounting for decolonization and other omitted variables can convincingly explain the puzzling large size of the earlier result, although many papers have succeeded at least in reducing the size of the impact or increasing the error bounds.

Aside from the straightforward policy implications of the central finding, the lesson that history matters and therefore that the gravity equation is dynamic has wide applicability. The importance of dynamics are consistent with the results of Nitsch (2008), who used a simple time trend to remove the Euro's trade impact. It can also be interpreted as being consistent with the findings of Bergin and Lin (2010), who found gradually increasing trade intensity in the Eurozone predating the Euro, although the authors interpret this result as indicating that lower expected future trade costs increased trade even before the Euro's inception.

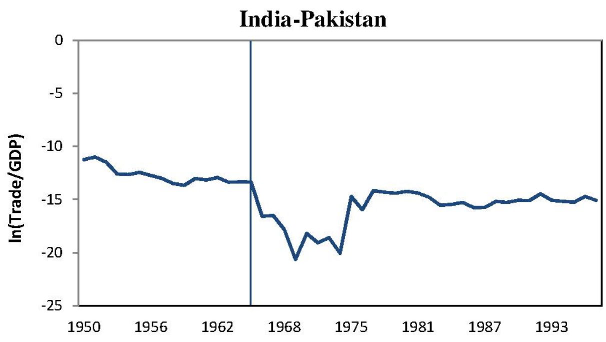
The central result is also consistent with Klein and Shambaugh's (2006) finding that while direct pegs increase trade substantially, the estimated impact is less than for currency unions, and that indirect pegs do not increase trade at all. One interpretation of this result is that indirect pegs are more likely to be random, and less likely to suffer from the same degree of endogeneity and omitted variables bias as are direct pegs, which in turn are likely to suffer less from endogeneity bias as currency unions, since a peg is easier to change. In a clever working paper, Baranga (2011) argues that the adoption of the Euro affords a similar natural experiment, since countries that previously endogenously pegged to a Euro member then found themselves randomly pegged to the other Euro members, and finds no effect of this random peg. While it may be argued that currency unions and direct pegs are seen as more permanent than indirect pegs, the difference in the point estimates is large—Klein and Shambaugh find an impact of 21% for direct pegs versus -1.4% for indirect pegs. That this difference could be reconciled based on expectations of the permanence of the type of peg is not credible given Klein and Shambaugh's other finding—that exchange rate volatility itself is hardly correlated with trade, as going from normal exchange rate volatility to no volatility at all implies an increase in trade of just one or two percent.

The central finding in this paper, that currency unions are not significantly correlated with higher trade flows when we incorporate dynamics, is also consistent with earlier findings on the gold standard era by Meissner and Lopez-Cordova (2003). In the period before World War II, there was a strong cross-sectional correlation between trade and sharing a currency, but the results were no longer significant when fixed effects were included (to correct for reverse causality, Baldwin and Taglioni's [2006] "gold metal error"), with the measured impact less than the standard error.

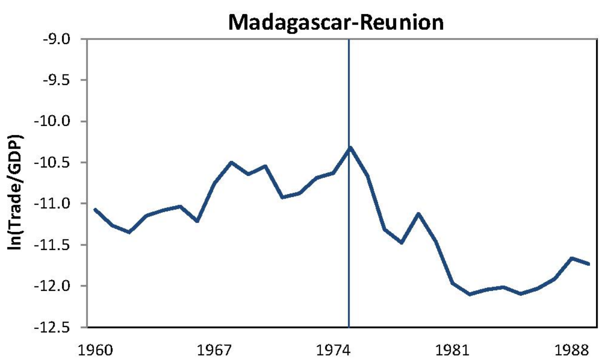
The basic insight for why dynamic estimates can yield such differing results can be seen in the graph below comparing the UK's trade with all of its colonies to those countries with which it started the period sharing a currency union (most of these countries exited during the Sterling crisis in the 1960s). The graph is a plot of the coefficients from a simple panel gravity regression with country-pair fixed effects and with a separate UK colony and UK currency union dummy for each year for the sample in GR (2002). It is readily apparent that the path of trade between the UK and countries with CU dissolutions, all but one of which were former colonies, did not differ significantly from colonies which were never involved in currency unions. Hence, including a simple time trend specific to all UK colonial pairs eliminates the result (regression results in Table 1 in the next section).



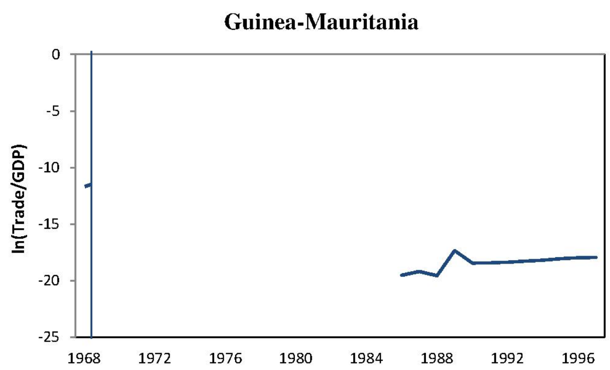
To get a flavor for how omitted variables can bias the estimates, the graph below shows the trade relationship for India and Pakistan, which experienced one of the 134 dissolutions in the GR (2002) sample in 1966, and *one of 26* which dissolved their currency at the same time as a war broke out. However, the CU dissolution (vertical blue line in chart) occurred at the same time as the outbreak of a brutal border war in 1965, in what was not an unrelated event. Trade as a share of GDP was depressed for years and never fully recovered, while hostilities between the two countries continue. Another example is Tanzania and Uganda, which ended their CU amid the Liberation War resulting in the overthrow of Ugandan dictator Idi Amin.[6]



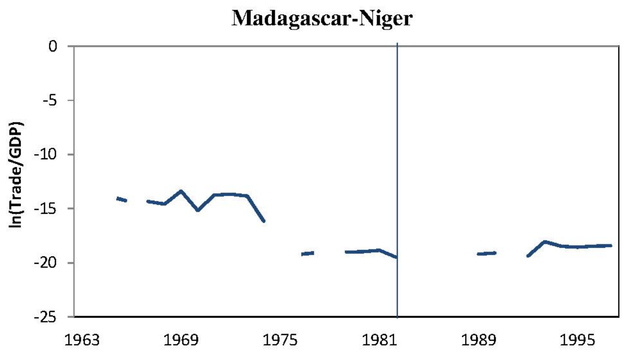
Aside from warfare, there are a multitude of other omitted variables. Madagascar and Reunion, below, experienced a dramatic trade decline after dissolving their currency union in 1976, the same year that there were widespread anti-islander riots in Madagascar, when at least 1,400 Comorians were killed in Mahajanga.[7] Thus, interpreting the subsequent trade decline as due solely to the end of the CU is problematic. India and Bangladesh ended their currency union in the wake of Operation Searchlight and the 1971 Bangladesh atrocities in which roughly 10 million Bengalis in East Pakistan took refuge in India to escape genocide.[8]



Another major issue with the original GR (2002) sample is missing data. There are numerous instances of currency unions dissolving and then no trade being recorded at all until a number of years later. For example, Guinea and Mauritania had just one year of data recorded in 1968, when they were joined in a CU, but then have no recorded data from 1969 to 1986 – nearly two decades – after which trade was substantially reduced. While this might still be an example for the currency union effect, one cannot help but suspect that whatever caused the data to be missing might be related to the decision not to continue sharing a common currency. We should like the result to be robust to the exclusion of examples such as this.

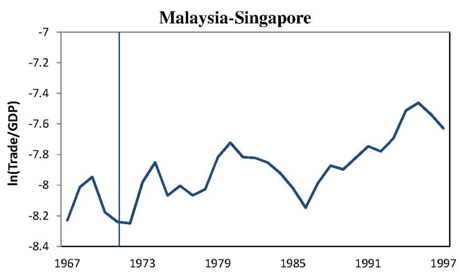


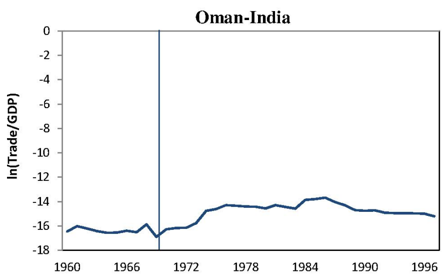
The importance of taking a time-series approach can also be seen from examples where currency union dissolutions followed on the heels of dramatic trade collapses, such as Madagascar and Niger below. In this case, inserting a simple dummy variable for currency union could be misleading as trade was on average much larger before the 1981 dissolution than after, but the timing of the trade decline does not fit the conclusion that the currency union dissolution had anything to do with it. For many of the cases where trade as a share of GDP was on average lower after dissolution, the timing of the trade declines is suspicious.

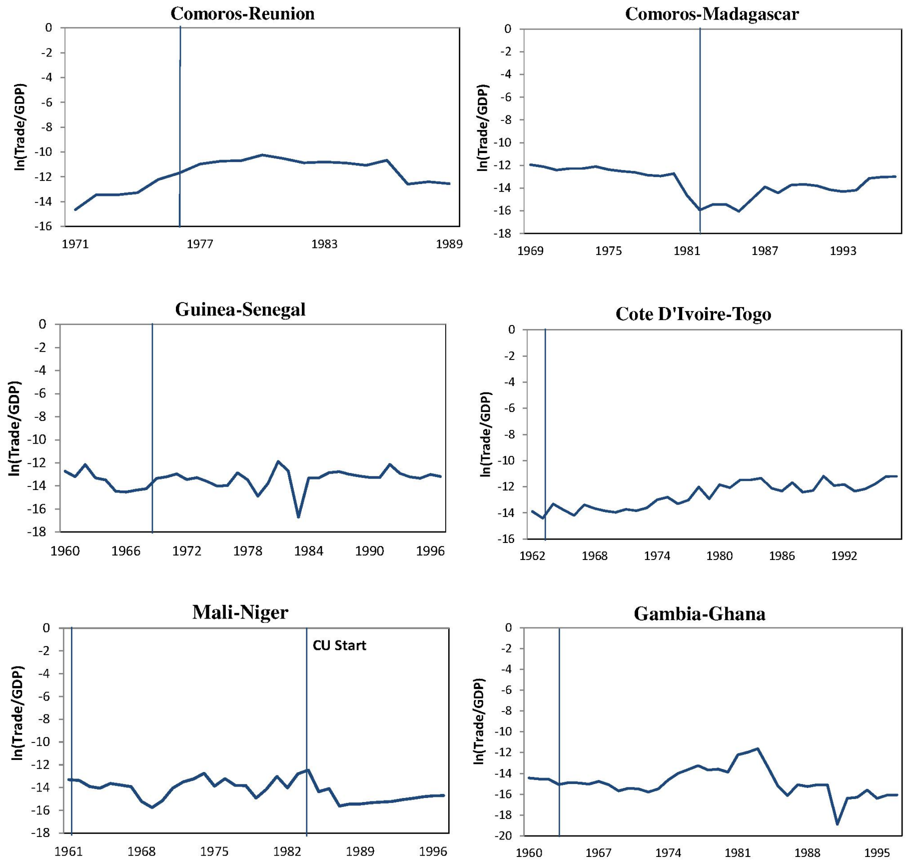


Indeed, while there are numerous counterexamples (Madagascar-Niger above is one), there are few, if any, clear examples which support the proposition that CUs substantially increase trade. Since there are only 134 switches, it is not difficult to simply scan the plots of each of the entrances and exits and ascertain in how many cases trade collapses coincided with CU dissolutions. Of the 55 CU changes not associated with decolonization, warfare, or missing data, there are roughly 40-45 counterexamples, 10-15 ambiguous cases, and arguably just four examples for. While many of these are debatable, the point estimate of the baseline regression in GR (2002) was 13 standard deviations above zero. This implies that out of 134 CU switches, if the standard errors are correct, there should not be any examples where a trade share remains constant following a CU dissolution, much less increase.

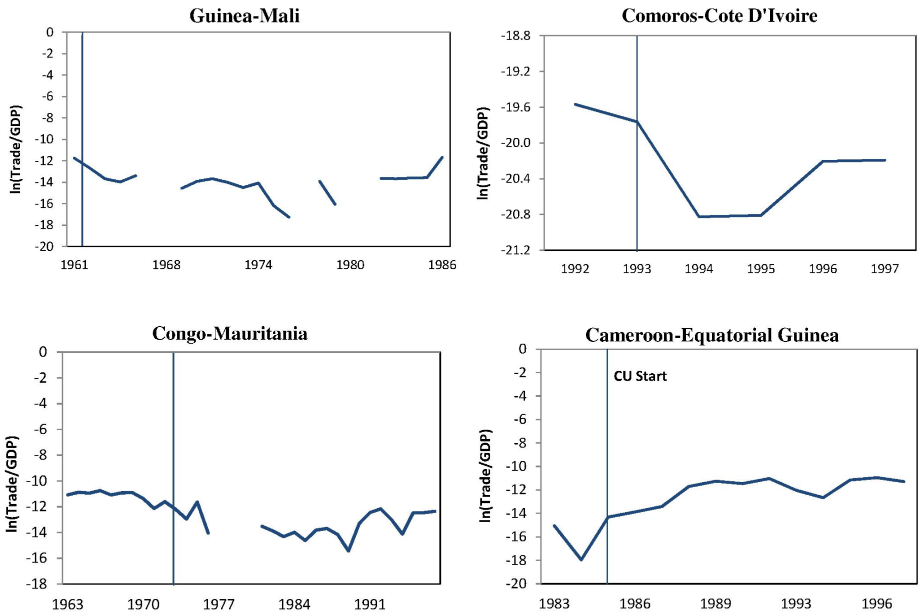
Here are eight samples of the roughly 40 counterexamples out of the 55 CU changes not associated with decolonization, warfare, or missing data.







By contrast, the four most promising examples (below) I could find which appear to be supportive point toward the conclusion that there could be still more omitted variables. Of the four, none have complete post-war data, or even data for three years on either side of a CU change. Three out of the four dissolutions here also coincided with recessions, and the change in CU status in the Cameroon case hinged on the results of the Civil War which took place in 1984 and likely depressed trade in that year, which would be an argument for why, if it were feasible, we would want to include country-year dummies as well. Lastly, for two of these four "examples" ostensibly in support of the theory that currency unions have a large impact on trade, trade as a share of GDP eventually recovered.



Yet, while classifying examples and counter-examples in this manner is illuminating, it also carries an element of subjectivity, and is less convincing than proper regression results, which we will get to after modelling the dynamic nature of trade in the next section.

1 Modeling Dynamic Gravity

As stressed by Krugman and Helpman (1985), trade is a dynamic process. Most New Trade Theory models (*e.g.*, Krugman 1979) include fixed costs, which Feenstra (2003) uses to motivate gravity. Once the fixed costs are paid, they tend to become sunk, and therefore imply that gravity might have important dynamics. And even a Heckscher-Ohlin model would imply that trade patterns are persistent if endowments evolve slowly. Examples of key papers which imply that trade should be a function of lagged trade costs include Krugman (1987), Baldwin (1990), Grossman and Helpman (1991), Feenstra and Kee (2008) and Melitz (2003), the last two of which assume a market-specific fixed cost of entry. When trade costs change, those costs become sunk, while trade costs themselves are likely to be autocorrelated. Empirically, trade patterns are highly persistent, as shown by Eichengreen and Irwin (2003), and in a recent working paper, Campbell (2010) shows that a dynamic gravity equation can arise out of a simple model of habit persistence in consumer tastes or in market-specific learning-by-doing or fixed costs, and finds that trade patterns and shocks are highly persistent, even across centuries. Many other recent papers study trade dynamics, including Jung (2009), Yotov and Olivero (2010), Horsewood, Martinez-Zarzoso, Nowak-Lehmann (2006), Bun and Klaassen (2002), Agosin, Alvarez, and Bravo-Ortega (2011), Egger (2001) and Qureshi and Tsangarides (2011), all of whom find that dynamics matter.

Equation (1) below is a very simple dynamic econometric model which emerges out of Campbell (2010), and is general enough that it encompasses any potential dynamic model, although which terms are relevant is an empirical question.

$$A(L)X_{ijt} = B(L)\tau_{ijt} + C(L)\epsilon_{ijt} \tag{1}$$

$A(L)$, $B(L)$, and $C(L)$ are lag polynomials, X_{ijt} is the log of trade over GDP, τ_{ijt} are trade costs and proxies for trade costs, including the usual geographic gravity controls, ϵ_{ijt} are the error terms, and $A(L)$ is invertible. Given that tariff and regulatory policy are highly persistent but not exactly known, shocks to trade are likely to be autocorrelated, which is the motivation for including $C(L)$. The $B(L)$ might represent potential J-curve type effects, where it may take agents time to react to unanticipated changes in trade costs, and hence lagged trade costs would be affecting current trade through a mechanism other than lagged trade (although my own intuition is that this term may be the least important of the three). Note that this model is general enough that it includes the "static" gravity case if the empirics dictate that the lagged variables do not matter. However, in practice, studies which examine dynamics in trade generally find that trade exhibits an $AR(p)$ process of some order. Inverting this process yields a dynamic gravity equation:

$$X_{ijt} = A(L)^{-1}B(L)\tau_{ijt} + A(L)^{-1}C(L)\epsilon_{ijt} \tag{2}$$

This can be rewritten as:

$$X_{ijt} = D(L)\tau_{ijt} + E(L)\epsilon_{ijt} \tag{3}$$

Where $D(L)$ and $E(L)$ are lag polynomials of infinite order. Equation (3) says that trade shares today depend on trade costs and shocks, past and present. Estimating this equation instead of a static equation can have a dramatic impact on our estimates of the impacts of currency unions on trade, and, just as importantly, on the measured standard errors. Glick and Rose (2002) estimated:

$$ln(X_{ijt}) = \beta_0 + \alpha_{ij} + \beta_1 ln(Y_{it}Y_{jt}) + \beta_2 ln(y_{it}y_{jt}) + \beta_3 CU_{ijt} + \epsilon_{ijt} \tag{4}$$

Where the Y_{it} is GDP, the lower-case y_{it} is GDP per capita, the dependent variable is now the log of the sum of bilateral trade, α_{ij} are time-invariant fixed effects, and ϵ_{ijt} are assumed to be *i.i.d.* shocks. There are three potential problems with estimating (4) instead of (3). The first is that the ϵ_{ijt} will not be *i.i.d.* in a world where trade follows an AR process. In practice, autocorrelated errors are a common feature of panel data. The second is that the impact of currency unions on trade should grow over time, which implies that using a simple dummy might *understate* the absolute value of the long-run effect as it is averaged with the short-term effects which should theoretically be lower. Thirdly, while putting in time-invariant fixed effects seems harmless enough, those fixed effects are likely to include factors which evolve in a particular way, which may be autocorrelated. Indeed, Bun and Klassen (2007) shrink the impact of currency unions on trade by including country-pair-specific trends. I will first propose something even less obtrusive: that we allow the coefficient on colonization to trend, reflecting the fact that colonial trade relationships have been observed to decay over time as noted in Head, Mayer, and Ries (2010), and estimate the following "dynamic" gravity relationship instead:

$$(5) \quad ln(X_{ijt}) = \beta_0 + \alpha_{ij} + \beta_1 ln(Y_{it}Y_{jt}) + \beta_2 ln(y_{it}y_{jt}) + \beta_3 CU_{ijt} + \beta_4 Colony_{ij} * year + \epsilon_{ijt}$$

Where we are comfortable assuming that $E[\epsilon_{ijt}\epsilon_{ist}] = 0$, but that we need to cluster at the country-pair level to get standard error estimates robust to autocorrelation (Bertrand, Duflo, Mullainathan [2004] suggest this for difference-in-difference estimates), so that we do not require that $E[\epsilon_{ijt}\epsilon_{ij(t-k)}] = 0, \forall k$.[9]

2 Empirics

The impact of the dissolution of UK currency unions is then estimated using the two approaches, the "static" equation in (4) and the "dynamic" equation in (5), with the results below in Table 1. The bilateral trade data come from the IMF's Direction of Trade (DOT) database for 217 countries from 1948 to 1997, by way of GR's [2002] data, but with many gaps. In the "static" formulation of gravity, our estimate of the impact of UK currency unions on trade is exp(.731)-1 which implies that currency unions more than double trade. Including a simple colony-year interaction—a very mild control—yields a point estimate close to zero, yet with sizable clustered standard errors.[10] Also, GDP and GDP per capita are both included as controls with an eye toward replicating Glick and Rose (2002), but the results are not sensitive to removing GDP per capita from the regression (and the same holds true for later regressions in this paper).

Table 1

Country-Pair FE	"Static" Gravity	"Dynamic" Gravity
UK Currency Unions	0.734*	-0.042
	(0.110)	(0.146)
UK Colonial Pair-Year		-0.038*
Trend Interaction		(0.003)
Log Real GDP	0.042*	0.043*
	(0.009)	(0.009)
Log Real GDP per capita	0.808*	0.811*
	(0.014)	(0.014)
Constant	-4.904*	-4.286*
	(0.228)	(0.232)
Observations	219,558	219,558
Number of pairid	11,178	11,178
R-squared	0.116	0.117

+ significant at 10%; ** significant at 5%; * significant at 1%;

Data from Glick and Rose (2002).

The results above for UK colonial pairs motivate the series of dynamic controls below in Table 2 on the full sample of currency union changes. The first row replicates the baseline result in GR (2002), which implies that CUs nearly double trade, with the other controls, such as GDP, GDP per capita, and the country-pair fixed effects suppressed. When the errors are clustered for this regression at the country-pair level, the estimated errors more than double. The second estimate includes a year fixed effect, which is a standard gravity control and is necessary because some shocks might affect all trade adversely in particular years, such as the oil shocks in the 1970s. The third row includes a UK Colony and year trend interaction, the same control in Table 1 above, which yields an implied impact of CUs on trade of just 58%. Yet, this result is driven by the CU changes due to wars or ethnic rioting, as if we remove those from the sample, the point estimate falls to just a 24% increase, and no longer significant at 5% (still including year FE and a UK colony and year trend interaction). This point estimate, in turn, is driven by the examples where a CU change is followed by missing data, as removing these examples shrinks the impact to just 16%, and not statistically significant. Finally, in the last row, a trend term for each country-pair is included in the estimation: $ln(X_{ijt}) = \beta_0 + \alpha_{ij} + \sum_t Year_t + \beta_1 ln(Y_{it}Y_{jt}) + \beta_2 ln(y_{it}y_{jt}) + \beta_3 CU_{ijt} + \sum_i \sum_j \beta_{4ij}(\alpha_{ij} * Year) + \epsilon_{ijt}$. The point estimate for the impact of currency unions on trade for this regression is now less than zero, although imprecisely estimated, with good reason to believe that the errors might still be biased downward since the assumption that $E[\epsilon_{ijt}\epsilon_{ikt}] = 0$ is not likely to hold in practice, even with country-pair FE included.

Table 2

	Estimates of CU on Trade
Baseline Result	.654*
(Normal SEs)	(0.044)
(Clustered SEs)	(0.111)
Include Year Fixed Effects	.584*
(Clustered SEs)	(0.109)
Include UK Colony*Year Trend	.457*
(Clustered SEs)	(0.120)
Eliminate Wars and Riots	.218***
(Clustered SEs)	(0.117)
Eliminate Missing Data	.151
(Clustered SEs)	(0.103)
Allow Country-Pair Trade to trend	-0.012
(Clustered SEs)	(0.089)

* Significant at 1%; *** Significant at 10%. All regressions include country-pair fixed effects, lrgdp, and and lrgdppc as controls.

To see that the OLS standard errors in a panel gravity setting do indeed exhibit autocorrelation, and thus need to be clustered, below I have reported the autocorrelation coefficients at various lags in the errors for the first regression in Table 2. This is evidence that at least some of the lagged terms in the dynamic gravity equation are empirically relevant.

lag #	**Autocorrelation in Errors from Baseline Specification (1st row) in Table 1:**
1	0.561
2	0.411
3	0.319
4	0.238
5	0.174
6	0.114
7	0.056
8	0.007
9	-0.041

There are alternative dynamic estimation approaches to the last row in Table 2. One option would be to use a lagged dependent variable, and then to use an Arellano-Bond or Blundell-Bond type of fix to correct for Nickel Bias.[11] Another simple alternative is a difference-in-difference estimate, running gravity in log changes—regressing the log change in trade on the log change in GDP and a CU dummy for the full sample with country-pair fixed effects. In the results below in the second column of Table 3, the point estimate on a CU Dummy is negative but insignificant, implying that countries which leave CUs do not experience faster trade declines after dissolution.[12] These results stand in stark contrast to the "Static" gravity equation estimated in levels, with Country-Pair Fixed Effects in the first column below.

Table 3

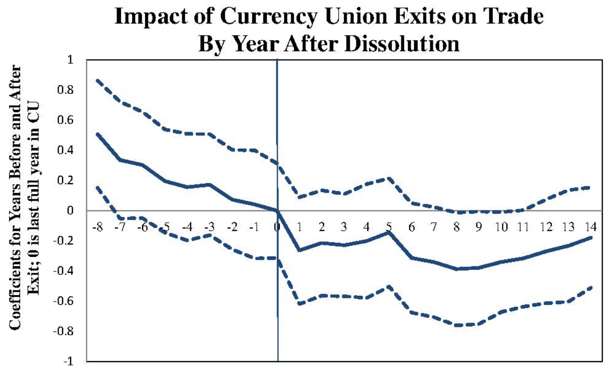
Country-Pair FE	"Static" Gravity	"Dynamic" Gravity
Currency Union	0.725*	-0.015
	(0.045)	(0.041)
LRGDP / $\partial LRGDP$	0.516*	0.389*
	(0.003)	(0.018)
Observations	218,087	195,183
Pairid	11,077	9,571
R-squared	0.4718	0.0026

"Dynamic" Gravity here is in Log Changes, "Static" Gravity in Levels.

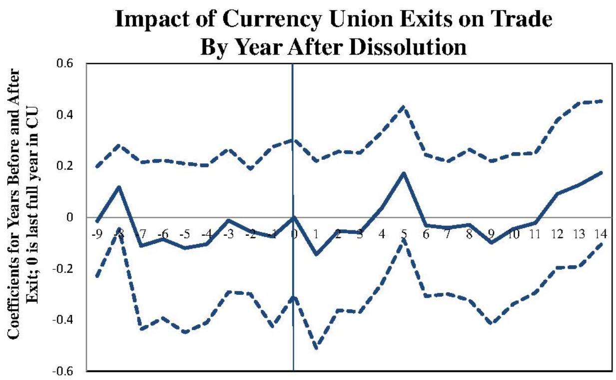
* significant at 1%; Data from GR (2002).

The results in Tables 2 and 3 imply that the impact of currency unions on trade can actually be eliminated in a number of ways—with a simple dynamic specification as in Table 3, or by controlling for omitted variables and estimating correct standard errors as in Table 2.

Yet, one of the implications of dynamic gravity is that the maximal impact of a change in trade costs could take years. Hence, treating each year after a dissolution as being the same could bias the estimate of the overall impact downward. An additional robustness check, then, is to estimate the impact of CUs on trade by year before and after dissolution. When we plot this with 90% error bounds for the baseline regression with Year FE (2nd regression in Table 2 above), one can see, first, that substantial trade declines precipitated dissolution. Secondly, compared with the last year in a CU, the decline in trade was only borderline significant in three individual years.[13]



When we include the same controls as in the last row of Table 2 (including time trends and excluding wars and CUs with missing data), the estimates of the impact of dissolution hovers about zero, with standard errors large enough that a large positive (or negative) impact of currency unions on trade is possible. This implies that the declining trade intensity before dissolution was a result of endogeneity rather than the expectations of higher future trade costs inducing a decline in trade.[14]



3 Conclusion

The results presented here reconcile the puzzling large apparent impact of currency unions on trade for the Glick and Rose (2002) sample with the much smaller and often statistically insignificant results of the Euro, with Klein and Shambaugh's findings on indirect pegs and exchange rate volatility, and with Meissner and Lopez-Cordova's time series results on the gold standard era. The early large, and precisely estimated results from the post-war period were driven by those examples of CU breakups coterminous with warfare, communist takeovers, coup d'etats, geopolitical strife, decolonization, and various other calamities likely to have adversely affected trade, and are sensitive to dynamic specifications. Given that the best point estimate of currency unions on trade is not statistically different from zero for all time periods, countries weighing their options on whether or not to join or leave a currency union, such as the decision Greece faces at the time of writing, would do well to discount previous evidence that there is a large trade channel in their calculations.

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Footnotes

1. Indeed, Jeff Frankel has called Rose's results on currency unions the most significant finding in International Macro in the past ten years, and Baldwin reports that Ken Rogoff assigned his Harvard students a "search and destroy" mission to try and discover what was causing the Rose results.

2. Other informative recent examples of this growing and active literature include Qureshi and Tsangarides (2010, 2011), Chen, Mirestean, and Tsangarides (2011), Baranga (2011), Katayama and Melatos (2011), and Dorn and Egger (2011).

3. These include Kelejian, Tavlas, and Petroulas (2011), who find a borderline effect, Baranga (2011), which finds none, and Escalona and Gomez (2011), who find a gradually increasing trade intensity in the Eurozone predating the Euro.

4. For example, possibly because of electronic transactions, traveller's checks peaked in the mid-1990s and are now on a sharp downward spiral:

http://research.stlouisfed.org/fred2/series/TVCKSSL.

5. In the abstract to Rose's response to Persson's critique, he writes "I have always maintained that the measured effect of a single currency on trade appears implausibly large..."

6. "An Idi-otic Invasion." TIME magazine, Nov. 13, 1978.

7. Madagascar: A Short History, by Randrianja and Ellis, 2009, University of Chicago Press.

8. "Bangladesh: Bringing a Forgotten Genocide to Justice" by Ishaan Tharoor in Time Magazine, Aug. 03, 2010, http://www.time.com/time/world/article/0,8599,2008085,00.html.

9. Of course, even with country-pair fixed effects, the assumption that $E[\epsilon_{ijt}\epsilon_{ikt}] = 0$ is also problematic – I thank Colin Cameron for pointing this out. Unfortunately, using country-year fixed effects for this dataset, even for only countries with CU switches, requires more computing power than I have access to.

10. An alternative would be to use Newey-West standard errors, which also correct for autocorrelation in the error terms, but could not be used on the full sample using Stata due to matsize limitations. On a reduced sample, the clustered errors and the Newey-West errors yield similar results. Another alternative would be to use panel-corrected standard errors – *xtpcse* in Stata – which also corrects for autocorrelation. Unfortunately, the general version of this command requires the years to be the same without gaps, and the panel-specific version runs into the same matsize issues as trying to run the Newey-West command. Hence, clustering at the country-pair level is the best choice for this dataset.

11. The existence of a sizable impact of CUs on trade is also sensitive to using Arellano-Bond type dynamic estimators. Arellano-Bond with the UK-Colony year trend included yields an estimate of 0.075 with a SE of (0.127). Arellano-Bond with trends

for all CU pairs yields a negative, insignificant estimate of CUs on trade, and various Blundell-Bond specifications yield similar results (see the authors homepage for further information).

12. *I.e.*, the "Dynamic" equation in Table 2 estimates: $ln(X_{ijt}) - ln(X_{(ijt-1)}) = \beta_0 + \alpha_{ij} + \beta_1(ln(Y_{it}Y_{jt}) - ln(Y_{(it-1)}Y_{(jt-1)})) + \beta_3 CU_{ijt} + \epsilon_{ijt}$.

13. This regression uses equation (4) : $\ln(X_{ijt}) = \beta_0 + \alpha_{ij} + \beta_1 ln(Y_{it}Y_{jt}) + \beta_2 ln(y_{it}y_{jt}) + \beta_3 CU_{ijt} + \epsilon_{ijt}$, with the only difference that the CU dummy is broken up by year.

14. This regression uses the equation: $ln(X_{ijt}) = \beta_0 + \alpha_{ij} + \sum_t Year_t + \beta_1 ln(Y_{it}Y_{jt}) + \beta_2 ln(y_{it}y_{jt}) + \beta_3 CU_{ijt} + \sum_i \sum_j \beta_{4ij}(\alpha_{ij} * Year) + \epsilon_{ijt}$.

Appendix A: List of Switches Coterminous with Warfare, Ethnic Cleansing, or Communist Takeovers

1. Tanzania-Uganda
2. Mauritania-Niger
3. Mauritania-Senegal
4. Mauritania-Togo
5. Kenya-Tanzania
6. Kenya-Uganda
7. Madagascar-Reunion
8. Madagascar-Senegal
9. Côte d'Ivoire (Ivory Coast)-Mali
10. Bangladesh-India
11. Burma(Myanmar)-India
12. Burma(Myanmar)-Pakistan
13. Sri Lanka-India
14. Sri Lanka-Pakistan
15. India-Pakistan

16. India-Mauritius
17. Pakistan-Mauritius
18. France-Algeria
19. France-Morocco
20. France-Tunisia
21. Portugal-Angola
22. Portugal-Cape Verde
23. Portugal-Guinea-Bissau
24. Portugal-Mozambique
25. Portugal-São Tomé and Príncipe
26. United Kingdom-Zimbabwe

Appendix B: List of Switches Coterminous with Missing Data

1. Cameroon-Mauritania
2. Central African Republic-Madagascar
3. Central African Republic-Mali
4. Chad-Madagascar
5. Republic of Congo-Madagascar
6. Benin-Guinea
7. Benin-Madagascar
8. Benin-Mauritania
9. Gabon-Guinea
10. Gabon-Madagascar
11. Guinea-Côte d'Ivoire (Ivory Coast)
12. Guinea-Mauritania
13. Côte d'Ivoire (Ivory Coast)-Mauritania
14. Madagascar-Niger

15. Madagascar-Togo
16. Madagascar-Burkina Faso
17. Mauritania-Niger
18. Mauritania-Togo
19. Cameroon-Comoros
20. Cameroon-Guinea-Bissau
21. Benin-Reunion
22. Gabon-Mali
23. Madagascar-Mauritania
24. Reunion-Senegal