Estimating the Impact of Currency Unions on Trade Using a Dynamic Gravity Framework

Campbell, Douglas L.

UC Davis

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Douglas L. Campbell
dlcampbell@ucdavis.edu
UC Davis
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Abstract

Does leaving a currency union reduce international trade? This paper reexamines time series estimates of currency unions on trade from a historical perspective using a dynamic gravity equation and by conducting in-depth case studies of currency union breakups. The early large estimates are sensitive to dynamic specifications, and were driven by omitted variables, as many breakups were caused by warfare, communist takeovers, coup d'etats and other major geopolitical events. The methodology has general applicability for the use of gravity equations in policy analysis, and yields an imprecise point estimate of currency unions on trade close to one percent.

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, or leave, a currency union (CU). Hence, it is not surprising that a large body of research in International Macroeconomics 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 (2003) have found that CUs increase trade on a 3-fold, 2-fold, 7-fold and 14-fold basis, with a voluminous literature reporting a similarly large and significant impact.[2] 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.

In this paper, I address this pressing policy issue by revisiting the early estimates of currency unions on trade from the original Glick and Rose (2002) dataset using a historical, dynamic gravity approach. I find that many of the dissolutions in the original GR sample were caused by major geopolitical events likely to have adversely affected trade, including warfare, communist takeovers, coup d'etats, ethnic cleansing episodes, anti-foreigner rioting, bloody wars of independence, genocide, financial crises, decolonization, and severe recessions. In addition, I augment the GR (2002) dataset with data from Feenstra and Lipsey (2005), as CU breakups were coterminous with missing trade or GDP data for one-sixth of the changes in CU status in the GR sample. Lastly, many of the CU dissolutions occurred between countries with past colonial ties. These country-pairs have experienced a gradual decaying of trade since colonial times, which implies the necessity of taking a dynamic gravity approach. I find that the early large estimates were driven by omitted variables such as war and are sensitive to controlling for dynamics. I arrive at a plausible point estimate of the impact of CUs on trade of one percent, but with relatively wide error bounds.

This finding, while distinct from other estimates in the literature which use the same time period as GR (2002), is consistent with more recent findings on the impact of the Euro and with historical estimates on the gold standard era, as the Rose Effect did not have out-of-sample predictive power. Many recent studies, including Berger and Nitsch (2008) and Santos Silva and Tenreyro (2009), have found no effect of the Euro on trade, while Havranek's (2010) meta-analysis found systematic evidence of publication bias for the Euro studies, and a mean impact of just 3.8% versus over 60% for earlier non-Euro episodes.[3] For the gold standard era, Meissner and Lopez-Cordova (2003) documented a cross-sectional correlation between gold standard membership and trade, but also showed that this relationship disappeared in a time series setting with the inclusion of necessary fixed effects (to correct for reverse causality, Baldwin and Taglioni's [2006] "gold metal error"). Ritschl and Wolf (2011) found that the interwar gold, Sterling, and Reichsmark blocs did not increase trade.

The findings presented in this paper are also consistent with the literature on the impact of pegs and exchange rate volatility. Klein and Shambaugh (2006) found that direct pegs increase trade substantially less than currency unions, and that indirect pegs do not increase trade at all. In addition, they found that exchange rate volatility itself is hardly correlated with trade, as going from normal to no volatility implies an increase in trade of just one or two percent. If the initial large estimates of currency unions on trade were largely the result of endogeneity and omitted variables, then these results are not puzzling, since indirect pegs are more likely to be random than direct pegs or currency unions, and thus provide a natural experiment yielding the most reliable estimate of fixed exchange rate regimes on trade (Baranga [2011] makes this argument for indirect pegs to the Euro). While pegs and currency unions are not identical, Klein and Shambaugh's findings do remove the most plausible channel, exchange rate volatility, by which currency unions were thought to increase trade.

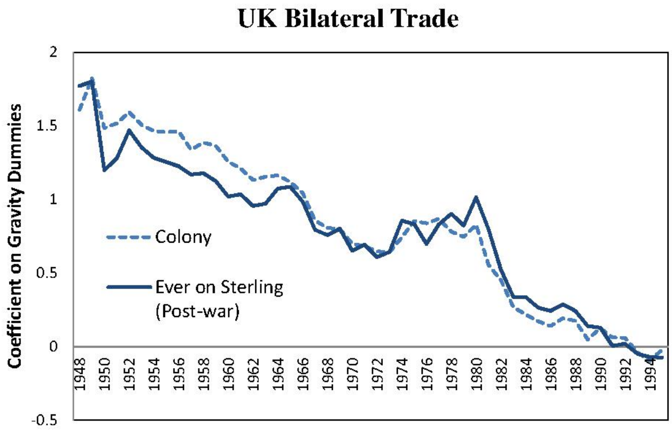
Of course, many scholars, including Rose himself,[4] have expressed doubt about the earlier large estimates of the CU effect, and there have also been many insightful critiques of the early estimates of common currencies on trade. These include studies by Persson (2001), Pakko and Wall (2001), Klein (2005), Nitsch (2005), Berger and Nitsch (2008), 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. For example, Nitsch (2005) finds no impact for CU entries, Pakko and Wall (2001), predating GR (2002), eliminate the impact on a smaller dataset, Klein (2005) finds no trade effect of 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. However, none of these papers articulate a comprehensive explanation of the factors driving the entire GR (2002) result. This paper strives to fill the gap.

The literature has generally employed gravity models along the lines of Anderson (1979) and Anderson and van Wincoop (2003) to ascertain the impact of currency unions on trade flows using panel data sets. This paper adopts the methodology of Campbell (2012) in using a dynamic gravity approach, and follows several earlier currency union papers which also consider dynamics, including Bomberger (2003), Nitsch (2005), Bun and Klaassen (2007), Bergin and Lin (2011), and Qureshi and Tsangarides (2011). This paper differentiates itself from the literature by being the first to show that dynamics alone can explain the entire GR (2002) result, and also by demonstrating the role played by other omitted variables, including war, and missing data. In doing so, this paper builds on the intuition of Thom and Walsh (2002), who point out that former colonies of France and Portugal ended their common currencies after coup d'etats and wars of independence which were in some cases followed by civil wars and communist takeovers. In these cases, 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 led these authors to find that currency unions increase trade by a still sizeable 25%, and precisely estimated.

The rest of the paper proceeds as follows: In Section 1, I provide graphical illustrations for the role of historical factors such as decolonization and warfare, and the role of missing data. In Section 2, I outline a dynamic gravity theory, and then in Section 3 I demonstrate empirically that these controls are influential.

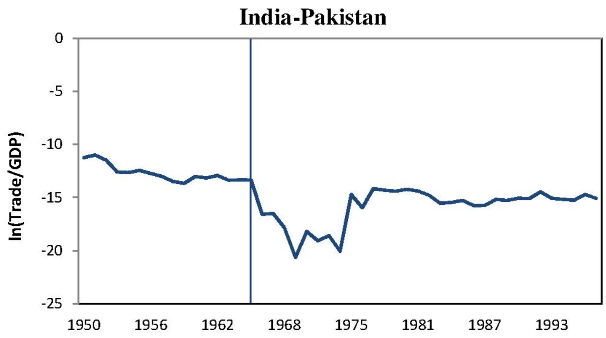
1 The Role of History Illustrated

First, early estimates of CUs on trade confused the decaying impact of historical colonization for the impact of CU dissolution. This can be seen in the graph below, which compares the UK's trade with all of its colonies to those 26 countries with which it started the period sharing a currency union, most of which exited during the Sterling crisis in the 1960s. It plots the coefficients of a panel gravity regression covering 217 countries from 1948 to 1997 (the GR 2002 sample), with country-pair fixed effects and with a separate UK colony and UK currency union dummy for each year. 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 (*i.e.*, the bilateral trade path of the UK and New Zealand is similar to the path of the UK and Australia). Hence, including a simple time trend specific to all UK colonial pairs to account for the decaying of colonial trade ties eliminates the result (regression results in Table 1 in Section 2).

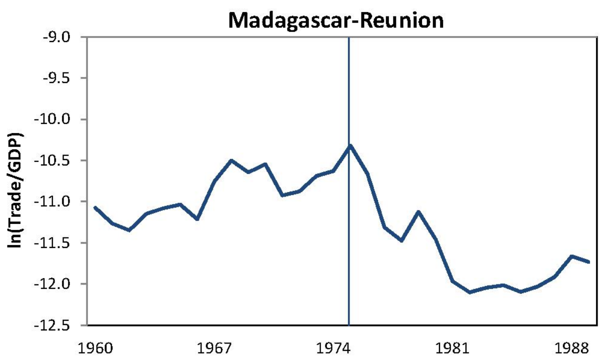


In the sample of CU changes in GR (2002), there are roughly only 100 country-pairs which did not have former colonial relationships. Only three quarters of these switches contain data before and after the change in CU status, and in another 15 cases, the CU dissolutions coincided with warfare or another major geopolical event, such as the ethnic cleansing of those who share a common currency. Of the rest, many followed dramatic trade declines (I count 17), and more than half of the rest followed recessions, 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 numerous counterexamples.

The role which warfare plays in the early CU estimates is readily apparent in the graph below, which shows the trade relationship for India and Pakistan, which experienced one of the 134 changes in CU status in the GR (2002) sample in 1966. However, the CU dissolution (vertical blue line in chart) occurred at the same time as the outbreak of a brutal border war in 1965. Trade as a share of GDP was depressed for years and never fully recovered, while hostilities between the two countries continue. A similar example is Tanzania and Uganda, which ended their CU amid the Liberation War resulting in the overthrow of Ugandan dictator Idi Amin.[5]

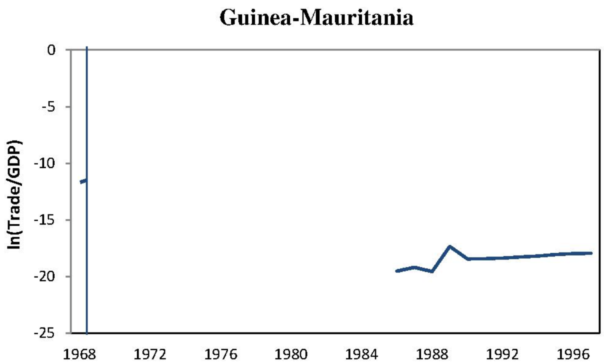


Aside from warfare, there is a multitude of other omitted variables. Madagascar and Reunion, below, experienced a dramatic trade decline after dissolving their currency union in 1976, the same year as widespread anti-islander riots in Madagascar, when at least 1,400 Comorians were killed in Mahajanga.[6] Thus, interpreting the subsequent trade decline as due solely to the end of the CU is problematic. Another example is India and Bangladesh, which ended their CU in the wake of Operation Searchlight and the 1971 Bangladesh atrocities that prompted roughly 10 million Bengalis in East Pakistan to take refuge in India to escape genocide.[7]

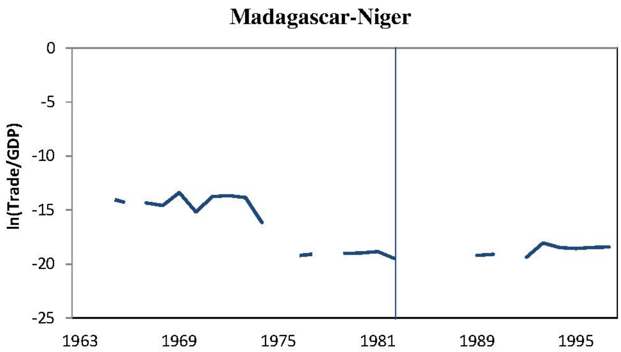


Another major issue with the original GR (2002) sample is missing data. There are numerous instances of currency unions dissolving and then no trade being recorded at all until a number of years later. For example, Mauritania and Guinea (also called

Guinea-Conakry) 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 lower. While this might still be an example for the CU effect, one cannot help but suspect that whatever caused the data to be missing might have been related to the decision not to continue sharing a common currency. Hence, I augmented the GR (2002) dataset with trade data from Feenstra and Lipsey (2005), GDP data from the World Bank, and CU status from the IMF's *Exchange Arrangements and Exchange Restrictions Annual Report* via Ewencyk, Hulej, and Tsangarides (2006). However, I find that the measured impact of CUs on trade for those observations which are still missing are much higher than for other CU switches.



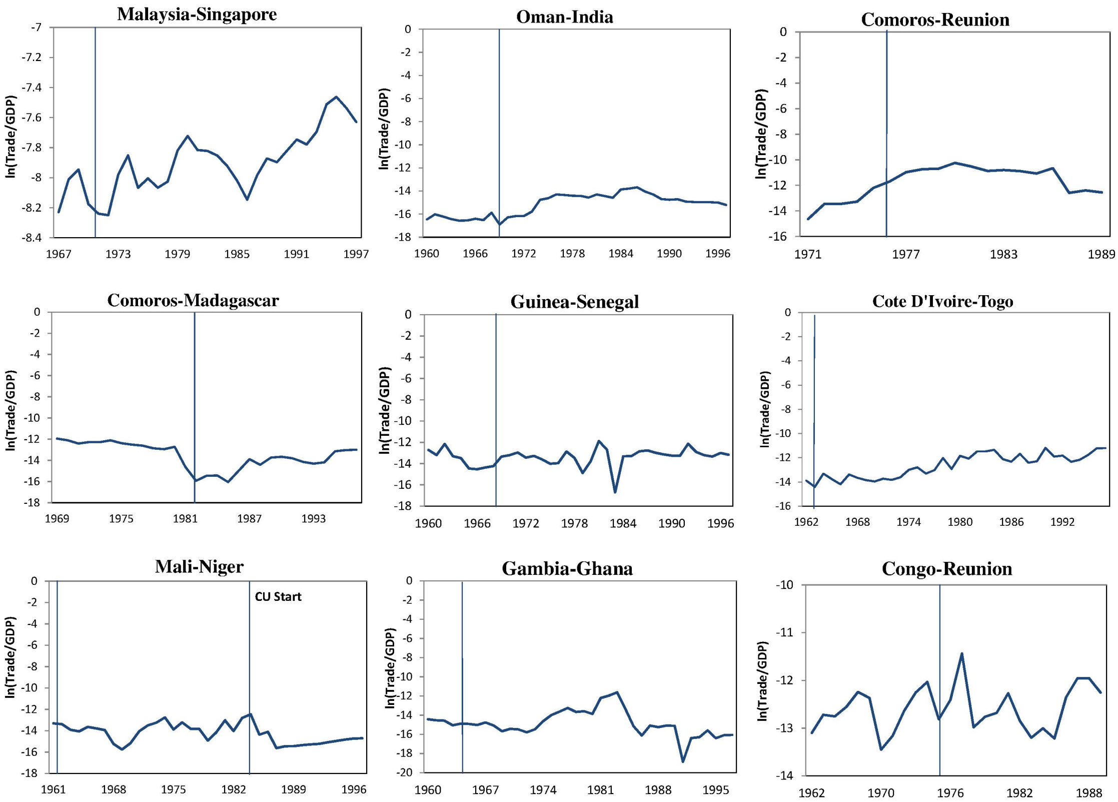
The importance of taking a time-series approach can also be seen from examples where currency union dissolutions followed on the heels of dramatic trade collapses, such as Madagascar and Niger below. In this case, inserting a simple dummy variable for CUs could be misleading as trade was on average much larger before the 1981 dissolution than after, but the timing of the trade decline does not substantiate the conclusion that the currency union dissolution caused the trade decline. 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.



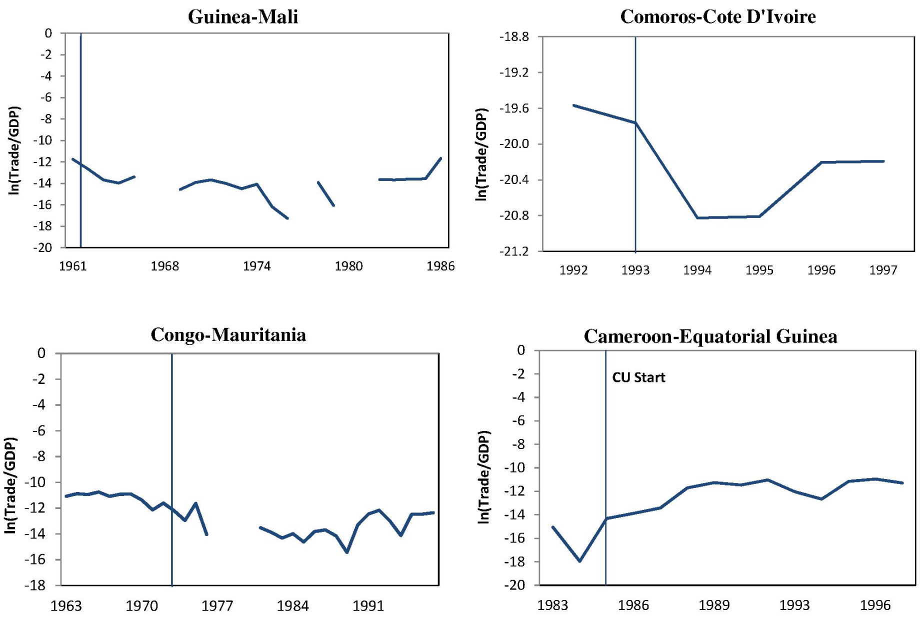
Indeed, while there are numerous counterexamples (Madagascar-Niger above is one), there are few, if any, clear examples which support the proposition that CUs substantially increase trade. Since there are only 134 switches, it is not difficult to scan the plots of each of the entrances and exits to ascertain the number of cases in which trade collapses coincided with CU dissolutions. Of the roughly 60 CU changes not associated with decolonization, warfare, or missing data, there appear to be roughly 40-45 counterexamples, 10-15 ambiguous cases, and arguably just four examples in favor. While many of these are debatable, the point estimate of the baseline regression in GR (2002) was 13 standard deviations above zero. If it were an unbiased estimate with accurate standard errors, it implies that out of 134 CU switches, we should not observe any examples where a trade share remains constant following a CU dissolution, much less increase.

In fact, there are numerous counterexamples. Below is a sample of nine out of the roughly 40-45 counterexamples from the switches not associated with decolonization, warfare, or missing data. For example, after Malaysia and Singapore dissolved their currency union, trade as a share of GDP increased dramatically. For Comoros and Reunion, trade continued to increase gradually after dissolution, and then stayed constant for another decade. Mali and Niger constitute two counterexamples, as trade stayed roughly flat after dissolving their currency union in 1961, and then fell after these countries rejoined their currencies in 1984.

A Sampling of the Counterexamples



By contrast, of the 16 examples that GR (2002) provide as evidence in their Table 1, 13 are associated with decolonization, warfare, ethnic cleansing, or missing GDP data. In two of the other cases, trade eventually recovered. That leaves Cameroon and Equatorial Guinea as the sole remaining example, which is also ambiguous given that the 1985 CU start hinged on the outcome of the Civil War in Cameroon the previous year and was more likely to have depressed trade than the lack of a common currency (see graph below).[8] This is an example of why, if it were computationally feasible, gravity regressions should include country-year dummies. I found three additional examples (below) that appear to be supportive, yet none have complete post-war data, or even data for three years before and after CU exits. Three out of the four changes in CU status here also coincided with recessions. 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 did eventually recover.



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 follow a brief primer on modelling dynamic gravity in the next section.

2 Modeling Dynamic Gravity

As stressed by Krugman and Helpman (1985), trade is a dynamic process. New Trade Theory models include increasing returns, which Feenstra (2003) uses to motivate gravity. Many of these models (*e.g.*, Krugman 1979) include fixed costs, which figure explicitly in Chaney's (2008) gravity equation. Although Chaney abstracts from dynamics, these fixed costs, once paid, are likely to become assets which do not depreciate completely each period, and so could give rise to important dynamics. Examples of key papers which imply that trade should be a function of lagged trade costs include Krugman (1987), Baldwin (1990), Grossman and Helpman (1993), Chaney (2008), Feenstra and Kee (2008), and Melitz (2003), the last two of which assume a market-specific fixed cost of entry. When trade costs change, market-specific investments become sunk, while unobserved trade costs are likely to be autocorrelated. Empirically, trade patterns are highly persistent, as shown by Eichengreen and Irwin (2003), and Campbell (2012) shows that trade patterns are persistent even across centuries. Campbell (2012) also shows that a dynamic gravity equation can arise out of a simple model of habit persistence in consumer tastes or via market-specific learning-by-doing. Many other recent papers confirm the importance of 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).

Equation (1) below is a very simple dynamic econometric model which emerges from Campbell (2012), and is general enough that it encompasses any potential dynamic model. Identifying 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 logarithm of trade divided by 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 policies are highly persistent but not exactly known, shocks to trade are likely to be autocorrelated, which motivates the inclusion of $C(L)$. The $B(L)$ represents 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 the "static" gravity case is nested within 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 equation (1) 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) implies 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 estimates of the impact of CUs on trade, and 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.*. 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 (for UK colonies), 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:

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

The decaying of the colonial trade relationships most likely reflects the (declining) importance of lagged trade costs. Here we are comfortable assuming that $E[\epsilon_{ijt}\epsilon_{ikt}] = 0$, 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 it is not required that $E[\epsilon_{ijt}\epsilon_{ij(t-k)}] = 0, \forall k$.[9]

3 Empirics

To motivate the dynamic controls, I begin by contrasting the measured impact of the dissolution of UK currency unions using 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, which includes 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] GDP and GDP per capita are both included as controls with an eye toward replicating Glick and Rose (2002), yet the results are not sensitive to removing GDP per capita from the regression (and the same holds true for later regressions).

Table 1: The Impact of UK Currency Unions on Trade

	"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

Both Regressions include country-pair fixed effects and clustered SEs.

* significant at 1%; Data from Glick and Rose (2002).

The results above for UK colonial pairs motivate the 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 (exp(.654)-1=92.3%), with the other controls, such as GDP, GDP per capita, and the country-pair fixed effects suppressed. Clustering at the country-pair level substantially increases the measured errors. The second estimate includes a year fixed effect, a standard gravity control 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 that handful of observations from the sample, the point estimate falls to just a 23% increase, and no longer significant, even at 10% (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 augmenting the data from GR (2002) with data from Feenstra and Lipsey (2005), while removing the rest of the CUs which coincide with missing data, cuts the point estimate in half. 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 a plausible one percent, although imprecisely estimated. Even so, the errors reported here are still likely to 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, as some trade shocks may adversely affect all of a nation's trade in any given year.

Table 2: The Impact of Currency Unions 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	.207
(Clustered SEs)	(0.118)
Eliminate CUs with Missing Data	.108
(Clustered SEs)	(0.107)
Allow Country-Pair Trade to trend	0.009
(Clustered SEs)	(0.090)

* Significant at 1%. All regressions include country-pair fixed effects, log GDP and log GDP per capita as controls. Each row includes progressively more controls. The last row is the preferred point estimate. Data from Glick and Rose (2002) augmented with more data from Feenstra and Lipsey (2005), and GDP data from the World Bank.

To see that the OLS standard errors in a panel gravity setting do indeed exhibit autocorrelation, and thus need to be clustered, in the Appendix I report 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.

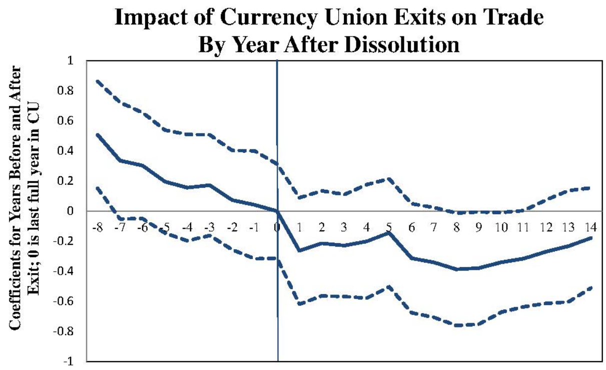
There are alternative dynamic estimation approaches to the last row in Table 2. One option would be to use a lagged dependent variable, and then to use an Arellano-Bond or Blundell-Bond type of fix to correct for Nickell Bias (1981).[11] Another simple alternative would be to estimate gravity in log 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 (second column of Table 3 below). In this regression, 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, and show that the impact of CUs on trade can actually be eliminated multiple ways—with a simple dynamic specification as in Table 3, or by controlling for omitted variables and estimating clustered errors as in Table 2.

Table 3: Gravity in Levels vs. Log Changes

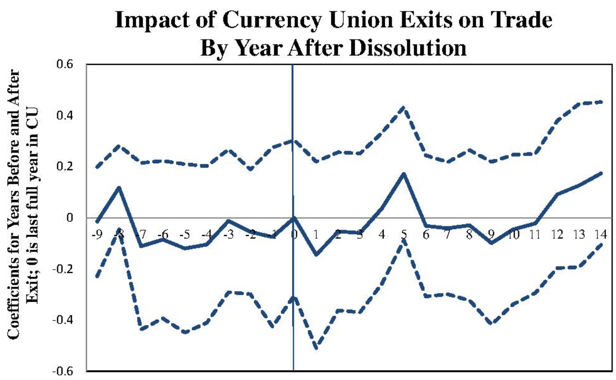
	“Static” Gravity	“Dynamic” Gravity
Currency Union	0.725*	-0.015
	(0.045)	(0.041)
Log GDP / $\Delta LogGDP$	0.516*	0.389*
	(0.003)	(0.018)
Observations	218,087	195,183
Pairid	11,077	9,571
R-squared	0.4718	0.0026

“Dynamic” Gravity here is in Log Changes; “Static” Gravity in Levels.
* significant at 1%; Data from GR (2002). Both regressions include country-pair fixed effects.

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 the same controls are included 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. That the pre-dissolution declining trade intensity disappears in this regression implies that it was the result of omitted variables rather than anticipation effects.[14]



4 Conclusion

That the early large and precisely-estimated impact of CUs on trade can be eliminated with the inclusion of historical factors carries implications for policy, for the estimation of gravity equations generally, and for development. The policy implication is straightforward – 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, should discount previous evidence that there is a large trade channel in their considerations. Secondly, the historical, dynamic gravity approach detailed here applies equally to the usage of gravity equations in policy analysis generally, including for the impact of pegs, exchange-rate volatility, and FTAs on trade. Lastly, that historical trade costs, as proxied by former colonial status, decay slowly and are still important implies that history matters for development – a topic worthy of further research.

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Footnotes

1. Indeed, in his comments on Baldwin (2005), Jeff Frankel called Rose's results on currency unions the most significant finding in International Macro in the past ten years, and Rose (2001) reports that Ken Rogoff assigned his Harvard students a "search and destroy" mission to try and explain the original Rose Effect.

2. Other informative recent contributions to this growing literature include Baldwin (2006), Helpman, Melitz, and Rubinstein (2008), Qureshi and Tsangarides (2010, 2011), Chen, Mirestean, and Tsangarides (2011), Baranga (2009, 2011), Katayama and Melatos (2011), and Dorn and Egger (2011).

3. These include work by Kelejian, Tavlas, and Petroulas (2011), who find a borderline effect, Baranga (2011), who finds none, and Escalona and Gomez (2011), who find a gradually increasing trade intensity in the Eurozone prior to the Euro. One nearly insurmountable difficulty facing any researcher hoping to tease out the trade impact of the Euro is that the Euro was accompanied by a long series of other policy changes designed to facilitate trade within Europe, resulting in trend growth in regional trade integration (see Berger and Nitsch, 2008).

4. 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..."

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

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

7. "Bangladesh: Bringing a Forgotten Genocide to Justice" by Ishaan Tharoor in Time Magazine, Aug. 03, 2010.

8. "Cameroon Says Rebels Are Being 'Mopped Up'." The New York Times, April 9, 1984.

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, is computationally demanding.

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: $ln(X_{ijt}) = \beta_0+\alpha_{ij}+\sum_t Year_t+\beta_1 ln(Y_{it}Y_{jt})+\beta_2 ln(y_{it}y_{jt}) + \beta_3 CU_{ijt} + \sum_i \sum_j \beta_{4ij}(\alpha_{ij} * Year) + \epsilon_{ijt}$. Most currency union dissolutions, unlike unions, were not known very far in advance.

Appendix

Appendix Table 1

lag #	Error Autocorrelation 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

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 in GR Sample

1. Cameroon-Mauritania
2. Central African Republic-Madagascar
3. Central African Republic-Mali
4. Chad-Madagascar (in Feenstra-Lipsey)
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) (in Feenstra-Lipsey)
12. Guinea-Mauritania
13. Côte d'Ivoire (Ivory Coast)-Mauritania (in Feenstra-Lipsey)
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 (in Feenstra-Lipsey)
23. Madagascar-Mauritania (in Feenstra-Lipsey)
24. Reunion-Senegal