Estimating the Impact of Currency Unions on Trade: Solving the Glick and Rose (2002) Puzzle

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Abstract

Does leaving a currency union reduce international trade? This paper uses a historical approach to reexamine the puzzling large apparent impact of currency unions on trade. I find that the early time series estimates were driven by the gradual decaying of colonial trade ties and other major geopolitical factors including warfare, communist takeovers, and ethnic cleansing episodes. My methodology, which carries lessons for other uses of gravity equations in policy analysis, yields point estimates of currency unions on trade that are not statistically distinct from zero.

JEL Classification: F15, F33, F54

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A key policy decision for many nations is 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. What is surprising, however, is the magnitude of the measured increase in trade due to sharing a common currency, as Rose (2000), Glick and Rose (2002), Barro and Tenreyro (2007), and Alesina, Barro, and Tenreyro (2003) found that CUs increased trade 3-fold, 2-fold, 7-fold and 14-fold, respectively. A voluminous and growing body of literature has reported a similarly large and significant impact. In 2005, Jeff Frankel called Rose's discovery of the puzzling large ostensible impact of CUs on trade the most significant finding in International Macroeconomics in the preceding ten years. Given these sizeable estimates, a technocrat on the European periphery now facing a difficult decision about whether to remain in the euro zone could be forgiven for fearing that leaving would reduce trade and thus welfare.

In this paper, I address this pressing policy issue by revisiting the early estimates for the impact of currency unions on trade from the original Glick and Rose (2002)—henceforth GR—dataset using a historical approach, controlling for dynamics and correcting the errors for autocorrelation. Many of the 134 switches in currency union status in the GR (2002) sample were dissolutions caused by major geopolitical events likely to have adversely affected trade. These events include warfare, communist takeovers, coup d'etats, ethnic cleansing episodes, anti-foreigner rioting, bloody wars of independence, genocide, financial crises, and severe recessions. In addition, one-sixth of the CU breakups were coterminous with missing trade or GDP data. Lastly, many of the CU dissolutions occurred between countries with past colonial ties. In former colonies that had won their independence after bitter struggles, trade often declined sharply in the subsequent unstable political environment. Colonies that experienced smooth transitions to independence tended to experience a gradual decaying of relative trade intensities with their former colonizers over a period of decades. Hence, including country-pair trends, generally advisable for panel estimation, yields dramatically different point estimates for the impact of CUs on trade for the colonial sample. Overall, 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 an imprecise point estimate of minus four percent for the impact of CUs on trade, but with clustered standard errors close to ten percent.

This finding, while distinct from other estimates in the literature based on the same time period as GR (2002), is consistent with recent findings on the impact of European currency unification and estimates from earlier time periods. Berger and Nitsch (2008) and Santos Silva and Tenreyro (2009) 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. De Sousa (2012) argues that the impact of CUs on trade has dampened over time due to improvements in financial technology, yet there was also little measured impact in the prewar era. Meissner and Lopez-Cordova (2003) documented a cross-sectional correlation between gold standard membership and trade, and also showed that this relationship disappeared in a time series setting with the inclusion of necessary fixed effects (Baldwin and Taglioni's [2006] "gold medal error" of common gravity estimation mistakes). Ritschl and Wolf (2011) did not find evidence that the interwar gold, sterling, and reichsmark blocs increased trade.

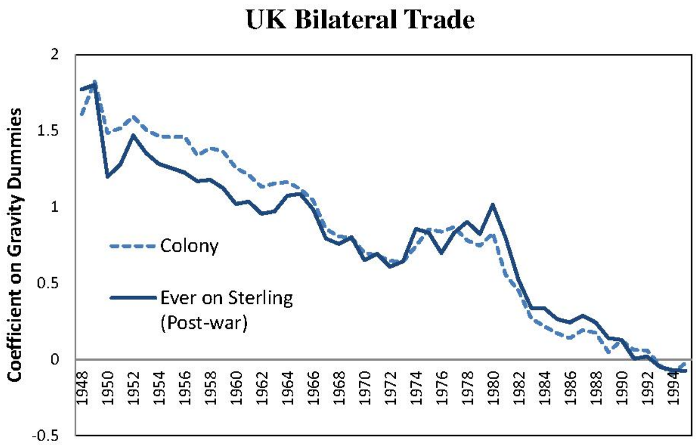
The findings presented in this paper are also consistent with the literature on the impact of pegs and exchange rate volatility on trade. Klein and Shambaugh (2006) found that direct pegs increase trade substantially less than currency unions, and that indirect pegs do not increase trade at all. In addition, they found that exchange rate volatility itself is only slightly 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 estimated impact of currency unions on trade were primarily the result of endogeneity and omitted variables, these results are not puzzling, since switches in indirect peg status are more likely to be random, and less likely to be driven by endogeneity than direct pegs or currency unions. Thus indirect pegs provide a natural experiment yielding the most reliable estimate of the impact 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 have expressed doubt about the early large estimates of the CU effect. Even Rose, who discovered this puzzle, himself wrote that "I have always maintained that the measured effect of a single currency on trade appears implausibly large..." Since Ken Rogoff assigned his Harvard students a "search and destroy" mission to explain the original Rose Effect in the early 2000s, there have been many insightful critiques of the early estimates for the CU effect on trade which have succeeded in shrinking the estimated impact or reducing the estimate to zero for subsamples.[1] For example, Persson (2001) and Pakko and Wall (2001) followed Rose's (2000) original paper but predated GR (2002) and greatly reduce or eliminate the estimated impact on smaller datasets. Nitsch (2005) finds no impact for CU entries, 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 (2002) study. Bomberger (2003) and Bun and Klaassen (2007) include dynamic controls, but the former eliminates the result only on a subsample and the latter shrinks the impact to a still substantial 25%, and precisely estimated. These papers, and Baldwin's (2006) critical overview of this literature, succeed in casting doubt on the GR (2002) finding, but do not articulate a comprehensive explanation of the factors driving the result. This paper strives to fill the gap.

The rest of the paper proceeds as follows: In Section 1, I illustrate the role of historical factors such as decolonization and warfare, the problem of missing data, and provide numerous counterexamples. In Section 2, I demonstrate empirically using methods comparable to GR (2002) that these controls are influential. In Section 3, I provide further robustness checks and find that currency dissolutions were on average predated by trade collapses.

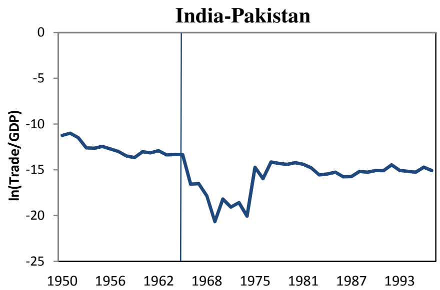
1 The Role of History

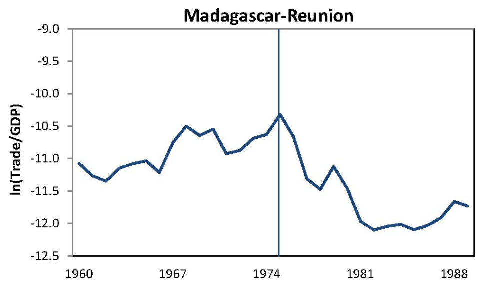
The key source of bias of early estimates of the impact of CUs on trade is the omission of various important historical factors. The most interesting of these from an economic perspective is that early estimates did not control for the slow and steady decay of colonial trade ties after independence. The graph below plots the coefficients for UK colonies and for UK currency unions by year from two panel gravity regressions covering 217 countries from 1948 to 1997 (the GR 2002 sample), with country-pair fixed effects. Hence we can compare the UK's trade with all of its colonies to those 25 countries with which it started the period sharing a currency union, many of which exited during the Sterling crisis in the 1960s or shortly thereafter. 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 that were never involved in currency unions. (*I.e.*, the bilateral trade path of the UK and New Zealand, which began the period in a CU, is similar to the path of the UK and Australia, which did not.) 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).



The entire sample contains 134 switches with time series variation in the data, and since 25 of these involve the UK, the issue of former colonization alone would substantially alter the character of the results. However, decolonization was not the only key variable omitted, as many CU dissolutions were caused by warfare and ethnic cleansing, as is evident below. At left is the trade relationship for India and Pakistan, who ended their currency union (vertical blue line in chart) 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. Another example of war overlapping with dissolution is Tanzania and Uganda, who ended their CU amid the Liberation War resulting in the overthrow of Ugandan dictator Idi Amin.[2]

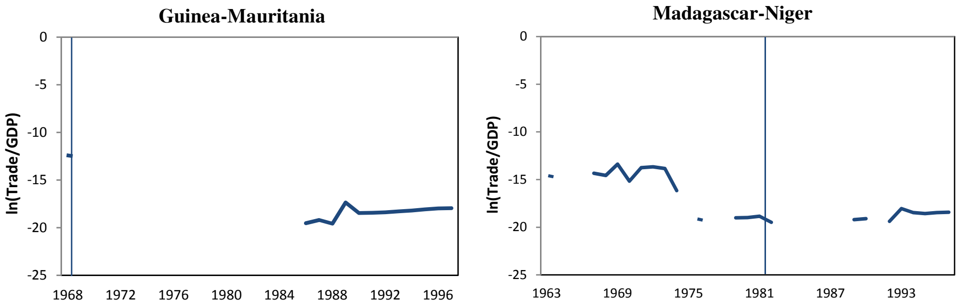
Madagascar and Reunion (below, right) experienced a dramatic trade decline after dissolving their currency union in 1976, the same year as widespread anti-islander riots in Madagascar, when at least 1,400 Comorians were killed in Mahajanga.[3] Another incredible example is India and Bangladesh, which ended their CU in the wake of Operation Searchlight and the 1971 Bangladesh atrocities that prompted roughly 10 million Bengalis in Bangladesh to take refuge in India to escape genocide.[4] In Appendix Table 1 I list 26 cases in which there appears to be a major geopolitical event likely to have adversely impacted trade more than a change in CU status, although this list is not meant to be exhaustive and does not include democratic changes in political control, severe recessions, or financial or currency crises.





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, featured below, left) 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 suspects that whatever caused the data to be missing might have been related to the decision not to continue sharing a common currency. Alternatively, it might be that successful membership in an international currency union proxies political and economic stability, particularly in Africa, a region that experienced substantial turmoil during this period. The measured impact of CUs on trade for those 22 observations (detailed in Appendix Table 2) that have missing data are higher than for other CU switches.

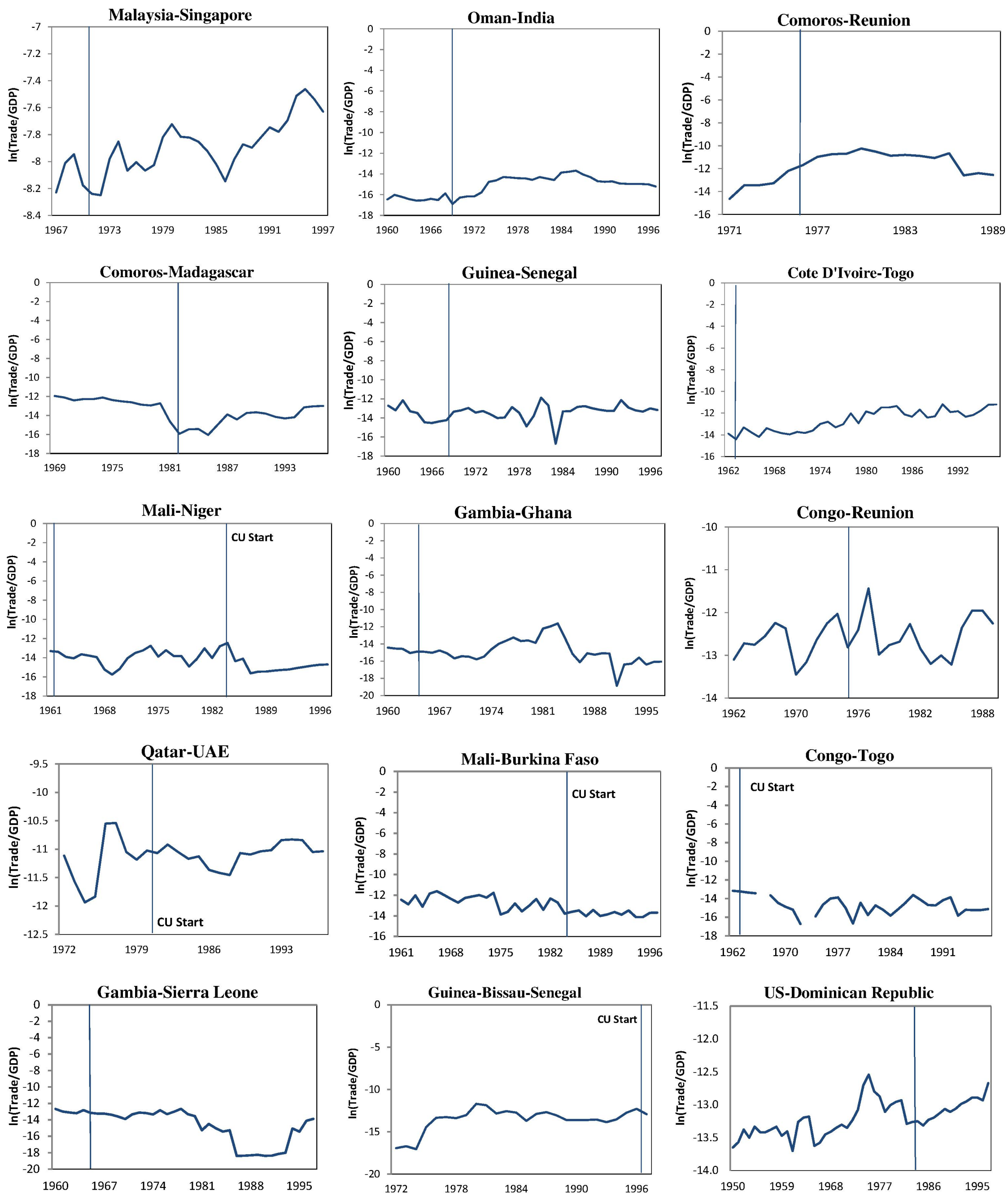
Often, the timing of trade collapses does not substantiate the conclusion that the CU dissolution was the cause, such as is the case with Madagascar and Niger below, right. In this case, inserting a simple dummy variable for CUs could be misleading as trade was on average much larger before the 1981 dissolution than after, but trade collapsed well before the end of the currency union.



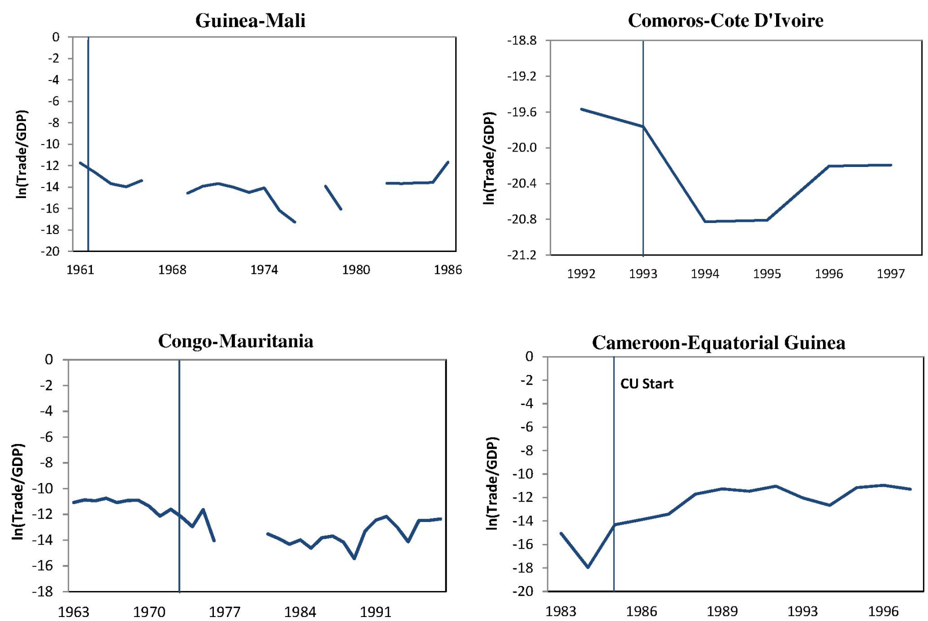
Indeed, while there are numerous counterexamples (Madagascar-Niger above is one), there are few, if any, clear examples which support the proposition that CUs substantially increase trade. Since there are only, by my accounting (tallies in Appendix Table 3), 63 switches that do not involve pairs with former colonial relationships, war, missing data, or some combination thereof, 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 roughly coincided with CU dissolutions. Of this sample of 63 CU switches, there appear to be roughly 45 counterexamples, 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 15 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 for several years after dissolution, and then stayed constant for another decade. Mali and Niger constitute not just one, but two counterexamples, as trade stayed roughly flat after dissolving their currency union in 1961, and then fell after these countries reunified 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 could also be considered to be ambiguous given that the 1985 CU start hinged on the outcome of a Civil War in Cameroon the previous year that was more likely to have depressed trade than the lack of a common currency (graph below).[5] This is one reason 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, while three of these changes 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. In the next section I demonstrate that these three factors—decolonization, war, and missing data—can fully explain the positive correlation between CUs and trade.

2 Estimation

Glick and Rose (2002) estimated the following gravity regression for their baseline estimate:

$$(1) \qquad ln(X_{ijt}) = \beta_0 + \alpha_{ij} + \beta_1 ln(Y_{it}Y_{jt}) + \beta_2 ln(y_{it}y_{jt}) + \beta_3 CU_{ijt} + \epsilon_{ijt}.$$

Where Y_{it} is GDP for country i at time t, 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.* One problem is that autocorrelated errors are a common feature of panel data, and so it is necessary to cluster at the country-pair level to arrive at standard errors robust to autocorrelation (Bertrand, Duflo, Mullainathan [2004] suggest this for difference-in-difference estimates), so that it is no longer required that $E[\epsilon_{ijt}\epsilon_{ij(t-k)}] = 0, \forall k$.[6] The second is that the impact of currency unions on trade may grow over time due to sunk costs, which imply 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 could be smaller. Thirdly, there are no trend terms. Recent work, including Bergin and Lin (2012) and Chen, Mirestean, and Tsangarides (2011), considers dynamics explicitly, while Bun and Klassen (2007) shrink the impact of currency unions on trade by including country-pair specific time trends. I will first propose something even less obtrusive: that we allow the coefficient on colonization to trend (for countries which were ever UK colonies), reflecting the fact that colonial trade relationships have been observed to decay over time as found by Head, Mayer, and Ries (2010), and estimate the following gravity relationship while including a colonial trend instead:

$$(2) \quad ln(X_{ijt}) = \beta_0 + \alpha_{ij} + \beta_1 ln(Y_{it}Y_{jt}) + \beta_2 ln(y_{it}y_{jt}) + \beta_3 CU_{ijt} + \beta_4 Colony_{ij} * Year + \epsilon_{ijt}.$$

The decaying of colonial trade relationships most likely reflects the (declining) importance of lagged trade costs. As stressed by Krugman and Helpman (1985), trade is a dynamic process. Many New Trade Theory models (*e.g.*, Burstein and Melitz, 2011) include sunk fixed costs, which imply that when trade costs change, it could take time to reach a new equilibrium. Empirically, the impact of shocks to trade patterns decay slowly over time, as shown by Eichengreen and Irwin (1998) and Campbell (2010), the latter of which shows that trade patterns are persistent across centuries and that either consumer habits or market-specific learning-by-doing leads to an explicit dynamic gravity formulation whereby trade is a function of lagged trade costs.

To motivate the dynamic controls, I begin by contrasting the measured impact of the dissolution of UK currency unions using the "static" equation (1) vs. the "dynamic" equation (2), 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 baseline regression in column (1), our estimate of the impact of the 26 UK currency unions in the sample on trade is exp(.734)-1 which implies that currency unions increase trade by 108%. Including a simple colony-year interaction—a very mild control—yields a point estimate close to zero, yet with sizable clustered standard errors.[7] GDP and GDP per capita are both included as controls in order to replicate Glick and Rose (2002), yet the results are not sensitive to removing GDP per capita from the regression.[8] In the third column, I have included year dummies for UK colonial pairs, with results similar to column 2 but with even larger errors.

Table 1: The Impact of UK Currency Unions on Trade

	Baseline	Add UK Colony Trend	UK Colony-Year FE
UK Currency Unions	0.734*	-0.043	-0.052
	(0.109)	(0.146)	(.185)
UK Colonial Pair-Year		-0.038*	
Trend Interaction		(0.005)	
Log Real GDP	0.062*	0.063*	0.063**
	(0.025)	(0.025)	(.025)
Log Real GDP per capita	0.778*	0.782*	0.782*
	(0.039)	(0.039)	(.039)
Constant	-5.317*	-4.286*	-5.42*
	(0.667)	(0.670)	(0.667)
Observations	218,087	218,087	218,087
Number of pairid	11,077	11,077	11,077

Each regression includes data from 217 countries from 1948-1997, and includes country-pair fixed effects and clustered SEs. * Significant at 1%; **Significant at 5%. Data fromGlick and Rose (2002). UK colony-year dummies in Column 3 suppressed.

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), implying 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 estimate in the second row 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 regression in 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 57.9%.

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 only significant 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 removing 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 negative, although with a point estimate much smaller than the standard error. Even so, the errors reported here are still likely to be biased downward since the assumption that $E[\epsilon_{ijt}\epsilon_{ikt}] = 0$ was made out of computational necessity, not because it is realistic, as some trade shocks may adversely affect a nation's trade with all of its partners in a 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)
Exclude Wars and Riots	.207***
(Clustered SEs)	(0.118)
Exclude CUs with Missing Data	.110
(Clustered SEs)	(0.107)
Allow Country-Pair Trade to trend	-0.046
(Clustered SEs)	(0.089)

* Significant at 1%; *** Significant at 10%. Each row is a separate regression, with 218,087 observations from 217 countries and 11,077 country-pairs. 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), covering 1948-1997.

3 Robustness

In this section I consider alternative specifications that one could use to strengthen the conclusion from Section 2. One concern is that the classification of CU dissolutions as being coterminous with other major geopolitical events (Appendix Table 1) is inherently a subjective process. Hence, in the second row of Appendix Table 4 I show that the point estimate of CUs on trade is not statistically distinct from zero even when all of the CU switches from the "War and Riots" sample are included. The first row of Appendix Table 4 shows that the estimates for the CU impact on trade for the full sample are significant at 90%. The third row includes the missing data sample, but includes war and country-pair trends as a control, yielding a point estimate close to zero.

To alleviate the missing data problem, I also added observations to the GR (2002) data and used an alternative data set. In the last row of Appendix Table 4, I augment the GR (2002) dataset for five of the country-pairs I classified as having missing data immediately after dissolution using data from Feenstra *et. al.* (2005) and constant-dollar GDP and GDP per capita data from the World Bank.[9] This yields a positive point estimate of about 4.3%, with standard errors more than twice as large. As an alternative specification, I used unilateral exports as the dependent variable, and controlled for exporter and importer GDP separately. In this case, I found that the impact of CUs on trade is not statistically distinct from zero when controlling for country-pair trends alone, and that the point estimate is negative but not significant for the sample that does not include missing data or war. This data set was created directly using IMF DOTS and GDP data from the World Bank, and so has fewer country-pairs.[10]

There are alternative dynamic estimation techniques. One popular option would be to use a lagged dependent variable, and then to use an Arellano-Bond or Blundell-Bond (1998) type of fix to correct for Nickell Bias (1981). I found that the impact of CUs on trade is in fact sensitive to instrumenting for lagged dependent variables, with widely varying point estimates resulting from small, seemingly innocuous changes in the specification.[11] However, Arellano-Bond type estimators have been criticized on the grounds that the availability of a vast number of potential moment restrictions (Bowsher, 2002; Roodman 2009) leads the researcher to either overfit or pick the results most aligned with their preconceived biases.

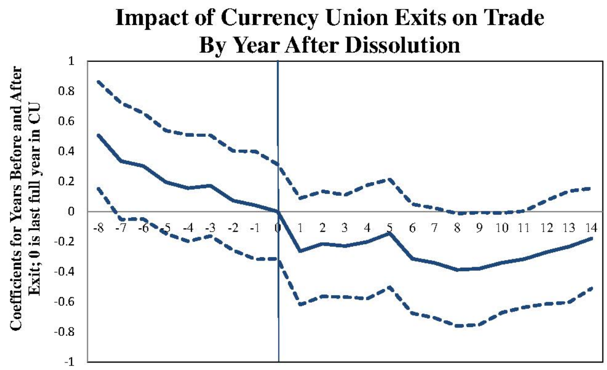
A simple alternative would be to estimate gravity in log changes—regressing the log change in trade on the log change in GDP and a CU dummy for the full sample with country-pair fixed effects. In this regression, using the same CU Dummy as before now carries a different interpretation, as the small, insignificant negative effect implies 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 reveal 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

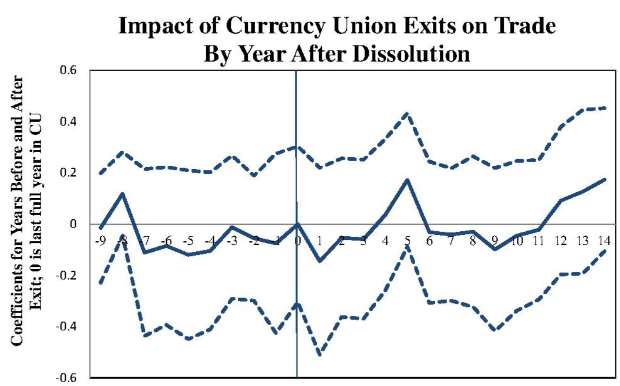
	Levels	Log Changes
Currency Union	0.725*	-0.015
	(0.045)	(0.041)
Log GDP / $\Delta LogGDP$	0.516*	0.389*
	(0.003)	(0.018)
Observations	218,087	195,183
Pairid	11,077	9,571

The first column estimates gravity in levels; the second column in log changes. *significant at 1%; Data from GR (2002). Both regressions include country-pair fixed effects. The CU variable here is an indicator.

One issue with estimating the impact of CUs on trade in log changes, as with including time trends, is that treating each year after a dissolution as being the same could bias the estimate of the overall effect downward if CUs take time to reach their full impact and then plateau. An additional robustness check is to estimate the impact of CUs on trade by year before and after dissolution. The results with 90% error bounds for the baseline regression on the full sample of CU dissolutions with Year FE (2nd regression in Table 2) are plotted below. The results show that substantial trade declines precipitated dissolution, and that the subsequent decline in trade after dissolution was only borderline significant in three individual years thereafter.[12]



The figure below repeats the exercise with the same controls as were included as in the last row of Table 2, which included controls for GDP, GDP per capita, year dummies, country-pair time trends, and the CU observations coterminous with wars and missing data excluded. The estimated 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.[13]



4 Conclusion

The early large and precise estimates for the impact of CUs on trade were driven by major geopolitical events including decolonization and war, and are sensitive to including dynamic controls. My findings reconcile the nonexistent measured effects for indirect pegs, the prewar era, and the more recent euro experience with the large estimated effects for the 1948-1997 period. That the impact of CUs on trade is not robust 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 facing many European countries at the time of writing, should discount previous evidence that there is a large trade channel in their considerations. Secondly, the historical, dynamic 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 implies that economic outcomes today continue to be strongly shaped by history – a topic worthy of further research.

Footnotes

1. Rose (2001) reported on the Rogoff assignment.

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

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

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

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

6. 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.

7. 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.

8. I have posted additional robustness checks such as this on my website; the point estimates for UK CUs are closer to zero but still negative.

9. The country-pairs for which the trade data was augmented with the Feenstra *et. al.* (2005) data include Chad-Madagascar, Guina-Côte d'Ivoire, Côte d'Ivoire-Mauritania, Gabon-Mali, and Madagascar-Mauritania.

10. On the baseline regression, the point estimate of CUs on exports was about 20%, with a t-score above 3 with clustered standard errors. Including country-pair trends reducsed the impact to just 6%, and no longer statistically signficant. Limiting the CU sample by removing the war and missing samples results, once again, in negative point estimates.

11. For example, Arellano-Bond with GDP and per capita GDP included as controls yields a coefficient on CUs of .243, and significant at 95%, while removing per capita GDP as a control cuts the estimate in half and is then no longer statistically significant, even though including this as a control generally has little impact on the level gravity equations. 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 3 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 before and after dissolution.

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: List of Switches Coterminous with a Major Geopolitical Event

Country-Pair	*Last Year of CU*	*Year(s) of Other Events*	*Description*
1. United Kingdom-Zimbabwe	1966	1965; 1964-1979	Independence and Trade Sanctions; Rhodesian Bush War
2. France-Algeria	1968	1954-1962; 1965; 1968	War of Independence; Assasination; Military Consolidation of Govt.
3. France-Morocco	1958	1956	Moroccan Independence following Anti-Colonial Rioting
4. France-Tunisia	1957	1956	Tunisian Independence granted after separatist bombings
5. Portugal-Angola	1975	1961-1975	Angolan War for Independence followed by Civil War
6. Portugal-Cape Verde	1976	1962-1974	Cape Verde part of Guinea-Bissauan War of Independence
7. Portugal-Guinea-Bissau	1976	1962-1975	War for Independence; Marxist takeover, opposition slaughtered
8. Portugal-Mozambique	1976	1964-1975; 1977-1992	War for Independence; Civil War
9. Portugal-Sao Tome and Principe	1976	1974-1975	Declared Independence following Coup in Portugal
10. Bangladesh-India	1973	1971	The Bangladesh Atrocities; 10 million Bengalis Take Refuge in India
11. Burma (Myanmar)-India	1965	1965	India-Pakistan war in 1965
12. Burma (Myanmar)-Pakistan	1970	1965; 1971; 1978	Indo-Pakistani Wars; Myanmar expels 250,000 Muslims
13. Sri Lanka-India	1965	1965	India-Pakistan war in 1965
14. Sri Lanka-Pakistan	1966	1965	India-Pakistan war in 1965
15. India-Pakistan	1965	1965	Border War, repeated conflicts thereafter
16. Côte d'Ivoire-Mali	1961; 1984 (start)	1968; 1980s	Coup in Mali in 1968, movement from Socialism to Free Enterprise in 1980s
17. Kenya-Tanzania	1977	1978	Uganda-Tanzania War and overthrow of Idi Amin
18. Kenya-Uganda	1977	1978	Uganda-Tanzania War and overthrow of Idi Amin
19. Mauritania-Niger	1973	1974	Military Coup in Niger; Nationalization of mines in Mauritania
20. Mauritania-Senegal	1973	1974; 1975; 1978	Nationalization of Mines in Mauritania; Invasion of Western Sahara; Coup
21. Mauritania-Togo	1972	1974; 1975; 1979	Nationalization of Mines in Mauritania; Invasion of Western Sahara; Coup
22. Tanzania-Uganda	1977	1978-1979	Liberation War and Overthrow of Idi Amin
23. Madagascar-Senegal	1981	1982-present; 1989-1991	Low-Grade Civil War in Casamance Region; Senegal-Mauritania Border War
24. India-Mauritius	1965	1965	India-Pakistan war in 1965
25. Pakistan-Mauritius	1966	1965	India-Pakistan war in 1966
26. Madagascar-Reunion	1975	1976	Anti-Islander Rioting in Mahajanga

Appendix Table 2: List of Switches Coterminous with Misssing Data

Country-Pair	*Last Year of CU*	*Year(s) Data is Missing*
1. Cameroon-Mauritania	1972	1973
2. Central African Republic-Madagascar	1977	1976; 1978-1987
3. Central African Republic-Mali	1991 (start)	1974-1990
4. Chad-Madagascar	1970	1971-1985
5. Republic of Congo-Madagascar	1980	1981-2
6. Benin-Guinea	1964	1962-3; 1965-1971; 1974-1986
7. Benin-Madagascar	1974	1975-1987
8. Benin-Mauritania	1973	1972; 1974-1981
9. Gabon-Guinea	1967	1968-1970; 1972-1973
10. Gabon-Madagascar	1975	1976-1983; 1985; 1987
11. Guinea-Côte d'Ivoire	1965	1966-1972
12. Guinea-Mauritania	1968	1969-1985
13. Madagascar-Niger	1981	1983-1988
14. Madagascar-Togo	1974	1972-1973; 1975-1978
15. Madagascar-Burkina Faso	1972	1973-1987
16. Mauritania-Niger	1973	1970; 1972; 1975-1978
17. Mauritania-Togo	1972	1971; 1973-1981
18. Cameroon-Comoros	1987	1988-1994
19. Benin-Reunion	1973	Before 1973; 1974-1991
20. Gabon-Mali	1984 (start)	1979-1983
21. Madagascar-Mauritania	1968	1969-1984; 1985-1989
22. Reunion-Senegal	1975	1976

Appendix Table 3: Number of Changes of CU Status

Entrants	28
Exits	132
Total Switches	160
Entrants with Time Series Variation	27
Exits with Time Series Variation	107
Total Switches with Times Series Variation	134
Missing Data Immediately Before or After Switch	22
War or Other Major Geopolitical Event	27
Switches ex Missing Data or War:	89
Switches ex Missing Data, War, or Former Colonial Relationships:	63

Appendix Table 4: Additional Estimates of CUs on Trade

Full Sample with Country-Pair Trends	.205***
(Clustered SEs)	(0.113)
Eliminate Missing, Include Country-Pair Trends	.172
(Clustered SEs)	(0.117)
Eliminate War CUs, Include Country-Pair Trends	.019
(Clustered SEs)	(0.089)
Eliminate War CUs, add data & include Country-Pair Trends	.042
(Clustered SEs)	(0.091)

***Significant at 10%. Each row is a separate regression, with 218,087 observations and 11,077 country-pairs. All regressions include country-pair fixed effects, log GDP and log GDP per capita as controls. Data from Glick and Rose (2002); the 4th row is augmented with trade data from Feenstra *et. al.* (2005) and GDP and GDP per capita data from the World Bank.