Centre for
Economic
and Financial
Research
at
New Economic
School

June 2017

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Working Paper No 241

CEFIR / NES Working Paper series

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Abstract

As several European countries debate entering, or exiting, the Euro, a key policy question is how much currency unions (CUs) affect trade. Recently, Glick and Rose (2016) confirmed that currency unions increase trade on average by 100%, and that the Euro has increased trade by a still-large 50%. In this paper, we find that the apparent large impact of CUs on trade is driven by other major geopolitical events correlated with CU switches, including communist takeovers, decolonization, warfare, ethnic cleansing episodes, the fall of the Berlin Wall and the whole history of European integration. We find that moving from robust standard errors to multi-way clustered errors alone reduces the t-score of the Euro impact by 75%. Looking at individual CUs, we find that in no cases does the time series evidence support a large trade effect, and that the effect breaks particularly badly once we find suitable control groups. Overall, we find that intuitive controls and omitting the CU switches coterminous with war and missing data render the trade impact of the Euro and all CUs together statistically insignificant.

JEL Classification: F15, F33, F54
Keywords: The Euro, Currency Unions and Trade, Gravity Regressions for Policy Analysis

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†. Special thanks are in order for the comments we have received presenting an early draft to students in our International Macroeconomics course at both the undergraduate and graduate levels at the New Economic School, particularly to Dmitry Limin, and to the Riga BICEPS conference. All errors are our own.

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Greek GDP is currently 26% below its 2007 peak. The economies of Portugal, Spain, and Italy, are still 4%, 8%, and 7%, respectively, smaller in 2016 than their pre-2008 recession peaks.[1] Despite the continued poor economic performance and obvious problems with surrendering control of one's domestic monetary policy, the Euro Zone has continued to grow in size, as Slovakia (2009), Estonia (2011), Latvia (2014), and Lithuania (2015) have all recently joined the Euro, while others still appear to be debating entry, even if it now appears increasingly unlikely.[2] While questions of politics and identity may be the driving force behind the enlargement of the Euro Zone, particularly since the EU has seemed intent on requiring usage of the Euro as a condition for entering the EU, another reason is surely that these countries want to foster closer trade ties with Western Europe.

And, according to recent research Glick and Rose (2016) (hereafter GR), countries would be justified in believing that Euro membership leads to significant trade integration.[3] Using panel data with a dummy for currency unions, GR estimate that, overall, CUs increase trade 100%, while the Euro has increased trade by a still-impressive 50%. This suggests that the benefits of joining a currency union are real, and perhaps large enough to outweigh the detrimental effect of ceding control over one's domestic monetary policy, other benefits of the Euro (such as freer financial flows) notwithstanding. GR also find that not all CUs increase trade – the Eastern Caribbean Currency Union *decreased* trade by 80%, while US dollar unions have no effect. These heterogeneous effects constitute a puzzle in need of an explanation.

In this paper, we subject these findings to a number of robustness checks. First, we consider each disaggregated CU separately, plotting the pre- and post-treatment trend of each major union. We search for a suitable control group for each CU switch, similar in spirit to the "matching" approach implemented by Persson (2001). We try to understand what other major geopolitical events may be correlated with changes in CU status. In

1. Computed using annual data from Eurostat data via FRED. For each country, peak GDP was taken from 2007 or 2008, and compared to 2016. While this period has been christened the "Lesser Depression", the fact remains that the economic performance was worse than it was during the Great Depression. Many economists have noted parallels, faulting the "Cross of Euros" as a modern-day golden straitjacket, particularly as the difficulties of one monetary policy for all became more apparent with the divergent fortunes of the north and south in wake of the Great Recession.

2. Countries that have contemplated joining within the last few years include Bulgaria, Croatia, the Czech Republic, and Poland.

3. Holding fixed the author, Glick (2017) argues for a Euro effect 25% smaller than GR (2016), while Glick and Rose (2016)(b), which pre-dated GR (2016), found that the impact of CUs on trade is sensitive to the specification. Other recent papers that find a positive impact include Kunroo et al. (2016), Camarero et al. (2014), Felbermayr et al. (2017), Gunnella et al. (2015), Gil-Pareja et al. (2008), Martínez-Zarzoso and Johannsen (2017), and Rotili et al. (2014), while Macedoni (2017) finds evidence consistent with the Euro reducing trade costs.

the second part of our paper, adopt a holistic panel regression approach similar to GR (2016), using what we have learned in individual CU cases, to test the robustness of the currency union effect on trade. We find that the CU effect is driven by omitted variables including warfare and communist takeovers, CU switches with missing data, and is sensitive to including additional control variables. In addition, multi-way clustering can reduce t-scores by as much as 75%, as in the case of the Euro. Overall, we arrive at a point estimate of CUs on trade of about 5%, but with standard errors of 6%. We caution that our estimates are still likely to suffer from non-random selection of countries in and out of currency unions, which could bias the results in either direction. Our main finding is that the apparent large impact of currency unions on trade continues to "break badly" when confronted with rather mild controls. Thus, countries on the European periphery should not expect large trade effects of adopting the Euro.

As an example of the endogeneity driving the result, consider the case of India and Pakistan. At first glance, it may look – to one unfamiliar with their history – like a textbook case in which a currency union dissolution crippled trade (see Figure 1), as bilateral trade fell by 99.8%. However, the dissolution occurred simultaneously with the outbreak of a brutal border over the legacy of partition, while political relations between the two countries remain frigid to this day.[4] In addition, all of the countries which left the French Franc – Tunisia, Algeria, and Morocco – did so after major conflicts resulting in independence.[5] These included separatist bombings in the case of Tunisia, a war of independence in the Algerian case, and anti-colonial rioting in Morocco. All five of Portugal's former colonies which had also shared currency unions likewise had to fight for their independence, some of which included prolonged guerrilla wars.[6]

Endogeneity and third factors also play a role in the Euro. The whole history of European integration, from the Coal and Steel Community, to the Fall of the Berlin Wall and the formation of the EU, implies that there are other reasons, aside from the Euro, why European trade has increased over time. Thus, our approach is to compare Western European countries which joined the Euro to those that did not (and, Western European countries which joined the Euro with those that joined the EU). Similarly, for Eastern European Euro entrants (by 2013), we use as a control group other Eastern European countries which either joined the Euro later (Latvia and Lithuania), or are otherwise similar. We find no evidence that the Euro significantly increased trade compared to

4. See Prasad, The India-Pakistan War of 1965: A History (2011).

5. See Thom and Walsh (2002).

6. See, for example, Venter, Portugal's Guerrilla Wars in Africa: Lisbon's Three Wars in Angola, Mozambique and Portuguese Guinea 1961-74, (2013). Note that Cape Verde was also part of the Guinea-Bissauan War of Independence.

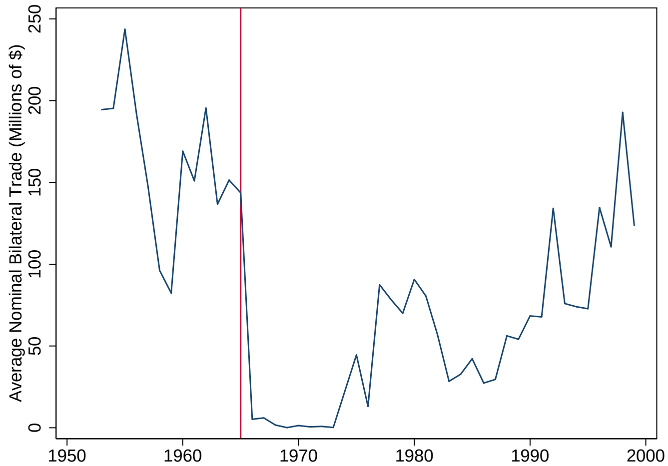


Figure 1: Indo-Pakistani Trade: A Textbook Case of the CU Effect or Endogeneity?

Notes: This figure plots bilateral trade over time for India and Pakistan, one of the CUs in the GR (2016) data. These countries exited a shared currency union in 1965, after which trade plummeted. However, this was the same year as the Indo-Pakistani War of 1965 following Pakistan's Operation Gibraltar.

the most reasonable control groups.

This literature began with A. Rose (2000), who found that CUs triple trade. This effect sounded suspiciously large to some[7], and so a subsequent literature set out to dampen the effect, with titles such as "Honey I Shrunk the Currency Union Effect on Trade" (Nitsch (2002)).[8] However, Rose would typically respond with larger data sets, as Glick and Rose (2002) found that currency unions double trade in a time series setting even when including country-pair fixed effects.[9]

Nevertheless, doubts remained.[10] Nitsch (2005) found no impact for CU entries, Klein (2005) found no trade effect of dollarization episodes, and Bomberger (2003) found that a simple time trend eliminated the effect for the UK colonial sample (one-fifth of the total switches in GR 2002). Thom and Walsh (2002) noted that many CU exits

7. Including to Rose himself, who once wrote "I have always maintained that the measured effect of a single currency on trade appears implausibly large..."

8. Additionally, 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.

9. The result appeared robust enough that in 2005, Harvard's Jeff Frankel called Rose's discovery of the large apparent impact of CUs on trade the most significant finding in International Macroeconomics in the preceding ten years. On the other hand, worried about the endogenous nature of CUs, Alesina et al. (2002) and Barro and Tenreyro (2007) opted for geographic instrumental variable approaches, and found that CUs actually increase trade on a 14-fold and a 7-fold basis.

10. For example, Baldwin (2006) wrote a nice overview of the literature to that point, discussing several reasons why not to trust the larger impacts of currency unions on trade, and concluded that the Euro might have increased trade by a still sizeable 5-10%. Additionally, Bun and Klaassen (2007) include dynamic controls and shrinks the CU impact to a still substantial 25%, and precisely estimated.

had obvious omitted variables such as wars of independence and communist takeovers. Berger and Nitsch (2008) considered early evidence on the Euro, and argued that, given the long history of European trade integration, the key question is whether the Euro increased trade relative to the long-run trend, and found that it did not.[11] Klein and Shambaugh (2006) found that hard currency pegs have a much smaller impact on trade than currency unions, and that indirect pegs – which are much more likely to be random – have no effect on trade at all.

These insights paved the way for Campbell (2013), who showed that the apparent impact of CUs on trade was sensitive to: (a) excluding the CU observations coterminous with other major political events or missing data, (b) including a UK colony time trend, and (c) clustering the standard errors. Campbell also found that much of the impact was suspiciously driven by a decline in trade in the last 10 years before CUs had even dissolved. Thus, controlling for the negative pre-trend, one could arrive at point estimates of CUs on trade which are *negative* and insignificant. However, GR (2016) once again responded with a larger data set updated to 2013 (from 1997), as it included data on 423 switches instead of 136 used in GR (2002).[12]

This paper is the first to use this much larger dataset and test whether the apparent large impact of CUs on trade is driven by other major geopolitical factors. Compared to Campbell (2013), we also study individual CUs at length, plotting the pre- and post-treatment trends for each, in order to find omitted variables and suitable control groups. In addition, we also implement multi-way clustered standard errors, and implement Importer*Year and Exporter*Year FEs in a specification using directional exports, all of which Campbell (2013) does not do. While some other studies also find no effect of the Euro (Nähle (2015), Tykkyläinen (2012), and Figueiredo et al. (2016))[13], A. K. Rose (2017) argues in a meta-analysis that the reason that some Euro studies find smaller, no, or even a negative impact is that they use either fewer countries or fewer years. By contrast, we show that the key is really controlling for other key aspects of European

11. Santos Silva and Tenreyro (2009) also found no effect of the euro on trade. In a meta-analysis, Havránek (2010) 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) argued 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 according to Wolf and Ritschl (2011). López-Córdova and Meissner (2003) find mixed support.

12. The authors should further be commended for plotting pre- and post-treatment trends, and for adopting a new specification with one-directional exports as the dependent variable while controlling for importer*year and exporter*year interactive fixed effects.

13. In a nice test, Martínez-Zarzoso (2017) finds no evidence in the aggregate of trade between African countries pegged to the Franc and EMU countries. Mika and Zymek (2017) focuses on late entrants using data from 1992-2013 using a PPML estimator (used also by GR (2016b)), arguing that the problem is log-linear OLS. We show that even using log-linear OLS, the Euro effect is not robust.

integration, as we use the same data as GR (2016).

In the rest of the paper, we first describe the data and methodology before examining the disaggregated CUs in detail. Then, we proceed with a panel regression analysis.

1 Data

We use the same data set provided by Glick and Rose (2016), with trade data from the IMF's *Direction of Trade Statistics* (DOTs) between 1948 and 2013. Population and real GDP data come from the World Bank's *World Development Indicators*, supplemented with the Penn World Table v7.1 and the IMF's *International Financial Statistics*. Glick and Rose also took data on regional trade agreements from the World Trade Organization. For consistency, we also use the same definition of currency union provided by GR: "that money was interchangeable between the two countries at a 1:1 par for an extended period of time, so that there was no need to convert prices when trading between a pair of countries". Currency union classification were taken by GR from the IMF – see 2016 for details.

Table 1 compares the number of CU switches in GR (2002) vs. GR (2016), for both the Euro and non-Euro observations. GR (2002) had a total of 136 CU switches, 108 of which came from exits. Only 79 of these switches remained, however, after excluding those switches with obvious large other geopolitical events, missing data, or colonial histories. In GR (2016), this sample increases to 368. Thus, we have a much larger sample of CU switches than Campbell (2013) had to consider the impact of CUs on trade. Table 2 sums up switches and observations by disaggregated CU. The largest CU in terms of separate country-pairs was actually the UK, followed by the EMU.

Lastly, Table 6 includes the list of 26 CUs coterminous with warfare or other major geopolitical events from, and the description of events (from Campbell, 2013).

2 Methodology

GR (2002) and GR (2016) begin by estimating the following panel regression in levels:

$$ln(T_{ijt}) = \gamma CU_{ijt} + \beta Z_{ijt} + \gamma_{ij} + \delta_t + \epsilon_{ijt} \tag{2.1}$$

where T_{ijt} is the average of bilateral imports and exports between country i and j at time t reported by both countries, CU_{ijt} is a 0/1 dummy for currency union status, γ_{ij} is a country-pair FE, δ_t is a year FE, and Z_{ijt} includes several other controls. These

Table 1: Number of Changes in Currency Union Status

	GR 2002	GR 2016	GR 2016 (One-directional)
Entrants with Time Series Variation in Data	28	171	372
Exits with Time Series Variation in Data	108	252	558
Total Switches with Time Series Variation in Data	136	423	930
Missing Data Immediately Before or After Switch	0	79	214
War or Other Major Geopolitical Event	25	25	50
Switches ex Missing Data or War:	101	314	646
Switches ex Missing Data, War, or Former Colony:	79	290	588
Total Country Pairs:	11077	14801	34104
% of Country-Pairs with CUs:	1.86	3.54	3.46
Total Observations:	218,087	426,959	879,794
% of Observations with CUs:	1.45	1.74	1.95
Time Period	1948-1997	1948-2013	1948-2013

In the first column, the numbers of switches with time series variation represent the number of switches for country-pairs with non-missing GDP product and bilateral trade for at least one observation both in and out of a CU. For column (2) the same applies for both countries' GDPs and bilateral trade; for column (3) the only required non-missing variable is the (log) export value.

Table 2: Changes in CU Status by Currency Union

Currency Union	GR 2002	GR 2016	GR 2016 (One-Directional)	Observations (2016)
EMU	0	124	270	5024
CFA Franc	53	49	99	15062
ECCA	5	6	11	3062
Australia	2	3	6	1446
UK	25	150	308	14672
French Franc (pre-Euro)	3	2	26	1448
Indian Rupee	6	7	28	2280
US Dollar	4	40	77	5236
Portugal	4	12	22	860
Other CUs (ex-Portugal)	21	25	68	3744
Total	123	418	915	52834

This table plots numbers of country-pairs with at least one CU status switch (note that one country-pair might have more than one switch, so that the totals here will not necessarily add up to the above) with time series variation in data for disaggregated currency unions, requiring the same non-missing variables as for Table 1.

include bilateral log GDP, log GDP per capita, a dummy for regional trade agreements, and another dummy for current colonial status.

We modify this specification by introducing two new variables to control for country-year-specific openness measures. The first control is the log of total exports for country i (minus country i's exports to j) plus total exports for country j (minus country j's exports to i). The second is the same measure, but for imports. The idea is to control for general, year-specific measures of a country's trade costs, since we are interested in isolating the impact of currency unions only on specific country-pair trade. We also include controls for dummies for sovereignty of each country separately, which *a priori* could be expected to be a mild control, yet we find to be influential in some cases.

GR (2016) additionally include a version of this model with richer importer-year and exporter-year FEs, using one-way directional exports as the dependent variable:

$$ln(X_{ijt}) = \alpha CU_{ijt} + \beta Z_{ijt} + \lambda_{it} + \psi_{jt} + \gamma_{ij} + \epsilon_{ijt}, \tag{2.2}$$

where X_{ijt} is the average of exports from i to j reported by i and the same variable reported by country j at time t. CU_{ijt} is a 0/1 dummy for currency union status, λ_{it} are exporter-year interactive FEs, ψ_{jt} are importer-year interactive FEs, δ_{ij} are country-pair FEs, and Z_{ijt} are a number of other controls.

Equations 2.1 and 2.2 form our starting point. Equation 2.1 identifies the impact of a currency union from the time series variation. It asks how much more do countries trade after they join, or before they leave, a currency union. Thus, if countries join a currency union precisely because they trade more, the country-pair fixed effect will control for this. The second specification is similar, only now it asks how much more a country will export to another country which shares a currency union, relative to other exports for that country in a given year, and relative to their trading partner's imports in the same year. The problems with this methodology are two-fold. First, it neglects dynamics. If two countries form a currency union, not just because they trade more, but because their trade intensity is increasing over time, then this method will bias up the results. The second (closely related) problem also stems from the non-random nature of currency union formation. Sharing a currency union is likely to be a proxy for good, or at least stable, political relations. Countries do not leave or form currency unions for no reason. Often, as emphasized by Campbell (2013), currency union dissolutions are associated with wars, the end of colonization, ethnic cleansing episodes, and financial or currency crises.

A fundamental problem with running a massive panel regression with nearly a million observations is that it can be a challenge to understand what is driving the results. Thus, we start by considering each major CU separately in order to understand what potential omitted variables and other factors may be influencing estimates for particular CUs. This will help us find appropriate control groups, similar to the "matching" approach which has been tried by others. In doing so, we will also look at the evolution of trade growth before and after dissolution using equations 2.1 and 2.2, and also run some panel regressions for some of the CUs individually, in order to confirm the intuition gained in the figures. Once we have examined each separate currency union, we'll use what we've learned in returning to a more holistic, general panel regression approach using the full sample.

3 Disaggregating Currency Unions

3.1 The Euro

We start with the Euro, given that this represents 29% of the bilateral switches in currency union status in the one-directional trade regressions with time series variation in the data. Given the different histories of Western Europe and the former Warsaw Pact entrants to the Euro, we consider each case separately, and then take what we learn and move to a panel regression approach.

The differences between these two regions are stark. Among Western European countries, integration in the post-World War II period began in earnest with the European Coal and Steel Community signed at the Treaty of Paris in 1951, the beginning of the European Economic Community in 1958, the Schengen Agreement in 1985, and the formation of the EU in 1993. Thus, the formation of the Euro can be viewed as the culmination of decades of economic integration within Europe. In addition, trade was disrupted during World War II, and thus, as Glick and Taylor (2010) and Campbell (2010) argue, it naturally took decades for historical trade patterns to be reestablished, while wartime animosities might also have depressed trade between, for example, France and Germany for decades.

The collapse of the Soviet Union and the opening of former Warsaw Pact countries to trade with the west was another seminal event which naturally would take decades to play out in full. The history of trade integration in Eastern Europe took a very different path than the rest of Europe, however, so we will consider Eastern Europe separately.

3.1.1 The Euro: Western Europe

We begin by simply plotting the evolution of trade intensity between Western European countries that eventually joined the Euro over time, in Figure 2(a)[14], from the following gravity regression with annual Euro dummies:

$$ln(T_{ijt}) = \alpha_t * I_{ij}^{Euro} + \beta Z_{ijt} + \gamma_{ij} + \delta_t + \epsilon_{ijt}. \tag{3.1}$$

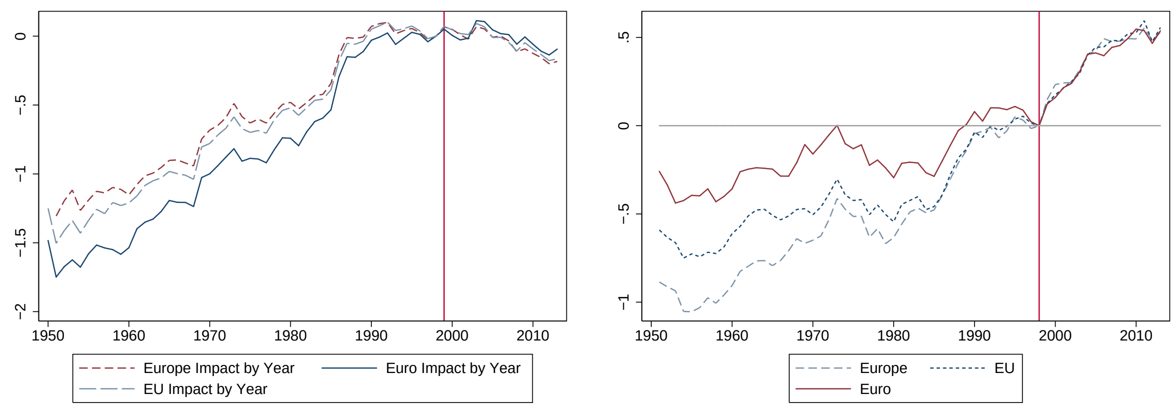
14. *I.e.*, it includes Austria, Germany, France, Belguim-Luxembourg, the Netherlands, Portugal, Italy, Greece, Spain, Ireland, and Finland.

This slight modification of equation 2.1 is a regression of bilateral trade T_{ijt} on annual dummies for country-pairs of countries that eventually joined the Euro (I_{ij}^{Euro}), otherwise controlling for the same variables as in equation 2.1. We also run the completely analogous regression with Euro*year interactive dummies using the directional export specification from equation 2.2, with the results displayed in Figure 2(b). In both cases, all data for country-pairs with at least 40 data points are used to ensure that the results are not driven by changes in the sample over time.

Figure 2(a) also compares the evolution among future Euro countries to all Western European countries and all Western European EU countries, where both are residuals from equation 3.1 plotted from separate regressions. The figure shows that there was a steady increase in trade integration in Western Europe from 1950 to 1990, but that trade then plateaued, or even declined thereafter. The coefficient of -1 in 1970 means that countries that eventually joined the Euro traded about 63% less (=exp(-1)-1) than they did in 1998 (the last year prior to the Euro), relative to what would have been expected based on changes in GDP. Of course, if one ignores dynamics, and merely takes an average of trade before and after, then one would find that trade was vastly higher after the formation of the Euro. Yet, the timing of the increase in trade intensity – from 1950 to 1990 – does not suggest that the formation of the Euro was a driving factor.

The comparison with the EU and all of Western Europe makes for a slightly more optimistic picture of the Euro's effect on trade. The evolution of trade for Europe and the EU naturally look similar to the Euro, as these are largely the same countries. However, trade among Euro countries has decreased slightly less (relatively) than trade between EU or all Western European countries. The caveats to this result are that the positive Euro effect here is too small to be statistically significant, and also that trade intensity among Euro countries had a slight positive pre-trend in the years before the formation of the Euro.

In Figure 2(b), we plot the same relationship, only now directional exports is the dependent variable, and equation 2.2, which also includes importer*year and exporter*year interactive FEs in addition to country-pair FEs. The picture is a bit different, but the conclusion we can draw is much the same. In this specification, trade did increase substantially after the formation of the Euro, but trade among EU and all European countries changed by exactly the same amount compared to 1998, the last year prior to the Euro. However, on the other hand, in this specification, the Euro countries experienced less of a positive pre-trend than the other countries. This suggests that, relative to trend, the Euro might have actually had a positive impact on trade. However, one



(a) Model with CPFEs

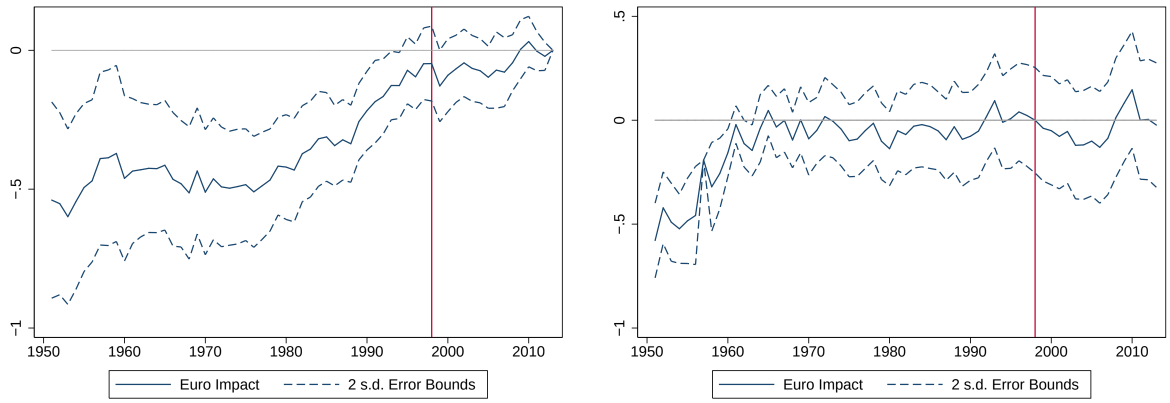
(b) Model with Importer/Exporter*Year FEs

Figure 2: The Euro Impact vs. the EU, Europe

Notes: Panel (a) shows the evolution of the trade intensity of countries which eventually joined the Euro, vs. those that eventually joined the EU, and vs. all of Europe. *I.e.*, it plots annual gravity dummies from equation 3.1. The red bar denotes the last year prior to the formation of the Euro, 1999. All country-pairs with at least 40 observations are used as controls, and this exercise only includes non-Warsaw Pact countries. Panel (b) provides the same comparison, only using directional exports as the dependent variable, importer and exporter*year interactive FEs, from model 2.2.

can only arrive at this conclusion by including pre-trends in the data, and, once again, this difference is not large enough to be statistically significant. Also, the size of the trend differential is likely to be roughly an order of magnitude smaller than the 50% estimate favored by GR (2016).

An alternative approach is to note that since the most natural control group for Euro countries are other countries in Europe (or EU countries), if we re-run each of our models using only data for European countries (Figure 3, *i.e.*, we drop data for other continents from the regression), the picture looks less sanguine. We find that there is no more trade between Euro-Area countries relative to trade with the rest of Europe in 2013 as compared with 1998, the last year pre-Euro, in either specification. In panel (b), in the version of the model with directional exports as the dependent variable, it actually appears that trade in Euro Area countries had declined slightly by 2013 relative to 1998. Of course, this amount is far from being statistically significant. This does raise another problem – our estimated standard errors are actually larger than what many people would find to be an intuitively plausible effect size. This makes it more likely than not that any significant measured effect will simply be spurious. Note that, even in this last specification, if we simply include a dummy for trade before and after the Euro, we will get a spurious positive result for the Euro, since trade did increase significantly in the Euro countries from 1950 to 1965. This increase was far too early to have been due to "anticipation effects". If we estimate from 1965 instead, the estimated effect will shrink (which we show in our panel regressions below).



(a) Model with CPFEs

(b) Model with Importer/Exporter*Year FEs

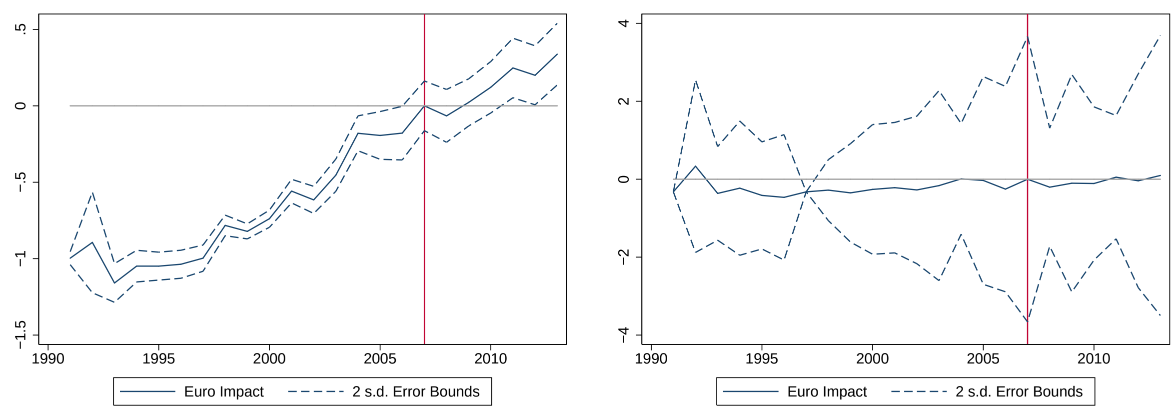
Figure 3: The Euro Effect by Year, with Europe as Control Group

Notes: Panel (a) shows the evolution of the trade intensity of countries which eventually joined the euro vs. the rest of Europe, using equation 3.1. All European countries with at least 40 observations are used as controls. Panel (b) uses the model with Importer/Exporter*Year FEs as in equation 2.2.

3.1.2 Eastern European Euro Entrants

Given the vastly different history of the former Warsaw Pact countries which joined the Euro later, we consider the Eastern European countries separately in Figure 4. In panel (a), we plot the evolution of trade between countries that eventually joined the Euro from Eastern Europe during our sample – Slovenia, the Slovak Republic, and Estonia (who joined in 2007, 2009, and 2011) – and the original Euro entrants using the bilateral trade specification in equation 3.1. Trade growth was slow in the years following the collapse of the Soviet Union, yet has trended up since the mid-1990s. This upward trend long pre-dated the decisions of these countries to join the Euro. Next we use as the control group other Eastern European countries that did not join the Euro, or which joined much later. Thus, we choose Latvia, Lithuania, Hungary, the Czech Republic, and Croatia as our control countries.

In panel (b), we add in annual dummies for these countries' trade with the original Euro entrants (also including Slovenia, the Slovak Republic, and Estonia), effectively to control for trends in trade between Eastern and Western Europe. What we find is that much of the trend is gone, and that, compared with 2007, trade actually declined in most of the years thereafter. However, we also find very large standard errors, and so in the end we conclude that we cannot say much other than that the significant impact we get in panel (a) is gone. In the appendix (Figure 16) we do a version of this model using bilateral trade as the dependent variable and get similar results, except that in that case, the trend goes away completely. Thus we conclude that the case for a large Euro effect on trade for the Eastern European countries is sensitive to the control group used.



(a) Full Sample as Control

(b) Add Eastern European Control Group

Figure 4: Eastern European Euro Entrants

Notes: Panel (a) shows the evolution of the trade intensity of countries which eventually joined the EMU from 2007 to 2011 – Slovenia, the Slovak Republic, and Estonia – and prior EU entrants using equation 3.1, and using the full sample as controls. Panel (b) adds in a control group using annual dummies for trade between a larger number of Eastern European countries, some of whom joined the Euro later and others not at all – Latvia, Lithuania, Hungary, the Czech Republic, and Croatia.

3.1.3 The Euro: Panel Regression Results

Next, we move to a panel regression approach so that we can definitively answer whether the Euro effect is statistically significant pooled across years, and report the results in Table 3. In this table, we use equation 2.1 (using bilateral trade as the dependent variable) in the first three columns and equation 2.2 (which uses directional exports instead), in the following four columns. In column (1), we benchmark the results from GR (2016). In column (2), we add in EU*Year and Eastern Europe-Euro Area*Year interactive fixed effects, using the same control group as in Figure 4. We also add in multi-way clusters. When we do this, the impact of the Euro is approximately cut in half, and the standard errors increase slightly. In column (3), we limit the control group to Western Europe, and also include a simple time trend control and limit the period to after 1975 when the trend starts, as is implied by Figure 3(a). We find that this trend control eliminates the significance of the Euro.

Table 3: How Robust is the Euro Effect on Trade?

	(1) GR (2016)	(2) +Controls	(3) W.Europe	(4) GR (2016)	(5) +Controls	(6) W.Europe	(7) Post-1965
EMU Dummy	0.42***	0.20**	0.089	0.43***	0.055	0.12	0.032
	(0.054)	(0.066)	(0.067)	(0.083)	(0.069)	(0.079)	(0.068)
Ever EMU*Year			0.0078				
			(0.0039)				
Observations	375643	375412	7216	877736	877736	24337	18205
Dep.Variable	Bil.Trade	Bil.Trade	Bil.Trade	Exports	Exports	Exports	Exports
Sample	World	World	W.Europe	World	World	W.Europe	W.Europe

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in the first three columns is the average of 4-way log bilateral trade flows, and in the last four columns it is the average of exports from country 1 to 2 reported by country 1 and reported by country 2. The first three columns include country-pair and year fixed effects, while the last four columns include Importer*year, Exporter*year, and country-pair FEs. In column (1), errors are clustered by country-pair in parentheses, and by country-pair and year from column (2). Column (1) replicates the results from Glick and Rose (2016), Table 2 column (4). Other controls, including GDP and GDP per capita, and dummies for regional trade agreement and currently a colony are omitted for space. Columns (2) and (5) add EU*year and EE-Euro Area*Year interactive FEs. Columns (3), (6), and (7) limit the control group to Western Europe. Columns (3) includes a control variable for trends in trade for countries that eventually joined the Euro. Columns (3) and (7) limit the sample to the post-1975 and post-1965 periods, respectively.

Next, in column (4) we use the model with unilateral exports, and replicate the results from GR (2016), Table 5 column 5, only adding in multi-way clusters, by both country-pair and year. This alone reduces the t-score by 75% (GR reported errors of .02 vs. .083). When we add in the same EU*Year and Eastern Europe-Euro Area*Year interactive controls as in column (2), we get a point estimate of .055 (roughly 5%), but with a standard error of .069. In column (6), we limit the sample and control group to Western Europe (and drop the controls). This time we get a point estimate of 12%, although not significant. In column (7), following the logic learned from plotting our data in Figure 3(b), we limit the sample to the post-1965 period, and find that that the point estimate shrinks to just 3%, again imprecisely estimated.

To conclude, in this section we found that the Euro impact on trade is sensitive to the control group chosen, and can thus be eliminated even without including time trends. We conclude from all these exercises that the Euro Effect on trade is not robust, and that earlier large positive impacts conflated the Euro with the long history of European trade integration, the EU, and the collapse of the Soviet Union and the opening of Eastern Europe to trade. And since the Euro observations constitute 29% of all the CU switches with time series variation, we believe this section alone casts significant doubt on the overall "currency union effect".

3.2 UK Currency Unions

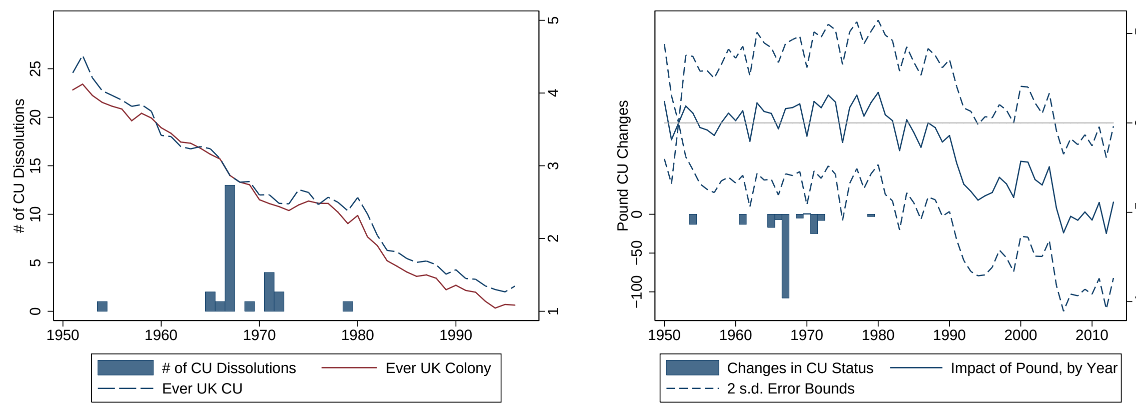
In GR (2002), 26 of the 136 CU switches in the sample involved the UK. In GR (2016), slightly over a third of the CU switches in our data set are of currency unions involving the British Pound.

In this case, since most country-pairs with time series variation in CU status exhibit just one change in status, a dissolution, a panel regression in levels with no controls for trends could be prone to finding correlations even when a true relationship does not exist. The basic problem can be seen in Figure 5(a) below when we compare the evolution of trade between the UK and its former colonies vs. the UK and countries with which it used to share currency unions with (adding yearly dummies to equation 2.1).[15] There is a lot of overlap between the two, as all but one country that shared former currency unions with the UK in this sample were also former colonies, while nearly half of the former colonies had currency unions (30 of 67). The trend for each is negative, consistent with the gradual decaying of former colonial trade ties as stressed by Eichengreen and Irwin (1998), Head et al. (2010), Head and Mayer (2013) and Campbell (2010). In addition, many of the currency unions were dissolved during the Sterling Crisis in the late 1960s. Thus, if one naively takes an average of trade in the 1950 to 1968 period, and compares it with trade thereafter, one will conclude that the currency union dissolution caused the decline in trade. If one includes a simple trend for UK trade with its former colonies, by contrast, one will not find a correlation between CUs and trade.

However, with our new, much larger data set, many of the observations of countries which used the pound did not necessarily involve the UK. Running the second version of our model with directional exports as in equation 2.2 in Figure 5(b) on all UK CUs, we find that trade did decline after, although the decline in trade happened much later, starting in the late 1980s, while most of the dissolutions happened in the late 1960s. Thus, the timing appears suspicious.

Next, we run panel regressions in Table 4, using the regression in equation 2.2 for the first three columns and the model in 2.1 in the last two columns. We separate out the Pound CUs involving the UK from the others. In column (2), we add in UK-UK Colony*Year FEs, and another set of annual FEs when both countries are UK colonies,

15. Indeed, both of the lines plotted come from 2013, who claimed to have "solved" the Glick and Rose puzzle.



(a) Evolution of UK Trade with Colonies vs. CUs (b) All Pound CUs (Dir. Export Specification)

Figure 5: Trade and the Pound

Notes: Panel (a) plots the evolution of gravity dummies over time between the UK and its former colonies, compared to the evolution of trade between the UK and countries with which it shared a currency union (from a gravity regression with only time FEs). Panel (a) is from a model with bilateral trade as the dependent variable and includes year and CPFEs. Panel (b) includes all Pound CUs (including those not involving the UK), and uses the model with directional exports as the dependent variable and includes importer-year and exporter-year fixed effects (and thus has many more observations). Panel (a) uses just the original GR (2002) data. The net decline in CUs each year is a bar chart with magnitude on the left axis. A coefficient near unity in 1997 indicates that trade was (=exp(1)-1) approximately 170% larger than one would otherwise expect.

and the coefficients on both UK CUs and other Pound CUs both shrink moderately. Next, we limit the sample to the pre-1990 period, as suggested by Figure 5(b), and find that the UK CUs are no longer significant. Turning to the model using bilateral trade as the dependent variable (equation 2.1), we find that the Pound increased trade among those who used it by a magical 153% (=exp(.93)-1). If true, this would imply that adopting the Pound might have quite large effects for not just trade, but also for welfare, growth, and development. However, when we included our controls from column (2) in Column (5), the ostensible trade elixir now appears to *reduce* trade by a sizeable 16%, although imprecisely estimated. The non-UK Pound CUs admittedly provide the best evidence for the CU effect, although, as we see, the significance of these depends on the specification.

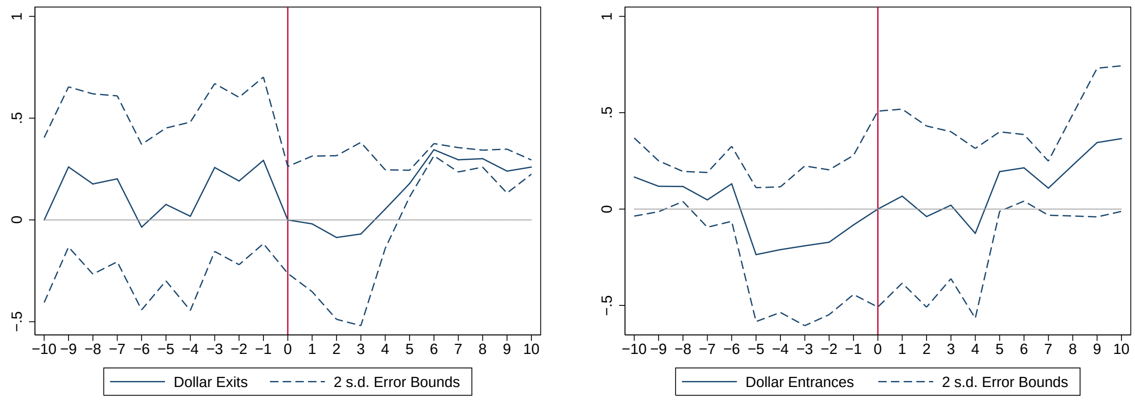
Table 4: British Pound Currency Unions and Trade: How Robust?

	(1) Benchmark	(2) +Controls	(3) Pre-1990	(4) Benchmark	(5) +Controls
UK CUs	0.54***	0.30**	0.13		
	(0.039)	(0.15)	(0.13)		
Pound CUs (ex-UK)	0.56***	0.46***	0.28**		
	(0.045)	(0.13)	(0.13)		
British Pound				0.93***	-0.17
				(0.12)	(0.12)
Observations	877736	871392	368103	426507	372625
Dep.Var.	Exports	Exports	Exports	Trade	Trade

$^*p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in the first three regressions is log exports, and is log trade in the last two columns. The first three columns include Importer*Year, Exporter*Year, and Country-Pair FEs. The last two columns include country-pair and year FEs. Column (1) reproduces the specification from GR (2016), Table 5 (equation 2.2). Column (2) adds in controls as described in the text. Column (3) additionally limits the sample to the pre-1990 period. Column (4) uses equation 2.1, and benchmarks GR (2016), Table 2. Column (5) adds in the same controls as columns (2) and (3).

3.3 US Dollar-based Currency Unions

We begin by plotting the pretreatment and post-treatment trends of exiting and entering dollar unions in Figure 6. The graphs are created by re-running equation 3.1 on annual dummies indicating how many years before or after a change in CU status. What we see is that, reassuringly, there is not much of a long-term "pre-treatment trend" before exits, although trade did fall a lot in the last year of the currency union. However, after exit, within 5 years, country-pairs on the dollar traded significantly more than in the last year prior to exit. Thus, dollar exits appear to foster trade (spurious, in our view) but nevertheless constitute a counterexample.



(a) Exits

(b) Entrants

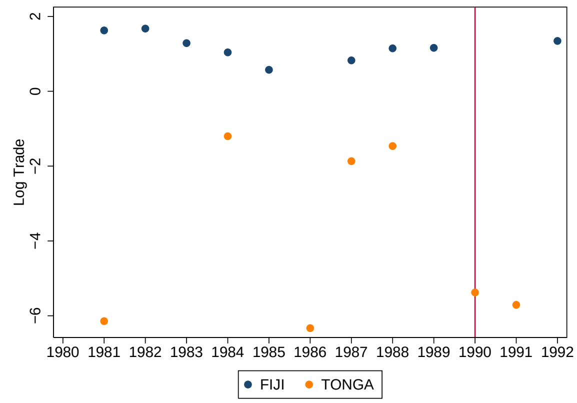
Figure 6: The Effect of Dollar Entrants and Exits

Notes: Panel (a) shows the evolution of the trade intensity of countries which eventually exited the dollar, using equation 2.2. Panel (b) shows the evolution of gravity dummies for the sample of countries that began using the dollar.

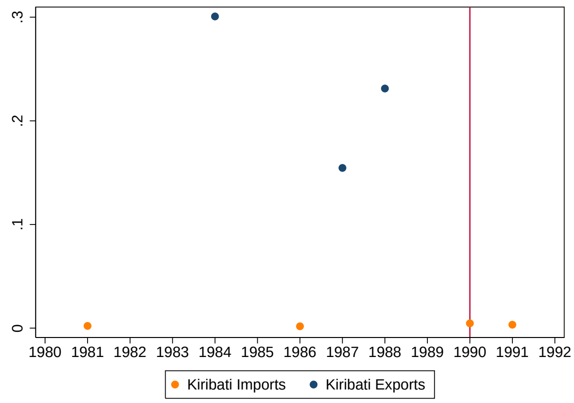
The entrances do not tend to show much, although there appears to have been trade collapses about five years prior to entry. Indeed, on the whole, even Glick and Rose do not find that US Dollar CUs increase trade. Once again, we would argue that this result is another reason to doubt a large CU effect in other settings.

3.4 Australia CUs

Australia shared currencies unions with several small Pacific islands. Thus, we begin by simply plotting trade for several of these islands to try to understand the factors which might be driving the results. Figure 7(a) plots trade between Kiribati and Tonga, who exited a currency union in 1990, and compare it to trade between Kiribati and Fiji, which were never in a currency union. Indeed, we see that trade between Kiribati and Tonga was much lower in the year of dissolution and thereafter, even relative to the "control" of Kiribati and Fiji, matching the theory of Glick and Rose. However, in Figure 7(b), when we separate out imports to Kiribati from Tonga vs. exports, we see that this ostensible trade collapse was driven by missing data. There are only four readings for Kiribati imports from Tonga in the data set, and each one reports similar values. The results are drive by exports from Kiribati to Fiji only being recorded for the date pre-dissolution. Each time they were recorded, they were at a much higher level than imports.



(a) Kiribati-Tonga Trade (vs. Fiji)



(b) Kiribati-Tonga Trade

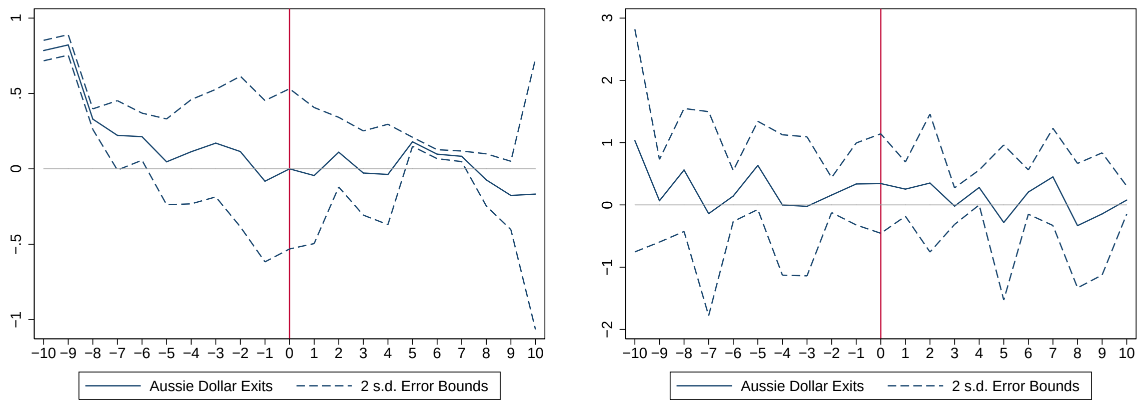
Figure 7: Missing Data Drive "Collapse" in Kiribati-Tonga Trade

Notes: Panel (a) shows the evolution of the trade between Kiribati and Tonga vs. Kiribati and Fiji. Kiribati and Tonga ended their currency union in 1990. After, trade was lower. Kiribati-Fiji might be a good control, but its data is missing in 1990 and 1991. In panel (b), we disaggregate Kiribati-Tonga trade into imports and exports. There were no years in which both exports and import data was recorded.

Next, we repeat the exercise we did for the US, and plot annual indicator dummies for years before leaving an Aussie CU (there are no entrants) in Figure 8(a). While we see little action after dissolution, there happens to be a trade collapse during the CU period starting about 10 years prior to dissolution, which culminates in the year before dissolution. After dissolution, trade stabilizes. Thus, once again, Australian CUs appear to be another counter-example, and one in which a simple dummy strategy in a panel regression in levels will provide misleading inference.

Thinking about an appropriate control group for Australian CUs, and obvious control country could be New Zealand. Thus, in Figure 8(b), we run the same regression only limiting the control group to all countries that ever used the Australian dollar plus their trade with New Zealand. This time, the trade collapse ten years prior to dissolution is much less pronounced, and is no longer statistically significant.

Lastly, we run a few panel regressions based on equation 2.1. In column (1), we replicate the GR (2016) benchmark from their Table (2), column (4). In column (2), we simply restrict the Kiribati-Tonga trade to Kiribati imports, since we have this data recorded before and after dissolution. This small change alone shrinks the magnitude of the impact by about 6%, and also increases the error by about 3%. In column (3), we add in several other mild controls, the log of total exports for each country (ex-bilateral



(a) Impact of Leaving

(b) Relative to New Zealand

Figure 8: Australian Currency Unions

Notes: Panel (a) shows the evolution of the trade intensity of countries which had currency unions with Australia (Tuvalu, Tonga, and Kiribati) after separation, using equation 2.2, and using the full sample as controls. Panel (b) looks uses these countries' trade with New Zealand as the main control.

trade), and total imports (also ex-bilateral trade). These mild controls further reduce the coefficient by another 8%, at which the coefficient on Australian currency unions is only significant at 10%. In column (4), we add in country*year interactive FEs for each of the countries that have Aussie CUs – Australia, Tonga, the Solomon Islands, and Kiribati. This time, we get a negative coefficient, albeit with large standard errors. Finally, in column (5), we create a matching sample, limiting to these countries trade between each other and New Zealand. Now the estimate returns to a fairly large 20%, albeit once again imprecisely estimated.

Table 5: Australian Currency Unions and Trade: How Robust?

	(1) Baseline	(2) Data Adjustment	(3) Add Controls	(4) Add CPFE	(5) Matching Sample
Australian Dollar	0.84**	0.78**	0.70*	-0.049	0.20
	(0.37)	(0.40)	(0.38)	(0.46)	(0.21)
Observations	426952	426945	426272	426272	175

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each regression includes country-pair fixed effects, and errors clustered by country-pair in parentheses. Other controls, including GDP and year FEs omitted for space. Columns (2) and (3) substitutes Kiribati log imports from Tonga in place of total trade. Column (3) uses trade between the Solomon Islands and Tonga with New Zealand as the matching control group.

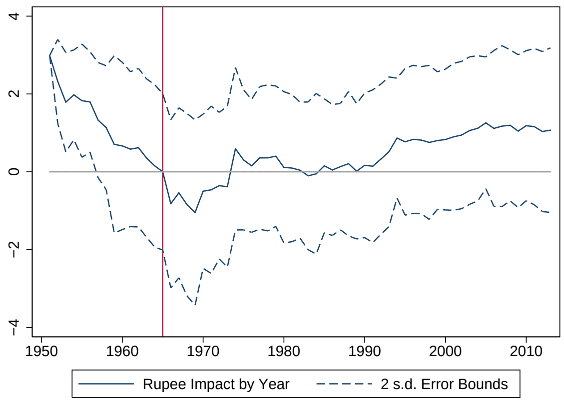
3.5 The Demise of French and Portuguese Currency Unions

In our sample, France had two currency unions with time-series variation in the data, and Portugal had five. However, in all of these cases, the end of these currency unions was coterminous with often violent wars for independence. The tamest of these was Morocco, where independence was granted following widespread anti-colonial rioting. Each of Portugal's colonies that shared currency unions – Angola, Cape Verde, Guinea-Bissau, Mozambique, and Sao Tome and Principe – had to fight for their independence. In Guinea-Bissau, the war for independence ended with a Marxist takeover in which the opposition was slaughtered. It is simply unimaginable that, in cases like this, the currency had a large negative effect on trade, but that a communist takeover of the government did not affect trade at all. Thus, in our panel regression results in the next section, we will test whether the results are sensitive to dropping this sample.

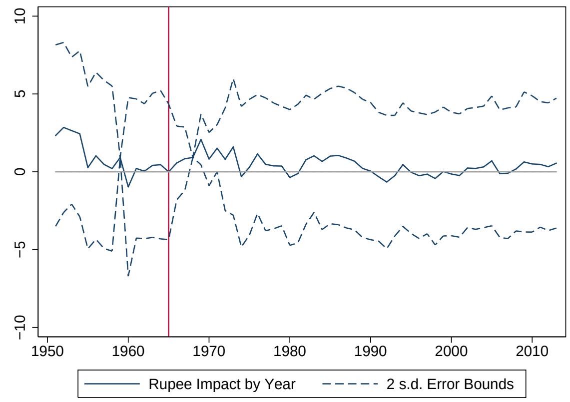
3.6 The Rupee Zone

Next, we turn our attention to the Indian Rupee zone. As mentioned in the introduction, this is another example of a CU effect perhaps driven by endogeneity. This is true not just for India and Pakistan, which fought a war in 1965, which also likely affected trade between Pakistan and Sri Lanka. Bangladesh and India ended their currency union in 1973, just following the Bangladesh Atrocities, after which 10 million Bengalis took refuge in India. These massive events likely overshadowed the impact of a change in currency union status.

Our first exercise is to plot a yearly dummy variable for country pairs which had ever shared the Rupee in Figure 9(a) (analogous to equation 3.1). We find a negative trend in trade from the early 1950s to 1965, when several Rupee unions first dissolved (the other two dissolved in 1969 and 1971). Again, this is something of a counterexample, as it implies that trade declines during currency unions. However, when we exclude the observations involving war in panel (b), the pre-trend becomes much less prominent, particularly relative to large standard errors. Indeed, in panel (b), it appears there is no discernable effect of leaving the Rupee, although if anything, trade appeared to be slightly higher in many years after dissolution relative to the last year before the Rupee union unwound.



(a) Impact of Leaving the Rupee



(b) Excluding War

Figure 9: The Rupee Zone

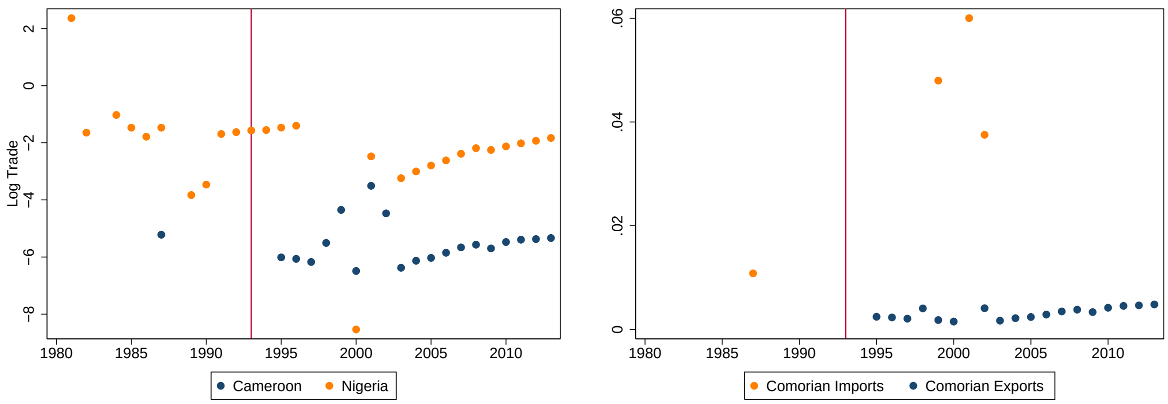
Notes: Panel (a) shows the evolution of the trade intensity of countries over time which shared the Rupee. The vertical red line indicates the dissolution of four of these unions, with two others dissolving in 1969 and 1973. Panel (b) excludes India and Pakistan.

3.7 The CFA Franc Zone

We plot the evolution of trade between Comoros and Cameroon, who dissolved their currency union in 1993, vs. Comoros and Nigeria, the latter of which was never part of the CFA in Figure 10(a). After the dissolution, trade was lower on average. However, trade between Comoros and Nigeria also fell, despite no CU dissolution. Next, in panel (b), we break up trade for Comoros and Cameroon into imports and exports. We see that, in fact, Comorian imports actually increased after dissolution – another counterexample – but that exports were only recorded after dissolution. These were always at a lower level than the one import reading available before dissolution. Thus, there is a reason to be concerned that missing data might be driving the apparent large impact of the CFA Franc on trade as well.

Next we plot the evolution of CFA Franc trade before and after dissolution (Figure 11(a)), and entrance (Figure 11(b)).[16] We find, once again, that the timing of the trade collapse in the case of exits is a bit odd. There is a significant, and massive, decline in trade from 15 to 5 years before dissolution. After that, trade was relatively flat before and after dissolution. The timing of the trade decline, and subsequent recovery post-dissolution, suggests that the CFA constitutes another counterexample, even though a simple dummy variable regression approach will find that trade flows were significantly higher in the pre-dissolution period. The trade dynamics for entrants in panel (b), on

16. Once again, here we are using model 3.1.



(a) Comoros-Cameroon Trade (vs. Nigeria)

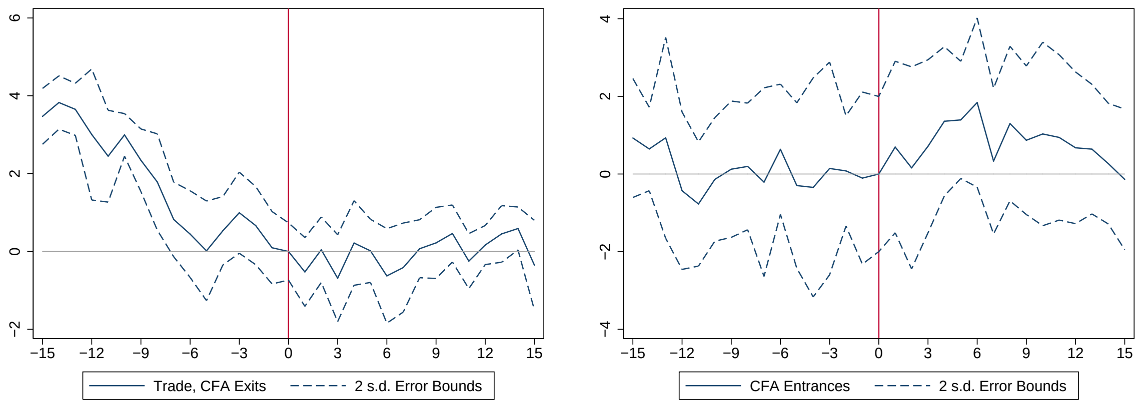
(b) Comoros-Cameroon Trade

Figure 10: Missing Data Drive the "Collapse" in Trade

Notes: Panel (a) shows the evolution of the trade between Comoros and Cameroon, who ended their CU in 1993, vs. a control group of Comoros and Nigeria. In panel (b), we disaggregate Comoros-Cameroon trade into imports and exports. We see that Comorian imports actually rose after dissolution, and that there is no Comorian export data before.

the other hand, admittedly does provide suggestive evidence for the proposition that currency unions increase trade. However, even then, the dynamics look questionable, as bilateral trade was roughly the same 15 years after the CU as there was before.

Next, in Table 6, we test the impact of the CFA Franc using a panel data regression approach as in 2.1. In column (1), we benchmark the results in Table 2 column (4) of GR (2016). In column (2), we exclude the trade collapse that took place more than 5 years before the end of CUs, effectively comparing trade in the last 5 years of a CFA Franc relationship to the period after. The coefficient shrinks to .29 with a standard error of .34 and is thus insignificant. In column (3), we limit the control group to Africa, and include separate dummies for the CUs more than 5 years before dissolution. In column (4), we exclude observations where either import or export data is missing. In fact, in this case, the coefficient actually increases slightly, although so does the standard error. In column (5), we add in Africa*Year FEs instead. In column (6), we use the second model (equation 2.2) with exports as the dependent variable, and include our separate dummies for the CFA observations more than 5 years before dissolution. In column (7), we additionally limit the sample to Africa. The main message here is that, while we do not necessarily have a favorite specification, the original estimates of .89 and .72 is not robust, even if the point estimate is still large.



(a) Exits

(b) Entrances

Figure 11: Impact of CFA Exits and Entrances

Notes: Panel (a) shows the evolution of trade before and after exits into the CFA Franc using equation 3.1. Panel (b) shows the evolution of trade before and after entrances.

Table 6: The CFA Franc and Trade: How Robust?

	(1) Benchmark	(2) +Controls	(3) Only Afr.	(4) +Controls	(5) Ex-Missing	(6) Model 2	(7) Only Afr.
CFA Franc	0.89***	0.29	0.41	0.43	0.49	0.75**	0.36
	(0.33)	(48.2)	(0.35)	(79.9)	(98.4)	(0.35)	(0.41)
Observations	376176	375412	20240	313088	313149	871392	41762
Sample	Full	Full	Africa	Full	ex-Missing	Full	Africa
Dep. Var.	ln Trade	ln Trade	ln Trade	ln Trade	ln Trade	ln Exp.	ln Exp.

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The first five columns include country-pair and year fixed effects. Column (1) includes errors clustered by country-pair, and columns 2-5 include errors clustered by country-pair and year. Columns (6) and (7) include errors clustered by country-pair. Column (1) replicated Table 2 column (4) from GR (2016). Other controls, including GDP, GDPPC, and dummies for RTAs and Currently a Colony are omitted for space. In column (2), we added in controls for total exports and imports (ex-bilateral trade).

3.8 Concluding Thoughts on Disaggregated CUs

In this section we looked at CUs involving the Euro, the British Pound, the US dollar, the Australian dollar, French and Portuguese CUs predating the Euro, and the CFA Franc zone. In total, these CUs accounted for around 92% of the CUs in the GR (2016) sample. (We leave out the East Caribbean Currency Area and the "Other" CUs, as GR (2016) also did not find a significant positive effect for either.) Essentially nowhere did we find robust evidence of a CU effect, and, on the contrary, we found many counterexamples. CFA entrances and Pound CUs not involving Britain probably give the most hope, but even in both of these cases the evidence is mixed.

4 Panel Regressions

Next we turn to panel regressions, employing what we have learned in the previous section, and test how robust the apparent large impacts of CUs, both for individual CUs and overall, is on trade.

4.1 Importer/Exporter-Year Interactive Fixed Effects

First, we run the following regression, very similar to equation 2.2 but with disaggregated CUs:

$$ln(X_{ijt}) = \sum_{k=1}^{K} \alpha^k CU_{ijt}^k + \beta Z_{ijt} + \lambda_{it} + \psi_{jt} + \delta_{ij} + \epsilon_{ijt}, \tag{4.1}$$

where X_{ijt} is exports from i to j at t,[17] CU_{ijt}^k is a 0/1 dummy for currency union k status (examples include the Euro, the Pound, etc., of K=9 unions total), λ_{it} are exporter-year interactive FEs, ψ_{jt} are importer-year interactive FEs, δ_{ij} are country-pair FEs, and Z_{ijt} are a number of other controls. These other controls include dummies for current colonial status and regional trade agreements.

We estimate this model in Table 7. In column (1), we benchmark the results from Glick and Rose Table 5 (right panel), which uses robust standard errors. Note that each disaggregated currency union ostensibly has a widely-varying impact on trade. If we interpret this as a causal relationship, then it would be a major puzzle, as the Eastern Caribbean CU apparently reduced trade by 81% (exp(-1.64)-1), while the French Franc apparently increased trade by a staggering 139%. If, however, these effects are driven by endogeneity, omitted variables, and the non-random nature of CU formation and dissolution, then this is simply noise and not a puzzle, as different historical factors drove the formation of each individual currency union. In column (2), we add in multi-way clusters, by country-pair and year. This tends to cause standard errors to increase substantially, as the error on the EMU increases from .021 to .086. The Australian Dollar and the Indian Rupee unions are now no longer statistically significant. In column (3), when we exclude the CU observations in which switches were associated with warfare or missing data, as in Campbell 2013 (see Appendix Table 1), we find that impact of the French Franc shrinks from 140% (exp(.87)-1) to just 48%, and no longer has a statistically significant impact. Several of the other coefficients change radically – the rupee goes from a coefficient of .52 (with a standard error of .32), to an impact of -.079, for a decline of trade of roughly 8%. However, the apparent impact of the CFA Franc

17. In fact, it is the average of exports from i to j at t reported by i and the same flow as reported by j. When only one country reports the value, that value is used rather than an average.

actually increases to .9, for an increase in trade of 146%. Once again, we believe the most likely explanation for these disparate results is the non-random nature of currency union switches.

In column (4), we add in a number of intuitive controls meant to capture the various factors discussed in the previous section. These include (a) UK Colony*year interactive fixed effects, (b) Ever UK colony*year FEs, (c) year*Ever EU effects, to control for a dynamic EU effect (thus we ask, what was the impact of the Euro relative to the EU?), (d) a Eastern Europe-EU*Year interactive control (as discussed above in Section 3.1.2) (e) A dummy to capture the trade collapse of CFA exiters more than 5 years before dissolution. The result of including these controls is that none of the currency unions by themselves have surviving significant positive impacts on trade with the exception of the CFA Franc and the British Pound, both at just 95% rather than the 99% estimated by GR (2016). However, the coefficient on the CFA Franc, at .75, is too large to be believed, and we also previously found that when we limit the control group to Africa, the point estimate turns negative. When we aggregate into the EMU vs. all other currency unions in column (5), we once again see that neither currency union grouping has a significant impact on trade. When we combine all currency unions together in column (6), we get a point estimate of .051, or about 5%, albeit imprecisely estimated. We would caution, though, that aside from the large errors here, there is likely to be remaining endogeneity and omitted third factors which could cause this estimate to be biased upwards or downwards. All we claim from this exercise is that the Glick and Rose estimate that currency unions increase trade by 40% (using the Importer and Exporter-year interactive FEs model with country-pair FEs as well), is sensitive to the inclusion of intuitive controls. We make no claim that even this regression is free from additional omitted variables and endogeneity – quite likely it is. We actually prefer the analysis in the previous section, which also suggests there is scant evidence that individual currency unions had a significant positive impact on trade.

4.2 Country-Pair Fixed Effects Regressions

Next, we run the following country-pair fixed effects (CPFE) regression:

$$ln(T_{ijt}) = \sum_{k=1}^{K} \alpha^k CU^k_{ijt} + \beta Z_{ijt} + \gamma_t + \delta_{ij} + \epsilon_{ijt}, \tag{4.2}$$

where T_{ijt} is bilateral trade between country i and j at time t, CU^k_{ijt} is a 0/1 dummy for the status of currency union k between country i and j at time t, γ_t are year FEs,

Table 7: How Robust is the CU Impact on Trade? (Full Suite of FEs)

	GR Benchmark	Cluster	Ex-War	+Controls	More Agg.	Overall
EMU	0.43***	0.43***	0.43***	0.075	0.071	
	(0.021)	(0.086)	(0.085)	(0.071)	(0.071)	
CFA Franc	0.58***	0.58**	0.90***	0.75**		
	(0.100)	(0.24)	(0.31)	(0.35)		
East Caribbean CU	-1.64***	-1.64***	-1.64***	-1.68***		
	(0.11)	(0.25)	(0.25)	(0.21)		
Aussie	0.39**	0.39	0.36	0.34		
	(0.20)	(0.41)	(0.42)	(0.40)		
British Pound	0.55***	0.55***	0.51***	0.22**		
	(0.034)	(0.096)	(0.10)	(0.093)		
French Franc	0.87***	0.87***	0.39	0.46		
	(0.083)	(0.27)	(0.27)	(0.31)		
Indian Rupee	0.52***	0.52	-0.079	-0.064		
	(0.11)	(0.40)	(0.49)	(0.47)		
US Dollar	-0.051	-0.051	0.031	0.031		
	(0.063)	(0.19)	(0.20)	(0.19)		
Other CUs	-0.10*	-0.10	-0.39	-0.40		
	(0.058)	(0.23)	(0.27)	(0.27)		
Non-EMU CUs (ex-War, Missing)					0.040	
					(0.089)	
CUs (ex-War, Missing)						0.051
						(0.064)
Observations	877736	877736	871392	871392	877736	877736

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the average of log exports from country 1 to country 2 reported by each. Each regression includes country-pair and Importer*year and Exporter*year interactive fixed effects. Other controls, including a dummy for regional trade agreements and currently a colony are omitted for space. Column (1) replicates Table 5 column (6) of Glick and Rose, 2016. Column (2) clusters the errors by both country-pair and year. Column (3) excludes the CU observations in which switches in status are coterminous with major geopolitical events or missing data. Column (4) includes a number of additional intuitive controls. Columns (5) and (6) use the same controls as in column (3), only aggregating the currency unions into the EMU vs. all others in column (5), and all currency unions together in column (6).

δ_{ij} are country-pair FEs, and Z_{ijt} are a number of other controls. These other controls include standard gravity arguments, including bilateral GDP, bilateral GDP per capita, total exports and imports of both country pairs (ex-bilateral trade), dummies for current colonial status, regional trade agreements, and also dummies for whether a country is a sovereign nation or not.

In Table 8 column (1), we replicate Table 2, column (4) of GR (2016). In the second column, we add in a number of intuitive controls, and also multi-way clustered errors, the latter of which only have a mild impact in this case. The additional controls include dummies for sovereign nations, and also total exports to the rest of the world (of both countries summed) and total imports from the rest of the world (of both countries summed; both figures are ex-bilateral trade). While these sound like mild controls, they have a dramatic impact on about half of the coefficients. The coefficient on Indian Rupee goes from 1.7 to 1.39, and the coefficient on “Other CUs” goes from 1.15, and highly significant, to just .73. The coefficient on East Caribbean CU goes from -.24 to -.85, and significant. In column (3), we exclude the CUs in which changes in CU status were coterminous with warfare or another significant geopolitical event. This kills the impact of the Indian Rupee, as it removes the CU between India and Pakistan. It also turned out that the dissolution of all three of the French CUs with countries that have GDP data happened to have been coterminous with warfare. Column (4) includes a number of intuitive controls, analogous to Table 7 column (3). In column (4), the only CU which is still significantly positive is the EMU. This is also true in column (5), when we additionally exclude the CUs in which switches in CU status are coterminous with missing data. In this case, the coefficient on the British pound is reduced to an imprecise -.17, quite distinct from the estimates we had in Table 7.

Next, in Table 9, we compare the country-pair fixed effects estimates on all currency unions aggregated together from various estimates in the literature, and our new estimates. In Glick and Rose (2002), the authors found a coefficient on currency unions of .65, implying a near doubling of trade, precisely estimated with a t-score of over 15. However, Campbell (2013), using the same data, found that the coefficient fell to just .11, and imprecisely estimated, when (1) year FEs were included (these alone actually shrank the impact significantly), (2) CUs with switches coterminous with wars or missing data were excluded, and (3) a trend control for UK trade with its former colonies was included. In this case, we have also clustered the errors by both country and year, which do not make a major difference. Column (3) benchmarks the results from GR (2016), which greatly expanded the sample and again implied a doubling of trade. However, when we exclude the war CUs and observations coterminous with missing data in column (4), and also include a number of intuitive controls (including UK Colony*year interactive FEs), the coefficient on currency unions falls to just .11, and once again imprecisely estimated. When we separate the effect into the EMU vs. the non-EMU CUs, once again the GR (2016) results benchmarked in column (5) are not robust in column (6) when we omit the war CUs and add other controls in column (6). While it might seem a suggestive coincidence that both columns (2) and (4) imply a still-large impact of currency unions on trade close to 11%, neither are precisely estimated, while Campbell (2013) also found that including trend controls yield an impact of -5%, while Table 7 yields an estimate of 5%. Clearly, these are noisy estimates which are likely to

Table 8: How Robust is the CU Impact on Trade? (CPFE Regressions)

	(1) GR 2016	(2) +Controls, MWCs	(3) Ex-War	(4) +Controls	(5) Ex-Missing
EMU Dummy	0.41***	0.43***	0.43***	0.16**	0.16**
	(0.054)	(0.068)	(0.068)	(0.068)	(0.068)
CFA Franc Zone	0.72**	0.67**	0.72**	0.29	0.71
	(0.29)	(0.28)	(0.31)	(0.35)	(0.50)
East Caribbean CU	-0.24	-0.85***	-0.91***	-1.39***	-1.39***
	(0.29)	(0.29)	(0.25)	(0.28)	(0.28)
Australian Dollar	0.81**	0.66	0.63	0.088	0.090
	(0.37)	(0.42)	(0.43)	(0.47)	(0.57)
British Pound	0.93***	0.79***	0.63***	0.036	-0.17
	(0.12)	(0.14)	(0.13)	(0.13)	(0.12)
French Franc	1.00***	1.04***			
	(0.15)	(0.14)			
Indian Rupee	1.70***	1.39**	0.79	0.22	-0.019
	(0.55)	(0.57)	(0.94)	(0.38)	(0.26)
US Dollar	0.093	0.093	0.058	0.10	0.25
	(0.21)	(0.22)	(0.22)	(0.21)	(0.20)
Other CUs	1.15***	0.73**	0.56	0.42	0.19
	(0.35)	(0.34)	(0.53)	(0.56)	(0.57)
Observations	426507	425836	375196	375115	372625

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the average of 4-way log bilateral trade flows. Each regression includes country-pair and year fixed effects. In column (1), errors are clustered by country-pair in parentheses, and by country-pair and year from column (2). Column (1) replicates the results from Glick and Rose (2017), Table 2 column (4). Other controls, including GDP and GDP per capita, and dummies for regional trade agreement and currently a colony are omitted for space. Column (2) adds in multi-way clusters, and additional control variables, including total exports (ex-bilateral exports) for both countries, dummies for sovereignty. Column (3) excludes CU switches coterminous with warfare. Column (4) adds in the additional controls mentioned in the text. Column (5) excludes CU switches coterminous with missing data.

be influenced further by additional controls.

4.3 Pre- and Post-Treatment Trends

One of the major findings in Campbell (2013), also repeated in GR (2016), was that there are clear pre- and post-treatment trends. This indicates non-random treatment and suggests the need to control for dynamics. First, we plot the pre- and post-treatment trends (before and after CU exit/entry) in Figure 12 using the GR Table 5 specification using exports as the dependent variable. Similar to Campbell and GR, we again find that trade had fallen a significant amount in the last five years of a CU in Figure 12(a), and that trade had increased about 40% in the last 15 years before a CU was formed in

Table 9: The Currency Union Effect over Time: Booms and Busts

	(1) GR 2002	(2) Campbell 2013	(3) GR 2016	(4) +Controls	(5) GR 2016	(6) +Controls
Strict Currency Union	0.65***					
	(0.043)					
CU (Ex-War, Missing)		0.11				
		(0.11)				
Currency Union			0.63***	0.11		
			(0.067)	(0.073)		
EMU					0.41***	0.16**
					(0.054)	(0.068)
Non-EMU CUs					0.75***	0.076
					(0.099)	(0.11)
Observations	216941	216941	426507	372611	426507	372611

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each regression includes country-pair FEs (CPFEs). Column (1) benchmarks the baseline estimate from GR (2002), absent year FEs. Column (2) benchmarks the results (absent trend controls) from Campbell (2013), and includes year FEs. Columns (3) and (5) benchmark the CPFE results from GR (2016). Columns (4) and (6) omit the CUs in which switches were coterminous with war or missing data, and also includes other intuitive controls.

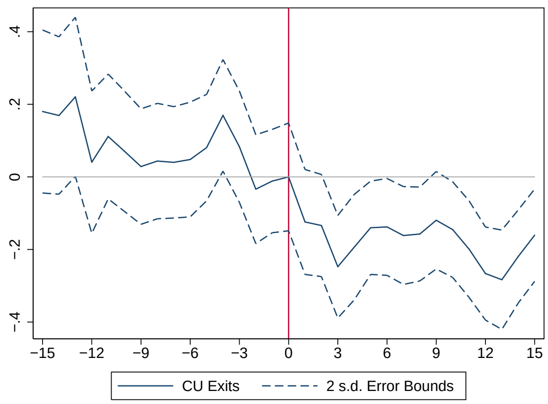
(Figure 12(b)).

What happens to the pre- and post-treatment trends when we exclude the "war" CUs, and missing data, and include our controls? In Figure 13, we explore just that. In this case, there is no pre- or post-treatment effect for CU exits. For entrances, there is still a suspicious jump in trade the last three years before a CU Entrance, and some further increase after entry, although the average increase is still fairly volatile, and comes with large standard errors (six years after entry, trade had not increased). We read this as a likely indication that we have not quite controlled for all forms of endogeneity with the entrances. However, we also do not necessarily see a pressing need to control for dynamics, as the pre- and post-treatment trends seen in Figure 12 are largely gone in Figure 13.[18]

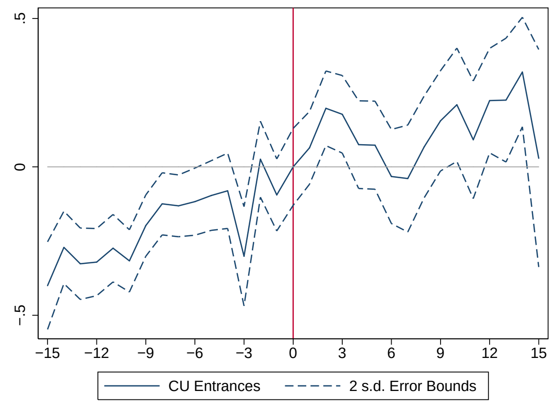
5 Conclusion

To conclude, we find that the apparent large impact of currency unions on trade is driven by third factors, and is sensitive to intuitive controls. Once the EU is controlled for, the Euro does not appear to have a significant positive impact on trade. The apparent large

18. Nevertheless, we provide additional dynamic regression results in the Appendix in 7.1, which show that the apparent impact of CUs on trade is also sensitive to dynamic specifications, including running the specification in log changes or including a lagged dependent variable.



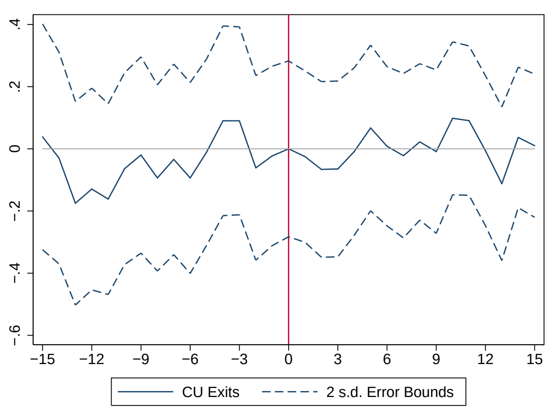
(a) Exits



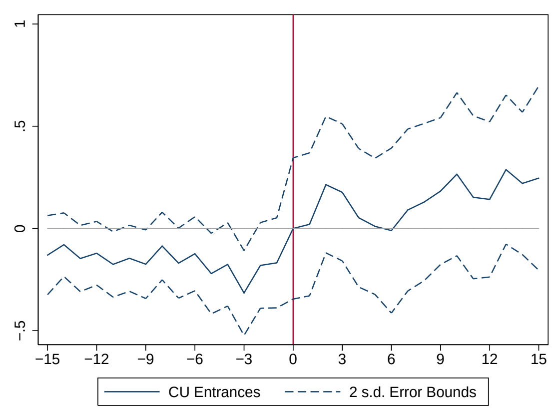
(b) Entrances

Figure 12: Impact of CU Exits and Entrances (GR Specification)

Notes: Panel (a) shows the evolution of trade before and after CU exits using equation 3.1. Panel (b) shows the evolution of trade before and after entrances, ex-EMU.



(a) Exits



(b) Entrances

Figure 13: Impact of CU Exits and Entrances (Ex-War and Missing, w/ Controls)

Notes: Panel (a) shows the evolution of trade before and after CU exits using equation 3.1, excluding the “War” CUs, and including the controls from Column (6) of Table 7. Panel (b) shows the evolution of trade before and after entrances, using the same regression as in Panel (a).

trade impact of nearly every other individual CU breaks, often badly, once one (1) plots the pre- and post-treatment trends, (2) restricts analysis to reasonable control groups, (3) excludes CUs coterminous with war, missing data, or some other large geopolitical event, and (4) clusters the standard errors. A limitation of our study is that even we do not believe we have removed all sources of endogeneity or controlled for all possible omitted variables in our panel regressions, so that our final insignificant results could still be biased in either direction.

Although a surprising amount of resources in terms of economists have been devoted to this question, we believe it has been an avoidable distraction, and deserves to be a textbook case-study of the dangers of non-random treatment. One reason to have been skeptical initially is that the magnitude of the measured effect – a doubling of trade – is simply too large to be believed and does not square with related results in the literature. For example, Irwin (1998) finds that the Smoot-Hawley tariff was estimated to have decreased trade by 4-8%. How plausible is it that CUs have an impact 12-20 times larger? Particularly since Klein and Shambaugh (2006) found that indirect currency pegs – more likely to be random – are also not correlated with higher trade flows. Indeed, even GR implicitly assume that hard pegs at par values other than 1:1 have no effect on trade. We believe our results are reasonable since there is no theoretical or intuitive reason to expect that there is anything special about a 1:1 par value vs., for example, a 1.2:1 par. Our findings can also help explain why different CUs appeared to have wildly different impacts on trade – the results are simply spurious, and not robust in any case.

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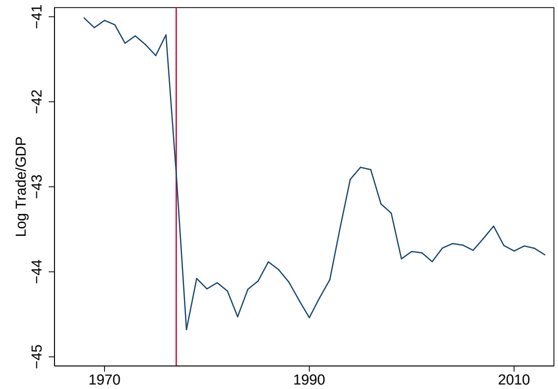
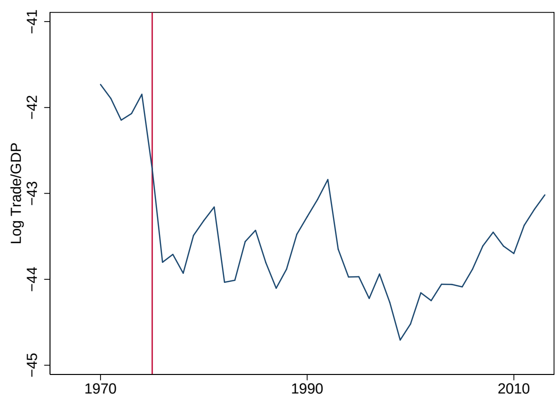
6 Appendix

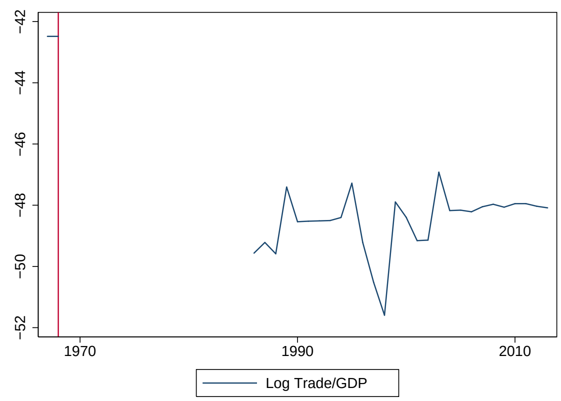
(a) India-Pakistan



(b) Kenya-Tanzania



(c) Portugal-Angola



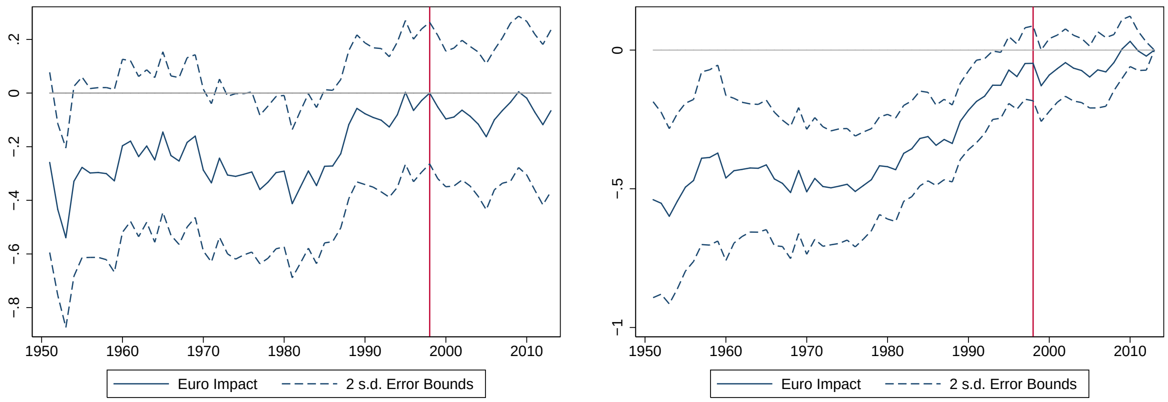
(d) Guinea-Mauritania

Figure 14: Trade Collapses following CU Dissolutions, which follow Wars

Notes: Panel (a) shows the evolution of trade over GDP between India and Pakistan, who dissolved a currency union as the same time as a brutal border war. In panel (b), Kenya and Tanzania ended their currency union amidst the Liberation War and overthrow of the Dictator Idi Amin. In panel (c), Portugal and Angola ended their currency union after a bloody civil war resulted in a communist takeover. In panel (d), it can be seen that after Guinea and Mauritania ended their currency union in 1968, trade was not even recorded again for another two decades.

Appendix Table 1: List of Switches Coterminous with a Major Geopolitical Event (from Campbell, 2013)

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



(a) No Country-Pair FEs

(b) With Country-Pair FEs

Figure 15: The Euro Effect by Year (Europe as Control Group)

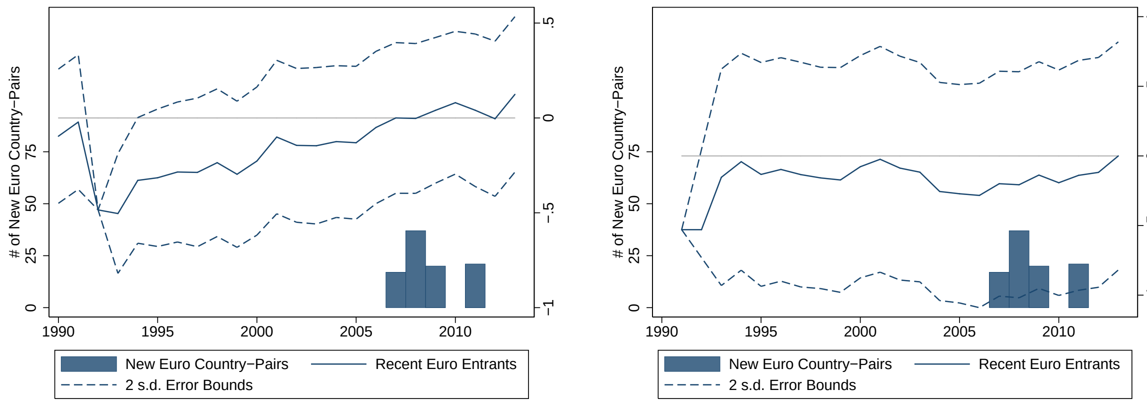
Notes: Panel (a) shows the evolution of the trade intensity of countries which eventually joined the euro vs. the rest of Europe, using equation 2.2. All European countries with at least 40 observations are used as controls. Panel (b) adds in country-pair fixed effects.

7 Not for Publication Appendix

Table 10: Impact of the Euro: Post-1990 Data Only

	(1) GR, CPFE	(2) +Controls	(3) GR, I/M*Year FEs	(4) +Controls
EMU Dummy	0.095**	0.080	0.41***	0.17**
	(0.036)	(0.046)	(0.049)	(0.052)
Observations	252877	223636	489298	489298

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the average of 4-way log bilateral trade flows. Each regression includes country-pair and year fixed effects. Column (1) reproduces the results from GR (2002). Column (2) reproduces the results from Campbell (2014). Column (3) replicates the results from Glick and Rose (2017), Table 2 column (4). Column (4) includes the controls, data adjustments, and multi-way clusteres from the previous table. Other controls, including GDP and GDP per capita, and dummies for regional trade agreement and currently a colony are omitted for space. Column (2) adds in multi-way clusters, and additional control variables, including total exports (ex-bilateral exports) for both countries, for countryseparating GDP by country 1 and country 2, and . Columns (2), (5), and (6) include controls for country-pair trends for countries with time series variation in CU status. EMU = European Monetary Union. "CUs, Ex-War, Missing" means Currency Unions in which the changes are not associated with war or some other major geopolitical event or missing data.

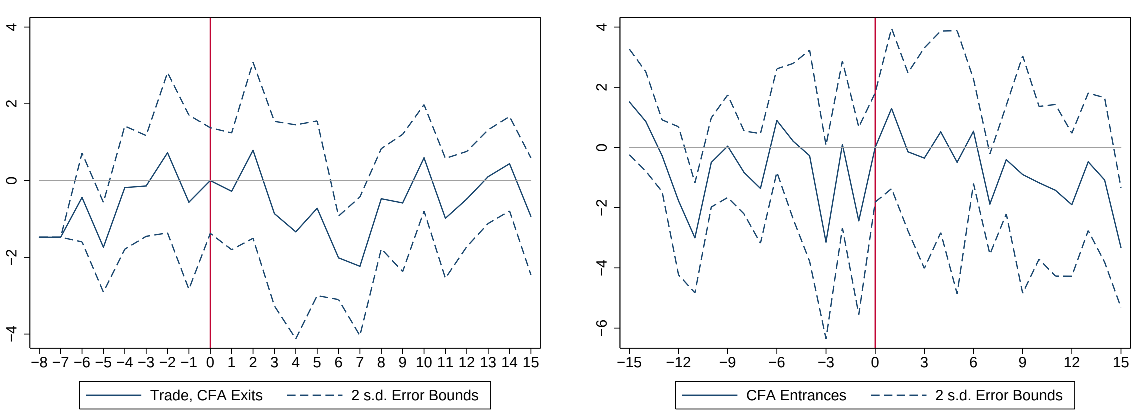


(a) Full Sample as Control

(b) Eastern European Control Group

Figure 16: New Euro Entrants

Notes: Panel (a) shows the evolution of the trade intensity of countries which eventually joined the EMU from 2007 to 2011, using equation 2.2, and using the full sample as controls. Panel (b) looks at just the Eastern European entrants – Slovenia, the Slovak Republic, and Estonia, and uses a control group of Latvia, Lithuania, Hungary, the Czech Republic, and Croatia.

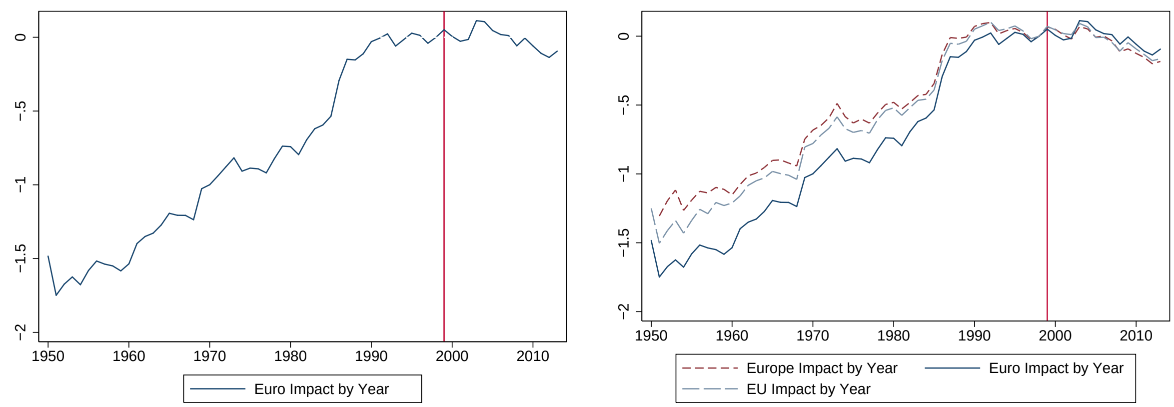


(a) Exits

(b) Entrances

Figure 17: Impact of CFA Exits and Entrances

Notes: Panel (a) shows the evolution of trade before and after exits into the CFA Franc using log directional exports as the dependent variable (equation 2.2. Panel (b) shows the evolution of trade before and after entrances.



(a) Gravity Dummies on Intra-Euro Area Trade

(b) vs. Europe and EU

Figure 18: Assessing the Euro Impact by Year

Notes: Panel (a) shows the evolution of the trade intensity of countries which eventually joined the euro. The red bar denotes the year the Euro was formed, 1999. *I.e.*, it plots annual gravity dummies from equation 3.1. All country-pairs with at least 40 observations are used as controls. Panel (b) compares this measure to gravity dummies for all European countries, and countries which would eventually join the EU.

7.1 Additional Results on Dynamics

On the whole, figure 13 does not necessarily imply a pressing need to take a dynamic approach, as excluding the CUs coternminous with wars and missing data, and adding in other controls mostly eliminated the pre-treatment trends. On the other hand, panel (b) suggests this might be advisable. Thus, next we show our main result – that the impact of CUs on trade is not statistically significant – holds up even when we add in a lagged dependent variable. We do this for both the GR specification in Column (1) in Tables 7 and 8, and to our preferred specification in Column (6) of the same tables, which excludes the CU switches coterminous with wars and missing data, and adds in controls such as the "Ever EU*Year" interactive FE. Thus, in column (1) of Table 12, we add in lagged log bilateral trade as a control variable to the regression in equation 2.1. Of course, since this equation also includes fixed effects, this will induce Nickell Bias (Nickell (1981)). However, Nickell showed that this bias will be small in a long panel. Thus, we limit to panels with T>40, which happens to make no difference to the key coefficients, but gives us an average panel of 50 years, long enough to provide an upper bound on the bias which is relatively small.[19]. In column (1), a coefficient of .21 implies a long-run impact of 56% (=.21/(1-.63)). In column (2), however, when we add in our controls and exclude the War CUs and those with missing data, we get an impact of 8.3% (=.024/(1-.71)), although not statistically significant. Columns (3) and (4), which use the directional exports instead of bilateral trade as the dependent variable, and which also control for Importer*Year and Exporter*Year FEs, points to

19. For reasonably large values of T, the formula for the bias is approximately $\frac{-(1+\rho)}{(T-1)}$. In this case, the bias is approximately -1.63/49=.033.

similar conclusions: the effect of CUs on trade is not robust.

Table 11: Adding a Lagged Dependent Variable

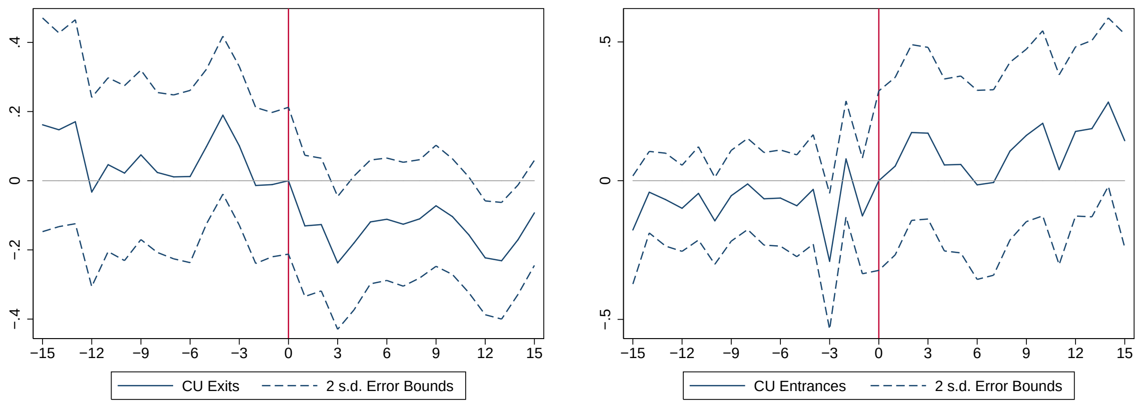
	(1) Model 1	(2) +Controls	(3) Model 2	(4) +Controls
Currency Union	0.21***		0.14***	
	(0.026)		(0.011)	
L.ln(Trade)	0.63***	0.71***		
	(0.0046)	(0.015)		
Currency Union (ex-War, Missing)		0.024		0.021
		(0.024)		(0.024)
L.ln(Exports)			0.68***	0.68***
			(0.0023)	(0.0034)
Observations	246165	208128	456315	456315

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in the first two columns is log bilateral trade and log directional exports in the last two columns. The first two columns include country-pair and year FEs, and the latter two add Importer and Exporter*year FEs. Column (1) adds in a lagged dependent variable to the GR (2016), Table 2 specification. Column (2) adds in a number of controls, and limits the CU observations to those ex-War and missing. Column (3) adds in a LDV to the specification in Table 5 of GR (2016). Column (4) adds in in a number of controls, and limits the CU observations to those ex-War and missing.

Table 12: Dynamic Models

	(1) ln(Trade)	(2) ln(Trade)	(3) ln Δ Trade	(4) ln(Exports)	(5) ln(Exports)	(6) ln Δ Exports
Currency Union	0.25***		-0.0077	0.18***		-0.0032
	(0.029)		(0.018)	(0.013)		(0.0067)
L.ln(Trade)	0.57***	0.56***				
	(0.0037)	(0.016)				
CUs (ex-War, Missing)		0.052			0.031	
		(0.037)			(14.2)	
L.ln(Exports)				0.54***	0.54***	
				(0.0018)	(0.013)	
Observations	392148	351303	351303	783749	783749	716727

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in the first two columns is bilateral trade, the log change in bilateral trade in the third column, log bilateral exports in columns (3) and (4), and the log change in bilateral exports in column (6). Each regression includes country-pair FEs (CPFEs). Column (1) benchmarks the baseline estimate from GR (2002), absent year FEs. Column (2) benchmarks the results (absent trend controls) from Campbell (2013), and includes year FEs. Columns (3) and (5) benchmark the CPFE results from GR (2016). Columns (4) and (6) omit the CUs in which switches were coterminous with war or missing data, and also includes other intuitive controls.



(a) Exits

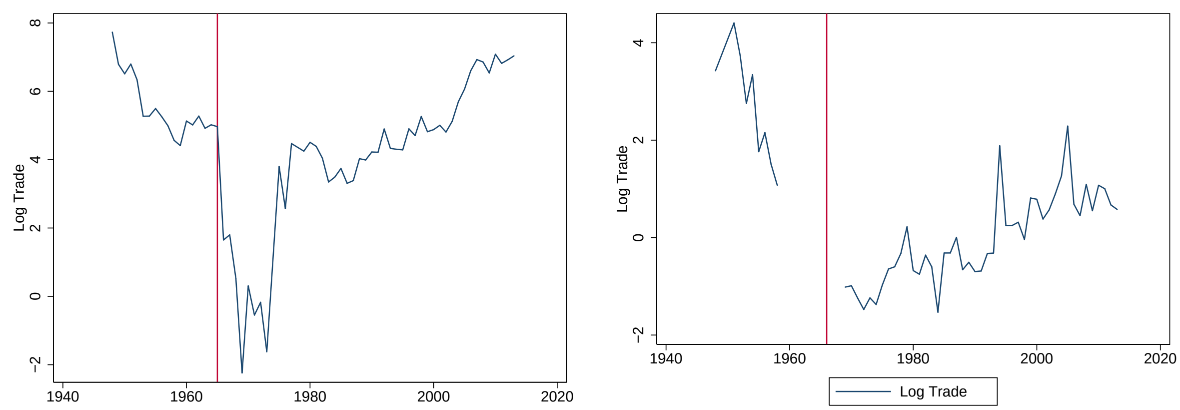
(b) Entrances (ex-EMU)

Figure 19: Impact of CU Exits and Entrances; Over Time

Notes: Panel (a) shows the evolution of trade before and after CU exits using equation 3.1. Panel (b) shows the evolution of trade before and after entrances, ex-EMU.

7.2 Additional Plots of Trade (cut material)

Figure 20(b) shows the evolution of bilateral trade between Sri Lanka and Mauritius. This highlights two related problems: first, while trade was generally lower after the 1966 currency union dissolution, suspiciously there was no trade recorded for the entire 1960s. Secondly, the trade data pre-dissolution which does exist suggests that trade had been plunging for years. Thus, trade *growth* was actually faster in the period without a currency union. Campbell (2013) also found that if one omits CU switches coterminous with missing data, that the estimated results tend to shrink, and, secondly, that CU status does not predict trade growth.



(a) India-Pakistan

(b) Sri Lanka-Mauritius

Figure 20: The Rupee Zone

Notes: Panel (a) shows the evolution of the trade intensity of countries over time which shared the Rupee. The vertical red line indicates the dissolution of four of these unions, with two others dissolving in 1969 and 1973. Panel (b) looks uses these countries' trade with New Zealand as the main control.