BREAKING BADLY: THE CURRENCY UNION EFFECT ON TRADE

Douglas L. Campbell
Aleksandr Chentsov

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Douglas L. Campbell[§]
New Economic School

Aleksandr Chentsov[‡]
New Economic School

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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. Despite the longstanding academic debate on the topic, even recent research has continued to find that CUs exert a large effect on trade. We find, by contrast, that the sizeable recent estimated impact of CUs on trade is driven by other major geopolitical events and is also sensitive to dynamic controls. Overall, we estimate that the impact of CUs on trade is typically indistinct from zero, depending on the specification and controls, but with fairly large standard errors.

JEL Classification: F15, F33, F54
Keywords: Euro, Currency Union Effect, Gravity Estimation

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§. Corresponding author. E-mail: dcampbell@nes.ru, dolcampb@gmail.com.

‡. Also affiliated with the Gaidar Economic Policy Institute.

As several European countries debate entering, or exiting, the euro, a key policy question is how much currency unions (CUs) affect trade. In recent years, for example, the eurozone has continued to grow, with the addition of Estonia (2011), Latvia (2014), and Lithuania (2015), despite a mixed eurozone economic record. While questions of politics and identity drive the enlargement of the eurozone, another factor is that these countries want to foster closer trade ties with Western Europe.

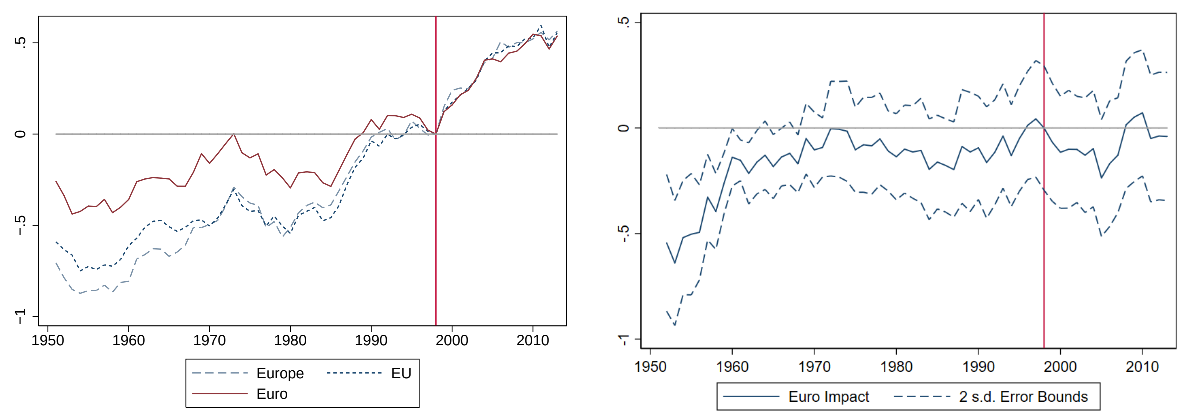
According to recent research, CUs have led to a large increase in trade. Glick and Rose (2016) (hereafter GR), find a 40%+ impact using using a traditional OLS approach with trade in logs. Larch et al. (2019), introducing a Poisson pseudo-maximum-likelihood (PPML) estimator with high-dimensional FEs, place the overall impact at a still sizeable 13%. Saia (2017) finds a medium-run impact of 16% using a synthetic control approach for the counterfactual world in which the U.K. had adopted the Euro; he concludes that the Euro may have increased intra-European trade by an astounding 55%.[1] In this paper, we revisit the important policy question of whether CUs increase trade using the full panoply of modern applied micro tools. These include plotting pre- and posttreatment trends, adopting more suitable control groups (e.g., the EU as a control group for Euro countries), controlling for other major geopolitical factors likely to be correlated with changes in CU status, controlling for dynamics, estimation using PPML, and adopting a synthetic control approach. We find estimates of CUs on trade that are usually, but not always, positive, and noisy, as they are often not statistically significant and sensitive to the specification. Overall, when using OLS with controls, a synthetic control approach, or a high-dimensional PPML estimator, we do not find robust evidence that CUs have a large positive impact on trade, as has been measured repeatedly in the literature.

To fix ideas about our basic methodology and results, in Figure 1 we plot the residualized evolution of trade in Western European EMU country-pairs and compare it to the evolution of trade in EU countries (adding in all country-pairs involving the U.K., Denmark, and Sweden), and to the evolution of all Western European country-pairs (adding all country-pairs that involve Norway, Switzerland, and Iceland).[2] We find that trade

1. Glick (2017) argues for a Euro effect 25% smaller than GR (2016), while Glick and Rose (2015), which predated GR (2016), find that the impact of the Euro on trade is sensitive to using a PPML specification. Other recent papers that find a positive impact include Camarero et al. (2014), Chen and Novy (2018), Esteve-Pérez et al. (2020), Felbermayr et al. (2017), Gil-Pareja et al. (2008), Gunnella et al. (2015), Kopecky (2019), Kunroo et al. (2016), Martínez-Zarzoso and Johannsen (2017), and Rotili et al. (2014), while Macedoni (2017) finds evidence consistent with the Euro reducing trade costs.

2. For example, we run the following regression: $ln(X_{ijt}) = \alpha_t * I_{ij}^{EU} + \beta Z_{ijt} + \lambda_{it} + \psi_{jt} + \gamma_{ij} + \epsilon_{ijt}$. where $\alpha_t * I_{ij}^{EU}$ is a year*EverEU interactive FE (separate FE for each year for two Western European countries that joined the EU), X_{ijt} are exports from country i to country j at time t, and Z are other controls, and this regression includes a full suite of importer*year, exporter*year, and country-pair FEs. Then we run additional regressions replacing EU countries with all Euro countries, and with all

was higher, on average, after the Euro than before (this is what others have measured). However, one can also see that trade was already trending up before the advent of the Euro, and that, after its formation, trade among all EU and all Western European countries increased by almost exactly the same amount relative to 1998, the last year prior to the Euro. On the other hand, in this specification, the Euro countries experienced less of a positive pretrend than EU countries, or all of Western Europe. This suggests that, relative to trend, the Euro might have actually had a positive impact on trade. However, one can arrive at this conclusion only by including pretrends as controls—highlighting the importance of controlling for dynamics, as has been missing from the recent literature. This difference turns out not to be statistically significant, as can be seen by the slightly different specification in Figure 1 Panel (b), which plots the residualized Euro impact using only Western European countries as the control group.



(a) Impact of Euro (All Data as Control)

(b) Impact of Euro (W.Europe as Control)

Figure 1: The Euro Effect by Year, vs. the EU and All of Western Europe

Notes: Panel (a) shows the evolution of export intensity of countries which eventually joined the Euro vs. the rest of Europe, using equation 2.1 and all available data. Panel (b) plots the impact of the Euro over time while limiting the control group to Western Europe.

For Eastern Europe, we find that residualized pre- and posttreatment trends imply that the evolution of trade with EMU countries is similar for those countries that eventually joined the EMU and those that did not. Next, we show that the pre- and posttreatment trends for non-EMU countries with at least 10 years of consecutive data before and after a switch also show no indication that CUs increase trade. Looking at other individual CUs, we find that trade fell by 86% prior to the breakup of CFA Franc countries, but that trade actually *increased* after dissolution. Thus, if we take the CFA

Western European bilateral pairs. We then plot these annual dummies for the EMU, the EU, and Western Europe over time.

Franc countries' time-series evidence at face value, we should conclude that CUs reduce trade. Yet, if we ignore the dynamics and take a static approach, as is standard in this literature, one would wind up with the opposite, and incorrect, conclusion, since trade was on average higher prior to breakup.

Next, we control for a small set of relevant omitted variables in a Panel regression setting with directional exports as the dependent variable, and importer*year, exporter*year, and country-pair FEs (i.e., with the same data and FEs as GR). For example, in the case of Western European Euro countries, we include a dynamic EU control, creating a set of annual dummies for trade between all Western European EU countries (or, alternatively, all Western European countries, including those not in the EU). For Eastern European countries, we use a number of other Eastern Bloc countries that did not join the Euro during our sample period as a control group, and create an Eastern Europe*Euro*year interactive dummy. This is a way to control for the end of the Cold War on trade between Eastern and Western Europe. For the U.K.'s trade with countries that used the Pound, we select other prior British colonial possessions as the control group. In addition, we include a simple dummy variable for hostile colonial breakups. This is a small fraction of the CU switches in the sample, or of colonial breakups, but it also proves influential as a control. For the CFA Franc, we include a simple time trend for country-pairs that eventually experienced dissolutions. Even with these controls, we find a borderline significant CU effect of about 11%. However, the statistical significance of this estimate disappears when we multiway cluster, and it is likely sensitive to further controls for omitted variables, as we also show it is driven in part by the sample with missing data coterminous with a CU switch.

Third, realizing that both trade and CU status are highly persistent, and given the evidence that trade shocks leave persistent changes in trade patterns, we explore several simple dynamic models. Using log changes in trade as the dependent variable, we find that trade actually grows more slowly under CUs, though the effect is insignificant. A lagged dependent variable (LDV) model implies a large and significant impact of CUs on trade, but the controls discussed above kill this result. A 65-year panel in this case helps to reduce Nickell-type biases.

Fourth, we show that the CU effect on trade is also not significant when we use a PPML specification instead of a log-linear model. Once again, the controls we introduce are decisive.

Lastly, we replicate the synthetic control approach used by Saia (2017), and we propose a few improvements. Namely, we set our synthetic controls to target the log of trade as a share of bilateral GDP rather than just nominal trade (no log), and we also match on the growth in trade rather than just the level. We find that British trade relative to GDP would not have been dramatically higher had Britain adopted the Euro, and would even have been less as many as seven years after adoption. This matches our findings using other methodologies.

This literature began with Rose (2000), who find that CUs triple trade. This sounded suspiciously large to some, and so subsequent research set out to dampen the effect (e.g., Nitsch (2002)).[3] However, Glick and Rose (2002), using a larger data set, find that CUs double trade in a time-series setting, even when including country-pair fixed effects.[4]

Nevertheless, doubts remained.[5] Nitsch (2005) finds no impact for CU entries; Klein (2005) finds no trade effect of dollarization episodes; and Bomberger (2003) finds that a simple time trend eliminated the effect for the U.K. colonial sample (one-fifth of the total switches in GR 2002). Thom and Walsh (2002) notes that many CU exits coincided with obvious omitted variables such as wars of independence and communist takeovers. Berger and Nitsch (2008) consider 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. They find that it did not.[6] Klein and Shambaugh (2006) found that hard currency pegs have a much smaller impact on trade than CUs, and that indirect pegs—which are much more likely to be random—have no effect on trade at all.

Campbell (2013) draws on these insights, showing 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 U.K.-colony time trend, and (c) clustering the standard errors. He also finds it possible to arrive at point estimates of CUs on trade that are *negative* and insignificant by controlling for country-pair trends.

3. In addition, Persson (2001) and Pakko and Wall (2001) followed Rose's (2000) original paper but predated GR (2002), and they greatly reduce or eliminate the estimated impact on smaller data sets.

4. 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 IV approaches, finding that CUs actually increase trade on a 14-fold and a 7-fold basis, respectively.

5. For example, in an influential overview of the literature Baldwin (2006) provides several reasons why the larger estimates of the impact of CUs on trade were unreliable, concluding that the Euro might have increased trade by (a still-sizeable) 5% to 10%. Bun and Klaassen (2007) include dynamic controls and shrink the CU impact to a precisely estimated 25%.

6. Santos Silva and Tenreyro (2009) also find no effect of the Euro on trade. In a meta-analysis, Havránek (2010) find systematic evidence of publication bias for the Euro studies, as well as 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, according to Wolf and Ritschl (2011). López-Córdova and Meissner (2003) find mixed support.

However, GR (2016) responded with a data set extended to 2013 (an additional 16 years of data) that included 423 bilateral CU switches compared to 136 in GR (2002).[7]

This paper is the first to test whether omitted variables and dynamic specifications discussed in Campbell (2013) drive the apparent large impact of CUs on trade using GR's (2016) much larger data set (we study four times as many CUs as in Campbell (2013)). Compared with Campbell (2013), we also include the Euro period, implement superior importer*year and exporter*year FEs in a specification using directional exports, do not drop the observations coterminous with war or missing data, and use PPML and synthetic controls as supplemental approaches. While some other studies also find no effect of the Euro (Figueiredo et al. (2016), Nähle (2015), and Tykkyläinen (2012))[8], 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 aspects of European integration, since we use the same data and estimation strategy as GR (2016).

In the rest of the paper, we first describe the data and methodology, then we implement our empirical approaches: (1) plotting pre- and posttreatment trends, and then running (2) static Panel regressions, (3) dynamic panel regressions, and (4) PPML regressions, and finally (5) implementing a synthetic control approach.

1 Data

We use the same data set provided by GR (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*. Data on regional trade agreements come from the World Trade Organization. CU classifications were taken by GR from the IMF—see GR (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

7. The authors should further be commended for plotting pre- and posttreatment 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. They should also be credited for creating a much larger data set of CU observations, and for sharing this data set publicly, to our advantage.

8. In a useful test, Martínez-Zarzoso (2019) finds no evidence in the aggregate of increased trade between African countries pegged to the Franc and EMU countries after the formation of the Euro. Mika and Zymek (2017) focus on late entrants with data from 1992 to 2013 using a PPML estimator (used also by GR (2016b)); they argue that the problem is log-linear OLS. We show that even using log-linear OLS, the Euro effect is not robust.

of which came from exits. Only 66 of these switches remained, however, after excluding switches with (1) obvious major geopolitical events which overshadow changes in CU status, (2) missing trade data before or after a CU switch, or (3) colonial histories. In GR (2016), this sample increases to 647 in terms of one-way trade flows (each country pair is in the data twice). Table 2 sums up switches and observations by disaggregated CU. The largest CU in terms of separate country-pairs was actually the British Pound, followed by the EMU.

Table 1: Number of Changes in Currency Union Status

	GR 2002	GR 2016 (One-Directional)
Entrances with Time Series Variation in Data	28	372
Exits with Time Series Variation in Data	108	556
Total Pairs with Time Series Variation in Data	136	901
Missing Data Immediately Before or After Switch	26	209
War or Other Major Geopolitical Event (Using Campbell 2013 definition)	26	49
Switches ex Missing Data or War	88	647
Switches ex Missing Data, War, or Former Colonial Relationship:	66	447
Total Country Pairs	11077	34104
% of Country-Pairs with CUs	1.86	3.46
Total Observations	218,087	879,794
% of Observations with CUs	1.45	1.95
Time Period	1948–1997	1948–2013

Notes: In the first column, the numbers of switches with time series variation represent the number of switches for country-pairs with nonmissing GDP product and bilateral trade for at least one observation both in and out of a CU. For the second column, the only required nonmissing variable is the (log) export value.

2 Methodology

2.1 Plotting Pre- and Posttreatment Trends

First, to understand how changes in CU status affect trade dynamics, we plot the pre- and posttreatment trends. To do so, we run the following panel regression:

$$ln(X_{ijt}) = \alpha_{ijt}^{EnterCU_k} + \eta_{ijt}^{ExitCU_k} + \beta Z_{ijt} + \lambda_{it} + \psi_{jt} + \gamma_{ij} + \epsilon_{ijt}, \tag{2.1}$$

where $ln(X_{ijt})$ is log directional exports from country i to country j at time t, $\alpha_{ijt}^{EnterCU_k}$ is a set of dummies for an exporter-importer country pair for specific years

Table 2: Changes in CU Status by Currency Union

Currency Union	GR 2002	GR 2016 (One-Directional)	Observations (2016)
EMU	0	270	5024
CFA Franc	53	99	15062
ECCA	5	11	3062
Australian Dollar	2	6	1446
British Pound	25	308	14672
French Franc (pre-Euro)	3	26	1448
Indian Rupee	6	28	2280
U.S. Dollar	4	77	5236
Portuguese Escudo	4	22	860
Other CUs (ex-Portugal)	21	68	3744
Total	123	915	52834

Notes: This table plots the number of country-pairs with at least one CU status switch with time series variation in data for disaggregated CUs, requiring the same nonmissing variables as for Table 1.

before and after a specific CU entrance, and $\eta_{ijt}^{ExitCU_k}$ is another set of dummies for years before and after a CU exit. We also include (1) λ_{it}, exporter*year interactive FEs, (2) ψ_{jt}, importer*year interactive FEs, and (3) γ_{ij}, country-pair FEs. To ensure we have a consistent sample, we use only CUs with 10 years of data before and after a switch.

This allows us to visualize the dynamic effects of joining or leaving a CU. If it takes several years for the maximum benefit of CU on trade to emerge, then a static dummy-variable approach as in GR (2016) would actually be downward-biased. We also plot pre- and posttreatment trends separately for the Euro and for the CFA Franc, two of the largest CUs in our sample, in part to help motivate controls in the panel regressions that follow.

2.2 Panel Regression Methodology

2.2.1 Benchmark Methodology

Following GR 2016, we estimate a gravity equation with rich importer-year, exporter-year, and bilateral country-pair 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 CU status in a given year, λ_{it} are exporter-year interactive FEs, ψ_{jt} are importer-year interactive FEs, δ_{ij} are country-pair FEs, and Z_{ijt} include other controls. We cluster at the country-pair level (each country-pair appears twice for a given year), in the interests of conservatism. (In the Online Appendix, we show that the standard errors increase when we cluster in multiple dimensions.)

Equation 2.2 identifies the impact of a CU from the time series variation. It asks how much one country will export to another on average after they join, or before they leave, a CU, relative to all exports for the exporting country and relative to all imports for the importing country. If two countries enter into a CU precisely because they trade more, the country-pair fixed effect will control for this. The two main problems with this methodology are that CU switches are potentially endogenous in a way in which a time-invariant FE is not a sufficient control, and secondly that it ignores trade dynamics. If two countries form a CU, not just because they trade more but because their trade intensity is increasing over time, then this static FE dummy-variable method will bias up the results. We might instead want to ask if they are trading more relative to their prior trend. However, even if they were trading more relative to the trend (we will see that they do not), sharing a CU is likely to be a proxy for good, or at least stable, political relations. CU dissolutions are likely to be caused by other major geopolitical events that dwarf the impact of a change in CU status for their implications on trade volumes, such as warfare or the ethnic cleansing of one's neighbors. Lastly, when two countries form a CU, trade might actually increase for a few years before arriving at a new steady state. In that case, a static dummy-variable approach might bias the true effect downward.

2.2.2 Additional Controls

To mitigate endogeneity and omitted-variable bias, we propose a series of controls that are essentially designed to provide more specific, and intuitive control groups for each given CU. These include the following:

(1) For Western Europe, the control group includes annual dummies for all exports within country-pairs that are both members of the EU. This includes three nations in the EU but not in the eurozone. Thus, we ask, how much did trade within Euro members increase relative to their trade with other EU members that did not join the eurozone? We implement the same exercise for all countries in Western Europe, including the three nations we have data for that are not in the EU, but in practice the results are not statistically distinct from what we report.

(2) For Eastern European (EE) Euro entrants, we include other nearby countries that did not join the eurozone in the control group. Thus, we ask, what is the impact of

joining the eurozone for EE countries relative to other EE countries that did not adopt the Euro? (The non-Euro EE sample includes Latvia, Lithuania, Hungary, the Czech Republic, and Croatia, countries that either adopted the Euro after the sample ends or did not adopt the Euro at all.)
(3) We include U.K.-colony*year FEs. Every country that shared a CU with Britain was a former British colony except for one. Thus, we ask, what was the level of trade for those countries who left a British Pound CU relative to the U.K.'s trade with former colonies that never had a CU? This control is motivated by the observed decaying trend in trade between the U.K. and its former colonies over time, and the decline in trade between those colonies themselves.
(4) We also include a common-U.K.-colonizer*year interactive trend. For each former UK colony, we control for the trend in its trade with other former U.K. colonies. Trade intensity among this sample also decays over time, although less strongly than trade with the U.K. itself.
(5) For the CFA countries, we include a simple time trend for country pairs that had CFA Franc exits. Thus, the variable takes the value zero for country pairs that were never part of the CFA Franc, and equals the "year" variable for country pairs with exits from CFA Franc unions.
(6) We include a separate colony dummy for those countries that had some form of colonial relationship but then experienced hostilities. We include in this dummy pairs of countries, such as India and Pakistan, that had a common colonizer, but then had hostilities at the end of the colonization period, using the onset of war as the event date. Figure 2 shows how misleading the standard approach can be, as trade dropped by 99.8% following Operation Gibraltar and the resulting war between India and Pakistan, which was unlikely to have had much to do with their CU dissolution. It has been observed (e.g., Head et al. (2010)) that the evolution of trade for countries with hostile versus amicable breakups varies greatly. Note that hostile colonial breakups constitute a small fraction of the total CU dissolutions, and the timing of the colonial breakup does not always coincide exactly with the recorded CU dissolution.

In addition, we already know from Campbell (2013) that the GR (2002) estimates of CUs were driven in part by CU switches coterminous with missing data. Thus, we also do an exercise where we separate the CU switches that had continuous data before and after a CU switch from CUs that had missing data, and we compare the results.

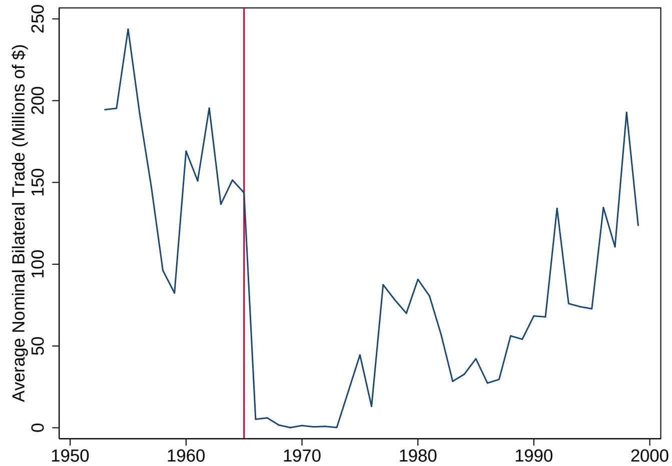


Figure 2: Indo-Pakistani Trade: A Textbook Case of the CU Effect or Endogeneity?

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 CU in 1965, after which trade plummeted. However, this was the same year as the Indo-Pakistani War of 1965, following Operation Gibraltar, a secret Pakistani operation to infiltrate Jammu and Kashmir to foment rebellion against Indian rule (Prasad, 2011). Relations between these countries remain tense.

2.3 A Dynamic Gravity Regression

Theoretically, the existence of large sunk costs, learning-by-doing, or consumer habits would imply that "the gravity equation" should be a dynamic, where trade is a function of current trade costs, and also the past history of trade costs (see Campbell (2020) and Campbell (2010) for proofs). A "dynamic" form of a gravity model, an LDV model, is:

$$ln(X_{ijt}) = \sum_{k=1}^{K} \rho_k ln(X_{ij,t-k}) + \alpha CU_{ijt} + \beta Z_{ijt} + \lambda_{it} + \psi_{jt} + \gamma_{ij} + \epsilon_{ijt}, \qquad (2.3)$$

where the Zs are other controls, and this time we have included LDVs. In this specification, we allow the short-run and long-run impacts of a CU to be different. Note that, typically, the main problem this specification faces is a type of Nickell (1981) bias, as an LDV model with panel FEs induces a well-known bias. However, this bias disappears in long panels. In this case, we have 65 years of data, though admittedly with gaps. Nickell found that, for reasonably large values of T, the limit of $(\hat{p} - p)$ as $N \to \infty$ will approximate to $-(1+\rho)/(T-1)$. This implies the standard Nickell bias on the order of .03—relatively modest in the context of the CUs and trade literature, where 100% effect sizes are not uncommon. One alternative to this would be to simply run with the dependent variable in first differences. In this version, we would be testing whether trade grows faster for countries when they are in a CU vs. when they are not.

2.4 Poisson Pseudo-Maximum Likelihood (PPML)

An alternative to gravity in logs is to estimate a PPML regression. The main benefit of this method is that we no longer need to drop zero observations for exports, as we do when we take logs. Thus, we run the following PPML regression:

$$X_{ijt} = exp(\alpha CU_{ijt} + \beta Z_{ijt} + \lambda_{it} + \psi_{jt} + \gamma_{ij}) + \nu_{ijt}, \tag{2.4}$$

where now X_{ijt} is the level of exports from country i to j at time t (no log), and we include the same set of importer*year, exporter*year, and country-pair FEs as before. We then also add in our controls from 2.2.2.

2.5 Synthetic Control Method

Another option would be to implement a synthetic control method. Saia (2017) implements such an approach, conducting a counterfactual analysis for the U.K., implying that trade would have been much higher had Britain joined the eurozone. We might like to use a synthetic control method on the entire sample; however, the Stata package Saia used, the "synth" package, requires a balanced panel and is computationally intensive. This may be why Saia uses a sample of just nine countries (our main panel regressions contain 213 countries). Saia formed synthetic control groups based on absolute nominal trade levels (not logs) pre-Euro, and thus effectively does not control for GDP or trends in total trade. He also uses bilateral distance, an adjacency dummy, and a language dummy to match. We first replicate Saia, then we show what happens when we use a slightly different, theoretically motivated choice of weights and when we design our synthetic control to predict the trade-to-GDP ratio rather than just trade levels.

In particular, when we create our counterfactual, we

(1) use $ln(trade_{12}/(gdp_1 + gdp_2))$—log directional exports scaled by bilateral GDP—as the dependent variable. This is the analog of controlling for GDP in a panel gravity setting.

(2) include in the weights a "same country" dummy. Thus, for the synethic control for British trade with Portugal, we want Portuguese trade with other countries to receive a greater weight. This is because there might be Portugal-specific shocks to hit in a given year.

(3) match on both the first and last two years of trade in the pretreatment period in

order to effectively match on the growth rate in trade.
(4) we run a regression of the log trade share of GDP on total bilateral exports and imports outside of the countries in our sample, and also include year and country-pair FEs. We then use the residuals from this regression, which essentially control for trends in total trade ex-Europe, as the variable we create our synthetic controls to match.[9] We do this because we also want to control for trends in trade with countries outside of Europe. It might just be that all British trade increased relatively slowly after 1999 for reasons unrelated to its decision not to join the Euro.
(5) use bilateral distance, an adjacency dummy, and a language dummy in our matching algorithm, as well as use the synth package in Stata, all following Saia.

3 Results

3.1 Results: Plotting Pre- and Posttreatment Trends

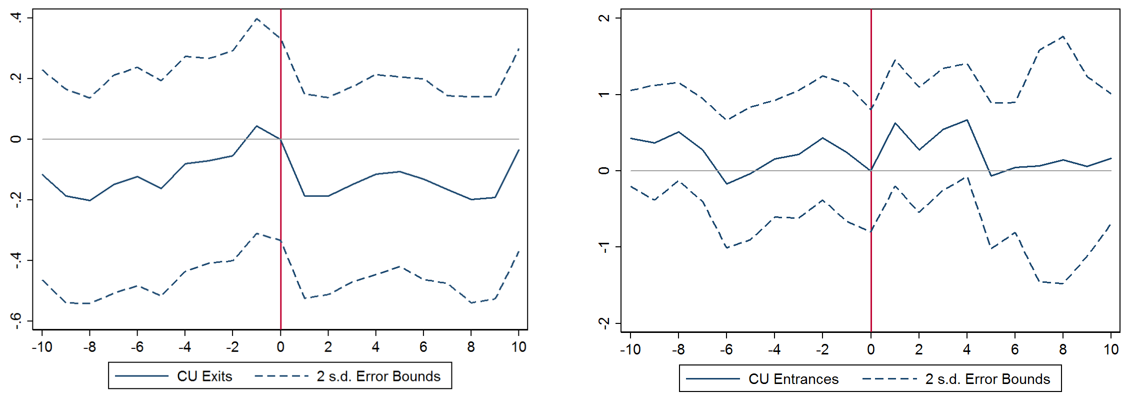
3.1.1 Pre- and Posttreatment Trends for the Entire Sample

We implement regression (2.1) and plot the pre- and posttreatment trends for the full sample of CUs (before and after a CU exit) in Figure 3, and also before and after a CU entrance for the non-EMU group. We find that trade did appear to fall about 20% after dissolution, but we also find that this amount was not statistically significant. Ten years after dissolution, the effect was closer to –2%, although insignificant. For the entrants, trade also increases after the formation of the CU, but the effect is small relative to estimated clustered standard errors. In both cases, it appears there is too much noise to make any hard conclusions. Note that, in this case, our model with importer*year and exporter*year FEs removes much of the troubling pre-trend GR (2016) find when they do a similar exercise for a model that only includes country-pair and year FEs, but not the importer*year and exporter*year FEs.

3.1.2 Pre-and Posttreatment Trends for the Euro

We next focus on the Euro, as this union represents 29% of the bilateral switches in CU status in our data. Given the different histories of Western Europe and the former

9. Thus, we run the following regression: $ln(trade_{ij}/(gdp_i + gdp_j)) = \beta_1 Tot.Exports.ExEurope_{ij} + \beta_2 Tot.Imports.ExEurope_{ij} + \gamma_{ij} + \theta_t + \epsilon_{ijt}$, where $Tot.Exports.ExEurope_{ij}$ are total exports for country i and j to countries outside of Europe, and $Tot.Imports.ExEurope_{ij}$ are total bilateral imports of country i and j from outside of Europe.



(a) Exits

(b) Entrances

Figure 3: Impact of CU Exits and Entrances

Notes: Panel (a) shows the evolution of trade before and after CU exits using equation 2.1. Panel (b) shows the evolution of trade before and after entrances, ex-EMU. Both use only CUs with full data at least 10 years before and after changes in CU status.

Warsaw Pact entrants to the eurozone, we choose separate control groups for Eastern and Western Europe.

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, which itself updates and adjusts its rules frequently. Thus, the formation of the Euro can be viewed as merely one step of a decades-long economic integration within Europe. In addition, trade was disrupted during WWII, and thus, as Glick and Taylor (2010) and Campbell (2010) argue, it naturally took decades for historical trade patterns to be reestablished, while lingering wartime animosities might also have depressed trade between, for example, France and Germany for decades.

In Eastern Europe, the Russian Revolution (1917), the end of WWII and the formation of the Warsaw Pact effectively cut off most trade with Western Europe. The collapse of the Soviet Union and the opening of former Warsaw Pact countries to trade with the West was another seminal event that would naturally lead to increased trade integration between Eastern and Western Europe. Intuitively, the transition to a new, higher steady-state level of trade might take time, and would not be realized overnight.

In Figure 4(a), we plot the trade trajectory for the EE Euro entrants—Slovenia, the Slovak Republic, and Estonia—which adopted the Euro late (2007, 2009, and 2011). Note that the upward trend in trade long predates the period when these countries joined the eurozone.

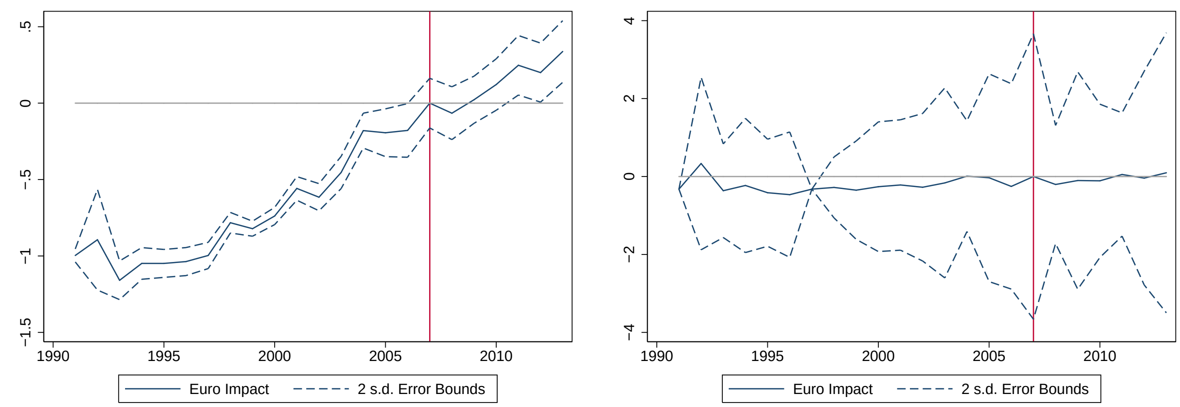
Next, in Figure 4(b), we use as our control group other EE countries that did not join the eurozone, or that joined much later. Thus, we choose Latvia, Lithuania, Hungary, the Czech Republic, and Croatia as our control countries. What we find is that much of the trend is gone and that, compared with 2007, trade actually declined in most years thereafter. However, we also find very large standard errors—in the end, we conclude that we cannot say much other than that the significant impact we get in Panel (a) is gone. Thus we conclude that the case for a large Euro effect on trade for the Eastern European countries is sensitive to the control group, and Figure 4(a) suggests it would be sensitive to a control for trends in the post-Soviet period.

Previously, in Figure 1, we plotted the dummies from equation 2.1 for before and after the formation of the eurozone for just Western European eurozone countries, and we compared them to a slight modification with annual dummies for EU members. Bergin and Lin (2012) point out that there could be anticipation effects with the Euro, but in this particular specification, trade intensity increases from 1950 to 1970, likely too early to have been related with the Euro. If one were to run the Glick-Rose specification with data from 1965 instead, the estimated Euro effect would also shrink (see the Additional Appendix).

Another way to control for the EU would be to include a set of EU dummies for each separate year before and after entry, as countries joined the EU at different times. Comparing the results in Figure 8, we find that they are not materially different (the upward pre-trend in trade among eurozone countries before 1970 is somewhat mitigated). The Euro does not appear to have significantly boosted trade. A referee also suggested looking at the effect on trade between non-Euro and Euro country trade, and there, too, taking a similar approach, we do not see a strong case that the Euro increased trade with the outside world. There is even a hint of a negative impact relative to trend (see Figure 10).

3.1.3 The CFA Franc

We also plot pre- and posttreatment trends for the CFA franc ("*Colonies Françaises d'Afrique*") in Figure 5, as this is the third largest CU in our sample, and to help motivate a mild control. What we see is that in the last 15 years before exit, trade declined by two logs, or 86% (=exp(–2)–1). After the dissolution, if anything, trade appears to have increased. This is a key example of how a simple dummy-variable approach can be misleading, and also why one might want to control for dynamics.



(a) Full Sample as Control

(b) Compared with Eastern Europe

Figure 4: Eastern European Euro Entrants

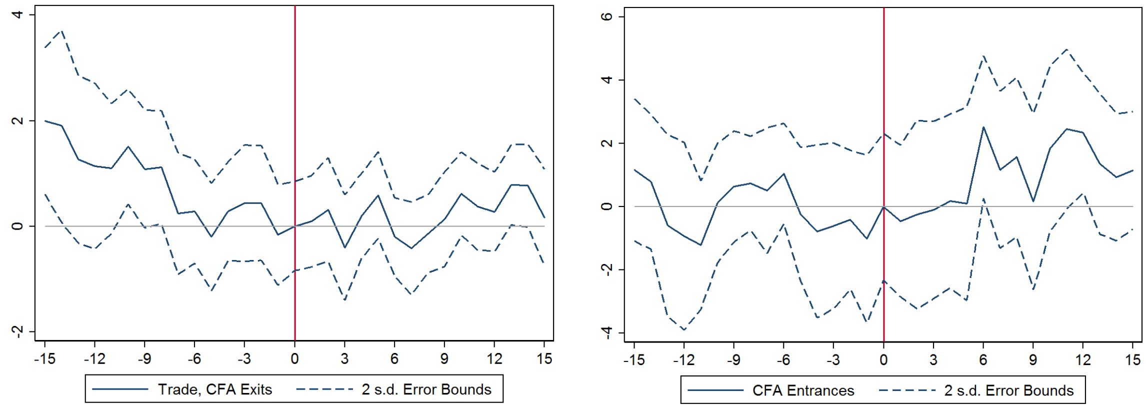
Notes: Panel (a) shows the evolution of the trade intensity of countries which eventually joined the EMU from 2007 to 2011—Slovenia, the Slovak Republic, and Estonia—and prior EU entrants using equation 2.1, and using the full sample as controls. Panel (b) adds in a control group using annual dummies for trade between a larger number of EE countries, some of whom joined the Euro later and others not at all—Latvia, Lithuania, Hungary, the Czech Republic, and Croatia.

What we propose is a simple time trend for this sample of countries.

3.2 Main Regression Results

Next we turn to panel regressions in Table 3, where we estimate equation 2.2. In column (1), we benchmark the results from Glick and Rose's (2016) Table 5 (right panel). We confirm their estimated coefficient of .34 (implied trade impact of over 40%) and a t-score of nearly 19, seemingly leaving little doubt as to the trade-creating powers of CUs. Then, we benchmark the GR results for more disaggregated CUs in column (2). Note that each disaggregated CU ostensibly has a widely varying impact on trade. If we interpret this as a causal relationship, then it would be a major puzzle: 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, endogeneity, omitted variables, and the nonrandom nature of CU formation and dissolution drive these effects, then the findings are simply noise rather than a puzzle. Put simply, the wide variation in the apparent impact of individual CUs is the result of the different historical factors that drove the formation/dissolution of each individual CU. In addition, the highly significant t-scores may suggest the presence of spatial and autocorrelation in the errors.

In column (3), we add in clustered standard errors at the country-pair level to address serially correlated errors. This addition causes the errors to increase substantially. For



(a) Exits

(b) Entrances

Figure 5: Impact of CFA Exits and Entrances

Notes: Panel (a) shows the evolution of trade before and after CFA Franc exits using equation 2.1. Panel (b) shows the evolution of trade before and after entrances.

example, the error on the EMU increases from .021 to .061. The Australian Dollar and the Indian rupee unions are now no longer statistically significant. Note that if we were to three-way cluster, by the importer country, exporter country, and year, as Cameron et al. (2011) and Egger and Tarlea (2015) suggest, the standard errors would rise further for some CUs. For example, the error for the EMU would rise to .14. Clustering by country-pair should address autocorrelation but would leave our errors biased downward in the presence of spatial correlation, which some recent studies have found is nearly ubiquitous in econometric work using spatial data (e.g., Kelly (2019)).

In column (4), we add in our controls discussed in Section 2.3. Now we find that the French franc, the CFA franc, and the Euro no longer have a significant effect, while the estimated coefficient for the British Pound has shrunk. How do each of the controls separately impact the coefficients? In general, the coefficients for an individual CU are only impacted by controls for that specific CU—*e.g.*, the Euro controls do not, in practice, affect the coefficients or standard errors for the other CUs. The only exception is the hostile colonial breakup dummy, which lowers the estimated impact for the French franc, the Indian Rupee, and for the "Other CUs," but which has scant effect on the other coefficients. (In the Online Appendix, Table 22, we add in the controls one by one.) In column (5), we again use the controls as in column (4) and aggregate all the non-Euro CUs into one variable. In column (6), we report the aggregate results, which we find are borderline significant at 90%. Lastly, in column (7), we separate the CU switches coterminous with missing data from the others. In doing so, we find a point estimate of .087 for those switches without missing data, versus a much larger impact for those CUs with missing data coinciding with a switch. One could interpret this in a number of ways: either that CUs have a larger impact on trading pairs for which trade (and trade-reporting agencies) is fragile, or that perhaps the data is missing for these country pairs for some other reason that might be correlated with why the CU collapsed (for example, a war or other conflict).

Note that we have not even controlled for all known sources of endogeneity. For example, we have done nothing to control for war generally or for ethnic-cleansing episodes. Bangladesh and India shared a CU for two years in the wake of Bangladesh's liberation war with Pakistan (which India joined), but much of the high trade volume recorded during those two years was more likely due to military and economic assistance than to having a common fix to the British Pound. Such examples are difficult to control for systematically. Researchers working in this area may consider dropping the small number of influential CU switches that coincide with obvious major geopolitical events or missing data, although we have not done so here in the interests of conservatism.

3.3 Dynamic Gravity Regressions

Next, we move to our dynamic regressions in Table 4, using the LDV model presented in equation 2.3. First, for easy comparison, we repeat the GR benchmark estimate of the CU effect in column (1). In column (2), we run the same regression in log changes. This time, we find that exports actually grow more slowly when two countries share a CU—a rather stark difference. This conclusion holds up when we add in our controls from Section 2.3.

Next, in column (4), we run the version with LDVs. We go out to the third lag; we find that the fourth lag is not significant. This time, the impact of CUs is still highly significant, with a coefficient of .14, implying a short-run impact of roughly 15% and a long-run impact of 43.5%, the latter of which is a bit higher than what the static regression framework suggests. However, when we include our controls from Section 2.3 in column (5), the coefficient on CUs shrinks to .025 and loses significance. One concern is that the combination of an LDV and FEs biases our coefficient. One way to mitigate this is with a longer (average) panel. Thus, in column (6), we limit our sample to country-pair combinations with at least 40 years of data and an average panel length of 56 years—the coefficient on CUs shrinks even further. Still, there are two caveats: the FEs in this regression will induce a (relatively small) bias, and the first-differenced

Table 3: How Robust Is the CU Impact on Trade?

	(1) GR Benchmark	(2) GR Benchmark	(3) Cluster	(4) +Controls	(5) More Agg.	(6) Agg.	(7) Missing
Currency Union	0.34***					0.11*	
	(0.018)					(0.065)	
EMU		0.43***	0.43***	0.11	0.11		
		(0.021)	(0.061)	(0.072)	(0.071)		
CFA Franc		0.58***	0.58**	0.38			
		(0.100)	(0.24)	(0.30)			
East Caribbean CU		-1.64***	-1.64***	-1.62***			
		(0.11)	(0.21)	(0.21)			
Australian Dollar		0.39**	0.39	0.37			
		(0.20)	(0.38)	(0.38)			
British Pound		0.55***	0.55***	0.34***			
		(0.034)	(0.096)	(0.10)			
French Franc		0.87***	0.87***	0.42			
		(0.083)	(0.27)	(0.29)			
Indian Rupee		0.52***	0.52	0.35			
		(0.11)	(0.40)	(0.32)			
U.S. Dollar		-0.051	-0.051	-0.051			
		(0.063)	(0.19)	(0.19)			
Other CUs		-0.10*	-0.10	-0.23			
		(0.058)	(0.22)	(0.23)			
Non-EMU CUs					0.11		
					(0.085)		
CU (Nonmissing)							0.087
							(0.066)
CU (Missing)							0.28
							(0.18)
Observations	877736	877736	877736	877736	877736	877736	877736

Notes: Standard errors here are clustered in only one dimension from the third column, on pairid (we thank an editor for the suggestion not to multiway cluster, as is standard in this context, in the interests of conservatism). 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", "currently a colony" and "same country" are omitted for space. Columns (1) and (2) replicate Table 5 of GR. Column (3) clusters the errors by country-pair. Column (4)–(7) include the controls from Section 2.2.2: (a) EU*Year FEs, (b) EE*Euro*Year FEs, (c) U.K. Colony*Year FEs, (d) Common UK Colony year trend, (e) CFA Exit*year trend, (f) hostile colonial breakup dummy (=1 for still a colony). *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

regression is likely slightly over-differenced given the FEs. Omitting some of the FEs would also reduce the bias and degree of over-differencing (a regression of exports on lags of itself without FEs would yield a sum of lagged coefficients of .97, very close to a unit root) but would not materially alter our conclusions.

Table 4: Dynamic Models

	(1) ln(X)	(2) Δ ln X	(3) Δ ln X (+Controls)	(4) ln(X)	(5) ln(X) (+Controls)	(6) ln(X) (long only)
Currency Union	0.34***	-0.00024	-0.0040	0.14***	0.025	0.016
	(0.057)	(0.0058)	(0.0073)	(0.022)	(0.025)	(0.021)
L.ln(Exports)				0.44***	0.44***	0.53***
				(0.0030)	(0.0030)	(0.0042)
L2.ln(Exports)				0.13***	0.13***	0.14***
				(0.0029)	(0.0029)	(0.0043)
L3.ln(Exports)				0.085***	0.084***	0.092***
				(0.0023)	(0.0023)	(0.0033)
Observations	877736	716727	716727	680737	680737	437331

Notes: The dependent variable in columns (1) and (4)-(6) is log exports, and the log change in exports in columns (2) and (3). Each regression includes country-pair FEs (CPFEs) and importer*year and exporter*year FEs. Columns (3), (5), and (6) add in the set of controls described in Section 2.3. $^*p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

3.4 PPML Results

In Table 5, we present our results for the PPML regressions. In the first two columns, we present the benchmark results with none of our added controls from Section 2.2.2. In the second two columns, we add in the controls, and the results change. In particular, the results for CUs overall go from a coefficient of .13 (implying an impact of about 14%), and highly significant, to a coefficient of .07 (implying an impact of about 7%), and no longer statistically significant. Once again, the estimates differ wildly across CUs, but our conclusion is the same: the CU effect is not robust.

Table 5: PPML

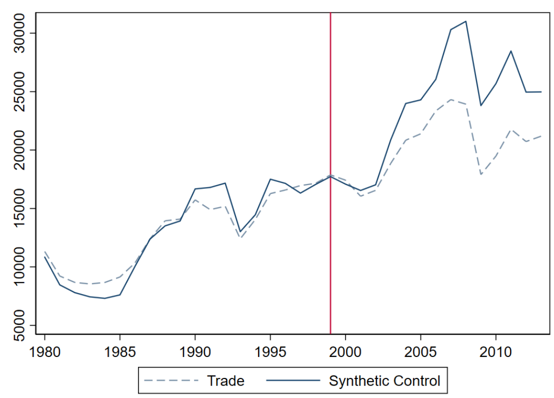
	(1) Benchmark	(2) Benchmark, Disagg.	(3) +Controls	(4) Agg.
Currency Union	0.13**			0.070
	(0.042)			(0.050)
EMU		0.027	-0.040	
		(0.041)	(0.052)	
CFA Franc		0.14	0.39	
		(0.34)	(0.28)	
East Caribbean CU		-1.01***	-0.92***	
		(0.28)	(0.23)	
Australian Dollar		0.17	0.18	
		(0.29)	(0.29)	
British Pound		1.00***	0.69***	
		(0.14)	(0.14)	
French Franc		2.10***	1.58***	
		(0.22)	(0.24)	
Indian Rupee		0.082	-0.057	
		(0.37)	(0.31)	
U.S. Dollar		0.014	0.0084	
		(0.068)	(0.069)	
Other CUs		0.79***	0.62***	
		(0.19)	(0.16)	
Observations	879794	879794	879507	879507

Notes: Standard errors clustered at the country-pair level. The dependent variable is the level of directional exports (not the log) from country i to j at time t. Each regression includes country-pair, importer*year, and exporter*year FEs. Columns (3) and (4) add in the same controls as discussed in Section 2.2.2: (a) EU*Year FEs, (b) EE*Euro*Year FEs, (c) U.K. Colony*Year FEs, (d) Common U.K. Colony year trend, (e) CFA Exit*year trend, (f) hostile colonial breakup dummy (=1 for still a colony). *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

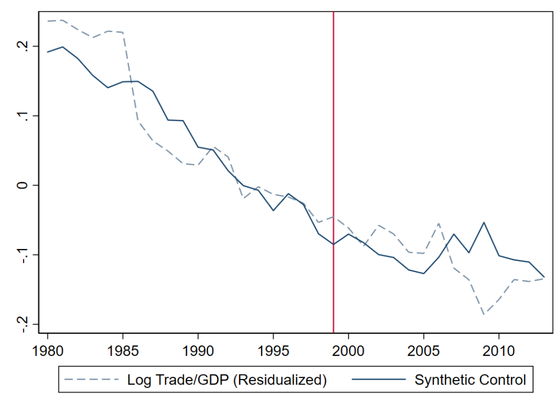
3.5 Synthetic Control Method

First, we replicate Saia's (2017) results in Figure 6(a) and the first row of Table 6, which shows that had the U.K. adopted the Euro, its trade would have been much higher. We then present our alternative counterfactual in Figure 6(b) and the second row of Table 6, and we find starkly different results. In this counterfactual, the U.K.'s trade–to–GDP ratio would have been about 4.7% less than it actually was in 2006 had it decided to join the eurozone. By 2013, the counterfactual implies slightly higher trade (0.5%), but given the noise in the year-to-year time series, this appears to be well within any reasonable margin of error. Thus, the evidence using the synthetic control method also matches findings in the previous sections using panel regressions.

What might be less obvious is that these results are also in line with some of Saia's analysis. He finds that the U.K.'s trade with Japan, for example, would have been 19% higher had the U.K. adopted the Euro—curiously a larger estimated impact than the U.K.'s estimated trade increase with actual eurozone economies. While Saia interprets this as more evidence of the trade-creating effects of the Euro, it could instead be interpreted as a falsification exercise. Theoretically and intuitively, if adoption of the Euro causes a decline in trade costs, we should find a larger trade impact for country pairs that experienced the decline in trade costs, while third-country trade might even be expected to suffer from crowding out, although the overall sign is theoretically uncertain. An explanation might be that British trade growth was generally sluggish after 1999 for reasons unrelated to the Euro. Note that, separately in the Appendix, we find no evidence that eurozone countries' trade with the rest of the world increased more than non-Euro Western European countries' trade with the rest of the world after the adoption of the Euro, and, relative to trend, appears to have declined.



(a) Saia Benchmark



(b) Robustness

Figure 6: Did the U.K. Miss Out? A Sythetic Control Approach

Notes: Panel (a) replicates Saia (2017). The dotted line represents actual U.K. trade with the eurozone, while the blue represents trade from a synthetic control group using only trade within eurozone members. Panel (b) uses trade over bilateral GDPs as the dependent variable, chooses a control group using growth, and includes a "country 2 dummy" in the selection criteria.

4 Conclusion

We estimate the impact of CUs on trade and find a noisy zero. Previous large estimated impacts of CUs on trade (such as GR (2016) and Saia (2017)) were driven by third factors and are sensitive to various intuitive controls, including dynamic and PPML

Table 6: Synthetic Control Results

	2006	2013
Log Diff. in Trade Rel. to 1997 (Saia Replication)	.115	.170
Log Diff. in Trade/GDP Rel. to 1997 (Our Robustness)	-.047	.005

Notes: This table shows the log increase in trade from 1997 had the U.K. joined the Euro relative to how trade actually evolved, according to the Saia synthetic control counterfactual (row 1) and our counterfactual (row 2). For example, in the Saia counterfactual trade would have been about 17% higher in 2013 relative to 1997 had the U.K. joined the Euro, while in our counterfactual trade/GDP would have been about 0.5% higher.

specifications. Pre- and posttreatment trends overall are not supportive of a CU effect on trade. When we adopt a dummy-variable matching approach to compare the evolution of trade between country-pairs that have a CU switch and intuitive control groups, we find that the apparent impact of CUs on trade shrinks to more plausible levels, albeit imprecisely estimated with single-way clustered standard errors. We also find that the apparent CU effect is sensitive to dynamic specifications and that our findings hold up when we adopt a synthetic control approach.

A limitation of our study is that 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 results—insignificant in the dynamic or PPML specifications, or at best borderline significant in a static log specification—could still be biased in either direction. That the estimated CU effect is much larger (and significant) for CU switches coterminous with missing data also suggests that there may be other important omitted variables, such as political alliances, and various aspects of trade policy, such as tariffs and quotas, that are difficult to control for with such a large data set. Also, in the interests of being conservative, we report results when clustering in a single direction. Yet, we cannot guarantee that we have removed all the sources of correlation in the errors, and when we multiway cluster, the estimated errors tend to increase and statistical significance is reduced further.

We believe the finding of large causal effects of CUs on trade in the literature deserves to be a textbook case-study of endogeneity and omitted-variable bias in empirical international trade. This literature also shows the importance of Bayesian priors, since our initial reason for skepticism was that the magnitude of the measured effect—a doubling of trade—is simply too large relative to related results in the literature. For example, Irwin (1998) finds that the Smoot-Hawley tariff was estimated to have decreased trade by 4% to 8%. How plausible is it that CUs could have had an impact 12 to 20 times larger? This is particularly so since Klein and Shambaugh (2006) find that indirect currency pegs—more likely to be random—are also uncorrelated with higher trade flows. 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 (the definition of a CU). Our findings can also help explain why different CUs appeared to have wildly different effects on trade: the results may simply be spurious. Finally, note that there is nothing in this analysis to rule out a positive (or negative) effect size of 1% to 2%, or even 10%, given that one estimated standard deviation from zero in our static regression corresponds to roughly a 6% increase in trade. The conclusion therefore, is twofold: the previous large, highly significant estimates are fragile, and our best estimates are that CUs have a much smaller effect on trade.

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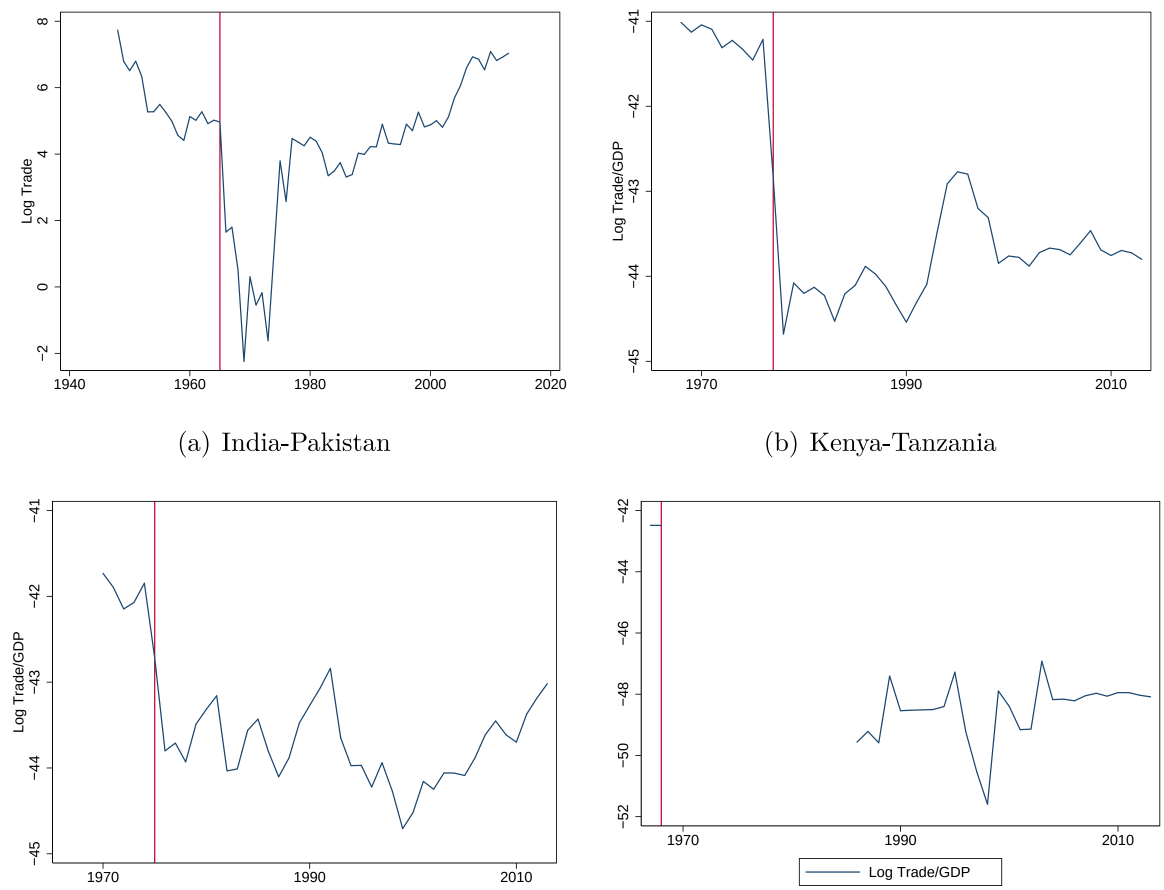
5 Appendix



(a) India-Pakistan

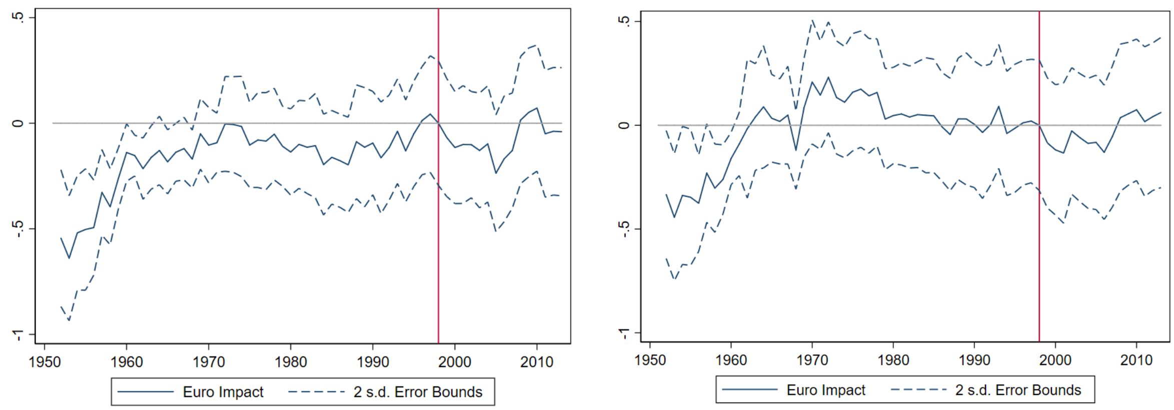
(b) Kenya-Tanzania

(c) Portugal-Angola

(d) Guinea-Mauritania

Figure 7: Currency Unions, Wars, Missing Data, and Trade Collapses

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

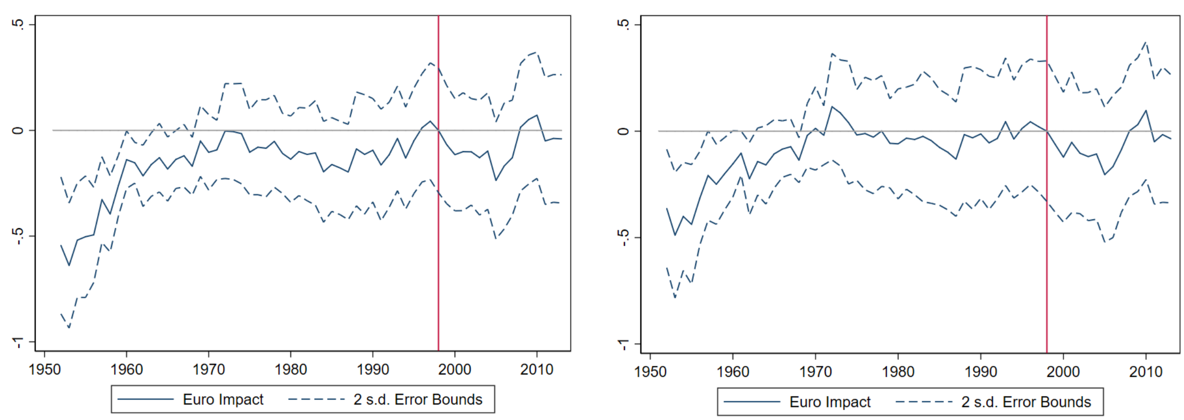


(a) No EU Control

(b) Adding Dynamic EU Control

Figure 8: The Euro Effect by Year (Europe as Control Group)

Notes: Panel (a) shows the evolution of directional exports of countries that eventually joined the eurozone using the rest of Europe as a control, using equation 2.2. All Western European countries with at least 40 observations are included. Panel (b) also controls for a dynamic EU effect using dummies for years before and after EU accession.

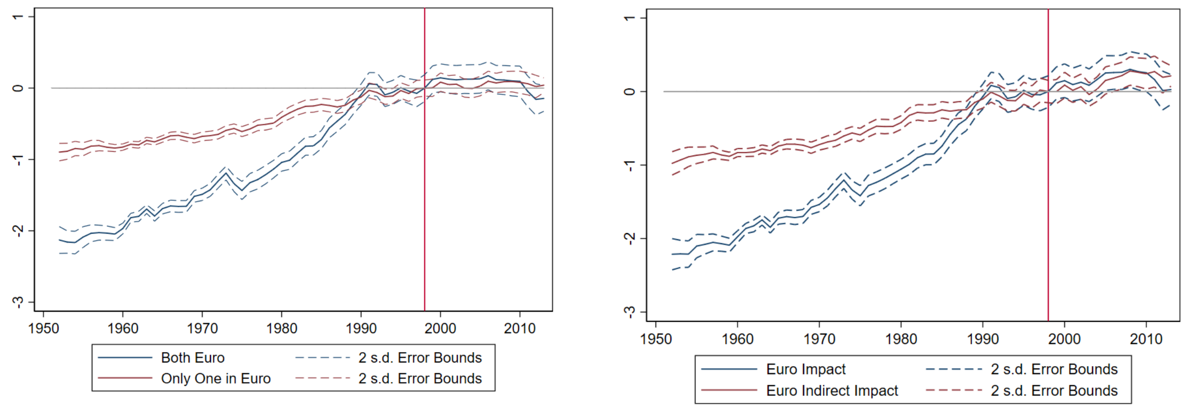


(a) No EU Control

(b) Adding Dynamic EU Control

Figure 9: The Euro Effect by Year (Europe as Control Group)

Notes: Panel (a) shows the evolution of directional exports of countries that eventually adopted the Euro using the rest of Europe as a control, using equation 2.2. All Western European countries with at least 40 observations are included. Panel (b) also controls for a dynamic EU effect, this time using a separate dummy for each year for a country pair where both were EU members (regardless of whether it was before or after joining). This slightly different method of controlling for a dynamic EU effect yields similar results to those in Figure 8. We thank a referee for suggesting.



(a) One in Western Europe as Control (b) One in Europe as Control

Figure 10: Did the Euro Increase Trade With the Rest of the World?

Notes: Panel (a) shows the evolution of directional exports of countries that eventually adopted the Euro and compares it to the evolution of exports for country-pairs with exactly one country using the Euro, using all country-pairs with at least one country in Western Europe as controls. Panel (b) uses as a control group all country-pairs in which one country eventually became a member of the EU. These regressions do not (cannot) control for importer*year and exporter*year FEs.

(a) Euro vs. Europe (b) Euro vs. the EU

Figure 11: Trade Impact of the Euro, Europe, and EU: World as Control

Notes: Panel (a) plots ever Euro*year FEs (thus measures the Euro effect over time) in a regression that also includes Western Europe*year FEs in the same regression. In Panel (b), we compare the annual Euro dummies with annual "Ever EU" dummies ("Ever EU"*year interactive FEs), in the same regression as the EMU. The full data set is the control group.

Appendix Table 1: List of Switches Coterminous with a Hostile Colonial Separation

Country-Pair	*Last Year of Event*	*Description*
1. United Kingdom-Zimbabwe of Colony	1964	Independence and Trade Sanctions; Rhodesian Bush War
2. France-Algeria	1962	War of Independence; Assasination; Military Consolidation of Govt.
3. France-Morocco	1956	Moroccan Independence following Anti-Colonial Rioting
4. France-Tunisia	1956	Tunisian Independence granted after separatist bombings
5. Portugal-Angola	1975	Angolan War for Independence followed by Civil War
6. Portugal-Cape Verde	1975	Cape Verde part of Guinea-Bissauan War of Independence
7. Portugal-Guinea-Bissau	1974	War for Independence; Marxist takeover, opposition slaughtered
8. Portugal-Mozambique	1975	War for Independence; Civil War
9. Portugal-Sao Tome and Principe	1975	Declared Independence following Coup in Portugal
10. Burma (Myanmar)-Pakistan	1970	Indo-Pakistani Wars; Myanmar expels 250,000 Muslims
11. Sri Lanka-India	1965	India-Pakistan war in 1965
12. India-Pakistan	1965	Border War, repeated conflicts thereafter
13. Algeria-Guadeloupe	1962	War of Independence; Assasination; Military Consolidation of Govt.
14. Guiana-Algeria	1962	War of Independence; Assasination; Military Consolidation of Govt.
15. Martinique-Algeria	1962	War of Independence; Assasination; Military Consolidation of Govt.
16. Algeria-Morocco	1956	Moroccan Independence following Anti-Colonial Rioting
17. Tunisia-Algeria	1962	War of Independence; Assasination; Military Consolidation of Govt.
18. Tunisia-Morocco	1956	Moroccan Independence following Anti-Colonial Rioting

6 Not for Publication Appendix

6.1 Using Country-Pair FEs Regression Framework Instead

The standard way to try to identify the CU effects is with a gravity equation in levels:

$$ln(T_{ijt}) = \gamma CU_{ijt} + \beta Z_{ijt} + \gamma_{ij} + \delta_t + \epsilon_{ijt}, \tag{6.1}$$

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 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 (country i's imports from all countries except for j). 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 CUs 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.

In Table 7 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 multiway clustered errors, the latter of which have only 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 the Indian Rupee CU 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 the 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 3 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 3.

Figure 12 compares the evolution among future Euro countries to all Western European EU countries (adding Sweden, Denmark, and Great Britain), and to all countries in Western Europe (adding in Norway, Switzerland, and Iceland). The figure shows that there was a steady increase in trade integration in Western Europe from 1950 to 1990,

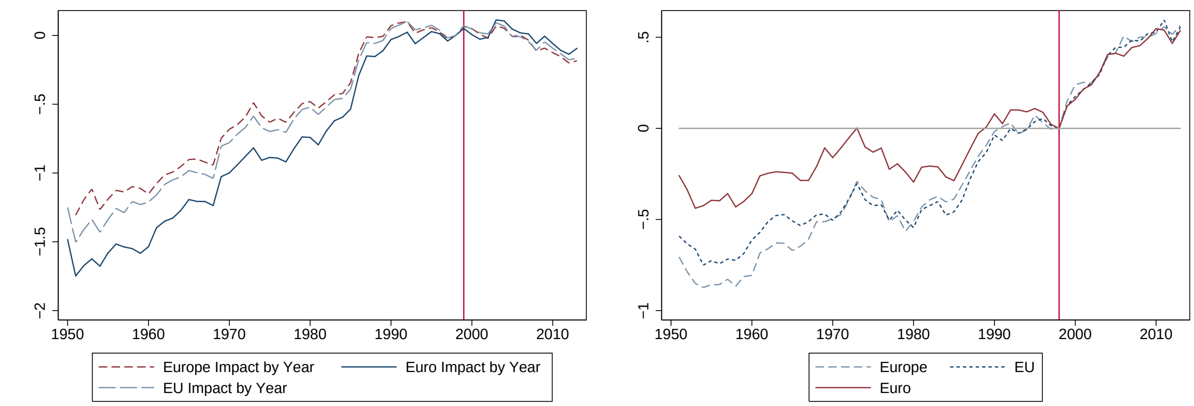
Table 7: How Robust Is the CU Impact on Trade? (CPFE Regressions)

	(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 multiway 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.

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 (Iceland, Norway, and Switzerland are not in the EU, and the U.K., Sweden, and Denmark are EU countries not in the eurozone). 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 pretrend in the years before the formation of the Euro.



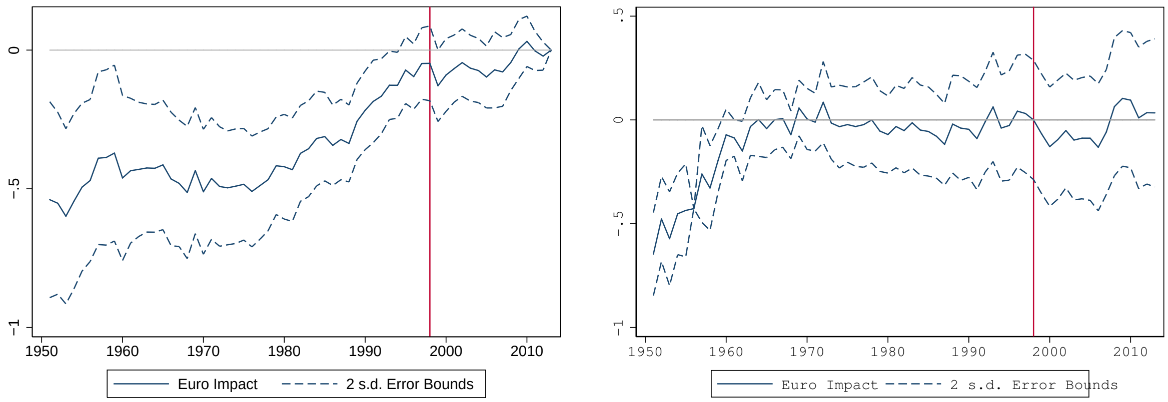
(a) Model with CPFEs

(b) Model with Importer/Exporter*Year FEs

Figure 12: The Euro Impact vs. the EU, Europe

Notes: Panel (a) shows the evolution of the trade intensity of countries that eventually adopted the Euro vs. those that eventually joined the EU and vs. all of Europe. I.e., it plots annual gravity dummies from equation 2.1. 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 includes only non–Warsaw Pact countries. Panel (b) provides the same comparison, using directional exports as the dependent variable, importer and exporter*year interactive FEs, from model 2.2.

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 1, 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 eurozone 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 eurozone 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 13: 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 2.1. 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.

6.1.1 Additional Country-Pair Fixed Effects Regressions (Online Appendix)

Next, both for robustness, and for comparison with previous research, we run the following country-pair fixed effects (CPFE) regression:

$$ln(T_{ijt}) = \sum_{k=1}^{K} \alpha^k CU_{ijt}^k + \beta Z_{ijt} + \gamma_t + \delta_{ij} + \epsilon_{ijt}, \tag{6.2}$$

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

Next, in Table 8, we compare the country-pair fixed effects estimates on all CUs aggregated from various estimates in the literature, and our new estimates. Glick and Rose (2002) find a coefficient on CUs of .65, implying a near doubling of trade, precisely estimated with a t-score of over 15. By contrast, Campbell (2013), using the same data, find that the coefficient fell to just .11, and imprecisely estimated, although he also finds a negative coefficient when controlling for country-pair specific trends.[10] 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

10. Note that, using the GR (2016) sample, we also find that including CU specific trends alone kills the results.

coterminous with missing data in column (4), and also include a number of intuitive controls (including U.K.-colony*year interactive FEs), the coefficient on CUs 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 CUs on trade close to 11%, neither are precisely estimated, while Campbell (2013) also found that including trend controls yields an impact of –5%, while Table 3 yields an estimate of 5%. Clearly, these are noisy estimates which are likely to be influenced further by additional controls.

Table 8: 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 include other intuitive controls.

6.2 Additional Results for the Euro (Online Appendix)

6.2.1 Additional Euro Results: Limiting the Sample to Northern Europe

Most of the control countries for the Euro—i.e., countries that are in the EU but not in the Euro, or countries that are in Western Europe but not in the EU—are actually located in Northern Europe. Thus, the more comparable portion of the treatment group for this particular control group would actually be other Northern European countries that are in the eurozone. Thus, we repeat the exercise we did above in Figure 13, only comparing the evolution of trade between Euro countries, EU countries, and all countries in Western Europe while limiting ourselves only to countries in Northwest Europe (thus, we exclude Portugal, Spain, Italy, Greece, Cyprus, and Malta from this analysis). The results are presented in Figure 14. In this case, it does look as though Euro countries slightly outperformed other countries in NW Europe after the formation of the Euro, although the difference is not statistically different. The differences with Figure 1 in the main paper are slight, which is why we relegate this version to the Online Appendix.

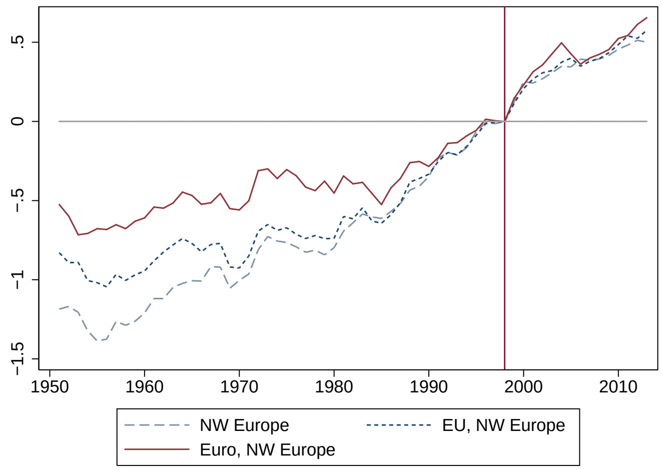


Figure 14: The Euro Impact vs. the EU, Northwest Europe

Notes: This graph shows the evolution of the export intensity of countries in Northwest Europe that eventually joined the Euro, vs. those that eventually joined the EU, and vs. all of Northwest Europe (we exclude Portugal, Spain, Italy, Greece, Cyprus, and Malta from this analysis). *I.e.*, it plots annual gravity dummies from equation 2.1. 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.

6.2.2 Additional Panel Regression Results for the Euro

Next, we move to a panel regression approach so that we can definitively answer whether the Euro effect is statistically significant pooled across years; we report the results in Table 9. In this table, we use equation 6.1 (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-eurozone*Year interactive fixed effects, using the same control group as in Figure 4. We also add in multiway 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 we also include a simple time trend control and limit the period to after 1975, when the trend starts, as is implied by Figure 13(a). We find that this trend control eliminates the significance of the Euro.

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 multiway 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-eurozone*Year interactive controls as in column (2), we get a point estimate of .055 (roughly 5%), but

Table 9: 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-eurozone*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 adopted the Euro. Columns (3) and (7) limit the sample to the post-1975 and post-1965 periods, respectively.

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 13(b), we limit the sample to the post-1965 period, and find 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."

6.2.3 Did the Euro Increase Trade With Non-Euro Members?

In the main part of the paper, we tested whether trade between Euro countries increased relative to trade between trading pairs where one country was in the Euro and one was not. However, the Euro might also have raised trade generally. Thus, here we test first whether the Euro increased overall trade, and secondly, whether CUs in general increase trade-to-GDP ratios.

We propose a simple model:

$$ln(X_{it}/GDP_{it}) = \alpha Euro_{it} + \alpha_i + \gamma_t + \epsilon_{ijt}, \tag{6.3}$$

a regression of annual dummies for all Western European countries that joined the Euro, with controlling for country and year dummies. The sample is all countries in the world that have both World Bank GDP data and IMF DOTS trade data. The annual dummies with two standard deviation errors are plotted in Figure 15. Panel (a) shows that while trade over GDP rose strongly in eurozone countries until the early 1990s, since 2005 this intensity has fallen. This could be for many factors unrelated to the Euro, such as relatively rising trade integration in other parts of the world, or to the growth slowdown that may have lowered trade more than one-for-one.

(a) Euro Countries

(b) Euro Countries vs. All of Europe

Figure 15: The Impact of the Euro on Total Trade/GDP

Notes: Panel (a) shows the average trade/GDP relative to 1998 of Western European countries (not Warsaw Pact countries in Europe) with 2-standard-deviation error bounds. Panel (b) compares future Euro countries to all of Western Europe. E.g., this sample adds the U.K., Iceland, Sweden, Switzerland, Norway, and Denmark to the sample of Western European Euro countries.

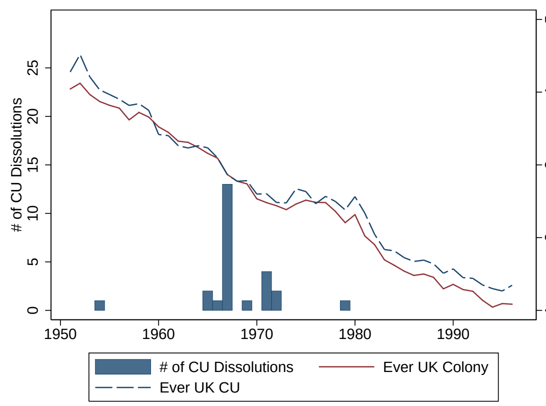
6.3 U.K. Currency Unions

In GR (2002), 26 of the 136 CU switches in the sample involved the U.K.. In GR (2016), slightly over a third (150 of 408 of the bilateral pairs) of the CU switches involve the British Pound.

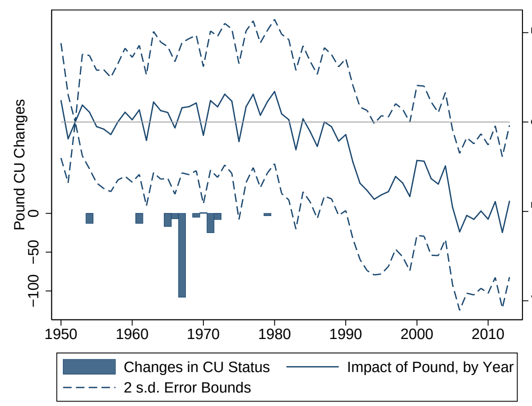
In this case, since most country-pairs with time series variation in CU status exhibit just one change in status—a dissolution—a panel regression in levels with no controls for trends could be prone to finding correlations even when a true relationship does not exist. The basic problem can be seen in Figure 16(a), below, when we compare the evolution of trade between the U.K. and its former colonies vs. the U.K. and countries with which it used to share CUs with (adding yearly dummies to equation 6.1).[11] There is a lot of overlap between the two, as all but one country that shared former CUs with the U.K. in this sample were also former colonies, while nearly half of the former

11. Both of the lines plotted come from Campbell (2013), who claimed to have "solved" the Glick and Rose puzzle.

colonies had CUs (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 CUs 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 U.K. trade with its former colonies, by contrast, one will not find a correlation between CUs and trade.



(a) Evolution of U.K. Trade With Colonies vs. CUs



(b) All Pound CUs (Dir. Export Spec.)

Figure 16: Trade and the Pound

Notes: Panel (a) plots the evolution of gravity dummies over time between the U.K. and its former colonies, compared to the evolution of trade between the U.K. 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 U.K.), 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.

However, with this new, much larger data set, many of the observations of countries that used the Pound did not necessarily involve the U.K. Running the second version of our model with directional exports as in equation 2.2 in Figure 16(b) on all U.K. 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 10, using the regression in equation 2.2 for the first three columns and the model in 6.1 in the last two columns. We separate out the Pound CUs involving the U.K. from the others. In column (2), we add in U.K.-U.K. colony*year FEs, and another set of annual FEs when both countries are U.K. colonies, and the coefficients on both U.K. CUs and other Pound CUs both shrink moderately. Next, we limit the sample to the pre-1990 period, as suggested by Figure 16(b); we find that the U.K. CUs are no longer significant. Turning to the model using bilateral trade as the dependent variable (equation 6.1), we find that the Pound increased trade among those who used it by 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 include the same controls as we did in column (2) (with the results in column 5), UCs now appear to *reduce* trade by a sizeable 17%, although imprecisely estimated. The non-U.K. Pound CUs admittedly provide the best evidence for the CU effect, although, as we see, the significance of these depends on the specification.

Table 10: British Pound Currency Unions and Trade: How Robust?

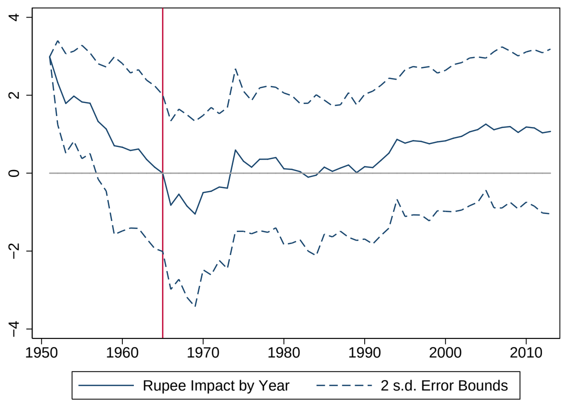
	(1) Benchmark	(2) +Controls	(3) Pre-1990	(4) Benchmark	(5) +Controls
UK CUs	0.54***	0.30**	0.13		
	(0.039)	(0.15)	(0.13)		
Pound CUs (ex-UK)	0.56***	0.46***	0.28**		
	(0.045)	(0.13)	(0.13)		
British Pound				0.93***	-0.17
				(0.12)	(0.12)
Observations	877736	871392	368103	426507	372625
Dep.Var.	Exports	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 6.1, and benchmarks GR (2016), Table 2. Column (5) adds in the same controls as columns (2) and (3).

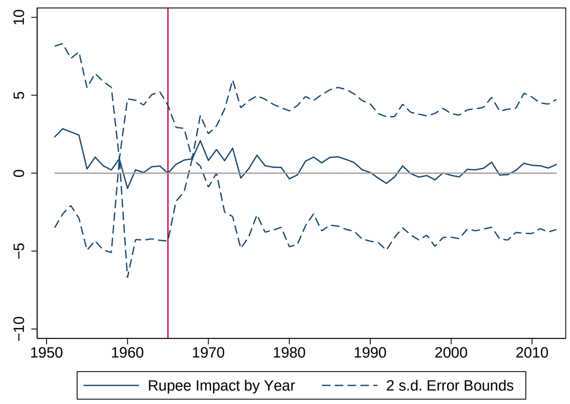
6.4 The Indian Rupee Zone

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, that also likely effected 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 political 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 that had ever shared the Rupee in Figure 17(a) (analogous to equation 2.1). 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 CUs. 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 17: The Rupee Zone

Notes: Panel (a) shows the evolution of the trade intensity over time of countries that 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.

6.5 The CFA Franc Zone

When we look at data for the CFA Franc Zone, we see a potential role for missing data in helping to explain "the Rose Effect." 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 18(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 recorded only 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 19(a)), and entrance (Figure 19(b)).[12] 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 five 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

12. Once again, here we are using model 2.1.

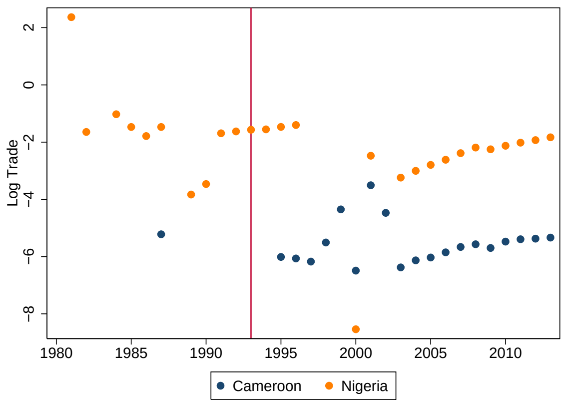
Table 11: Checking the Rupee Effect

	GR Benchmark	Cluster
Indian Rupee	0.52	
	(0.40)	
Indian Rupee (ex India-PAK & India-SRI)		0.10
		(0.31)
Observations	877736	877736

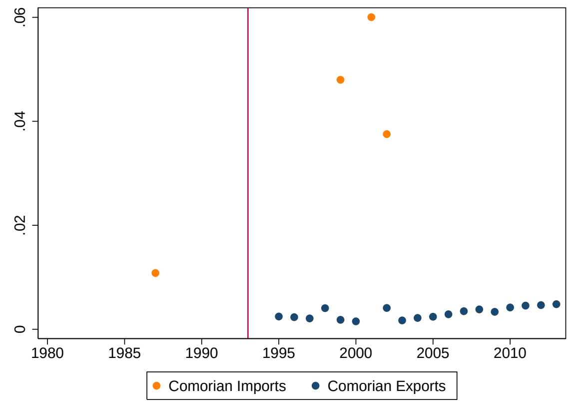
*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in the first three columns is the average of 4-way log bilateral trade flows, and in the last four columns it is the average of exports from country 1 to 2 reported by country 1 and reported by country 2. The first three columns include 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. Column (3) includes a control variable for trends in trade for countries that eventually adopted the Euro. Columns (3) and (7) limit the sample to the post-1975 and post-1965 periods, respectively.

naive dummy variable regression ignoring dynamics will find that trade flows were significantly higher in the pre-dissolution period. The trade dynamics for entrants in Panel (b), on the other hand, admittedly does provide suggestive evidence for the proposition that CUs increase trade. However, even then, the dynamics look questionable, as bilateral trade was roughly the same 15 years after the CU as it was before.

Next, in Table 12, we test the impact of the CFA Franc using a panel data regression approach as in 6.1. 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 five 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 five years before dissolution, and again find insignificant results. 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 five 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.



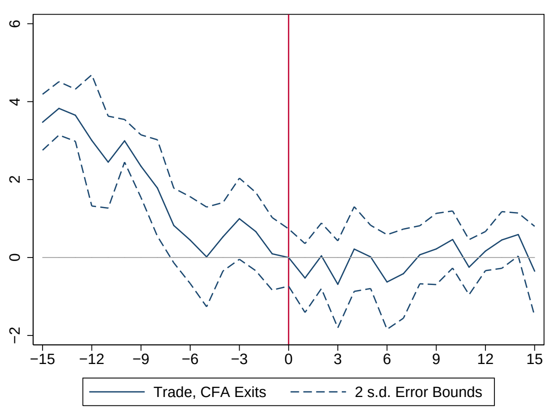
(a) Comoros-Cameroon Trade (vs. Nigeria)



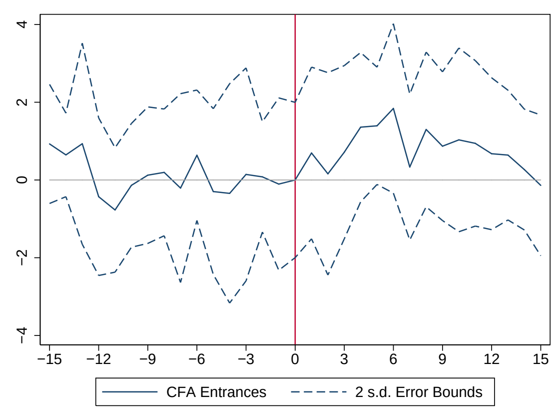
(b) Comoros-Cameroon Trade

Figure 18: Missing Data Drive the "Collapse" in Trade

Notes: Panel (a) shows the evolution of the trade between Comoros and Cameroon, which 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 dissolution.



(a) Exits



(b) Entrances

Figure 19: Impact of CFA Exits and Entrances

Notes: Panel (a) shows the evolution of trade before and after exits into the CFA Franc using equation 2.1. Panel (b) shows the evolution of trade before and after entrances.

Table 12: 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) replicates 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).

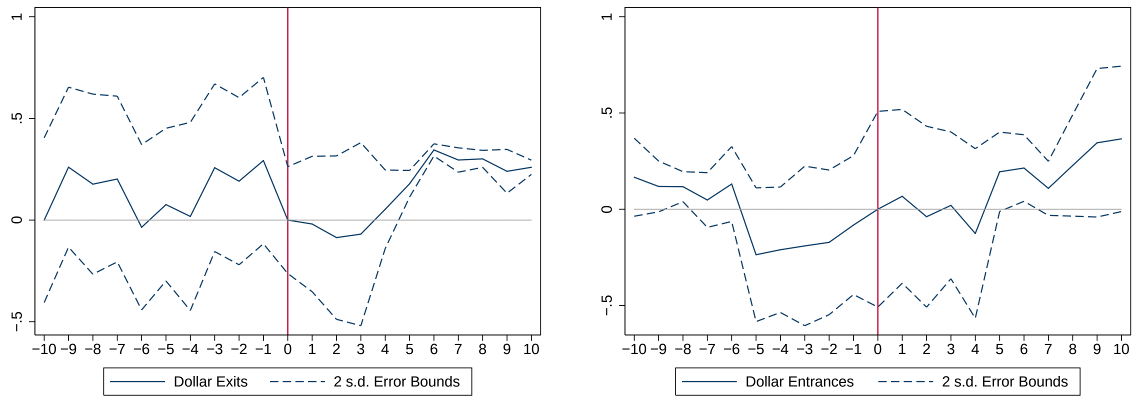
6.6 The Demise of French and Portuguese Currency Unions

In our sample, France had two CUs with time-series variation in the data, and Portugal had five. However, in all of these cases, the end of these CUs 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 CUs—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.

6.7 U.S. Dollar-based Currency Unions

We begin by plotting the pretreatment and posttreatment trends of exiting and entering U.S. Dollar unions in Figure 20. The graphs are created by rerunning equation 2.1 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 "pretreatment trend" before exits, although trade did fall a lot in the last year of the currency union. However, after exit, within five 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.

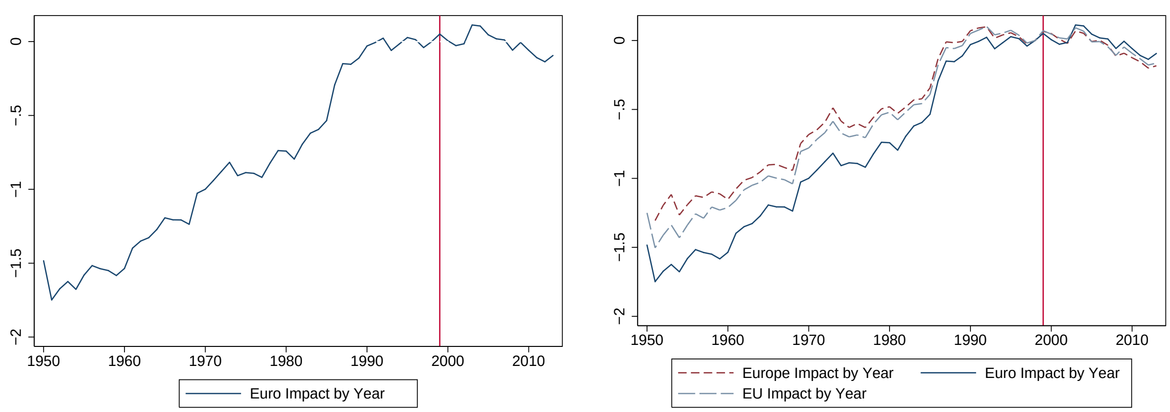
The entrances do not tend to show much, although there appears to have been trade collapses about five years prior to entry. Indeed, on the whole, even Glick and Rose do not find that U.S. Dollar CUs increase trade. Once again, we would argue that this result is another reason to doubt a large CU effect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



(a) Exits (b) Entrances

Figure 20: The Effect of U.S. 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.



(a) Gravity Dummies on Intra-Eurozone Trade (b) vs. Europe and EU

Figure 21: 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 2.1. 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 that would eventually join the EU.

Table 13: 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

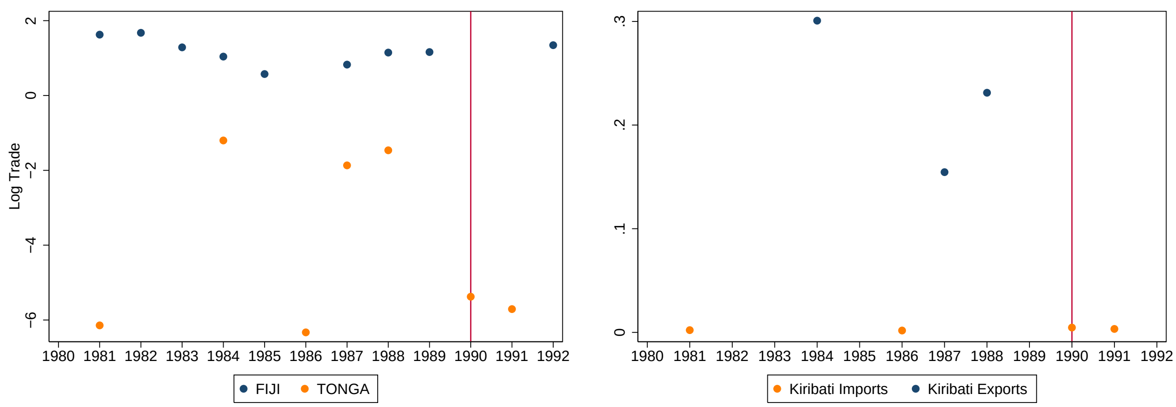
*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the average of 4-way log bilateral trade flows. Each regression includes country-pair and year fixed effects. Column (1) reproduces the results from GR (2002). Column (2) reproduces the results from Campbell (2014). Column (3) replicates the results from Glick and Rose (2017), Table 2 column (4). Column (4) includes the controls, data adjustments, and multiway 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 multiway clusters, and additional control variables, including total exports (ex-bilateral exports) for both countries. 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 CUs in which the changes are not associated with war or some other major geopolitical event or missing data.

6.8 Australian Dollar CUs

Australia shared CUs with several small Pacific islands. Thus, we begin by simply plotting trade for several of these islands to try to understand the factors that might be driving the results. Figure 22(a) plots trade between Kiribati and Tonga, which exited a CU in 1990, and compare it to trade between Kiribati and Fiji, which were never in a CU. 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 22(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 driven by exports from Kiribati to Fiji being recorded only for the date pre-dissolution. Each time they were recorded, they were at a much higher level than imports.

Next, we repeat the exercise we did for the U.S., and plot annual indicator dummies for years before leaving an Australian Dollar CU (there are no entrants) in Figure 23(a). While we see little action after dissolution, there happens to be a trade collapse during the CU period starting about ten 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 23(b), we run the same regression only limiting the control group to all countries that ever used the Australian Dollar plus their



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

(b) Kiribati-Tonga Trade

Figure 22: Missing Data Drive "Collapse" in Kiribati-Tonga Trade

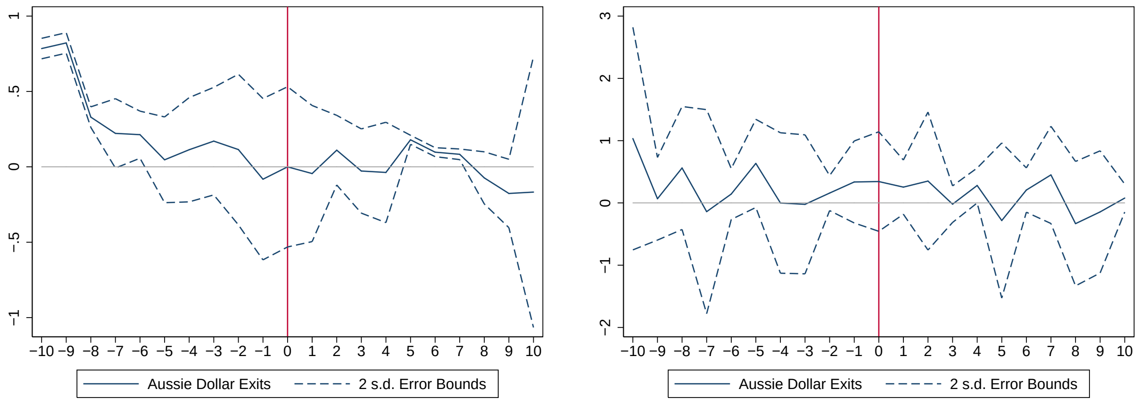
Notes: Panel (a) shows the evolution of the trade between Kiribati and Tonga vs. Kiribati and Fiji. Kiribati and Tonga ended their currency union in 1990. After, trade was lower. Kiribati-Fiji might be a good control, but its data is missing in 1990 and 1991. In Panel (b), we disaggregate Kiribati-Tonga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

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 6.1. 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 trade), and total imports (also ex-bilateral trade). These mild controls further reduce the coefficient by another 8%, at which the coefficient on Australian CUs is only significant at 10%. In column (4), we add in country*year interactive FEs for each of the countries that have Australian Dollar 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.

6.9 Additional Results on Dynamics

On the whole, Figure 3 does not necessarily imply a pressing need to take a dynamic approach, as excluding the CUs coterminous with wars and missing data, and adding in other controls mostly eliminated the pretreatment trends. On the other hand, Panel (b) suggests this might be advisable. Thus, next we show our main result—that the



(a) Impact of Leaving

(b) Relative to New Zealand

Figure 23: Australian Currency Unions

Notes: Panel (a) shows the evolution of the trade intensity of countries that had CUs 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.

Table 14: 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 FEs, and errors clustered by country-pair in parentheses. Other controls, including GDP and year FEs, are 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.

impact of CUs on trade is not statistically significant—holds up even when we add in an LDV. We do this for both the GR specification in column (1) in Tables 3 and 7, 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 18, we add in lagged log bilateral trade as a control variable to the regression in equation 6.1. 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.[13] 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

13. 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.

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

Table 15: 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*year and exporter*year FEs. Column (1) adds in an LDVto 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 an 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.

6.10 A Systematic Control for War (Online Appendix)

Table 16: 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.

Table 17: Dynamic Models II

	(1) ln(Trade)	(2) ln(Trade)	(3) ln Δ Trade	(4) ln(Exports)
Currency Union	0.0017	-0.0018	-0.0017	-0.0064
	(0.0063)	(0.0055)	(0.0074)	(4.68)
L.lchexp1to2		-0.36***		-0.36***
		(0.0045)		(0.0053)
Observations	716727	628929	716727	628929

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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 column (4). Each regression includes country-pair FEs (CPFEs).

6.11 Additional Plots of Trade (Online Appendix)

Figure 25(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 CU dissolution, there was no trade recorded for the entire 1960s. Secondly, the trade data pre-dissolution that 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.

Table 18: Dynamic Models III

	(1) ln(Trade)	(2) ln(Trade)	(3) ln Δ Trade	(4) ln(Exports)	(5) ln(Exports)	(6) ln Δ Exports	(7) est7
Currency Union	0.16*	0.072*	0.046	-0.0017	-0.0017	0.0017	0.035
	(0.064)	(0.031)	(0.025)	(0.0070)	(0.0070)	(0.0059)	(0.021)
L.lexp1to2		0.54***	0.44***				0.53***
		(0.0028)	(0.0030)				(0.0042)
L2.lexp1to2			0.13***				0.14***
			(0.0029)				(0.0043)
L3.lexp1to2			0.084***				0.094***
			(0.0023)				(0.0034)
Observations	877736	783749	680737	716727	716727	716727	428053

$^*p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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.

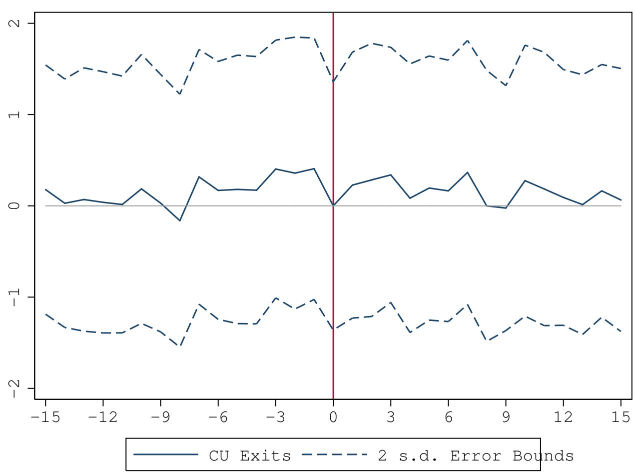
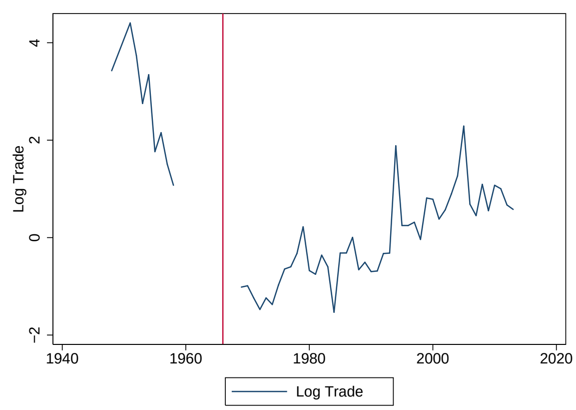


Figure 24: Impact of the “Correlates of War” Dummy on Trade

Notes: This figure plots leads and lags of a dummy for two countries being in a war with each other. The dummy when entered itself in a panel regression. It’s likely because occasionally one country is occupying part of the other country, either during, or after the war ends.

(a) India-Pakistan



(b) Sri Lanka-Mauritius

Figure 25: 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.

Table 19: PPML Estimation: Glick and Rose Replication

	(1) All CUs	(2) Disagg. EMU	(3) Disagg CUs	(4) All CUs	(5) Disagg. EMU	(6) Disagg CUs
All CUs	-0.13***			0.13**		
	(0.019)			(0.042)		
All CUs w/o EMU		0.22***			0.70***	
		(0.040)			(0.11)	
EMU Dummy		-0.20***			0.030	
		(0.022)			(0.042)	
Aussie			1.19***			0.17
			(0.093)			(0.29)
CFA Frank Zone			0.53***			0.14
			(0.053)			(0.34)
East Carribean CU			0.72***			-1.01***
			(0.080)			(0.28)
EMU			-0.19***			0.027
			(0.022)			(0.041)
French Frank			2.41***			2.10***
			(0.048)			(0.22)
British Pound			1.02***			1.00***
			(0.044)			(0.14)
Indian Rupee			-0.36***			0.082
			(0.090)			(0.37)
US			-0.35***			0.014
			(0.040)			(0.068)
Other CUs			-0.12			0.79***
			(0.17)			(0.19)
Observations	879794	879794	879794	879794	879794	879794

$*p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. A PPML analog of Glick and Rose (2016) Table 5. Columns (1)-(3) report estimation with Importer/Exporter time fixed effects only, and columns (4)-(6) add country-pair fixed effects. The other regressor estimates are omitted for space.

Table 20: PPML estimation

	(1) GR Benchmark	(2) Cluster	(3) Ex-War	(4) +Controls	(5) More Agg.	(6) Overall
EMU Dummy	0.027***	0.027	0.024	0.024	0.033	
	(0.010)	(0.091)	(0.091)	(0.091)	(0.092)	
CFA Franc	0.14	0.14	0.59**	0.59**		
	(0.11)	(0.31)	(0.29)	(0.29)		
East Caribbean CU	-1.01***	-1.01***	-1.03***	-1.03***		
	(0.081)	(0.32)	(0.32)	(0.32)		
Aussie	0.17	0.17	0.17	0.17		
	(0.12)	(0.28)	(0.28)	(0.28)		
British Pound	1.00***	1.00***	1.03***	1.03***		
	(0.034)	(0.23)	(0.23)	(0.23)		
French Franc	2.10***	2.10***	0.65	0.66		
	(0.062)	(0.30)	(0.57)	(0.69)		
Indian Rupee	0.082	0.082	-1.06	-1.06		
	(0.15)	(0.31)	(0.88)	(0.88)		
US	0.014	0.014	0.015	0.015		
	(0.022)	(0.066)	(0.066)	(0.066)		
Other CUs	0.79***	0.79***	0.63***	0.63***		
	(0.052)	(0.25)	(0.24)	(0.24)		
Non-EMU CUs (ex-War, Missing)					0.54***	
					(0.19)	
CUs (ex-War, Missing)						0.097
						(0.077)
Observations	879794	879794	873459	873459	879794	879794

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is unilateral exports, averaged from Importer and Exporter reports. Each regression includes country-pair FEs and Exporter/Importer time fixed effects. Column (1) benchmarks the estimate from Glick and Rose (2016) Table 5, column (6). In column (2) we apply three-way clustered standard errors to the results of column (1). In column (3) we omit the CUs in which switches were coterminous with war or missing data. Column (4) adds intuitive control variables to the specification of column (3). Columns (5) and (6) provide the results of specification (4) for more aggregated CUs.

6.12 Alternative Ways of Clustering (Not-for-Publication Appendix)

Table 21: Multiway Clustering

	(1) One-way Cluster	(2) 2-way Cluster	(3) 3-way Cluster	(4) +Controls, 1way Cl.	(5) +controls, 2way	(6) +Controls, 3way
Currency Union	0.34***	0.34***	0.34***	0.11*	0.11	0.11
	(0.057)	(0.063)	(0.080)	(0.065)	(0.066)	(0.10)
Observations	877736	877736	877736	877736	877736	877736

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is unilateral exports, averaged from Importer and Exporter reports. Each regression includes country-pair FEs and Exporter/Importer time fixed effects. Column (1) runs the Glick and Rose (2016) Table 5, column (6) benchmark, only with country-pair clustered standard errors. In column (2), we cluster by country-pair, and year. In column (3), we cluster by importer, exporter, and year. In column (4), we include our full set of controls detailed in section 2.3. In column (5), we cluster by country-pair and year in our regression with the controls. In column (6), we cluster by importer, exporter, and year, in a regression with controls.

6.13 Additional Robustness Tables (Not for Publication Appendix)

Table 22: Adding in Controls One-by-One

	(1) Benchmark	(2) +Colonial Break.	(3) +CFA Trend	(4) +UK Controls	(5) +EU Controls	(6) +EE Controls
EMU	0.43***	0.43***	0.43***	0.41***	0.25***	0.11
	(0.061)	(0.061)	(0.061)	(0.060)	(0.071)	(0.072)
CFA Franc	0.58**	0.59**	0.39	0.39	0.39	0.38
	(0.24)	(0.24)	(0.30)	(0.30)	(0.30)	(0.30)
East Carribean CU	-1.64***	-1.64***	-1.64***	-1.61***	-1.61***	-1.62***
	(0.21)	(0.21)	(0.21)	(0.21)	(0.21)	(0.21)
Aussie	0.39	0.39	0.39	0.37	0.37	0.37
	(0.38)	(0.38)	(0.38)	(0.38)	(0.38)	(0.38)
British Pound	0.55***	0.55***	0.55***	0.32***	0.33***	0.34***
	(0.096)	(0.095)	(0.095)	(0.10)	(0.10)	(0.10)
French Franc	0.87***	0.42	0.42	0.44	0.43	0.42
	(0.27)	(0.28)	(0.28)	(0.29)	(0.29)	(0.29)
Indian Rupee	0.52	0.28	0.28	0.35	0.35	0.35
	(0.40)	(0.33)	(0.33)	(0.32)	(0.32)	(0.32)
US	-0.051	-0.051	-0.050	-0.048	-0.048	-0.051
	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)
Other CUs	-0.10	-0.29	-0.29	-0.23	-0.23	-0.23
	(0.22)	(0.22)	(0.22)	(0.23)	(0.23)	(0.23)
Observations	877736	877736	877736	877736	877736	877736

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is unilateral exports, averaged from Importer and Exporter reports. Each regression includes country-pair FEs and Exporter/Importer time fixed effects, and errors clustered at the country-pair level. Column (1) runs our Table 3, column (2) specification. Column (2) adds in a dummy for colonial relationships that did not end well. Column (3) adds in a time trend for CFA Franc exits. Column (4) adds in the U.K. colony*year dummies and common U.K. colonizer*year trends. Column (5) adds in a dynamic EU control. Column (6) adds in the Eastern Europe*Western European EMU*year interactive FEs.

Table 23: Previous Master Regression Table (Dec. 2018 Version)

	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 unilateral exports, averaged from Importer and Exporter reports. Each regression includes country-pair FEs and Exporter/Importer time fixed effects, and errors clustered at the country-pair level. Column (1) runs our Table 3, column (2) specification. Column (2) adds in a dummy for colonial relationships that did not end well. Column (3) adds in a time trend for CFA Franc exits. Column (4) adds in the U.K. colony*year dummies and common U.K. colonizer*year trends. Column (5) adds in a dynamic EU control. Column (6) adds in the Eastern Europe*Western European EMU*year interactive FEs.