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Breaking badly: The currency union effect on trade ☆

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ABSTRACT

As several European countries debate entering, or exiting, the euro, a key policy question concerns how much currency unions (CUs) affect trade. Despite the longstanding academic debate on the topic, 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, using various specifications and controls, we estimate that the impact of CUs on trade is indistinct from zero but with relatively large standard errors.

As several European countries debate entering, or exiting, the euro, a key policy question concerns how much currency unions (CUs) affect trade. The eurozone has continued to grow, with the recent additions of Estonia (2011), Latvia (2014), and Lithuania (2015), despite a mixed eurozone economic record. While monetary policy (and questions of politics and identity) may primarily 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 fostered a large increase in trade. Glick and Rose (2016) (hereafter GR), find a greater than 40% impact 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 UK had adopted the Euro; he concludes that the Euro may have increased intra-European trade by an astounding 55%.[2]

In this paper, we revisit the important policy question of whether CUs increase trade using standard tools from modern applied microeconomics. 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

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[2] Glick (2017) argues for a Euro effect 25% smaller than GR (2016), while Glick and Rose (2015), one year before GR (2016), find that the effect 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 (2022), 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),Rotili et al. (2014), Stoykova (2021),He et al. (2021), while Macedoni (2017) finds evidence consistent with the Euro reducing trade costs. Timini (2018) looking historically, finds heterogenous effects.

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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 Fig. 1 we plot the pre- and posttreatment trends in the form of 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 Denmark, Sweden, and the UK, which was in the EU during our sample), and to the evolution of all Western European country-pairs (adding all country-pairs that involve Norway, Switzerland, and Iceland).[3] We find that trade 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 before the Euro was introduced.

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 the trend, the Euro might have actually had a positive impact on trade. This difference turns out not to be statistically significant, as can be seen by the slightly different specification in Fig. 1 Panel (b), which plots the residualized Euro impact using only Western European countries as the control group. Nevertheless, one can get a positive point estimate here only by including pretrends as controls—highlighting the importance of controlling for dynamics, as is missing from some recent key papers. For example, Glick and Rose (2016), Glick (2017),Saia (2017), and Larch et al. (2019) do not control for trends, and they find a positive impact, even though older papers in this literature, including Micco et al. (2003),Flam and Nordström (2006), and Berger and Nitsch (2008), had also highlighted the importance of controlling for trends.

In Section 3.1, we show that a simple plot of pre- and posttreatment trends for CUs overall or for several key individual CUs does not suggest that CUs have a positive impact on trade. For example, for Eastern Europe, 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. 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 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 we 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.

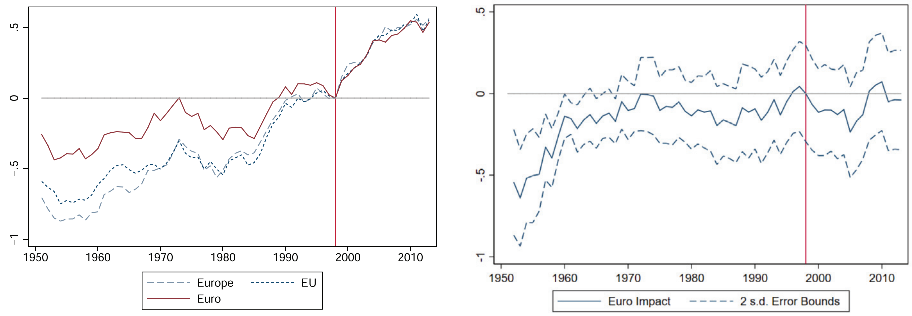
For the UK's trade with countries that used the British Pound, we select other prior British colonial possessions as the control group. In addition, we include a simple dummy variable for hostile colonial breakups. This is a small fraction of the CU switches in the sample, or of colonial breakups, but 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. Balancing the sample from 1970 even results in a negative coefficient for the impact of CUs on trade.

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.

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



(a) Impact of Euro (All Data as Control) (b) Impact of Euro (W.Europe as Control)

Fig. 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 that eventually joined the Euro vs. the rest of Europe, using Eq. 1 and all available data. Panel (b) plots the impact of the Euro over time while limiting the control group to Western Europe. Here the red line is for 1998, the last full year before the start of the Euro, which began on January 1, 1999.

This literature began with Rose (2000), who finds that CUs triple trade. This sounded suspiciously large to some, so subsequent research Nitsch (2002)) set out to dampen the effect.[4] 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.[5]

Nevertheless, doubts remained.[6] 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 UK 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.[7] Klein and Shambaugh (2006) find 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 UK-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. 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).[8]

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 (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))[9], 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).

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

[5] The result appeared robust enough that in 2005, Jeff Frankel (2005), p. 76, 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 Robert (2007) opted for geographic IV approaches, finding that CUs actually increase trade on a 14-fold and a 7-fold basis, respectively.

[6] 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%.

[7] 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). Lopez-Cordova and Meissner (2003) find mixed support.

[8] 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.

[9] 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. In complementary work following early drafts of this paper, Kopecky (2022) also finds that the Euro effect depends crucially on the control group.

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, 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 Glick and Rose (2016), with trade data from the IMF's *Direction of Trade Statistics* (DOTs) between 1948 and 2013. Data on regional trade agreements come from the World Trade Organization. CU classifications were taken by GR from the IMF—see GR (Glick and Rose (2016)) for details. GR's definition of a CU: we use is that "money was interchangeable between the two countries at a 1:1 par for an extended period of time.".

Table 1 compares the number of CU switches in GR (2002) versus GR (2016), for both the Euro and non-Euro observations. GR (2002) had a total of 136 CU switches, 108 of which came from exits. Only 66 of these switches remained, however, after excluding switches with (1) obvious major geopolitical events that 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.

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:

$$ln(X_{ijt}) = \alpha_{ijt}^{EnterCU_k} + \eta_{ijt}^{ExitCU_k} + \beta Z_{ijt} + \lambda_{it} + \psi_{jt} + \gamma_{ij} + \epsilon_{ijt}, \tag{1}$$

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 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}, directional country-pair FEs (note that each trading pair has a separate FE for imports and exports). 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 standard gravity equation with rich importer-year, exporter-year, and bilateral country-pair FEs, using one-way directional exports as the dependent variable:

$$ln(X_{ijt}) = \alpha CU_{ijt} + \beta Z_{ijt} + \lambda_{it} + \psi_{jt} + \gamma_{ij} + \epsilon_{ijt}, \tag{2}$$

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. Note that this is specification is standard in this literature. 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.)

Eq. 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.

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.

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.

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 for the Euro countries includes annual dummies for all exports within Western European country-pairs that are both EU members. 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 non-Euro EU members? 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 in the control group other nearby countries that did not join the eurozone. Thus, we ask, what is the impact of joining the eurozone for EE countries relative to EE countries that did not adopt the Euro? (The non-Euro EE sample contains 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 UK-colony*year FEs (thus, a separate dummy for each year interacted with whether a country-pair was ever in a UK-colonial relationship). 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 UK's trade with former colonies that never had a CU? This control is motivated by the observed decaying trend in trade between the UK and its former colonies over time, and the decline in trade between those colonies themselves.

(4) We also include a common-UK-colonizer*year interactive trend. For each former UK colony, we control for the trend in its trade with other former UK colonies. Trade intensity among this sample also decays over time, although less strongly than trade with the UK 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 country-pairs that had some form of colonial relationship but then experienced hostilities (see Table 1 for the full list). 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. Thus, this variable takes a value of one for the year when hostilities began between country-pairs with previous colonial relationships and each year thereafter. Why is this a necessary control? Fig. 2 shows an example where the standard approach can be misleading–trade between India and Pakistan dropped by 99.8% in 1965 following Operation Gibraltar and the resulting war. It is unlikely this trade collapse had much to do with the CU dissolution. Observers (e.g., Head et al. (2010)) have noted that the evolution of trade for countries with hostile versus amicable breakups varies greatly. (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.) This should be a relatively mild control, but it turns out to be influential.[10]

In addition, we already know from Campbell (2013) that the Glick and Rose (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. We also see what happens when we move to a strictly balanced panel.

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 (Campbell (2010)) for proofs, and Eichengreen and Irwin (1998) for empirical support). A "dynamic" form of a gravity model, an LDV model, is:

$$ln(X_{ijt}) = \sum_{k=1}^{K} \rho_k ln(X_{ij,t-k}) + \alpha CU_{ijt} + \beta Z_{ijt} + \lambda_{it} + \psi_{jt} + \gamma_{ij} + \epsilon_{ijt}, \tag{3}$$

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:

$$X_{ijt} = exp(\alpha CU_{ijt} + \beta Z_{ijt} + \lambda_{it} + \psi_{jt} + \gamma_{ij}) + v_{ijt}, \tag{4}$$

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 Section 2.2.2. Note that this specification was also used by Larch et al. (2019) and is also common in this literature.

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 UK, 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 synthetic 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.

[10] Following this paper, Chen and Novy (2022) also use this control.

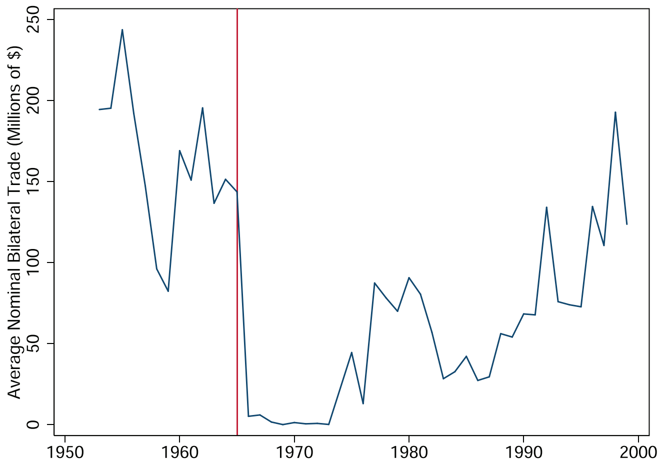


Fig. 2. Indo-Pakistani Trade: A Textbook Case of the CU Effect or Endogeneity?. Notes: This figure plots bilateral trade over time for India and Pakistan, one of the CUs in the GR (2016) data. These countries exited a shared CU in 1965, after which trade plummeted. However, this was the same year as the Indo-Pakistani War of 1965, which followed Operation Gibraltar, a secret Pakistani operation to infiltrate Jammu and Kashmir to foment rebellion against Indian rule (Prasad Prasad (2017)). Relations between these countries remain tense.

(3) match on both the first and last two years of trade in the pretreatment period in order to effectively match based on the growth of trade in the pre-treatment period.

(4) we run a regression of the log bilateral 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.[11] 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

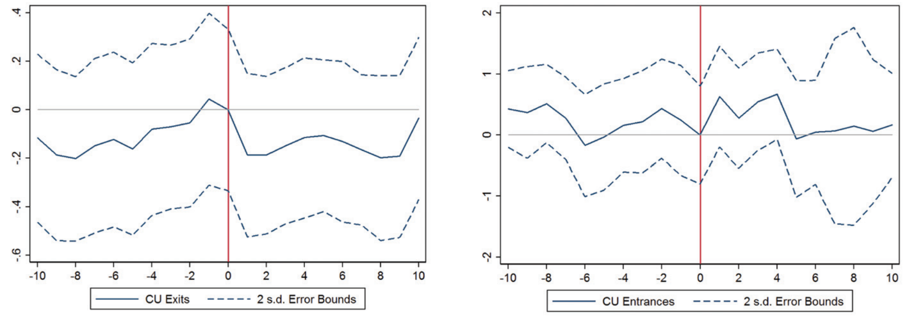
We implement regression (1) and plot the pre- and posttreatment trends for the full sample of CUs (before and after a CU exit) in Fig. 3, and also before and after a CU entrance for the non-EMU group. We find that trade did appear to fall about 20% after dissolution, but we also find that this amount 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 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

[11] 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

Fig. 3. Impact of CU Exits and Entrances. Notes: Panel (a) shows the evolution of trade before and after CU exits using Eq. 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.

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 Fig. 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 Fig. 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 Fig. 4(a) suggests it would be sensitive to a control for trends in the post-Soviet period.

Previously, in Fig. 1, we plotted the dummies from Eq. 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).

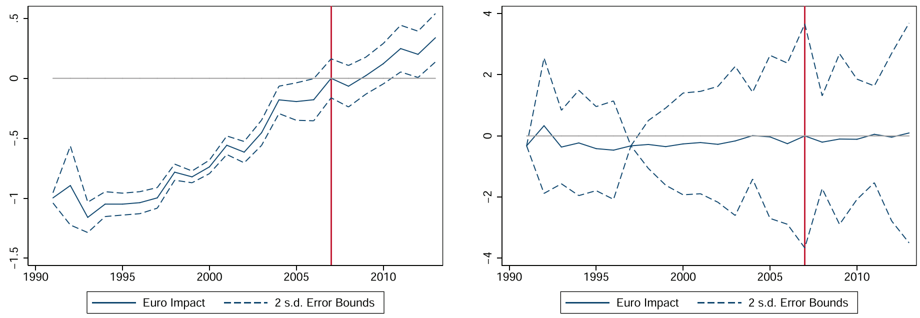
Another way to control for the EU would be to include a set of EU dummies for each separate year before and after entry, as countries joined the EU at different times. Comparing the results in Fig. 8, we find that they are not materially different (the upward pre-trend in trade among eurozone countries 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 Fig. 9).

3.1.3. The CFA Franc

We also plot pre- and posttreatment trends for the CFA franc (“*Colonies Françaises d’Afrique*”) in Fig. 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. What we propose is a simple time trend for this sample of countries.

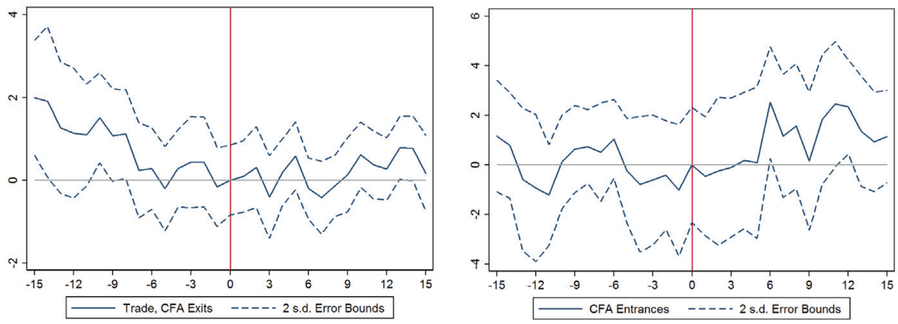
3.2. Main regression results

Next we turn to panel regressions in Table 3, where we estimate Eq. 2. In column (1), we benchmark the results from GR’s (2016) Table 5 (right panel). We confirm their estimated coefficient of 0.34 (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, in column (2), we benchmark the GR results for more disaggregated CUs. 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



(a) Full Sample as Control (b) Compared with Eastern Europe

Fig. 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 Eq. 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. (Note that two years of data in panel (b) were dropped due to orthogonal FEs, which here include exporter*year, importer*year, and country-pair FEs. The first b/c there was a lack of obvservations for Eastern European countries in the early 1990s, and the second as one year needed to be dropped randomly to serve as a baseline.)



(a) Exits (b) Entrances

Fig. 5. Impact of CFA exits and entrances. Notes: Panel (a) shows the evolution of trade before and after CFA Franc exits using Eq. 1. Panel (b) shows the evolution of trade before and after entrances.

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 results from 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 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. 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 affected 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 23, 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

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) Disagg.
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. Columns (4)–(7) include the controls from Section 2.2.2: (a) EU*Year FEs, (b) EE*Euro*Year FEs, (c) UK Colony*Year FEs, (d) Common UK Colony year trend, (e) CFA Exit*year trend, (f) hostile colonial breakup dummy (=1 for still a colony). $^*p < 0.1$,** $p < 0.05$, *** $p < 0.01$.

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).

Thus, in Table 4, we repeat our regressions in columns (1)–(6) of Table 3 using only balanced data from 1970, and we find that the apparent impact of CUs on trade falls further. Now, the EMU impact is down to .045 (with a standard error of.071), and the overall impact of CUs on trade is now −0.0071, implying that CUs have a slight negative impact on trade, with a standard error of 0.085. Thus, even the sign of the CU effect is not even consistently positive.

3.3. Dynamic gravity regressions

Next, we move to our dynamic regressions in Table 5, using the LDV model presented in Eq. 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 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
How robust Is the CU Impact? Balanced sample from 1970 Only

	(1) GR Benchmark	(2) GR Benchmark	(3) Cluster	(4) +Controls	(5) More Agg.	(6) Agg.	(7) Missing
Currency Union	0.33***					−0.0071	
	(0.019)					(0.085)	
EMU		0.43***	0.43***	0.032	0.045		
		(0.018)	(0.062)	(0.070)	(0.071)		
CFA Franc		1.50***	1.50***	−0.54			
		(0.21)	(0.25)	(1.16)			
East Carribean CU		−0.56***	−0.56**	−0.66***			
		(0.16)	(0.25)	(0.25)			
Aussie		1.10***	1.10***	1.03***			
		(0.23)	(0.35)	(0.34)			
British Pound		0.42***	0.42**	0.083			
		(0.13)	(0.18)	(0.18)			
French Franc		−0.92***	−0.92**	−1.30***			
		(0.27)	(0.40)	(0.42)			
Indian Rupee		0	0	0			
		(.)	(.)	(.)			
US		−0.14**	−0.14	−0.14			
		(0.058)	(0.24)	(0.24)			
Other CUs		0.55***	0.55	0.29			
		(0.11)	(0.34)	(0.38)			
Non-EMU CUs					−0.077		
					(0.17)		
CU (No Missing)							−0.0071
							(0.085)
CU (Missing)							0
							(.)
Observations	258060	258060	258060	258060	258060	258060	258060

Notes: Standard errors here are clustered in only one dimension from the third column, on pairid. This sample is perfectly balanced, as all the country-pairs here have complete data from 1970. 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) UK Colony*Year FEs, (d) Common UK Colony year trend, (e) CFA Exit*year trend, (f) hostile colonial breakup dummy (=1 for still a colony). $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5
Dynamic models.

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(X)	ln Δ X	ln Δ X (+Controls)	ln(X)	ln(X) (+Controls)	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 6, 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. Note that the implied impact of a CU on trade in this case is already substantially different from the GR benchmark in the previous section. 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 6
PPML.

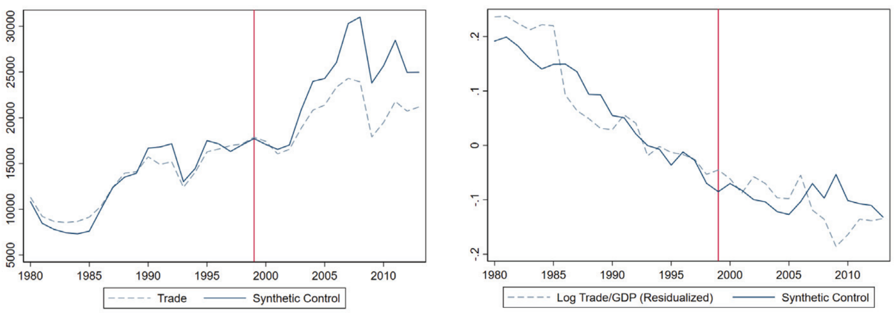
	(1) Benchmark	(2) Benchmark, Disagg.	(3) + Controls	(4) Agg.
Currency Union	0.13**			0.070
	(0.042)			(0.050)
EMU		0.027	−0.040	
		(0.041)	(0.052)	
CFA Franc		0.14	0.39	
		(0.34)	(0.28)	
East Carribean CU		−1.01***	−0.92***	
		(0.28)	(0.23)	
Aussie		0.17	0.18	
		(0.29)	(0.29)	
British Pound		1.00***	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)	
US		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) UK Colony*Year FEs, (d) Common UK Colony year trend, (e) CFA Exit*year trend, (f) hostile colonial breakup dummy (=1 for still a colony). $^{*}p < 0.1$,** $p < 0.05$, *** $p < 0.01$.

3.5. Synthetic control method

First, we replicate Saia's (2017) results in Fig. 6(a) and the first row of Table 7, which shows that had the UK adopted the Euro, its trade would have been much higher. We then present our alternative counterfactual in Fig. 6(b) and the second row of Table 7, and we find starkly different results. In this counterfactual, the UK'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 UK's trade with Japan, for example, would have been 19% higher had the UK adopted the Euro—curiously a larger estimated impact than the UK'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 would expect to 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 over-



(a) Saia Benchmark (b) Robustness

Fig. 6. Did the UK Miss Out? A Sythetic Control Approach. Notes: Panel (a) replicates Saia (2017). The dotted line represents actual UK 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.

Table 7
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 UK 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 UK joined the Euro, while in our counterfactual trade/GDP would have been about 0.5% higher.

all sign is theoretically uncertain. While it is possible that Saia's interpretation is correct, another plausible explanation is that British trade growth was generally sluggish after 1999 for reasons unrelated to the Euro. This latter explanation is also supported by our finding that the Euro, and currency unions in general, do not typically have a statistically significant impact on trade. In the Online Appendix we also 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.

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 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. Thus 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 interest 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? The larger estimates of CUs on trade seem particularly implausible given the Klein and Shambaugh (2006) result that indirect currency pegs—more likely to be random than direct pegs—are also uncorrelated with higher trade flows. This would seemingly remove the most plausible mechanism—exchange rate volatility—by which CUs are thought to increase trade.

We believe our results are reasonable, since intuitively it is hard to imagine that a 1:1 par value would really reduce trade costs by much or reduce exchange-rate volatility enough to increase trade, given that pegs at other par values do not seem to increase trade. Our findings can also help explain why different CUs appear to have had wildly different effects on trade: the results may simply be spurious.

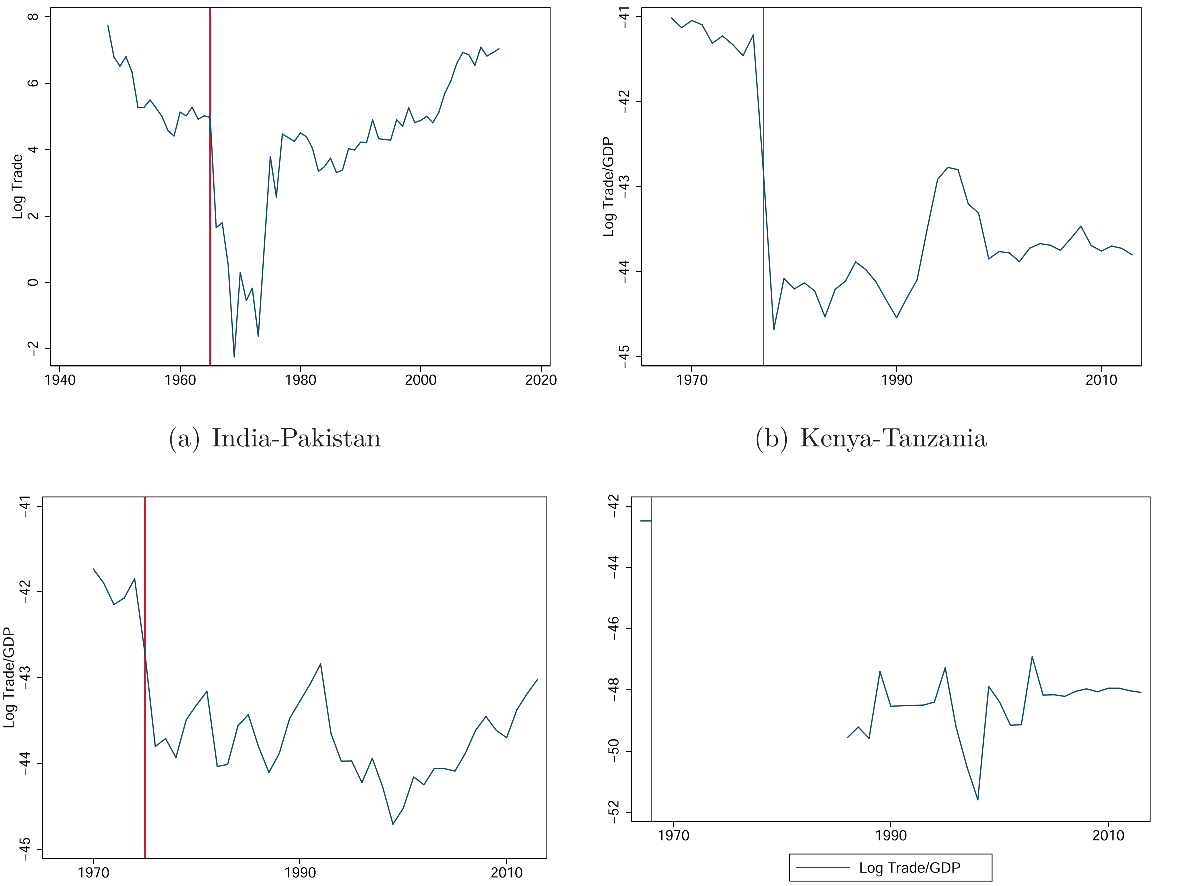
Finally, nothing in this analysis can rule out a positive (or negative) effect size of 1% to 2%, or even 10%, given that one estimated standard deviation from zero in our static regression corresponds to roughly a 6% increase in trade. The conclusion, therefore, is twofold: the previous large, highly significant estimates are fragile, and our best estimates are that CUs have a much smaller effect on trade.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Appendix

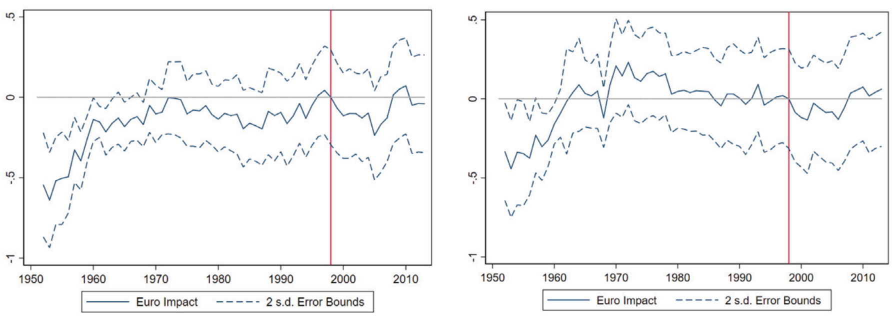
Figs. 7–9 and Table 1.



(a) India-Pakistan (b) Kenya-Tanzania

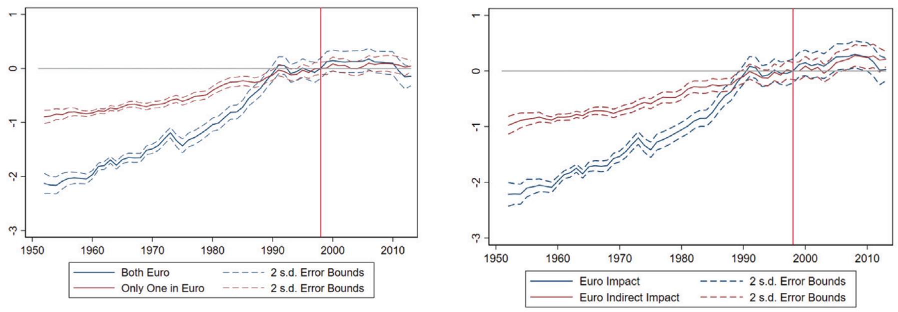
(c) Portugal-Angola (d) Guinea-Mauritania

Fig. 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

Fig. 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 Eq. 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) One in Western Europe as Control

(b) One in EU as Control

Fig. 9. 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.

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

Appendix B. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jimonfin.2023.102840.

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