Estimating the Effects of Fixed Exchange Rate Regimes on Trade: Evidence from the Formation of the Euro

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Abstract

The euro's formation provides a natural experiment to estimate fixed exchange rates' effects on trade. 31 countries historically fixed their currencies against the DM or FFr, and continued fixing against the euro since 1999, when these countries acquired fixed rates against the other eurozone members. Unlike typical changes in exchange rate regime, these were exogenous to trade, and are associated with significantly smaller effects on trade than the typical peg. Standard estimates may be inflated by countries' tendency to select into fixed exchange rate regimes with major trading partners.

Keywords: Exchange Rate Regimes, Gravity Model, Trade

1. Exchange Rate Regimes and Trade Flows

The choice of exchange rate regime is a perennial question of international macroeconomics. One argument for stabilising the exchange rate is that reducting volatility and risk might stimulate bilateral trade. Both the

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theoretical[1] and empirical evidence on the magnitude of these effects is mixed: empirical estimates of the direct impact of exchange rate volatility on trade are typically small; but many studies of currency unions and fixed exchange rate regimes find dramatically elevated trade flows. This paper exploits a natural experiment to estimate the causal impact of the exchange rate regime (ERR) on trade, and concludes that estimates from a standard gravity equation framework are biased up by the tendency of countries that stabilise their currencies to do so selectively vis-à-vis a major trading partner.

Many papers have built on the seminal paper of Rose (2000), which inferred the ERR's influence from its coefficient as a trade barrier in the gravity equation[2]. This approach consistently finds very large effects (typically that trade between countries sharing a common currency is on the order of three times higher than between those that do not), a finding widely described as the "Rose effect" in honour of its leading proponent[3].

This methodology typically finds a smaller effect for fixed exchange rates than for common currencies, but it is consistently both economically and statistically significant, on the order of 15%-40% higher trade between countries sharing a fixed exchange rate.

A similar methodology has been applied to estimate the impact of indi-

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[1]Bacchetta and van Wincoop (2000) show greater expected exchange rate volatility can increase trade, if demand from foreign export markets provides insurance against domestic labour market conditions.

[2]Variants of this specification have been used by Rose and van Wincoop (2001), Glick and Rose (2002), Frankel and Rose (2002), Micco et al. (2003), Klein and Shambaugh (2006), Baxter and Kouparitsas (2006), Adam and Cobham (2007), Bergin and Lin (2008), Lee and Shin (2010), and Eicher and Henn (2011), among others.

[3]Jeffrey Frankel's assessment is that 'Andrew Rose's 2000 paper, "One Money, One Market..." was perhaps the most influential international economics paper of the last ten years' (Frankel, 2008, p.2).

rectly fixed exchange rates on trade[4]. Klein and Shambaugh (2004) report negative, and in most specifications economically small, effects for indirectly fixed rates[5].

This literature relies on the assumption that countries adopt their ERR independently of their trade flows; but it is a reasonable presumption that typically countries do not randomly enter into a fixed ERR, but rather choose to peg against the currency of a major trading partner[6]. Frankel and Wei (1993) and Bayoumi and Eichengreen (1998) both conclude that in practice governments intervene selectively against the exchange rates of their principal trading partners[7]. This would naturally lead to a positive correlation between sharing a stabilised exchange rate and bilateral trade, reflecting a selection bias, even if the ERR itself has no effect on trade flows.

The creation of the euro in 1999 led to a reconfiguration of ERR arrangements that were unrelated to trends in their participants' underlying trade flows, and which are suitable for estimating the causal impact of a fixed rate on trade. Prior to the euro's formation, 18 countries had been pegging against the French Franc and 13 against the German DM, and they replaced their historical anchor currencies with the euro from 1999. By upgrading its peg against the FFr with a peg to the euro, a client country such as Cameroon simultaneously adopted fixed exchange rates against all of the new members

[4]Ie the impact of Thailand and Malaysia simultaneously pegging their currencies against a third country anchor, e.g. the US$, on Thai-Malay bilateral trade.

[5]Klein and Shambaugh (2004), Tables 2, 4, 6 and 7.

[6]Alesina and Barro (2002) analyse determinants of optimal currency unions, and the conditions under which higher trade flows increase the net benefits of monetary union.

[7]"When bilateral trade is relatively important ... governments will intervene on the foreign exchange market to stabilize it." (Bayoumi and Eichengreen, 1998, p.193).

of the eurozone[8].

These fixed ERRs between former clients of France and Germany and the other eurozone members were an incidental consequence of the formation of the euro and these clients' historical ERRs with their traditional anchors. Since these policy changes were independent of the adopting country-pairs' bilateral trade patterns, they allow clean estimation of their impact.

The future Eurozone members were also fixing their exchange rates against the DM before 1999, so the clients pegging to the FFR or DM were also indirectly pegged against them. This raises concerns that the impact of adopting direct pegs in this episode could be muted as the pre-existing indirect pegs might have elevated the baseline levels of bilateral trade.

Two possible approaches to address this concern are to directly control for the history of indirect pegs in the estimation; or to restrict the estimation to a sample excluding observations potentially contaminated by indirect pegs.

Two control groups are available for a difference-in-differences estimation: trade between a client and a country floating against the euro (e.g. Cameroon-Argentina); and trade between members of the eurozone and countries floating against the euro (e.g. Belgium-Argentina). Both groups of country-pairs experienced no change in their ERRs before or after the formation of the euro, and so can serve as control groups for the "treatment" (e.g. Cameroon-Belgium) group which adopted direct pegs after 1999.

The group of country-pairs that adopt directly fixed ERRs after 1999 were

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[8] The founding eurozone members were Austria, Belgium, Finland, France, Germany, Ireland, Italy, Luxembourg, the Netherlands, Portugal and Spain. Greece acceded on January 1st 2001.

indirectly pegged before 1999 if both countries were simultaneously participating in a system of fixed ERR arrangements. In fact, many of the clients and future eurozone members experienced episodes of floating in the years before the euro was adopted. Restricting the sample before 1999 to observations for which all country-pairs were freely floating against one another generates an appropriate set of floating pre-treatment observations, whose trade provides a natural benchmark against which to estimate the impact of transitioning from a float to a direct peg.

Either controlling for the presence of indirect pegs, or dropping indirectly pegged observations from the sample, there is no evidence of any trade-creating effect of adopting fixed exchange rates generated by this natural experiment.

These findings help to resolve the disconnect between estimates of the impact of the ERR, and analysis of the principal channel through which these effects are presumed to flow, exchange rate volatility. Estimates of the direct impact of nominal exchange rate volatility on trade flows are small[9]. Towards the upper end of these estimates, Rose (2000) finds that a one standard deviation reduction in volatility (0.07) would increase bilateral trade by 13%[10].

[9]See Hooper and Kohlhagen (1978); Baxter and Stockman (1989); Frankel and Wei (1993); Rose (2000); de Grauwe and Skudelny (2000); Broda and Romalis (2003) and Tenreyro (2007). Baldwin et al. (2005) provide a survey.

[10]Rose (2000), p.17. In the same regression he finds that eliminating volatility altogether by sharing a currency raises trade by 225%. This specification also faces identification issues if the volume of trade influences the volatility of the exchange rate, and several studies have concluded that OLS estimates of the effects of volatility on trade are biased in the direction of showing excessively large negative effects.

Frankel and Wei (1993) use the volatility of money supplies as an instrument for exchange rate volatility and conclude that OLS estimates are overstated by the endogeneity

One resolution to this tension proposed by Baldwin et al. (2005) is that volatility has convex effects on trade. Linear OLS specifications would then associate an ERR with a large kick to trade, as it is correlated with low volatility. However, the results in this paper can discount this explanation of large OLS estimates for fixed ERRs, as even allowing arbitrarily non-linear volatility effects to work through the ERR mechanism, there is no evidence of any causal effect on trade.

The episode around the formation of the euro also provides an opportunity to investigate the direct effect of volatility on trade. The adoption of the direct pegs significantly reduced bilateral volatility. There is no obvious alternative channel through which this change in ERR would impact bilateral trade, except through the reduction in volatility. The exogenous ERR changes arising out of the natural experiment provide ideal instruments with which to estimate the impact of exchange rate volatility, and they pass overidentification tests of validity.

IV estimates of volatility's effect differ significantly from OLS estimates,

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of government policy: "Such an interpetation [of OLS estimates of volatility on trade] is threatened, however, by the likelihood of simultaneity bias in the above regressions. Governments may choose deliberately to stabilize bilateral exchange rates with their major trading partners ... Hence, there could be a strong correlation between trade patterns and currency linkages even if exchange rate volatility does not depress trade." IV results suggest "that part of the apparent depressing effect of the volatility was indeed due to the simultaneity bias." (Frankel and Wei, 1993, pp.18-19).

Bayoumi and Eichengreen (1998) implement an alternative IV strategy and also find that trade dampens exchange rate volatility, and that governments intervene more in foreign exchange markets with larger trade flows, corroborating that OLS estimates of the effect of volatility on trade may be overstated. Broda and Romalis (2003) and Tenreyro (2007) also address this identification problem and do not find evidence of a substantial negative effect of volatility on trade. Probable positive biass of OLS volatility estimates deepens the disconnect with the results for ERRs.

and are very sensitive to the choice of instruments: ERRs that countries deliberately select into have a comparable impact on dampening exchange rate volatility, but imply very different results for the impact of volatility on trade.

However, overidentification tests strongly reject that the only channel through which these arrangements correlate with trade is their influence on bilateral volatility. This could be because there are alternative channels through which trade is impacted (eg a reduction in transaction costs for countries sharing a common currency), or because selection into a stable ERR arrangement with a major trading partner generates correlation between the ERR and trade that does not go through the volatility channel. For endogenous fixed ERRs, the absence of a plausible alternative mechanism through which the peg could influence trade, other than dampening volatility, is suggestive that selection is a significant source of the positive correlation between trade and the regime.

The selection effect is investigated by inverting the gravity equation, and regressing the ERR on trade flows, instrumented by the distance between country-pairs. These estimates suggest that larger underlying trade flows positively influence the propensity to avoid a floating bilateral ERR, and the magnitude of trade's influence on the adoption of different policies correlates strongly with the magnitude of gravity equation estimates of these ERRs' effects on trade[11], consistent with an important role for selection bias in the standard gravity equation. However, participation in the natural ex-

[11]Trade has a bigger influence on joining currency unions than fixed exchange rates, and none at all on participation in the natural experiment.

periment is completely uncorrelated with trade, supporting the paper's main identification assumption that these pegs were randomly adopted.

Previous approaches to control for potential selection bias have been inconclusive. Persson (2001) uses propensity score matching to control for differences between floating and common currencies, and his estimates are smaller than OLS, albeit imprecise.

Barro and Tenreyro (2007) and Lee and Shin (2010) use a similar instrumental variable strategy: they estimate the propensity of a country to adopt different anchor currencies from bilateral characteristics, and then calculate the implied probability that a country-pair adopts the currency of the same anchor, and so shares a common currency. Using this implied probability as an instrument yields even larger estimates for the ERR than OLS[12].

The natural experiment arising from the formation of the euro provides a unique opportunity for a clean estimate of the effect of a fixed ERR, based on simple identifying assumptions. The coefficients on fixed ERRs are tightly estimated compared to alternative estimation strategies, and there is no evidence of a positive effect on trade in any of the eight years of data in the sample.

[12]However, the validity of the instrument depends on the assumption that countries choose their anchor currency independently. Since members of the CFA Franc, for example, all choose to share a currency with each other as well as to simultaneously peg against the FFr, this assumption of independent anchor choice is open to question.

2. Empirical Specification

2.1. The Gravity Equation

This paper adopts the methodology of Baier and Bergstrand (2009) to estimate a gravity equation. Baier and Bergstrand develop a first-order Taylor approximation to the Anderson and van Wincoop (2003) system of nonlinear "multilateral resistance" (MR) equations, which control for general equilibrium effects and are straightforward to compute[13].

Following Anderson and van Wincoop (2003), the underlying structural gravity equation is

$$X_{ijt} = \frac{Y_{it}Y_{jt}}{Y_t^W}\left(\frac{\tau_{ijt}}{P_{it}P_{jt}}\right)^{1-\sigma} \tag{1}$$

where X_{ijt} are exports from country i to j, Y_{it} is i's GDP, Y_t^W is world GDP, P_{it} and P_{jt} are MR terms, and τ_{ijt} are bilateral trade barriers for period t. τ_{ijt} is assumed to take the following form

$$\tau_{ijt} = e^{\vec{\beta}\vec{D}_{ijt}} \tag{2}$$

where $\vec{D}_{ijt}$ is the vector of bilateral trade barriers. Taking the log of the gravity equation and assuming a multiplicative error term yields the following estimating equation

$$x_{ijt} = \alpha_t + \alpha_{Y_E} y_{it} + \alpha_{Y_I} y_{jt} + (1-\sigma)\vec{\beta}\vec{D}_{ijt} - (1-\sigma)p_{it} - (1-\sigma)p_{jt} + \epsilon_{ijt} \tag{3}$$

[13] Additional issues in gravity equation estimation, such as the heteroskedasticity bias described by Santos Silva and Tenreyro (2006) or the heterogeneity bias analysed by Helpman et al. (2008), are not addressed here. Both papers conclude that these biases tends to inflate coefficient estimates away from zero, which works against this paper's findings that fixed ERRs have no significant impact.

where lower-case variables indicate logs.

The MR terms p_{it} and p_{jt} are non-linear functions of all the trade barriers. Baier and Bergstrand (2009) solve for a first-order approximation of the non-linear MR in terms of all the underlying trade barriers

$$p_{it} = \sum_{j=1}^{N} \theta_{jt} \ln \tau_{ijt} - \frac{1}{2} \sum_{k=1}^{N} \sum_{m=1}^{N} \theta_{kt}\theta_{mt} \ln \tau_{kmt} \tag{4}$$

where θ_{it} is country i's share of world GDP, Y_{it}/Y_t^W. These MR terms can be rearranged in terms of each individual trade barrier, such as

$$\text{MRDIST}_{ijt} = \sum_{k=1}^{N} \theta_{kt} \ln \text{dist}_{ikt} + \sum_{m=1}^{N} \theta_{mt} \ln \text{dist}_{mjt} - \sum_{k=1}^{N} \sum_{m=1}^{N} \theta_{kt}\theta_{mt} \ln \text{dist}_{kmt} \tag{5}$$

in the case of distance[14]. Theory suggests that these MR corrections enter with an equal but oppositely signed coefficient to their corresponding trade barrier into the estimating equation. This restriction is easily implemented by adjusting the standard trade barrier for its MR correction, ie estimating β_{dist} from the variable log(dist) - MRDIST.

2.2. Data

The trade data is drawn from the UN's Comtrade database, for a panel of 180 countries from 1962 through 2006[15]. $ GDP is from the World Bank.

[14] As in Anderson and van Wincoop (2003), calculation of these MR terms requires data on internal trade barriers. Following them, internal distance is calculated as 1/4 of the distance to the nearest foreign capital. Substituting the relevant regressor, such as border or currency union, in for distance into equation (5) yields the correction terms for each of the other trade barriers.

[15] As discussed in Baranga (2009), this is a more complete trade dataset, including many (typically relatively small) trade flows that are missing from other datasets such as the

The trade barriers included are standard in the literature, and listed in Table 1. The index of religious similarity is calculated in Baranga (2009), as a Herfindahl index of different religious affiliations using data from Barrett et al. (2001).

Continuous	Discrete
log(Importer's GDP)	Common Border
log(Exporter's GDP)	Common Legal System
log(Distance)	Common Language
Religious Similarity	Colonial Relationship
	Either Country Landlocked
	Either Country an Island
	Common EU membership

Table 1: Standard Trade Barriers in the Gravity Equation

The classification of ERRs follows the de facto classification of Reinhart and Rogoff (2004) and Ilzetzki et al. (2008) (IRR)[16]. IRR classify bilateral ERRs every month from 1940-2007 based on the movements of market exchange rates. They divide arrangements into 14 categories, depending on the range of realised exchange rate movements and the stated policy of the governments involved. The 14 "fine" categories are collapsed into 5 "coarse" categories[17].

IMF's Direction of Trade Statistics. Differing reports between importers and exporters are reconciled by adopting the importer's report, following Feenstra et al. (1997) and Feenstra et al. (2005).

[16]Augmented by data from Global Financial Data's Global History of Currencies database on shared currency arrangements for some smaller countries not covered by Ilzetzki et al. (2008), available at https://www.globalfinancialdata.com/news/GHOC.aspx

[17]See Table V of (Reinhart and Rogoff, 2004, p.25). Coarse category 1 corresponds to the first four fine categories: (1) no separate legal tender, (2) preannounced peg or currency board arrangement, (3) preannounced horizontal band that is narrower than or equal to $\pm$ 2%, and (4) de facto peg. Coarse category 2 corresponds to fine categories 5-9:

This paper interprets IRR's coarse categories 1 and 2 as fixed exchange rate regimes, with the exception of fine category 1 (a subset of IRR's coarse category 1), countries sharing a common currency, which are treated separately. In robustness checks we will also distinguish between "hard" and "soft" pegs, corresponding to IRR's coarse categories 1 and 2[18].

regime type	frequency
common currency (non-€)	3383
euro member	1012
hard or soft peg treatment	5006
hard peg treatment	3571
soft peg treatment	1435
non-experimental fixed rate	13379
non-experimental hard peg	7023
non-experimental soft peg	6356
indirectly fixed	113290
indirectly fixed by 2 hard pegs	36448
indirectly fixed by a soft peg	76842
freely floating	410926
total observations	546996

Table 2: Summary Statistics of Exchange Rate Regimes

In moving from a monthly to an annual classification countries are coded as sharing a currency or fixed rate only if the arrangement holds for all 12 months of the year. The paper's results are also robust to a looser definition of regimes, which codes them applying for a year in which they held in any month. Table 2 gives the frequency of the different regime types in the data.

(5) preannounced crawling peg, (6) preannounced crawling band that is narrower than or equal to ± 2%, (7) de facto crawling peg, (8) de facto crawling band that is narrower than or equal to ± 2%, and (9) preannounced crawling band that is wider than ± 2%.

[18]Ie "hard" pegs correspond to IRR fine categories 2-4, and "soft" pegs to IRR fine categories 5-9.

In the section of the paper exploring the role of exchange rate volatility as the mechanism through which the ERR acts, nominal exchange rate volatility is measured as the standard deviation of the monthly bilateral nominal exchange rate over the calendar year, derived from the IFS' series of monthly exchange rates[19].

2.2.1. A Set of Exogenous Exchange Rate Regime Changes

On the eve of the formation of the euro in January 1999, 31 countries had already been stabilising their currency against one of the euro's prospective members (either France or Germany) for some time. These "client" countries maintained the same relationship against the euro, from its adoption through the end of the sample period (Dec 2006)[20], and so from 1999 adopted the same ERR with the other eurozone members as they had historically with their original anchor.

For the country-pairs involved, these changes in bilateral exchange rate policy were an accidental consequence of the euro's formation and the clients' historical relationships with France or Germany. Since trade with these clients had no influence on countries' decisions to join the euro (triggering the ERR changes), this episode constitutes a natural experiment. In contrast the typical ERR, such as the historical client-anchor ties described in Table 3, were actively selected into by the participants, possibly in part on the strength of their trade links.

[19] Missing data on some monthly exchange rates leads to a reduction in the sample from 546,996 to 532,097 observations for those specifications including measures of exchange rate volatility.

[20] According to the Ilzetzki et al. (2008) classification, except Slovenia, which moved from a de facto crawling 2% band to a hard peg in September 2001.

Client	Peg History & IRR Classification
French Clients	
Morocco	1962-73 IRR1; 1973-2006 IRR2
Benin	1962-2006 IRR1
Burkina Faso	1962-2006 IRR1
Cote d'Ivoire	1962-2006 IRR1
Niger	1962-2006 IRR1
Senegal	1962-2006 IRR1
Togo	1962-2006 IRR1
Cameroon	1962-2006 IRR1
Central African Rep	1962-2006 IRR1
Chad	1962-2006 IRR1
Congo	1962-2006 IRR1
Gabon	1962-2006 IRR1
Tunisia	1962-74 IRR1; 1974-2006 IRR2
Comoros	1962-71 IRR1; 1971-3 float; 1973-5 IRR1; 1975-80 ?; 1981-2006 IRR1
Equatorial Guinea	1962-79 CU/peg w/ peseta; 1979-84 IRR2; 1984-2006 IRR1
Mali	1962 IRR1; 1962-67 peg w/ $US; 1967-2006 IRR1
Algeria	1962-64 IRR1; 1964-95 float; 1995-2006 IRR2
Guinea-Bissau	1962-92 escudo/SDR; 1993-97 float; 1997-2006 IRR1
German Clients	
Denmark	1962-71 IRR1; 1971-98 IRR2; 1999-2006 IRR1
Hungary	1962-94 float; 1994-2006 IRR2
Cyprus	1962-72 peg w/ sterling; 1972-3 gold; 1973-92 IRR2; 1992-2006 IRR1
Switzerland	1962-73 peg w/$US; 1973-81 float; 1981-2006 IRR2
Iceland	1962-73 peg w/$US; 1973-86 float; 1986-2000 IRR2; 2000-06 float
Czech Rep	1962-90 ?; 1990-96 IRR2; 1996-97 float; 1997-98 IRR2; 1999-2001 IRR1; 2002-06 IRR2
Estonia	1962-90 ?; 1991-92 float; 1992-2006 IRR1
Slovakia	1962-90 ?; 1990-92 IRR2; 1993 float; 1993-97 IRR2; 1997-98 float; 1998-2006 IRR2
Slovenia	1962-91 ?; 1991-93 float; 1993-2001 IRR2; 2001-2006 IRR1
Croatia	1962-93 ?; 1993-94 float; 1994-2006 IRR2
Bosnia	1962-94 ?; 1994-2006 IRR1
Macedonia	1962-92 ?; 1993-94 float; 1995-2000 IRR2; 2001-06 IRR1
Bulgaria	1962-90 ?; 1990-96 float; 1997-2006 IRR1

Table 3: Exchange Rate History of Clients of the FFr and DM, 1962-2006

Table 3 describes the historical client ERRs and their duration, as classified by Ilzetzki et al. (2008). “IRR1” denotes that the client had a hard peg to its anchor, “IRR2” that the client had a soft peg. “?” indicates that the nature of the historical exchange rate regime is unknown. Clients’ arrangements with third countries are also noted. “Float” denotes an IRR coarse classification of 3, 4 or 5.

There is heterogeneity in both the duration of the ERR and its formality, with clients of the FFr tending to be both longer-established and to favour a more formal peg. French clients are all former African colonies, with the exception of Equatorial Guinea and Guinea-Bissau, while German clients are all European. The large majority of both sets of clients established their ERR well before the decision to form the euro was taken[21], so anticipation of the euro’s creation is unlikely to have been a factor motivating the clients’ initial pegs.

From 1999, each of the 31 original clients acquired a newly fixed ERR with the 10 eurozone countries to which it had not historically pegged (augmented by Greece’s entry into the euro in 2001), generating 341 exogenously adopted fixed ERR pairs, of which about 70% were hard fixes (see Table 2).

[21]The first official proposal to create a single European currency was the Werner Report of 1970, but the idea did not become a practical possibility until ratification of the Maastricht Treaty in November 1993, which laid out a framework for currency unification. The decision to proceed with the euro was only taken in May 1998, when the European Council abrogated the findings of an excessive deficit in Belgium, Germany, Spain, France, Italy, Austria, Portugal, Sweden and the UK, and announced that the 11 founding countries satisfied the conditions for membership. See EU Bulletin 5, 1998, point 1.2.2., and European Commission (1998).

3. Empirical Results

3.1. Estimates from the Natural Experiment

3.1.1. Exogenous Treatment Effect

Column (1) of Table 4 reports OLS estimates of equation (3), including importer and exporter dummies to control for MR terms[22]. Column (1) contrasts estimates for the fixed rates adopted as an accidental consequence of the adoption of the euro (the "treatment") with the non-random ERRs, using the sample of all available data from 1962-2006. The potentially endogenous ERRs are associated with significantly higher trade, as in the previous literature, but this effect disappears for the pegs arising out of the natural experiment[23]. Estimates for sharing a currency or fixed exchange rate are very similar to the previous literature[24], as are the coefficients on the stan-

[22]Anderson and van Wincoop (2003) note that a full set of importer- and exporter-year dummies controls consistently for MR effects. Including time-invariant importer and exporter dummies is a widely-used approximation, including by Rose (2000), appropriate if the MR terms are not time-varying, which greatly reduces the computational burden in a large panel. Subsequent regressions control parametrically for MR, using Baier and Bergstrand's technique, which allows for calculation of comparative statics that reflect endogenous adjustments of MR as trade barriers change.

[23]These results differ significantly from the findings of Frankel (2008), which looks at the change in trade between CFA Franc members and the eurozone after 1999, and reports a treatment effect of 0.572 (s.e. 0.119), Table 7B. Comparison of the results is complicated by the use of different datasets: Frankel's trade data runs from 1948-2006, but contains fewer observations, even allowing for the fact that that his dependent variable is a country-pair's total bilateral trade, rather than unidirectional exports. The key difference appears to be in the specification: Frankel does not include distance, importer or exporter fixed effects in his gravity equation. Omitting distance and dummies, I find a treatment effect for CFA-euro trade of 0.117 (s.e. 0.05) in my sample; including distance this falls to -0.12 (s.e. 0.047), so Frankel's results may reflect an unconventional specification of the gravity equation.

[24]Papers applying Rose's methodology to explore the impact of the euro on trade have uniformly found much smaller effects than for other currency unions. See Micco et al. (2003), Frankel (2008), or Eicher and Henn (2011) for examples. Reflecting these findings,

Sample:	All Countries' Trade	Eurozone or Clients' Trade Only	Treatment & Control Groups I & II
treatment	-0.0841	0.00796	-0.190*
	(0.0618)	(0.0648)	(0.0883)
common currency (non-€)	1.719**	2.190**	
	(0.149)	(0.201)	
€	0.207	0.140	
	(0.129)	(0.109)	
endogenous fixed rate	0.359**	0.282*	
	(0.0699)	(0.130)	
indirect fixed rate	0.113**	0.0662*	
	(0.0214)	(0.0329)	
log(distance)	-1.371**	-0.962**	-1.033**
	(0.0221)	(0.0447)	(0.0562)
border	0.422**	0.360*	0.785**
	(0.112)	(0.178)	(0.202)
island	-0.614**	-0.224*	-0.342**
	(0.0661)	(0.108)	(0.108)
landlock	-0.404**	-0.173*	-0.210**
	(0.0759)	(0.0813)	(0.0808)
language	0.388**	0.439**	0.354**
	(0.0421)	(0.0602)	(0.0622)
colonial	0.750**	0.676**	0.667**
	(0.0935)	(0.112)	(0.117)
legal	0.347**	0.491**	0.354**
	(0.0290)	(0.0427)	(0.0420)
religion	0.474**	0.502**	0.595**
	(0.0611)	(0.0770)	(0.0795)
EU	-1.195**	-0.658**	-0.468**
	(0.128)	(0.113)	(0.143)
log(GDP_I)	0.544**	0.630**	0.614**
	(0.0187)	(0.0269)	(0.0306)
log(GDP_E)	0.709**	0.811**	0.755**
	(0.0203)	(0.0302)	(0.0347)
Observations	546,996	268,839	210,623
R^2	0.700	0.751	0.740

Standard errors clustered by country-pair: ** p<0.01, * p<0.05

Year, Importer and Exporter dummies not reported

Table 4: Treatment Effect without MR Correction

dard gravity variables in the lower half of the table[25]; but one can easily reject that the treatment fixed rates have a similar effect to the endogenous fixed rates, and the standard error is estimated quite precisely.

Column (2) narrows the sample by dropping observations that do not involve either one of the client or Eurozone countries. This still includes observation with endogenous ERRs[26]. The results focusing on this narrower sample of European and client trade are very similar to those from a global sample.

The estimates in columns (1) and (2) show that randomly adopting a fixed rate after 1999 had no discernible effect on trade, was statistically indistinguishable from zero and less than the estimate associated with the endogenous fixed rates. These estimates control for the fact that baseline trade among the treatment group before 1999 was potentially higher than for a typical bilaterally floating country-pair (as the "treated" countries were indirectly stabilising their exchange rates by participating in a fixed exchange rate system centered on the German DM) by including a control for indirect pegging directly in the specification[27].

this paper distinguishes the euro from other common currencies, and also finds relatively modest results for euro membership.

[25]These all have the signs one would intuitively expect, with the exception of a strong negative correlation between trade and common membership of the European Union. This is quite a common finding in similar specifications. See Eicher and Henn (2011), p.426, for a discussion of this result and review of papers with similar findings.

[26]Most of the French clients participate in the two CFA Franc common currencies; examples of endogenous fixed rates in this sample include between the French and German anchors and their clients, as well as future Eurozone members to the DM. The effects of indirectly fixed rates are identified by countries moving into and out of a network of fixed ERRs centered on Germany.

[27]Specifications dropping the indirect fixed rate variable are associated with more negative estimates for the treatment effect, consistent with the finding that baseline trade

An alternative strategy to exploit the natural experiment restricts the pre-treatment baseline observations for the treatment group to only those years in which their currencies were freely floating against one another. This involves dropping all observations for which the country-pairs were sharing a currency, or directly or indirectly fixed against one another, except for the treatment pegs adopted after 1999[28]. Using this sample one can no longer estimate the effects of the endogenous arrangements, but the sample allows for a clean comparison of the shift from floating to fixed rates, using a set of quasi-randomly adopted pegs.

The core sample of treated country-pairs is augmented by two control groups that help to pin down the baseline levels of each country's trade. Control Group I consists of trade between clients and countries in the rest of the world outside the Eurozone with bilaterally floating exchange rates. Control Group II consists of trade between Eurozone members and countries in the rest of the world outside the pegging client group with bilaterally floating exchange rates[29].

Column (3) estimates the treatment effect on this sample, which will also be used for a difference-in-differences estimation below. The other gravity variables retain broadly similar coefficients to the world sample. Estimating the treatment effect by comparing the treated country-pairs' trade to com-

levels pre-treatment were higher, reflecting the pre-existing indirect pegs.

[28]Since there were frequent episodes between 1962-1999 in which either the future Eurozone members or the client countries floated their currencies, even after dropping observations potentially affected by endogenous ERRs there is a reasonably large sample of observations of treated countries' trade: between 1962-1993 there are 3,504 bilaterally floating observations for treated country-pairs, and 5,006 treatment observations from 1999-2006.

[29]There are 116,292 observations in Control Group I and 85,669 in Control Group II.

parable bilaterally floating country-pairs, the treatment is associated with a fall in bilateral trade.

3.1.2. Correcting for Multilateral Resistance

As pointed out by Anderson and van Wincoop (2003), attempts to recover the coefficient of the *ceteris paribus* impact of a bilateral trade barrier from a cross-country gravity equation can be confounded by general equilibrium effects, working through price levels. FOB prices of producers based in markets isolated behind high average trade barriers will be lower in equilibrium, generating foreign demand despite high bilateral transport costs; and an exporter trying to sell into a market with high average import barriers will be less disadvantaged by high bilateral trade costs as their competitors from other countries will also be charging higher prices.

Through these general equilibrium effects, third country trade barriers, labelled Multilateral Resistance effects by Anderson and van Wincoop, are omitted variables, which could potentially bias estimates. While the effect can go either way, the typical direction of MR bias discussed in the literature is to inflate the magnitude of uncorrected estimates[30].

Table 5 repeats the analysis of Table 4, but applies Baier and Bergstrand's MR correction procedure. The MR correction has modest effects on the trade barriers: in line with previous findings, the coefficient on sharing a common currency or fixed exchange rate is slightly smaller, and the impact of bilateral

[30]For example, Anderson and van Wincoop (2003) found that McCallum (1995) overestimated the importance of international borders; and Rose and van Wincoop (2001) and Eicher and Henn (2011) that accounting for MR tempered the impact of sharing a common currency on trade.

Sample:	All Countries' Trade	Eurozone or Clients' Trade Only	Treatment & Control Groups I & II
treatment	0.0660	-0.0681	-0.257**
	(0.0681)	(0.0738)	(0.0932)
common currency (non-€)	1.606**	2.148**	
	(0.140)	(0.190)	
€	0.317*	-0.224	
	(0.126)	(0.127)	
endogenous fixed rate	0.160**	0.157*	
	(0.0478)	(0.0711)	
indirect fixed rate	0.0931**	0.0710	
	(0.0276)	(0.0400)	
log(distance)	-1.355**	-0.941**	-1.000**
	(0.0221)	(0.0439)	(0.0544)
border	0.420**	0.403*	0.790**
	(0.112)	(0.173)	(0.200)
island	-0.627**	-0.221*	-0.340**
	(0.0662)	(0.108)	(0.108)
landlock	-0.387**	-0.165*	-0.199*
	(0.0753)	(0.0805)	(0.0801)
language	0.379**	0.431**	0.349**
	(0.0421)	(0.0593)	(0.0619)
colonial	0.736**	0.688**	0.655**
	(0.0935)	(0.109)	(0.116)
legal	0.350**	0.497**	0.360**
	(0.0290)	(0.0425)	(0.0419)
religion	0.478**	0.504**	0.599**
	(0.0612)	(0.0765)	(0.0794)
EU	-0.957**	-0.721**	-0.289*
	(0.123)	(0.106)	(0.119)
$\log(GDP_I)$	0.639**	0.687**	0.653**
	(0.0190)	(0.0268)	(0.0310)
$\log(GDP_E)$	0.810**	0.871**	0.798**
	(0.0208)	(0.0307)	(0.0350)
Observations	546,996	268,839	210,623
R^2	0.699	0.750	0.739

Standard errors clustered by country-pair: ** p<0.01, * p<0.05

Year, Importer and Exporter dummies not reported

Table 5: Treatment Effect with MR Correction

distance is also slightly more muted. In contrast, the coefficient on the euro increases[31]. The treatment effect remains statistically insignificant from zero on the first two samples, and one can reject a positive effect using the sample that will serve for a formal difference-in-differences estimation.

3.1.3. Difference-in-Differences Estimates

The negative effect estimated in columns (3) of Tables 4 and 5 could be biased if trade ties between the country-pairs randomly treated were historically lower than average. Supplementing the specification wiith a control for the average level of the treatment group's trade leads to a difference-in-differences specification in which the treatment dummy cleanly picks up any change in trade as a result of adopting a fixed exchange rate.

Two sets of country-pairs with floating exchange rates can serve as control groups for a difference-in-differences estimation. Country-pairs in control group I consist of a country that pegs to the euro and another that

[31]Gravity equation coefficients estimate the *ceteris paribus* impact on bilateral trade of changes in a bilateral trade barrier: by how much would France and Germany's trade increase if they adopted a common currency, holding other trade barriers constant? By joining the €, France and Germany also simultaneously adopted a shared currency with 10 other countries. In general equilibrium, the simultaneous adoption of the euro by and with other trade partners leads to some French exports being diverted towards its other eurozone trade partners, and hence away from Germany. The simultaneous multilateral adoption of the euro by other trading partners is a source of omitted variable bias in the traditional gravity equation that dampens OLS estimates of the impact of a country-pair bilaterally adopting the currency.

A multilateral change in trade barriers leads to a smaller effect on bilateral trade than a bilateral change in trade barriers would have, and this must be controlled for to recover the true bilateral coefficient. Explicitly controlling for MR recovers a larger ERR effect for the euro, albeit still significantly lower than for other currency unions. This effect provides a partial answer to the question posed by Frankel (2008): "The Estimated Effects of the Euro on Trade: Why are they Below Historical Effects of Monetary Unions Among Smaller Countries?", as most of the studies finding a small coefficient do not control for MR.

floats against the eurozone (e.g. Cameroon-Brazil). Country-pairs in control group II include a country from the rest of the world that floats against the eurozone, and a eurozone member (e.g. Brazil-Belgium). Both sets provide benchmark levels of trade against which to assess the impact of Cameroon and Belgium shifting from a floating to a fixed exchange rate.

Control Group:	I & II	I	II
Traditional post-treatment	0.00243	0.0901	-0.0296
	(0.107)	(0.120)	(0.0853)
MR-corrected post-treatment	-0.0432	0.0966	0.0491
	(0.112)	(0.122)	(0.124)
Observations	210,623	124,954	94,331

Control Group I : peg's trade partners in RoW
Control Group II : eurozone trade partners in RoW
Standard errors clustered by country-pair: ** $p<0.01$, * $p<0.05$
Standard trade barriers listed in Table 1 unreported
Treatment group, Year, Importer and Exporter dummies not reported

Table 6: Difference-in-Differences Estimates using Control Groups I and II

Table 6 reports difference-in-differences estimates of the effects on bilateral trade of adopting a peg in the natural experiment, using groups I and II as controls[32]. The first row of Table 6 reports OLS estimates with no correction for MR, and the second row applies the Baier-Bergstrand correction.

[32]The standard trade barriers listed in Table 1 are included and have similar coefficients to the benchmark regressions, but are not reported in the interests of space.

The difference-in-differences estimates are very similar across all three combinations of control groups, whether using the pegging countries' trade with the rest of the world, the eurozone's trade with the rest of the world, or both; and they are not sensitive to the MR correction either[33]. The coefficients are tightly estimated, although arbitrary serial correlation is allowed for by clustering standard errors by country-pair, per the critique of difference-in-differences estimates of Bertrand et al. (2004).

Comparing the diff-in-diff estimates in column (1) to those in column (3) of Tables 4 and 5, the significant negative coefficients reflect low historical trade ties between the treated country-pairs. Controlling for the average trade flows within the treatment group, difference-in-differences estimates of the change in trade associated with adopting a fixed rate is then found to be very close to zero. There is no evidence from this natural experiment that a fixed exchange rate significantly increases trade, either through a direct reduction in volatility or other unspecified channels. The results are robust to arbitrarily non-linear effects of volatility.

3.2. Robustness Checks

This section explores some potential concerns about the robustness of the estimation. Do positive treatment effects gradually emerge? Are the results sensitive to anticipation of the introduction of the euro? Is the simultaneous adoption of many new pegs and the euro generating substantial trade diversion that confounds the estimates? Has including soft as well as hard pegs

[33]It is unsurprising to find a small general equilibrium response to a trivial partial equilibrium effect.

obscured potentially larger treatment effects? None of these concerns appear to be warranted.

3.2.1. Dynamic Effects of Adopting a Peg

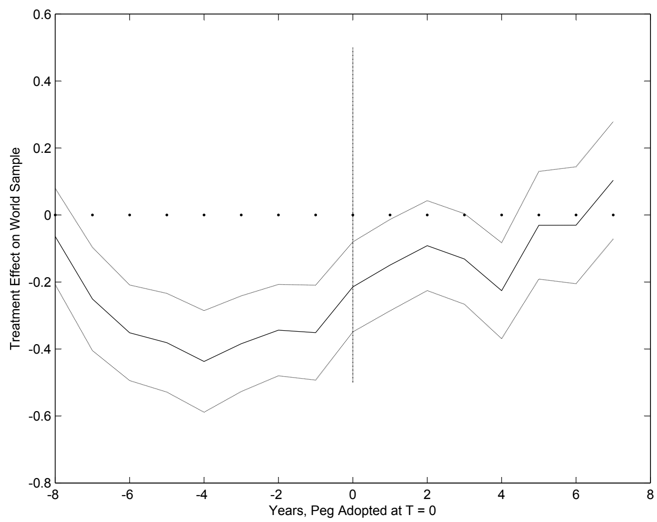

Figure 1: Dynamics of Treatment estimated on the full sample

The sample contains eight years of data since the introduction of the euro. If the impact of a fixed ERR grows gradually over time, the estimates above could be diluted by weak effects in the early years, masking a significant subsequent impact. To explore this, Figure 1 illustrates year-by-year estimates with their 95% confidence interval. Figure 1 was estimated as year-by-year pre-treatment and treatment effects for an eight year window on the sample of all countries' trade, otherwise using the same specification as in column (1) of Table 4[34].

[34]Ie including year, importer and exporter dummies, the standard gravity trade barriers and controlling for country-pairs' other exchange rate arrangements.

The estimates remain quite tight even when distinguishing the effects of the treatment after one year, two years, etc, and show no evidence of any significant positive effect of the treatment even after eight years. However, the pre-treatment effects are quite negative, troughing at four years before treatment is adopted, after which this negative effect appears subsequently to unwind steadily. A positive treatment effect could be obscured by very weak initial trade ties between the treated country-pairs, which are restored to a normal level by the anticipation of and then adoption of stabilised exchange rates.

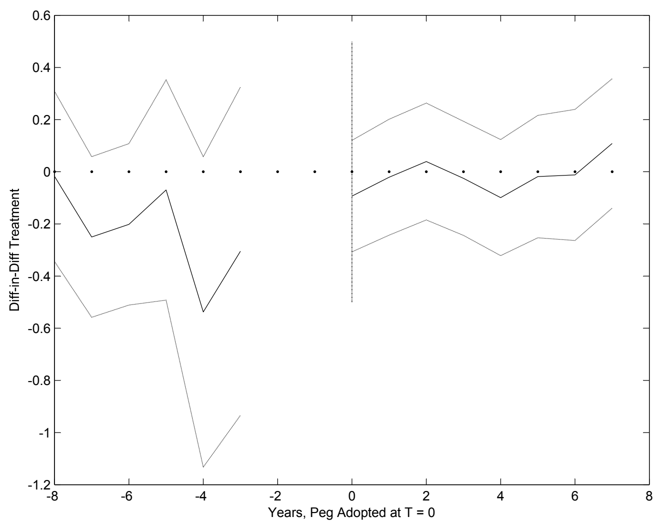

Figure 2: Dynamics of the Diff-in-Diff Estimates

To explore this possibility, Figure 2 illustrates year-by-year estimates on the difference-in-differences sample, in a specification otherwise similar to the pooled all-year treatment effect reported in row (1), column (1) of Table 6. Since this sample drops pre-treatment observations for which the treated country-pairs were indirectly pegged against one another, and the last episode of floating pre-treatment by a client ended in 1997 (for both Guinea-Bissau and the Czech Republic), there are no observations to obtain a completely clean estimate of the pre-treatment dummy one or two years before adopting the peg, as all the treated country-pairs were indirectly pegged one to two years in advance. However, year-by-year treatment effects, as well as pre-treatment effects for three or more years ahead can still be estimated on this sample.

Controlling for the baseline levels of trade among the treated country-pairs, there is no evidence that trade for the treated group dipped statistically significantly before adoption of the treatment, nor of any positive impact in the eight years of treatment. There are a sufficient number of observations of the first year of treatment, second year, etc, to estimate quite precise standard errors, and there is no indication of a positive effect in any year, or of an increasing trend over time.

3.2.2. Anticipation of the Introduction of the Euro

While the ratification and implementation of the Maastricht Treaty was politically uncertain, policymakers and exporters may have anticipated that the core euro members would adopt the currency before its launch in 1999. The estimates of dynamic pre-treatment effects in Figures 1 and 2 hint that trade among the treated country-pairs may have dropped in the years between the Maastricht Treaty and the introduction of the euro.

The expectation of a future single currency could affect identification through three channels: anticipation of the single currency could lead to an intensification of intra-eurozone trade at the expense of trade between clients and future eurozone members, depressing trade flows in advance and biasing treatment effects up; policymakers mught have foreseen that they could stabilise their currencies multilaterally by pegging to a euro member, so a country like Bulgaria may have chosen to peg to Germany in part because it also anticipated growing trade with Austria (which might also cause a positive selection bias); and firms that need to make fixed investments in order to export which rise in cost over time may be induced to start exporting in advance of an anticipated policy change that lowers trade costs, as in the model of Bergin and Lin (2012). These anticipation effects could bias estimates of the treatment down.

Sample:	D-in-D I & II	D-in-D I	D-in-D II
Traditional post-treatment	-0.112	0.211	-0.131
	(0.109)	(0.123)	(0.0988)
MR-corrected post-treatment	-0.268*	0.142	-0.102
	(0.117)	(0.128)	(0.166)
Observations	161,156	76,155	89,740

Control Group I : peg's trade partners in RoW
Control Group II : eurozone trade partners in RoW
Standard errors clustered by country-pair: ** $p<0.01$, * $p<0.05$
Standard trade barriers listed in Table 1 unreported
Treatment group, Year, Imp and Exp dummies unreported

Table 7: Restricting treatment to clients established before Maastricht

The first concern should be addressed by the difference-in-differences estimates, as trade diversion driven by anticipation of the euro should affect eurozone-rest of the world trade to a similar extent as client-eurozone trade, so that an intensification of trade within the eurozone at the expense of external partners should be differenced out by comparing eurozone-client trade to eurozone-rest of world trade. Figure 2 shows that the pre-treatment effects are no longer estimated to be statistically significant on this sample, although the standard errors are quite imprecise.

To address the second and third concerns, we can exclude from the sample treatment pegs adopted by clients after the Maastricht Treaty was signed in 1992. This removes any treatment pegs which could have been selected into in anticipation of the multilateral benefits of exchange rate stabilisation following the euro's implementation. It also means that all of the pre-implementation observations for treated country-pairs pre-date 1992, as post-Maastricht observations of established client relationships were indirectly pegged and so already excluded. This also avoids contamination by any anticipation effects by firms, unless these exceeded an eight year horizon.

Table 7 reports difference-in-differences estimates restricted to this sample: again there is no evidence of a significant positive impact, or that the benchmark estimates were biased down by anticipation effects. Column (1) presents estimates using both sets of trade partners as control groups. Dropping data from 1992-1998 the impact is actually more negative, suggesting that some of the late peggers[35] could have been motivated to peg to their initial anchor in anticipation of growing trade ties with other Eurozone economies.

Using only one set of trade partners as controls, the picture is more mixed.

[35] Algeria, Guinea-Bissau, Hungary, the Czech Republic, Estonia, Slovakia, Slovenia, Croatia, Bosnia, Macedonia and Bulgaria. See Table 3

Column (2) of Table 7 restricts the control sample to trade between the pegging clients and non-Eurozone members, and there is a modest increase in the coefficient, which is comparable to benchmark estimates for typical fixed exchange rates in Tables 4 and 5. However, the drop in the sample size means the standard error is quite large, and the effect is not statistically significant. This finding is also balanced out by a more negative coefficient when estimated using only Eurozone trade partners as controls for the diff-in-diff.

3.2.3. Trade Diversion

Trade diversion is another potential source of bias. Results from the natural experiment could be lower because many countries adopted fixed ERRs at the same time, leading to smaller than typical bilateral trade responses. For example, the rise in Cameroon's trade with Belgium could be atypically small because Cameroon simultaneously adopted a fixed ERR against Austria.

Baier and Bergstrand's MR methodology explicitly controls for these general equilibrium trade diverting effects. OLS and MR-corrected estimates are very similar, which suggests that there is very little multilateral trade diversion (consistent with the main effect being negligibly small)[36].

Another potential source of trade diversion arises from the simultaneous impact of the euro on trade of the eurozone members. The baseline estimates in Table 6 do not control for the introduction of the euro, which is not

[36]As will be discussed in more detail in section 4.1, the large adjustments to MR estimates for volatility, where we also know there are multilateral changes to trade barriers, gives additional confidence that this procedure would pick up on multilateral trade diversion in the natural experiment were it occurring.

included as a regressor as none of the eurozone country-pairs are in the control groups. However, the Baier-Bergstrand methodology can also be applied to control for this source of trade diversion, even on a sample that does not contain any members of the eurozone.

The MR terms associated with the introduction of the euro are non-zero for all country pairs, capturing the trade diversion that arises if the euro leads its members to trade more intensively with one another and thus less with the rest of the world. Including these terms allows estimation of the euro's impact on both intra- and extra-eurozone trade, inferred from the "shadow" it casts on trade with members' external partners.

treatment	-0.0639	-0.0690
	(0.117)	(0.117)
€	-0.0967	-0.105
	(0.184)	(0.186)
common currency (non-€)		1.597**
		(0.421)
endogenous fixed rate		-0.135
		(0.0901)
indirect fixed rate		0.119
		(0.113)
Observations	210,623	210,623

Standard errors clustered by country-pair: ** p<0.01, * p<0.05
Standard trade barriers listed in Table 1 unreported
Year, Importer and Exporter dummies not reported

Table 8: Treatment Effects Controlling for Trade Diversion by the euro

Column (1) of Table 8 repeats the difference-in-differences estimation of Table 6, using both control groups I and II, augmented by the MR variable associated with the euro's introduction. The estimate of the euro's effect on trade among eurozone members in column (1) is similar in magnitude to the direct estimates on the sample of Eurozone and client trade reported in column (2) of Table 5, even though the sample in Table 8 has excluded any observations between countries that directly share the euro. Table 8 estimates the impact only from the "shadow" cast by its implicit trade diversion, with no bilateral trade flows within the eurozone itself in the sample.

Column (2) of Table 8 explores further whether this is a reasonable technique, by further augmenting the regression with the MR terms associated with the other common currency arrangements, as well as directly and indirectly fixed exchange rates. By construction, there are no direct observations with these exchange rate arrangements in the sample either, so their effect is estimated just from the "shadow" cast by their potential trade diversion.

The impact of a common currency or indirect peg are very similar to the estimate one would find using the full sample in Tables 4 and 5, even though they are estimated without any direct observations of these regime types in the sample, which is a reassuring indication that this procedure is effective (although endogenous direct pegs seem somewhat different). Comparing the treatment estimates in Table 8 to the benchmark difference-in-differences specification in column (1) of Table 6, there is no evidence for any bias driven by trade diversion induced by the simultaneous introduction of the euro. The estimated treatment effect shades marginally more negative, and remains quite precisely estimated.

Another angle from which to approach this issue is to compare difference-in-differences estimates from only control group II with the benchmark estimates. Since Belgian-Argentine and Belgian-Cameroonian trade should be similarly affected by any trade diversion due to Belgium joining the euro, using only this control group for a difference-in-differences estimate should be unaffected by euro-induced trade diversion. Column (3) of Table 6 reports a very similar treatment effect to column (1), and is more negative than column (2), which used the client's other trade flows as benchmarks. If the estimates in column (2) were biased down by trade diversion induced by the euro's introduction, we would expect them to be more negative than the specification in column (3), but in fact the reverse is the case.

3.2.4. Is the Definition of a Fixed Rate Too Broad?

As detailed in Table 3, this paper adopted a broad definition of a fixed exchange rate, so it is possible that the results reported so far have diluted a true ERR effect by including ineffective pegs.

Table 9 reports estimates distinguishing hard and soft pegs in the natural experiment, as categorised by Reinhart and Rogoff into their coarse categories 1 and 2[37]. Column (1) of Table 9 repeats the specification of column (1) of Table 4, but distinguishes between soft and hard, and experimental and non-random pegs.

Hard pegs which countries have endogenously adopted are much more strongly correlated with trade than endogenous soft pegs, with the estimates bracketing the average effect for endogenous pegs reported in Table 4. The impact is slightly diluted once MR effects are controlled for, in column (2),

[37] As discussed in section 2.2, the hard peg excludes shared currencies from Reinhart and Rogoff's category 1.

Sample:	All Countries		Diff-in-Diff	
treatment (IRR1)	-0.0561	0.0988	-0.0255	-0.107
	(0.0710)	(0.0780)	(0.117)	(0.123)
treatment (IRR2)	-0.148	0.0354	0.0699	0.112
	(0.105)	(0.114)	(0.129)	(0.136)
common currency (non-€)	1.744**	1.639**		
	(0.149)	(0.141)		
€	0.196	0.336**		
	(0.128)	(0.126)		
endogenous fixed rate (IRR1)	0.512**	0.302**		
	(0.0888)	(0.0647)		
endogenous fixed rate (IRR2)	0.212*	0.0595		
	(0.0830)	(0.0507)		
indirect fixed rate (IRR1)	0.305**	0.328**		
	(0.0364)	(0.0492)		
indirect fixed rate (IRR2)	0.0462*	0.0261		
	(0.0225)	(0.0281)		
MR Correction	No	Yes	No	Yes
Observations	546,996	546,996	210,623	210,623

Standard errors clustered by country-pair: ** p<0.01, * p<0.05

Standard trade barriers listed in Table 1 unreported

Year, Importer and Exporter dummies not reported

Table 9: Treatment effects for hard and soft pegs

but hard pegs remain statistically and economically significantly positively correlated with trade, and the difference between hard and soft pegs is statistically significant whether adjusting for MR or not.

Distinguishing between countries indirectly pegged by two hard pegs (indirect IRR1) and countries indirectly pegged by at least one soft peg (IRR2), column (1) also indicates a signfiicant positive correlation for relatively tightly indirectly pegged currencies, with only a borderline effect for loosely indirectly pegged rates. These differences are also both statistically significant in both regressions.

However, the distinction between hard and soft pegs does not seem relevant to the treatment group. The coefficients on both forms of treatment are statistically insignificant in all four specifications. In the estimates on the full sample, the hard peg treatment has a more positive impact than the soft peg, but these estimates are not statistically significantly different from one another, or from zero. However, the hard treatment is statistically significantly less than the endogenous hard pegs in both columns (1) and (2); the soft treatment is also statistically significantly less than the soft endogenous peg in a traditional gravity equation, although not once the MR correction has been applied (both coefficients being close to 0).

Columns (3) and (4) report difference-in-difference estimates with the distinction between hard and soft treatments. The pattern of coefficients is reversed compared to the full sample, as the hard treatment is associated with smaller effects than the soft treatment whether correcting for MR or not. More importantly the estimates for both types of treatment remain statistically insignificantly different from zero in both regressions.

There is no evidence that the benchmark specifications failed to find a positive ERR effect by confounding proper fixed exchange rates with flimsier arrangements, and the standard errors remain reasonably tight even when distinguishing between the two regime types.

4. Exchange Rate Volatility and the ERR Effect

4.1. Do ERRs Raise Trade by Reducing Volatility?

An open question in this literature is how to reconcile the large positive correlations typically estimated between trade and sharing a currency or fixed exchange rate, and the modest negative effects associated with higher bilateral exchange rate volatility. The disconnect can be demonstrated by including a measure of exchange rate volatility as an additional control in the gravity equation.

Column (1) of Table 10 supplements the standard gravity variables listed in Table 1 with the volatility of the bilateral exchange rate, measured as the standard deviation of monthly exchange rates over the calendar year[38]. The coefficient estimated on volatility is statistically very significant but economically rather modest. The standard deviation of volatility in the sample is 0.18, so a one standard deviation increase in volatility would lead to an 2% drop in trade according to the estimate in column (1)[39].

By contrast, column (2) indicates that countries that have chosen to adopt

[38]This is the same measure used by Rose (2000) and Klein and Shambaugh (2006), and the estimate for volatility in column (2) lie inbetween their benchmark estimates: -0.017 (Rose, 2000, Table 2, p.16); -0.271 (Klein and Shambaugh, 2006, column 2, Table 2, p.368).

[39]$e^{-0.0202} = 0.98$

volatility	-0.112**	-0.0838**	-1.278**	-0.813**
	(0.0222)	(0.0221)	(0.186)	(0.168)
treatment (IRR1)		-0.0738		0.0669
		(0.0707)		(0.0779)
treatment (IRR2)		-0.155		-0.0108
		(0.105)		(0.114)
common currency (non-€)		1.752**		1.629**
		(0.149)		(0.141)
€		0.189		0.344**
		(0.128)		(0.126)
endogenous fixed rate (IRR1)		0.501**		0.283**
		(0.0892)		(0.0649)
endogenous fixed rate (IRR2)		0.195*		0.0505
		(0.0829)		(0.0510)
indirect fixed rate (IRR1)		0.306**		0.309**
		(0.0365)		(0.0493)
indirect fixed rate (IRR2)		0.0376		0.00517
		(0.0225)		(0.0281)
MR Correction	No	No	Yes	Yes
Observations	532,097	532,097	532,097	532,097

Standard errors clustered by country-pair: ** p<0.01, * p<0.05
Standard trade barriers listed in Table 1 unreported
Treatment group, Year, Importer and Exporter dummies not reported

Table 10: Is Exchange Rate Volatility the Mechanism Underpinning Positive ERR Effects?

a hard bilateral peg trade 65% more with one another[40]. Since volatility is included as a control in this specification, this is on top of any trade-creating effects delivered through the mechanism of reduced volatility. Comparing the estimates in column (2) of Table 10 to column (1) of Table 9, there is no significant reduction in the impact associated with sharing a currency, or a direct or indirect peg, taking into account the volatility channel. While common currencies allow for additional mechanisms than a reduction in volatility (such as greater ease of price comparison and a reduction in the cost of exchanging currencies), it is much harder to imagine alternative mechanisms through which direct or indirectly fixed exchange rates could be acting. The inclusion of volatility does not change the conclusion that the treatment pegs have no effect on trade.

Columns (3) and (4) apply the MR correction to the specification of columns (1) and (2). While this continues to have only modest effects on the estimates of exchange rate arrangements, it has quite a dramatic impact on the estimate for volatility. The estimate in column (3) implies a one standard deviation increase in bilateral volatility would be associated with a 20% decline in bilateral trade[41]. However, comparison of column (4) of

[40] $e^{0.501} = 1.65$

[41] This back-of-the-envelope calculation may be somewhat misleading, as taking the problem of multilateral resistance seriously, comparative statics cannot be calculated directly from the gravity equation coefficients.

The gravity equation attempts to estimate the ceteris paribus effect of increasing the volatility of a particular bilateral exchange rate; but in general equilibrium it is not possible for only one out of $n(n-1)/2$ exchange rates to move, holding the others constant. In the data, high volatility of one of a country's bilateral exchange rates will be positively correlated with high volatility in its other bilateral exchange rates; and in general equilibrium high multilateral volatility confounds the measurement of the effects of bilateral volatility, as trade with other country-pairs is simultaneously displaced.

Table 10 to column (2) of Table 9 shows that including volatility has very little effect on the estimates of the exchange rate regimes. Adjusting for MR leads to a larger coefficient on volatility, but does not affect the main point that volatility does not appear to be the mechanism driving the positive correlation between common currencies or fixed exchange rates and trade.

4.2. Identifying the Effect of Volatility on Trade

The theoretical effect of uncertainty on trade is ambiguous. Bacchetta and van Wincoop (2000) show that in the presence of price stickiness, exchange rate volatility can encourage firms to trade more, as exposure to foreign markets provides firms with valuable insurance against shocks to the cost of domestic labour. However, this effect can reverse depending on the substitutability between consumption and leisure; and in the presence of fixed costs of exporting, greater uncertainty could discourage market entry.

The negative coefficients estimated in Table 10 do not conclusively settle the theoretical question, as bilateral exchange rate volatility could also be endogenous in a gravity equation, either because a greater volume of transactions in goods markets provides a thicker and more liquid market for forex, or because policy is deliberately set to target greater stability in exchange

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To give an example, if the Argentine peso is volatile against the US$, it is likely that the peso-euro exchange rate is also volatile, and high peso-euro volatility will divert some exports that might have gone from Argentina to France towards, among other Argentine trade partners, the US, confounding and shrinking the estimated partial equilibrium effects of $-peso volatility on US-Argentine trade. MR correction is necessary to control for this omitted variables bias, and here leads to a larger estimated coefficient.

However, the comparative statics of an increase in a country's exchange rate volatility with all its trade partners, taking into account the change in multilateral resistance, would have a smaller effect than a purely bilateral increase in volatility, as measured by direct application of the gravity equation coefficient.

Sample:	All Countries		Diff-in-Diff	
Stage:	first	second	first	second
Dep Var:	volatility	log(trade)	volatility	log(trade)
volatility		9.188**		5.482*
		(3.164)		(2.328)
treatment (IRR1)	-0.0215**		-0.0418**	
	(0.00186)		(0.00286)	
treatment (IRR2)	-0.00930**		-0.0275**	
	(0.00295)		(0.00379)	
log(distance)	0.00273**	-1.436**	-0.00134	-1.016**
	(0.000376)	(0.0250)	(0.000805)	(0.0551)
border	-0.00326	0.500**	0.00106	0.768**
	(0.00212)	(0.119)	(0.00270)	(0.203)
island	0.00290**	-0.654**	0.00107	-0.333**
	(0.000846)	(0.0697)	(0.00145)	(0.110)
landlock	0.000657	-0.336**	0.000584	-0.165*
	(0.00205)	(0.0814)	(0.00219)	(0.0813)
language	-0.00415**	0.472**	-0.00213	0.366**
	(0.000759)	(0.0464)	(0.00110)	(0.0632)
colonial	-0.00177	0.787**	0.000131	0.671**
	(0.00127)	(0.0975)	(0.00145)	(0.117)
legal	-0.00179**	0.350**	-0.00242*	0.334**
	(0.000559)	(0.0306)	(0.000988)	(0.0430)
religion	0.00189	0.397**	0.000755	0.576**
	(0.00119)	(0.0640)	(0.00168)	(0.0808)
EU	-0.0127**	-1.045**	0.00966**	-0.529**
	(0.00170)	(0.136)	(0.00285)	(0.135)
$\log(\text{GDP}_I)$	-0.0349**	0.888**	-0.0381**	0.838**
	(0.00107)	(0.112)	(0.00192)	(0.0940)
$\log(\text{GDP}_E)$	-0.0372**	1.078**	-0.0412**	1.018**
	(0.00111)	(0.120)	(0.00194)	(0.102)
Observations	532,097	532,097	204,803	204,803
R^2	0.143	0.531	0.155	0.691
First-stage F	123.7		118	
Hansen's J χ^2		1.615		0.142
pval χ^2		0.204		0.706

Standard errors clustered by country-pair: ** p<0.01, * p<0.05

Year, Importer and Exporter dummies not reported

Table 11: Treatment Pegs as Instruments for Exchange Rate Volatility

rates between major trading partners.

The natural experiment provides an opportunity to explore this question further. The principal mechanism through which one would expect an exogenous shift to a fixed rate to affect trade is through a reduction in bilateral volatility, so the treatment can be used as an instrument to measure the impact of volatility. Distinguishing between hard and soft pegs provides two instruments with differential effects on volatility, allowing overidentification tests of the treatments' validity as instruments.

Table 11 presents GMM estimates of a gravity equation, instrumenting for volatility with the hard and soft treatments, on both the full and difference-in-differences samples. Columns (1) and (3) of Table 11 report the first-stage estimates. Both hard and soft treatments have a very statistically significant effect in reducing volatility. The mean volatility in the whole sample is 0.07, 0.073 on the diff-in-diff sample, so the hard treatment reduces volatility by about a third to a half. The coefficient approximately doubles moving from the full to diff-in-diff sample, which reflects the construction of the sample used for the diff-in-diff, which dropped indirectly pegged observations. The large reduction in volatility associated with the treatment confirms that the diff-in-diff sample is effective in contrasting floating and pegged observations. The standard gravity variables are also strongly correlated with bilateral volatility, so the F-statistic comfortably passes the Stock and Yogo (2005) weak instrument test.

Columns (2) and (4) present the second-stage estimates. The coefficients on the standard gravity variables are very comparable to the benchmark regressions in Table 4. However, the IV estimates for volatility are dramatically reversed. While the standard errors are quite large, on both samples volatility has a statistically significant positive effect. Table 11 reports Hansen's J χ^2 statistic of the overidentification restrictions, and the associated p-value. The treatments comfortably pass the overidentification tests on both samples.

These findings are consistent with the recent literature on the effect of exchange rate volatility. Broda and Romalis (2003) find that OLS estimates of the impact of volatility are negatively biased compared to their IV estimates. Tenreyro (2007) reports OLS estimates of -0.3 that flip sign to 1.63 as IV estimates, and using her preferred Poisson estimator, a coefficient of -0.388 that flips to 9.9 when instrumented[42]. One contribution relative to Tenreyro (2007) is that this set of instruments delivers considerably tighter standard errors.

It is informative to contrast the performance of the treatment with other exchange rate arrangements as potential instruments. Table 12 presents GMM regressions run on the whole sample, including the full range of exchange rate arrangements, and varying the exclusion restrictions.

Column (1) reports the first-stage, which is common to all the specifications in Table 12. All of the exchange rate arrangements have a significant effect in dampening bilateral volatility, and the treatments are of a comparable order of magnitude to the other arrangements, validating that directly controlling for the presence of indirect pegs, the treatment has a similar effect on volatility as endogenous fixed exchange rate regimes. The instruments also easily pass the weak instrument F-test in the first-stage.

[42]See Tables 2 and 3, pp.496 and 498, Tenreyro (2007).

Stage:	first	second	second	second
Dep Var:	volatility	log(trade)	log(trade)	log(trade)
volatility		2.540	-3.428**	-3.075**
		(1.850)	(1.266)	(0.541)
treatment (IRR1)	-0.0363**			
	(0.00174)			
treatment (IRR2)	-0.0223**			
	(0.00286)			
endogenous fixed rate (IRR1)	-0.0420**	0.611**		
	(0.00256)	(0.117)		
endogenous fixed rate (IRR2)	-0.0437**	0.310**		
	(0.00327)	(0.115)		
common currency (non-€)	-0.0413**	1.858**	1.642**	
	(0.00237)	(0.162)	(0.158)	
€	-0.0325**	0.280*	0.0802	
	(0.00278)	(0.135)	(0.133)	
indirect fixed rate (IRR1)	-0.0380**	0.404**	0.189**	
	(0.00103)	(0.0749)	(0.0571)	
indirect fixed rate (IRR2)	-0.0373**	0.137*	-0.0730	
	(0.000945)	(0.0674)	(0.0492)	
Observations	532,097	532,097	532,097	532,097
R^2	0.149	0.690	0.682	0.685
First-stage F	138.6			
Hansen's J χ^2		1.065	25.32	121.3
pval χ^2		0.302	1.32e-05	0

Standard errors clustered by country-pair: ** p<0.01, * p<0.05

Standard trade barriers unreported

Year, Importer and Exporter dummies not reported

Table 12: Endogenous ERRs as Instruments for Exchange Rate Volatility

Columns (2)-(4) vary the exclusion restrictions in the second-stage. Column (2) maintains the same identification asssumption as Table 11, that the treatment pegs only affect trade through their impact on volatility, while allowing the other regimes to work both through volatility and other unspecified channels. It is interesting to compare the results to columns (2) of Table 10 and 11. As in Table 10, the endogenous regimes are correlated with trade through a channel that is orthogonal to their effect on volatility, with very similar coefficients, while instrumenting with the treatment pegs the effect of volatility reverses sign compared to OLS estimates. The treatment instruments pass the overidentification tests at the 5% level.

Column (3) extends the exclusion restrictions to include the endogenously adopted fixed exchange rates. The sign of volatility's effect remains negative, but the overidentification tests are heavily rejected. Column (3) estimates the same regression treating all of the exchange rate regimes as excludable, and the overidentification tests are even more strongly violated.

The contrast between the results in columns (2) and (3) of Table 12 are very striking. The rejection of the overidentification restrictions in column (3) and the dramatic changes in sign on volatility's coefficient begs the question as to the critical difference between the treatment fixed exchange rates compared to the others. The key difference between the two set of pegs is that one came about accidentally, while the other was adopted as a deliberate policy decision.

The evidence presented here is consistent with the hypothesis that the choice of exchange rate anchor is dependent on the strength of the underlying trading relationship, as this is the obvious alternative channel for correlation between trade and the exchange rate regime that does not work through the volatility of the exchange rate. However, a positive correlation driven by the selection of exchange rate regime should not be interpreted as a causal impact of the choice of regime back onto trade.

5. Trade's Influence on the Choice of ERR

The impact of the treatment fixed ERR on trade is robustly much smaller than the typical fixed rate in the data. One mechanism that could account for this discrepancy is if countries which intervene in foreign exchange rate markets are more likely to target stability with respect to a major trading partner. This pattern would yield a positive association between bilateral trade and a managed exchange rate, inflating estimates of the regime's impact on trade flows.

An instrument for trade allows analysis of the influence of the volume of trade on the choice of ERR. Several of the traditional gravity model trade barriers, such as colonial ties or religious similarity, reflect cultural and political linkages which might directly influence the choice of currency anchor. But a geographic trade barrier, such as distance, plausibly affects this decision only through its influence on bilateral trade flows.

Table 13 presents probit regressions of the adoption of a common currency, endogenous fixed rate, and treatment fixed rate, instrumenting for trade with bilateral distance[43]. The instrument, log of bilateral distance, is

[43]Only countries that adopt a regime at some point can be included in the sample as the outcome is perfectly predicted by a country dummy for countries that never adopt the relevant regime.

Stage:	first	second	first	second	first	second
Dependent Variable	log(trade)	common currency	log(trade)	endogenous fixed rate	log(trade)	treatment fixed rate
log(trade)		0.965**		0.180**		0.0277
		(0.0280)		(0.00630)		(0.0239)
log(distance)	-1.426**		-1.406**		-1.550**	
	(0.0140)		(0.00474)		(0.0268)	
border	0.0629	0.0291	0.440**	-0.0523	-0.150*	-0.419**
	(0.0460)	(0.0675)	(0.0207)	(0.0335)	(0.0695)	(0.0875)
island	-1.121**	-0.907**	-0.569**	0.266**	0.320	-1.135**
	(0.0565)	(0.164)	(0.0170)	(0.0339)	(0.210)	(0.250)
landlock	-0.280**	1.331**	-0.327**	0.578**	-0.189**	-0.101
	(0.0648)	(0.117)	(0.0229)	(0.0940)	(0.0713)	(0.0852)
language	0.634**	-0.509**	0.427**	0.445**	0.779**	0.461**
	(0.0314)	(0.0864)	(0.0100)	(0.0210)	(0.0545)	(0.0701)
colonial	0.747**	1.181**	0.736**	0.785**	0.491**	-1.155**
	(0.0395)	(0.0894)	(0.0193)	(0.0228)	(0.0769)	(0.0785)
legal	0.367**	0.0308	0.324**	0.108**	1.030**	-0.710**
	(0.0273)	(0.0864)	(0.00748)	(0.0164)	(0.0438)	(0.0653)
religion	0.245**	-0.135	0.428**	-1.528**	0.363**	-0.758**
	(0.0513)	(0.137)	(0.0159)	(0.0476)	(0.0735)	(0.102)
EU	-0.970**	5.984**	-1.181**	1.328**	-0.135*	-3.695**
	(0.0461)	(0.273)	(0.0306)	(0.0329)	(0.0577)	(0.0710)
log(GDP_I)	0.367**	-0.0849	0.574**	0.269**	0.588**	0.530**
	(0.0250)	(0.0523)	(0.00887)	(0.0185)	(0.0764)	(0.153)
log(GDP_E)	0.793**	-0.488**	0.727**	0.236**	0.834**	0.608**
	(0.0261)	(0.0569)	(0.00906)	(0.0191)	(0.0810)	(0.158)
F-Statistic	1272.42		3067.12		693.91	
Observations	56,528	56,528	506,400	506,400	13,950	13,950

Year, Importer and Exporter dummies unreported. ** p<0.01, * p<0.05

Table 13: Inverted Gravity Equation: Influence of Trade on Regime Choice

very strongly correlated with log(trade) in the first stage for all three samples, and all three first-stages have very high F-statistics (as would be expected since the gravity variables do a good job predicting trade flows). The gravity variables have similar coefficients in the first-stage across the three samples used to estimate the propensity to adopt different ERRs.

The second stage estimates show that the propensity to adopt either a common currency or an endogenous fixed rate is strongly positively influenced by trade flows. The influence of trade is much stronger for common currencies than fixed rates, suggesting that OLS estimates of the effect of a common currency on trade may be particularly impacted by selection bias. These results correlate well with the pattern of OLS ERR effects presented in Tables 4 and 5, consistent with the possibility that standard specifications are inflated by endogenous selection. They are also consistent with the findings of Bayoumi and Eichengreen (1998), that the extent of governments' interventions in foreign exchange markets (as measured by the change in reserves) is positively influenced by larger trade flows.

In contrast, there is no such effect for the countries that happened to adopt fixed exchange rates as a result of the natural experiment, as reported in column 6 of Table 13. The coefficient on trade is statistically insignificantly different from zero, much smaller than for common currencies or endogenous fixed rates, and estimated with a small standard error. This supports the argument that the regime changes in the natural experiment are exogenous with respect to trade, and yield clean estimates of the true fixed ERR effect, while gravity equation estimates using endogenous regimes are positively inflated by selection bias.

6. Conclusions

This paper studies the natural experiment arising out of the creation of the eurozone, which led many countries to change their bilateral exhange rate relationships as an incidental consequence, rather than a deliberate act, of policy. Exploiting this natural experiment to estimate the impact of fixed ERRs on trade disentangles the influence of trade over exchange rate policy from these policies' effects back onto trade, as this set of exogenous policy switches yields clean estimates of the fixed ERR's causal impact.

The surprisingly large effects associated with a typical change in exchange rate regime cannot be explained by controlling for exchange rate volatility, the main channel through which an effect would be expected to flow; but they disappear when estimated on an appropriately exogenous sample.

The effects of adoption of directly fixed ERRs during the natural experiment are estimated with small standard errors, yet find no evidence of a significant positive impact on trade, in either treatment or difference-in-differences estimates, at any point over the eight years of data in the sample. These findings are robust to volatility having arbitrarily non-linear effects, and control for the possibility of trade diversion using both a structural gravity model and a difference-in-differences estimation strategy.

Insignificant impacts of adopting a fixed exchange rate are consistent with the finding that the causal effect of exchange rate volatility on trade may even be positive, as estimated by using the random adoption of the treatment pegs as an instrument for volatility's effect on trade. While this may not be in line with most readers' a priori expectations, there are theoretical mechanisms that could drive these results.

Consistent with the finding that when countries exogenously adopt fixed ERRs there is little effect on trade, this paper also presents evidence that a significant direction of causality in typical exchange rate regimes runs from trade flows to regime choice. Countries which manage their exchange rate choose to stabilise their currency vis-à-vis a major trade partner; and overidentification tests strongly reject the hypothesis that the only channel through which deliberately adopted fixed exchange rates affect trade is through their impact on exchange rate volatility. The absence of any other obvious channel of influence reinforce the hypothesis that trade flows influence policymakers' choice of currency anchor. This endogeneity leads to inflated estimates of ERRs' effects on trade in a standard gravity equation, and hence the difference between standard estimates and those from the natural experiment.

The paper's identifying assumption is supported by the estimates of the determinants of participation in the treatment group (in which trade flows were found not to be a factor), in contrast to the non-random ERRs, which appear to be strongly influenced by bilateral trade. The treatment pegs also pass the overidentification tests as instruments for volatility, suggesting that their influence on trade is limited to the volatility channel.

There is a disconnect between literature finding that exchange rate volatility has an economically small impact on trade, but that policies intended to dampen volatility stimulate trade very powerfully. The results of this paper suggest that indeed the effects of volatility are modest, but that standard estimates of exchange rate policy are inflated up by selection bias, and disappear when estimated with a suitably exogenous set of policy changes.

There are sensible arguments in favour of adopting an external anchor for monetary policymaking, from importing the credibility of a well-run anchor's disciplined monetary policy to promoting deeper political integration. Promotion of stronger trade links is also a frequently discussed motivation, and it is easy to generate estimates that appear to give this claim empirical support. This paper challenges that interpretation: large positive correlations between ERRs and trade appear to predominantly reflect selection bias, not a trade-creating effect of reducing exchange rate volatility.

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