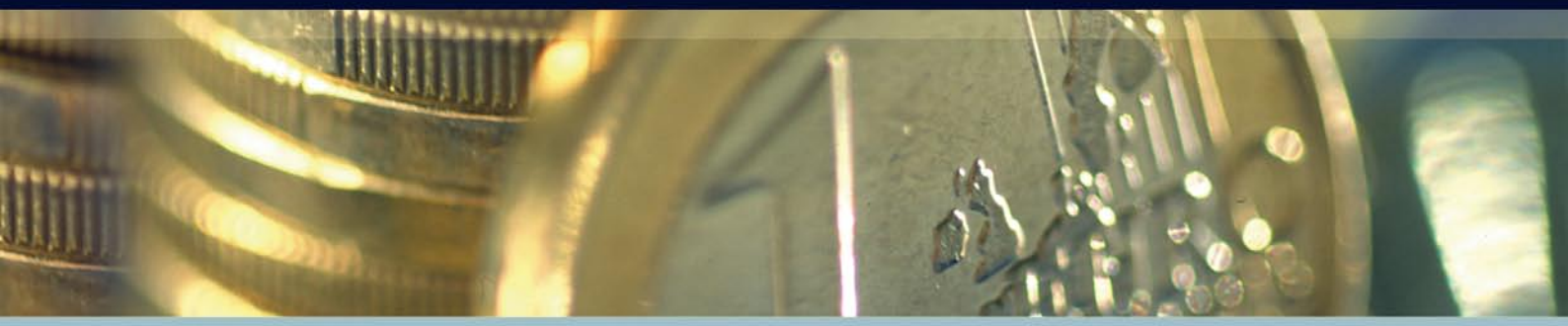
EUROPEAN ECONOMY

Economic Papers 321 | May 2008



Study on the Impact of the Euro on Trade and Foreign Direct Investment

Richard Baldwin, Virginia DiNino, Lionel Fontagné,
Roberto A. De Santis and Daria Taglioni

Economic and Financial Affairs
DIRECTORATE-GENERAL

EUROPEAN COMMISSION

EMU@10 Research

In May 2008, it will be ten years since the final decision to move to the third and final stage of Economic and Monetary Union (EMU), and the decision on which countries would be the first to introduce the euro. To mark this anniversary, the Commission is undertaking a strategic review of EMU. This paper constitutes part of the research that was either conducted or financed by the Commission as source material for the review.

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Directorate-General for Economic and Financial Affairs
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B-1049 Brussels
Belgium
E-mail: Ecfin-Info@ec.europa.eu

This paper exists in English only and can be downloaded from the website
http://ec.europa.eu/economy_finance/publications

A great deal of additional information is available on the Internet. It can be accessed through the Europa server (http://europa.eu)

ISBN 978-92-79-08246-7
doi: 10.2765/65274

STUDY ON THE IMPACT OF THE EURO ON TRADE AND FOREIGN DIRECT INVESTMENT

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April 2008

[*] The views expressed in this paper are solely the responsibility of the authors and should not be interpreted as reflecting the views of the ECB, the Bank of Italy or the European Commission.

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Executive summary

The introduction of the euro was an immense political and symbolic step towards an integrated Europe. It was also the world's largest economic 'experiment.' This experiment opens the door to a major advance in our understanding of how a common currency affects economic activity ranging from trade and foreign direct investment to wage-setting behaviour and corporate business strategies. A series of studies stretching back to the early years of the decade have begun to piece together a wide range of results. The resulting collage is still not fully coherent, this report has moved in that direction.

When it comes to the euro's trade effects the first contribution of the report is to refine "the number". Using the latest data and best empirical methodology, we confirm the received wisdom that the euro has promoted trade significantly, with the aggregate impact being in the range of 5% or so. Note that we have made great efforts to separate the euro's impact from the impact of other pro-integration policies that were also being implemented in the 1999-2006 period, notably the Single Market programmes. This effort has tended to shift down the aggregate number but it is necessary to be absolutely sure that it was the euro causing the effect.

The second main contribution of the report was to advance and refine our understanding of exact *how* the euro was boosting trade. Which economic channels were important and which were not. Logically, there are two main channels to consider, each with a number of sub-channels. The first is the relative price channel. Simply put, this argues that the euro boosted trade inside the Eurozone since it lowered the relative price of traded goods coming from the Eurozone. Since prices depend upon marginal cost and the price-cost mark-up, the lower relative price could come from two main sources – a reduction in bilateral trade costs among Eurozone nations (e.g. lower transaction costs, hedging costs, etc.), or an increase in competition that pushed down trade prices via a pro-competitive effect. The two sources are easily distinguished since to the extent that the lower trade costs were preferential – affecting only intra-Eurozone trade – it should have led to trade diversion. It did not, so we can be fairly confident that the transaction cost story is not of first-order importance. Direct evidence from pricing regressions suggests that the euro did indeed have a pro-competitive effect on exporters' prices, for both EZ and non-EZ based exporters. We also found evidence that outsiders were moving towards pricing-to-market strategies suggestive of their viewing the Eurozone as a single market. Indirectly evidence on this exact point comes from the heighten convergence of export prices within the Eurozone.

The second main channel – first posited by Baldwin and Taglioni (2004) and elaborated in Baldwin (2005, 2006) – is the newly-trade goods channel. The basic idea is that the euro induced firms to export a wider range of their products to the Eurozone. Thus, it was not merely that trade in existing products was stimulated; the pro-trade effect also came from newly trade goods. This report presents new evidence based on four firm-level data sets (2 from insiders and 2 from outsiders) that seems to confirm the new-goods hypothesis rather resoundingly. Moreover, this new evidence sheds important light on the lack of trade diversion. Earlier studies had suggested that the lack of trade diversion might have been caused by an expansion of the range of goods exported by the 'outs' to

the 'ins'. Firm-level trade data from Sweden and Hungary, however, shows that this did not happen. In the Swedish case the main rise in their exports to the Eurozone was due to the so-called intensive margin, namely the old-fashioned way of raising trade – increasing the average sales per product exported. Combining this evidence with our new pricing regressions suggest that the absence of trade diversion was due to the pro-competitive effect of the euro on outsider's export pricing strategy. That is, as the euro made the euro-using nations more like a single market, boosting pricing transparency and making third-party arbitrage safer, Eurozone import customers became more price sensitive and the outsider exporters responded by cutting price-cost margins. This lowered their relative prices and thus stimulated sales.

The report also looks at new evidence on this second channel that come from data that is more aggregate than the firm level data but still very disaggregated and available for all nations. The new evidence in this chapter confirms the firm-level findings.

The third main contribution of the report concerns the euro's pro-FDI effects. The empirical work on the FDI effects is much less rich than that on the trade effect. This is not due to a lack of interest; in the world of modern business, cross-border investment is an integral part of firms' international strategies – especially when it comes to large firms. Moreover, it is widely thought that FDI brings with it valuable foreign know-how that does come with just trade. The problem is that both the data and the empirical methodology are much less well developed.

Using the best available data, theory and econometric techniques, the report concludes that both Single Market integration and and euro area membership have pro-FDI effects. The key points were:

- The euro's pro-FDI effect was much larger in manufacturing versus services. It is likely that the level of protection and barriers to entry in the service sector act as a strong deterrent to cross-border M&As in services across countries. To the extent that the new 2006 services directive breaks down such barriers, it may trigger a new wave of cross border M&As within the EU.
- The euro's pro-FDI effect was much larger for deals within sectors as opposed to across sectors. Thus the euro facilitated cross-border M&As within the euro area, which aimed at restructuring capital within the same sector of activity, rather then boosting the formation of conglomerate activities between sectors.
- The euro fostered domestic and cross-border M&A activity by both large and small firms, but its effect on small firms was biased towards cross-border activity.
- The euro's adoption promoted FDI from outside the euro area, but this effect was only about half as strong as the impact within the area.
- The 'bottomline' number – the overall pro-FDI effect – is not clear. Most authors find it is positive, but the estimates range from +15% for in-to-in flows and +7.5% for out-to-in, to +200% and +100%, respectively for the in-to-in and out-to-in flows.

The empirical work on FDI must be subject to an important caveat concerning data quality. Most of the investigation was conducted on FDI data gathered for capital account statistics. This data deals with financial flows which may or may not be good proxies for real investment activities such production and employment. Indeed, for the one nation where we did have access to firm-level data on FDI measures that include employment and sales, the correlation with the capital account data is not very good and deviates significantly exactly aournd the euro's introduction. Moreover, the FDI data is dominated by M&A activity which is clearly influenced by financial market trends such as the stock market booms and tax avoidence. For example, about 70% of all intra-Eurozone FDI comes from or to Luxembourg – a fact that surely reflects the Grand Dutchy tax and transparency advantage as concerns M&A activity.

Chapter 1: Introduction

The closing years of the second millennium witnessed a strange occurrence in Europe. Three hundred million Europeans abandoned their familiar francs, marks, guilders and shillings to embrace a newly fabricated currency called the euro. As part of the exercise, they removed responsibility for monetary policy – the main macroeconomic stabilisation tool in the modern economy – from the hands of their familiar Central Banks and turned it over to an entirely untested, entirely unfamiliar and newly fabricated central bank called the European Central Bank.

This was a bold move. No set of nations had ever tried anything like it on this scale at least not peacefully. Of course, there have been monetary unions formed in living memory – the Belgian-Luxembourg union for example – but never on this scale; the new single monetary area accounts for about 15% of world income.

Europe's move to a common currency was part of a larger strategy – the Economic and Monetary Union (EMU) – introduced by the Treaty on European Union, known commonly as the Maastricht Treaty. Not all EU members have wanted to or been able to participate in the monetary part of this; all 27 EU nations are members of EMU, but only a subset are inside the monetary and currency union – the Eurozone, or the 'euro area' as the ECB calls its. The fact that monetary union was pursued by only some EU members but economic union was pursed by all bedevils empirical efforts to isolate the microeconomic impact of the euro. A series of studies stretching back to the early years of the current century have suggested a wide range of results. The resulting collage is still not very coherent since no one has put in the effort to do a root-and-stem stocktaking. To a large extent, the fragmented nature of the literature stems from the lack of data. Each year, studies using another year's worth of data emerged, making systematic comparison difficult.

The goal of this report is make a major advance in our understanding of the euro's impact on trade and foreign direct investment. There are two key elements to this effort. The first consolidates what we know about the euro's trade and investment effects, using the latest data and empirical techniques. The second pushes out the frontiers of our knowledge by investigating the precise channels thorough which the euro is affecting trade and investment – the microeconomics of the euro-trade effect. The time is ripe for this endeavour. We now have enough data – both at the aggregate level and sector and firm-level – to go well beyond generalities and conjecture; to really figure out what happened and why.

Why the microeconomics matters. The euro was created for political reasons. Economics – especially the trade and investment effects – were a minor issue in the minds of the men and women who launched Europe's monetary union. Of course, they were discussed in the Commission's famous "One Market, One Money" report, but that came after the big political push organised by Helmut Kohl, Francois Mitterrand and Jacques Delors.

Going forward, however, economic is all that really matters. Politics will incessantly swirl around the European monetary union, however the big decisions are fait accompli. Economics are what matter most for Eurozone enlargement and smooth functioning of the area. The microeconomics

especially concerns two distinct groups of policy makers: EU member state policy makers who must decided whether they want to join, and those who run the Eurozone and decide on whether/when to let them in. Current thinking views monetary union membership as a trade-off between macroeconomic *costs* and microeconomic *gains*. While the macroeconomic costs are relatively well understood, the gains from common euro usage are much less clear. Hence the need for a report that illuminates the euro's trade and investment effects to the best extent possible.

Chapter 2: The Rose effect: the euro's impact on aggregate trade flows

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1. INTRODUCTION

For most of the last hundred years, the received wisdom of economists and policymakers held that exchange rate volatility and multiple currencies depressed trade. This wisdom derived from causal empiricism – most of it related to the growth of trade during the "classical gold standard", in the period from 1880 to 1914 (Bordo 2002). Mundell (1960) used this received wisdom to assert that more trade would be the main microeconomic gain enjoyed when two nations formed a currency union.

Strange as it may seem, this cornerstone of Mundell's famous 'optimal currency area' theory rested on a no econometric evidence. Until relatively recently , economists could not find robust empirical evidence for a negative impact of exchange-rates and volatility on trade flows – and this despite three decades of increasingly sophisticated empirical methods and larger datasets following the introduction in the 1960s of computers in economic research. Clear results remained elusive, even when empiricists focused on the massive exchange rate turmoil accompanying the break-up of the Bretton Woods system in the 1970s (see, for instance, Wei 1999 or McKenzie 1999).

The situation changed dramatically at the turn of the 21st century. Rose (2000) published the startling finding that both exchange rate stability and a common currency were powerful stimulants to trade. More importantly, his estimates seemed to be robust. They withstood an initial barrage of cross checks and sensitivity analyses.

The pendulum had swung from one extreme to the other. Prior to 2000, research-based estimates of the trade effects of a common currency and exchange rate volatility were close to zero. Rose (2000) suggested that a currency union would increase trade by 200%, on top of the large and positive effect of eliminating exchange rate volatility. A pendulum at the height of its swing is motionless for a fraction of a second, but this is the point where it has the maximum potential energy. As it swings back, potential energy is swapped for kinetics and the pendulum accelerates. At its nadir – the place where it will eventually stop – it has zero potential energy but maximum speed. This carries it beyond its natural resting point to another extreme. The cycle repeats until air resistance 'takes the wind out' of the process, so to speak. This is a pretty good description of how the 'Rose Effect' literature developed since 2000.

[1] The view expressed in this study are those of the authors and do not necessarily reflect those of the European Central Bank.

While it is impossible to fully understand the euro's trade impact without another decade or so of data, the pendulum seems to be coming to rest. This chapter argues that the aggregate trade effect of the euro – the Rose effect – is positive but small.

Plan of chapter

After an introduction to the basic theoretical and econometric tools that are essential for understanding the euro's trade effects (Section 2), we turn to the empirical literature, reviewing how the pendulum has swung from 2000 onwards (Section 3) before turning new evidence that updates the Rose estimates using the latest data and best empirical techniques. The chapter ends with a summary and concluding remarks on what might be driving the Rose effects.

2. THE ESSENTIAL TOOLKIT: GRAVITY EQUATION THEORY AND ECONOMETRICS

While the theory behind the gravity model is simple and uncontroversial, it is largely ignored by empirical researchers studying the euro's trade effects. This shortcoming has produced an impressive array of econometric flaws in the literature. This section shows that the basic theory is simply explained using standard notation and solution techniques.

2.1. Physics and the naïve trade gravity equation

The trade gravity model's namesake in physics describes the force of gravity between two objects as proportional to the product of the masses of the two objects divided by the square of the distance between them. In symbols:

$$\begin{matrix} force\ of \\ gravity \end{matrix} = G\frac{M_1 M_2}{\left(dist_{12}\right)^2};$$

where the M's are the two masses and 'dist' is the distance between them. Here "G" is the gravitational constant (equal to 6.67300×10^{-11} m^3 kg^{-1} s^{-2}, where m, kg and s stand for meters, kilos and seconds).

The naïve form the trade gravity equations replaces physical mass with economic mass (GDP) and removes the power function on distance.

$$\begin{matrix} bilateral \\ trade \end{matrix} = G\frac{GDP_1 \quad GDP_2}{dist_{12}};$$

The resulting specification has proved very popular, providing an extremely good fit to trade data; typical R^2's are above 90% (Harrigan 2003).

This goodness-of-fit, however, is a trap that has caught many scholars who simply throw in variables into the naïve equation – say an FTA or currency union dummy – and assume that the coefficient estimated with OLS is unbiased. As we shall see, this assumption is incorrect. The basic problem is that G is not constant in the trade specification of the gravity equation; it varies by trade partner and over time and it is correlated with many if not all policy variables affecting trade. Consequently, the estimates of the impact of those policy variables – including the currency union dummy – are biased and often severely so.

2.2. Economic foundations of the gravity model: Anderson-Van Wincoop made easy

All of the complete mainstream foundations of the gravity are variants of the appendix in Anderson (1979),[2] including the famous but widely misinterpreted Anderson and Van Wincoop (2003). Here we follow the Anderson-Van Wincoop derivation using mainstream notation to facilitate the exposition.

The starting point of Anderson-Van Wincoop (and all the other complete derivations of the gravity equation) is a CES demand structure and the 'assumption' that each firm produces a unique variety of a unique good.[3] Since trade data are collected in value terms, it is convenient to work with the CES expenditure function rather than the CES demand function. The solution to the standard utility maximisation problem tells us that spending on an imported good that is produced in nation 'o' (short for 'origin') and consumed in nation 'd' (short 'destination') is:

$$v_{od} \equiv \left(\frac{p_{od}}{P_d} \right)^{1-\sigma} E_d ; \qquad \sigma > 1 \tag{1}$$

where v_{od} is the expenditure in destination-country d on a variety made in origin nation-o, P_d is nation-d's CES price index, σ is the elasticity of substitution among varieties, and E_d is nation-d expenditure.

Solution to the standard profit maximisation problem tells us that the consumer price is:

$$p_{od} = \mu_{od}\, p_o \tau_{od} \tag{2}$$

where p_{od} is the consumer price in nation-d of goods produced in nation-o, p_o is nation-o's domestic price, , μ_{od} is the bilateral price mark up (which depends upon the assumed market structure) and τ_{od} is the bilateral trade costs. This is the "pass-through equation". Combining this with (1) gives us the per-variety relationship. Aggregating over all varieties exported from nation-o to nation-d (assuming all varieties produced in nation-o are symmetric) yields aggregate bilateral trade:

$$V_{od} = n_{od} (\mu_{od}\, p_o \tau_{od})^{1-\sigma} \frac{E_d}{P_d^{1-\sigma}} \tag{3}$$

where V_{od} indicates the value of the aggregate trade flow (measured in terms of the numeraire), and n_{od} indicates the number of nation-o varieties sold in nation-d (Anderson-Van Wincoop simply by assuming that $n_{od}=1$ for all origin and destination markets).

Lesson #1: Every complete derivation of the gravity model is based on an expenditure function. This explains two key aspects:

- Destination country's GDP enters the gravity equation since it captures the standard income effect in an expenditure function.

---

[2] See Bergstrand (1985, 1989, 1990), Deardorff (1995), Helpman (1987), Harrigan (1996), Helliwell (1997) Wei (1996), and Feenstra, Markusen and Rose (1998) inter alia; interestingly, many of these authors re-derive the main results – apparently unaware of earlier work. A number of foundations, e.g. Helpman and Krugman (1985), and the early sections of Deardorff (1995) and Anderson (1979) assume trade is frictionless and so do not provide guidance on the critical issue of how one handles relative prices and their link with distance.

[3] In some variants, like Dixit-Stiglitz monopolistic competition, one can deduce that each firm would produce only one variety of only one good, but most derivations just assume it – justifying the assumption on a Heckscher-Ohlin model with many more goods than factors, or minor Ricardian differences. In the old literature, the assumption was justified by the so-called Armington assumption, i.e. the assumption that internationally traded products are differentiated by country of origin.

- Bilateral distance enters the gravity equation since it proxies for bilateral trade costs which get passed through to consumer prices and thus diminishes bilateral trade, *ceteris paribus*.

Lesson #2: Expenditure functions depend upon *relative* prices, not *absolute* prices, so (3) tells us that the naïve gravity equation is mis-specified because it excludes the importing nation's price index, P_d. Of course, the price index P_d is time varying, so it will not wash out into the constant except in the case of cross-section data.

2.2.1. Why does the exporting nation's GDP enter the gravity equation?

The answer is simple: it reflects the supply available for exporting. Roughly speaking, the bigger is the exporting nation, the more it has to offer. The exact economic logic, however, depends upon the details of the underlying theory.

The Anderson-Van Wincoop derivation assumes that each nation makes only one product (p. 174), so all the adjustment happens in the *price* of the nation's good. Big nations (in terms of GDP) thus export more to all destinations since their good is relatively cheap. Or more precisely, their good must be relatively cheap if they are to sell all the output produced under full employment. Helpman and Krugman (1995) make assumptions that prevent prices from adjusting (frictionless trade and factor price equalisation), so all the adjustment happens in the number of varieties that each nation has to offer. Big nations (in terms of GDP) thus export more to all destinations since they offer more varieties.

Sticking with the Anderson-Van Wincoop derivation, the idea is that nations with big GDPs must have low relative prices so that they can sell their whole production. To determine the nation-o price, p_o, that will clear the market, we sum-up nation-o's sales over all markets, including its own and set this equal to production. This condition, which is nothing more than nation-o's market clearing condition (Anderson 1979 calls it a trade balance condition), can be written as:

$$Y_o = \Sigma_d n_{od} v_{od} \qquad \Leftrightarrow \qquad Y_o = p_o^{1-\sigma} \left((n_{od}\, \mu_{od}\, \tau_{od})^{1-\sigma} \frac{E_d}{P_d^{1-\sigma}} \right) \tag{4}$$

where the second equality follows from the substitution of the expression for v_{od} , that is produced in turn by the substitution of (2) into (1). Solving (4) for $p_o^{1-\sigma}$, yields:

$$p_o^{1-\sigma} = \frac{Y_d}{\Omega_o}; \qquad \Omega_0 \equiv \Sigma_j \left(n_{oj} (\mu_{oj}\, \tau_{oj})^{1-\sigma} \frac{E_j}{P_j^{1-\sigma}} \right) \tag{5}$$

In words, this says that nations with big GDPs must have low prices (recall $\sigma>1$). The variable we call Ω has many names in the literature since several authors have re-invented it since the original Anderson (1979) article.[4]

Using (5) in (3) yields the most basic 'non-naïve' gravity equation:

$$V_{od} = n_{od} (\mu_{od}\, \tau_{od})^{1-\sigma} \frac{E_d Y_o}{P_d^{1-\sigma} \Omega_o} \tag{6}$$

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[4] In the economic geography literature Ω_o is called the exporting nation's 'market access' or 'market potential'. Wei (1996) calls it nation-o's "remoteness". Deardorff (1995) – whose goal is to solve the theoretical integral of what one must assume to get the naïve gravity equation (instead of working out the right way to estimate a gravity-like equation) – provides a number of aesthetic twists that allow him to use the definitional phrase 'relative distance to suppliers/demanders'. Apart from the aesthetic introduction of GDP shares and world GDP, Ω is the same as Anderson-Van Wincoop's Π; we don't adopt Π since – in standard notation – this Greek letter is reserved for 'profit' (and we'll use Π as such later).

Lesson #3: Origin-country GDP enters the gravity model since economic big nations offer goods which are either relatively competitive, or abundant in variety, or both.

Lesson #4: The derivation that shows why origin-country GDP enters the gravity model also shows that the exporting nation's market access (Ω) matters, so the naïve gravity equation is mis-specified due to its exclusion.

Expression (6) is identical to Anderson-Van Wincoop's expression 9 (apart from their inclusion of GDP shares and world GDP for aesthetic reasons). They go one step further and this last step appears to be the source of many the econometric flaws in the euro trade-effect literature, so it is worth studying why it is inappropriate in the case at hand – and indeed in most applications to studies of trade barriers.

2.2.2. Anderson-Van Wincoop's unnecessary and misleading last step

Anderson and Van-Wincoop assert that $\Omega_i=P_i^{1-\sigma}$ for all nations since $\Omega_i=P_i^{1-\sigma}$ is a solution to the system of equations that define Ω and $P^{1-\sigma}$ (p. 175). There are two critical assumptions behind this:

1. They assume that trade costs are two-way symmetric across all pairs of countries. This assumption is automatically violated in the case of preferential trade arrangements – such as the EU. One could test it directly, but every estimate that shows that preference trade agreement matters demonstrates that it is false, so little would be gained by further testing. It is also violated when we assume that the euro usage may have a different effect on exports to the Eurozone from non-Eurozone nations than it does on Eurozoners' exports to outsiders.

2. They assume that there is only one period of data. The point here is that one can easily show that one solution of Ω_i and $P_i^{1-\sigma}$ is that they are proportional, i.e. that $\chi\Omega_i=P_i^{1-\sigma}$ for someχ. With just one year of data, the theorist can choose units to set $\chi=1$. However, as Baldwin and Taglioni (2006) show, when there are more years entering the estimation there must be one χ *per year*. For the case of euro trade effect studies, the second problem would be easily dealt with via time dummies if the first problem did not exist.

In conclusion Anderson-Van Wincoop's last step is unnecessary and it introduces confusion into the theory. By contrast, with P and Ω separate, it is simple to see that the naïve gravity equation is missing a time-varying variable related to the importing nation and reflecting the relative price of imports from any particular partner. It also is missing a time-varying variable related to the exporting nation's competitiveness. Hence, keeping these variables separate, it appears evident that the solution is to introduce time-varying, country fixed effects.

Of course, in cross-section data on can sweep the product $\Omega_i\times P_i^{1-\sigma}$ into a pair fixed effect, but this does not work with panel data. This point is widely missed in the literature and has led to many mistakes as authors assume that the solutions adopted by Anderson and Van Wincoop (2001, 2003) for cross-section data were equally valid for panel data. This mistake is so common that it is useful to have a name for it, the "Anderson-van-Wincoop misinterpretation". Note that this is not a misinterpretation *by* Anderson-van-Wincoop, it is a misinterpretation *of* Anderson-van-Wincoop.[5] There is no mistake in Anderson and Van Wincoop (2003), since they do their estimation only with cross-section data.

2.3. The euro's trade effect

According to the theory, the effects of the euro's introduction could show up in the number of varieties sold bilaterally, n_{od}, the bilateral mark-up, μ_{od}, or the bilateral trade costs, τ_{od}. Taking the GDP of nation-o as a proxy for its production of traded goods, and nation-d's GDP as a proxy for its

[5] See Baldwin and Taglioni (2006) for details. It has also been demonstrated by Balistreri (2007).

expenditure on traded goods, and bilateral distance ($dist_{od}$) as a proxy for τ, (6) can be re-written to look just like the physical law of gravity.

$$V_{od} = G_{od} \frac{Y_o Y_d}{\left(dist_{od}\right)^{elasticy-1}}; \qquad G_{od} \equiv n_{od} \mu_{od}^{1-\sigma} \frac{1}{\Omega_o} \frac{1}{P_d^{1-elasticity}}$$

We have demonstrated in the previous discussion that, unlike its namesake from physics, G_{od} in the economics version of the gravity equation is not constant. Failure to recognise this has led to an impressive range of errors and an astounding frequency of repetition of the main errors over the years.

For panel data, we have (6) for each period so taking logs and adding the time subscript, the equation to be estimated is:

$$\ln V_{odt} = (1-\sigma)\ln(\tau_{odt}) + \ln Y_{ot} E_{dt} - \ln G_{odt}$$

The "gravitational unconstant" G_{od} is the key to a correct specification of the gravity equation. Since Ω includes E's and P's, it varies over time and over nations. Since P includes prices, it varies over time and across nations. It is important to note that the potential drivers of the euro effect, n_{od}, μ_{od}, and τ_{od} all enter into G_{odt} directly and/or indirectly via P and Ω. Most of the abundant econometric errors in the euro trade literature stem from a failure to properly specify the "gravitational unconstant" $G_{odt.}$

Assuming that bilateral trade costs τ are related to bilateral distance, common euro usage and other factors (which potentially vary over time and across partners and that we denote as Z_{odt}), the log of τ_{odt} equals $-\beta_1 EZ_{odt} + \beta_2 \ln Dist_{od} + \beta_3 \ln Z_{odt}$. Using the definition of G_{odt}, the model to be estimated is:

$$\begin{aligned} \ln V_{odt} = {} & \beta_1(\sigma-1) EZ_{odt} - \beta_2(\sigma-1)\ln Dist_{od} + \ln Y_{ot} E_{dt} \\ & + \ln n_{odt} - \beta_3(\sigma-1)\ln Z_{odt} - (\sigma-1)\ln \mu_{odt} - \ln \Omega_{ot} - \ln(P_{dt}^{1-\sigma}) \end{aligned} \qquad (7)$$

All empirical studies of the euro's trade effects take account of the terms in the first row of (7); the mistakes arise when they fail to account for the terms in the second row. When these terms are ignored, they show up in the residuals and end up biasing the estimated coefficients for the variables in the first row. The points made above are worth repeating;

- Ω and P are time-varying, so they cannot be controlled by time-invariant country or pair fixed effects – except of course, if one uses cross-section data as in Anderson-Van Wincoop (2001, 2003).
- The euro's impact on trade shows up via Z, n and μ as well as EZ; if the second row terms are ignored, the residuals will be correlated with EZ and thus the coefficient for the euro impact on trade will be biased.

2.4. Common empirical errors: the gold, silver and bronze medal mistakes and others

Failure to consider the simple theoretical foundations of the gravity model has lead researchers to commit all sorts of econometric errors when estimating the gravity equation. Three are particularly common when it comes to the euro trade estimates. Baldwin (2006) dubs them the 'gold', 'silver' and 'bronze' medal errors; Baldwin and Taglioni (2006) illustrate the impact of each on the estimated trade effect of the euro. Here is a list of the most common ones.

Maxi-gold. The gold medal mistake was committed in Rose's original article, although it was quickly recognised and corrected (by Rose himself in Rose and van Wincoop 2001 and Glick and

Rose 2002). The error is to estimate (7) on panel data using pooled OLS. The problem is that the variable of interest, the euro dummy, is including the omitted variables, Ω and P, so the euro's trade impact is biased (often in a spectacular manner as in Rose 2000). What Rose did might be called the maxi-gold error since he made no attempt at all to correct for the correlation between EZ and the omitted Ω and P terms.

The maxi-gold medal error is rarely committed since the fixed-effect method developed by Harrigan (1996) was popularized by Anderson and van Wincoop (2003). Throwing in either country fixed effect – as in Rose and Van Wincoop (2001), or pair fixed effects – as in Rose and Glick (2001) – tends to reduce the bias by removing the pure cross-section correlation between euro-usage and the omitted Ω and P terms. However, since the entire set of euro studies are estimated on data that spans more years of data before and after the euro's introduction, such fixed effects do not remove the bias stemming from the time-series correlation. This is the Anderson-van-Wincoop misinterpretation (dubbed such since most authors learned of the fixed effect technique from that well-known paper).

Silver. The silver medal error stems from a quick fix that was often used to improve the point estimates. This involves the averaging of the two-way bilateral trade flow instead of using the uni-directional trade flow as suggested by the theory.[6] In fact, there is nothing wrong with the concept – just its implementation. Given that the gravity equation is multiplicative, it is clear that one should work with the product of the two directional flows rather than the sum. The silver medal error arises when authors instead employ the log of the sum of bilateral trade as left-hand side variable. Using the notation above, they use $V_{od}+V_{do}$; since the equation is always estimated in logs, this ends up translating into using the log of the sum instead of the sum of the logs. This treatment became especially popular in the US after empirical work by Frankel on regionalism. Following this line of work, Rose (2000) committed the silver medal mistake and many studies in the field, including some very recently published works, have followed his lead.

The silver medal mistake will create no bias if bilateral trade is balanced. However, if nations in a currency union tend to have larger than usual bilateral imbalances – as Baldwin and Taglioni (2006) show is the case for the Eurozone countries – then the silver medal mis-specification leads to an upward bias. The point is that the log of the sum (wrong procedure) overestimates the sum of the log (correct procedure), so the silver medal mistake implies that the researcher is working with an overestimate of trade flows among Eurozone nations.

Bronze. The bronze medal mistake concerns price deflation. All the prices in the gravity equation are measured in terms of a common numeraire, so there is no price illusion. Many researchers, however, feel the need to deflate trade flows with a price index and a common choice is the US CPI (following Rose's example). Fortunately, the bronze medal bias is eliminated by including the time dummies – a practice that is almost universal. Baldwin and Taglioni (2006) show that adding time dummies yields estimates identical to those without time dummies and without the incorrect deflation.

Real GDP mistake. Many authors think of the gravity equation as based on a demand equation and so the proxy the E and Y terms with *real* GDP of the destination and origin nations. To match this, they then have to deflate the trade data (which only comes in value terms, not quantities) by some price index so they can act as if their measure of bilateral trade is measuring quantities of trade. The problem with all this is that it is impossible to get good price indices for bilateral trade flows. Instead, a variety of imaginative solutions have been adopted. Rose, as mentioned above, deflated all bilateral trade flows by means of the US CPI. Others use the origin-nation's producer price index, PPI, (e.g. Flam and Nordstrom 2003, 2006, 2007). The consequence of this inappropriate deflation can be unpredictable. For example, if there is a correlation between the

[6] The problem this 'fixes' is that point estimates without this technique are often too far below the theoretical level of unity. Many justifications are offered for this fix, including the importance of bilateral exchange rate fluctuations.

inappropriate trade deflator and the origin-nation's GDP deflator, the estimate on Y_o is likely to be biased. Likewise, if the authors also use the exporting nation's PPI in constructing a real exchange rate, they will have transformed the left and right hand sides by the same variable.

Fortunately, this 'real mistake' seems to have limited impact on the EZ estimate since euro-usage is likely to be quite orthogonal to the various trade deflators and the various GDP deflators.

2.4.1. The common misinterpretation of Anderson-van-Wincoop

Much of the euro trade effect literature takes its basic theory and econometrics from Anderson and van Wincoop (2001), eventually published as Anderson and van Wincoop (2003). As mentioned, it is unfortunate that while those authors work out the theory and econometrics for the case of cross-section data, many researchers simply assume that it extends without modification to panel data. This assumption is what we call the "Anderson-van-Wincoop misinterpretation" above, or more precisely the panel-data misinterpretation of Anderson-van-Wincoop's cross-section technique of controlling for the omitted Ω and P with pair fixed effects.[7] Note that Anderson and van Wincoop (2004) explicitly points out this issue, but unfortunately most economists working in the field seem to have focused only on the 2003 paper.

2.4.2. Omitted variable bias

The gravity model uses distance as a proxy for bilateral trade costs, but all sorts of other factors also affect bilateral trade costs that get passed on at least partially to consumers. The econometrician has data only on a subset of these factors, which is a problem since many of the additional determinants affecting bilateral trade costs are almost surely correlated with joint membership in the Eurozone.[8] Given this correlation between omitted variables and the variable-of-interest, a simple OLS estimate surely yields a biased estimate, most likely an upward bias. In terms of the analysis above, it is the correlation between Z and EZ that is creating the bias.

This point was recognised by Glick and Rose (2002) and corrected by the inclusion of pair dummies. Most subsequent studies have followed this practice. Note however, that time-invariant pair dummies are not enough if the omitted factors – what we called Z in equation (7) – is likely to have an important time dimension, especially in long data samples. For example, the EU's Economic and Monetary Union (EMU) is, by design, a programme in which integration of the market and monetary integration go hand in hand. Since we have only imperfect measures of the progressive market integration, the inclusion of pair fixed effects will not be enough to offset the bias arising from the omitted variables.

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[7] The basic issue is simple to illustrate. Remember from the derivation, the terms Ω and $P^{1-\sigma}$ entered the specification of the trade gravity equation because relative prices matter in the destination nation ($P_d^{1-\sigma}$) and producer prices of the origin nation depend upon global market access (Ω_o) of this latter. The terms Ω and $P^{1-\sigma}$ are country specific, so in cross-section data a dummy for each exporter and another for each importer is the perfect way to handle the lack of data on Ω and P. In panel data, however, ($\Omega P^{1-\sigma}$) – which is exactly equal to the Anderson-van-Wincoop "multilateral trade resistance" term – has an important time dimension. Consequently, country dummies only remove the average impact, leaving the time-variation in the residual. This will obviously bias the results. Or, to use Anderson-van-Wincoop terminology, multilateral trade resistance is serially correlated, so time-invariant country dummies are not enough.

[8] The set of nations that abandoned their national currencies for the euro is far from random. For example, the so-called DM-bloc nations had been more tightly integrated than the average EU nation for decades.

Box 1: The gravity model: the theoretical literature

The gravity model emerged in the 1960s as an empirical specification with hand-waving theoretical underpinnings (Tinbergen 1962, Poyhonen 1963, Linnemann 1966). Leamer and Stern's famous 1970 book provided some foundations (three distinct sets, in fact). The best is based on what could be called the 'potluck assumption.' Nations produce their goods and throw them all into a pot; then each nation draws its consumption out of the pot in proportion to its income. The expected value of nation-i's consumption produced by nation-j will equal the product of nation-i's share of world GDP times nation-j's share of world GDP. In this way, bilateral trade is proportional the product of the GDP shares.

Anderson (1979) seems to be the first to provide clear microfoundations that rely only on assumptions that would strike present-day readers as absolutely standard. The cornerstone of Anderson's theory, however, rested on an assumption that was viewed as ad hoc at the time, namely that each nation produced a unique good that was only imperfectly substitutable with other nations' goods. The gravity model fell into disrepute in the 1970s and 1980s; for example, Alan Deardoff refers to the gravity model as having "somewhat dubious theoretical heritage" (Deardoff 1984 p. 503).

The gravity equation's next set of theoretical foundations came when Bergstrand (1985) sought to provide theoretical foundations based on neoclassical trade theory; in particular he developed a theoretical connection between factor endowments and bilateral trade. He did not manage to reduce the complicated price terms to an empirically implementable equation; "calculating the complex price terms in [his expression] is beyond this paper's scope," he wrote. He argued that one could approximate the theory-based price terms with various existing price indices. Bergstrand (1989, 1990) re-did his earlier effort using the Helpman-Krugman model (Helpman and Krugman 1985) that married the new and old trade theory, but he continued to use existing price indices instead of the ones he justifies with his theory (Bergstrand 1989, p. 147).

The 'new trade theory' (e.g. Krugman 1979, 1980, 1981, Helpman 1981) breathed new life into the gravity model. Indeed a trend began where the gravity model went from having too few theoretical foundations to having too many. For example, in a 1995 paper on the gravity model Deardorff provides a nearly 180 degree turn around by writing: "it is not all that difficult to justify even simple forms of the gravity equation from standard trade theories." Also see Evenett and Keller (2002) on this point.

Empirical work using the gravity model enjoyed renewed popularity in the early 1990s in Europe, e.g. in Wang and Winters (1992), and Baldwin (1994). It caught on in the US with McCallum (1995) and Frankel and Wei (1997).

The most recent advances have focused on using theory to inform the econometrics, e.g. Anderson and Van Wincoop (2001) which was published as Anderson and Van Wincoop (2003). The basic theory in Anderson and Van Wincoop (2001) is very close to Anderson (1979); the main value added is the derivation of a practical way of using the full expenditure system to estimate key parameters on cross-section data. Since this procedure is difficult, Anderson and Van Wincoop also exploit an alternative procedure, i.e. the use of nation-dummies – a procedure first employed by Harrigan (1996). The theory in Anderson and Van Wincoop (2003) only applies to cross-section data, but this point is deeply embedded in the article's footnotes and so has not been well appreciated (see Balistreri 2007, and Balistreri and Hillberry 2007).

Recent years have seen a number of papers by empirical trade economists that take the theory seriously, but these are typically viewed as contributions to narrow empirical topics – e.g. the size of the border effect, or the magnitude of the elasticity of substitution – so the methodological advances in these papers have been generally ignored in the wider literature. Some of the key papers in this line are Harrigan (1996) and Head and Mayer (2000), and Combes, Lafourcade and Mayer (2005). In a similar vein, a number of papers have tackled the question of 'zeros' in the trade matrix, for example, Helpman, Melitz and Rubinstein (2005), and Westerlund and Wilhelmsson (2006). The former uses an sophisticated two step procedure, the later suggests using a Poisson fixed effects estimator; both show that estimates can be severely biased by incorrect treatment of zero trade flows.

3. LITERATURE REVIEW

Taken at face value, the empirical literature on the boost to trade due to the formation of a monetary and currency union – the so-called Rose effect – is a disaster. Estimates published by eminent professors range from 0% (Berger and Nitsch 2005) to 1,387% (Alesina, Barro and Tenreyro 2002). Looking beyond the face value, however, the literature is quite cohesive – indeed one can say that a consensus view has emerged. To gain this informed perspective, however, one needs to feel confident about which estimates can be dismissed and why. This requires an investment in understanding the main empirical tool in the field – the gravity equation. As we shall see, once this understanding is firmly in hand, it is absolutely clear that most of the published estimates in this area are fatally flawed by misspecification and/or econometric errors. Hence most of the articles reporting such estimates may still be interesting from a history-of-thought perspective but they are useless for today's policymakers.

3.1. Early literature: Evidence from non-Eurozone currency unions

3.1.1. Rose (2000)

Rose (2000) started the debate using a gravity equation on data for bilateral trade among 186 nations. In his favourite regression, the coefficient on his currency union dummy was 1.21 which implies trade between common-currency pairs was $e^{1.21}=3.35$ times larger than the baseline model would suggest, i.e. a 'Rose effect' of 235%. This was too big to be believed and the profession's assault on this claim began even before he presented the preliminary draft of the study at the October 1999 Economic Policy Panel. A highly readable presentation of the main early critiques can be found in Nitsch (2002, 2005). Four arguments dominated the criticisms to the study by Rose.

- Omitted variables,

Omitting variables that are pro-trade and correlated with the currency union (CU) dummy biases the estimate upwards;

- Reverse causality,

Big bilateral trade flows cause a common currency rather than vice versa, so the high estimate reflects the impact of trade on currency union formation not the other way round;

- Model misspecification.

This one is quite complicated so we postpone it.

- Most of the common currency pairs in Rose's sample involved nations that were very small and very poor and these factors might have biased the result

In a paper presented at a June 2005 ECB conference, Baldwin (2006b), argues that there are three types of CUs in Rose's data. 1) Hub-and-spoke CU arrangements (small nations, the spokes, adopting the currency of their dominant trade partner, the hub) with the US, France, Britain, Australia and New Zealand as hubs. 2) Multilateral currency unions among small poor nations, such as the West African CFA. 3) highly idiosyncratic unions involving a very local hegemony, like Switzerland and Liechtenstein, or Italy and San Marino. That paper also shows that some extremely open nations also share a currency with some other country and these nations' openness is so unusual that it is hard to use evidence from them to interpret what is going on with the rest of the sample. For example, there are 6 nations with openness above 200% (Bahamas 1400%, Singapore 750%, Liberia 600%, Bahrain 400%, Kiribati 370%, and Belgium-Luxembourg 320%). All but one of these are involved in a currency union. This is just one indication given in Baldwin (2006b) but it clearly shows that nations involved in currency unions are a long way from average nations. The income levels of currency union members are either noticeably higher than the average

nation (this is the case for the hubs like the US and Britain) or considerably lower than the average nation (this is the case for the spokes).

3.1.2. Correcting biases piecemeal

The naïve gravity equation used by Rose (2000) had long been known to be mis-specified, but the knowledge was forgotten and re-invented several times since it was first discussed in Anderson (1979). In particular, Anderson and Van Wincoop (2001) published an influential paper in the American Economic Review that pointed out two ways of estimating the gravity equation on cross-section data without the 'gold medal' bias. The first was an awkward full system estimation of the expenditure system. The second re-invented the Harrigan (1996) fixed effects approach of including origin-nation and destination-nation dummies in a cross section regression.

Using the country dummies approach, Rose and van Wincoop (2001) showed that the Rose effect fell by 2.7 standard deviations. This estimate, however, is marred by the Anderson-van-Wincoop misinterpretation (as we discussed in the earlier sections of this chapter this mis-applies the Anderson-van-Wincoop correction designed for cross-section data to panel data). This is probably why Rose and van Wincoop (2001) get a very different result when they use the harder full-system method that avoids the Anderson-van-Wincoop misinterpretation. The results are not clearly shown in Rose and van Wincoop (2001), but rough calculations on the numbers that are revealed in the paper suggest that the coefficient on the common currency dummy fell to 0.65, i.e. a Rose effect of 91%.

To address the issue of omitted variables that are correlated with the currency union dummy, Glick and Rose (2002) throw in pair-specific dummies in a data set that covers the 1948 to 1997 period. This only addresses time-invariant omitted variables (a somewhat limited set given the changes in the half century covered), but nevertheless the pair dummies greatly lower the estimated Rose effect from 3.7 to 1.9 times more trade among CU pairs.

Pakko and Wall (2001) refute the finding in Rose (2000)

Quite independently of Rose's efforts with various co-authors, Pakko and Wall (2001) started applying theory and econometric reasoning in a systematic way to the problem. For example, they use the Rose (2000) data set but instead of averaging the two-way bilateral flows (i.e. Germany's exports to Denmark and Denmark's exports to Germany), they preserve the directional flows. This allows them to impose direction-specific pair dummies, i.e. two different dummies per bilateral flow – a technique that is more general than in Rose (2001). Although they get Rose-like estimates of the Rose effect without pair dummies, they find that the Rose effect droops and withers away completely with pair dummies. The point estimate is negative and not significantly different than zero. Rather than pushing quickly on to the next dataset and empirical technique as does Rose (2001), Pakko and Wall take the time to identify the sources of the biases that lead to the big mistakes in Rose (2000). They show that when one does carry out the estimates correctly on the dataset used by Rose (2000) , the headline results disappear. In other words, they showed that the famous Rose (2000) result was due entirely to an econometric mistake. Once corrected, the proper estimate of the Rose effect on the Rose (2000) dataset is zero – entirely in line with the findings of researchers in the last century.

In short, Pakko and Wall (2001) demonstrate clearly that Rose (2000) should be ignored for policy or scientific purposes. Yet, it remains a great seminal paper that, despite proving to be wrong in its conclusion, has the great merit to have stimulated a debate that revealed a new truth.

Persson (2001): Model misspecification

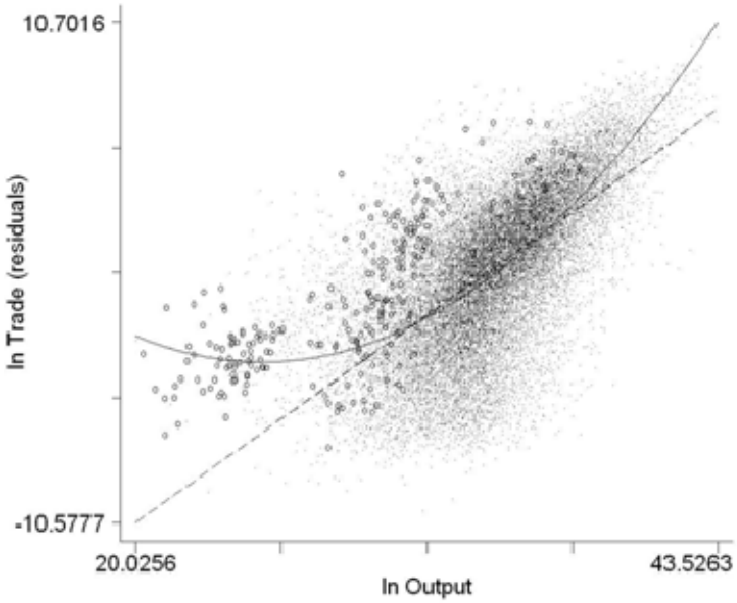
The next turn in the pre-euro Rose effect literature is more technical. Persson (2001) employs a matching technique that can control for this sort of nonlinearity-with-self-selection to the Rose (2000) dataset and finds that the point estimate for the Rose effect is much lower – Persson's

estimates of the Rose effect range from 1.13 to 1.66 – and they are not significant statistically. Kenen (2001) confirms part of the basic result using a different matching technique, but obtains very different results in the regression analysis.

If the underlying model (in this case the impact of a currency union on trade) is non-linear and at the same time allows for self-selection into the treatment group (i.e. nations that choose currency unions), standard regression techniques can produce important biases. What the matching technique does is to control for the impact of self-selection by using as control group observations that are 1) not 'treated', and 2) similar to the observations that are treated.

Persson suggests that the key non-linearity of the model concerns the openness and output link. The basic point is summed up neatly in Figure 1. Non-CU observations are shown with black dots, CU observations with circles. The straight line shows the estimated linear relation between bilateral trade and output – i.e. the linear model imposed by Rose. The curved line shows the best fit allowing for a nonlinear relationship between openness and output. Just as in the parable above, the non-random distribution of CU pairs team up with the 'true' model's nonlinearity to produce an overestimation of the effect. The point is that if one compares the positions of the circles to the straight line, it looks like they have far greater trade than they should have had. If one compares them to the curved line, the circles are, on average, above the predicted relationship, but much less so than if one takes the straight line as the true model. Thus, the linear regression substantially overestimates the impact of a common currency on trade because it underestimates how much trade they would have had without a common currency. In short, Persson asserts that Rose (2000) overestimated the effect since he was comparing the actual trade to a mis-specified model of what trade should have been in the case of absent common currency.

Figure 1: Persson's hypothesis for why the Rose effect is overestimated



Further evidence comes from the fact that allowing a quadratic term in Rose's regression (i.e. pooled cross-section without country or pair dummies), the Rose effect estimate drops radically. Rose (2000) included a squared output and per-capita output terms in one of his dozens of regressions. When doing so, he found that the Rose effect dropped dramatically, from boosting trade by 3.39 times to boosting trade by a much lower 1.95 times; the difference corresponds to a drop in the coefficient for the euro impact on trade by four standard deviations.

3.1.3. Lessons for the Eurozone from non-European experiences

The cleanest estimates of non-European currency unions are those that use matching techniques since these go a long way towards controlling for the major critiques of Rose (2000). On the original Rose dataset, Persson (2001) found the effect to be between 15% and 66%, but not statistically different to zero. Rose (2001) found it to be between 21% and 43% on a much larger dataset.

Nevertheless, it is important to close this part of the literature with a proviso. It seems that non-European evidence contains no useful information for the Eurozone. The non-European results are driven by nations that are very small, very poor, very open and who are leaving currency unions, not joining them.

3.2. Evidence on the Eurozone's Rose effect

3.2.1. Micco, Stein and Ordoñez (2003

Micco, Stein and Ordoñez (2003) is the first published study applying the techniques of Rose (2000) to the Eurozone. Subsequent work by Gomes et al (2004), Flam and Nordstrom (2003), Berger and Nitsch (2005), Barr, Breedon and Miles (2003), Bun and Klaassen (2002), De Souza (2002), Piscitelli (2003), Baldwin and Taglioni (2004), Baldwin, Skudelny and Taglioni (2004), Anderton, Baltagi, Skudelny and De Souza (2002), Mancini-Griffoli and Pauwels (2006), and De Nardis and Vicarelli (2003) are discussed extensively in the Chapter 2.2 of Baldwin (2006).

Here we establish the baseline by discussing the results in Micco, Stein and Ordoñez (2003) (MSO hereafter) and then focus for the most part on papers that were not covered in the Baldwin 2006 survey.

The cleanest results in the MSO paper are estimates done with pair fixed effects on the EU15 sample for the 1992 to 2002 period. Using this technique, MSO find that the Rose effect induced 6% more trade among Eurozone members.[9] This estimation strategy can be thought of as a difference-in-differences estimate. Using terminology that comes from medical studies, one group of trade pairs gets 'treated', while the control group of pairs gets a placebo; here, the treatment is the Eurozone membership and the placebo is the non-membership. Using the gravity model to control for observable differences between the control group and the treatment group, the estimate tells us by how much bilateral trade increased in the 'treatment' group relative to the rise in the 'control' group. This is called difference in differences, since it compares the before-and-after difference for the treatment group to the before-and-after difference for the control group.

In doing this sort of exercise, it is important to get a control group that is as comparable as possible to the treatment group as far as unobservable factors are concerned (any observable factors can be controlled for via regression analysis). Given that EU membership is an extremely complex thing – one that involves literally thousands of laws, regulations and practices that affect trade within the EU and with third nations, most of which are unobservable to the econometrician – limiting the control group to EU members is very useful.[10] Moreover, limiting the data to post 1992 data is useful since the EU changed the way it collected trade statistics in 1993.[11]

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[9] MSO, like many authors in this literature, use EMU to stand for "European monetary union"; unfortunately, EMU stands for "Economic and Monetary Union" – at least since the Maastricht Treaty that implemented EMU in both correct and incorrect senses of the phrase. All EU members are part of EMU, so writers who are familiar with European integration use the terms Eurozone, Euroland or euro area to refer to those EU members who have adopted the euro. Also EZ is shorter than EMU.

[10] Just to take one example, the EU signed dozens of preferential trade agreements during the 1992-2002 period. Since each of these erodes the preference margin of EU members, they should alter intra-EU trade flows.

[11] In fact, MSO should probably have used 1993-2002 data since the new data collection systems started with 1993 data.

This difference-in-differences technique does not control for differences between Eurozone and non-Eurozone members' implementation of EU-wide reform. The EU is continuously 'deepening' its integration, removing various barriers to the free movement of goods, people, capital and services. All EU members must adopt these measures, but many EU members delay adoption – sometimes for years – and so the Single Market is not really a single market at any given moment. If the delays are systematically more important for the 'outs', i.e. non-Eurozone members than they are for the 'ins', then the Eurozone dummy may be biased upwards. In fact, the fastest implementers include all three of the outs (Britain, Denmark and Sweden) while the three laggards (Italy, Portugal and Ireland) are 'ins', so the MSO 6% may be biased.[12]

MSO also perform the difference-in-difference technique using a broader sample that includes 8 additional rich nations (Iceland, Norway, Switzerland, Australia, Canada, Japan, New Zealand and the US). Trying to control for other forms of EU integration with dummies and proxies, they find that the Rose effect is 4% or 5%. This result is likely to be subject to more biases than the EU sample since many omitted factors affect trade with and among these additional nations.

3.2.2. Flam and Nordstrom (2003)

Flam and Nordstrom (2003) introduced a number of innovations that corrected some the standard econometric mistakes in the literature. For example, they avoid the silver medal mistake by using uni-directional bilateral trade flows as suggested by Anderson (1979). They also avoid the maxi-gold medal mistake and some of the omitted-variable mistakes by including pair fixed effects. The use of direction-specific bilateral trade flows moreover allows them to look at an issue that concerns all the non-Eurozone nations, whether the euro puts their exporters at a disadvantage in Euroland. This innovation provides a more subtle view of the MSO finding of no trade diversion. Additionally, they point out problems with European trade data collection during this period, especially relating to the VAT fraud (see Baldwin 2006a, Chapter 3.1 for details). Finally, they perform their regressions on sector data as well as aggregate data.

Econometric flaws

The estimates, however, contain a number of errors that have been repeated in subsequent literature. The most obvious is the Anderson-van-Wincoop misinterpretation, i.e. failure to account for the time-varying aspect of the 'gravitational unconstant' (see Section 2). Moreover, their treatment of exchanges rates, while reducing their estimate of the Rose effect by about 4 standard deviations, is ad hoc (in the sense that it is not justified by a theoretical derivation) and leaves the door open to possible flaws in the estimations. In particular, bilateral trade is deflated by the exporting nation's producer price index (PPI) and then the same index is used to construct the real bilateral exchange rate; this potentially introduces a spurious correlation among left- and right-hand side variables. Indeed, the study finds the estimated coefficient for the bilateral real exchange rates to be -1.0 and highly significant, which is in line with the idea that the same series was entered twice: once in the numerator of the right-hand side and a second time on the denominator of the left-hand side. The 'competitors' real exchange rate is estimated to be positive and highly significant, but it is unclear what to draw from this. The constructed 'third country' real exchange rate measure is an export-weighted sum of the PPIs of each importing nation's partners (excluding the origin-nation in question) divided by the importing nation's PPI. Since this weighted PPI is likely to be highly correlated with the excluded $P_d^{\sigma-1}$ term, the positive correlation is not surprising, but its interpretations is obscured by a lack of theoretical discussion of why it should be included when the $P_d^{\sigma-1}$ term is ignored.

These authors also follow most of the literature in failing to fully account for the gradual implementation of the Single Market integration measures, which continue, even today, to deepen

[12] Note that MSO try to control for the observable part of this, but the measure they use is extremely crude and so surely it fails in its purpose. See http://europa.eu.int/comm/internal_market/score/index_en.htm.

the integration of markets in the EU and in the whole Western Europe (via EEA and EU-Swiss Bilateral Accords).

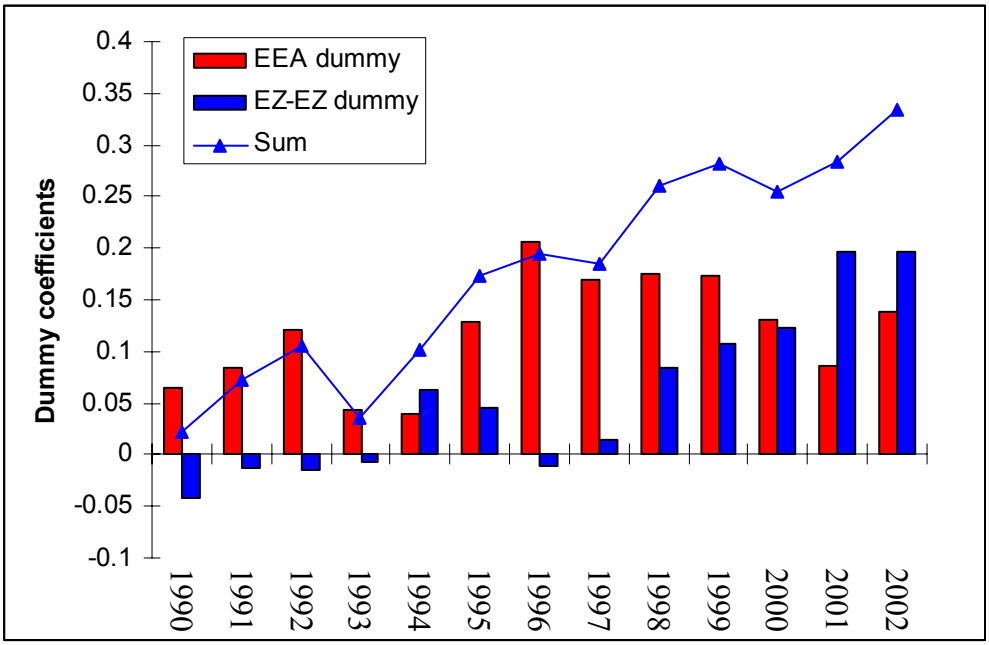
Results

The basic findings of Flam and Nordstrom on the aggregate data (which are biased by the econometric flaws discussed) are in line with MSO, both in terms of size and timing. Their preferred estimate uses the three non-Eurozone and eight additional high-income nations as the control group and finds that the Rose effect implies about 15% higher trade; Eurozone trade with other nations (in either direction) is boosted by about half that. Using the cleanest definition of the control group – other EU nations – the Rose effect is only 9%. The findings on the sectoral data suggest that the Rose effect is only present in sectors marked by differentiated products, confirming the earlier results of Taglioni (2002), and Baldwin, Skudelny and Taglioni (2005).

There are a few puzzling findings in Flam-Nordstrom. As in MSO, this study finds that the Single Market has about the same magnitude effect on trade as the euro. Other hints to the possibility that the euro dummy is picking up part of the effect due to Single Market policies emerges from the results of the authors' experiments with the sample. When the sample includes Norway and Switzerland in addition to the EU14[13], the estimate of the intra-Eurozone dummy is higher by about one standard deviation and the dummy on Eurozone exports to non-EZ nations is higher by two standard deviations.

The leftmost bar in Figure 2 shows the year by year EZ dummy; the other bar in each pair of bars shows the estimated EU dummy (actually it is the EEA dummy even though they call it the EU dummy). What we see is that just as the Rose effect is estimated to be increasing sharply, in the 1999 to 2001 period, the effectiveness of the single market is estimated as diminishing by almost as much. The line shows the sum of the two. This also 'explains' why De Souza (2002) finds no Rose effect when he includes a time trend for EU integration, and why Berger and Nitsch (2005) are able to shrink the Rose effect to nothing by including a time trend for integration among the would-be Eurozone members.

Figure 2: Flam-Nordstrom estimates of Single Market and Eurozone dummies



[13] Flam and Nordstrom exclude Greece from their aggregate regressions since they lacked Greek data for the sectoral regressions

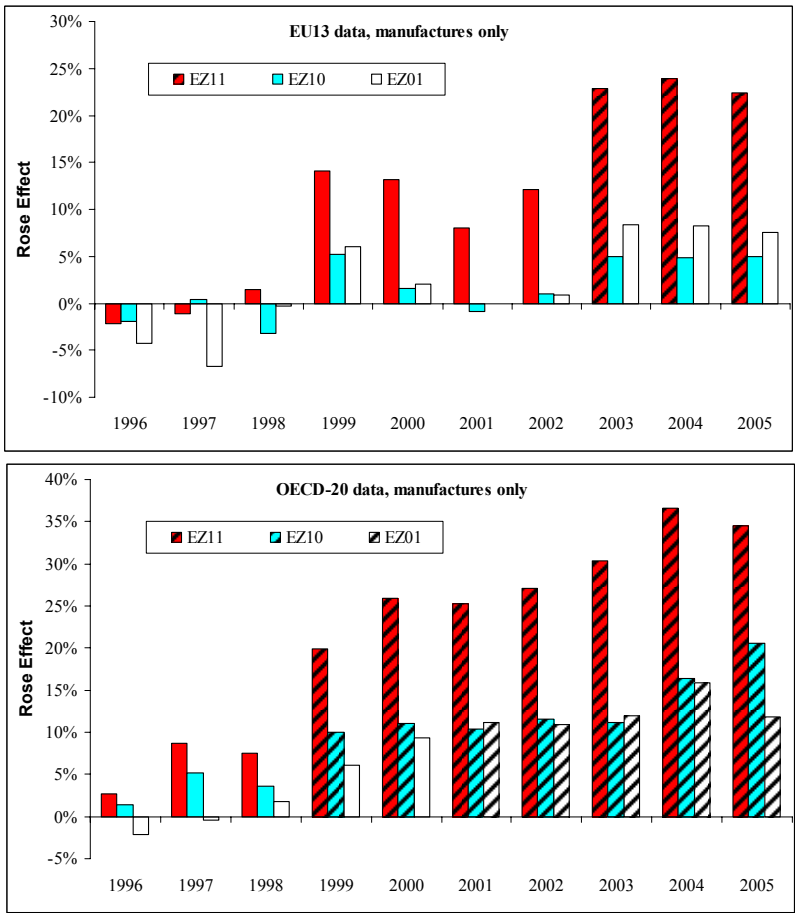
3.2.3. Flam and Nordstrom (2006)

Flam and Nordstrom (2006) updates Flam and Nordstrom (2003) by using more recent data. Furthermore, they provide Rose-effect estimates for parts-and-components trade as opposed to trade in final goods, to study the possibility already flagged in their 2003 paper that the euro is encouraging the fragmentation of production. They also test the extensive margin effect of Baldwin and Taglioni (2004) that Baldwin (2006a) popularised as the 'new-goods' hypothesis (their test results are covered in the Chapter 4 on the extensive margin hypothesis).

The authors estimate bilateral exports using OLS on a panel of data from 1995 to 2005. By starting in 1995, they avoid many of the statistical problems associated with the change in data collection techniques in 1993, and the EU enlargement in 1994. They focus on manufacturing trade only since they believe that big movements in commodity prices cannot be sufficiently controlled for using price indices.

Their estimation technique can be thought of as a difference-in-difference approach, so they have to think carefully about their control group. One set of regressions uses the narrowest control group, namely the members of the EU15 that were not inside the Eurozone, Britain, Sweden and Denmark. Another set uses 7 additional OECD nations. In many ways the narrower group is preferred since they can only imperfectly control for the extensive economic integration that was on-going inside the EU during this period (and continues today), so including the non-EU nations in the sample risks conflating the single market measures and the euro's impact.

Figure 3: Year-by-year estimates of the euro trade effect



Note: Static OLS estimation with pair and time dummies, the standard gravity regressors and a measure of the real bilateral exchange rate.

Source: Authors' manipulation of estimates reported in Flam and Nordstrom (2006).

Rose effect results

The authors' estimates of the annual euro effects are shown in Figure 3. The top panel is for the narrow control group. In both panels the statistically significant estimates are indicated with hash marks inside the columns. The key results are for the EZ11 variable, namely the impact of the euro on trade flows among Eurozone nations (EZ stands for Eurozone and the first '1' indicates that the origin-nation uses the currency while the second '1' indicates that the destination-nation uses it). On the broader data set, they find that the euro's effect jumps up and becomes significant in 1999. In the narrower data set, they find that the estimated euro effect jumps up in 1999, but only becomes significant when the paper currency is introduced in 2002. Since the authors work with bilateral exports, they can estimate the impact of euro usage on the Eurozone's imports and exports from non-EZ nations. The estimated coefficient on imports is denoted as EZ01 (the '0' indicates that the origin-nation does not use the euro while the '1' indicates that the destination-nation does). The symbol for the coefficient on Eurozone exports is EZ10. In the narrow control group data set, neither of these is statistically significant although they are almost always positive during the euro period. Note that these results are driven only by the exports of Britain, Sweden and Denmark to the Eurozone nations, so there may be too little variation to get accurate estimates. The results on the broader control group suggest that the euro boosted the Eurozone's imports and exports.

Monetary versus currency union

The authors also estimate the average euro effect by including euro dummies for two periods, the electronic euro, 1999-2001 (when Eurozone nations were in a monetary union but not yet a currency union), and the paper euro, 2002-2005 (full currency union). They simultaneously estimate the EZ11, EZ10 and EZ01 dummies on the two data sets. Their findings, which are again marred by the 'Anderson-van-Wincoop misinterpretation' suggest that the euro boosted intra-Eurozone trade by 10% in the first period and 19% in the second using the narrow control group (17% and 26% in the OECD control group). The EZ10 and EZ01 dummies are not significant in the narrow data, but range from 7% to 12% in the other data set.

Table 1: Rose effects by stage of processing

	Aggregate trade	Raw materials	Semi-Finished	Finished
13 EU nations sample				
EZ11p1	0.114***	-0.040	0.177***	0.119***
EZ11p2	0.187***	-0.064	0.201***	0.217***
EZ10p1	0.032	0.091	0.094**	0.024
EZ10p2	0.052	0.078	0.045	0.063
EZ01p1	0.053	-0.299***	0.106*	0.061*
20 OECD nations sample				
EZ11p1	0.165***	0.061	0.212***	0.190***
EZ11p2	0.232***	0.001	0.246***	0.313***
EZ10p1	0.074***	0.083	0.099***	0.083***
EZ10p2	0.113***	0.049	0.101***	0.155***
EZ01p1	0.085***	-0.036	0.129***	0.092***
EZ01p2	0.120***	-0.084*	0.189***	0.151***

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%.
'p1' and 'p2' indicate the 1999-2001, and 2002-2005

Sector-level evidence

One interesting exercise undertaken by this paper is to estimate the euro's trade effect level of processing, namely raw materials, semi-finished and finished products . Using their narrow sample of countries, for which the control group is more closely matched with the treatment group, they find that a positive and significant Rose effect for semi-finished and finished products but not for raw materials, i.e. their EZ11 dummy is positive for both the monetary and full currency union periods (1999-2001 and 2002-2005). The effect, however, is only sporadically significant for

Eurozone exports to and import from outsiders (the EZ10's and EZ01s) and strongly negative for imports from outsiders in the second period. In their broad sample of nations (which makes it much more difficult to separate the impact of ongoing Single Market integration and the euro) they find a positive and significant Rose effect for semi-finished and finished products but not for raw materials in both periods. They suggest that the large and positive Rose effects they find for aggregate exports should be attributed to effects on trade in semi-finished and finished products. Again, that all of these regressions are upward biased, since they suffer from the Anderson-van-Wincoop misinterpretation. Moreover, one may wonder what sort of raw materials the Eurozone nations import from each other and whether the idiosyncrasies of agricultural markets and the Common Agricultural Policy (CAP) are driving the results.

Table 2: Flam and Nordstrom (2006) sectoral estimates, euro trade effect

	EZ11p1	EZ11p2	EZ10p1	EZ10p2	EZ01p1	EZ01p2
Mining & quarrying of energy producing materials	-0.881	0.772	-0.491	0.463	-0.106	0.54
Pharmaceuticals	0.265	0.586	0.211	0.386	0.095	0.345
Motor vehicles, trailers & semi-trailers	0.221	0.482	0.148	0.489	0.189	0.49
Other transport equipment	0.221	0.482	0.148	0.489	0.189	0.49
Wood & gds of wood & cork	0.289	0.422	0.066	0.307	0.245	0.343
Radio, television & communication equipment	0.153	0.351	-0.019	-0.001	0.129	0.311
Fabricated metal gds, ex. machinery & equipment	0.181	0.31	0.081	0.192	0.108	0.188
Other non-metallic mineral gds	0.158	0.254	0.121	0.163	0.282	0.406
Machinery & equipment, n.e.c.	0.183	0.253	0.029	0.084	0.145	0.131
Aggregate	0.165	0.232	0.074	0.113	0.085	0.12
Basic metals	0.286	0.21	0.209	0.154	0.298	0.445
Chemicals excluding pharmaceuticals	0.048	0.178	-0.012	-0.027	0.086	0.253
Pulp, paper & paper gds	0.165	0.169	-0.016	-0.046	0.101	0.121
Rubber & plastics gds	0.198	0.169	0.122	0.109	0.202	0.143
Medical, precision & optical instruments	0.127	0.166	0.036	0.095	0.017	0.021
Furniture; manufacturing, n.e.c	0.143	0.151	0.089	0.105	0.144	0.238
Electrical machinery & apparatus, nec	0.135	0.15	0.097	0.111	0.059	0.153
Coke, refined petroleum gds & nuclear fuel	0.045	0.135	-0.318	0.237	-0.007	0.268
Printing & publishing	0.094	0.116	0.119	0.011	-0.072	-0.13
Agriculture, hunting, forestry & fishing	0.062	0.092	0.002	0.115	-0.008	-0.044
Office, accounting & computing machinery	0.097	0.084	-0.093	-0.015	0.212	0.215
Food gds, beverages & tobacco	0.069	0.05	-0.009	-0.009	0.04	-0.096
Textiles, textile gds, leather & footwear	0.011	0.027	-0.029	-0.083	0.004	-0.086
Mining & quarrying ex. energy producing materials	-0.112	0.01	0.013	-0.021	-0.294	-0.165

Source: derived from estimates reported in Flam and Nordstrom (2006). Aggregate is the estimate for all trade, included for comparison.

Flam and Nordstrom (2006) also looks at more finely disaggregated results (Table 2). The sectors are arranged in order of decreasing EZ11p2 coefficients, i.e. the size of the euro's trade effect after the euro cash was introduced (the table includes the aggregate estimates for reference) and

correspond to the broader data set. Estimates that are significant at the 5% level are highlighted. Recall however that these estimates are marred by the 'Anderson-van-Wincoop misinterpretation' and they may be conflating EU economic integration policies with adoption of the euro since they are based on the broader data set and only control for the myriad of ongoing Single Market integration measures with a digital dummy. Be that as it may, causal inspection of the table suggests that that most of the sectors with above-average euro trade effects are those marked by scale economies and/or imperfect competition while there is very little impact in primary industries.

3.2.4. Flam and Nordstrom (2007)

The most recent paper by Flam and Nordstrom updates the aggregate estimates performed by these authors' earlier papers by including data for 2006, however in this latter study they only report results for the broad dataset of countries, where gradual implementation of Single Market measures are in danger of being conflated with the euro's effect. The paper also looks at FDI but we deal with that in the FDI chapter, Chapter 6.

Keeping the Rose effect biases in mind, the main results are shown in Table 3 (results for the standard gravity model controls are omitted for brevity). The authors find rather large Rose effects – 17% for the first period and 28% for the second – but this may be related to biases in their estimator. This impression is reinforced by the fact that they find no impact of Single Market measurement. That is, the trade within the Single Market and the trade between the Single Market and the rest of the world is estimated to be no greater than trade among nations from the rest of the world. In short, they find that the Single market had no effect on trade at all – a finding that would be rather remarkable if true, given the enormous political and economic interest in Single Market policies both inside and outside the EU.

Part of the shortcomings of these results may fall to the fact that they try to control for Single Market integration with a time-invariant dummy when in fact the integration is continually progressing and not in a linear fashion. For example, the European Commission keeps a scorecard of the number of Single Market Directives that the various member states have implemented. This shows that Single Market integration varies both across years and across member states. Part of the large size of their Rose effect estimate would therefore seem to be conflating the euro's and Single Market's impact on trade.

Table 3: Aggregate Rose effects (data on 20 OECD nations, 1995-2006).

Eurozone membership impact		Single Market membership impact	
EZ11, 1999-2001	0.161***	SM11, 1999-2001	0.009
EZ11, 2002-2006	0.248***	SM11, 2001-2006	-0.044
EZ10, 1999-2001	0.075**	SM10, 1999-2001	0.027
EZ10, 2002-2006	0.111**	SM10, 2001-2006	-0.04
EZ01, 1999-2001	0.080**	SM01, 1999-2001	0.093*
EZ01, 2002-2006	0.134***	SM01, 2001-2006	0.094

Source: Flam and Nordstrom (2006), Table 1.

Exchange rate variable problems

The ad hoc approach to exchange rate adopted in their 2003 paper is reproduced here, as it was in their 2006 paper. A worrisome sign is that the point estimates jump around radically in the different papers moving more than 10 standard errors from the 2003 estimates. This hints at a serious mis-specification of some type. For example, in the 2003 paper the bilateral real exchange rate had a point estimate of -1.058 which becomes -0.189 in the 2006 paper and -0.125 in the 2007 paper (all significant at better than the 1% level of confidence). For the third-nation real exchange rate, which they call the 'competitors' RER in this paper, the 2003 point estimate of 0.722 switches to -0.297 in

the 2006 paper and back to positive, 0.399, in the 2007 paper; again all estimates are significant at 1% or better.

3.2.5. Bun and Klaassen (2007)

Bun and Klaassen (2007) updates their earlier paper, Bun and Klaassen (2002). Unfortunately their results are spoiled by the silver medal mistake. This is somewhat surprising since their earlier paper, Bun and Klaassen (2002) is one of the few early studies to avoid the silver medal mistake.

The main innovation of Bun and Klaassen (2007) is to include pair specific time trends in the gravity equation – justifying this loosely on the existence of time-varying "country and country-pair specific ones, such as factor productivity, capital/labour ratios, transportation costs and tariffs." This is very much in the spirit of Berger and Nitsch (2005), which includes a common time trend. There is surely something appealing to the logic that estimation of the euro's trade effect is conflated with other integration policies since EU integration is an on going process. As Berger and Nitsch (2005) argue, the adoption of the euro is "a continuation, or culmination, of a series of policy changes that have led over the last five decades to greater economic integration among the countries that now constitute the [Eurozone]." This Eurozone-as-a-continuation idea suggests that it is hard to separate the euro trade effect from the effects of other integration initiatives. It is, however, too blunt just to throw in a time trend. Not surprisingly, Bun and Klaassen (2007) find, in line with Berger and Nitsch (2005) that the time trend makes the euro effect disappear.

Whatever merit there may be to throwing time trends into a structural equation is negated by the Bun and Klaassen (2007) use of the wrong left-hand side variable; the log of the sum of bilateral trade flows instead of the sum of the logs. When they check for the impact of the silver medal mistake – by redoing their preferred regression using directional bilateral trade, their results fall apart.

Specifically, their main results (using the mis-measured bilateral trade variable) are shown in their table 2. First they reproduced the results from the early literature (all of which are vitiated by well known problems[14]) by finding estimates of the euro trade effect equal to 51% and 18% for their 1967-2002 and 1992-2002 data sets, respectively. When they include their time trends, the estimates fall to 3% and 2% for the respective data sets.

This is the main point of their article – including trends brings down the estimate. However, when they do this on the correctly calculated trade flows (using direction-specific exports), they find that the euro's impact is insignificant in both data sets. In short, correcting for the silver medal leads them to find that there is no euro effect.[15] Since this is exactly the conclusion of Berger and Nitsch (2005), the value added of Bun and Klaassen (2007) is uncertain.

One interesting feature of the article is a neat time-series graph that illustrates the importance of accounting for the 'gravitational inconstant' term in (6), i.e. the 'Anderson-van-Wincoop misinterpretation'. The model Bun and Klaassen (2007) estimate is:

$$\ln(V_{odt}+V_{dot}) = \beta_1 \ln(E_{dt}Y_{ot}) + \beta_2 \ln\{(E_{dt}Y_{ot})/(Pop_{dt}Pop_{ot})\} + \delta_1 EZ_{odt} + \delta_2 FTA_{odt} + \delta_3 Pair_{od} + \sum_t \delta_{4t} DT_t + \delta_5 Trend_{odt}$$

using the notation introduced above together with FTA for a free-trade-agreement dummy.

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[14] See Baldwin (2006a).

[15] Curiously, their discussion in the text does not match the findings in the table. They write: "we use real exports instead of trade as dependent variable (following Bun and Klaassen, 2002) … The estimated euro effects become 0.01 and−0.00. They are similar to our baseline estimates, although they tend to be somewhat lower." In fact, neither is statistically different to zero so they are in no way "similar to our baseline estimates."

Comparing this to the theory-based gravity model (6), we see one small deviation and two important ones. The small one is the inclusion of the per-capita income (which does appear is some derivations of the model that allow for non-homogenous demand structures). The two big deviations are the silver medal mistake as mentioned (they use the log of the sums rather than the sum of the logs on the left-hand side), and the omission of the 'gravitational inconstant' term involving the time-varying and country-varying terms.[16]

They estimate this – excluding their pair-specific 'trend' variable – and plot the average residual for all Eurozone pairs. What they find is a clear upward trend in the average residual for the 1967 to 2002 data sample. Looking at the gravity model, (6), we can take this as evidence that the average 'gravitational inconstant' for the Eurozone nations has been falling during this period – direct evidence that proper estimation of the euro effect requires one to allow time-varying country dummies as suggested by the theory. In short, they graphically illustrate the the relevance of the estimation biases that the 'Anderson-van-Wincoop misinterpretation' might have on point estimates and standard errors in a static OLS approach.

3.2.6. De Nardis, De Santis and Vicarelli (2007)

De Nardis, De Santis and Vicarelli (2007) is an update of de Nardis and Vicarelli (2003). The focus of the paper is to apply sophisticated dynamic panel techniques to the estimation of the euro effect. Unfortunately their results are difficult to interpret since their work is marked by the 'Anderson-van-Wincoop misinterpretation' that is so common in the literature.

To see this, recall that bilateral trade flows are affected by relative prices in the importing nation (P_d) and the market access in the exporting nation (Ω_o). Since these involve prices and expenditures, they are time varying and almost surely serially correlated. Or to put it in Anderson and Van Wincoop (2003) terms, both components of the 'multilateral trade resistance' term is both pair-specific and varies over time. Since De Nardis, De Santis and Vicarelli (2007) do not control for these variable in their model, it ends up in the residuals of their regressions ensuring that the residuals are serially correlated. Indeed the authors test for first-order serial correlation and detect it – a finding that is fully expected after having seen the residual plots in Bun and Klaassen (2007).

Since De Nardis, De Santis and Vicarelli are assuming that the 'gravitational inconstant' term is constant, they interpret the serial correlation as evidence that a classic lagged-adjustment process is in operation.[17] To account for this, they apply sophisticated dynamic panel techniques to estimate the euro's trade effects. The results are clearly biased; their variable-of-interest is in the omitted Ω and P terms, so the residual is correlated with the variable-of-interest.

The instrumenting that they do with the two-stage GMM technique seems to be for the lagged left-hand-side variable, namely $\ln(V_{odt-1})$, but it is not clear whether it instruments for the contemporaneous correlation between the euro dummy and the omitted Ω and P variables. The model they work with also seems flawed in another, inexplicable but probably minor way; they use the log of the sum of the partner GDPs rather than the log of the product. Keeping all these econometric flaws in mind, the paper reports a euro effect of around 4 to 5%.

3.2.7. Gil-Pareja, Llorca-Vivero and Martınez-Serrano (2008)

This little known paper uses the gravity model on data for 25 OECD nations over the 1950-2004 period to estimate the euro trade effect along with the trade impact of a host of other trade and monetary arrangements.

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[16] As often happens in the literature, they use an inappropriate deflation of GDP but offset the error by including a time dummy; see Baldwin (2006) on this 'bronze medal' mistake, or Baldwin and Taglioni (2006) for how the time dummy corrects it.

[17] They justify lags in the gravity model on the basis of 'beachhead costs', i.e. sunk market entry costs of the type introduced by Baldwin (1988).

Table 4: Data sources and manipulations.

Variable name	Plain language definition	Exact definition	Source & units
v_{od}	Value of exports from o to d	For most nations there are 2 observations for bilateral exports, one from the exporting nation's statistics and one from the importing nation's statistics.	IMF DOTS; units: current USD
m_{od}	Value of imports by d from o.	For most nations there are 2 observations for bilateral imports, one from the exporting nation's statistics and one from the importing nation's statistics; 'm' denotes the value of imports.	IMF DOTS; units: current USD
lv_avg	Wrongly averaged bilateral trade (silver mistake)	$\ln\{(v_{od}+v_{do}+ m_{od}+m_{do})/4\}$ As noted above, usually there are 4 data series for bilateral trade; exports from o to d measured by nation-o and nation-d, and exports from d to o measured by nation-o and nation-d.	authors' calculations; units: current USD
lvus_avg	Wrongly averaged bilateral trade; wrongly deflated by US price index (silver & bronze mistake)	$\ln\{1/cpi\_us\}+lv\_avg$ Following common practice in the literature, this deflates the current dollar price value of trade by a US price index.	OECD for US price index (CPI).
lv_prd	Correctly averaged value of bilateral trade.	$\ln\{ (v_{od} \cdot v_{do} \cdot m_{od} \cdot m_{do})^{(1/4)} \}$	authors' calculations; Units: current USD
Lry	Log of the product of real GDP.	$\frac{e_{nc/\$} GDP_o^{nc_o}}{P_o^{gdp}} \times \frac{e_{nc/\$} GDP_o^{nc_o}}{P_o^{gdp}}$	IMF; units: current USD.
Ld	log of bilateral distance.	Geodesic (great circle) distance between capitals,	CEPII; units: kilometres

Notes: USD = US dollars; DOTS = Direction of Trade Statistics.

All the results in the main body of the paper suffer from the 'Anderson-van-Wincoop misinterpretation' since they rely on pair and time fixed effects (or random effects). This imparts the usual bias but it is especially marked given the enormously long data sample. The 54 year sample also means that the results suffer from a form of omitted variable bias. For example, the main results include a dummy for EU membership, but the EU went from a nascent customs union to the most economically integrated region in the world during the sample period. Since the euro was introduced at the end of this period, the failure to account for the time-varying impact of EU membership surely means that they are conflating some the standard trade integration measures with the euro's impact. In a sense, Gil-Pareja et al (2008) are the polar opposite to Berger and Nitsch (2005). Instead of throwing in a time trend, they assume that EU membership implied an unchanging degree of integration from 1958 to 2004.

This problem is made even worse by the fact that they choose a very wide 'control group', one that includes nations as diverse as Turkey, Mexico, Iceland and Germany. This is a problem since the estimation implicitly assumes that the unobserved factors governing bilateral trade for Mexico in the 1950s have the same distribution as those governing trade between Germany and Austria in the new century. To the extent that members of the Eurozone are likely to have adopted many unobserved trade-boosting policies that Mexico and Japan did not, their point estimate on the euro will be over estimated.

The result of these problems is perfectly evident in their results. In their main estimates, the euro trade effect range from 38% to 71%.

The authors do try to correct for the 'Anderson-van-Wincoop misinterpretation' by using time-varying country dummies and fixed effect (the fourth column in their table 4). Since they fail to correct for the time-varying nature of the included and omitted measures of economic integration, they are still conflating the impact of gradual EU integration and the euro's pro-trade effects. This probably explains their enormously high estimates ranging – all above 60%.

Table 5: Dummies and gold and bronze medal mistakes, panel estimation.

Mistakes:	(1) Gold, Silver & Bronze	(2) Gold & Silver	(3) Partial Gold, Silver & Bronze	(4) Partial Gold & Silver	(5) Partial Gold & Silver
OLS estimates (fixed effects):	(none)	(Time Only)	(Pair Only)	(Time & Pair)	(Nation & Time)
Eurozone dummy, EZ11	**0.01**	**0.17**	**0.05**	**0.10**	**0.26**
	0.87	0.00	0.00	0.00	0.00
Lry	**0.77**	**0.77**	**0.37**	**1.13**	**1.18**
	0.00	0.00	0.00	0.00	0.00
EU	**0.21**	**0.18**	**0.09**	**0.03**	**0.21**
	0.00	0.00	0.00	0.12	0.00
Ld	**-0.76**	**-0.76**			**-0.86**
	0.00	0.00			0.00
locked_o	**-0.34**	**-0.34**	**2.81**	**-3.03**	
	0.00	0.00	0.00	0.00	
locked_d	**-0.12**	**-0.11**	**2.80**	**-0.13**	
	0.16	0.21	0.00	0.36	
Contig	**0.32**	**0.30**			**0.17**
	0.00	0.00			0.00
lang_off	**0.58**	**0.59**			**0.47**
	0.00	0.00			0.00
_cons	13.8	-13.8	-0.6	-36.9	-33.4
	0	0	0.534	0	0
Number of observations	2431	2431	2431	2431	2431
R^2	0.92	0.92	0.99	1.00	0.95

Notes: p-values under the point estimates. The nations in the sample are the EU15 plus the Australia, Canada, Switzerland, Iceland., Japan, Norway, New Zealand and the USA , . The time period is 1992 to 2002 as in Micco, Stein and Ordoñez (2003). There are no zeros in this dataset since it involves only large nations.
Definitions: EZ11= common use of euro, lry = product of real GDPs in US $, eu = common membership in EU, locked_i indicates nation i is landlocked (i=o for origin, i=d for destination), contig indicates the nations are geographically contiguous, lang_off indicates the pair shares an official langage, and _cons is the constant.

As Baldwin (2006a) argued about the pre-euro literature, such high estimates are entirely lacking credibility. If trade among the euro using countries had in 1999 jumped up by 60% to 70% above that of other non-euro using EU members, we would not need careful econometrics to detect it – a simple data plot would have made it obvious since the other standard determinants of trade have not been that different between the euro area and the rest of the EU15.

3.2.8. Baldwin and Taglioni (2006)

This paper examines the size and sign of the biases introduced by the various econometric flaws discussed above both the theoretical (econometric) and empirical points of view. The basic econometric theory issues have been aired information in 2, so it is not reviewed here. The paper also discusses a number of detailed issues surrounding data construction and manipulation. Here we focus on the paper's gauging of the size of the biases. To provide a standard benchmark, it take as

the reference point Micco, Stein and Ordoñez (2003), using a data set as close to theirs as possible in terms of country and year coverage. See Table 4 for details.

Results

Table 5 shows the main results. The first column includes the gold, silver and bronze medal errors since the bilateral trade data is wrongly averaged (silver) and wrongly deflated (bronze) and the P and Ω terms are ignored (gold). Despite these errors, there is nothing overtly wrong with the estimates; they fall well within most researchers' priors. Although the Rose effect is insignificant, economic mass variables, distance and EU membership have fairly standard point estimates and are significant.

Column two shows what happens with time fixed effects. As discussed earlier in this chapter, this corrects the bronze medal mistake (incorrect deflation of bilateral trade), which was biasing the Rose effect downward. To illustrate that the time dummies actually do correct for the bronze mistake, the authors re-do the column 2 estimation using trade data that has not been wrongly deflated and find the point estimates with time dummies are exactly those in column 2.

As the review of the literature showed, one the most common estimators involves OLS with both time and pair dummies. For example, this is the preferred regression of Micco, Stein and Ordoñez (2003), and it is repeated by all the Flam-Nordstrom papers. The results for the estimation which includes the Anderson-Van Wincoop misinterpretation, are shown in column 4.

As argued above, the time dummies eliminate the bronze mistake and the pair dummies reduce the severity of the gold-medal mistake by eliminating the cross-section correlation between the omitted Ω and P terms and the included variables. Indeed, comparing column 2 (time only) to column 4 (time and pair), we see that the Rose effect estimate falls from 0.17 to 0.10. The point estimates on the economic mass variable also appear more in line with theory at 1.13 instead of 0.77. The pair dummies, however, remove much of the variation that was helping to estimate the EU dummy and the coefficient drops enormously and becomes insignificant at the 10% level in column 4. (The data period includes only three switches in EU status, Austria, Finland and Sweden joined in 1994 and the pair dummy wipes out all the cross-section correlation between membership and bilateral trade). This, of course, is the downside of using pair dummies to estimate the impact of policies that have not varied much over time. Column 3 re-confirms the importance of the bronze medal mistake. With pair dummies only the results seem to be all wrong.

Nation dummies are the next most common correction for the gold-medal mistake (i.e. failure to account for what Anderson and Van Wincoop 2003 referred to as the multilateral trade resistance term). This estimator is shown in column 5 with time dummies included. This estimator controls for another aspect of the correlation between the Ω and P terms and the EZ dummy, but fails to catch the time-series correlation. As a consequence the Rose effect estimate is probably upward biased.

Results for improved estimators

The paper also presents estimates on the same data set when the various econometric flaws are corrected. Specifically, when the estimation avoids the mistake of using real values for trade and GDP data and uses instead nominal values. They ,handle the numeraire issues with time-varying country dummies – one to pick up Ω_o for each nation as an exporter, and one for P_d for each nation as an importer.[18] To soak up the unmeasured omitted variables correlated with EZ membership, they include time-invariant pair dummies. While this involves the introduction of a lot of fixed effect, namely 2NT for uni-direction trade data, where N is the number of nations and T is the number of years, this is handled easily because with a square panel there are 2N(N-1)T observations. If T and N are large, there will be many degrees of freedom, even with T time dummies added on.

[18] That is, each nation as an exporter has a fixed effect that is the same for all of its export destinations for each year.

The results are shown in Table 6 in the right-most column, but before this, we study an estimate that is akin to that of Gil-Pareja, Llorca-Vivero and Martınez-Serrano (2008), which allows the time-varying country dummies to properly correct for the time-varying Ω and P variables, but fails to correct for omitted pair-linked factors that may be correlated with EZ. Looking at the point estimates in isolation, the estimator seems to do a fine job of eliminating biases. All variables are significant at any reasonable level of significant and have the right sign and roughly plausible magnitudes. The point estimate on the economic mass variable is quite close to unity as predicted by the simple theory (and tightly estimated). The point estimate on EU membership has a plausible size of 0.22 implying that intra-EU trade flows are boosted by 24.6%.[19] The other standard explanatory variable, distance, is estimated at -0.93 which is quite close to the traditional prior of -1.0 (although this is not a theoretical prediction). The one point estimate that seems somewhat out of line is the Eurozone impact. At 0.34, or 40%, the figure seems a bit high; it is definitely much higher than the consensus estimate of 5 to 15 percent. This outcome is not unexpected due to the omitted-variable bias discussed several times above.

The final column corrects for this bias by including time-invariant pair dummies in addition to the time-varying nation dummies. This has a radical impact on the Rose effect estimate, turning it negative and statistically significant. This result, however, is somewhat suspect since the pair dummies also greatly reduce the estimated impact of EU members and render it statistically insignificant.

One interpretation of these results turns on the fact that the pair dummies wipe out information in the cross-section variation, so all identification comes from time variation in the variables. Since EU membership varied very little during our period (Austria, Finland and Sweden became members in 1995), it is possible that the regression is having difficulty in distinguishing between the pair dummies which are absolutely time-invariant and the EU membership which is almost time-invariant. As always, the pair dummies absorb all time-invariant determinants of bilateral trade costs such as distance and common language.

Having thoroughly reviewed and critiqued the existing empirical literature, we are ready to proceed to new estimates that apply the latest data and econometric techniques.

[19] The formula is exp(coefficient)-1.

Table 6: Gravity estimates with uni-directional nominal trade and GDPs.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	**Time only**	**Pair & Time**	**Nation & Time**	**Importer, Exporter & Time**	**Time-varying Nation**	**Time-varying Nation and Pair**
Eurozone dummy, EZ11	**0.17**	**0.13**	**0.09**	**0.24**	**0.26**	**0.34**	**-0.09**
	0.00	0.00	0.00	0.00	0.00	0.00	0.00
ly	**0.81**	**0.81**	**0.57**	**0.65**	**0.70**	**0.98**	**0.94**
	0.00	0.00	0.00	0.00	0.00	0.00	0.00
EU	**0.14**	**0.14**	**0.06**	**0.20**	**0.21**	**0.22**	**0.04**
	0.00	0.00	0.00	0.00	0.00	0.00	0.23
locked_o	**-0.26**	**-0.25**	**5.17**				
	0.00	0.00	0.00				
locked_d	**-0.49**	**-0.49**	**-5.87**				
	0.00	0.00	0.00				
ld	**-0.86**	**-0.86**		**-0.93**	**-0.93**	**-0.93**	
	0.00	0.00		0.00	0.00	0.00	
contig	**0.17**	**0.17**		**0.11**	**0.11**	**0.11**	
	0.00	0.00		0.02	0.01	0.02	
lang_off	**0.68**	**0.67**		**0.53**	**0.53**	**0.53**	
	0.00	0.00		0.00	0.00	0.00	
_cons	**-15.50**	**-15.56**	**-7.23**	**-5.75**	**-8.18**	**-23.58**	**-30.77**
	0.00	0.00	0.00	0.19	0.00	0.00	0.00
Observations	4837	4837	4837	4837	4837	4837	4837
R^2	0.88	0.88	0.99	0.91	0.93	0.91	0.99

Notes: The dependent variable is uni-directional bilateral trade with no deflation; ly = product of GDPs in current dollars (no deflation). All other variables are as in the previous table. p-values under the point estimates.

4. UPDATING THE ROSE-EFFECT ESTIMATES

There are two sorts of problems with the existing estimates – econometric flaws and problems with the policy proxies. The first is easily fixed. The second requires some more thinking.

4.1. Problems with the policy proxies

4.1.1. The euro's accompanying integration measures

The euro was not a once -and-forever change in the European trading environment. Leaving aside the gradual adjustment of firms to the changes, the actual policy implementation itself was gradual. Most obviously, the Eurozone was only a monetary union from 1999 to 2001 becoming full monetary and currency union from January 2002 with the introduction of notes and coins. But there

is much more to it. Here we cover some of the details of just two major initiatives aimed at facilitating transactions in euro – measures that almost surely affect that cost of intra-Eurozone trade and investment, yet were phased in gradually.

Single Euro Payments Area (SEPA)

In modern commerce, using a common currency means much more than just paying with the same cash. Most payments are made electronically and the cost of this is determined by many policies ranging from international payment systems to banking competition.

As ECB (2006) notes, "Currently, the euro area economy is unable to exploit fully all the benefits of Monetary Union. Customers face difficulties when making euro retail payments to other euro area countries, as these payments often turn out to be more time-consuming. As long as this is the case, the euro cannot be viewed as a fully implemented single currency." Substantial facilitation has been achieved for large-value payments via Target (described below), the lower value electronic payments suffer from a myriad of payment instruments, standards and processing infrastructures. Consequently, companies with lots of cross-border payments often maintain bank accounts in more than one nation. Such fragmentation, ECB (2006) concludes, affects national euro payments as well since it dampens innovation and competition.

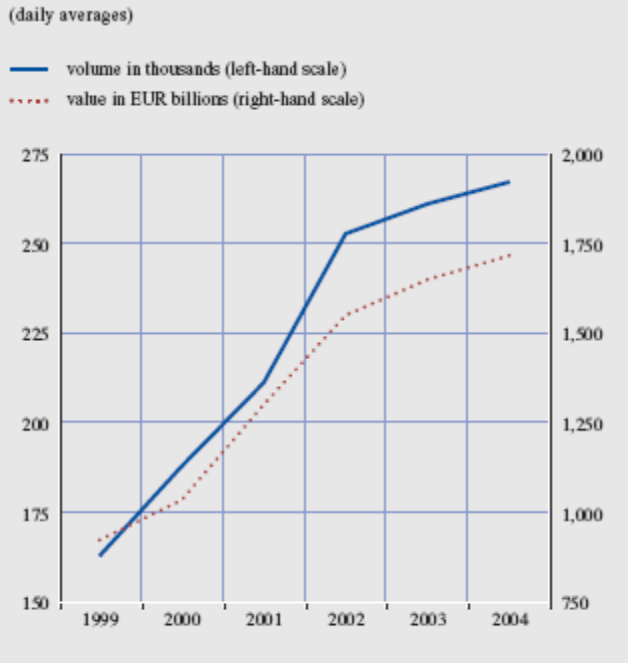
To counter this problems and to amplify the transaction-cost lowering impact of the euro, the so-called Single Euro Payments Area (SEPA) programme has been championed by the European Commission, the European Central Bank, and the European Payments Council (a collection of banks and other interested financial and payments private institutions). The key dates in SEPA's developments are:

- 2000, Lisbon Council of Europe creates a European Financial Services Action Plan as part of the Lisbon Agenda;
- 2001 The EU adopts a regulation that harmonises fees for cross-border and domestic euro transactions (Cross-Border Payments Regulation). Note that an EU Regulation is directly applicable in all Member States. No national implementing legislation is needed;
- 2002 EU Cross-Border Payments Regulation means customers pay no more to withdraw euros from cash machines or make card payments in euros in other EU Member States than they pay for the same services in the country where they live.
- 2003 First pan-European automated clearing house set up, and an EU Regulation concerning small euro transactions is adopted.
- 2008 SEPA pan-European payment instruments become operational in parallel to domestic instruments on 28 January.

TARGET

The Trans-European Automated Real-time Gross settlement Express Transfer system (TARGET) consists of the national real-time gross settlement systems (RTGSs) of the euro area countries and of the ECB payment mechanism; the RTGSs of Denmark, Poland, Estonia, Slovenia and Britain are connected to provide a uniform platform for the processing of euro payments.

Figure 4: TARGET usage, Volumes and values, 1999 – 2001.



The system went live on 4 January 1999, but it has continuously been refined and improved (ECB 2004). For example, the TARGET Information System (TIS) was introduced in October 2000, and upgrades were introduced every 6 to 12 months. In November 2001, the consolidation of large-value payment systems in the euro area progressed when the Deutsche Bundesbank shut down its hybrid system Euro Access Frankfurt and launched RTGSplus as the new German TARGET component. Integration with commercial systems such as the SWIFT standards and IBANs also proceeded gradually. In October 2002, the ECB Governing Council agreed a long-term strategy for further developing the system, the so-called TARGET2. It envisages a technical consolidation of the TARGET system, with a single TARGET-wide pricing structure for intra- Member State and inter-Member State payments, and a harmonised level of service. An oversight framework was established in 2003 and in 2004, three of Eurozone national central banks (Germany, France and Italy) approved the building of a Single Shared Platform for their TARGET2 operations. Most other national Central Banks have agreed to join this initiative.

The notion that TARGET only gradually facilitated payments in euros can also be seen in the evolution of the volume transaction in the system (Figure 4).

4.1.2. Controlling for the Single Market's impact on trade

Since implementation of the Single Market is proceeding in tandem with the introduction of the euro and accompanying policies, it would be easy to conflate the Single Market and single currency trade effects. Distinguishing between them requires an accurate proxy for Single Market measures. This is difficult.

The Single Market was not a once-and-for-all policy change. Of course, on the face of it, the Single Market started with the Single European Act of 1986 and was completed by December 1992. This reading of history – adopted by many authors in literature (including Baldwin and Taglioni 2006) – hides important initiatives that have happened since and in fact are still ongoing. Many of these 'in process' changes are likely to affect EU trade flows, including those among euro-using nations. This suggests that there is a serious possibility that the existing empirical studies of the Rose effect

are conflating unmeasured Single Market integration with euro-usage and its amplification by the measures discussed above.

The Internal Market as a work in progress

In 1985, the European Council decided to push European integration substantially beyond the customs union. It set the end of 1992 as the date for completing the Internal Market and directed the Commission to draw up an action plan. The result was the Commission 1985 White Paper listing about 300 legislative measures. To allow this hailstorm of new laws, EU leaders embraced the reform agenda in the Single European Act and, more importantly, agreed to switch to majority voting so that laws could be passed without undue delay.

When the 1993 deadline arrived, about 90% of the laws had been adopted, but the remaining 10% included a lot of important matters such as abolition of controls on persons, the common statute for European companies, full liberalisation of transport services, and tax harmonisation. It should be noted that - a significant part of the adopted directives were not transposed correctly, as the 1992 Sutherland report demonstrated (Sutherland 1992).

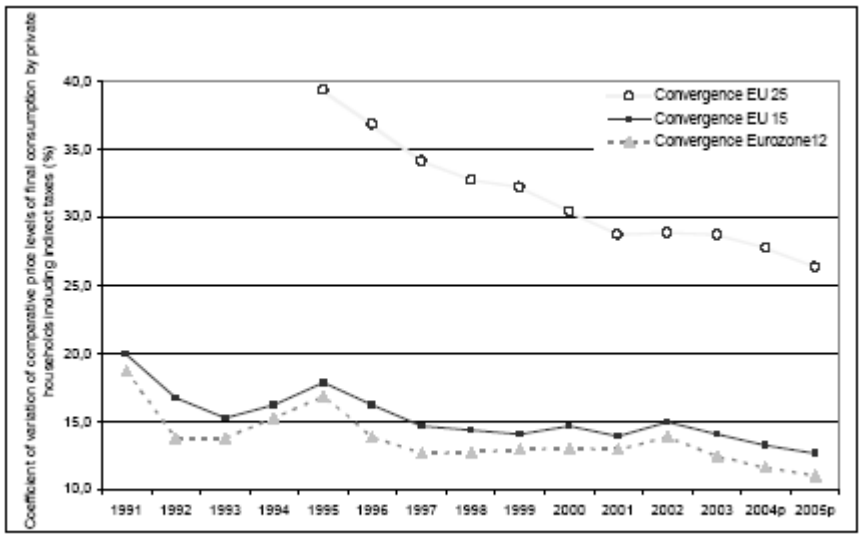
Recognising this shortcoming, and the realisation of the key role of services in Europe's innovation, growth and employment, new reform agendas were drawn up. The list of 'unfinished business' came to include the liberalisation of public service sectors, telecommunications, electricity, gas, postal services and the establishment of trans-European networks. Here are some of the hallmark initiatives, although it is really best to think of the Commission as continually pushing to fulfil the promises made in the Treaty of Rome:

- Commission Communication in June 1993 on improving the effectiveness of the Single Market, and a 'strategic programme' in December 1993;
- A Communication in October 1996 on 'The impact and effectiveness of the single market', and the 'Action plan for the single market' (June 1997);
- 'The strategy for Europe's internal market' (November 1999), which laid down strategic objectives up to 2004 and instituted annual reviews of progress on 'targeted measures';
- The 2000 Lisbon agenda;
- the Commission study (January 2003) 'The internal market – ten years without frontiers', and the matching, Commission Communication 'Internal market strategy priorities 2003-2006'.

The fact that the Single Market is on-going can also be seen in the data. For example,

Figure 5 shows that prices across EU nations have tended to converge substantially since 1993, but the convergence does not follow a straight line. In standard competitive markets, increased integration across markets tends to lead to a narrowing of price differentials, so the decline can be taken as a symptom of on-going integration.

Figure 5: Price convergence in the EU15, EU25 and Eurozone, 1991-2005.



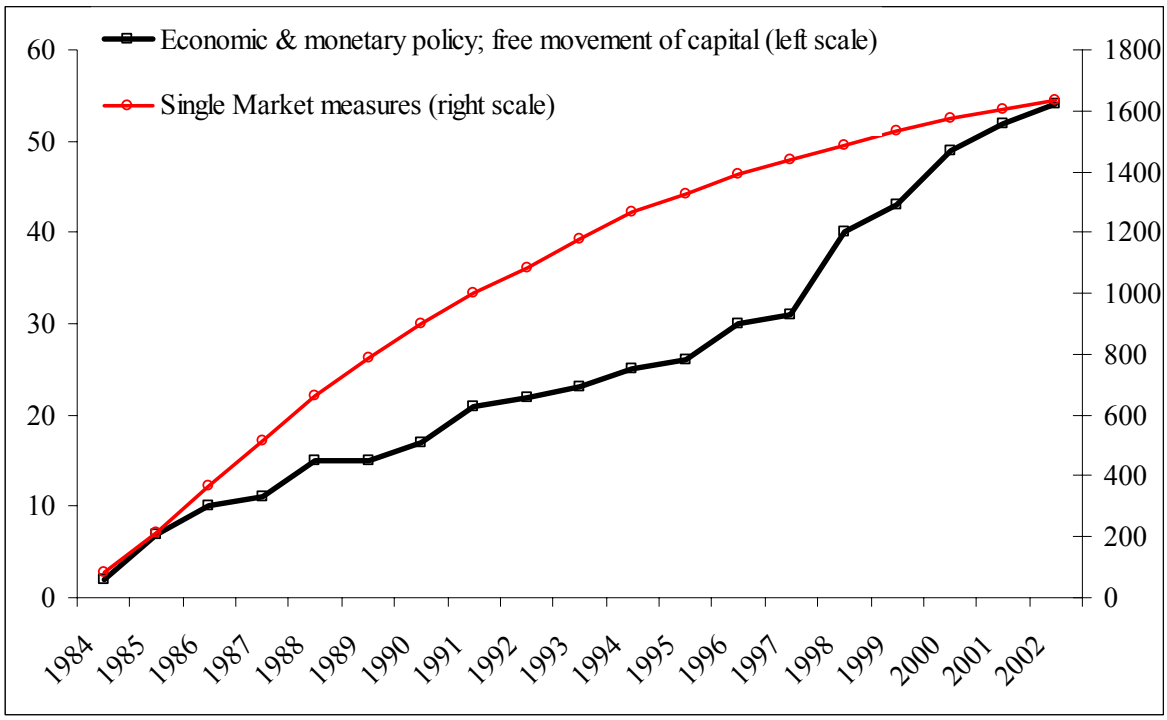
Source: Eurostat Structural Indicators

4.1.3. Direct measures of policy changes

In addition to the discussion of policy reforms and the indirect evidence on the economic impact of things that seem to reflect on-going integration, we also have two direct ways of measuring the policy changes.

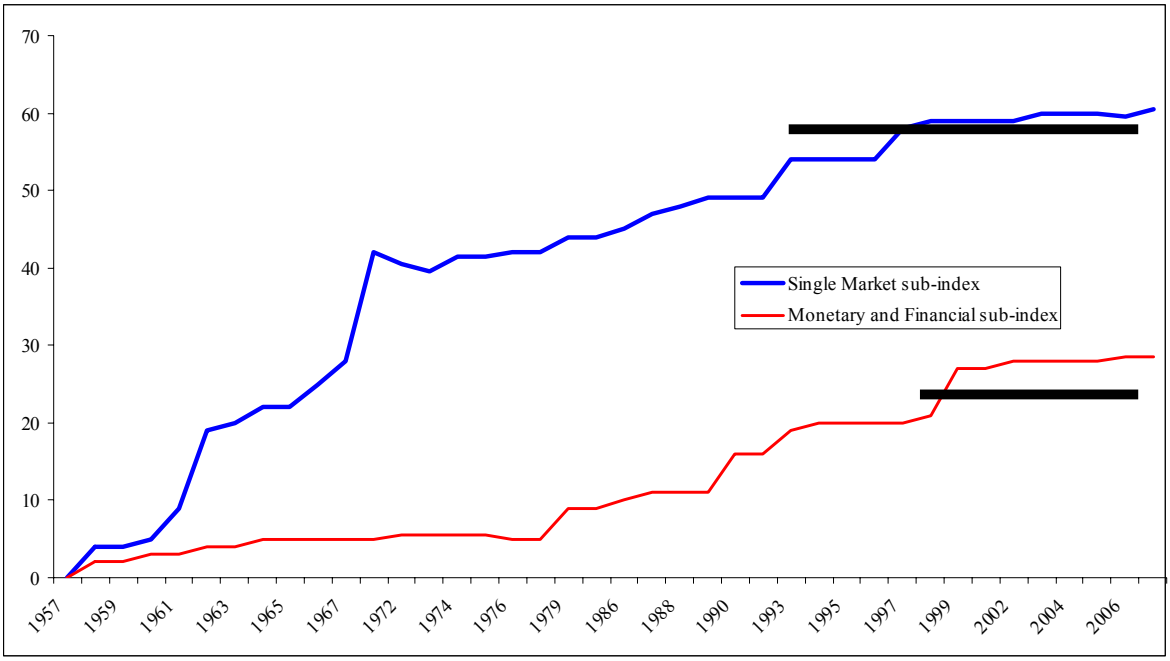
The first way is to look directly at EU legislation. Using a data set put together by political scientist Thomas Koenig (Koenig 2007), we see that the stock of EU laws concerning the Single Market and monetary and financial integration have risen since the 1993, but again not in a linear fashion.

Figure 6: Cumulative number of EU laws on the Single Market and Economic Monetary Union policies, 1984-2003.



The second way is to look at indices of institutional integration developed by economists following European integration. The first is the index by Mongelli et al (2005) shown in Figure 7.

Figure 7: The Mongelli et al index of EU integration: Single Market and Monetary and Financial sub-indices



Source: Mongelli, Dorrucci and Agur (2005) updated by data provided bilaterally by the authors.

4.1.4. Implications for the measurement of the euro's trade effect

The discussion above makes clear the problems one may encounter when proxying for the currency union with a simple digital dummy that becomes 1 from 1999 for bilateral trade flows among Eurozone nations. The same can be said for trying to control for the Single Market's impact with another digital dummy.

The points are made graphically in . Using dummies will leave a time-varying component in the errors that may well bias the Rose effect. This notion is entirely in line with the common result in the empirical literature (e.g. Berger and Nitsch 2005) that authors find a larger euro effect when they use longer data sets. Obviously the longer is the data set, the worse job a time-invariant dummy does in capturing the time-varying policy changes.

4.2. Estimation

Our strategy is to use the Mongelli index to improve the measurement of the two major integration policies operating in the EU over that past 15 years. Specifically, we shall use their Single Market index for the whole period to control for the impact of the Single Market. We shall also use the Monetary and Financial sub-index to improve the euro dummy. Specifically, we interact the standard euro dummies (EZ11, EZ10, EZ01) with the Mongelli index.

4.2.1. Data

Our dataset compromises bilateral goods trade among the EU-15 and the period, or 1996-2006. The details of the variables are given in the table below:

4.2.2. Estimators

Since we are introducing a new policy proxy, we show the results for a wide variety of fixed effects estimators, including those that we know are flawed. This is to allow comparison with the existing literature.

Specifically, two sets of estimations are carried out. One uses the traditional log-gravity specifically and the other uses the Poisson estimator proposed by Da Silva and Tenreyro (2005). This second method is normally used to solve problems of zero trade flows. While in this dataset, the zero trade flows are very small, the method has been showed by Da Silva and Tenreyro to deliver different coefficients than what traditionally found with log-gravity estimations. Each set of estimations proposes several fixed effect combinations. In left-to-right order of the columns in the table, they are: estimation with the 'real variables mistake' and time dummies only, estimation on the theoretically preferred nominal data with time dummies only, with nominal data and importer, exporter and time dummy (i.e. Anderson-Van Wincoop + time dummy), time-varying importer and exporter using log-gravity with nominal data, time and pair dummies with nominal data and finally the theoretically preferred specification time-varying importer and exporter dummies and time invariant pair fixed effects using nominal data for both trade and GDP.

Table 7: Variable definition and data sources.

Dependent variable
Nominal exports = direction-specific value of bilateral exports from country o to country d, current USD (IMF DOT), e.g. for France: $X_{€}$* (USD/EUR)= $X_{USD.}$
Real exports = value of bilateral exports, CPI deflated IFS and WDI, current USD
RHS variables
EZ11 = euro dummy* Index of Monetary and Financial Integration
EZ01 = euro importer dummy* Index of Monetary and Financial Integration
EZ10 = euro exporter dummy* Index of Monetary and Financial Integration
ly_o = Nominal GDP exporter (source: OECD)
ly_d = Nominal GDP importer (source: OECD)
lry_o= Real GDP exporter, CPI deflated (source: OECD and WDI, current USD)
lry_d= Real GDP exporter, CPI deflated (source: OECD and WDI, current USD)
ldistw= Weighted distance (CEPII)
contig= contiguity
comlang_off = common official language
lremot_o = remoteness exporter (GDP weighted average of distance to all other countries in sample)
lremot_d = remoteness importer (GDP weighted average of distance to all other countries in sample)
landlocked_o
landlocked_d
lrber = real bilateral exchange rate
lreer_d = real effective exchange rate destination country
smp_o= Index measuring EU integration in all fields excluding Monetary and Financial, country of origin
smp_d= Index measuring EU integration in all fields excluding Monetary and Financial, country of destination
tdef_o= Index measuring transposition deficit of EU regulations, country of origin
tdef_d= Index measuring transposition deficit of EU regulations, country of destination

4.3. Results

The results for the 1995-2006 period are shown in Table 8. Consider first the correctly specified estimator that takes account of the time-varying aspects of nations' Ω and P. Here the Rose effect is estimated to be positive and highly significant, but small – about 2 percent. Interestingly, we find some evidence of trade diversion since the EZ01 measure of the euro's impact (recall that this is now time-varying according to the Mongelli sub-index) is negative and significant at the 5% level, but very, very small. The impact on the Eurozone's exports to non-euro users, however, is 3% and

highly significant. What this says is that the finding that the euro had no impact or a positive impact on outsiders may have to be revised.

The other estimates, all of which are econometrically flawed, tell a similar story. Where the standard gravity controls can be estimated, they are of the expected sign and significance. This is comforting since it shows that our use of the novel proxy for the euro did not introduce major problems with the equation.

It is also noteworthy that our Single Market proxy has some problems in this very short sample; smp_o and smp_d (dummies variables that switch on when the origin and destination nations are in the Single Market, with these dummies interacted with the Mongelli sub-index) are estimated to have, respectively, a negative and significant effect and a positive but only borderline significant impact.

Table 8: OLS on EU15 data, 1995-2006

	A		B		C		D		E		F	
EZ11	0.04	***	-0.01		0.01		-0.01		0.01	***	0.02	***
EZ01	0.06	***	-0.01	***	0.00		0.00		0.00		-0.01	**
EZ10	-0.03	***	-0.01	*	0.00		-0.02		0.01	***	0.03	***
ly_o			0.69	***					0.2	***		
ly_d			0.76	***					0.68	***		
lry_o	0.71	***										
lry_d	0.62	***										
ldistw	-1.22	***	-1.12	***	-1.18	***	-1.27	***				
contig	0.13	**	0.1	**	0.15	***	0.22	***				
comlang_off	0.38	***	0.42	***	0.18	***	0.09					
lremot_o	-1.6	***	-1.66	***					0.00			
lremot_d	3.49	***	2.35	***					0.00			
landlocked_o	-0.78	***	-0.74	***					1.49	***		
landlocked_d	-0.69	***	-0.7	***					0.63	***		
lrber	-0.12		0.18	***	-0.01		0.39					
lreer_d	-0.97	**	-1.42	***					-0.37	**		
smp_o	0.04	***	0.01	***	-0.01	**	-0.04		-0.01	***	-0.04	***
smp_d	-0.06	***	0.01	**	0		-0.05	*	0.01	*	0.02	*
_cons	-65.06	**	-29.12		30.96	***	33.28	***	-1.82		21.5	***

Notes: A = OLS in real terms using log-gravity and time dummies ; B = OLS in nominal terms using log-gravity and time dummies; C = Importer, Exporter and time dummy (i.e. Anderson-Van Wincoop + time dummy) using log-gravity in nominal terms; D = Time-varying importer and exporter using log-gravity in nominal terms; E = Time and pair dummies using log-gravity in nominal terms; F = Time-varying importer and exporter and time invariant pair using log-gravity in nominal terms.

When we go to longer data sets (Table 9), which should be less of a problem with our time-varying policy proxies than it was for the simple digital dummy approach, we find that the Rose effect estimates are remarkably stable, except now the trade diversion result disappears. Still, we find that euro-usage seems to promote both trade among euro-users and exports from the Eurozone to non-euro using nations. The Single Market proxies are also more in line with expectations, although quite small. It would seem that there is still some work to be done on improving our proxy for the

Single Market. When we re-do these exercise with the Poisson maximum likelihood estimator, we find qualitatively identical results, which we do not show here for sake of brevity.

Table 9: OLS on EU15 data, 1990-2006

	A		B		C		D		E		F	
EZ11	0.02	***	0.01	***	0		-0.03		0.01	***	0.02	***
EZ01	0.02	***	0		-0.01	**	-0.05	***	0		0	
EZ10	-0.01		0		0	**	0.01		0	**	0.01	***
ly_o			0.68	***					0.38	***		
ly_d			0.75	***					0.63	***		
lry_o	0.7	***										
lry_d	0.59	***										
ldistw	-1.21	***	-1.11	***	-1.15	***	-1.14	***				
contig	0.18	***	0.14	***	0.16	***	0.24	***				
comlang_off	0.39	***	0.44	***	0.23	***	0.18	***				
lremot_o	-1.93	***	-1.31	***					0			
lremot_d	4.35	***	2.21	***					0			
landlocked_o	-0.82	***	-0.76	***					-0.81	***		
landlocked_d	-0.72	***	-0.69	***					1.79	***		
lrber	-0.25	***	0.15	***	-0.03		0.2					
lreer_d	-1.6	***	-1.06	***					-0.44	***		
smp_o	0	***	0	***	0	*	0.02	***	0	*	0.01	***
smp_d	0		0	***	0		0.02	***	0		0	
_cons	-81.24	***	-36.61	**	28.69	***	27.46	***	-2.91	*	20.41	***

Notes: A = OLS in real terms using log-gravity and time dummies ; B = OLS in nominal terms using log-gravity and time dummies; C = Importer, Exporter and time dummy (i.e. Anderson-Van Wincoop + time dummy) using log-gravity in nominal terms; D = Time-varying importer and exporter using log-gravity in nominal terms; E = Time and pair dummies using log-gravity in nominal terms; F = Time-varying importer and exporter and time invariant pair using log-gravity in nominal terms.

5. CONCLUDING REMARKS

This chapter has reviewed and synthesized the massive research effort that has been made in quantifying the euro's impact on trade. It also provides new evidence using the latest data and empirical techniques. The bottom line is that some form of 'Rose effect' happened. Trade among euro using nations is greater than it would have been without the euro. The impact on non-euro area nations, however, is less clear beyond the solid fact that it has not led to trade diversion.

The cleanest estimates are provided in this chapter. They suggest that aggregate trade was boosted by about 2%. Of course, it is a vast oversimplification to talk about 'the' impact of the euro on trade. Much evidence suggests that it is quite different across sectors. Many estimates also suggest that it is different across member states, but there is really not enough data to firmly establish such differences in a credible fashion.

APPENDIX: THE LIMITATION OF ANDERSON AND VAN WINCOOP (2003) TO PANEL DATA

The last step in the Anderson-Van-Wincoop derivation of their model does not work for panel data, so it is not directly relevant to the euro trade effects literature. Anderson-Van-Wincoop assert that $\Omega_i=P_i^{1-\sigma}$ for all nations since $\Omega_i=P_i^{1-\sigma}$ is a solution to the system of equations that define Ω and $P^{1-\sigma}$. Their point can be seen from the definition of the price index which yields

$$\Omega_o = \sum_k \tau_{ok}^{1-\sigma} \frac{Y_k}{\Delta_k} \qquad \Delta_o = \sum_k \frac{Y_k}{\Omega_k} \tau_{ko}^{1-\sigma} \qquad \forall \quad o = 1,...R$$

By inspection, the two definitions would continue to hold if $\chi\Omega_i=\Delta_i$, for any χ, as Anderson-Van Wincoop observe in their footnote 12 of the published paper. What this tells us beyond a doubt is that any set of Ω and $P^{1-\sigma}$ that solves this set of equations must be proportional.

This proportionality is obviously correct and indeed intuitively obvious. Since Ω measures the openness of the world to a nation's exports and $P_i^{1-\sigma}$ measures the openness of a nation to imports from the world, these two will be related when all bilateral trade costs are symmetric. If nation-o finds itself located in a place that has good market access (which makes exporting easy), then it will automatically be in a place where foreign exporters find it easy to sell into nation-o. The authors go beyond proportionality, claiming that the two are actually equal. The text asserts that the point is "easily verified." This is elaborated upon in footnote 12, which goes on to say that $\chi\Omega_i=\Delta_i$ and claims that taking $\chi=1$ is 'a particular normalisation.'

Here we show that $\chi=1$ cannot be a solution in general unless trade costs never vary. Since the Anderson Van Wincoop method is used for panel data, we can be sure that trade costs are varying in which case we cannot take $\Omega_i=P_i^{1-\sigma}$.

The Anderson-Van Wincoop model is difficult to manipulate since it is basically a CES expenditure system with market clearing conditions imposed. There are two basic problems. The first stems from the high dimensionality of the system. For example, with just 3 nations there are 3 expenditure equations for each nation as well as the definitions for the three Ω's and the three P's, and the three adding up constraints. Second, even given endowments and trade costs, it is mathematically impossible to solve for prices and the trade pattern with paper and pencil (the problem is non-integer powers). Given this, one cannot directly demonstrate that $\chi\neq1$ by finding χ and showing it is not unity. Instead, we offer a counter example which disproves the general rule and explains why Anderson and Van Wincoop's fourth step is correct for cross-section applications of their equation but incorrect for panel-data applications.

If $\chi=1$ is the solution, then it must work for all cases, including a simple one. What we do here is show that $\chi=1$ cannot be the general solution in the simplest possible case – namely, 3 identical nations with a single factor of production, and bilateral trade costs that are identical for every trade flow. In this case, the definition of Ω is (symmetry of nations allows us to drop subscripts):

$$(1) \qquad \Omega = \frac{Y}{P^{1-\sigma}}(1+2\tau^{1-\sigma}) \qquad \leftrightarrow \qquad \Omega = \sqrt{Y(1+2\tau^{1-\sigma})}$$

where the second expression follows by imposing $\Omega=P^{1-\sigma}$.

The problem is that this is inconsistent with a typical nation's market clearing condition. To make the point as simply as possible, we assume, as in Anderson and Van Wincoop (2003), that nations make a single good under conditions of perfect competition and constant returns; we also assume that nations are endowed with a single factor of production, L. Thus the typical nation's income is Y=wL where w is the typical nation's wage and from perfect competition the price of its good is p=wa, where 'a' is the unit labour input coefficient.

Using perfect-competition pricing namely p=wa, the definition of income Y=wL, and (1), the market clearing condition for the typical nation, namely $p^{1-\sigma}=Y/\Omega$, can be written as:

$$(wa)^{1-\sigma} = \frac{wL}{\sqrt{wL(1+2\tau^{1-\sigma})}}$$

The key point in the counter example is that if we take labour as numéraire, so w=1, then this holds but only if we choose to measure units of labour in a way such that 'a' exactly equates the left-hand side to the right-hand side. More to the point, once we chose units for labour, then this will only hold if there is no change in bilateral trade costs and no change in GDPs. The same point holds if one takes the typical goods price, p, as the numéraire. We have worked out more general examples numerically (using Maple) and we always find that the Ω and $P^{1-\sigma}$ are proportional regardless of the GDPs and bilateral trade costs, but the factor of proportionality depends upon GDP's and trade costs.

This shows that setting $\Omega=P^{1-\sigma}$ does indeed involve 'a particular normalisation' but we need a different normalisation for every set of GDPs and trade costs. In other words, Ω does not equal $P^{1-\sigma}$ in data that has a time dimension.

Chapter 3: Trade pricing effects of the euro

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1. INTRODUCTION

Intuitively, there are two channels through which the euro could theoretically affect trade pricing between euro-using nations. The first is the direct effect on variable trade costs. For example, if a common currency lowered transaction costs by, say 1%, the price of exports from France to Germany could be reduced by 1% without the exporters changing their price-cost margin. Hedging and administrative costs related to multiple currencies also fall in this direct-effect channel, although the have to affect marginal costs if they are to feed directly into pricing decisions. The second channel is more indirect; it concerns market structure and firms ability to price discriminate. Whenever firms are able to segment their consumers, they tend to charge different prices to different customers in pursuit of profit. This happens domestically – for example, as with lower movie theatre prices for students and retired people – but also internationally. To the extent that the euro moves the Eurozone toward one great big market (one market, one money, to coin a phrase), the euro should diminish firm's ability to segment their customers in different Eurozone markets. Such an outcome would clearly change the way Eurozone-based firms would price their exports in various Eurozone markets. A related mechanism could come from the often-cited transparency effect. That is, as customers are more easily able to compare prices across markets, they may be come more price-sensitive. Such a change could reduce the variance in prices across Eurozone markets, but it could also put overall downward pressure on export prices. Transparency, however, is not the only way in which the euro could affect price-cost margins in the Eurozone. To the extent that common euro usage increases market integration, the euro could well have a direct pro-competitive effect in the Eurozone markets.

In this chapter we first review the basic economics of pricing as a means of organising our thinking before turning to the existing literature. Subsequently we look at the prima facie evidence and then we derive a simple theoretical framework and use it to estimate the trade price effects of the euro's introduction using detailed trade price data.

2. THE BASIC ECONOMICS OF PRICE SETTING AND THE EURO

Prices are set by firms bent on making money. In a standard monopolistic competition setting, firms choose price to equate their perceived marginal revenue to their marginal cost. The well-known

[20] The view expressed in this study are those of the authors and do not necessarily reflect those of the Bank of Italy.

formula for marginal revenue is the price times 1-1/ε, where ε is the perceived demand elasticity.[21] Solving the 'perceived marginal revenue equals marginal cost' formula, we get the standard pricing equation: $p = \mu m$, where 'mu' stands for the price-cost mark-up (the inverse of 1-1/ε), and 'm' stands for marginal cost; p stands for price. As usual in microeconomics, all the variables here are measured in terms of the numeraire, so there is no price illusion.

In the closed economy context, it is just that simple. In an open economy, some extra considerations come into play. First, when a firm located in Home sells to a customer located in Foreign, the relevant marginal cost must included the bilateral trade cost as well as production cost. We can express this as a factor of proportionality, τ, where τ equals 1+T and T is the tariff-equivalent of the trade cost. Second, the foreign customer will see the price expressed in the Foreign currency while the exporter will see the price in the Home currency, so we have two consumer prices, p and p*; the Foreign price, p*, equals 'e' times p, where 'e' is the number of Foreign currency units per Domestic currency units. With these points in mind, for a given product (we omit the product subscript to reduce confusing clutter):

$$p^* = \mu[p^*]\tau(m/e) \qquad \Leftrightarrow \qquad p = \mu[p^*]\tau m \tag{8}$$

Here we stress the fact that the mark-up may depend on the local price, p*, by making the mark-up, μ, a function of p*. Since p* equals p times e, we approximate the true μ[p*] function with a log-linear function

$$\mu[p^*] \doteq \bar{\mu}(p\,e)^{\gamma}$$

Plugging this into the first order condition and gathering terms:

$$\ln p = \frac{\gamma}{1-\gamma}\ln e + \frac{\ln\bar{\mu} + \ln m + \ln\tau}{1+\gamma} \tag{9}$$

This is often called a pricing-to-market equation (for a given product) since it relates the change in the exchange rate to export prices in the origin country's currency. For example, the famous Dornbusch (1985) article on pricing-to-market works with a linear demand curve that implies less than full pass-through.[22]

2.1. Euro's impact on bilateral pricing

Now, consider the impact that adoption of would have on the pricing equation, (8), and thus on the (9). There are two main channels:

#1) the direct impact via the euro's effect on bilateral trade cost τ (exactly as in the Rose effect literature), and

#2) the indirect impact via the common currency's effect on the optimal mark-up via the demand elasticity.

The cost-lowering effect, #1, was discussed in the trade volume section and needs no repeating here. Effect #2 is a different. Since μ equals 1/(1-1/ε), the euro can only affect the mark-up via its impact

[21] 'Perceived' since it depends on the firm's perceptions of the strategic play of its competitors.

[22] Linear demand gets less elastic as prices fall, so a devaluation of the Home currency lowers p*, but by less than the depreciation since p rises.

on the perceived demand elasticity. Industrial organisation theory teaches us that perceived demand elasticity can depend upon many things such as the equilibrium price (if the residual demand curve is not isoelastic), the degree of competition, and the substitutability of rival goods. Most of the informal stories concerning the euro's impact on prices turn around 'pricing transparency' and facilitation of price arbitrage. Both of these suggest a change in firms' perceptions that would make them believe that the demand they face is more price-sensitive, i.e. as more elastic. This would lead to a lowering of the optimal mark-ups.

3. LITERATURE REVIEW

The literature on European price convergence has not come to a consensus view. The studies that found that the euro has had a significant effect include papers like Allington, Katttuman and Waldmann.(2005), Imbs et alias (2004), Isqut (2001), Matha (2003), and Parsley and Wei (2001). However other studies find no evidence of faster price convergence or changes in pricing behaviour. Baye et alias (2005) and (2002), Engel and Rogers(2004), Lutz (2003), Rogers (2002) are the main papers here.

The conflicting results are accounted for by several factors. First, the datasets employed in these studies cannot be exhaustive, many concentrate on a few goods like Baye et alias (2005) and (2002), Engel and Rogers (2004), Lutz (2003), Rogers (2002), Math(2003) and Parsley and Wei (2001). Some of these consider a single good although some are based on comprehensive datasets of hundreds of goods such as Allington, Katttuman and Waldmann (2005) and Isqut (2002). A second problem is the lack of consensus on the definition of price dispersion. Some studies use the log average of price difference, the average price volatility or mean squared error, the coefficient of variation, the log of absolute average difference, or the difference between minimum and maximum prices. Some authors use national prices, other use local (city) prices. Some studies are purely cross sectional while others use panel data and the studies different along other econometric-techniques dimensions as well.

Many of the studies use consumer prices. This choice has an important drawback if we believe the essential channel of price convergence to be trade. The idea that the euro can foster price convergence must square with the fact that the distribution chain is composed of different stages and involves numerous players. There exists an initial producer/exporter which sells to an importer, then at least one wholesale dealer and, before the good reaches the final consumer, there is an additional stage at retail level. Prices paid by each of them do not necessarily change identically. One can imagine that they together, but issues of market power and the curvature of demand and supply curves will enter the analysis, thus muddying the inference that any changes observed where due to the euro.

One approach that avoids such issues was introduced by Knetter (1989). Instead of using consumer prices, it uses export prices denominated in the exporter's currency as suggested by the sort of theory discussed in the previous section. He used used, for example, to study how US and German exporters discriminate across destinations. Goldberg and Knetter (1995) used the approach to look at the US beer market, and Goldberg and Knetter (1997) used it for the Japanese, Canadian and German automobile market. More recently a series of papers have focused on price discrimination of European exporters. Falk and Falk (2000) measures price discrimination of German exporters in 70 items during a period of large Deutsche Mark fluctuations. They conclude that pricing to market is observed for the USA, Japan, Italy and Spain in chemical and fertilizers, but not in machinery. Finally Gil-Pareja (2002) and Gil-Pareja and Sosvilla-Rivero (undated) analyze the level of price segmentation in Europe. They do not explicitly concentrate on the euro effects nor on pricing to market.

The empirical literature on trade pricing is underdeveloped compared to the trade volume literature, so the review is no where near as long. We start with one of the best papers on this subject to date.

3.1. Allington, Kattuman and Waldmann (2005)

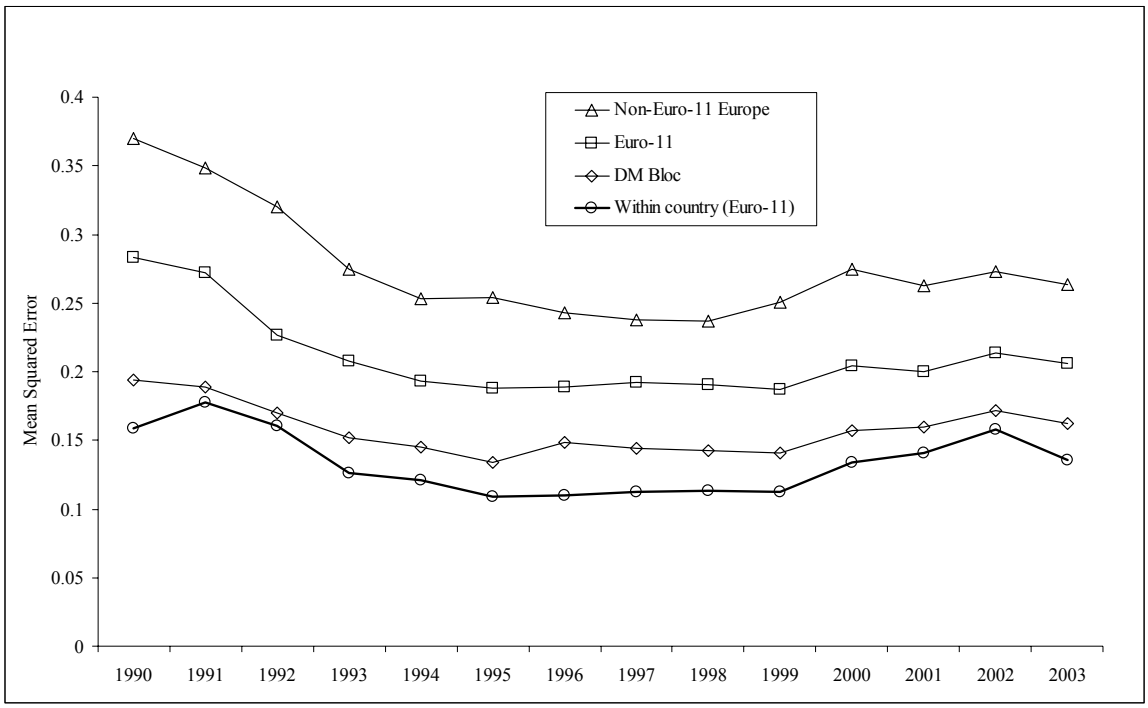
Allington et al (2005) focus on a measure of price dispersion, comparing the pre-euro and post-euro behaviour of their measure for nations that are inside the Eurozone and nations that are not. To control partly for many other integrating policy changes, they limit the universe to members of the EU15. They find robust results which show that the euro significantly lowered price dispersion within the euro group. The data they use is Eurostat's 'Comparative price level indices' for individual consumption expenditure in about 200 product groups for all EU15 countries during the 1995–2002 (annual data).

The authors also report that there was not a sudden change in dispersion, but that the euro's introduction accelerated the declining dispersion that was ongoing during the 1990s (which was probably driven by EU market integration). Moreover, they find enormous differences across product categories. The key to the authors' finding is a difference-in-difference result. The basic idea is to see whether the change in dispersion between the pre- and post-euro periods (the 'difference') is substantially different between the Eurozone nations and the other EU members (the difference between the differences). If the euro did diminish price dispersion, the euro group's pre-versus-post difference should be bigger than the non-EZ group's.

Comparing the differences, they find that while there are some products where the non-EZ group saw more convergence, there were far more product groups where the euro seems to have promoted price convergence. What all this suggests is that the euro does seem to have promoted price convergence in the euro group, although the effect is clearly not overwhelming.

Using a different price data set, Beck and Weber (2003) look at prices in 81 cities and 10 types of goods during 1991–2002, finding that that the euro significantly reduced cross-border relative price volatility. The effect, however, is not immediate and certainly not complete. Isgut (2002) finds similar results using two balanced panels of 116 cities and 69 goods and 79 cities and 123 goods in 2001 and concluded that the same currency reduces price differences generally by 2–3 percent (using standard deviations of log price differences across city pairs) and in the EMU specifically, by 5 percent, even when EU had been controlled for.

Figure 8: Engel and Rogers (2004) Price dispersion data by group.



Source: Engel and Rogers (2004), Figure 4.

The impact of currency union is confirmed by Lutz (2004) using data on the Belgium-Luxembourg currency union (set up in 1953) and the rest of the EU. He focuses on price convergence for 90 automobile models during 1993–98. His results suggest a 4% lower price differential within the currency union even when the other determinants of economic integration had been controlled.

Yet another study confirming the Allington et al (2005) results is Foad (2005). This paper uses an original dataset, namely monthly data on prices facing U.S. State Department for employees living abroad as reflected in their permitted per diem for lodging, meals and incidental expenses for 201 cities in 16 countries, from 1995 to 2002. The author finds that the impact of the euro on cross-border price volatility varied by country size. Within the Eurozone, cross-border price volatility did not changed between the small countries, but fell significantly between the large Eurozone countries.

Imbs et al (2004) use a unique dataset on television prices across European countries and regions. They find that Eurozone members display lower price dispersion than non-EMU countries and that regional price dispersion is comparable to intra-EMU dispersion.

While all these papers find that the euro had a price impact, none of them finds that it was sudden and well defined as is to case for the estimated trade volume relationships. One paper that contradicts these findings is Engel and Rogers (2004). They use data gathered by the Economists Intelligence Unit on consumer prices of 101 traded goods and 38 non-traded items in 18 European cities (11 in Eurozone countries and 7 in non Eurozone countries) for the years 1990-2003. The authors find no evidence that the euro decreased price dispersion among Eurozone members, although they do find that there has been a significant reduction in price dispersion throughout the decade of the 1990s.

Why do Engel and Rogers (2004) find such different results? A look at their data and the critique of one of their discussants at the Panel, Giovanni Veronese from the Bank of Italy, is revealing. The authors take as their measure of price dispersion the mean squared error of the log difference in prices between cities. The salient points from the cross-section aspect of the raw data are:

- Price dispersion is greater among non-euro nations than it is among the euro-11.
- Price dispersion among both euro and non-euro nations is greater than members of the DM bloc (a group that experienced had very little exchange rate variability in the 10 years leading up to the euro's introduction).
- Price dispersion across cities within a single nation is even lower than that of the DM bloc.

On the face of it, these cross-group comparisons suggest that the level of price dispersion is roughly correlated with the degree of exchange rate variability.

The time series facts, however, seem to tell a different story. Price dispersion in all four groups shows a clear decline in the early 1990s, but the decline stops around the time of the euro's introduction. Indeed, it even seems to increase somewhat. Thus the time-series facts seem to suggest that the euro had no impact on price dispersion, or even raised the degree of dispersion.

The problem with this conclusion is that these results are not conditional on other factors. In particular, Veronese suggests that there was a powerful force driving increased dispersion in the post-1999 period, namely the divergence of national inflation rates in the Eurozone that occurred just after the euro's launch. The big-push to meet the Maastricht criteria led to a substantial drop in Eurozone nations' inflation. In the figure this shows up in a drop in the standard deviation of inflation rates since they were all converging on the three lowest rates as per the criteria. However, once the list of 'winners' was announced a number of euro nations relaxed their efforts and inflation rates diverged again. The diverging inflation rates should have been reflected in an increased dispersion of prices. Moreover, since this belt-tightening-and-loosening exercise was not undertaken by the non-euro nations, one should have expected to see a greater rise in the Eurozone's price dispersion than that of the non-Eurozone. Of course controlling for this sort of

factor is exactly what the econometrics is for, but Engel and Rogers (2004) do not consider domestic inflation to be a factor.

Some of other earlier studies, such as, Parsley and Wei (2001), also find no euro effect, but this is not in contradiction to the later positive findings since their data stops at 2000, and papers such as Allington et al (2005) suggest that the euro's price effect does not involve a jump in 1999.

4. PRIMA FACIE EVIDENCE: EXPORT PRICE CONVERGENCE

Economic logic suggests that a common currency should narrow the range of price differences for similar goods within the Eurozone. In this section we consider the prima facie evidence on this by estimating some reduced form relationships, i.e. relationships that seem to make sense but are not clearly based on a model.

Table 10: Speed of convergence within EZ versus other control groups, 1995-2004.

	γ00	γ01	γ10	γ11	γ00*D99	γ01*D99	γ10*D99	γ11*D99
Converg. coefficient	-0.481	-0.692	-0.619	-0.869	0.168	0.24	0.244	-0.101
s.e.	[0.088]***	[0.234]***	[0.379]	[0.122]***	[0.086]**	[0.341]**	[0.254]	[0.127]
Obns	5,438,501							
R2	0.15							

Notes: Random effects estimator of speed of convergence and its change after the euro's introduction in 1999. The γ coefficient is the rate of convergence parameter with the suffix indicating the direction of the flow in the standard from-to sequence; that is, 11 is within the Eurozone, 01 is from outside to inside, etc.

We use export price data derived from HS6 bilateral trade data. Specifically, we use the so-called unit-price index which is the value measure divided by the quantity measure (the HS6 trade data reports both value and quantity for all trade flows). For each HS6 product, there exist as many export prices as there are origin-destination pairs. In our dataset that has 19 nations each good has potentially 342 different prices. Using euro membership as a discriminating factor, we put all 342 prices into one of four categories: prices that are from one EZ nation and in another (in-in), prices that are from non-EZ nations and in another non-EZ nations (out-out), prices that are from non-EZ nations and in an EZ nation (out-in), and finally the out-in prices. We look to see how price dispersion evolves in the four groups.

Table 11: Speed of export price convergence, Eurozone vs three control groups

	1996	1997	1998	1999	2000	2001	2002	2003	2004
γ00	-0.314	-0.241	-0.317	-0.031	0.485	-0.604	-0.743	0.417	0.232
s.e.	[0.056]***	[0.303]	[0.046]***	[0.065]	[0.074]***	[0.005]***	[0.010]***	[0.070]***	[0.036]***
γ10	-0.724	-0.969	-0.508	-0.69	0.074	-1.03	-0.86	-0.67	0.102
s.e.	[0.189]***	[0.131]***	[0.229]**	[0.149]	[0.004]***	[0.099]***	[0.312]**	[0.036]***	
γ01	-0.081	-0.9	-0.558	-0.636	-0.368	-0.181	-0.624	0.088	-0.232
s.e.	[0.037]**	[0.522]*	[0.047]***	[0.092]***	[0.045]***	[0.076]**	[0.237]***	[0.117]	[0.201]
γ11	-0.968	-0.919	-0.705	-0.519	-0.373	-0.249	-0.965	-0.976	-0.942
s.e.	[0.049]***	[0.066]***	[0.211]***	[0.105]***	[0.518]	[0.050]***	[0.062]***	[0.070]***	[0.037]***
Obns	589,592	595,625	599,375	604,014	607,183	608,081	603,612	610,986	620,033
R2	0.05	0.26	0.24	0.17	0.1	0.39	0.23	0.06	0.07

Notes: Random effects estimator of speed of convergence and its change after the euro's introduction in 1999.

It is easier to see the relative evolution of the convergence parameters when they are plotted as in Figure 9. While there is a good deal of noise in the chart, it does seem that the price convergence within the Eurozone was sharply accelerated (the coefficient becomes more negative so that the gap

between a typical price and the average closes more quickly). This is especially true after the currency union in 2001.

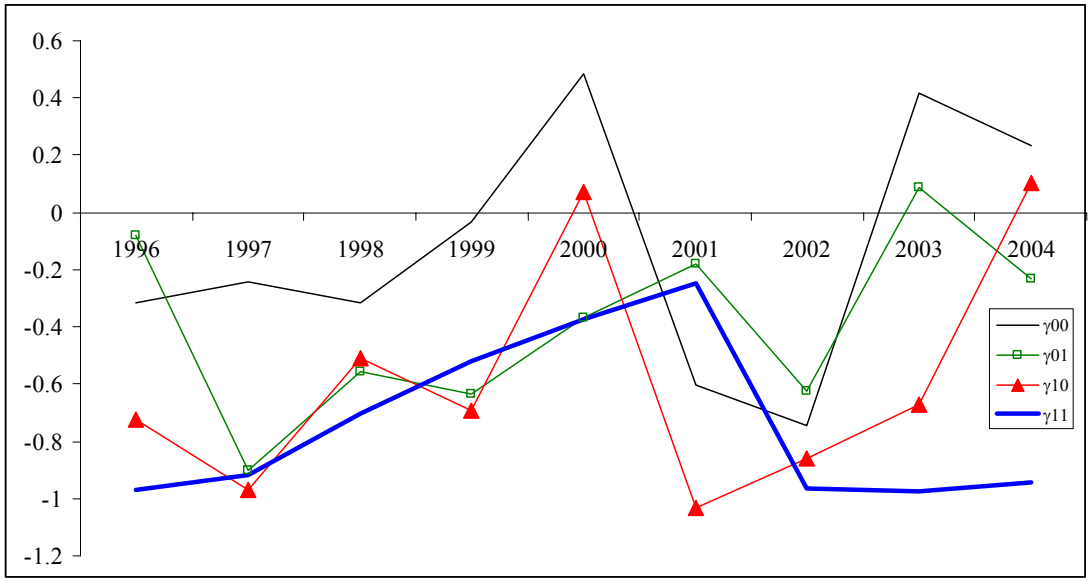
The concept of convergence we have in mind is not a convergence to long run values, but rather concerned purely with convergence to the actually average price within a group of prices. To this end, we compute price averages by product-year-origin for each group and estimate a standard speed of convergence regression. Specifically, the estimates were obtained using random the effect model.[23]

The results are shown in Table 11. What we see is that all the prices are converging to within group averages quite quickly, but the EZ11 group shows an especially high rate of convergence. Post 1999, the three control groups of prices experience the same or slower rates, but the point estimate on the EZ11 group is negative suggesting faster convergence, however the point estimate is not statistically different from zero.

Another way to look at the some issue is to estimate year-by-year convergence parameters for the four groups separately. This is done in Table 11.

This initial inspection of the data strongly suggests that the euro has had an effect on pricing. We turn now to a more formal investigation based on the pioneering work of Michael Knetter.

Figure 9: Convergence parameters in the EZ and three control groups.



5. NEW EVIDENCE

Using the simple theory discussed above, we estimate pricing-to-market equations on detailed trade data for the Eurozone nations (Germany, France, Italy, Spain, Netherlands, Portugal, Greece, Austria, Finland and Ireland) and 3 non-EZ members of the EU (Britain, Sweden and Denmark) and 5 outsiders (Switzerland, Norway, Canada, US, and Japan). The data we use is at the HS6 digit level (about 5,000 products) for 1995-2006 period. Computational limitations prevent us from working with the full data set (more than 9 million data points). This leads us to work with datasets organised by the exporter (i.e. each data set concerns only one origin nation, but 18 destinations) but even this is not sufficient since the dimension of the panel would be something like number of product categories (thousands) times 18 destinations and this exceeds our capacity. To reduce the

[23] We believe this to be the correct estimation procedure when first differences have already eliminated possible constant unobservable effect and price differences may vary considerably over products.

size, we focus only on products that are exported every year to every partner and with a minimum share of total export equal to 0.13%. The idea is that these are product where pricing in more consistent since they are more important to the exporter.

5.1. Estimating equations

Estimation of the pricing-to-market equation is made difficult by a lack of data on marginal costs and bilateral trade costs. To address the latter problem, we note that the τ here is identical to the one in the gravity model, so we can use the same proxies. Thus, we assume that the log of τ_{odt} equals $-\beta_1 EZ_{odt} + \beta_2 \ln Dist_{od} + \beta_3 \ln Z_{odt}$, where EZ is the euro dummy, Dist is bilateral distance and Z is all other factors, such as Single Market integration measures. Marginal cost is hard to proxy for, but in panel data, we can get around this by exploiting the fact that marginal cost for an exporter is the same for every destination in any given period. This allows us to replace the marginal cost term with a time-varying dummy for the exporter, denote as D_{ot}. Denoting the origin nation (i.e. the exporting nation) as nation-o and the destination nation as nation-d, the pricing-to-market equation (for a given product and for a given origin nation) is:

$$\ln p_{odt} = \frac{\gamma}{1-\gamma} \ln e_{odt} + \frac{\ln \bar{\mu} + \ln m_t - \beta_1 EZ_{odt} + \beta_2 \ln Dist_{od} + \beta_3 \ln Z_{odt}}{1+\gamma}$$

The second channel of euro effects is a change in the perceived elasticity, namely γ. In the estimating equation this shows up as interaction terms between all of the coefficients and the euro dummy, EZ, that 'turns on' after 1999 for exports to Eurozoners. The resulting estimating equation is thus:

$$\ln p_{odt} = c_o + EZ_{odt} + D_{ot} + D_{od} + \beta \ln e_{odt} + \beta_{EZ} EZ_{odt} \ln e_{odt} + \varepsilon_{odt} \qquad (10)$$

where we have collected the pair-specific variables that don't vary over time into the pair dummy D_{od}, the variables that vary over time but not by pair (e.g. the marginal cost) into the year dummy D_{ot}, and the variables that shift with the introduction of the euro (e.g. the reduction in τ) into the Eurozone dummy EZ_{odt}.

Importantly, for Eurozone members, the bilateral exchange rate becomes unity, so the term $\beta_{EZ} EZ_{odt} \ln e_{odt}$ is zero. Thus for Eurozone members, all the action will come through standard intercept dummy, EZ_{odt}. For non-Eurozone members, the effect may come both from the intercept dummy and the slope dummy on the exchange rate.

5.2. Results

The full regression results are shown in the next section. Here we just collect the estimates of the EZ dummy and its interaction term with the exchange (for non-EZ exporters) in Table 12.

The results Table 12 confirm the findings of existing studies that the euro did reduce export prices within the Eurozone. The left panel of the table shows the estimated EZ coefficient for the Eurozoners. Using the standard notations, the EZ dummy for EZ exporters to EZ markets written as EZ11.Recall that according to our theory, this coefficient includes both the impact of lower bilateral costs engendered by common euro-usage as well as the change in mark-up due to changes in the perceived demand elasticity stemming from, for example, greater pricing transparency. Unfortunately, we cannot distinguish between these two channels – what we called the direct and indirect effect – but is is noteworthy that it is almost always negative and highly significant.

The right panel of the table shows the EZ estimates for non-EZ exporters. There is a striking contrast between this set of results and those in the left panel. Here all but three of the EZ dummies are insignificant. Note that EZ here means that only the destination market uses the euro, so it is marked EZ01. The estimate is borderline significant and negative for Britain but not for Sweden or Denmark. It is also strongly negative for Japan, but even more strongly positive for Canada. The second estimate reported in the right panel "EZ10*Exch.Rate" show the estimate of how the pricing-to-market elasticity changed after the euro's introduction. One hypothesis that is commonly heard is that the euro should have made the Eurozone much more competitive and made outsiders treat the market as one and thus more likely to price-to-market. The results here suggests that this did not happen, or at least not yet.

Table 12: Impact of the euro on pricing to market

Exporter:	EZ11	s.e.	No. HS6 categories	Exporter:	EZ01	s.e.	EZ10* ExchRate		No. HS6 categories
Germany	**-0.31**	[0.039]***	1950	Britain	**-0.08**	[0.039]**	0.01	[0.008]	1907
France	**-1.91**	[0.050]***	2167	Sweden	-0.01	[0.097]	0.02	[0.009]*	1793
Italy	**-7.47**	[0.034]***	2898	Denmark	0.17	[0.324]	0.00	[0.006]	1948
Spain	**-4.82**	[0.041]***	1408	Switzerland	0.54	[0.365]	0.00	[0.008]	2413
Belgium	**-3.53**	[0.042]***	1876	Norway	0.05	[0.353]	0.02	[0.018]	601
Ireland	**0.38**	[0.142]***	264	Japan	**-0.27**	[0.116]**	**0.02**	[0.008]**	1111
NL	**-0.51**	[0.064]***	1992	USA	-0.07	[0.115]	-0.01	[0.013]	1824
Portugal	**-5.00**	[0.033]***	663	Canada	**1.53**	[0.568]***	0.03	[0.176]	32
Austria	**-2.47**	[0.099]***	2016						
Greece	**-3.63**	[0.184]***	118						
Finland	**-1.53**	[0.072]***	773						

Notes: OLS with year and product-partner fixed effects; Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

5.2.1. Price-to-market: EZ vs non-EZ destinations

One thing that our empirics can shed light on is the degree to which countries price to market when it comes to various destinations. Inspection of Table 13 show that most of the pricing-to-market (PTM) elasticities are already insignificant during the 1995-1999 period (we cannot estimate them afterwards since the logs of bilateral exchange rates are all zero by definition). What this means is that firms in the large Eurozone nations were already treating the other EZ nations as if they were domestic – at least in terms of pass-through pricing. To see this, note that the estimates – for example, Germany's with respect to exports to Belgium (ler_BEL in the table, -0.01) – indicate that an appreciation of the bilateral exchange rate had no impact on the DM export price. What this means is that the German firms did not adjust their prices to absorb some of the competitive loss stemming from an appreciation, or raise profit margins in response to a competitiveness-boosting depreciation. Of course, there was extremely little bilateral exchange rate variation for most of the EZ11 pairs. We note that of the 40 estimates, only 8 are significant and all are very small in size. Interesting, most of the significant ones are negative, suggesting that firms moved price mark-up in a way that amplified consumer price changes stemming from bilateral exchange rate changes, i.e. there was more than full pass through.

The findings are quite different for the non-EZ destinations.

Here most of the PTM elasticities are positive and significant, indicating that the large EZ nations were treating these nations as segmented from others. To understand that, it is worth noting that a positive coefficient tells us that firms absorb some of the consumer price changes that would have

otherwise stemmed from a bilateral exchange rate movement. We know that this implies segmented markets, since such absorption implies that there will be different prices in different markets when prices are converted to a common currency. If markets were thoroughly integrated, such price gaps would trigger arbitrage that would either eliminate the price differences or prevent the firms from creating them in the first place.

Studying the same estimates for the small EZ nations, namely Belgium, Ireland, NL, Portugal, Austria, Greece and Finland (Table 14), we note that there is more PTM within the euro group prior to 1999 for the small nations. Part of this may be due to the narrower range of products. The stark difference between the PTM coefficients within the insiders and the outsiders, however, is still present in these results. The pre-1999 average PTM elasticity for these nations with respect to markets that eventually joined the euro area is an order of magnitude smaller than those with respect to nations that were outside the euro area. For incompleteness, we include the regression results for the non-EZ exporters (Table 15).

Table 13: Pricing to market regression for large EZ nations, 1995-2006

	Germany	France	Italy	Spain	Avg
EZ11	**-0.31**	**-1.91**	**-7.47**	**-4.82**	-3.63
	[0.039]***	[0.050]***	[0.034]***	[0.041]***	
ler_AUT	**-0.03**	-0.05	0.00	0.01	-0.02
	[0.012]***	[0.045]	[0.004]	[0.018]	
ler_BEL	-0.01	0.02	0.00	0.04	0.01
	[0.008]	[0.017]	[0.005]	[0.029]	
ler_DEU		-0.03	0.00	0.00	-0.01
		[0.024]	[0.002]	[0.008]	
ler_ESP	**-0.02**	**0.02**	-0.01		0.00
	[0.005]***	[0.009]**	[0.007]		
ler_FIN	-0.04	0.11	0.00	-0.02	0.01
	[0.023]*	[0.306]	[0.004]	[0.015]	
ler_FRA	**-0.06**		0.00	0.01	-0.01
	[0.020]***		[0.003]	[0.012]	
ler_GRC	**0.01**	0.00	0.00	0.01	0.00
	[0.003]***	[0.005]	[0.002]	[0.006]	
ler_IRL	0.00	**-0.04**	0.00	0.02	-0.01
	[0.032]	[0.018]**	[0.003]	[0.010]*	
ler_ITA	**-0.01**	0.01		0.00	0.00
	[0.003]**	[0.005]		[0.015]	
ler_NLD	-0.32	0.00	0.00	-0.01	-0.08
	[0.203]	[0.028]	[0.003]	[0.009]	
ler_PRT	**-0.01**	0.01	0.00	-0.05	-0.01
	[0.006]**	[0.010]	[0.008]	[0.218]	
ler_SWE	**-0.22**	**-2.85**	**1.40**	**1.77**	0.02
	[0.093]**	[0.551]***	[0.100]***	[0.108]***	
ler_SWEez	**0.23**	**3.33**	**-1.44**	**-1.92**	0.05
	[0.036]***	[0.480]***	[0.346]***	[0.252]***	
ler_DNK	**0.19**	**-11.43**	**0.73**	**1.64**	-2.22
	[0.130]	[1.381]***	[0.215]***	[0.093]***	
ler_DNKez	**0.11**	**11.66**	**0.94**	**-1.73**	2.74
	[0.048]**	[1.299]***	[0.814]	[0.236]***	
ler_GBR	**0.35**	**0.82**	**0.98**	**0.91**	0.76
	[0.048]***	[0.034]***	[0.008]***	[0.018]***	
ler_GBRez	**-0.24**	**-1.00**	**-0.35**	**-0.87**	-0.62
	[0.057]***	[0.099]***	[0.111]***	[0.171]***	
ler_CHE	**1.61**	**1.30**	**1.01**	**1.05**	1.24
	[0.247]***	[0.042]***	[0.018]***	[0.016]***	
ler_CHEez	**-1.69**	**-1.28**	**-0.45**	**-1.00**	-1.11
	[0.297]***	[0.118]***	[0.295]	[0.159]***	
ler_NOR	**-0.26**	**-2.77**	**1.26**	**1.44**	-0.09
	[0.110]**	[0.812]***	[0.112]***	[0.119]***	
ler_NORez	**0.23**	**3.36**	**-1.02**	**-1.17**	0.35
	[0.040]***	[0.724]***	[0.408]**	[0.292]***	
ler_JPN	**-0.07**	**-0.11**	**0.89**	**0.13**	0.21
	[0.231]	[0.347]	[0.133]***	[0.636]	
ler_JPNez	**0.08**	**0.42**	**0.16**	**0.88**	0.38
	[0.025]***	[0.126]***	[0.207]	[0.663]	
ler_CAN	**1.72**	**1.23**	**1.02**	**1.09**	1.27
	[0.295]***	[0.057]***	[0.013]***	[0.020]***	
ler_CANez	**-1.74**	**-1.17**	**-0.51**	**-1.65**	-1.27
	[0.367]***	[0.211]***	[0.225]**	[0.247]***	
ler_USA	**0.54**	**1.03**	**1.00**	**0.98**	0.89
	[0.079]***	[0.030]***	[0.005]***	[0.009]***	
ler_USAez	**-0.20**	**-0.48**	**-0.36**	**-0.46**	-0.37
	[0.122]	[0.110]***	[0.058]***	[0.134]***	
Constant	3.01	3.82	7.08	5.07	
	[0.060]***	[0.061]***	[0.087]***	[0.041]***	
Observations	23400	26004	34776	16896	
Number of products	1950	2167	2898	1408	
R-squared	0.95	0.90	0.99	0.97	

Notes: OLS with year and product-partner fixed effects; Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 14: Pricing to market regression for small and medium EZ nations, 1995-2006

	Belgium	Ireland	NL	Portugal	Austria	Greece	Finland
EZ11	**-3.53**	**0.38**	**-0.51**	**-5.00**	**-2.47**	**-3.63**	**-1.53**
	[0.042]***	[0.142]***	[0.064]***	[0.033]***	[0.099]***	[0.184]***	[0.072]***
ler_AUT	0.07	**0.12**	0.03	0.02	0.00	**0.54**	0.10
	[0.033]**	[0.056]**	[0.024]	[0.014]	[0.000]	[0.056]***	[0.092]
ler_BEL	0.00	0.04	**0.04**	**0.07**	0.05	**0.53**	0.04
	[0.000]	[0.042]	[0.015]***	[0.020]***	[0.042]	[0.065]***	[0.039]
ler_DEU	0.01	0.06	-0.39	**0.03**	-0.03	**0.44**	0.00
	[0.010]	[0.179]	[0.358]	[0.007]***	[0.020]	[0.043]***	[0.062]
ler_ESP	-0.04	0.03	**0.02**	**0.39**	-0.02	**0.52**	0.02
	[0.023]*	[0.031]	[0.010]**	[0.174]**	[0.018]	[0.047]***	[0.023]
ler_FIN	0.03	0.01	0.08	0.01	-0.03	**0.49**	0.00
	[0.019]	[0.083]	[0.044]*	[0.010]	[0.054]	[0.051]***	[0.000]
ler_FRA	0.01	0.02	**0.09**	**0.03**	-0.03	**0.51**	0.31
	[0.018]	[0.070]	[0.042]**	[0.009]***	[0.057]	[0.062]***	[0.613]
ler_GRC	0.01	0.02	**0.03**	0.01	0.01	0.00	**-0.03**
	[0.005]	[0.022]	[0.007]***	[0.006]*	[0.007]	[0.000]	[0.013]**
ler_IRL	-0.02	0.00	**-0.20**	**0.02**	0.03	**0.36**	-0.03
	[0.009]*	[0.000]	[0.048]***	[0.008]**	[0.019]	[0.038]***	[0.047]
ler_ITA	-0.01	0.02	**0.02**	**-0.05**	0.01	**0.27**	0.01
	[0.008]	[0.021]	[0.006]***	[0.019]***	[0.008]	[0.055]***	[0.013]
ler_NLD	0.02	-0.02	0.00	**0.01**	-0.01	**0.38**	-0.09
	[0.011]	[0.157]	[0.000]	[0.007]**	[0.023]	[0.048]***	[0.075]
ler_PRT	-0.05	0.03	0.01	0.00	0.01	0.41	-0.01
	[0.022]**	[0.032]	[0.010]	[0.000]	[0.018]	[0.049]***	[0.022]
ler_SWE	**2.09**	**0.14**	-0.04	**1.47**	**2.31**	**1.12**	**-0.88**
	[0.131]***	[0.303]	[0.153]	[0.070]***	[0.378]***	[0.079]***	[0.460]*
ler_SWEez	**-1.91**	**-0.08**	**0.21**	**-1.24**	**-1.64**	**-0.51**	**1.44**
	[0.221]***	[0.061]	[0.065]***	[0.172]***	[0.446]***	[0.100]***	[0.376]***
ler_DNK	**2.05**	**0.71**	**0.13**	**1.42**	**4.37**	**1.13**	**-3.64**
	[0.110]***	[0.352]**	[0.168]	[0.068]***	[0.388]***	[0.082]***	[1.464]**
ler_DNKez	**-1.98**	**-0.05**	**0.18**	**-1.21**	**-4.46**	**-0.46**	**3.96**
	[0.201]***	[0.072]	[0.077]**	[0.181]***	[0.491]***	[0.097]***	[1.291]***
ler_GBR	**0.86**	**0.36**	**0.28**	**0.90**	**0.81**	**0.94**	**0.73**
	[0.017]***	[0.651]	[0.072]***	[0.013]***	[0.043]***	[0.050]***	[0.059]***
ler_GBRez	**-1.13**	**0.68**	**-0.57**	**-0.97**	**-1.18**	**-0.34**	**-0.52**
	[0.101]***	[0.607]	[0.093]***	[0.112]***	[0.135]***	[0.056]***	[0.174]***
ler_CHE	**1.11**	**0.41**	**1.37**	**1.06**	**1.25**	**0.94**	**1.04**
	[0.019]***	[0.377]	[0.251]***	[0.013]***	[0.049]***	[0.053]***	[0.071]***
ler_CHEez	**-1.13**	**-0.97**	**-1.37**	**-1.08**	**-1.60**	**-0.36**	**-0.97**
	[0.124]***	[0.356]***	[0.341]***	[0.144]***	[0.129]***	[0.070]***	[0.239]***
ler_NOR	**1.45**	**0.06**	**-0.18**	**1.57**	**4.76**	**1.11**	**-2.02**
	[0.178]***	[0.441]	[0.212]	[0.056]***	[0.506]***	[0.074]***	[0.721]***
ler_NORez	**-0.84**	**-0.35**	0.24	**-1.52**	**-4.78**	**-0.50**	**2.37**
	[0.313]***	[0.088]***	[0.085]***	[0.143]***	[0.619]***	[0.096]***	[0.606]***
ler_JPN	**-0.36**	**0.26**	-0.35	**0.89**	**-0.45**	**2.88**	**0.46**
	[0.604]	[0.868]	[0.440]	[0.762]	[0.457]	[0.441]***	[0.636]
ler_JPNez	**1.00**	**-0.10**	0.10	**0.13**	**0.76**	**-2.25**	**0.14**
	[0.449]**	[0.078]	[0.059]	[0.822]	[0.237]***	[0.488]***	[0.216]
ler_CAN	**1.07**	**0.28**	0.32	**1.08**	**1.15**	**0.98**	**1.09**
	[0.040]***	[0.360]	[0.375]	[0.013]***	[0.055]***	[0.055]***	[0.088]***
ler_CANez	**-0.70**	**-1.76**	0.15	**-1.14**	**-1.05**	**-0.37**	**-1.02**
	[0.358]**	[0.436]***	[0.566]	[0.156]***	[0.204]***	[0.079]***	[0.353]***
ler_USA	**0.99**	**1.00**	**0.52**	**0.98**	**1.02**	**0.91**	**0.89**
	[0.014]***	[0.314]***	[0.107]***	[0.007]***	[0.041]***	[0.052]***	[0.037]***
ler_USAez	**-1.04**	**-0.52**	-0.35	**-0.45**	**-0.65**	**-0.37**	**-0.55**
	[0.165]***	[0.517]	[0.189]*	[0.122]***	[0.115]***	[0.067]***	[0.182]***
Constant	4.45	3.87	2.86	5.78	4.87	5.59	4.35
	[0.048]***	[0.281]***	[0.097]***	[0.032]***	[0.072]***	[0.125]***	[0.122]***
Observations	22512	3168	23904	7956	24192	1416	9276
Number of products	1876	264	1992	663	2016	118	773
R-squared	0.96	0.43	0.87	0.99	0.91	0.95	0.85

Notes: OLS with year and product-partner fixed effects; Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 15: Pricing to market regression for non-EZ nations, 1995-2006

	Britain	Sweden	Denmark	Switzerland	Norway	Japan	USA	Canada
EZ01	**-0.08**	-0.01	0.17	0.54	0.05	**-0.27**	-0.07	**1.53**
	[0.039]**	[0.097]	[0.324]	[0.365]	[0.353]	[0.116]**	[0.115]	[0.568]***
lxrEURO	0.01	0.02	0.00	0.00	0.02	**0.02**	-0.01	0.03
	[0.008]	[0.009]*	[0.006]	[0.008]	[0.018]	[0.008]**	[0.013]	[0.176]
lxr_AUT	-0.02	0.07	-0.32	-0.28	-0.46	**-0.12**	-0.01	-0.01
	[0.017]	[0.222]	[0.538]	[0.171]	[0.710]	[0.052]**	[0.051]	[0.635]
lxr_BEL	0.01	-0.01	-0.12	-0.19	-0.10	-0.03	0.05	-0.14
	[0.011]	[0.064]	[0.194]	[0.114]*	[0.225]	[0.091]	[0.034]	[0.250]
lxr_DEU	0.07	0.03	0.13	-3.25	0.04	-0.04	0.27	-1.34
	[0.046]	[0.063]	[0.241]	[1.917]*	[0.244]	[0.027]	[0.242]	[1.972]
lxr_ESP	0.00	0.01	-0.06	-0.10	-0.06	**1.28**	0.01	-0.03
	[0.008]	[0.033]	[0.105]	[0.079]	[0.119]	[0.574]**	[0.024]	[0.171]
lxr_FIN	0.01	-0.10	1.01	-0.37	0.61	**-0.09**	0.02	-0.22
	[0.023]	[0.231]	[1.359]	[0.282]	[1.032]	[0.036]**	[0.078]	[0.608]
lxr_FRA	-0.03	-0.06	1.60	-0.43	-0.28	-0.05	0.05	-0.24
	[0.020]	[0.321]	[2.626]	[0.259]*	[1.548]	[0.037]	[0.069]	[0.596]
lxr_GRC	0.01	0.05	-0.05	-0.11	-0.01	**0.39**	0.02	-0.04
	[0.008]	[0.028]*	[0.087]	[0.070]	[0.098]	[0.139]***	[0.023]	[0.178]
lxr_IRL	-0.39	-0.04	0.10	0.64	0.04	-0.04	-0.15	-0.73
	[0.383]	[0.042]	[0.147]	[0.515]	[0.153]	[0.024]*	[0.280]	[1.698]
lxr_ITA	**0.01**	0.00	-0.04	-0.07	-0.03	0.04	0.01	-0.04
	[0.006]**	[0.018]	[0.058]	[0.051]	[0.065]	[0.042]	[0.016]	[0.110]
lxr_NLD	0.01	0.02	0.15	-1.98	0.10	-0.03	0.05	-0.93
	[0.045]	[0.069]	[0.263]	[1.191]*	[0.265]	[0.027]	[0.194]	[2.373]
lxr_PRT	0.01	0.01	-0.05	-0.12	-0.03	0.64	0.03	-0.22
	[0.008]	[0.031]	[0.099]	[0.076]	[0.113]	[0.300]**	[0.026]	[0.131]
lxr_DNK	0.29	-0.20	0.00	0.18	0.57	0.38	-0.21	-7.54
	[0.171]*	[0.300]	[0.000]	[0.469]	[0.686]	[0.200]*	[0.181]	[4.097]*
lxr_SWE	0.12	0.00	0.13	**0.51**	0.68	-0.01	**-0.32**	-8.77
	[0.173]	[0.000]	[0.312]	[0.210]**	[0.452]	[0.183]	[0.130]**	[5.217]*
lxr_GBR	0.00	**0.57**	0.22	0.21	**0.54**	-0.08	0.05	-13.75
	[0.000]	[0.120]***	[0.118]*	[0.128]	[0.239]**	[0.130]	[0.171]	[9.653]
lxr_CHE	0.33	0.32	-0.11	0.00	-0.97	0.17	0.14	-7.97
	[0.182]*	[0.228]	[0.407]	[0.000]	[1.179]	[0.185]	[0.147]	[4.699]*
lxr_NOR	-0.36	0.08	0.71	-0.29		0.28	-0.01	**-5.71**
	[0.192]*	[0.225]	[0.457]	[0.487]		[0.212]	[0.195]	[1.961]***
lxr_CAN	-0.69	**0.60**	-0.47	**0.51**	0.66	0.09	**-0.61**	0.00
	[0.427]	[0.177]***	[0.261]*	[0.222]**	[0.482]	[0.168]	[0.173]***	[0.000]
lxr_USA	**0.81**	**0.46**	**0.34**	**0.39**	**0.73**	-0.14		**-11.36**
	[0.212]***	[0.109]***	[0.115]***	[0.095]***	[0.191]***	[0.450]		[5.204]**
lxr_JPN	0.19	**0.47**	**0.63**	0.21	0.29	0.00	0.03	**-10.23**
	[0.180]	[0.156]***	[0.170]***	[0.129]	[0.265]	[0.000]	[0.160]	[4.244]**
Constant	2.60	4.58	4.71	3.95	5.47	8.58	4.01	1.32
	[0.078]***	[0.075]***	[0.208]***	[0.241]***	[0.257]***	[0.173]***	[0.092]***	[1.511]
Observations	22884	21516	23376	28956	7212	13332	21888	384
No.products	1907	1793	1948	2413	601	1111	1824	32
R-squared	0.86	0.82	0.71	0.65	0.32	0.72	0.53	0.11

Notes: OLS with year and product-partner fixed effects; Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

6. CONCLUDING REMARKS

This chapter reviewed the evidence in the existing literature the euro's effects on trade pricing. Some studies found the price convergence was faster after the euro's introduction but others found the opposite. As far as convergence is concerned, we present fresh evidence using the latest data and best econometric techniques. We confirm the negative finding – i.e. prices converge at the same rate before and after the euro – when we assume that there was only one change in the convergence rate and that changed occurred in 1999. However, when we estimate year-by-year convergence rates, we see that the rate has declined steadily in the Eurozone, especially since the currency union in 2001, but has not the control group of non-Eurozone nations. We believe that this finding helps

explain the lack of consensus in the empirical literature and it leads us to believe that the euro has indeed promoting market integration in terms of pricing.

To test this more carefully, this chapter provides a theoretically based empirical analysis (inspired by the innovative empirical strategy inspired of Knetter 1989) that presents more precise evidence on the market integration hypothesis. The basic idea is simple. When markets are segmented internationally, firms can charge different prices to different markets in order to maximise profits – what Krugman (1986) call pricing to market. Using panel data, we can test this by observing the co-movements of export prices for that same product to different nations in response to bilateral exchange rate movements. If the exporter is pricing to market – a clear sign that markets are segmented – then the export price in the exporter's currency will adjust to absorb some of the exchange rate fluctuation. For example, a bilateral appreciation would lead to a cut in the exporter's price measured in his own currency as the firm attempts to moderate the change in the price faced by consumers in the foreign market. A key drawback of this technique is its inability to directly address the disappearance of bilateral exchange rate fluctuation after the euro. Thus we cannot estimate whether Eurozone firms treated Eurozone markets as more or less integration after the euro. However, we can detect shifts in pricing and here our new empirical finding is important.

For all euro area members, except Ireland, we find that the introduction of the euro reduced export prices by approximately 1% to 5%. This estimated average export price drop could be due either to a reduction in transaction costs or to changes in market structure that made the market more competitive. Examples of the latter would include the oft mentioned notion that common pricing in euros increases market transparency and thus makes consumers more price sensitive. In an imperfect competition setting, such heightened price sensitive translates into lower price-marginal cost mark-ups. The export-price drop was observed for only 2 of the 8 non-Eurozone nations in our sample.

Chapter 4: Firm level evidence on the euro's trade effect

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1. INTRODUCTION

Dozens of empirical studies over the past few years have firmly established the fact that the euro has had a modest but positive impact on trade. These were reviewed at length in Chapter 2. A key empirical challenge is the identification of the economic mechanisms through which a common currency can affect trade. This chapter presents the results of efforts to identify such a mechanism using firm-level evidence.

The mechanism focused on in the classic optimal currency area articles of Mundell (1963) and his followers turn on the reduction of transaction costs – the story being a common currency reduces transaction costs among nations using it. Recent research as demonstrated quite clearly that this cannot be the main mechanism. There are three main pieces of evidence against the Mundellian "transaction cost" mechanism.

The first piece concerns the lack of trade diversion. A very robust theoretical prediction of the Mundellian "transaction cost" mechanism is that intra-Eurozone trade creation should be accompanied by extra-Eurozone trade diversion. The key is that under the Mundellian "transaction cost" mechanism, euro-using nations enjoy lower transaction costs on their bilateral trade while non-euro using nations do not. By lowering the cost of imports from other euro-using nations without altering the bilateral costs from non-euro nations, the reduced transaction costs should raise the *relative* price of imports from non-euro using nations. Since imports depend upon relative prices, the euro's trade creation within the Eurozone should have been accompanied by trade diversion for other nations.

Almost all empirical studies of the euro's trade effect fail to find trade diversion, so it is unlikely that the Mundellian "transaction cost" mechanism is important.

The second piece of evidence concerns the price effects. Under the Mundellian "transaction cost" story, the trade effect operates via prices. That is, some of the lower bilateral transaction costs are passed on to consumers and thus intra-Eurozone trade rises because the price of intra-Eurozone imports falls. Almost all empirical studies of trade pricing in the Eurozone reject the hypothesis that the euro has altered trade prices, so once again the Mundellian "transaction cost" mechanism cannot be the main conduit.

The third piece of evidence against the Mundellian "transaction cost" story concerns sectoral differences in the euro trade effect. If the key channel of trade effects were operating via transaction

costs, one would expect to see a positive trade effect on most goods. After all, international trade of all types of goods involves financial transactions so a reduction in these costs should boost all types of trade. Of course, different goods have different market structures and face different import demand elasticities, so one could anticipate a broad range of positive estimates, but all or almost should be positive and significant.

Almost all empirical studies of the euro's trade effects finds that the euro effect is concentrated in very few sectors – mainly sectors marked by product differentiation and imperfect competition. The fact that a large number of sectors experience no euro trade effect suggests that the economic mechanism cannot be as broad as the Mundellian "transaction cost" mechanism. The driving force must be something that is much more sector-specific than transaction costs.

An alternative to the classic Mundellian account was proposed by Baldwin and Taglioni (2004) and popularised by Baldwin (2005) and Baldwin (2006a) as the "new goods" hypothesis – or to be more precise – "newly-trade goods" hypothesis. Specifically, Baldwin and Taglioni (2004) show theoretically that one can account for all the empirical findings if one presumes that the euro is operating via the so-called extensive margin of trade. That is, the euro is stimulating the export of new products rather than simply increasing the volume of already-traded varieties. The "newly-traded goods" hypothesis draws on the new new trade theory (Melitz 2003) where firms are heterogeneous and their decision to export involves a two-step process. First they determine what their optimal exports to a given nation would be if they decided to export. Second, they decide whether the resulting profits would be sufficient to cover the cost of establishing a marketing beachhead in the given nation. Under the "newly traded goods" hypothesis, euro usage could reduce the fixed cost of exporting to an additional market without have much impact on the variable trade costs. In this way, the euro could simulate imports from Eurozone nations without shifting relative prices against non-euro-using exporters. Moreover, since the trade is coming from selling more varieties, there need be no change observed in the pricing of varieties that were sold both before an after the single currency. Finally, the Melitz model fits most naturally in markets characterised by product differentiation and imperfect competition. Sectors marked by constant returns, homogeneous goods and/or perfect competition should not observe an increase in the number of varieties traded and so should not observe a positive euro effect stemming from newly-traded goods.

A fictional example can provide the reader with a mental picture of what is likely to be going on in the theory and empircs. Consider a medium sized Swiss company that sells, for example, highly specialised Global Positioning System (GPS) unit for runners. Since its inception, the company exported to the German market since it was so large, but not in the Austrian market since the few additional sales would not have justified the 'beachhead' costs which include the cost of dealing with the Austrian shilling in the company's books, its financials accounts (hedging, more bank accounts, etc) and its sales force. For example, the Swiss company would have to set up a bank account in Vienna and some sort of hedging operation as well as working out a way to convert the shilling sales into its Swiss franc-based accounting system. They did all these things for the German market, but that was sensible since 80 million potential customers used deutschmarks. In 1999, the German banking/hedging/accounting /supervision operation was converted to euros. This lowered the extra fixed cost of entering the Austrian market, so the Swiss company started shipping to sports stores in Austria and another zero dropped out of the Swiss export vector to Austria. A similar story could be told for small German or French firms that initially only sold in one big export market but started exporting to other euro-using nations after 1999 due to the economies of scope in the financial aspects of exporting.

1.1. Empirical predictions

The simple newly-traded-goods hypothesis is based on the seminal heterogeneous-firms trade (HFT) model, namely Melitz (2003) model. This model has each firm as producing a single good according to a randomly assigned marginal cost. Since entering a market involves a beachhead costs

– the one-off fixed cost of establishing a marketing beachhead – only firms that are sufficiently competitive will find it profitable to export. Other firms may find it worth their while to selling domestically only.

1.1.1. The extensive and intensive margins

Assuming the euro reduces the beachhead cost and/or the variable cost of exporting, the Melitz model predicts that more firms based in Eurozone nations should start exporting to more Eurozone markets. This is the extensive margin, i.e. the number of firms/varieties being exported to other Eurozone nations (in the simple Melitz model each firm sells only variety only, so more products exported means more exporting firms and vice versa). The intensive margin is the average size per exported product.

We get important information on the nature of the euro's impact on trade costs by studying the impact on the intensive and extensive margin. If the euro only affects beachhead cost (fixed market entry costs), then we should see very little change in the exports per firm of the firms already exporting, with most of the extra exports coming from newly exported varieties. If the euro affects mostly the variable costs – i.e. transaction costs – then we should see both the average sales per product (for the usual reasons that lower trade costs stimulate exports) and the number of exported products rising (the lower transaction costs boost some Eurozone firm's competitiveness sufficiently to make it worth their while to export to other Eurozone nations).

Three extensive margins: more firms, to more markets, with more products

A more recent addition to the HFT literature, Bernard, Redding and Schott (2006), allows for the real-world possibility that firms may want to produce more than one variety. The predicted impact of the euro's introduction allowing for multi-product firms is somewhat different. Since some of the beachhead cost may be subject to economies of scope (i.e. falling average beachhead cost as the number of varieties per firm rises), the euro could be expected to have an especially large impact on the number of varieties exported by firms that are already exporting to the Eurozone. This is the second extensive margin, i.e. the average number of products that exporting firms export (the first extensive margin is the number of firms that export at all).

Most of the HFT theory is done with only two nations and thus a single export market, but there are more than one export destination in the Eurozone so we get a third extensive margin – the average number of markets in the Eurozone to which a single product is sold. We call this the geographic extensive margin. For example, the euro could have made it easier for a French firm that is already selling to Germany to also sell to Austria. An example of this version the hypothesis given in Baldwin (2005) focuses on economies of scope in financial and accounting fixed costs; a French firm that has established the necessary financial and banking arrangements to deal with export earnings in deutschmarks was forced to switch this to euros, but in doing so it found little additional cost of exporting to Austria or other Eurozone nations.

2. THE DATA SETS

Six national data sets had the information necessary to study the euro's impact, namely France, Belgium, Hungary, Sweden, Norway, and Germany. These data sets are not harmonised and each has its own idiosyncrasies.

Belgium

The Belgian team uses the Belgian Balance Sheet Trade Transactions Dataset (BBSTTD) that covers manufacturing firms with at least one full time equivalent employee. It contains export and FDI by destination. Exports are reported by 8-digit product level (NC-8), For exports within the EU, only firms that exceed the threshold of 250,000 euro are obliged to report. For extra-EU exports, the threshold is lower, 1000 euro (or less than one tonne). Some legal entities do export and have a

VAT number but do not file any accounts with the Central Balance Sheet Office. These are excluded the data base; while small in number, they accounted for over a third of the exports in 2004. The bulk of trade conducted by unmatched firms in 2004 was attributed to foreign firms with no actual production site in Belgium. It seems likely that much of this trade is transit trade (Antwerp is a major port for nations such as nations without ocean ports, such as Germany, Austria and Switzerland).

Table 16: Descriptive statistics, all industries, all firms and destinations

	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005
Total value of exports (billions)	113	128	133	135	156	162	175	176	194	211
Market share of 10% largest exporters	91%	91%	91%	90%	91%	91%	92%	92%	92%	93%
Nb of exporters	26,981	26,905	26,364	24,969	26,164	26,292	25,672	25,411	24,552	23,814
Avg number of products by exporter	8.6	8.7	8.9	9.3	9	9.1	9.4	9.7	10.1	10.3
Average value by exporter (billions)	4.2	4.8	5.0	5.4	5.9	6.1	6.8	6.9	7.9	8.8
Average value by variety	200,789	220,565	224,296	223,101	248,423	248,679	263,872	257,851	258,943	264,003

Source: NBB.

France

This database contains firm-level exports collected by the French Customs and reports the amount of exports by 8-digit NC level and by destination for each firm located on the French metropolitan territory, covering that 1998-2003 period. The threshold for intra-EU exports was 100,000 euro before 2006 and is 150,000 euros after. There are 225 destination countries, 11,578 products and about 102,300 exporting firms per year.

Table 17: Summary statistics, France, 1999

	Eurozone (EZ)	EU15 nonEZ3	nonEZ Europe	Rest of World
Number of exporting firms	46,390	22,923	36,824	63,707
Number of products /a	327,955	111,858	136,506	355,697
Total number of product-destination pairs	716,011	159,318	176,555	668,554
Avg number of destinations per variety	2.2	1.4	1.3	1.9
Total value of exports (billions)	121.0	30.3	15.8	68.5
Market shares 10% largest exporters to:	91%	90%	91%	94%

Source: Berthou and Fontagné (2008).
Notes: a/ Number of distinct NC8 categories per firm summed over all firms

Hungary

The Hungarian data is based on a sample of 2,043 large manufacturing firms for the 1992-2003 period. This sample accounts for 60% to 70% of exports and 50% to 60% of imports. Trade is reported at the HS 6 level, but we have both imports and exports.

Table 18: Summary statistics, Hungary, 1999

	Eurozone (EZ)	EU15 nonEZ3	nonEZ Europe	Rest of World
Number of exporting firms	10,451	1,819	7,669	5,147
Number of products /a	62,143	6,167	45,894	34,352
Total number of product-destination pairs	72,646	6,824	54,385	43,840
Avg number of destinations per variety	1.17	1.11	1.19	1.28
Total value of exports (billions)	15.02	1.18	2.08	2.19
Market shares 10% largest exporters to:	93%	92%	90%	91%

Source: Laszlo Halpern.
Notes: a/ Number of distinct NC8 categories per firm summed over all firms

Sweden

The data available to us from Sweden consists solely of trade data at the firm level, without any matching characteristics of the firms themselves. It is available from 1997 to 2004.

Table 19: Summary statistics, Sweden, 1999

	Eurozone (EZ)	EU15 nonEZ3	nonEZ Europe	Rest of World
Number of exporting firms	3,648	3,161	8,251	4,330
Number of products /a	24,214	18,049	54,362	30,624
Total number of product-destination pairs	75,134	25,185	100,780	98,756
Avg number of destinations per variety	3.10	1.40	1.85	3.22
Total value of exports (billions)	221,238	74,958	76,551	147,083
Market shares 10% largest exporters to:	86%	83%	89%	95%

Source: Martin Anderson.
Notes: a/ Number of distinct NC8 categories per firm summed over all firms

3. UNCONDITIONAL EVIDENCE ON THE NEWLY TRADE GOODS HYPOTHESIS

The basic assertion of the newly-trade-goods hypothesis is that the pro-trade effects of the euro manifest themselves in terms of the range of goods exported rather than an increase in the amount of each good exported. With firm-level data on exports that gives details on which products are exported and to where, the obvious test is a simple difference in difference comparison.

3.1. Treatment and control groups

Medical studies judge the effectiveness of a new medicine or 'treatment' by getting a group of patients and dividing them into a 'treatment' group that gets the medicine and a 'control' group that is as similar as possible to the treatment group but which does not get the medicine (usually they get a placebo). If the treatment group does better, the doctors concluded that the medicine was effective.

When it comes to the monetary union, the treatment is euro-usage and the treatment group consists of the member of the Eurozone. Since the 'medicine' may take time to work, we stick with the original 11 founding nations – France, Germany, Italy, Spain, Netherlands, Belgium, Portugal, Austria, Ireland, Finland and Luxembourg. The control group is less obvious. The most narrowly defined control group – i.e. the group that is otherwise most similar to the treatment group – consists of the three EU15 nations that did not join the Eurozone in 1999 but nevertheless were subject to the other EU integration schemes and faced broadly similar economic conditions. It is particularly important that these nations were 'treated' with a 'medicine' that the Eurozoners also

took – all the Single Market measures. (As noted in Chapter 2, the fact that Single Market measures were implemented in tandem with the euro-linked measures requires us to separate out the two effects.) The point is that if the euro boosted bilateral trade among euro-using nations above-and-beyond the effects of Single Market measures, then we should expect the data on intra-Eurozone trade flows to behave differently than the data on trade between the narrow control group and the Eurozone nations.

A somewhat less similar control group consists of the Western European nations who are not in the EU and thus not in the Eurozone, but who have deep integration agreements with the EU that implement all of the most relevant Single Market measures. This group consists of the rest of the EU27 plus Switzerland and Norway (Greece is again excluded due to data problems). The 12 new member states in Central Europe have been steadily implementing the EU's Single Market directives as part of their accession drives up to 2004. Norway is formally obliged to implement EU Single Market directives that concern trade as part the 1994 European Economic Area agreement; Switzerland does the same under the Bilateral Accords.

The third control group consists of the rest of the world.

Difference-in-difference tabulations and priors

In our tabulations, the first difference of our difference-in-difference technique is the growth in exports. The second difference compares the growth to the Eurozone to the growth to the control groups. This will be applied to five variables:

- Total exports;
- The number of firms exporting;
- The number of products exported by firms;

(here a product is defined as a single firm's export of a single NC8 category)

- The average number of destination markets per product exported.
- The average size of per-product export to a single destination market.

(this is total exports divided by the number of firm-NC8-destination combinations).

If the newly-trade-goods hypothesis is correct, we should see little difference in the differences when it comes to the average per-product export, but a big difference in differences when it comes to the number of products, the number of exporting firms and the number of destination markets per product.

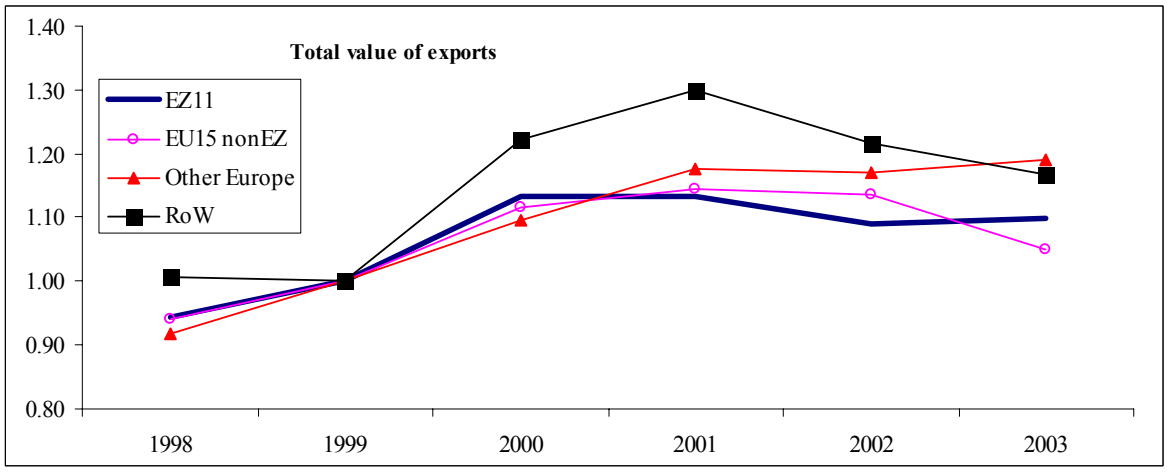
4. RESULTS

We consider the simple difference-in-difference evidence for each nation separately, starting with the French data.

4.1. Results from inside the Eurozone: the French data

Since we are looking at a simple hypothesis, we consider a very simple test. We plot the time series behaviour of total exports. Figure 10 shows the data indexed to be 1.00 in 1999 in order to highlight differences in post-1999 growth.

Figure 10: Total exports, diff-in-diff chart, France 1998-2003 (1999 = 1.0)



According to many of the aggregate trade flow studies, the euro trade impact had already started in this year, so the identification has to come from the deepening of the euro's integration discussed in the chapter on aggregate trade effects. Even taking this into consideration, Figure 10 makes it difficult to argue that there has been a substantial difference in French exports to the Eurozone and to the EU15 nonEZ, i.e. the other EU15 nations outside the zone (UK, Denmark and Sweden). Of course, this is not a fully satisfactory test since we have not controlled for non-euro factors such as GDP growth in the destination nations or real exchange rate movements, but it is at least clear that the unconditional differences are not massive.

4.1.1. Decomposing the change in total exports

Since the newly-exported-goods hypothesis focuses on the number of good exported and the number of markets served, it is useful to breakdown the growth in total exports into an intensive margin – the average exports per variety to the average destination – and an extensive margin – basically the number of varieties exported to all nations.

Since the mapping between the theory and the data is somewhat intricate, we define terms more precisely. The data gives us exports by firm, by NC8 category and by destination market. It does not give us information on varieties or products as defined in the models. To confront the theory with the data, we consider each firm-NC8 pair to be a unique variety. That is, if there are 10,000 firms and each exports in 2.1 NC8 categories, we count that as 21,000 varieties produced. If the average firm exports the average variety to 1.5 destination nations, we count that as 31,500 export links. Moreover, the number of 'export links' maps one-to-one into the number of beachhead costs that have been paid, assuming that each firm must pay a beachhead cost to introduce each product into each market.

In symbols, the decomposition is as follows:

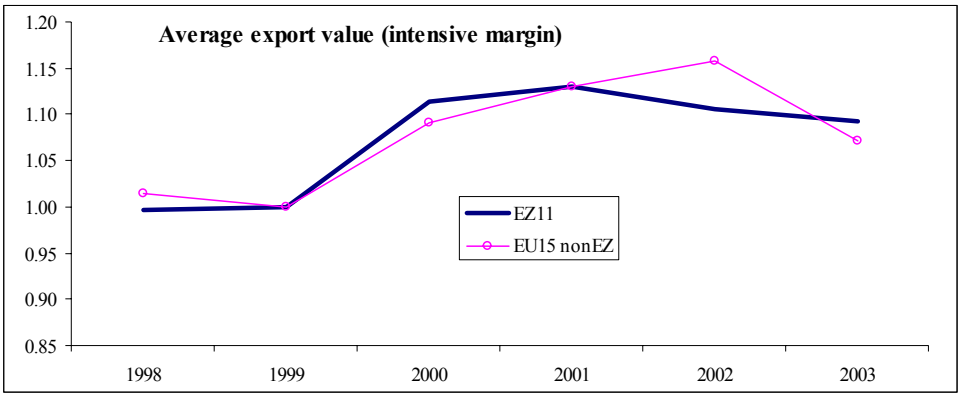
$$V = \bar{V} \times N_{links}\ ; \qquad N_{links} \equiv N_{firms} \times \bar{N}_{NC8} \times \bar{N}_{dest}$$

where V is the value of annual exports of all French firms to the group in question (i.e. the treatment and three control groups), $\bar{V}$ is the average sales per export "link", and the number of links is the number of distinct firm-NC8-destination combinations. The number of "links" can be broken down into the total number of firms, N_{firms}, the average number of NC8 products exported per firm, $\bar{N}_{NC8}$, and $\bar{N}_{dest}$ is the average number of destinations to which an average firm's average NC8's product is exported. It may be helpful to think of $\bar{N}_{NC8}$ as the 'product' extensive margin, $\bar{N}_{dest}$ as the 'geographical' extensive margin, and N_{firms} and the 'firm' extensive margin.

The intensive margin

We start with the intensive margin, namely the average export sale per variety per market by a given firm. To reduce clutter, we only plot data for the treatment group (EZ11) and the narrowest control group (EU15 nonEZ) in Figure 11 (see the appendix for a table with the full table of results). The data show that there does not seem to be much difference between the growth rates in the intensive margin between the two groups. This suggests that the action in the euro's trade effect – if there is any – must be coming from the extensive margin.

Figure 11: Average sales per product-firm-destination, France 1998-2003 (1999 = 1.0)



The extensive margins

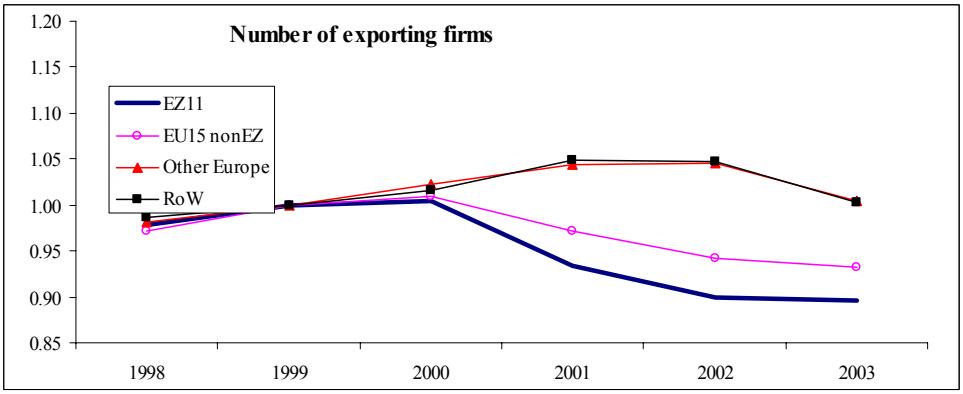
Relying on the simplest version of the simple newly-exported-goods hypothesis – the version that works with the Melitz (2003) model – we should expect to find that a trade enhancing policy change should raise the number of French firms to the Eurozone without changing the number of exporters to the control groups.

The 'firms' extensive margin

This simplest version is solidly rejected by the numbers shown in Figure 12. This shows that the number exporters to the Eurozone dropped significantly and indeed by a few percentage points more than the drop in exporting firms to the narrowest control group. This is not at odds with the basic theory – it may be that the Eurozone markets have become more competitive and thus fewer firms overall are exporting to those markets.[24]

[24] The basic theory tells us that a pro-trade policy reform should increase the share of producing firms that export, but the number of produced varieties should fall. In the Dixit-Stiglitz framework adopted by Melitz (2003), the absolute number of exporting firms must rise (see Baldwin 2005), but this need not hold in more general market structures, e.g. the Meltiz-Ottaviano (2008) model. In such models, it is possible that the total number of producing firms could fall enough (due to the fact that the euro made all the markets more competitive) to result in a drop in the number of exporters despite the rise in the share of exporters.

Figure 12: Number of exporting firms, by group, France 1998-2003 (1999 = 1.0)

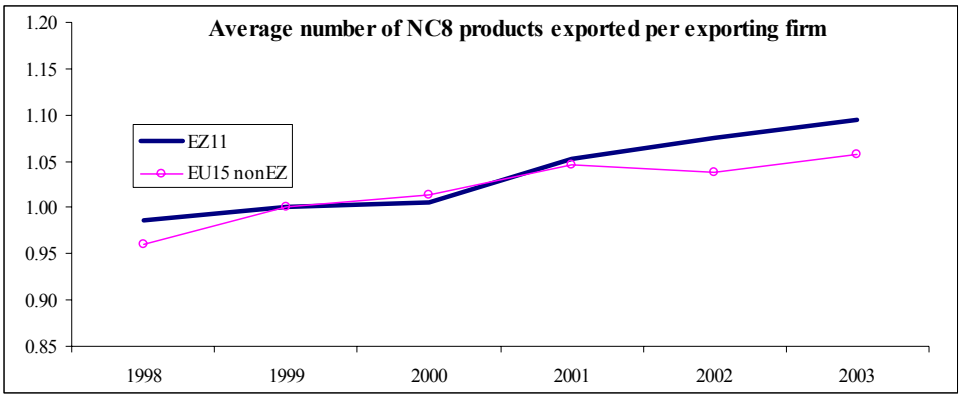


Interestingly, the depression of the firm-extensive margin is not observed for the two other control groups, or at least much less.

The 'product' extensive margin

The second part of the extensive margin we investigate is the average number of NC8 categories exported by the average firm. We can check this directly by looking at the growth differences by group for the total number of products exported, defining a product as a single NC8 category exported by a single firm. This is shown in Figure 13. Here we start to see a treatment effect in that the number of products exported per firm to the Eurozone markets rises more steeply than it does for the control group.

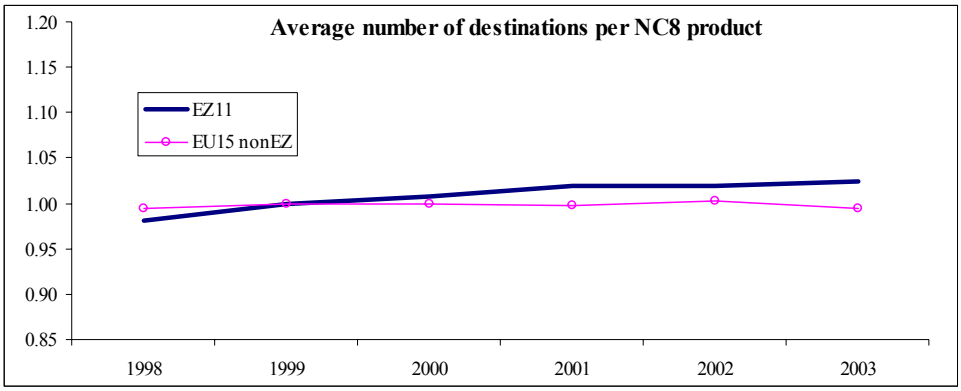
Figure 13: Number of exported products, by group, France 1998-2003 (1999 = 1.0)



The 'geographical' extensive margin

A final refinement margin concerns the range of destination markets -- the geographic extensive margin. Figure 14 shows the facts. Here we see a clear 'treatment effect'. That is, comparing the Eurozone (treatment group) and the narrowest control group (EU15 nonEZ), we see an important difference in the behaviour of the number of markets per product with the products exported to the Eurozone enjoying an growth of in the average number of markets while that same number of the non-EZ members of the EU15 did not.

Figure 14: Average number of markets per exported product, France 1998-2003 (1999 = 1.0)



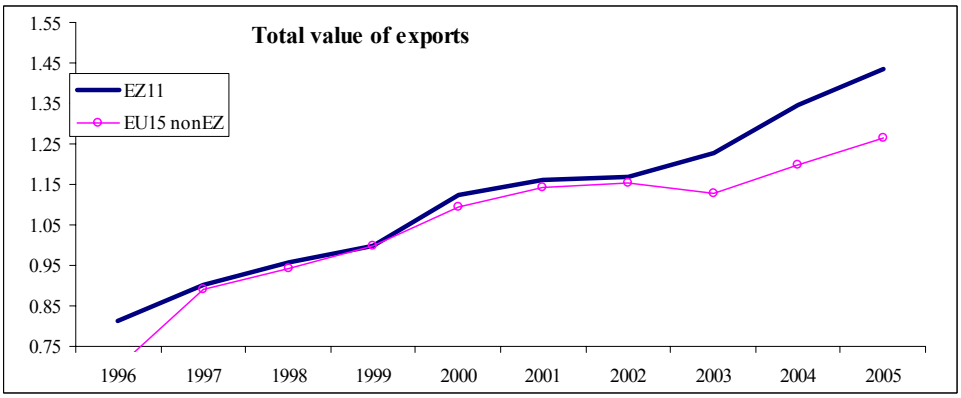
4.1.2. Summary

In summary, the French data provide no clear-cut prima facie evidence of an unconditional overall euro trade effect (Figure 10) or an unconditional impact on the intensive margin (Figure 11). What seems to have happened is that the euro has induced exporting French firms to export a wider range of NC8 categories and to a wider range of markets inside the Eurozone but these two extensive margins have been approximately offset by a reduction in the 'firms' extensive margin (i.e. fewer French firms exporting to the Eurozone, and other EU destinations, after the euro).

4.2. Results from inside the Eurozone: the Belgian data

The other Eurozone nation on which we have data is Belgium. For Belgium, the aggregate euro trade effect is clearer than it is in the French case, as Figure 15 shows, at least after the monetary union became a currency union in 2002.

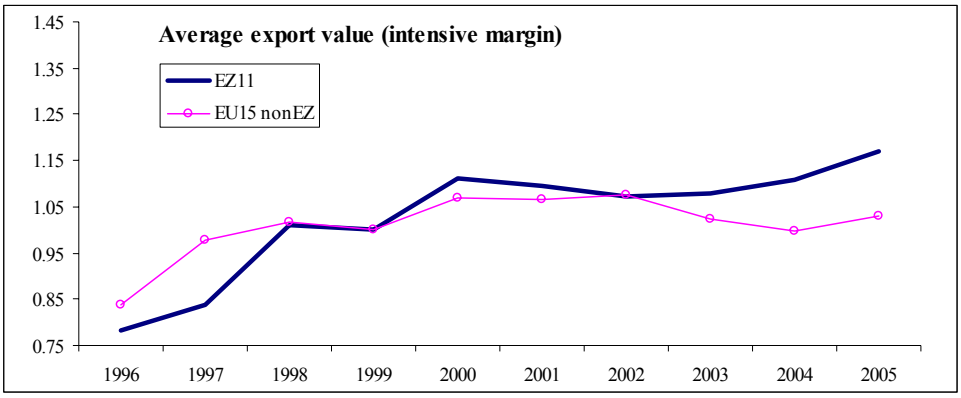
Figure 15: Total exports by group, Belgium 1996-2004 (1999 = 1.0)



The intensive margin

As a simple matter of logic, the overall effect is due either to an increase in the average exports per product, an increase in the number of products exported or a combination of the two. Turning first to the intensive margin, Figure 16 shows that common euro usage seems to have stimulated the average exports per variety per market, especially after 2002. This contrasts with the French experience.

Figure 16: Average sales per product-firm-destination, Belgium 1997-2004 (1999 = 1.0)



The extensive margins

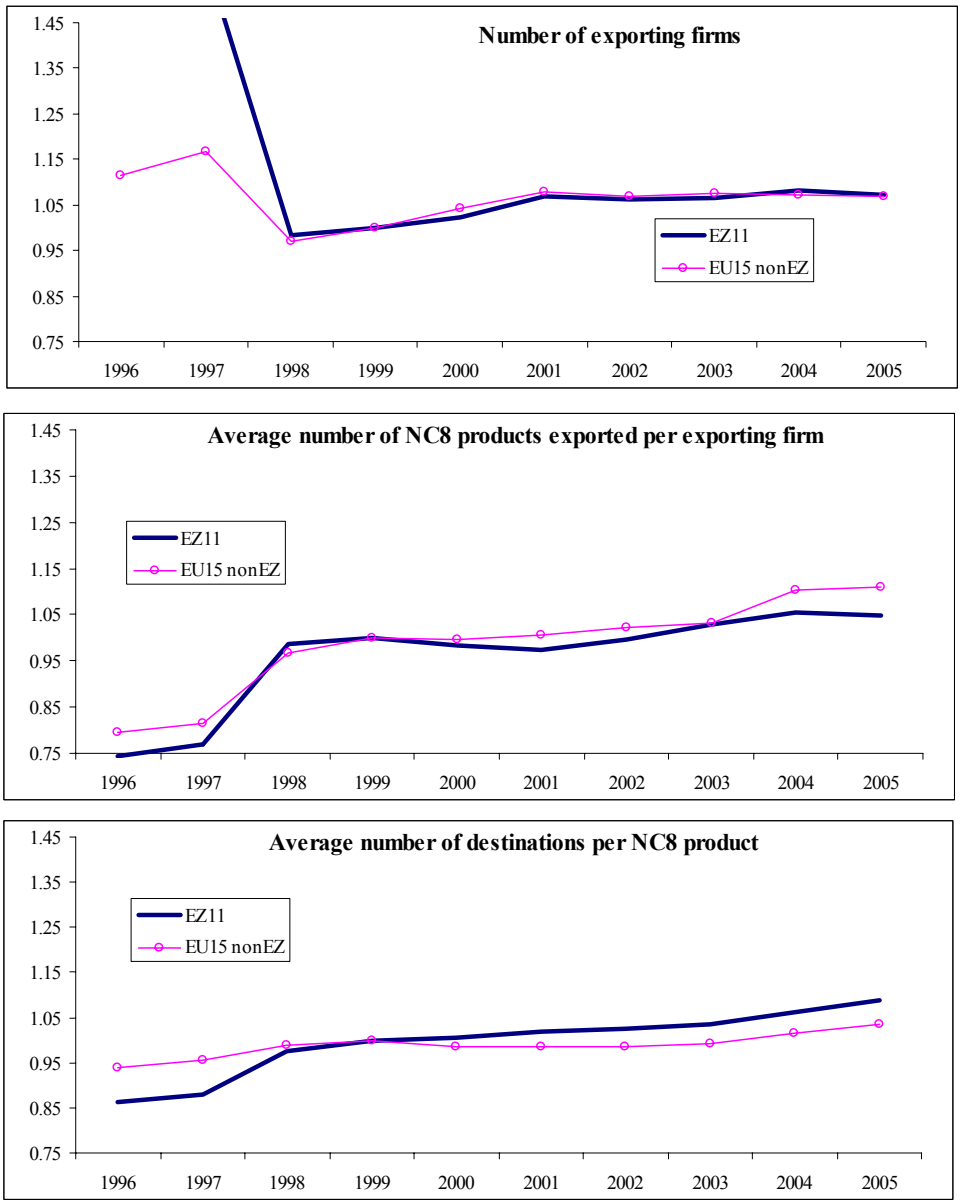
The extensive margin, however, also played a role in the Belgian case as the composite Figure 17 shows.

The number of exporting firms rose from 1999 (there was a problem with the reporting threshold in the data between the pre- and post 1998 period), but this occurred for both the EZ11 and the non-EZ EU15 nations (see top panel). The average number of products per exporter rose slightly more for the control group than for the treatment group (see middle panel), but the reverse held for the geographic extensive margin (bottom panel).

4.2.1. Summary

Overall, the Belgian data showed clearer evidence of a pro-trade effect of the euro – even without controlling for other factors such as GDP growth, but the source is more evenly split between the intensive margin and the extensive margin. In contrast to the French data, where there was no impact on the intensive margin – a finding that suggests very little role for the euro in reducing variable trade cost, i.e. transaction costs – we did find a difference in the difference when it comes to the intensive margin. In the Belgian data, there was a bigger increase in the exports per product to the Eurozone than to the most comparable control group. The theory would tell us that this would typically indicate that Belgian exporters benefited from a bigger increase in their competitiveness in Eurozone nations than they did in non-Eurozone nations.

Figure 17: The number of firms and average number of products and destinations, Belgium 1996-2004 (1999 = 1.0)



The extensive margin itself is mixed. The euro does not seem to have stimulated the number of firms exporting to the Eurozone compared to the control group, so the firm-extensive margin did not move. Nor did the product-extensive margin seem to behave differently in the treatment and control groups after the treatment (euro's introduction in 1999). The euro's effect on the extensive that showed up most in the geographical-extensive margin.

In summary, the Belgian data confirms the notion that newly-trade goods where an important part of the euro trade effect, but less categorically. In particular, there is evidence that the euro also lowered the transaction costs for Belgian firms.

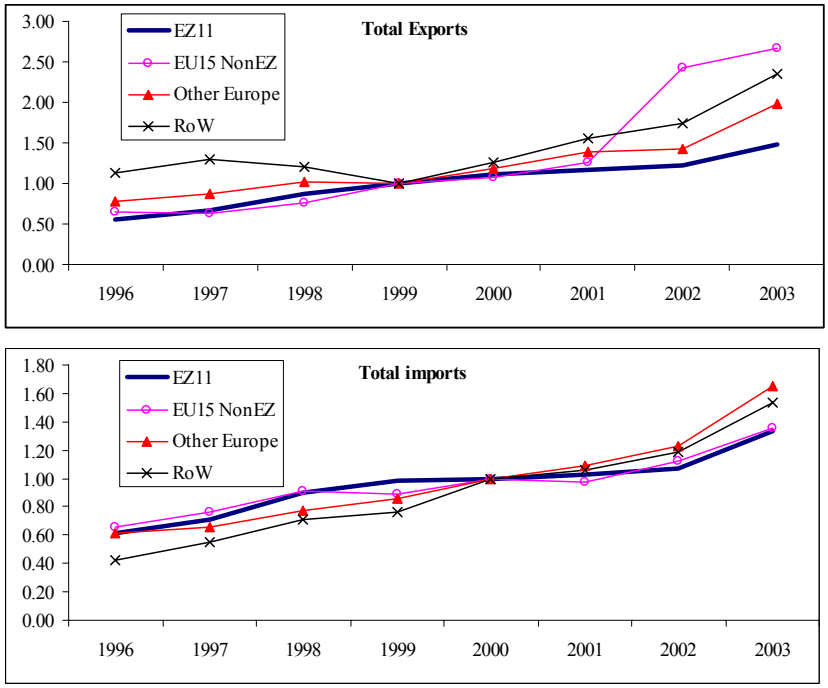
4.3. Results from outside the Eurozone: the Hungarian data

The newly-traded-good hypothesis concerns nations that use the euro in common, but there are several variants of the mechanism that would allow for a similar effect on export to the Eurozone

from non-euro using nations. It is thus very informative to consider similar data for outsiders. We start with Hungary which has excellent data both for its imports and exports.

When we first consider the unconditional change in Hungarian imports and exports (Figure 18), we see that there has been very little different between imports from Eurozone and other nations. On the export side, however, we see a very large jump in Hungarian exports to EU15 nonEZ nations. This is a statistic blip caused by Sweden's offset purchases that were agreed as part of Hungary's purchase of the Swedish fighter aircraft Grippen. Apart from this blip, however, there is no prima facie evidence that the euro's introduction has had much impact on Hungary's imports from the Eurozone or its exports to it.

Figure 18: Hungarian exports and imports by group, 1996-2003 (1999 = 1.0)



This in confirmed in a closer look at the elements of the extensive margin (Table 20). From this we see that there has been no noticeable difference in growth of the three elements comparing the treatment group EZ11 to the narrowest control group.

This result was not obvious beforehand since one could imagine that economies of scope in the usage of euro might have made it easier for Hungarian firms to export to smaller euro-using nations after having already sunk the cost to establish beachheads in the large Eurozone markets, especially Germany.

The basic result is the same for Hungarian imports, as Figure 19 shows. There is very little unconditional variation on the import per product (top left) and even less difference in the growth between to the treatment group and narrowest control group when it comes to three elements of the extensive margin, namely the number of importing firms, the number of NC8 categories per importing firm and the number of source nations per NC8 category.

In summary, the Hungarian data are a useful in two ways. First, it suggests that nations actually need to share the euro in order to get the effect. Second, it shows that in the case of trade flows that did not receive the euro 'treatment', the data behave in a way that could be expected, i.e. there is not real difference.

Table 20: Hungary's extensive-margin elements, exports.

	Number of Exporters			
	EZ11	EU15 NonEZ	Other Europe	RoW
1996	0.88	0.83	0.90	1.11
1997	0.93	0.90	0.93	1.13
1998	0.98	0.94	1.03	1.15
1999	1.00	1.00	1.00	1.00
2000	1.04	1.08	1.05	1.04
2001	1.06	1.12	1.11	1.14
2002	1.04	1.14	1.11	1.17
2003	1.00	1.16	1.13	1.14
	Number of NC8 categories by exporter			
	EZ11	EU15 NonEZ	Other Europe	RoW
1996	0.88	0.87	0.80	0.97
1997	0.91	0.86	0.85	0.96
1998	0.96	0.92	0.94	1.00
1999	1.00	1.00	1.00	1.00
2000	1.03	1.04	1.06	1.04
2001	1.06	1.09	1.10	1.12
2002	1.06	1.12	1.12	1.21
2003	1.07	1.19	1.08	1.14
	Destination markets per exported variety			
	EZ11	EU15 NonEZ	Other Europe	RoW
1996	0.99	1.00	0.98	0.99
1997	0.99	1.00	0.99	1.00
1998	0.99	1.01	0.99	0.99
1999	1.00	1.00	1.00	1.00
2000	1.00	1.01	1.01	1.02
2001	1.01	1.00	1.01	1.01
2002	1.01	1.01	1.01	1.01
2003	1.02	1.00	1.02	1.01

Figure 19: Hungarian imports, intensive margin and number of imports by group (1999 = 1.0)

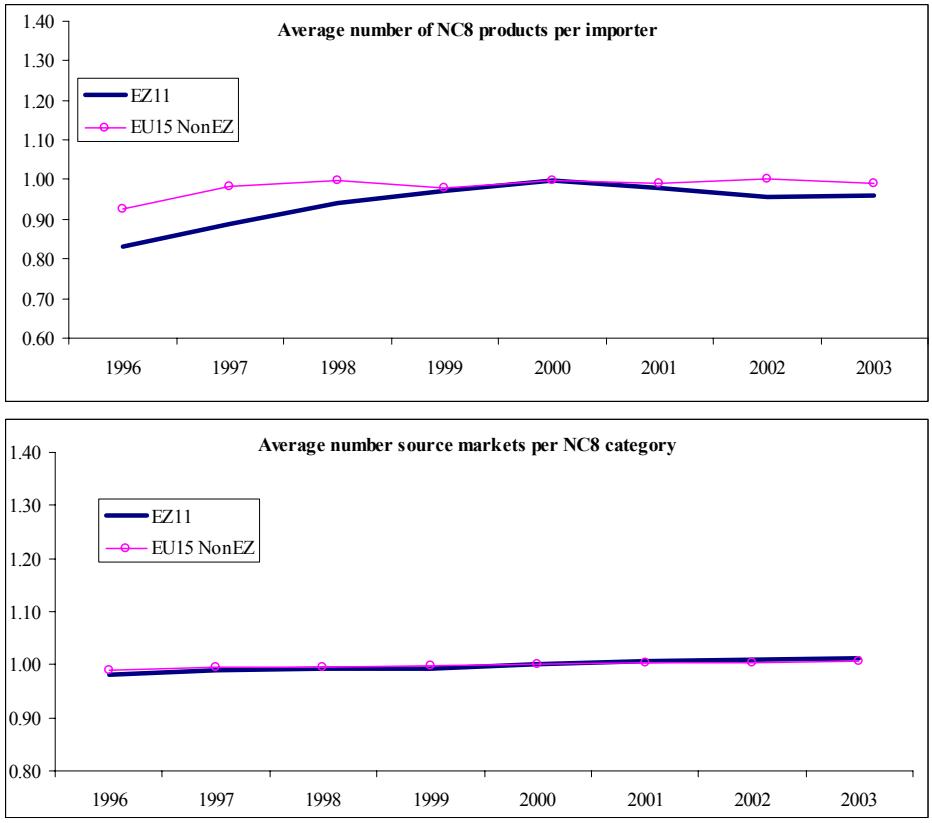
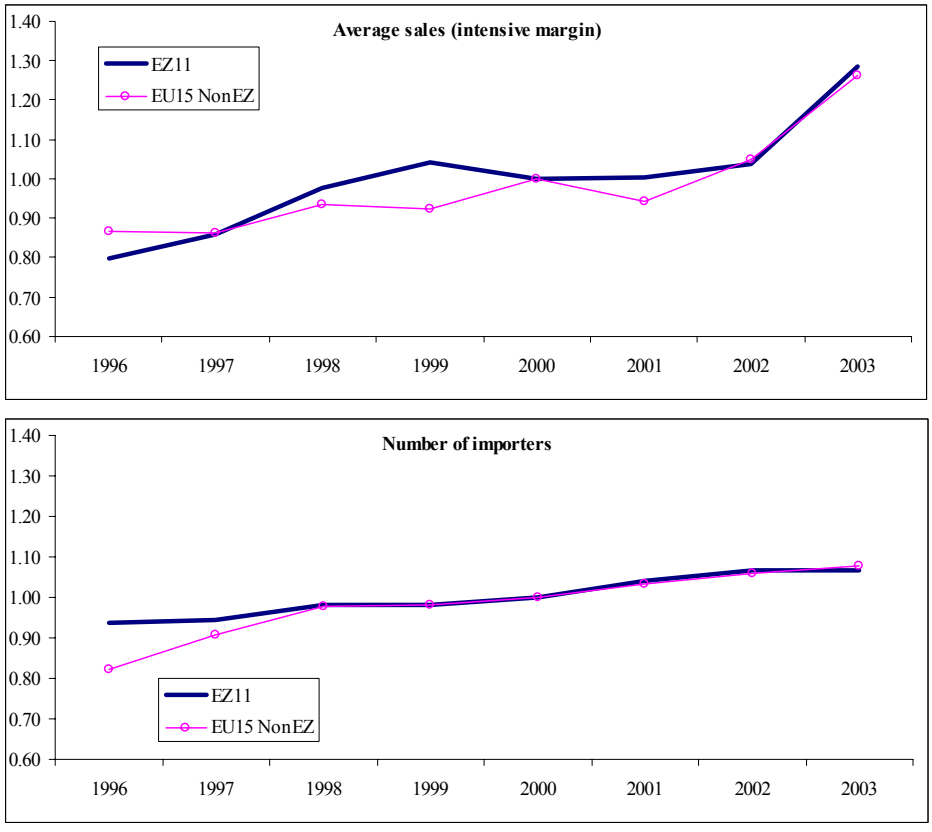


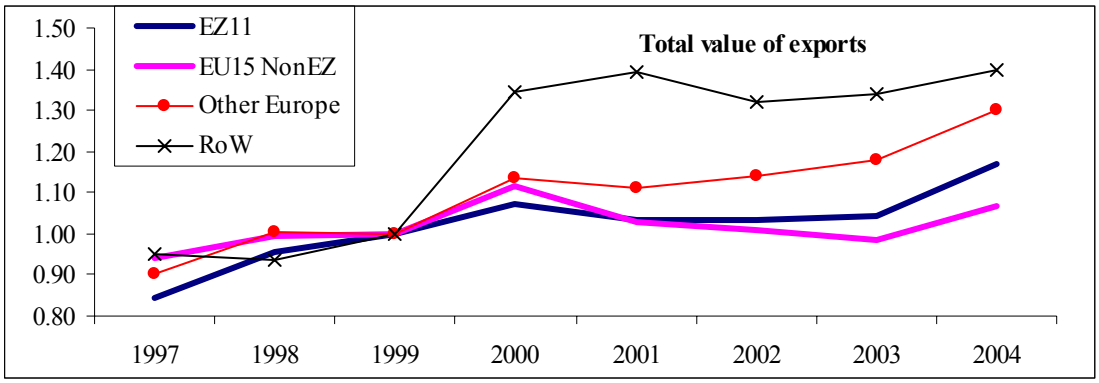
Figure 20: Hungarian imports, extensive margins by group (1999 = 1.0)



4.4. Results from outside the Eurozone: the Swedish data

As a member of the narrowest control group, the Swedish goods should shed a good deal of light on euro trade phenomenon. It is an advanced industrial nations with an export mix that is quite similar to that of the Eurozone. Moreover, as a member of the EU since 1994, it has been subject to the same Single Market integration policies that have affected trade among the Eurozone nations.

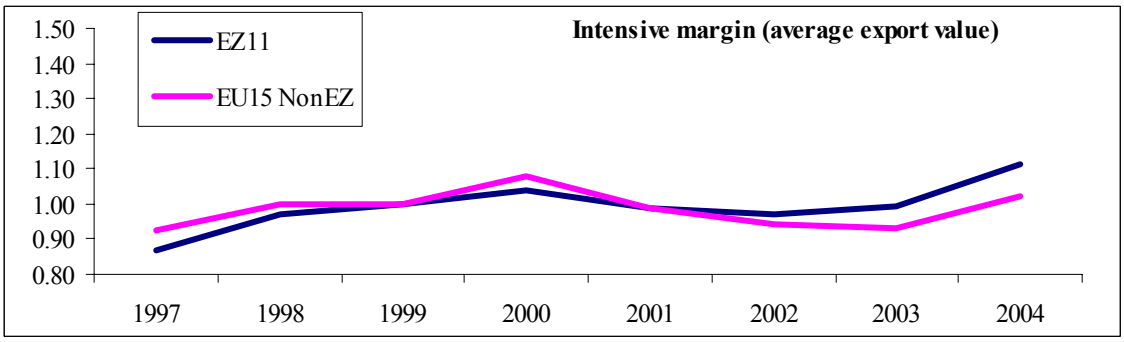
Figure 21: Swedish export by group, 1997-2004 (1999 = 1.0)



The fact show in Figure 21 reveal that there has been about a 10% difference in the growth of exports from Sweden to the Eurozone compared with the growth to the nearest control group (which contains only Denmark and UK when it comes to Swedish exports).

Much of the difference in differences that appeared in Figure 21 is in fact due to the intensive margin, as Figure 22 illustrates. The chart, which is drawn to the same scale as Figure 21, shows that 9 percentage points of the 10 point difference is due to an increase in the average export sale, i.e. the intensive margin (defined as total exports in a year divided by the number of firm-NC8 category-destination combinations).

Figure 22: Swedish intensive margin to the EZ11 and EU15 nonEZ groups (1999 = 1.0)



We can see this more directly by comparing the various elements of the extensive margin. While the evolution of the three elements is not identical for the treatment and EU15 non-EZ control group, the variation is quite small. The number of exporting firms rose somewhat more for the EZ11 as did the average number of products exported per exporting firm (defined as the number of NC8 categories). By contrast, the number of destination markets serviced actually fell slightly for the Eurozone but held its own for the EU15 nonEZ group. It should be noted, however, that this latter group contains only two nations in the Swedish case.

Table 21: Elements of the Swedish extensive margin

Number of exporting firms

	1997	1998	1999	2000	2001	2002	2003	2004
EZ11	0.95	0.90	1.00	1.03	1.03	1.05	1.05	1.05
EU15 NonEZ	1.03	0.90	1.00	1.02	1.02	1.03	1.03	1.01
Other Europe	0.99	1.02	1.00	1.00	0.99	1.00	0.99	0.97
RoW	0.97	0.98	1.00	1.04	1.05	1.04	0.98	0.97

Number of products

	1997	1998	1999	2000	2001	2002	2003	2004
EZ11	1.00	1.01	1.00	1.03	1.05	1.08	1.07	1.08
nonEZ3	1.03	1.00	1.00	1.03	1.04	1.08	1.06	1.05
nonEZeurope	0.98	1.03	1.00	1.03	1.02	1.10	1.04	0.97
nonEZworld	0.97	0.99	1.00	1.10	1.09	1.11	0.96	0.92

Number of destinations

	1997	1998	1999	2000	2001	2002	2003	2004
EZ11	0.97	0.98	1.00	1.00	0.99	0.99	0.98	0.97
nonEZ3	0.99	1.00	1.00	1.00	1.00	1.00	0.99	0.99
nonEZeurope	0.96	0.97	1.00	1.01	1.04	1.00	1.03	1.06
nonEZworld	1.00	1.00	1.00	0.98	0.99	0.95	1.04	1.07

In summary, the Swedish figures tend to confirm the Hungarian data. There is very little evidence of an unconditional newly-traded-goods effect when it comes to outsiders' sales to the Eurozone.

5. CONCLUDING REMARKS

Many of the unexpected features of the by now well-documented euro trade effect could be accounted for by the newly-traded-goods hypothesis. The obvious way to test this is to look at firm level data that give a break by firms, by narrowly defined product group and by destination nation. In this chapter we reviewed the prima facia evidence using data from 4 nations – two of them inside the Eurozone and two of them outside.

The findings suggest that some sort of newly-traded-goods hypothesis is plausible when it comes to exports within the Eurozone, but not with respect to outsiders. Of course, the evidence presented here does not control for other determinants of bilateral trade, but doing so would take us beyond the scope of this investigation. Indeed, three of the four national teams are starting on the necessary econometric exercises. Preliminary results for France suggest that the results from the unconditional data exercises in this chapter seem to hold up to more rigorous testing (see Berthou and Fontagné 2008).

Chapter 5: The newly-traded goods hypothesis: evidence from the trade data

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1. INTRODUCTION

The classic optimal currency area framework views adoption of a common currency as working like a customs union. Using a common currency reduces transaction costs and this stimulates trade among the currency union members. The economic logic of relative prices and the law of demand, however, imply that such trade diversion would result, if the euro's trade effects worked in this way. After all, if the euro makes it, say, 2% cheaper for a French firm to sell in Germany but does little to change the cost facing a British firm, the euro should have shifted competitiveness in the favour of the French firm and away from the British firm. In the data, this would have shown up as a negative coefficient on the EZ01 dummy in the Rose regressions.

As was amply demonstrated in Chapter 2, this did not happen. Like the famous clue in the classic Sherlock Holmes tale "Silver Blaze" – the dog did not bark – it is this absence of an effect that leads us to conclude tha the standard story cannot be right – or at least is cannot be the main mechanism. Chapter 4 explained the "newly-trade goods" hypothesis and used firm level data to test it. That evidence provided direct and fairly robust evidence that the euro has encouraged firms to export a wider range of products to the Eurozone. While firm level data is exactly what one needs to address the issue, it has a major drawback. It is not currently available for all, or even most, Eurozone nations. This chapter pursues a different line of empirics. It uses trade data that is publicly available for all Eurozone members and indeed for almost all nations in the world. The finest level of aggregation – the so-called HS6 digit level – involves something like 5,000 different product categories. From the perspective of the Chapter 2 regressions, which lumped all goods into a single aggregate, 5,000 products seems highly disaggregated, but from the perspective of Chapter 4, with its

[25] The view expressed in this study are those of the authors and do not necessarily reflect those of the Bank of Italy.

tens or hundreds of thousands of products, it seems hopelessly aggregated. The fundamental trade-off is between wide country coverage and data details. Chapter 4 and 5 should thus be viewed as complements.

This chapter reviews the existing literature and the essential theory before providing fresh evidence using the latest data and best econometric techniques.

2. LITERATURE REVIEW

Baldwin and Di Nino (2006) is the first paper to test the hypothesis that the euro boosted trade via the extensive margin; an early version of these results were published in Baldwin 2006a Chapter 6.

Baldwin and Di Nino (2006)

This paper uses highly disaggregated trade data, namely 6-digit HS export data extracted from the UN's Comtrade database (which amounts to approximately 5,000 different product categories) to study the impact of euro usage on the range of categories traded. Before turning to the details, we note that their empirical evidence is supportive of the newly-trade-goods hypothesis but not conclusive. Their estimation strategy is based on the Melitz (2003) heterogeneous-firms trade model. Since the Melitz model is similar in its foundation to the monopolistic competition trade model of Krugman (1980) and others, it very naturally generates a gravity-like estimating equation just as does the Krugman model. The key addition is the heterogeneity of firm's competitive. In equilibrium, only the most competitive firms find it worth their while to pay the costs of overcoming international barriers and thus exporting. To the extent that the euro reduced such barriers, more firms should export more products to the Eurozone. Taking this idea to the 6-digit data, the authors note that in some product categories no firms may find it worth exporting, but the integration implied by the euro could change this. Thus the key to their estimation comes from studying the impact of the euro on trade flows that already existed (the intensive margin) and its impact on promoting exports in previously un-exported goods. As it turns out, even at the 6-digit HS level – which certainly involves an aggregation of many products – there is a significant number of zeros even among the Eurozoners.

Plainly, this line of thinking suggests Tobit as a way of simultaneous estimating the impact on no-exports threshold and the change in exports of goods with positive values. An alternative, approach, to measure the effect on the no-export threshold (likelihood of exporting) is to run a Logit regression on the binary variable which sorts out bilaterally exported and non exported varieties, Baldwin and Di Nino (2006) do both.

The size of the dataset, however, posed practical problems for the authors. With 20 nations (the EU14 and 6 others) , there are 20 times 19 uni-directional country pairs for each product category. With about 5,000 categories, there are about 1.9 million sections in the panel data. This exceeds standard computational capacities of mainstream statistical packages. To get around this, the authors estimate the model with a single nation as the reporter and the other 19 nations as partners.

Note that this prevents the authors from simultaneously estimating EZ10 and EZ01, i.e. the euro's impact on external trade distinguished between external exports and external imports. To get around this limitation, they arrange the data set in two ways

and run regressions on both. The first views each of the 20 nations as exporters with the other nations as importers, e.g. Germany as the exporter and 19 other nations as importers. For Germany, this implies about 100,000 panels (19 nations each of which imports on average 5000 Germany product categories). For minor and less diverse nations such as Iceland, the number of panels is much smaller. The second set of databases involves a given nation as the importer and the other 19 nations as exporters, e.g. Germany as the importer and 19 other nations as exporters.

The authors find that the impact of common euro usage on trade is positive in all the exporter databases – although there is a good deal of variation across the 11 nations (one for each Eurozone nation as an exporter). The simple average suggests a Rose effect of between 3 and 4 percent. The highest estimate is 0.11 for Spain and the lowest is 0.004 for Belgium-Luxembourg. Only 5 of the 11 point estimates are statistically significant. In particular the point estimates tend to fall into two groups, those that are very small, between 0 and 0.03, and those that are relatively large, above 0.04; it is the large ones that are significant. The GDP marginal effects are small compared to the value theoretically predicted. Detailed data have never been used before in gravity estimation, if at aggregate level we would expect that an increase in the GDP of the destination country would entail a proportionally equal increase in trade with that country, we do not really know what is the relationship between each product exported by a country and the GDP of the destination country. The estimates on the Europe-only sample are quite similar (see Table 22).

Table 22: Tobit estimations for EZ members.

Database:	On Exporter Databases						On Importer Databases					
Sample:	Europe+3			Europe			Europe+3			Europe		
	Coef.	s.e.		Coef.	s.e.		Coef.	s.e.		Coef.	s.e.	
					EZ11 estimates							
Austria	0.06	0.01	*	0.06	0.01	*	0.05	0.01	*	0.05	0.01	*
Bel-Lux	0.00	0.02		0.00	0.02		0.02	0.01		0.02	0.01	*
Germany	0.01	0.02		0.02	0.02		0.02	0.02		0.02	0.02	
Spain	0.11	0.01	*	0.12	0.01	*	0.04	0.01	*	0.04	0.01	*
Finland	0.04	0.01	*	0.05	0.01	*	0.04	0.01	*	0.03	0.01	*
France	0.02	0.02		0.02	0.02		0.05	0.02	*	0.05	0.02	*
Greece	0.03	0.00	*	0.03	0.01	*	0.04	0.01	*	0.04	0.01	*
Ireland	0.00	0.01		0.04	0.05		0.08	0.01	*	0.07	0.01	*
Italy	0.01	0.02		0.01	0.02		0.02	0.01		0.02	0.01	
Netherlands	0.00	0.02		0.01	0.02		0.06	0.01	*	0.06	0.01	*
Portugal	0.06	0.01	*	0.06	0.04	*	0.08	0.01	*	0.09	0.01	*

Notes: an "*" indicates the variable is significantly different than zero at the 5% level or better. All regressions are done with Tobit estimation with time and partner dummies included (i.e. the classic fixed effects with a time-invariant dummy for each section). We also included a time dummies and a dummy for the time-varying aspects of EU membership but do not report the coefficients. EZ11 indicates that both partners use the euro.

Estimates of the euro's impact on the exports of non-Eurozone members suggest that the effect is quite different. Among the six EZ-outsiders, only Great Britain and

Norway saw significant increases in exports due to the Eurozone's adoption of a common currency. This conclusion does not change when we estimate the parameters using only European nations (second set of results). Intriguingly, it seems euro-usage had the biggest impact on non-European exporters to the Eurozone (EZ01), especially Canada. When using the full dataset, i.e. 17 European nations plus the US, Japan and Canada, they find that the euro-usage substantially boost the exports of outsiders to the Eurozone.

2.1. Logit regressions

The Tobit regressions pick up the total impact of the euro on trade, both expansion of trade in existing categories and the change in the number of categories traded. The logit regressions help separate out the impact on the number of goods traded by asking: How much euro does usage affect the probability that a particular product category is traded among nations when one or both trade partners use the euro?" The authors use the logit model, estimating the marginal impact of euro-usage on an observed (positive) trade flow controlling for economic mass via maximum likelihood. The results, shown in Table 23, provide direct empirical support for the 'new goods hypothesis'.

On the European-only sample (the second and fourth pairs of columns), most the point estimates are positive and the negative point estimates are statistically insignificantly different from zero in all cases except one. The average size of the impact is fairly modest, only about 2% on average in exporter databases and 6% on the importer databases.

The Europe-only datasets are more cautious in that they include only nations that have experienced Single Market integration (to varying degrees). In principle, this helps us separate the impact of the Single Market from the impact of the euro since all the nations have been 'treated' with Single Market integration, but only the EZ members have been treated with the single currency as well. It is useful, however, to check whether the results are robust to the inclusion of non-European nations. It is thus reassuring that on the full data samples (first and third sets of results) we find qualitatively and quantitatively similar results. The only two exceptions are represented by Portugal, whose positive effect seems to capture more the effect of Single Market Integration and Netherlands where the estimate on the restricted sample proves that euro-usage had a significantly positive impact on the probability of exporting new goods.

Table 23: Logit estimates: euro's impact on observing a positive trade flow (EZ11 estimates).

Database:	On Exporter databases						On Importer databases					
Sample:	Europe+3			Europe			Europe+3			Europe		
	Coef.	s.e.		Coef.	s.e.		Coef.	s.e.		Coef.	s.e.	
Austria	0.05	0.01	*	0.06	0.01	*	0.04	0.01	*	0.05	0.01	*
Bel-Lux	-0.01	0.01		-0.01	0.01		0.02	0.01	*	0.03	0.01	*
Germany	-0.04	0.01	*	-0.04	0.01	*	-0.02	0.01		-0.02	0.01	
Spain	0.07	0.01	*	0.07	0.01	*	0.07	0.01	*	0.02	0.01	
Finland	0.04	0.01	*	0.04	0.01	*	0.09	0.01	*	0.06	0.01	*
France	-0.01	0.01		-0.01	0.01		0.00	0.01		0.00	0.01	
Greece	0.11	0.01	*	0.11	0.01	*	0.13	0.01	*	0.05	0.01	*
Ireland	-0.01	0.01		-0.01	0.01		0.16	0.01	*	0.10	0.01	*
Italy	-0.02	0.01		-0.02	0.01		0.03	0.01	*	-0.01	0.01	
Netherlands	-0.03	0.01	*	0.08	0.01	*	0.04	0.01	*	0.04	0.01	*
Portugal	0.08	0.01	*	-0.03	0.01	*	0.11	0.01	*	0.07	0.01	*

Notes: an "*" indicates the variable is significantly different than zero at the 5% level or better. All regressions are done with logit maximum likelihood estimation with time and partner dummies included (i.e. the classic fixed effects with a time-invariant dummy for each section). We also included a dummy for the time-varying aspects of EU membership but do not report the coefficients. The dependent variable is 0 if there is no trade for a particular pair-product-year combination and 1 if there is. The pseudo R-squared in this set of estimation range always between 5% and 15%.

The impact of the euro on the probability of observing a positive trade flow between Eurozone nations and non-Eurozone nations is quite different from the impact on intra-Eurozone flows. The estimates are small and two of the six coefficients that are significant are negative (a negative coefficient here indicates trade diversion, i.e. the use of the euro by the importing nation but not the exporting nation makes it less likely to observe a positive trade flow in the typical product category).

Flam and Nordstrom (2006)

Flam and Nordstrom (2006) updates Flam and Nordstrom (2003) by using more recent data and testing the 'new-goods' hypothesis of Baldwin (2006). The authors estimate bilateral exports using OLS on a panel of data from 1995 to 2005. By starting in 1995, they avoid many of the statistical problems associated with the change in data collection techniques in 1993, and the EU enlargement in 1994. They focus on manufacturing trade only since they believe that big movements in commodity prices cannot be sufficiently controlled for with price indices.

Much of Flam and Nordstrom (2006) is concerned with the aggregate Rose effect (this part of the paper was reviewed in the chapter on aggregate effects), but they also test the newly-trade-goods hypothesis. Specifically, they first regress the number of products traded (using HS 6-digit level data) on their gravity equation variables. This empirical strategy is less clearly tied to the theory, but the use of count data (as opposed to zeros and ones and in Baldwin and Di Nino 2006) is a natural interpretation of the newly-traded-goods hypothesis. Using OLS regression techniques, they find that the EZ dummies are positive and significant in the 1999-2001 and 2002-2005 periods. Of course, the left-hand variable in these regressions are count data, and so are bounded at zero. This means that the simple OLS technique is not entirely appropriate, so we must be cautious in interpreting the results;for these type of estimations a poisson regression is the natural candidate.

In a second empirical approach, the authors classifies bilateral trade into products that were always traded and the rest. Roughly speaking, one can associate changes in the former with the intensive margin and the residual data with the extensive margin. Using their usual gravity specification, they show that while both intensive and extensive margin EZ effects are highly significant, the extensive margin effects are about three times larger.

2.2. Nitsch (2007)

This paper summarise some of the findings from the author's engagingly title (but as yet unavailable) 2006 paper "Scalpel, Please! Dissecting the Euro's Effect on Trade." Specifically it presents a plot of the number of zeros at the NC8 digit level (which is a finer disaggregation that the HS6, but which is only available for EU members). It shows that a time series plot of the number of categories with positive exports to the Eurozone from 1995 to 2004 rose in approximately the way for exports coming from Eurozone nations as it did from non-Eurozone nations. He takes this as prima facie evidence that the euro has had no impact on trade in Europe.

3. UPDATING THE ESTIMATES

The newly-trade-goods hypothesis is intuitively obvious, once one grasps a simple fact. The Eurozone trade matrix is full of zeros; at the HS6 level of disaggregation, more than half of all possible trade flows are missing – even between nations as thoroughly integrated as Eurozoners. Since small nations tend to export a narrower range of products, the number of zeros is even higher for Eurozoners other than the big three (Germany, France and Italy). Given this fact, it is clear that the euro may be stimulating trade by filling in some of those zeros even though the standard Mundellian thinking on the subject is that the euro would stimulate the amount of trade in pre-existing products.

Going from this "intuitively obvious" explanation to an econometric specification is a process that is full of pitfalls. As the discussion of the Rose regressions in Chapter 2 revealed, one can commit all sort of serious errors if one does not consider the theory. Fortunately, the theory of trade zeros has made huge strides in the last 5 years or so, with the seminal article being Melitz (2003).

3.1. Theoretical underpinnings of the estimation strategy

The Melitz model is well-known so here we merely review the features that shape our empirical strategy (see Baldwin 2005 for a simplified introduction to the model and its main properties). The Melitz model is basically a Krugman (1980) model with two key innovations: fixed cost of entering a new market (so-called beachhead costs) and differences in firm's marginal production costs.

Here is the Melitz model's economic logic in a nutshell. Due to the fixed market-entry costs, only firms that are sufficiently competitive will find it profitable to pay the beachhead costs and thus export to foreign markets. Since firm's marginal costs are heterogeneous, we can have cases where some firms will export while other will not, but we can also have cases where no firms located in a particular origin-nation will export to a particular destination-nation in a particular product category. This explains the zeros on the trade matrix.

Here is the economic logic of the euro's impact in a nutshell. To the extent that the euro reduces the beachhead costs (e.g. overhead costs like hedging or forex costs) and/or the variable costs of trade (e.g. transaction costs), then firms in the Eurozone will find it more profitable to export to Eurozone markets. If the effect is big enough, some of the zero trade flows will switch to positive trade flows. Aggregating across all products, this would show up as a positive Rose effect.

More formally, exports from nation-o to nation-d are determined by two conditions: 1) nation-o's cut-off condition for exports to nation-d and 2) nation-o's total mass of produced varieties.[26] The condition that defines the threshold-marginal-cost for firms exporting from nation-o to nation-d is the pair-specific export cut-off condition:

$$\left(\frac{\overline{a}_{od}\tau_{od}}{1-1/\sigma}\right)^{1-\sigma}\frac{\mathbf{B}_d}{\sigma}=F_d^X \tag{2}$$

Here 'a' is firm-specific marginal cost (using the standard Ricardian notation of 'a' for a unit-labour-input coefficient), and $\overline{a}_{od}$ and τ_{od} are the pair-specific threshold-marginal-cost and the bilateral trade costs; B_d is the demand shifter in nation-d, namely $E_d/P_d^{1-\sigma}$ where E_d is total expenditure in nation-d on all varieties, P_d is the usual CES price index. F_d^X is the fixed cost of entering the market in nation-d; $\sigma>1$ is the constant elasticity of substitution.

The equilibrium in nation-o is characterised by one cut-off condition for every market in the world, including nation-o's own market.

Turning to the value of sales from nation-o to nation-d we have from the usual CES demand function that the value of *per-firm* bilateral exports measured in terms of the numeraire is:

$$v_{od}[a]=\begin{cases}\left(\dfrac{a\tau_{od}}{1-1/\sigma}\right)^{1-\sigma}\mathbf{B}_d\,, & a\le\overline{a}_{od}\\ 0\,, & a>\overline{a}_{od}\end{cases}$$

We do not have data on the firm-level exports, so taking this to the data requires us to sum over all firms. This in turn requires us to be specific about the distribution of firms with various marginal costs, i.e. the a's. As in Melitz, one assumes that the distribution is given by a cumulative probability function G[a], where the support of *a* ranges from zero to some maximum. Note, however, that typically the most inefficient firm that actually produces will have a level of marginal cost that is below the maximum. The reason is that firms must pay a beachhead cost to enter their domestic market so only sufficiently competitive firms will do so – other firms will either abandon their projects "on the drawing board" or will start up production but go under quickly. Since the determinants of the domestic threshold are quite similar to those of the export threshold, as per (2), we denote it as $\overline{a}_{oo}$, i.e. the maximum marginal cost of an o-based firm selling to nation-o. Given this, we can find *total* bilateral exports by integrating of all possible a's:

$$V_{od}[a]=\int_0^{\overline{a}_{od}} n_o\left(\frac{a\tau_{od}}{1-1/\sigma}\right)^{1-\sigma}B_d\,dG\left[a\middle|\overline{a}_{oo}\right],\quad a\le\overline{a}_{od},\qquad V_{od}\left[a\right]=0\,,\quad a>\overline{a}_{od}$$

[26] For a complete presentation of a multi-country version of the Melitz model, see Helpman, Melitz and Yeaple (2004).

where $G[a|\bar{a}_{oo}]$ is the conditional density function that describes the distribution of marginal costs in nation-o; it is conditioned on the domestic threshold marginal cost $\bar{a}_{oo}$ since only firms that produce (i.e. have a's below $\bar{a}_{oo}$) can export.

Re-grouping the variables that are specific to nations o and d, and those that are specific to the bilateral relationship, we have:

$$V_{od} = \begin{cases} \tau_{od}^{1-\sigma} B_d \left\{ n_o \int_0^{\bar{a}_{od}} a^{1-\sigma} dG[a|\bar{a}_{oo}] \right\} \left(1 - \frac{1}{\sigma}\right)^{\sigma-1}, & a \le \bar{a}_{od} \\ 0, & a > \bar{a}_{od} \end{cases} \quad (3)$$

As far as our estimation strategy is concerned, the salient points of (3) are that a drop in the bilateral trade cost, τ, or the fixed market entry costs, F_X, will stimulate bilateral exports. Crucially, these trade cost reductions can induce firms to start exporting across a bilateral relation when previously there was no trade. Our empirical strategy is to focus on exactly the bilateral trade flows that switch from zero to a positive number, although we also look at the change in existing trade flows.

3.2. Estimation strategy

The ideal dataset for this kind of study should contain product-level, firm-level bilateral trade data. This would allow us to pick up bilateral switches in export behaviour at the individual product and firm level. Since these data are not available to us, we use the most detailed direction-of-trade data that is available for a wide range of nations, namely trade data at the Harmonised System's (HS) six digits level (available from the UN's Comtrade database). We focus on the 1995 to 2006 period since 2006 is the last available year and the data before 1995 are contaminated with issues concerning VAT fraud (see Baldwin 2006 for a discussion). The countries in our sample are the EU15 nations (Germany, Greece, Spain, France, Ireland, Italy, the Netherlands, Austria, Portugal, Belgium and Finland; data for Luxembourg and Belgium are fused), three other West European nations (Switzerland, Norway, Iceland) and three non-European nations (USA, Canada and Japan). There are more than 5000 product lines at the 6-digit level so the total data set for our 20 nations and 12 years is large. Comtrade does not report zero flows but rather omits the product-line altogether; we deduce the existence of the zeros by squaring the database nation by nation. This means that we have no zeros for product-lines that were never traded by the nation concerned.[27] After this procedure, we have about 20 million observations. Data on GDPs were gathered from the World Development Indicators of World Bank.

[27] For example, for the database that has Germany as an exporter, there are no zeros for product lines that Germany never exported to any of the 19 partners in any of the years. This is not a problem given our inclusion of pair dummies for each product line since this means that the coefficients are identified off of time-series variation, i.e. that time-dummy fixed effects would have absorbed the always-zero sections in any case.

Each of our product categories encompasses a range of individual goods, so we cannot hope to pick up the full extensive margin. For example, we cannot identify cases where euro usage induces more varieties to be bilaterally traded in an HS 6-digit category that has always had positive trade flows. We can, however, detect the extensive margin in cases where a bilateral trade flow switches from zero to positive since we know that the number of trade varieties was zero before and positive afterwards. In other words, when we observe a positive bilateral trade flow between a pair of nations in a particular product category, we suspect that it includes many different varieties but we do not know how many. Thus we cannot ascertain the full link between euro-usage and the number of varieties.

The bilateral trade volume equation (3) suggests a gravity-like estimating equation with a twist. The twist comes from the computing limitations that prevent us from specifying a standard gravity equation panel where each potential product-pair combination has its own column with years making up the rows. Instead, we organise the data by exporters or by importers. For exporters, this means that we have 20 data sets (one for each nation) so there is only one exporter in each data set and the number of columns is the number of HS6 products (about 5,000) times the number of partners (i.e. 19); the rows are the years as usual. For the importer datasets, the organisation is the same but there is only one importer and 19 partners (i.e. sources of imports). What this means is that the single-nation's GDP will be fully co-linear with the time dummy, so we omit it.

A second issue arises regardless of the particular organisation of the data. Here some of the trade flows are "censored". Were it not for the beachhead cost, the model predicts that there would be no zeros in the trade matrix – as in the Krugman (1980) trade model, there would be at least a tiny amount of trade as long as bilateral trade costs are less than infinite. The beachhead costs, however, means that if the trade flow is small enough, firms will not find exporting worthwhile and so we will observe a zero trade flow. In econometric terms, the observation is censored even though the latent variable (trade as predicted by the top expression in (3)) is positive.

Dealing with this sort of censored data is routine, but requires some care – the standard OLS procedure is not correct. The new issue we address in this paper is the effect the euro had on trade in products that were not previously traded. To illustrate this we undertake binary estimation, namely where the left-hand side consists of zeros and ones (zeros if there is no bilateral trade in the particular product category in a particular year and one otherwise). We estimate this with the logit model. Note that the estimation coefficients do not directly give us the marginal effect of, for example the EZ dummy, on the probability of observing a positive trade flow, but they are related. In logit models the marginal effects are obtained as follows:

$$\frac{\partial P\left(b_{od}=1|x\right)}{\partial x_i}=\frac{1}{1+e^{x\beta}}\frac{e^{x\beta}}{1+e^{x\beta}}\beta$$

which in the case of binary regressors simplifies to:

$$\frac{\partial P\left(b_{od}=1|x\right)}{\partial d}=\left[\frac{e^{x\beta+\alpha+d}}{1+e^{x\beta+\alpha+d}}\left|(dummy=1)\right.\right]-\left[\frac{e^{x\beta+\alpha}}{1+e^{x\beta+\alpha}}\left|(dummy=0)\right.\right].$$

When dummy coefficient is small, the effect on the binary regressors can be approximated by:

$$\frac{\partial P\left(b_{od}=1|x\right)}{\partial d_i}=\frac{e^{x\beta+\alpha}}{1+e^{x\beta+\alpha}}\left(e^{d}-1\right)\approx\frac{e^{x\beta+\alpha}}{1+e^{x\beta+\alpha}}d$$

The last equation proves that the marginal effects can be computed multiplying estimation coefficient by the cumulative density function of a logistic distribution evaluated at a given point.

The logit is one specific distribution of errors. The same logic, however, can be applied to the probit model.

3.2.1. Data

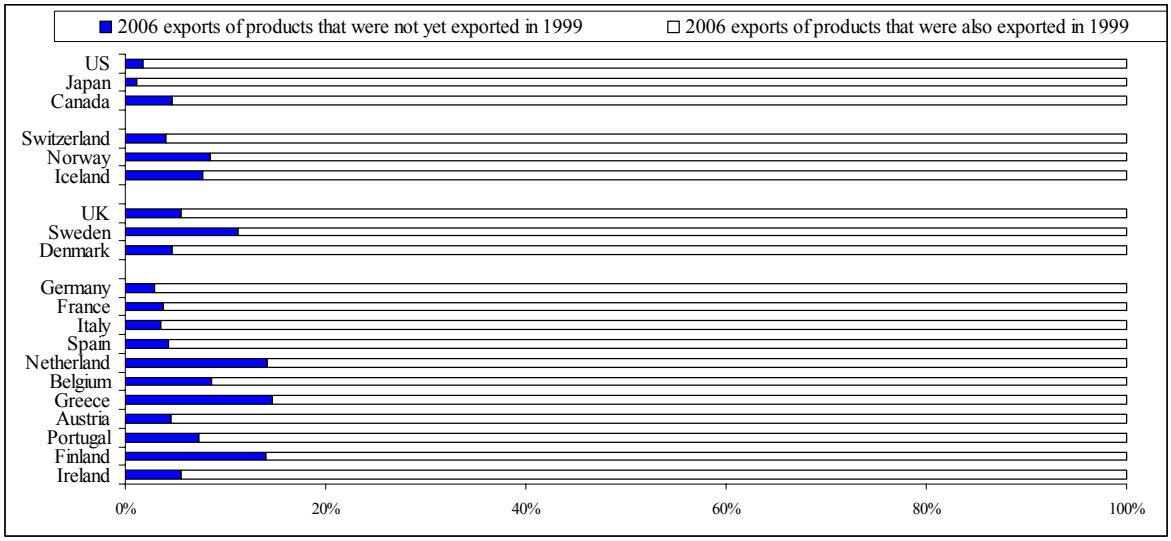
Taking theory to empirics, we notice that B_d equals $E_d/P_d^{1-\sigma}$, where E is expenditure. We proxy for E_d with GDP of the importing (destination) nation. Since n_o is related to the endowment of the exporting (origin) nation, we proxy for it with GDP of the exporting nation.[28] The remaining terms, including bilateral trade costs, the $P_d^{1-\sigma}$ terms and the additional nation-o specific factors affecting n_o are controlled for with time-invariant pair dummies.

Since euro usage is time-varying, we identify its pro-trade effect with three direction-specific dummies: EZ11, EZ01 and EZ10, where the first '1' indicates euro usage by the origin nation, and '0' for non-usage; the second digit indicates th

e same for the destination nation. Thus EZ11 indicates that both nations use the euro, EZ01 that only in importing (destination) uses it, etc. The control group is EZ00, i.e. nations that do not use the euro. Finally we include dummies to control for single market membership (eu_t).

The inclusion of pair dummies means that all the identification of EZ and GDP parameters will come from time-series variation. Thus, we can think of our regressions as difference-in-differences, where the first difference is change in bilateral exports over time and the second difference is between the behaviour of 'euro-treated' bilateral flows and 'untreated' flows.

Figure 23: Overall importance of the extensive margin by nation



[28] See Helpman, Melitz and Yeaple (2004) on the relationship between n_o and nation-o's endowment.

We estimate the model using three estimators: Tobit, OLS and Logit.

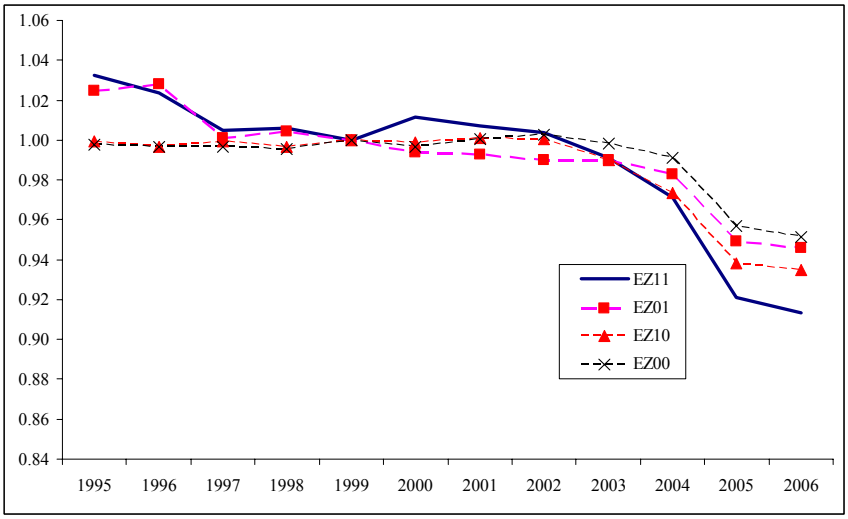
3.3. Prima facie evidence

The newly-traded-goods hypothesis is all about reducing the number of zero trade flows. To start with, it is useful to get a feeling for how important this sort of 'new' trade is for the nations in our sample. The answer, shown in Figure 23, is that the extensive margin accounts for between 5% and 15% of trade in 2006, taking 1999 as the base year. In calculating this, we consider nations' exports to all of the other 19 the nations in our sample in 2006. Some of this trade involves products that were already exported in 1999, while the balance consists of products that were 'new' in the sense that they had not been exported in 1999. What this tells us is that the extensive margin is significant in its size, but hardly dominant. For comparison, keep in mind the fact that credible estimates of the euro's pro-trade effect are around 5%.

More specifically, the newly-traded-goods hypothesis concerns the reduction in the number of zero export flows into nations using the euro. The obvious thing to look at is the evolution of the number of zero before and after the euro's introduction in 1999. To focus more clearly on the impact of euro-usage – as opposed to other determinants of zeros – we take the difference in difference approach. We look at the number of zeros in the export flows of Eurozoners to non-Eurozone nations and compare it to the number in the Eurozoners' exports to fellow Eurozone members. If the newly-trade-goods hypothesis is correct, the number of zeros on the EZ-to-EZ flows should drop more than it does for EZ-to-nonEZ flows. This gives us a first hint, however, it is not conclusive. Logically, it could be something particular about the Eurozoners that leads them to import a wider range of goods from all nations. If this is true, then the first hint is spurious; it is not the euro causing the effect on the trade flows but rather changes inside nations using the euro (say, a consumption boom or switch to imported intermediate goods). It is easy to check for this, however. All we have to do is look at the evolution of zeros from non-EZ nations to the Eurozone and compare it with the zeros in the non-EZ nations' exports to other non-EZ nations. If the 'first hint' result was driven by some peculiarity of the Eurozoners as opposed to common euro usage, then we should see a similar pattern for the non-EZ nations' zeros.

To control roughly for the impact of the Single Market, we consider only the EU15 nations, so the non-Eurozoners comprise Britain, Denmark and Sweden.

Figure 24: Evolution of zeros, prima facie evidence on the newly-traded-goods hypothesis



The facts are presented in Figure 24. Four lines are plotted: the index (1999=1.0) of the number of zeros in EZ-to-EZ trade, EZ-to-nonEZ trade, nonEZ-to-EZ trade, and nonEZ-to-nonEZ trade. The solid line – which is the index of number of zeros in in EZ-to-EZ flows – show clear evidence in support of the hypothesis since it falls more steeply after the monetary union in 1999 and especially after the currency union in 2001. The key control group in this comparison is the evolution of zeros between non-users of the euro, namely the EZ00 index. This index is quite flat up to 2003 or 2004 when it declines, but not as sharply as the EZ11 index. We take this as clear evidence in support of the notion that common euro usage promoted trade in previously un-traded goods.

To check that it was the common euro usage as opposed to something special about the EZ nations as importers, we look at the index for EZ01, i.e. for non-Eurozoners' exports to the Eurozone. This also falls, especially during the late part of the sample, but not by as much as the EZ11 index. To check whether the euro might be exclusive working by making Eurozoners better exporters, consider the EZ10 index, which reflects the increase in exports from insiders to outsiders. The time pattern is similar to EZ11 but muted.

In summary, Figure 24 provides the basis for believing our econometric results below – namely that it seems that the euro has had an independent impact of the extensive margin of trade.

3.4. Econometric results

The econometric test of the newly-traded-goods hypothesis that is simplest to motivation is a straightforward estimation of the impact of euro-usage on the likelihood that any particular product will be exported. Applying the Melitz theory to our HS6 level data, this translates into the probability that at least one firm in the origin nation has a marginal cost that is below the cut-off marginal cost for a particular destination country. As discussed above, the threshold falls as the bilateral trade costs rise (both variable and fixed) while the threshold rises as the size of the destination market rises. It is natural to take the destination nation's GDP as a

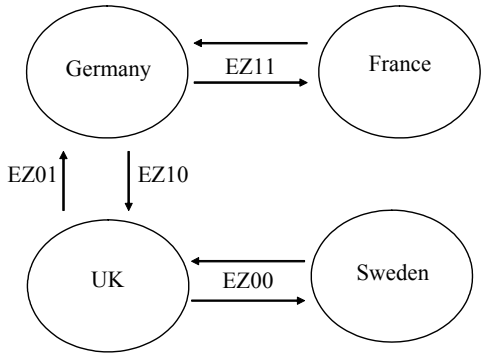
measure of market size, with distance, EU and EZ membership as affecting bilateral trade costs.

Subtle interpretation of the numbers

Given the computational limits that forced us to use nation-specific databases, we have to be careful in interpreting the estimates of the euro effect. The issue concerns the 'control group', and it is best understood with the help of a diagram and a comparison with the aggregate Rose regressions. In a sample that includes Euozoners and others, there are four types of uni-directional trade flows, as shown in Figure 25. Using the digit from-to notation, trade flows between Eurozoners is marked as EZ11.[29] Those from a Eurozoner to other nations are indicated by EZ10, and EZ01 indicate flows from non-Eurozoners to members of the Eurozone.

Economically, it is easy to believe that the true values of the three dummies have different magnitudes. EZ11 picks up the effect of common euro usage, while EZ10 picks up the euro's impact on Eurozoners' export ability to the rest of the world, and EZ01 picks up the euro's effect on non-Eurozoners' tendency to export more to the Eurozone. The classic Mundellian optimal currency area stories involving reduced transaction costs apply only to EZ11, but it is easy to think of economic mechanisms that would make the Eurozone a more attractive export market to outsiders. For example, there would be economies of scope in the financial, hedging and account issues involved in the foreign currency generated by export sales. It is harder to think of reasonable mechanism that would suggest the euro-usage makes Eurozoners better exporters to the rest of the world.[30]

Figure 25: Interpreting the estimates: treatment and control groups



When we can use the full panel, as we did with the aggregate estimates in Chapter ??, we can always take EZ00 as the control group by including dummies for the EZ11, EZ10, and EZ01 trade flows. This option, however, is not available to us due to computational problems discussed above. When we use our bilateral datasets, we cannot use the same control for each dataset. Critically, this fact will mean that the interpretation of the EZ dummy will vary across datasets.

Consider the estimate when we use the database consisting of, for example, French imports from the 19 nations. Here the EZ dummy is 'on' when France is importing

[29] The first "1" means the origin nation is a Eurozoner and a "0" means it is not; the second indicates the Eurozone status of the destination nation with 1 and 0 indicating yes and no respectively.

[30] Of course, if the euro boost firm-level scale economies sufficiently, it could produce extra exports.

from another Eurozoner, but 'off' for imports from non-Eurozoners. In this case, the EZ dummy is really EZ11 as per Figure 25. Moreover, the control group is EZ01, namely the flows from non-Eurozoners to France. Since the value of EZ01 will be folded into the regression constant, what we are actually estimating is the difference between EZ11 and EZ01.

Consider next the French export dataset, i.e. the data arranged with France as the origin and the 19 other nations as destinations. Here the EZ dummy will be EZ11 as before, but the control group is EZ10 (instead of EZ01). The coefficient on the EZ dummy will therefore be the difference between EZ11 and EZ10. According to most aggregate estimates, which confirm the initial evidence in figure 2, EZ10 is smaller than EZ01, so we should expect the euro dummy coefficient to be larger on average with the import databases.

Finally, when we look at the import and export dataset for an outsider, say, Britain, the estimated EZ dummy is really the difference between EZ10 and EZ00 for the export datasets, and EZ10 and EZ00 for the import datasets.

In summary, the coefficients estimated for different groups and different datasets (imports versus exports) are actually measuring different things. With this in mind, we turn to the estimates.

3.4.1. Probability of exporting a particular product to a particular market

As discussed, there are many econometric approaches to dealing zero-one dependent variables. We start by discussion the results for the standard logit and probit probability models, estimating the models using product-partner and time fixed effects.

Import databases for Eurozone nations

The first set of results are for the Eurozoners using the import databases, i.e. panels that have the listed nation's imports of various product-partner combinations as the columns and years as the rows (the dependent variable is digital). The results are shown in Table 24 (only the coefficients on the EZ dummies are show for convenience; see the appendix from examples of the full regressions).

Table 24: Euro effect on probability of exporting, Eurozone nations, import databases

	Logit			Probit		
	EZ11 vs EZ01	s.e.	p-value	EZ11 vs EZ01	s.e.	p-value
Finland	3.1%	0.00	0.00	3.0%	0.00	0.00
Ireland	3.6%	0.00	0.00	3.8%	0.00	0.00
Austria	1.9%	0.00	0.00	1.9%	0.00	0.00
Greece	2.7%	0.00	0.00	2.8%	0.00	0.00
Portugal	3.0%	0.00	0.00	3.0%	0.00	0.00
Belgium	1.2%	0.00	0.00	1.1%	0.00	0.00
Netherland	1.9%	0.00	0.00	1.7%	0.00	0.00
Spain	3.2%	0.00	0.00	3.0%	0.00	0.00
Italy	1.9%	0.00	0.00	2.0%	0.00	0.00
France	1.9%	0.00	0.00	1.8%	0.00	0.00
Germany	0.8%	0.00	0.00	0.8%	0.00	0.00

Notes:

What the Table 24 results show is that euro adoption has had a positive impact on the likelihood of importing a random product from another Eurozoner by about 2 or 3 percent. Note that the impact is generally larger for smaller nations. This might be expected since the large nations already export a much wider range of goods. Or, to put it differently, the aggregation bias that comes from using HS6 data instead of firm-level data should be bigger for nations with fewer zeros to start with; that is, even if euro-usage stimulates individual firms in the same way in big and small nations, in big nations many of the newly exported varieties will be in categories where other firms are already exporting. Such newly-traded goods will not show up at the HS6 level since we only look at flows that switch from zero to positive. For small nations, which has fewer positive trade flows to begin with, the same amount of firm switching will lead to a large number of zero to positive switches at the HS6 level of aggregation. The results for Logit and Probit are nearly identical.

Export databases for Eurozone nations

Consider next the results of the same regressions run on the Eurozoners' export databases, i.e. where the listed nation's imports of various product-partner combinations are the columns and the years 1995-2006 are the rows. Table 25 shows the results.

Table 25: Euro effect on probability of exporting, Eurozone nations, export data bases

	Logit			Probit		
	EZ11 vs EZ10	s.e.	p-value	EZ11 vs EZ10	s.e.	p-value
Finland	2.8%	0.00	0.00	2.8%	0.00	0.00
Ireland	0.1%	0.00	0.69	0.1%	0.00	0.74
Austria	3.1%	0.00	0.00	3.2%	0.00	0.00
Greece	2.1%	0.00	0.00	2.3%	0.00	0.00
Portugal	1.8%	0.00	0.00	1.9%	0.00	0.00
Belgium	2.7%	0.00	0.00	2.7%	0.00	0.00
Netherland	1.0%	0.00	0.00	1.0%	0.00	0.00
Spain	2.6%	0.00	0.00	2.6%	0.00	0.00
Italy	1.7%	0.00	0.00	1.7%	0.00	0.00
France	0.9%	0.00	0.00	0.9%	0.00	0.00
Germany	0.6%	0.00	0.01	0.5%	0.00	0.01

:

Again we see strong support for the hypothesis that the euro boosted the likelihood that any particular HS6 category would be positive. All the EZ effects are positive and significant apart from Ireland's. The control group in this set of regressions is the exports of Eurozoners to non-Eurozones, i.e. the EZ10 effect. As anticipated in the discussion above, these numbers are generally higher than the numbers in Table 24, although the differences are fairly small. The main conclusion is that common euro usage boost the probability by 2 to 3 percentage points. Note that again we see some evidence that the aggregation bias (i.e. the underestimate of the impact) is greater for large nations.

Results for non-Eurozone nations

We can estimate the impact of the euro on non-Eurozoners' probability using the import and export databases. The results, shown in Table 26, suggest that euro usage has also stimulated the extensive margin between the Eurozone and outsiders; the marginal impacts are all positive and significant at any reasonable level of confidence. (In the interest of brevity, we omit the Probit results as with the Eurozone nations, the Logit and Probit results are qualitatively identical.) Averaging across the 11 Eurozones and the 9 non-Eurozoners, we see that the average impact is slightly high for Eurozone nations.

Table 26: Euro effect on probability of exporting, non-Eurozone nations,

	Logit (import database)			Logit (export database)		
	EZ11 vs EZ10	s.e.	p-value	EZ11 vs EZ10	s.e.	p-value
UK	0.01	0.00	0.00	0.02	0.00	0.00
Sweden	0.03	0.00	0.00	0.02	0.00	0.00
Denmark	0.02	0.00	0.00	0.01	0.00	0.00
Switzerland	0.02	0.00	0.00	0.02	0.00	0.00
Norway	0.01	0.00	0.00	0.01	0.00	0.00
Iceland	0.02	0.00	0.00	0.00	0.00	0.00
USA	0.03	0.00	0.00	0.00	0.00	0.16
Japan	0.02	0.00	0.00	0.01	0.00	0.00
Canada	0.04	0.00	0.00	0.03	0.00	0.00

4. CONCLUDING REMARKS

This chapter reviews evidence on the newly-trade-goods hypothesis from highly disaggregated trade data. The merit of this data is that it is available for all Eurozone nations. The drawback is that it is not at the firm-level, and thus is not fully appropriate for testing the hypothesis directly. Specifically, we can only detect a newly-exported good when the initial trade flow was zero, i.e. when previously no firms in the given origin nation had found it worth exporting a product in the HS6 category to the given destination nation. If the Melitz model and the newly-traded-goods hypotheses are correct, there should be many undetected newly-traded goods. In this sense, the estimates in this chapter are likely to be underestimates of the euro's effect on the extensive margin at the firm level.

The basic conclusion from the estimates was that the euro increased the chances of a particular HS6 product being exported to a particular nation by about two or three percentage points. This is very much consistent with the conclusions of the firm-level results discussed in Chapter 4.

Chapter 6: The impact of the euro on foreign direct investment and cross-border mergers and acquisitions

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1. INTRODUCTION

In theory, the European monetary and currency union should have facilitated the movement of capital by boosting financial integration in the euro area. The heightened integration comes via a number of direct and indirect channels: the elimination of exchange rate risk, the sharing of common trading platforms such as the cross-border merger of the Amsterdam, Brussels; Lisbon and Paris exchanges (Euronext), the development of the Eurobond market, and a reduction of the cost of capital that come with bigger and better functioning financial markets. Financial integration helps reduce the impact of borders on investment decisions, thereby facilitating efficient cross-border allocation of capital.

But in practice, has the euro fostered capital reallocation through FDI and M&As across their member states? Has the euro increased euro area members' capacity to attract capital from the rest of the world?

These are crucial questions for potential entrants who would like to assess the benefits of joining the euro area.

The reminder of the paper is organised as follows. Section 2 discusses the data issues related to FDI and the difficulties in measuring FDI. Section 3 presents key stylised facts and the unconditional evidence related to the impact of the euro. Section 4 summarises existing empirical literature on the conditional impact of the euro. Section 6 presents our concluding remarks.

2. THE EURO AND FDI

The FDI effects of the euro's adoption have been subject to much less investigation than have the trade effects. This is not due to a lack of interest; in the world of modern business, cross-border investment is an integral part of firms' international strategies – especially when it comes to large firms. Moreover, it is widely thought that FDI brings with it valuable foreign know-how that does

[31] The views expressed in this paper are solely the responsibility of the authors and should not be interpreted as reflecting the views of the ECB.

come with just trade. The problem, as we shall see, is that both the data and the empirical methodology are much less well developed.

2.1. Caveat Emptor: Difficulties with defining and measuring FDI

FDI is quite different from trade. Trade is the international sale of a good. With few exceptions, any policy reform that increases trade will also increase efficiency since it tends to equalise marginal rates of transformation internationally.

FDI involves quite different considerations. In the trade model, we ask why would a foreign good be able to compete in the local market with locally produced goods. When it comes to FDI, the question is why would a firm located in one nation find it profitable to own an asset (factory, company, etc.) in another nation? More precisely, why would the asset be worth more to the foreign company than some local company? There are two aspects of this, conveniently summarised in the phrase 'multinational corporation' (MNCs). The first is why should assets be located in multiple nations. The answer to such question is the same as in trade theory: technological and factor endowment differences or scale economies and imperfect competition. The second is why these assets should be owned by the same firm. This led Hymer (1960) to formulate the 'advantages approach' to FDI that has guided almost all subsequent theory. The idea is that to make FDI worthwhile, the foreign owner must have some advantage over local firms. To explain the existence of MNCs, management science formulated the 'eclectic' approach of Dunning (1977)[32] while trade theorists brought MNCs into the 'new trade theory' using the economics of industrial organisation and general equilibrium trade models (Markusen 1984, Helpman 1984 and developed by, inter alia, Markusen and Venables, 1998, and De Santis and Stähler, 2004).

It is clear that FDI and MNC activity turns on economic logic that leaves no direct trace in the data. FDI is about corporate control, but control cannot be defined directly and indeed takes many forms. Moreover, the mother company's advantages will typically involve intangible assets such as idiosyncratic technology, know-how or organisational networks. For this reason, FDI is a much less well defined concept compared to trade – a fact that is manifested in the myriad of ways it is measured.

2.1.1. Measuring FDI

There are two main approaches to measuring FDI – that of central bankers and that of economics ministers. Central bankers consider FDI as part of the capital account of balance of payments. They gather statistics accordingly and study the links between FDI activity, stock market developments and financial deepening. Economics ministries, by contrast, tend to focus on the impact of inward FDI on job creation and assess the potential spillovers such as knowledge transfers and access to international sales networks or management expertise. They gather data on the number of employees, sales and assets of foreign controlled firms. Outward FDI is about allowing domestic firms to better exploit their advantages in the wider world; again, the key statistics are all related to production and sales rather than investment flows.

For the purposes of this study, it would seem that the economic ministries' definition is more appropriate since it lines up more cleanly with the idea that more FDI is better. Unfortunately, the production/sales data is based on surveys and thus generally subject to confidentiality requirements

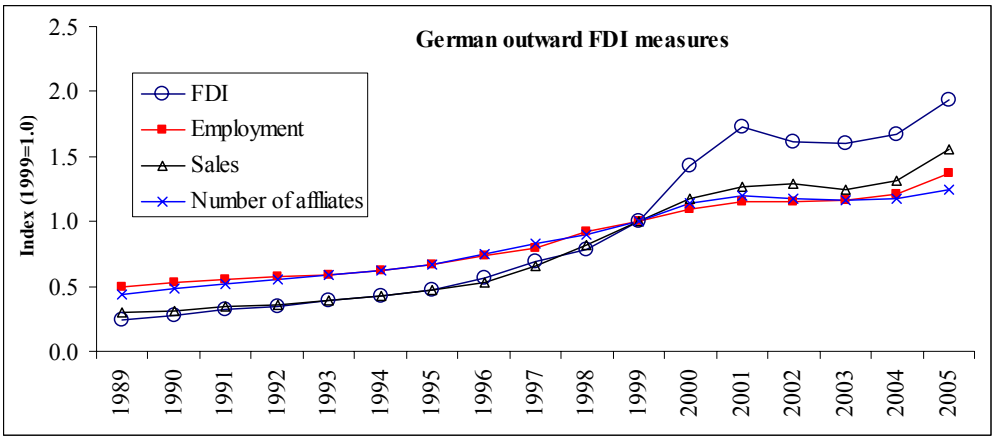
[32] Dunning (1977) identifies three conditions that must be satisfied for there to be a strong incentive for a firm to engage in FDI. First, a firm must have an *Ownership advantage* for a product or production process to which other firms do not have access (i.e., patent, blueprint, or trade secret). Second, the foreign country must offer a *Location advantage* such that goods can be produced or supplied more cheaply. Third, the multinational firm must have an *Internalisation advantage*, i.e. a strategic reason to exploit its ownership advantage internally rather than licensing or selling it to a foreign firm.

that make the data difficult to access for scholars. And when it is accessible, it is usually for just one nation since the various datasets are not compatible enough to pool the data.

By contrast, the capital account data is readily accessible even if it is only indirectly related to real economic activity. An FDI flow is said to occur when a firm from one nation establishes or takes control of a firm in another nation. This often requires some capital to cross the border, but not always; the subsidiary of a multinational can borrow locally or from another country to finance new investments or acquisitions and these activities may be difficult to trace. Even when capital that does cross the border is mapped out, the economic impact can be quite detached from the size of the flow. In some cases, the FDI involves setting up a wholly-owned affiliate fully financed by funds from the mother company. In others, it involves a controlling equity stake of just 10%. Moreover, FDI can involve assets that have very different economic impacts. FDI may entail building new factories or offices (so-called greenfield investments) or entail the merger with or acquisition of existing firms (M&A). In turn, FDI activity can take the form of equity capital, inter-company loans and reinvested earnings.

Such capital flows are subject to the usual financial market devilments – expectations, tax and regulatory treatment, and capital controls – to say nothing of various tricky financial innovations that can allow a particular real investment (say the building of a new factory) to be paid for either well before or well after the actual construction.

Figure 26: Various measures of German outward FDI from firm-level data



Source: Buch and Lipponer (2008)

These are not academic points – they dominate the data. We can illustrate this in two ways. The first is to look at the two measures in the German data from the firm-level database MiDi (Micro database Direct Investment, formerly 'International Capital Links') provided by the Deutsche Bundesbank (see Lipponer 2006 for details). In this case, the FDI measure is the accounting value of FDI from the firm's books. The measures are shown in Figure 26. What we see is that the 'real' measures of multilateral activity – employment, sales and number of affiliates – track together. The book value of FDI, however, departs in a serious way exactly around the time of the euro's introduction. The reason is that the value of FDI is subject to financial considerations. For example, the mother companies can change their stake in their affiliates in anticipation capital market gains. Or, it may be that the accounts are adjusting the book value for stock market prices. In any case, it is clear that the determinants of the 'real' measures are quite difference, especially given the stock market boom and bust cycle in the late 1990s and early new century.

The second way is to look at the Central Bank data and compare it with our priors.

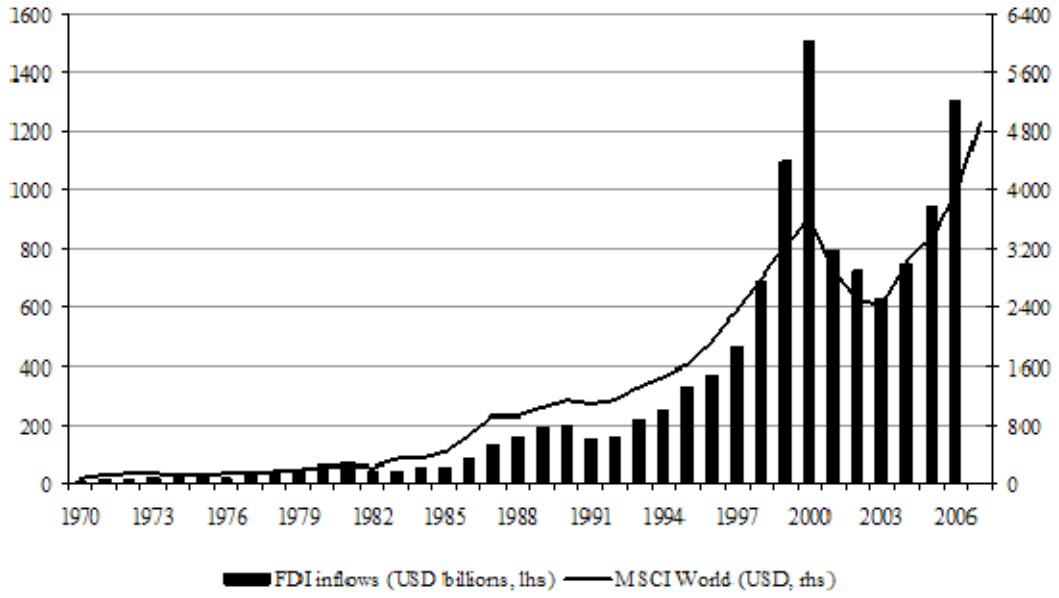
In the real world, everyone knows that France and Germany are the 'star players' in the Eurozone FDI game – both as source nations and as host nations. In the data however, Luxemburg dominates the terrain; almost 70% of all intra-Eurozone FDI flows in the 1999-2003 period were from or to

Luxembourg (Taylor 2007, Table 3) – plainly some sort of financial 'sleight of hand' is driving this number. Moreover, inward FDI follows a clear correlation with local stock market fluctuations (De Santis et al 2004) in ways that suggests a significant disconnect with the production and employment impact of FDI. Figure 27 and Figure 28 illustrate the point.

Figure 27 shows that for the world as a whole, FDI flows track world stock markets; when stock markets are booming, corporations do lots of FDI. Figure 28 shows the same correlation holds for Europe. This stylised fact led De Santis et al (2004) to propose an investment-based approach to FDI (à la Tobin, 1969).[33]

An additional confounding factor comes from exchange rates. Empirical work dating back to the collapse of the dollar in the 1980s has shown that large exchange rate movements have a big effect on bilateral FDI flows (the so-called fire-sale effect of Graham and Krugman 1989 which was formalised by Froot and Stein 1991).

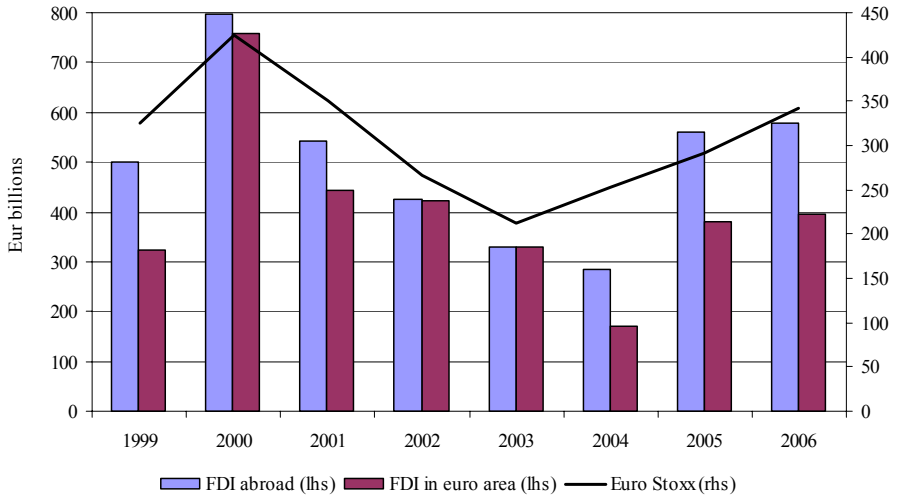
Figure 27: World FDI inflows (left scale) and the Stock Market (right scale), 1970-2007



Source: DataStream, UNCTAD and World Bank.

[33] De Santis, et al. (2004) argue that the q-theory of investment can also translate in higher FDI outflows and find that a rise in the euro area stock market (a proxy for euro area Tobin's q) led to an increase in euro area outward FDI to the United States over the period 1980 to 2001. Similarly, De Santis and Ehling (2007) – looking at the interlinkages between FDI and foreign portfolio investment among Germany, the other G7 economies and Switzerland over the quarterly period 1980-2006 – find that German FDI outflows and inflows are both function of Tobin's q.

Figure 28: Euro area FDI flows (left scale billion euro) and Euro Stoxx index (right scale) 1999-2006



Source: ECB calculations, DataStream.

As if all this were not enough to muddy the information content of the central bankers' data on FDI flows, a great deal of the capital account data on FDI – especially during the euro era – is dominated by mega-deals. Taylor (2007) defines these as mergers and acquisitions in excess of $10 billion (a threshold chosen since no deals approaching this size took place before 1997). Mega-deals account for almost 25% of all cross-border deals since 1996 and were concentrated in the 1998–2001 period. The largest 43 deals accounted for a third of M&A flows in the data.

Taylor (2008) lays out the facts on the largest deal in a way that makes one very aware of the fact that much of the data is driven by idiosyncratic factors on which the econometrician cannot get systematic data. The biggest deal was the acquisition by Vodaphone of Mannesmann in 2000 ($203 billion). However since Mannesmann bought the UK company Orange in 1999, Vodaphone was forced by the European Commission to divest Orange and this was sold to France Telecom in 2000. With the procedes, Vodaphone then bought the Spanish company Airtel and Japan Telecom. Such chains of acquisitions and disposals are not rare but they are not frequent enough to be viewed as white noise, especially in a short time period.

3. UNCONDITIONAL EVIDENCE

Econometric investigations of FDI flows are necessary to control for factors other than the euro. It is nevertheless enlightening to search the data for patterns that jump right out and suggest that the euro has had a first order impact. The obvious question is whether FDI inflows into one Eurozone nation from another exceeded the inflows from non-Eurozone nations. If the answer is 'yes' we would have prima facia evidence that the common euro usage stimulated bilateral FDI flows.

3.1. Unconditional evidence from Taylor (2008)

This simple difference-in-differences approach is illustrated in Table 27. We start with Taylor's full dataset that includes all OECD nations. In particular, in included flows to and from Luxembourg (see above for the problems). The first row holds hope for the pro-FDI idea since the FDI from the Eurozone to itself rose by 277% while the flow from the Eurozone to other nations by only 215%. This however does not control for the possibility that something other than euro stimulated inward FDI in the Eurozone. To check this we can look at the inward flows (first column of the first matrix)

into the Eurozone. Here we see that flows from outside the euro-using nations were much larger than those from inside, 400% versus 277%. This finding muddies the water a bit. If it were common euro usage that was driving the extra FDI, we should have seen most of the FDI boom coming from euro area members, not outsiders as it the actual case. Nevertheless, the basic result that the euro area seems to have become extraordinarily attractive as an FDI destination is striking and gives us confidence that there is a link between the euro and FDI.

To eliminate potential problems arising from Luxembourg's unusual standing as a tax and transparency haven, the next two matrices in the table show the data for the in's and out's where the out's are only the Big-3 FDI generators – the UK, US and Japan. This narrowing is necessary since many other OECD nations do not report the data necessary to strip out Luxembourg. The second matrix performs the same dif-in-dif exercise for the new data on the out's (same data on the in's). The results are qualitatively identical. The third matrix, uses only non-Luxembourg data for the in's and the Big-3 for the out's.

Table 27: Growth in FDI flows 1994-98 vs 1999-2003 for Eurozone in's and out's.

With Luxembourg and 'out' comprising all OECD nations	to:	
	Eurozone	Others
Eurozone	277%	215%
Others	400%	
With Luxembourg and 'out' comprising the big 3 (UK, US, Japan)		
Eurozone	277%	276%
Others	523%	
Without Luxembourg and 'out' comprising the big 3 (UK, US, Japan)		
Eurozone	82%	186%
Others	225%	

Source: Manipulation of data in Taylor (2008).

The finding is weight against the pro-FDI hypothesis. As the top row of the third matrix shows, the flows within the restricted Eurozone rose 82% while those from the Eurozone to the Big-3 rose by 186%. In other words, it seems that the Eurozone – abstracting from Luxembourg – was much less attractive of a destination for FDI-generators located inside the Eurozone than are the UK, US and Japan. The finding that flows from the out's into the Eurozone rose by more than the flows from inside the Eurozone continues to hold (255% versus 82%). Of course, we do not know what would have happened if the 'Luxembourg door' had not been open. It might have been that all the FDI would have gone directly to the large euro area nations; we just cannot know.

These results are very approximate. They do not control for many factors that are clearly important determinants of bilateral FDI flows such as economic growth, which – over the sample period – has been particularly strong in the US and in UK. Nevertheless, the results suggests that we must be cautious in interpreting our econometric results since it seems that something made the Eurozone an extraordinarily attractive place to invest via FDI. While the euro may be part of this story, it does not seem that common euro usage is a necessary condition. Moreover, the results may be very sensitive to the treatment of Luxembourg.

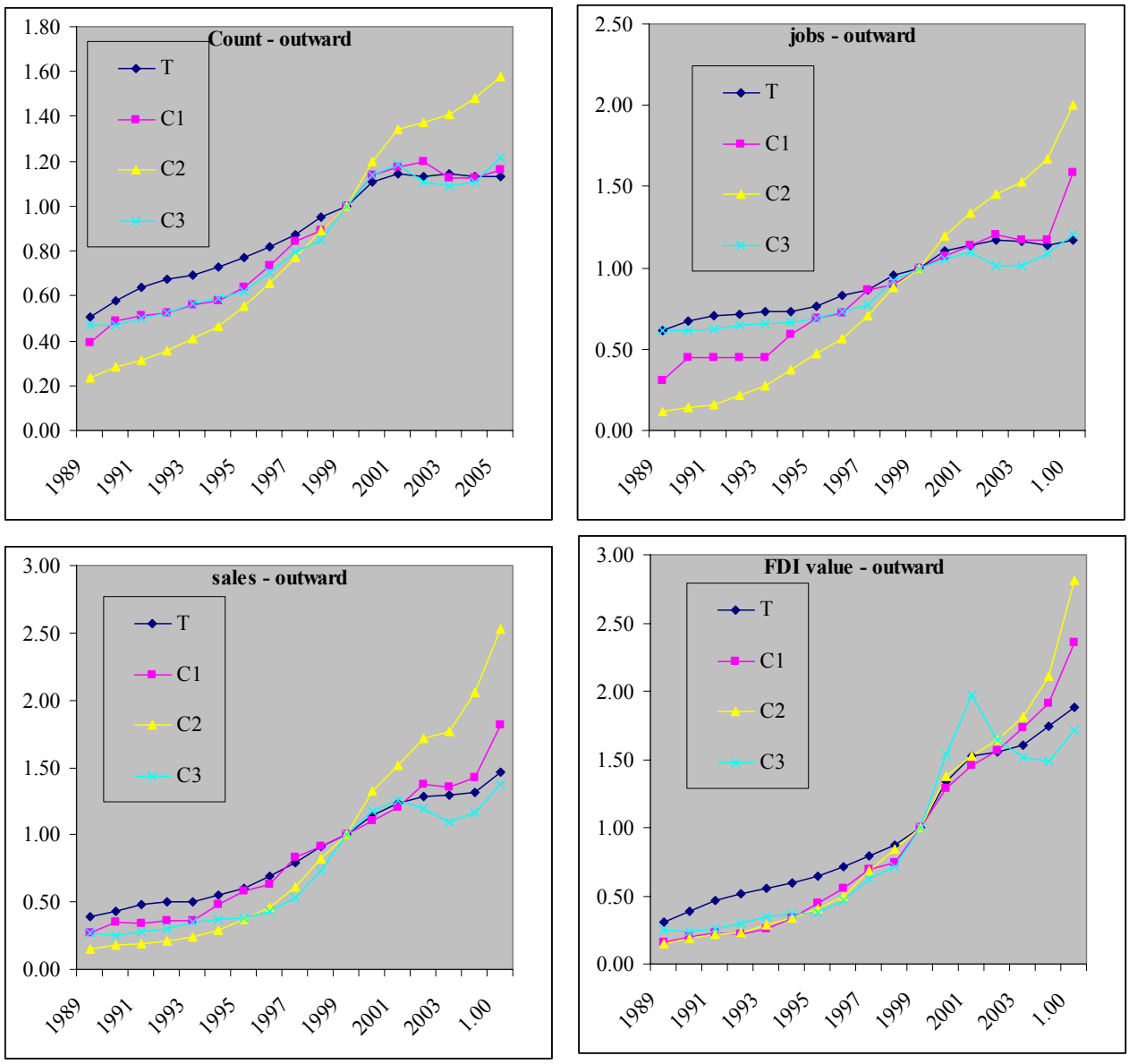
3.2. Unconditional evidence from Buch and Lipponer (2008)

As Taylor (2008) abundant demonstrates, there are many shortcomings with the capital account FDI data. Buch and Lipponer (2008) have provided us with preliminary data from their forthcoming study on FDI and the euro. These data are similar in spirit to the dif-in-dif tables above, but the FDI-generator is Germany alone and, more importantly, the data are much more reliable as they are

based on a mandatory firm-level survey of all German organised by the Bundesbank (MiDi database).

As far as the euro's impact on German outward FDI is concerned, the simplest pro-FDI hypothesis would be that German outward-FDI generators should have been especially attracted by Eurozone destinations (which received the 'treatment' of adopting the euro) compared to other control groups. We consider three control groups that are progressive less likely to share common shocks. The first, C1, is the EU15 members who experienced all the Single Market integration but did not adopt the euro, namely Britain, Sweden and Denmark. The second includes other European nations, viz. Switzerland, Norway and the new Member States. The third includes all other nations in the world. Greece is left out of all calculations due to data problems.

Figure 29: German outward FDI by various measures to the treatment and control groups (index, 1999 =1.0).



Source: Authors' calculations on Buch and Lipponer data
Notes: T is the 'treatment' group, i.e. the original Eurozone members; C1 is Britain, Sweden and Denmark; C2 is Switzerland, Norway and the new Member States; C3 is all other nations in the world.

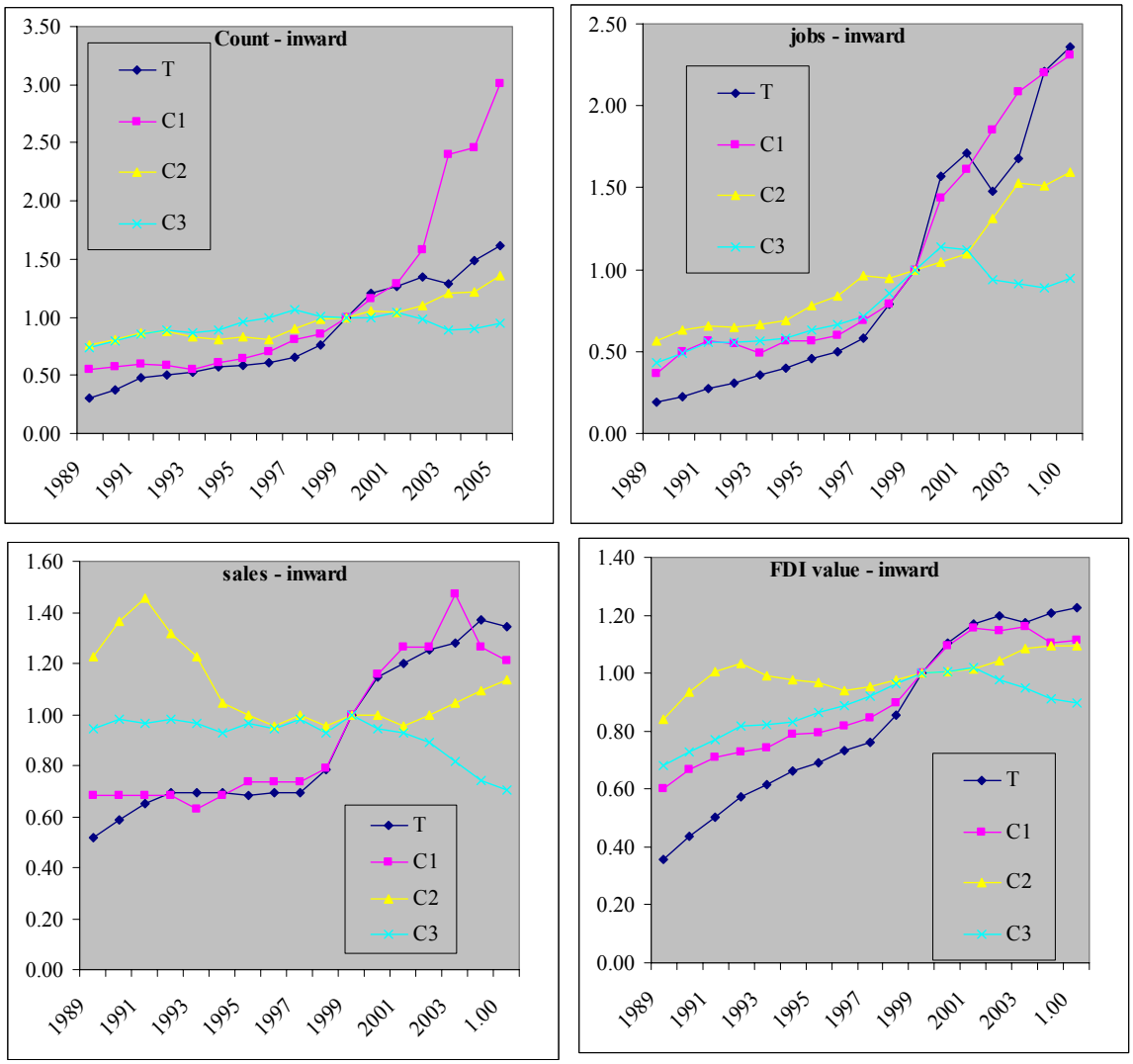
3.2.1. German Outward FDI

The database provides four distinct measures of FDI: data on the number of affiliates (count), number of employments in the affiliates (jobs), sales of the affiliates (sales) and book value of the investment (value). The data, displayed in Figure 29, shows no evidence in support of the simplest pro-FDI hypothesis. Using the three most reliable measures of real FDI activity, we see that the treatment group (Eurozone) and tightest control group (C1) perform more or less the same after the euro's introduction in 1999. If anything, German FDI to the control group is higher. The value of FDI measures clearly suggests that German firms have been focusing outside of the Eurozone since 1999, although this seems to be part of a trend. Note that the extraordinary re-orientation of German FDI to Central Europe shows up in the superior performance of the C2 group.

3.2.2. German Inward FDI

Under the simplest hypothesis that a common currency boosts FDI flows, there should be little difference between the inward and outward flows – presuming that the euro was the dominate factor during this period. We check this using four similar charts done for FDI going into Germany. The results are in Figure 30.

Figure 30: German inward FDI by various measures to the treatment and control groups (index, 1999 =1.0).



Source: Authors' calculations on Buch and Lipponer data

Notes: T is the 'treatment' group, i.e. the original Eurozone members; C1 is Britain, Sweden and Denmark; C2 is Switzerland, Norway and the new Member States; C3 is all other nations in the world.

The facts for German inward FDI are strikingly different. Germany seems to be especially attractive to EU15 members (the sum of the treatment, T, and C1 control groups) compared to the broader control groups C2 and C3. When it comes to differences between the treatment and tightest control group (C1), the four measures provide conflicting stories. The count data suggest that the non-euro EU15 nations have been keen to invest in Germany since the euro and much more than Eurozone nations; if the simple pro-FDI hypothesis were correct, we would think that more affiliates would have come from nations that shared Germany's use of the euro. The jobs and sales measure provide more mixed evidence on the hypothesis, while the value measure shows that FDI from euro-using nations rose by more than the non-euro EU15 nations.

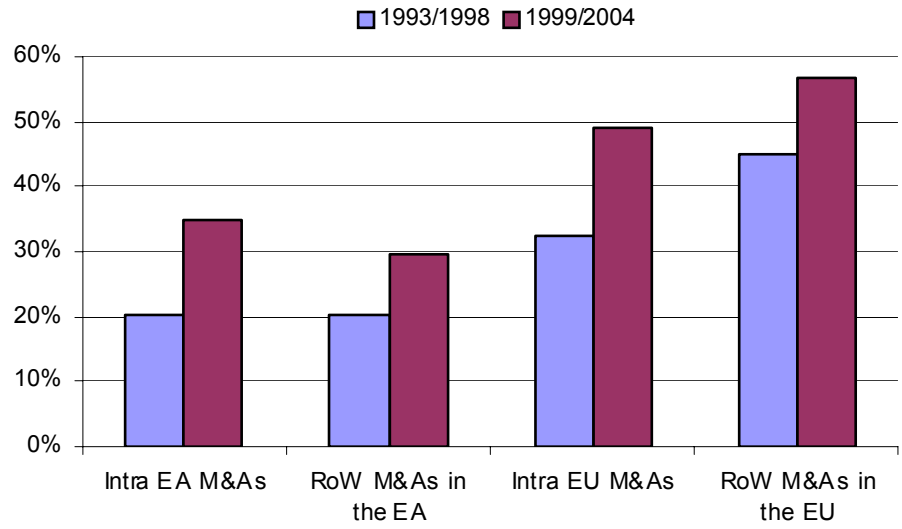
These facts invite all sorts of conjecture on what is going on, and – more generally – they highlight the problems of dealing with a single nation's data; one simply never knows whether the results are driven by idiosyncratic national features or are indicative of broader trends. Germany, after all, is not a typical euro area member. It is by far the largest nations and its manufacturing sector is highly competitive. Moreover, during this period, German firms engaged in a good deal of 'unbundling' of the manufacturing process with important investments in Central European nations.

3.3. Unconditional evidence from Coeurdacier, De Santis and Aviat (2008)

FDI activities take the form of greenfield investment as well as M&As where the transfer of financial claims from the acquiring firm brings along that of the underlying assets of the targeted firm with the main activity of the acquirer and target firms being registered in two different countries.

It is useful to distinguish between M&As of the manufacturing sector from M&As of the service sector, as they show different developments.

Figure 31: Intra euro area (EA) cross-border manufacturing M&As compared, 1993-2004.



Notes: The data includes only cross-border M&A in manufacturing; EU refers to EU15; the figures reflect growth over the indicated periods.

When it comes to intra-euro area cross-border manufacturing M&As as a share of total cross-border M&As (Figure 31), we see that the share has been much more important over the six year period after the euro's introduction in 1999 than it was during the six year previous years (see Figure 7, top panel). A key piece of evidence is that this difference in the before-and-after growth within the euro area is much larger than the difference in the before-and-after growth within the EU15 as a whole (73% versus 50%). This difference in differences tells us that there has been a higher degree of intra-activity within the euro area and among the EU15 nations not participating in the initiative. It

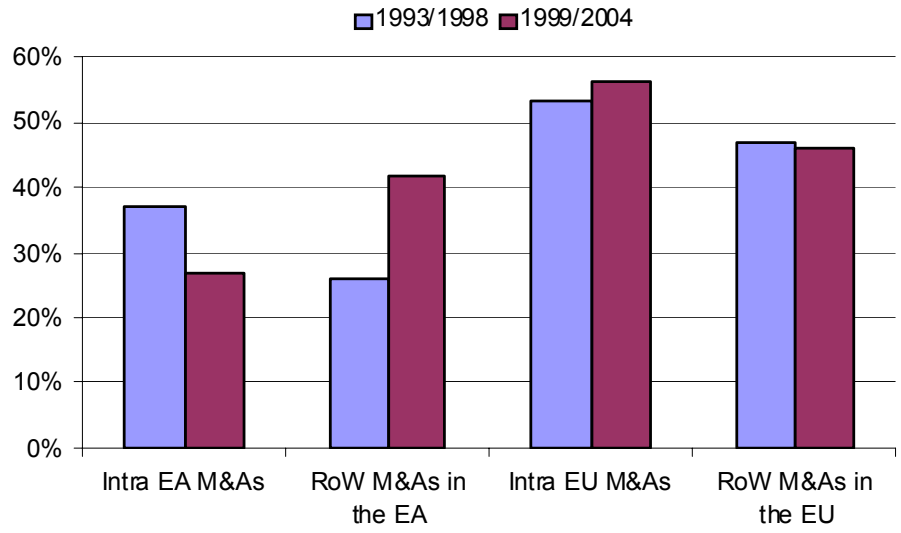
is also remarkable the share of M&A from the rest of the world relative to the total activity of the rest of the world manufacturing sector. It increased by 46% vis-à-vis the euro area and by 25% vis-à-vis the EU over the two considered sample periods before and after 1999.

This finding is very encouraging for the euro-boosts-real-FDI hypothesis. After all, when one thinks of the positive effects of FDI, it is the manufacturing sector that provides the best stories of spillovers and technological transfers that enhance worker productivity and create jobs upstream and downstream from the newly acquired plant. Of course, this result seems to contradict Taylor (2007). As we shall see, a possible reconciliation has to do with the fact that Taylor's number aggregate manufacturing and services.

A very dissimilar story emerges when looking at cross-border M&A services (Figure 32). Here the growth of M&A activity vis-à-vis the EU15 as a whole has remained almost unchanged across the two periods, whereas the share of intra euro area M&A declined (from 37% over the average period 1993-1998 to 27% over the average period 1999-2004). The shares of outsiders in the euro area growth are much larger for M&As concerning firms in the euro area as opposed to those in the EU15 as a whole.

This finding provides important clues to what was driving Taylor's overall results. It seems that the service sector in the euro area proved especially attractive to outsiders – much more so than to firms located in nations that already use the euro. While one cannot know exactly what is driving this difference between manufacturing and services, the notion that firms based in non-euro nations wanted a foothold inside the euro area financial sector is one plausible story.

Figure 32: Intra euro area (EA) cross-border service-sector M&As compared, 1993-2004



Notes: The data includes only cross-border M&A in services; EU refers to EU15; the figures reflect growth over the indicated periods.

All in all, the M&As data provide evidence of a pro-FDI hypothesis in manufacturing only. Therefore it is very important to disentangle FDI flows involving manufacturing from FDI flows related to services.

4. ECONOMETRIC APPROACHES

There are two main empirical approaches to studying FDI.

4.1. Proximity vs Scale

The first stems from the mainstream FDI theory that is dominated by the so-called knowledge capital model (Markusen and Venables 1998) that is based on a two tradeoffs. First, as concerns North-North FDI – the model most appropriate within Europe – the key trade-off is between the scale economy gains that come from spatially concentrating production and the transport-cost savings that come from dividing up production and spatially dispersing it to be near customer concentrations. That is, firms set up local production to avoid trade costs (trade costs being broadly defined to including any cost of separating the production and the customer).

Under this paradigm, the impact of the euro is quite clear. If the euro lowers transaction costs, it should discourage intra-Eurozone FDI. However, in the sense that the Eurozone becomes more like one big economy, the trade-off suggests that the euro should encourage FDI from non-Eurozone nations into the Eurozone.

The second trade-off in the knowledge capital model concerns productive factors, such as the cost of labour, agglomerations and the like. This so-called vertical FDI strives to place each stage of production in the nation where it is cheapest. For example, putting labour-intensive production segments in nations with cheap labour while keeping the HQ services in nations where skilled labour is relatively abundant. This is relevant to the 'new' Europe where the opening of high-skill-low-wage Central and Eastern nations in the 1990s made vertical FDI both possible and attractive. The euro's impact on this trade off is second order, but if it makes trade easier it should encourage FDI.

The latest advance in this approach allows for heterogeneous firms. The key paper – Helpman, Meltiz and Yeaple (2003) – considers firms that have different productivities and fixed costs of establishing 'beachhead' in various markets. The most competitive firms tend to sell a lot and thus tend to find the transport cost saving aspect of FDI especially attractive. This is how they explain the widely observed phenomenon that FDI is dominated by large firms.

This theoretical framework is associated with a particular approach to empirical work on FDI. See, for example, the 2006 on-line survey by Bruce Blonigen.[34] Carr, Markusen and Maskus (2001) provided the first empirical examination of the knowledge-capital model's hypotheses; they use the 'economics ministry' FDI data for the US, specifically the sales of affiliates. They use numerical simulations of their theoretical model to define empirical specification where affiliate sales in a host country is a function of GDP of the two countries, trade costs of the two countries, FDI costs, and differences in factor endowments between the parent and the host. The authors assert that so-called horizontal FDI (i.e. production of similar products in foreign markets to avoid trade costs) is associated with a positive coefficient on the GDP sum term, a negative coefficient on the GDP difference term, and a positive sign on the host trade cost variable. Vertical FDI (i.e. offshore production of certain parts and components of the mother company's product) suggests that the skill difference variable should be positive.

Using a panel data (US outbound and inbound affiliate sales in many nations from 1986-1994), they find evidence for both the horizontal and vertical motivations for FDI. Blonigen, Davies, and Head (2003) present an important critique of their econometrics which, when corrected, no longer supports the vertical motivations for MNE activity.

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[34]" Foreign Direct Investment Behavior of Multinational Corporations,"
http://www.nber.org/reporter/winter06/blonigen.html#N_2_

4.2. Gravity FDI models

The second approach is more frequently applied to the capital flows data from balance of payments sources. It uses a framework akin to the gravity model for trade. While originally the gravity FDI specification was justified by the fact that it fit pretty well, recent papers have provided some micro foundations. Head and Ries (2007) develop a model of cross-border M&A activity where a mother company has a randomly assigned advantage in controlling the company in host nation, but faces a disadvantage in monitoring technology that gets more severe with geographical distance. The likelihood that firms in a particular source nation will be the winner in a particular host nation varies positively with the size of the home and host nations and negatively with the bilateral distance. Since it is a contest, the host nation's relative geography also matters. Bergstrand and Egger (2007) add internationally mobile capital to the 'knowledge capital' model and find that a "modified gravity" model fits the data better. As with Head-Ries, the Bergstrand-Egger model stress the importance of relative distance; the amount of FDI expected between two nations depends upon the bilateral distance relative to some measure of the host and home nations' distance to alternative FDI sources and destinations.

4.3. Empirical estimates of the euro's FDI effects

The main approach to identify the euro FDI effects is based on the gravity model. A general specification of this model can be expressed as:

$$FDI_{od,t} = \alpha_1 EZ11 + \alpha_2 D_d + \alpha_3 D_t + \alpha_4 D_o + \alpha_5 X_{od,t} + \varepsilon_{od,t}$$

where $FDI_{od,t}$ is the FDI flow from the origin nation-o to the destination nation-d in year t (measured by capital account data), the D's are dummies (fixed effects) for origin and destination nations and time (using the standard notation), and X is the vector of other variables that may affect FDI flows, such as market size in the origin and destination countries, transaction costs, proximity, stock market measures such as Tobin's q in the origin nation, exchange rate misalignment, financial deepening, product market regulations, capital market liberalisation, privatisation of state-owned sectors, etc.

The two main, recent studies using the gravity FDI approach are Petroulas (2007) and Coeurdacier, De Santis and Aviat (2008). Petroulas (2007) uses a panel of unilateral FDI flows (equity and other capital) between 18 developed countries for the years 1992-2001. He finds the euro's effect on aggregate FDI flows within the euro area is 14.6 per cent. The estimated effect on FDI from non-euro to euro area countries corresponds to 7.5 per cent – in other words, he finds that the euro promotes inward FDI to the Eurozone from both members and non-members, with the effect on non-members being about half that of members. As far as EU membership is concerned, Petroulas (2007) finds no effect on FDI flows.

4.3.1. Coeurdacier, De Santis and Aviat (2008)

Coeurdacier, De Santis and Aviat (2008) use a panel of cross-border M&As flows (1985 – 2004) with 21 developed economies as source countries and 31 host economies, of which 20 developed and 11 developing countries, distinguishing the activity related to 10 manufacturing sectors from 10 service sectors.

The main aim of this study is to assess the role of European integration, Tobin's q, corporate taxation and product market regulations has key determinants of cross-border M&As using sectoral data. The part that interests us here is their assessment of the euro's impact. For this purpose, we can on the results of the aggregate regressions that are generally not reported in the study.

They have over 10,000 observations in the manufacturing and service sectors of which more than 2,500 are non-zero. The presence of significant number of zeros in the sample leads to biased estimators in standard OLS and Tobit regressions and therefore they use Poisson Maximum-

Likelihood estimators throughout the analysis (see Santos, Silva and Tenreyro 2006, and Head and Ries 2007).

Table 28: The impact of EMU and EU on cross-border M&As related to manufacturing in % (fixed effects)

	Total	within sectors	across sectors
		Euro impact	
Among euro area countries	200.7***	296.3***	20.1
From non-euro to euro area countries	100.6***	114.5**	86.1**
		EU impact	
Among EU countries	341.1***	329.3*	431.7***
From non-EU to EU countries	198.6**	167.5	313.7***
Source country dummies	yes	yes	yes
Target country dummies	yes	yes	yes
Time dummies	yes	yes	yes
Other gravity controls	yes	yes	yes

Source: Coeurdacier, De Santis and Aviat (2008), Tables 1 and 2.
Note: Estimation using Poisson-QMLE estimators. Country dummies of acquiring countries and target countries, sectoral dummies, and time-dummies are included but not reported. Standard errors in parentheses; statistical significance at the 10%,. 5% and 1% levels denoted by *, ** and ***; estimation with robust standard errors; observations are clustered within country pairs.

Based on the aggregate regressions, Coeurdacier, De Santis and Aviat (2008) find that the euro increased cross-border M&As activity among the euro area of the manufacturing sector by about 200 per cent. The estimated effect on M&As from non-euro to euro area countries corresponds to about 100 per cent. Therefore, the additional effect of joining common euro-usage is about 50 per cent (see Table 2). If annual M&As between Germany and France amounted to EUR 1 before 1999, they increased to EUR 2 after 1999 only due to formation of the euro area, and to EUR 3 due to usage of a common currency. Conversely, the impact of the euro on cross-border M&As of services is nil – a result that is fully in line with the unconditional facts discussed above. It seems that the service sector did not exploit the opportunities offered by the single currency.

Table 28 also shows the results relative to the EU effect. According to the findings of Petroulas (2007), the EU effect on FDI flows is null. However, it should be stressed that his control period for any pre-EU flows is very limited, as the sample period starts only in 1992. Therefore, the estimated EU effect is not reliable. Coeurdacier, De Santis and Aviat (2008), whose sample period starts in 1985, find that EU increased cross-border M&As activity among member states of the manufacturing sector by 340 per cent. The estimated effect on M&As from non-EU countries corresponds to about 200 per cent. Therefore, the additional effect of joining EU is about 50 per cent, which is very similar in magnitude to the additional effect of joining the euro area. The EU effect on services instead is not statistically significant. In summary, as a result of the EU integration process, a country belonging to the euro area saw the M&As activities of its manufacturing sector rising sharply.

To further assess the role of European integration on cross-border M&As, Coeurdacier, De Santis and Aviat (2008) divide the twenty years sectoral observations which account for 74% of cross-border M&As in the world in two main groups:

- M&As occur within the same sector ("within sectors"), if acquirer and target firms belong to the same sector, implicitly capturing horizontal M&As.

- M&As occur across sectors ("across sectors"), if the acquirer firm is targeting a firm whose main activity does not belong to the sector of the acquirer. The target firm can either be classified in another manufacturing sector or in another service sector.

Broadly speaking, this decomposition allows them to disentangle the determinants of M&As driven to allocate efficiently production across the globe from M&As that are intended to build conglomerates (and essentially driven by risk diversification motives or "empire building" motives). In the sample, around two thirds of M&As transactions (in value) have occurred within the same sector.

The impact of the euro is sector specific, as it is statistically significant for cross-border M&As within the same sector, while the coefficient is not different from zero for M&As across sector (Table 29). The impact of the euro on cross-border M&As within sectors is about 300%. The rest of the world instead targeted firms of the euro area horizontally as well as vertically. Overall, the euro has had the effect typical of unilateral financial liberalization and has fostered the re-allocation of capital across firms within the same sector.

Conversely, the service sector did not exploit the opportunity offered by the single currency or the single market. the euro and EU dummies are not statistically significant.

Table 29: The impact of euro and EU on cross-border M&As in manufacturing in % (pair fixed effects)

	Total	within sectors	across sectors
		the euro impact	
Among euro area countries	129.8**	158.1**	68.5
From non-euro to euro area countries	99.6**	97.8*	99.8*
		EU impact	
Among EU countries	148.2	83.1	267.3***
From non-EU to EU countries	334.5*	166.7	835.6***
Bilateral dummies	yes	yes	yes
Other gravity controls	yes	yes	yes

Source: Coeurdacier, De Santis and Aviat (2008).
Notes: Poisson maximum likelihood with bilateral fixed effects; ; statistical significance at the 10%,. 5% and 1% levels denoted by *, ** and ***; estimation with robust standard errors.

One common criticism in the literature on the role of common currencies on trade is that the usual regression does not control for some unobservable characteristics (constant over time) in the bilateral dimension; if such a variable increases both the probability of joining the same currency union and the intensity of transactions between the two countries, the coefficient related to the impact of the common currency would be biased upward (see Glick and Rose 2002, Baldwin and Taglioni 2006).

A standard solution to deal with this problem is to estimate the regression with fixed-effects per country pairs. Estimates are generally unchanged, only the significance has slightly decreased due to the enormous number of estimated bilateral fixed-effects (see Table 29).

To further address such criticisms, Coeurdacier, De Santis and Aviat (2008) also identify the impact of the euro in the time-dimension, by adding a dummy variable which is equal to one over the 1985-2004 period for country pairs inside the euro (in 2004) and run the gravity specification. Such a strategy allows them to identify the impact of the euro across-time by comparing cross-border M&As within the euro countries after the date of the introduction of the euro with cross-border M&As within the euro countries before the introduction of the euro. The results are qualitatively identical to the results of the previous regressions and indeed the additional dummy

(not reported) is not significant while the impact of the euro, now fully estimated in the time dimension, remains similar as in the previous table.

Table 30: The time-dimension impact of the euro on intra-euro area M&As in manufacturing (%)

	Total	within sectors	across sectors
	the euro impact identified off the time dimension alone		
Among euro area countries	171.8**	237.4**	44.2
From non-euro to euro area countries	116.0***	143.5**	82.8**
	the euro impact interacted with time dummies after 1999		
1999	405.8**	439.8**	93.7
2000	48.4	93.5	-12.2
2001	114.5	534.7*	63.1
2002	496.0***	838.4***	44.2
2003	25.9	37.4	-45.6
2004	162.7**	263.3**	-10.8
Source country dummies	yes	yes	yes
Target country dummies	yes	yes	yes
Time dummies	yes	yes	yes
Other gravity controls	yes	yes	yes

Source: Coeurdacier, De Santis and Aviat (2008).
Notes: Statistical significance at the 10%,. 5% and 1% levels denoted by *, ** and ***; estimation with robust standard errors.

Finally, in order to assess whether the euro effects are driven by a single year between 1999-2004, which would cast doubts on the average results, we estimate year-by-year euro dummies starting in 1999 (we checked that the same interaction dummy was no different from to zero, as expected, in 1998). The results show that the increase in cross-border M&As within the euro area is not restricted to a specific year though it has not been constant through time. Most of the effect comes from M&As within the same sector, confirming the previous results with the interacted dummies being significant in 1999, 2001, 2002 and 2004.

All in all, these findings suggest that the authors are not capturing a temporary jump of cross-border M&As within the euro, but rather a more long lasting phenomenon.

4.3.2. Russ (2007)

While the integration of theoretical and empirical work in the study of FDI has long lagged behind that of trade, the gap has recently narrowed with paper like Head and Ries (2007), and Bergstrand and Egger (2007), and Baltagi, Egger, and Pfaffermayr (2005) who use theory to guide their estimation of gravity-like FDI equations.

Another very recent line of work is by Russ (200b7), which views FDI from the new open economy macro perspective where exchange rate movements and studies the impact on FDI for common shocks affect for the demand conditions and exchange rates simultaneously. Using a model with upfront sunk costs, heterogeneous firms, and endogenous exchange rates, Russ (2007b) constructs a model where volatility nominal interest rates drive exchange rate volatility and simultaneously affect entry behaviour of MNCs. A key new distinction that arises from the sunk costs in her model is the distinction between veteran and first-time participant in M&As.

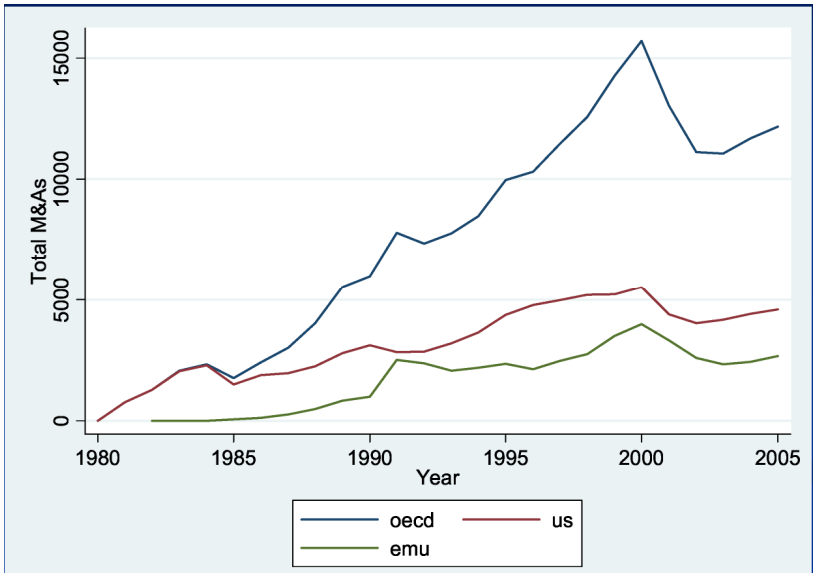
Russ estimates her model using data on M&A activity from Thomsons SDC Platinum database (1980-2005), defining first-time M&A participants as firms that did not engaged in deals between

1980 and 1985, so her database on veteran and first-time OECD-based investors stretches from 1986 to 2005.

Importantly, her data includes domestic M&A activity as well as international. This allows her to do a dif-in-dif regression that gauges the impact of the euro on intra-Eurozone M&A relative to domestic M&A in the same nations. Clearly, the first difference (i.e. cross-border versus domestic M&A activity) alone is not very informative. there are many theoretical channels where the euro's introduction could be responsible for both cross-border and domestic M&A activity in the euro area (e.g. if it deepened the euro area financial markets it could lower the cost of all manner of M&A activity). It is the second difference that is informative, namely the difference between the first difference in nations that were 'treated' with the euro (Eurozone members) and those that were not.

The need for this sort of check is made abundantly clear from Figure 33. This shows that the total M&A activity in the US tracks that of the euro area (marked as EMU in the figure) rather well. This concerns count data, i.e. the number of deals recorded.

Figure 33: OECD-wide boom in cross-border and domestic M&A activity, 1980-2005.



Source: Russ (2007b) presentation slide provided to the authors upon request.

Results

The main thrust of the Russ (2007b) model concerns the impact of interest rate volatility and thus the endogeneity of exchange rate volatility (following Russ 2007a). Fortunately, she includes as one of her controls a Eurozone dummy. Although the coefficients were not reported in the NBER working paper version (since they were not considered parameters of interest), the author provided us with expanded versions of the table the explicitly show the EZ11 dummy coefficients and t-statistics. These are reproduced in Table 31.

Russ (2007b) estimates her M&A model using a variable of control variables that we do not report. The first estimators are simple OLS (first column) and OLS with allowing for geographical clustering of the errors. The last two columns show here Feasible Generalised Least Squares (FGLS) estimators that correct for the serious autocorrelation in the data. The first two rows show the results of the level of M&A activity (she works with count data) for first-time investors and veteran investors. Note that Russ (2007) does not address the issue of the many EU integration schemes and the national privatisation schemes that were implemented during this time period apart from including pair fixed effects.

The results strongly confirm the findings reported above by from Coeurdacier, De Santis and Aviat (2008) and others; common euro usage seems to have a strong pro-M&A effect. Note that Coeurdacier, De Santis and Aviat (2008) use all FDI, rather than just M&A data, but M&A activity accounts for the bulk of the activity during this time (greenfield FDI is fairly limited). The main

difference is the magnitude of the effect. The pro-M&A effect is calculated as one minus the exponent of the coefficient, so the euro effect is around +20% with OLS estimates and +1% with correction for FDI. The figures are quite similar for veteran and first-timers. There are several possible explanations for the discrepancy. First this involves only M&A data and then only count data. As Taylor (2007) showed, much of the action in the FDI value data is driven by mega-deals, so it is not surprising that the count-data results are smaller.

The contribution of Russ (2007b) is most novel in the author's ability to look at the impact of cross-border M&A compared to domestic M&A – a tactic that is not possible when using FDI data gathered for capital account statistical purposes. These results are shown in rows 3 and 4. What we see immediately is that the euro's impact drops in magnitude and becomes insignificant except for the first-time investors with the auto-correlation correction. As mentioned above, this experience is clearly eliminating one important indirect channel – the euro's impact on general finance conditions that would stimulate both domestic and cross-border M&A inside the euro area – but it does allows us to isolate the euro's particular impact on the cross-border M&A. The key take-away point from these results is that the euro seemed to have a big impact on first-timers' cross-border M&A even though it had no particular impact on the cross-border M&A of veteran investors.

Table 31: Common euro usage impact on first-time and veteran M&A participants, 1986-2005

	OLS	OLS Clustered	FGLS	FGLS
		Level of M&A activity		
First-timers				
EZ11	.192***	.192***	.092***	.096***
t-stat	-4.23	-3.22	-4.43	-4.71
Veterans				
EZ11	.179***	.179**	.105***	.101***
t-stat	-3.52	-2.56	-4.12	-3.96
		Cross-border relative to domestic M&A activity		
First-timers				
EZ11	0.093	0.093	0.123***	0.122***
t-stat	-1.64	-1.11	-3.01	-3.00
Veterans				
EZ11	0.015	0.015	-0.012	-0.029
t-stat	-0.26	-0.16	(-0.32)	(-0.80)

Source: Russ (2007b) from expanded tables provided to authors upon request.
Notes: See the original paper for econometric details and the values of other coefficients.

What does this tell us? One of the standard stories concerning the euro's impact on FDI is that it is especially impactful on small and medium firms. Since most of the veteran M&A participants will be very large firms, while the first timers are smaller, the results tend to confirm the fact that the euro is especially important for smaller firms.

The model is estimated and she finds that common usage of the euro has a large and positive impact on first-time investors and investors that have already bought at least one firm in the destination country concerned.

4.3.3. Other papers

Two earlier econometric studies investigate the euro's FDI effect. De Sousa and Lochard (2006) use a gravity specification and panel data on bilateral FDI stock and flows of 21 OECD countries over the period 1982-2004. Schiavo (2007) also uses gravity specification and panel data estimation for bilateral FDI flows of 25 OECD countries over the period 1980-2001. Both studies find a positive

effect of the euro, but since they do not control for the OECD merger boom, it seems likely that their estimates are upward biased.

5. CONCLUDING REMARKS

Subject to the provisos on FDI data, this chapter presents evidence that both EU and euro area membership have had pro-FDI effects. The key points that emerged were:

The euro's pro-FDI effect was much larger in manufacturing versus services. It is likely that the level of protection and barriers to entry in the service sector act as a strong deterrent to cross-border M&As in services across countries. To the extent that the new 2006 services directive breaks down such barriers, it may trigger a new wave of cross border M&As within the EU.

The euro's pro-FDI effect was much larger for deals within sectors as opposed to across sectors. Thus the euro facilitated cross-border M&As within the euro area, which aimed at restructuring capital within the same sector of activity, rather then boosting the formation of conglomerate activities between sectors.

The euro fostered domestic and cross-border M&A activity by both large and small firms, but its effect on small firms was biased towards cross-border activity.

The euro's adoption promoted FDI from outside the euro area, but this effect was only about half as strong as the impact within the area.

The 'bottomline' number – the overall pro-FDI effect – is not clear. Most authors find it is positive, but the estimates range from +15% for in-to-in flows and +7.5% for out-to-in, to +200% and +100%, respectively for the in-to-in and out-to-in flows.

Chapter 7: Discussion of channels and the specific questions

1. TRADE AND FDI CHANNELS

1.1. Trade channels

The best way to organise a discussion of channels is to realise that all trade works through a consumer's or firm's demand function. Demand depends upon the good's relative price and the customer's expenditure on the product category. This tells us that the euro's trade effect must work either via a:

- Change in the relative price of the exported good, or
- Change in customers' expenditure shares on the good.[35]

The level of bilateral trade costs as well as the degree and nature of competition in the importing nation are the main determinants of a product's relative price. The euro's trade effect could thus conceivable work via either of these sub-channels (bilateral trade-cost effects or competition effects).

This reasoning applies to trade of a given product. Aggregate exports, however, consist of the sum of product-level exports, so a third channel comes from:

- Change in the range of products exported.

This third channel is the so-called 'extensive margin', or newly-traded goods hypothesis.[36]

The dominant assumption in the literature up until very recently was that the euro's trade effect was coming uniquely via the relative price channel. This was based on two points. First, people made the reasonable assumption that the euro-usage per se

[35] Of course if the euro had a growth effect it could boost trade by boosting expenditure, but this is not typically considered part of the euro's trade effect and indeed all empirical work controls directly or indirectly for expenditure, so when the literature (e.g. Rose 2000) speaks of the pro-trade effects of a currency union, it is explicitly ruling out effect that might occur via a change in the level of expenditure in the importing nation.

[36] There are a couple of more exotic possibilities, controlling for relative prices and expenditure. Preferences can change, especially for intermediate goods where preferences reflect production technology rather than subjective preferences, and this may raise or lower exports at constant relative prices and expenditure. There is also the possibility that customers' information sets change, i.e. they find out about foreign varieties that they did not know about previously. Normally this sort of change heightens trade.

does not shift the expenditure shares much, so the expenditure-share channel was negligible. Second, the dominate theoretical paradigm – the Helpman and Krugman (1985) synthesis of old and new trade theory – did not allow for changes in the range of traded products; it made assumptions that implied that every good produced was traded. The combination of the two tenets produced the standard, often implicit, assumption is that the euro lowered transaction costs and these savings where at least partly passed on to lower trade prices, stimulating trade in the process.

Since the emergence of the new-new trade theory (e.g. Melitz 2003), the extensive margin channel has gained a great deal of attention. Starting with Baldwin and Taglioni (2004) and Baldwin (2005, 2006a) this "new goods" hypothesis (or to be more precise, "newly-trade goods" hypothesis) suggested that in addition to the relative-price channel, the euro could boost trade by increasing the number of products exported to Eurozone nations. Specifically, Baldwin and Taglioni (2004) showed theoretically that one can account for all the empirical findings if one presumes that the euro's trade effect is operating by stimulating the export of new products rather than simply increasing the volume of already-traded varieties. The idea is simple to explain in the new-new trade theory model of Melitz (2003), also called the Heterogeneous Trade (HFT) model.

In the HFT model, firms' decision to export involves a two-step process. First firms determine what their optimal exports to a given nation would be if they decided to export. Second, they decide whether the resulting profits would be sufficient to cover the cost of establishing a marketing beachhead in the given nation. The model assumes that firms are heterogeneous in terms of their competitiveness. Different firms can thus look at the same two-step problem and come to different conclusions. In equilibrium, big, competitive firms will export; smaller, less competitive firms will not. The threshold level of competitive dividing the two groups is a key variable of adjustment in the HFT model.

Since the threshold is the key to the understanding the euro's impact on the range of exported products, we illustrate the basic issues with a diagram.

Figure 34: Export decision in the HFT model & the newly-traded goods hypothesis

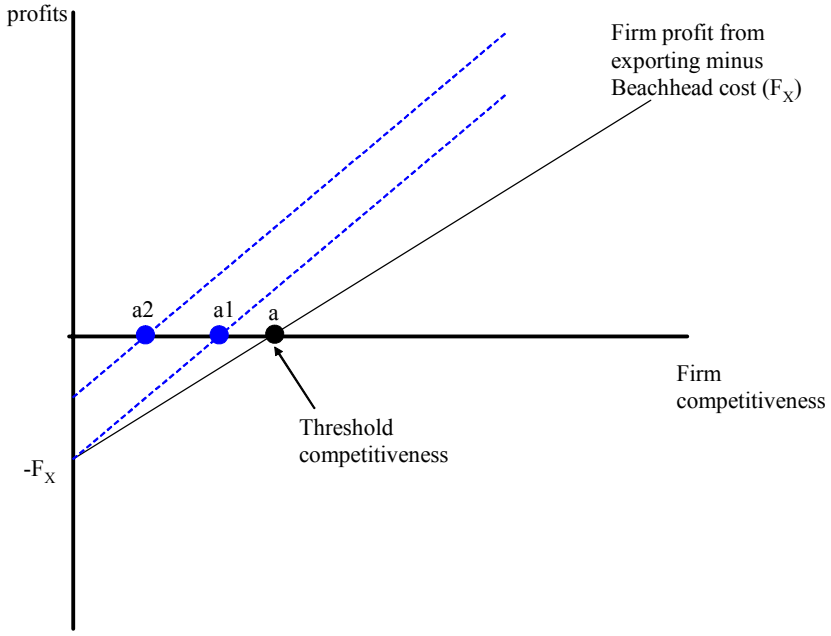


Figure 34 shows the basic logic. We arrange firms in order of increasing competitiveness and assume – for simplicity's sake – that each firm makes a single product. We assume a standard market structure (where more competitive firms sell more and make higher operating profit in any given market) so the firm profit line in the diagram is upward sloped. To account for the beachhead cost (i.e. the fixed cost of entering a particular export market) the firm profit line is shifted downward by the beachhead cost, namely F_X. In the extreme, a firm with zero competitiveness would sell nothing and thus earn nothing if it entered the export market in question. Its profit would be $-F_X$ as shown by the intercept.

Euro's impact. What happens when the euro is adopted by the origin and destination nations? Two things.

- First, to the extent that that the euro lowers *marginal* trade costs, the profit line rotates back towards the origin as shown by the middle dashed line.

The reason is simply that with lower marginal trade cost, each firm is more competitive in the export market than it was before the euro. This means that each firm would sell more and earn more from exporting (except of course, the extreme case of a firm with zero competitiveness, i.e. the intercept does not move). Since the trade costs are marginal, firms that sell the most initially – i.e. the most competitive firms – see their sales and profits rise the most. This is why the profit-line rotates rather than shifts upwards.

- Second, to the extent that the euro lowers beachhead costs – i.e. the fixed market entry costs – the profit line shifts up. The topmost dashed line in the diagram reflects both the marginal and fixed trade-cost effects of the euro.

The fixed trade-cost effect could come from many sources, but the easiest to imagine are those involving the administrative cost of dealing with an extra foreign currency. When the two nations share a currency, the usual finance overhead costs (accounting, hedging, bank accounts, etc.) are reduced. In this simple example we consider only a single export market, but it is trivial and useful to extend it to multiple markets using the euro. Doing so we see that even if the euro did not change the range of products that a particular nation exported, the euro's introduction could expand the list of export destination due to economies of scope in foreign currency costs. For example, a French firm that previously only found it worth exporting to Belgium might find it worthwhile exporting to the Netherlands since the cost of setting up a euro bank account would already have been sunk.

More formally, we can write the aggregate bilateral exports from origin nation-o to destination nation-d as:

$$V_{od} = \int_o^{\bar{i}} v_{od}(i)di\,; \qquad v_{od}(i) = \left(\frac{p_{od}(i)}{P_d}\right)^{1-\sigma} \alpha_d E_d$$

where V_{od} is the aggregate exports, $v_{od}(i)$ and $p_{od}(i)$ are the export value and price of product i, with P_d being the sectoral price index in nation-d. The products are arranged from zero to $\bar{i}$ in order of decreasing competitiveness, so $\bar{i}$ is the threshold product. Also, α is the sectoral expenditure share and E is total expenditure in nation-d. Given this, the change in aggregate exports can be decomposed into our three channels by totally differentiating V_{od} with respect to p, α and E. The result is:

$$dV_{od} = (1-\sigma)V_{od}\frac{d(p_{od}/P_d)}{p_{od}/P_d} + V_{od}\frac{d\alpha_d}{\alpha_d} + v_{od}(\bar{i})d\bar{i}\,; \qquad \frac{d(p_{od}/P_d)}{p_{od}/P_d} = \hat{p}_{od} - \hat{P}_d$$

where we have assumed that all products exported from nation-o to nation-d experience the same proportional change in relative price (this is why we can the term out of the integral).

The first right-hand term is the relative-price channel; it depends on the expenditure elasticity, $1-\sigma$, the initial level of trade, V_{od}, and the proportional change in relative prices. Note that as the last expression shows, the proportional change in relative prices is the proportional change direct impact on p_{od} minus the indirect impact that this price effect will have on the sectoral price index P_d. More precise, we can break down the relative price term into the changes in the average mark-up on sales from Eurozone nation-o to Eurozone destination nation-d:

$$\frac{d(p_{EZ,EZ}/P_{EZ})}{p_{EZ,EZ}/P_{EZ}} = \left(\hat{\mu}_{EZ,EZ} + \hat{\tau}_{EZ,EZ}\right) - s_{EZ}\left(\hat{\mu}_{EZ,EZ} + \hat{\tau}_{EZ,EZ}\right) - (1-s_{EZ})\left(\hat{\mu}_{NEZ,EZ} + \hat{\tau}_{NEZ,EZ}\right)$$

where μ is the mark-up, the ^ indicates proportional change as usual, and we use the standard 'from, to' subscript to indicate the direction of trade. Thus s_{EZ} is expenditure share of imports from the Eurozone in nation-d's price index, and the μ's and τ with

EZ (Eurozone) and NEZ (non-Eurozone) subscripts indicate the relevant variables for Eurozone and non-Eurozone partners. This expression shows that the relative price channel could be operating via a reduction in bilateral trade costs among Eurozone nations, or via a pro-competitive effect of the euro that depressed bilateral mark-ups. Moreover, the expression allows for the fact that euro-usage in the importing nation might raise or lower the bilateral cost of exporting for non-EZ nations. It also allows for a distinct pro-competitive effect for NEZ to EZ exports; for most market structures, however, the equilibrium mark-ups for EZ-based and NEZ-based firms would typically move together. For example, if the euro made competition in the importing market tougher – say due to increased transparency, or easier/safer third-party arbitrage – then we would expect both mark-ups (μ_{EZ} and μ_{NEZ}) to fall.

The second right-hand term is the expenditure channel and it depends on the extent to which euro-usages alters expenditure patterns. Typically, changes in expenditure shares are driven by changes in relative prices, so it normally difficult to separate this channel from the relative price channel. Most authors ignore this channel by assuming that expenditure shares are fixed.

The third right-hand term is the "newly-traded goods" channel. It depends upon the size of exports by firms with threshold level competitiveness and the size of the change in the threshold.

Trade diversion prediction. If the euro lowered the bilateral trade costs and thus export prices charged by firms based in the Eurozone, the price index P_d would fall; all the exporting firm's p's are inside P_d as well as the price of locally-produced substitutes. If the origin nation-o is not inside the euro zone, it would not benefit from lowering marginal trade cost, but it would still see the drop in P_d. To the extent that the relative-price channels is driving by a euro-related reduction in marginal trade costs – as in the classic transaction cost story – firms outside of the Eurozone should experience a drop in their sales to the Eurozone due to a P_d-driven increase in their relative price. This is commonly known as trade diversion.

1.2. FDI channels

Mainstream thinking on FDI classifies FDI into two basic types, horizontal and vertical. These are terms of art. They do not exactly correspond to the terminology in the data. 'Horizontal' here means that the firm doing the FDI is making the exact same good in one factory located in the home market and in another factory located in the foreign market. For example, Johnson & Johnson making the same sort of shampoo for the Southern European markets in Spain and for the Northern European markets in Poland. The basic motive is to reduce trade costs but placing manufacturing close to the consumer. This type of FDI is clearly as substitute for trade. Indeed reducing trade is the only benefit from such FDI in the simplest model (e.g. Markusen 2002).

The term 'vertical' means that the FDI-generating firm is engaged in a process in facility located at home that is different from the process undertaken in the facility located abroad. This FDI – what might be called unbundling FDI – is clearly a complement to trade. Before the FDI, both processes where bundled spatially at home and only the final output was exported. After the production process is spatially unbundled and part of it moved abroad, an intermediate good is exported from one factory to another and then the final good is exported to both markets. In other words,

unbundling FDI stimulates trade in intermediate goods. Note that in the early versions of this model, e.g. Helpman (1984), the intermediate 'good' was actually intangible headquarter services so the FDI stimulated trade in what used to be called invisible (intellectual property rights, administration, management, marketing, accounting and financial services, etc.).

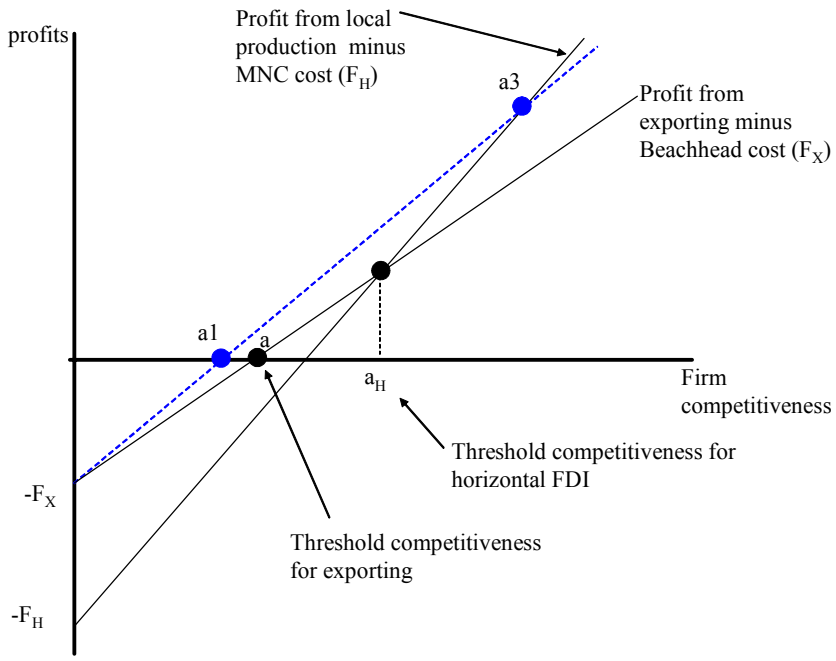
The latest twist on this approach to FDI has been to add firm heterogeneity. This is not an ideal intellectual exercise since one of the most obvious features of FDI is the role of firm size. While there are always exception, FDI is game played primarily by the largest, most productive firms. (Earlier thinking on the matter ignored firm differences for the most part.) In fact, it is quite easy and instructive to marry the trade effects and FDI effects in a framework where firms have the option of supplying the foreign market via exports, as discussed above, or via local production, i.e. horizontal FDI.

1.2.1. FDI and trade as substitutes: horizontal FDI with heterogeneous firms (Helpman, Melitz and Yeaple)

Adding in the extra option of local production requires only a minor modification of

Figure 34. This is done in Figure 35. The export profit line is as before, starting from –FX and rising with firm competitiveness. All firms that are more competitive than the threshold shown will choose to service export. The new element is the second solid curve that starts from $-F_{MNC}$ and rises faster than the export profit line. Here F_H represents the fixed cost of establishing a second factory to enable local production (H is short for horizontal FDI). It rises faster than the export-profit line since local production incurs no bilateral trade costs. (The slope of the FDI-profit line is exactly the slope the export-profit line would have if bilateral trade costs were zero.) The extra option of local production means that some firms with competitiveness above point 'a' will find it even more profitable to establish a local production facility. The switch-over level is indicated as a_H.

Figure 35: Export vs FDI decision in the HFT model



This equilibrium outcome has many useful contact points with reality. It generates a pattern where small firms sell only in their local market, bigger firms export and the biggest of the big supply the foreign market via horizontal FDI and local production. This is the Helpman, Melitz and Yeaple (2003) framework.

Euro's impact. What happens when the euro is adopted by the origin and destination nations? To keep the diagram simple, consider only the impact of a euro-driven reduction in bilateral marginal trade costs. Nothing happens to the FDI profit-line, but the export profit-line rotates counter clockwise as before. Interestingly, we see that this moves both the export threshold down and the horizontal FDI threshold up. In other words, the lower trade costs would produce a pro-trade extensive margin effect as more firms found it worthwhile to export, but it would also have an anti-FDI effect since the balance between saving trade costs and sacrificing manufacturing scale tilts towards exporting. More specifically, the euro would not affect the FDI choice of the most competitive firms that already have establish production facilities, but it would discourage new firms from setting up new plants aimed at jumping over trade costs. In the terminology of Russ (2007b), the euro would be anti-FDI for first-time participants, but less so for veterans.

1.2.2. FDI and trade as complements: vertical FDI with heterogeneous firms (Antras and Helpman)

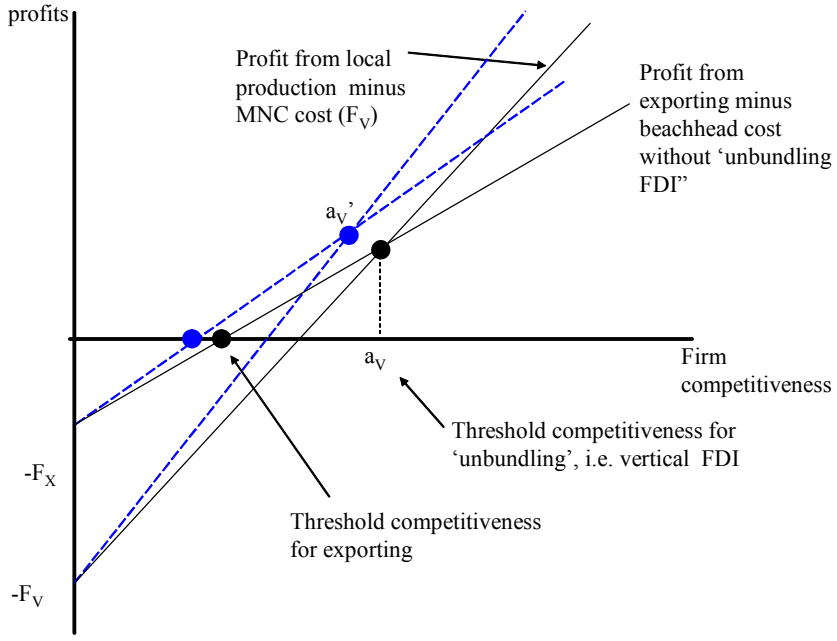
The prediction that trade and FDI should be substitutes is extremely unattractive for the job at hand since virtually every empirical investigation over that past two decades has found that they are complements. Indeed the world pattern of FDI is very close the world pattern of trade – both in terms of sectoral and geographic patterns. More to the point, the evidence in Chapter 6 suggested that the euro was had a positive effect on FDI, especially within the same manufacturing sector and especially by first-timers. Given the evidence in Chapters 2, 4 and 5 on the euro's pro-trade effect, a model that

necessarily predicts a negative correlation between the trade and FDI effects lacks some essential contact points with reality.

As mentioned above the vertical FDI models tend to predict a positive correlation between trade and FDI since the FDI is really just way of getting parts and components made in foreign nations for less than they can be made locally. To the extent that the euro made this offshoring easier, it would stimulate trade in intermediate goods and FDI, while potentially boosting final good exports as well.

Putting these considerations into a diagram is intrinsically more difficult since we deal with a minimum of two types of goods (final and intermediate) and two decisions (the decision to unbundle manufacturing and offshore intermediate good production and the final-good export decision). To deal with this, we'll have to subsume the cost-saving aspects of unbundling FDI (i.e. offshoring) into firms competitiveness in the final good market. Thus the thresholds will concern the competitiveness of firms in the final good market, with the trade implications in intermediate goods pushed behind the scene. Figure 36

Figure 36: Export and unbundling FDI decision in the HFT model



The diagram is quite similar Figure 35 since the basic economics logic is the same – a tradeoff between high fixed costs and low marginal cost (with FDI) and low fixed but higher marginal (with exporting). The difference is in the interpretation of the variables. More importantly, lower trade costs will rotate both curves since both involve trade. Indeed, in the case at hand – where the vertical FDI involves the offshored production of a component that is then re-imported to the home nation to be incorporated in a final good that is then exported – trade cost changes have a bigger impact on the FDI profit line. The reason is simply that this sort of unbundling-FDI involves more trade than the bundled production alternative; in both cases, the final good is exported but with unbundling FDI, there is the additional trade in components.

If the euro lower trade costs, both the export and FDI profit lines rotate counter clockwise for the usual reasons (see above). If, as argued above, the lower trade costs

have a bigger impact on the profitability of offshoring FDI (viz. vertical FDI, or unbundling FDI), the new threshold between exporting when production is bundled (i.e. no FDI) and exporting with unbundled production shift to the left. This means that the euro will raise the level of FDI. Moreover, the impact will be largest on first-timers, i.e. the firms that were competitive enough to export, but not quite large enough to make offshored production profitable. As before, the lower trade costs also boost the range of goods exported (the export threshold shifts left).

2. EVIDENCE ON THE CHANNELS

2.1. Relative price channel

The first bit of evidence on the relative price channel comes from the Pricing-to-Market (PTM) regressions in Chapter 3.What this regressions do is to look at the extent to which firms from various EZ and non-EZ nations were pricing-to-market before and after the euro. Before the euro the theory tells us that the relation between the export prices of origin nation-o (measured in its own currency) and the bilateral exchange rate for destination nation-d should be:

$$\ln p_{odt} = D_{ot} + D_{od} + \beta \ln e_{odt}$$

See Chapter 4 for details, but the basic idea is that the first dummy, D_{ot}, captures fluctuations in nation-o's marginal costs that affect the price of its exports to all nations, the second dummy, D_{od}, captures pair-specific features like bilateral trade costs and the equilibrium price-cost mark-up in market-d, and finally the last term reflects the pure PTM factor. Here the bilateral exchange rate e_{od} is defined as importer currency units per exporter currency unit, so a rise in e_{od} is an appreciation of the exporter's currency. If β is negative, then the exporter is engaging in PTM, since it does not allow the bilateral appreciation to be fully passed through to higher consumer prices in market-d.

When we estimated this over a sample period that includes data before and after the euro, we add a couple of dummies to look at the shift in β. Unfortunately, for exports within the Eurozone we cannot work out the change in β since the bilateral exchange rates are all unity and thus $\ln e_{odt}$ is zero after 1999.

The results in Chapter 4 (Tables 13, 14 and 15) provide direct evidence that the euro's introduction did change trade pricing inside the Eurozone. In particular, the euro dummy was negative and significant for all Eurozone exporters on their sales to the Eurozone with the sole exception of Ireland. Moreover, when we look at Eurozone exports to non-Eurozone nations, we do not find the effect. We have eight nations as destinations outside the Eurozone and we can estimate the change in the level dummy D_{od} (where o is a EZ member and d is not) after 1999, and the change in β. Of the eight possibles, only one of the level dummies drops (indicated a drop in bilateral trade cost or the equilibrium mark-up). Only one of the PTM coefficients (i.e. β) changes. Using our usual difference in difference interpretation, this suggest that it was the Eurozone's usage of the euro that was driving down exporter prices within the area rather than, for example, Single Market integration or a general trend towards tougher competition worldwide.

Unfortunately, wince the bilateral exchange rate disappears inside the euro area, we cannot separately identify the change in the level of the mark-up and the change in bilateral trade costs (i.e. the pro-competitive effect and transaction cost savings) from changes in the degree of PTM (reduction in β).

We do, nevertheless, have indirect evidence that adoption of the euro has lead to more unified pricing in the area. This evidence comes for the change in behaviour of the outsiders' pricing in Eurozone nations. The idea behind this deduction is simple. If the euro has made the Eurozone a more integrated, more competitive market due to, say, greater transparency and cheaper third-party arbitrage, then outsiders should have increased the extent to which the price-to-market in the euro area. Tables 13 and 14 show the relevant results. There are 11 possible destinations in the Eurozone; Danish firms moved toward pricing to market in 6 of these, Sweden in 7 of them and the UK in 10 of them. The US and Canada moved towards PTM in 10 and 11 of the 11 possible markets (respectively). Japanese exporters, by contrast, moved more toward PTM (i.e. the change in β was negative and significant) in only 3 of the 11 cases.

This indirect evidence is a long way from perfect since the different exporters send different bundles of goods to the Eurozone and our estimated changes are an average of the changes in all the markets. Thus is it conceivable that the market structure changes that clearly affected the export pricing behaviour of the Eurozone's major trading partners did not affect pricing within the Eurozone. Given the similarity of the export competition of the UK, Sweden and Denmark to that of the Eurozone, we believe that this possibility should not be taken too seriously. In short, it seems like the euro change trade pricing behaviour of flows inside the Eurozone due to changes in market structure as well as changes in bilateral trade costs.

2.1.1. Lack of trade diversion

While it is not possible to discern the impact of the euro on bilateral trade costs within the euro area (e.g. transaction cost savings), we have indirect evidence that this effect could not have been very large. If the euro stimulated trade by preferentially lowering the transaction cost of nations sharing the euro, we should have witnessed trade diversion at the same time as the trade creation. As Chapter 2 detailed at length, there is no clear evidence that the euro harmed exports from non-euro nations. Indeed some studies show that outsiders' exports to the Eurozone rose.

Note that the finding that there was a drop in the intra-EZ export prices after the euro (controlling for other factors) is perfectly consistent with the lack of a transaction-cost savings. The export price is always marginal cost of selling to the particular market marked-up by an equilibrium mark-up. Thus what seems to have happened was that the euro's introduction lowered the mark-ups and thus lowered the trade prices of all exporters to the Eurozone, not just those nations using the euro. To put it differently, the euro's pro-competitive effect impacted prices from all destinations and so did not necessarily lead to trade diversion.

2.2. The newly-traded goods channel

The presence of a common currency throughout the Eurozone is likely to have made it easier for firms to sell products in the Eurozone. One aspect of this may be the lower variable trade cost effect (e.g. transaction cost savings), but as argued above this effect does not seem to have been important. The euro, however, could reduce the

fixed cost of entering Eurozone markets via economies of scope. For example, instead of having to hold bank accounts in 11 different currencies, arranging hedging for them, making provisions in the company's books, post-1999 an exporter would have only a single currency to deal with. Moreover, if the exporter is located in a euro-using nation, then the forex dimension disappears all together. Plainly currency-linked fixed costs are not the only market-entry costs – and probably not the most important – but it seems plausible that the euro did lower these beachhead costs.

Lower beachhead costs would stimulate trade in products that firms were already producing and selling in their local markets. This would account for several important empirical facts.

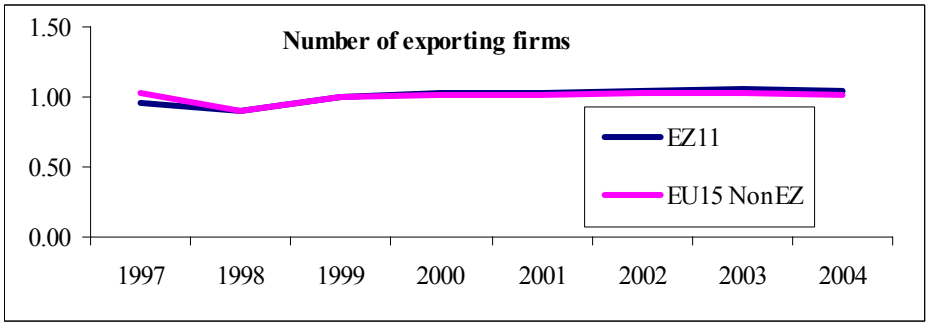
First, the euro's trade effect – as was extensively documented in Chapter 2 – happened rather quickly. Many studies pick up a change in 1998 and almost all see a break by 1999; some find an additional change when the monetary union became a currency union in 2001. If firms were merely selling a wide slice of their product range to foreigners, the pro-trade could happen extremely fast.

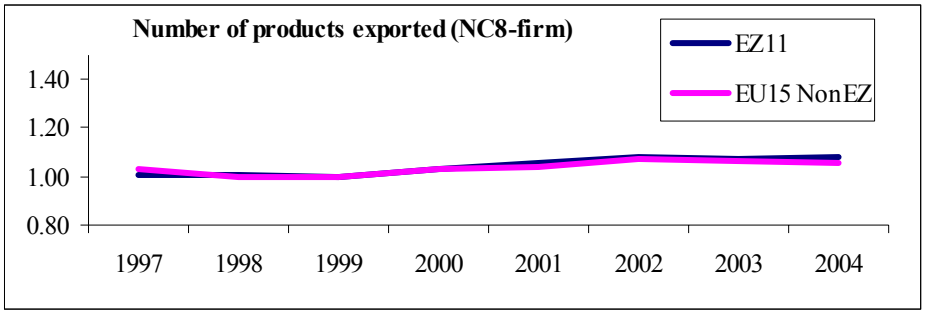
Second, the lack of trade diversion is also consistent with the newly-trade goods channel. As the discussion of how a common currency would yield economies of scope across markets revealed, the euro's adoption would easily broaden export range of outsiders as well as that of insiders. This was the story posited in Baldwin (2006). The new evidence presented in Chapter 4 based on firm level data contradicts this.

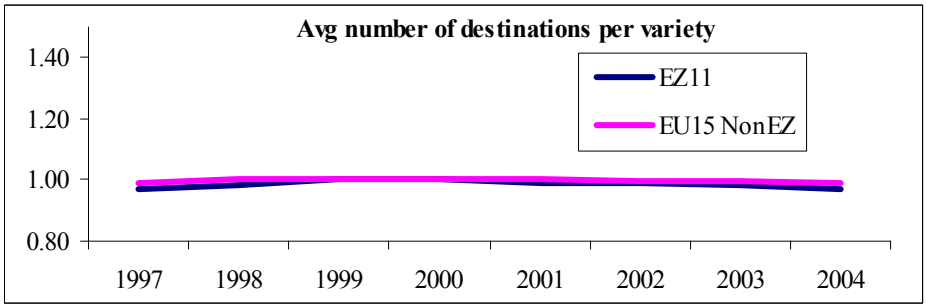
As Figure 37 shows for euro-outsider Sweden, and Figure 38 shows for euro-outsider Hungary, there was very little impact on any of the extensive margins comparing the behaviour for Eurozone and non-Eurozone markets that were in the EU15 (as thus subject to all the Single Market integration schemes even though they were not using the euro). The number of firms exporting to the two groups of markets tracks extremely closely for the Swedish case, and for the Hungarian case, it is the EU15 non-EZ markets that see and increases. (This had to do with a trade offset deal concerning Hungary's purchase of the Swedish Gippen fighter plane.) The products-per-firm extensive margin and geographical extensive margin (number of markets per product) move in precisely the same way for the in and out markets, as far as Swedish firms are concerned. The movement is not quite as similar for Hungarian firms, but there is no clear pattern that would suggest that the euro was systematically expanding the range of exported goods for EZ markets in excess of the effect for non-EZ markets.

What this leads us to conclude is that the newly-trade goods channel operated primarily among Eurozone nations. Note that at least for Sweden, the average sale per good per market rose around 1999. We would explain this with the relative price channel as discussed above.

Figure 37: Swedish firm-level data, intensive and extensive margins (1999 = 1.0)







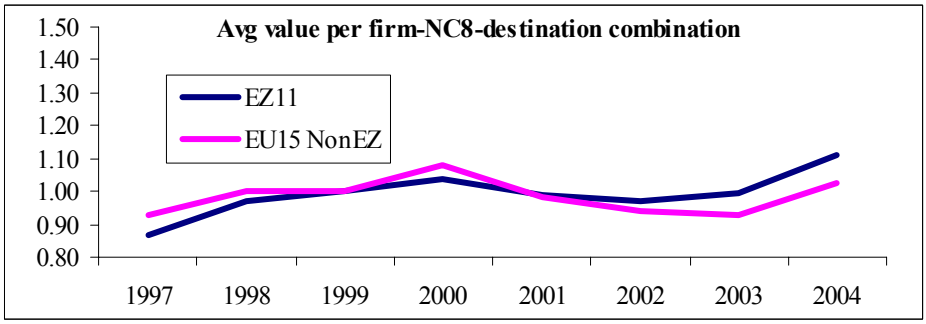
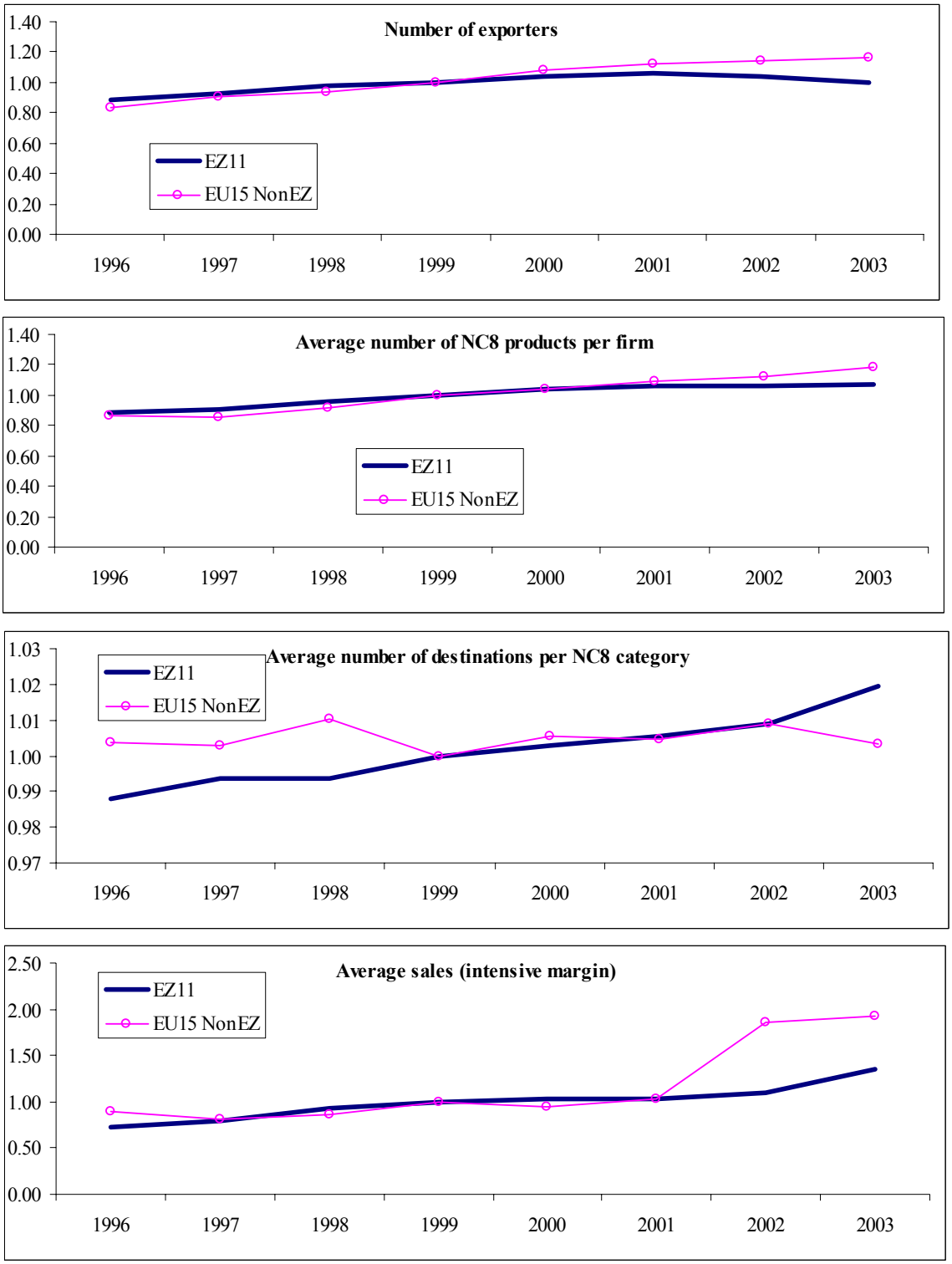


Figure 38: Hungarian firm-level data, intensive and extensive margins (1999=1.0)



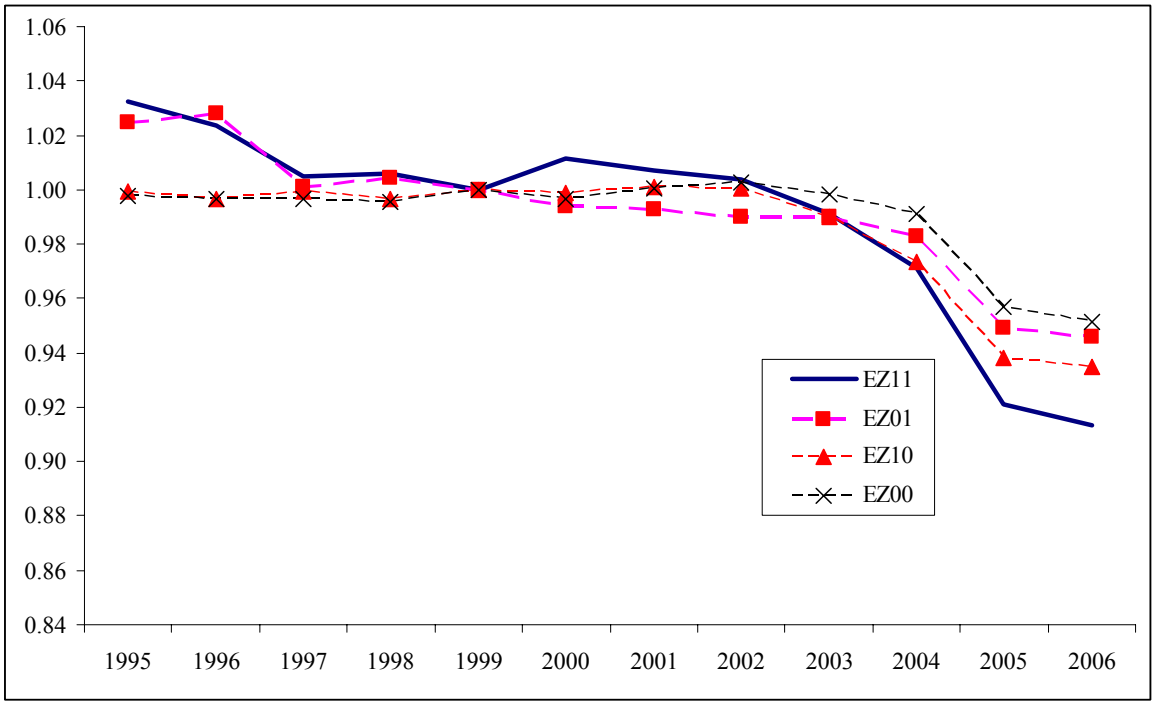
2.2.1. Evidence from trade data for a wider group of nations

The firm-level data is well suited for the task of finding extensive margin changes, but it is currently available only for certain nations. This caveat is what lead us to

estimate – in Chapter 5 – a quasi-extensive margin using publicly available trade data. The data is at the HS6 digit level (about 5,000 different product categories).

The basic conclusions can be seen in this chart (reproduced from Chapter 5 for convenience).

Figure 39: Evolution of zeros, prima facie evidence on the newly-traded-goods hypothesis



Since this data is not firm-level, we can only detect a newly-exported good when the initial trade flow was zero; this is why we call it the quasi-extensive margin. What we see is that the drop in the number of bilateral zeros (i.e. the number of HS6 product categories that are not exported from one nation to another) is much more marked for intra-Eurozone trade flows (shown as the EZ11 line) than it is for other flows. Although all of the zeros fall, especially after the physical euro was introduced, the fall is more clear-cut for the intra-EZ flows. In particular, we note that although the flows from the outs to the ins (shown as the EZ01 line) is below the out-to-out line (shown as EZ00), the difference is not great. Thus this bit of evidence is not far out of line with the firm level data that found the 'ins' and 'outs' we have firm-level data for (France and Belgium, and Sweden and Hungary respectively).

2.3. FDI

Foreign direct investment occurs with a single corporation controls assets in more than one nation. Trade economists shave found it useful to classify FDI into horizontal and vertical. Horizontal FDI is where the firm places production in the foreign market in order to avoid the trade cost involved in making the goods at home and shipping it to the foreign market. Plainly lower trade costs would make such investments less attractive so the euro's introduction would be expected to reduce

intra-Eurozone FDI. Vertical FDI – what we called unbundling FDI – involves a fragmentation of the manufacturing process with each production segment being produced in the nation with the most appropriation cost structure. This geographical dispersion of manufacturing depends upon trade in intermediate goods, and so the euro's pro-trade effects would be expected to stimulate such FDI.

The evidence in the existing literature reviewed in Chapter 6 as well as the new evidence suggest that overall the euro has been pro-FDI. Subject to the provisos on FDI data, this chapter presents evidence that both EU and euro area membership have had pro-FDI effects.

All the various elements seemed to suggest that the euro was stimulating vertical FDI. For example, the euro's pro-FDI effect was much larger in manufacturing than it was in services. This fits in perfectly with the unbundling production paradigm as well as with the newly-trade goods hypothesis since new vertical FDI would create trade in new intermediate goods. For one thing, while unbundling FDI does go on in services, it is not clear how the euro's adoption would directly affect the desirability of doing so, since little or no trade in goods in involved. It may also be that domestic regulator barriers in Europe's service sector deterrent such investments. Moreover, we found that the euro's pro-FDI effect was much larger for deals within sectors as opposed to across sectors. Here sectors are very broadly defined (at least compared to the trade data), so this would encompass both pure horizontal FDI, where the firm produces exactly the same good in more than one market and unbundling FDI that, for example, offshored the production of labour intensive automobile parts to lower wage nations inside the Eurozone. The euro fostered domestic and cross-border M&A activity by both large and small firms, but its effect on small firms was biased towards cross-border activity. Again, this fits right in with the Figure 36 analysis. As trade gets easier, more unbundling FDI occurs as firms that were previously indifferent to it now engage in FDI. Since the threshold firms are systematically smaller and less competitive than the firms already engaged in offshoring, we would expect that the euro would have the greatest impact on smaller firms.

When it comes to outsiders' investing in the Eurzone, the evidence points to a clear pro-FDI effect of the euro, but the effect is only about half as strong as the impact within the area. For such countries, the greater integration of the Eurozone market might make it more attractive to have a production platform inside the Eurozone. The conjecture should require further investigation into the nature of the outsiders' investment.

3. ANSWERING THE 5 SPECIFIC QUESTIONS

The Terms of Reference asks the researchers to address five specific questions. The forgoing chapter have addressed these implicitly to the extent possible. Here we gather the evidence to answer the questions directly.

3.1. Are transaction costs and exchange rate volatility sufficient to explain the pro-trade effects?

The answer to this question is clearly "No". We start with exchange rate volatility. Many of the econometric studies reviewed in Chapter 2 include exchange rate volatility measures in their regressions and still found the euro dummy to be positive

and significant. What this means in plain English is that the euro has had a pro-trade effect independently of the monetary union's impact on the exchange rate volatility.

The second part is transaction costs. As discussed above in Section 2.1, the transaction cost savings entailed by common euro usage cannot be a major part of the explanation for the pro-trade effect. Then main reasons is that if it were, the euro's pro-trade effect would have worked like a preferential trade liberalisation handing an advantage to euro-using nations that was not shared by outsiders. If this had happened, we would have seen a trade diversion effect. Since trade diversion did not occur – as the existing and new evidence in Chapters 2 and 4 and 5 illustrate – transaction costs cannot have played an important role. Additional evidence comes from the fact that the sectoral impact of the euro on trade varied a great deal while a transaction-cost story would suggest that the impact would touch most sectors.

As discussed above, the evidence instead points to a change in the degree of competitiveness of the Eurozone markets. Direct evidence from this came from the pricing-to-market equations presented in Chapter 3. There we found that the euro's adoption seems to have lowered the export prices of both Eurozone nations and most outsiders. If the cause of the effect was common euro-usage (as the transaction cost story would suggest), then we should see the break in pricing behaviour only across trade flows inside the EZ. Since the change also affected outsiders, it must have been coming via the price-cost market, which is a good measure to market structure competitiveness.

We can be fairly confident that this common impact was due to the euro instead of some Single Market measure or global trend, since we did not observe a similar phenomenon for Eurozone exports to outsiders. In short, it seems that use of euros in the importing nations is what is driving the change. We could not directly identify the exact nature of the market structure change, but we noted that the vast majority of outsiders shifted more towards price-to-market (i.e. they stabilize the price for their exports in terms of the currency of the importing nation) on their sales to most EZ nations. This is in line with the general findings in the empirical literature, that firms tend to price-to-market much more to big market than they do to small markets. Indeed, in our new work in Chapter 3, we found that before the euro there was very little pricing to market among future EZ members – with the clear exception of the largest market, Germany. In this sense the Chapter 3 evidence suggests that firms are shifting towards treating the Eurozone as one big market.

The key here is that currency-linked transaction costs and exchange rate volatility are things that should affect all goods in a fairly similar manner. The key to identifying whether these are enough is to look at cross-sector and cross-firm responses to a given euro-adopter. If, as existing literature suggests, the euro's trade volume and price effects vary substantially across sectors and firms, then we can be sure that 'macro' determinates are not enough. To move beyond this, our new empirical work will attempt to explain the sectoral variations using industrial organisation explanations, such as the extent of concentration, ease of entry and scale economies (using the standard proxies for these in the empirical IO literature).

3.2. Lack of trade diversion

The terms of reference ask: A striking result of several recent studies is that the euro seems to have boosted intra-euro-area trade but also trade between the euro area and the rest of the world. How should this result be interpreted?

Chapter 3, 2, 4 and 5 have lengthy discussions of this. The main points are summarised in the section above on Channels. Out initial hypothesis was that the euro lowered the beachhead costs (i.e. fixed market entry costs) in Eurozone markets and this stimulated exports from within the Eurozone and from outside the Eurozone. This hypothesis, however, was rejected by our new firm-level data. There we found that the newly-trade goods hypothesis worked, but only for intra-Eurozone trade. The two nations for which we have the necessary firm-level data did not experience a significant difference in the evolution of their range of products to the EU15 members that joined the Eurozone and those that did not.

3.3. Diversity of effects, and interactions with domestic regulation

The terms of reference ask: There is some evidence that that the trade- and FDI-boosting effects of the euro vary significantly across Member States and industrial sectors. What are the main factors explaining this heterogeneity? And To what extent does the effect of the euro on trade and FDI depend on Member States' progress in product market liberalisation and labour market flexibility?

We were unable to make progress on these questions. In the Chapter 3 pricing to market regressions, we found some patterns relating to the size of markets, but did not really have enough observations to pin down causes. The same holds for sectors. As we came to see that the new-goods hypothesis was critical and that the novel and important pricing effects were emerging, we focused our research efforts on pinning down these issues. As for the interaction between euro effect and domestic product and labour market effects, we did not any headway in tackling these questions. We shall have to leave these questions for the future.

3.4. Interactions between trade and FDI effects

• How are the effects of the euro on FDI and trade related? Should the FDI and trade effects be considered as substitutes or complements? • Has the euro fostered off-shoring and to what extent can this explain the simultaneous increase in intra and extra-area trade?

The theory discussed above in Section 1 showed these two questions are intertwined and that there is a very clear relationship between the trade and FDI effects. Indeed the results of the independent trade and FDI studies allow us to get a much better idea about the nature of the FDI effects. Specifically, the evidence reviewed in Section 2 suggests that the pro-FDI effect is coming primarily from investment related to the establishment of international supply chains, so-called unbundling FDI.

Chapter 8: Concluding remarks

The introduction of the euro was an immense political and symbolic step towards an integrated Europe. It was also the world's largest economic 'experiment.' This experiment opens the door to a major advance in our understanding of how a common currency affects economic activity ranging from trade and foreign direct investment to wage-setting behaviour and corporate business strategies. A series of studies stretching back to the early years of the decade have begun to piece together a wide range of results. The resulting collage is still not fully coherent, this report has moved in that direction.

When it comes to the euro's trade effects the first contribution of the report is to refine "the number". Using the latest data and best empirical methodology, we confirm the received wisdom that the euro has promoted trade significantly, with the aggregate impact being in the range of 5% or so. Note that we have made great efforts to separate the euro's impact from the impact of other pro-integration policies that were also being implemented in the 1999-2006 period, notably the Single Market programmes. This effort has tended to shift down the aggregate number but it is necessary to be absolutely sure that it was the euro causing the effect.

The second main contribution of the report was to advance and refine our understanding of exact how the euro was boosting trade. Which economic channels were important and which were not. Logically, there are two main channels to consider, each with a number of sub-channels. The first is the relative price channel. Simply put, this argues that the euro boosted trade inside the Eurozone since it lowered the relative price of traded goods coming from the Eurozone. Since prices depend upon marginal cost and the price-cost mark-up, the lower relative price could come from two main sources – a reduction in bilateral trade costs among Eurozone nations (e.g. lower transaction costs, hedging costs, etc.), or an increase in competition that pushed down trade prices via a pro-competitive effect. The two sources are easily distinguished since to the extent that the lower trade costs were preferential – affecting only intra-Eurozone trade – it should have led to trade diversion. It did not, so we can be fairly confident that the transaction cost story is not of first-order importance. Direct evidence from pricing regressions suggests that the euro did indeed have a pro-competitive effect on exporters' prices, for both EZ and non-EZ based exporters. We also found evidence that outsiders were moving towards pricing-to-market strategies suggestive of their viewing the Eurozone as a single market. Indirectly evidence on this exact point comes from the heighten convergence of export prices within the Eurozone.

The second main channel – first posited by Baldwin and Taglioni (2004) and elaborated in Baldwin (2005, 2006) – is the newly-trade goods channel. The basic idea is that the euro induced firms to export a wider range of their products to the Eurozone. Thus, it was not merely that trade in existing products was stimulated, the pro-trade effect also came from newly trade goods. This report presents new evidence based on four firm-level data sets (2 from insiders and 2 from outsiders) that seems to confirm the new-goods hypothesis rather resoundingly. Moreover, this new evidence sheds important light on the lack of trade diversion. Earlier studies had suggested that the lack of trade diversion might have been caused by an expansion of the range of goods exported by the 'outs' to the 'ins'. Firm-level trade data from Sweden and Hungary, however, shows that this did not happen. In the Swedish case the main rise in their exports to the Eurozone was due to the so-called intensive margin, namely the old-fashioned way of raising trade – increasing the average sales per product exported. Combining this evidence with our new pricing regressions suggest that the absence of trade diversion was due to the pro-competitive effect of the euro on outsider's export pricing strategy. That is, as the euro made the euro-using nations more like a single market, boosting pricing transparency and making third-party arbitrage safer, Eurozone import customers became more price sensitive and the outsider exporters responded by cutting price-cost margins. This lowered their relative prices and thus stimulated sales.

The report also looks at new evidence on this second channel that come from data that is more aggregate than the firm level data but still very disaggregated and available for all nations. The new evidence in this chapter confirms the firm-level findings.

The third main contribution of the report concerns the euro's pro-FDI effects. The empirical work on the FDI effects is much less rich than that on the trade effect. This is not due to a lack of interest; in the world of modern business, cross-border investment is an integral part of firms' international strategies – especially when it comes to large firms. Moreover, it is widely thought that FDI brings with it valuable foreign know-how that does come with just trade. The problem is that both the data and the empirical methodology are much less well developed.

Using the best available data, theory and econometric techniques, the report concludes that both Single Market integration and and euro area membership have pro-FDI effects. The key points were:

- The euro's pro-FDI effect was much larger in manufacturing versus services. It is likely that the level of protection and barriers to entry in the service sector act as a strong deterrent to cross-border M&As in services across countries. To the extent that the new 2006 services directive breaks down such barriers, it may trigger a new wave of cross border M&As within the EU.
- The euro's pro-FDI effect was much larger for deals within sectors as opposed to across sectors. Thus the euro facilitated cross-border M&As within the euro area, which aimed at restructuring capital within the same sector of activity, rather then boosting the formation of conglomerate activities between sectors.
- The euro fostered domestic and cross-border M&A activity by both large and small firms, but its effect on small firms was biased towards cross-border activity.
- The euro's adoption promoted FDI from outside the euro area, but this effect was only about half as strong as the impact within the area.

- The ‘bottomline’ number – the overall pro-FDI effect – is not clear. Most authors find it is positive, but the estimates range from +15% for in-to-in flows and +7.5% for out-to-in, to +200% and +100%, respectively for the in-to-in and out-to-in flows.

Future research

We cannot pretend to have provided definitive answers to the key questions surrounding the euro’s trade and FDI effects. Even for trade, we only had data for 8 or 9 years and the best data – the firm-level data is so far only available for four nations. There is much work to be done, especially in refine the nation-specific and sector-specific effects, but that shall have to wait for the emergence of better data.

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