THE EFFECT OF THE EURO ON TRADE, INCOME AND PRICES

by

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ABSTRACT

The first paper uses European data to estimate the euro effect on bilateral trade. An extended gravity model and a variety of fixed effects estimators are used to robustly quantify the results. The estimates are then compared to similar ones obtained with both European and non-European data. The findings are consistent with previous studies - the euro has increased bilateral trade by between 9% and 38% for the first 10 years. The link between the euro and trade is crucial for analysing the benefits of common currencies in terms of business cycle synchronization and standards of living. The results strengthen the argument in favour of common currencies in general, and euro adoption in particular.

Keywords: Euro Trade Effects; Gravity Model; Fixed Effects Estimation

The second paper uses European data to estimate the effect of trade on income. A growth equation and an instrumental variable approach are used in a Two-Stage-Least-Squares regression. The estimates are then compared to similar ones obtained with non-European data. The findings are consistent with previous studies – a 1% increase in the trade to GDP ratio increases income by between .25% and 1.21% . This result provides a link between the euro, trade and income. In particular, it suggests that more trade, resulting from common currencies, increases standards of living. The finding is of utmost policy relevance for countries considering joining a common currency in general and the euro in particular.

Keywords: Euro; Trade Effect on Income; Instrumental Variables

The third paper investigates to what extent prices become more flexible after a country adopts the euro. If price flexibility is significantly enhanced, it can potentially offset some of the negative effects of a common currency, such as the lack of monetary independence and exchange rate adjustment in the face of asymmetric shocks. Thus, one of the main drawbacks of a monetary union would be discredited. The evidence suggests a small positive effect of the euro on price flexibility based on time-series micro data from six euro countries.

Keywords: Euro; Price Flexibility; Endogeneity

DEDICATION

To my wife

To my parents

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CHAPTER 1 HAS THE EURO INCREASED TRADE? A ROBUST ANALYSIS

1 Introduction and Motivation

How are bilateral trade flows affected by the euro adoption? Until recently, this question was investigated using data on countries in a monetary union, which were very small or very poor or both. Thus, direct extrapolation of results to the euro was misleading. This paper analyses the question using European data, addresses several critiques of previous studies, and finds that the euro has increased bilateral trade by between 9% and 38%, which potentially could contribute to a higher income per capita. The result is consistent with previous findings and is of utmost policy relevance for countries considering adoption of common currencies in general and the euro in particular.

Andrew Rose's (2000) paper is a seminal contribution to quantifying the trade effects of common currencies. He famously found a tripling of trade - the "Rose effect" - based on a sample of 186 countries. About 1% of those were in a monetary union. Later, others replicated and extended his study, which lead to a smaller, but still economically and statistically significant effect. Micco, Stein and Ordonez (2003) (hereafter MSO) launched the investigation of the euro trade effects and, again, found a significant effect of between 4% and 16%. Baldwin (2006) summarizes and critiques the theoretical and empirical findings of both the euro and pre-euro studies, suggesting that they contain almost exclusively overestimates. After considering all the available evidence, he concludes that the "Rose effect" for the euro has been between 5% and 10% for the first 4 years, but is likely to change thereafter.

This paper updates the estimates of Frankel and Rose (2002) focusing on the effects of the *euro* (rather than common currencies in general) on trade. It has a number of contributions over previous studies:

a) it uses the most recent European Union (EU) data, up to 2008

b) the sample starts in 1995, thereby correcting issues related with Austria, Sweden and Finland's EU membership as of 1995, issues related to the change in reporting of European trade statistics in 1993, issues related with the "Rotterdam effect", and issues concerning the Single Market implementation (see Baldwin, 2006 and Flam and Nordström, 2007)

c) it addresses the Baldwin (2006) critiques

d) it provides direct comparison to previous, pre-euro studies

The results are broadly comparable to previous studies, both for the euro and with non-euro data. I find that the euro has increased trade by between 9% and 38 %, with the most preferred specification resulting in around 13% increase. This is much smaller than the "Rose effect" on non-European data, but is consistent with other findings for the euro. The result is also consistent with Baldwin's (2006) expectation that, although the effect was between 5% and 10% for the first 4 years, it is likely to change thereafter. Finally, the finding is compatible with MSO (2003), which finds a smaller effect than mine, but it is based, again, on just the first 4 years. Overall, this paper suggests that there is a strongly significant euro trade effect, which, although smaller in magnitude than the effect found in non-European data, is still around 13% for the first 10 years, and increasing.

The remaining of the paper proceeds as follows: section 2 reviews the theoretical and empirical literature; section 3 outlines the methodology; section 4 discusses the results and compares them to previous studies; section 5 checks results' sensitivity to various specifications; section 6 summarises and concludes.

2 Background

How the euro affects member economies is of utmost policy relevance for countries considering adopting a common currency (like the Gulf Cooperation Council and the Union of South American Nations, among others), as well as future euro members. To illustrate the importance of the issue, consider the fact that more than two thirds of the sovereign countries today either consider abandoning their national currencies or have already done so (Nitsch, 2008).

While adoption has costs mainly in terms of losing independent monetary policy - as is evident in the case of Greece in 2010, which is unable to devalue its way out of trouble - it also has benefits in terms of reducing transaction costs and exchange rate volatility, lowering interest rates and inflation, and hence increasing trade, investment and income. If empirical evidence corroborates that the benefits outweigh the costs, then countries are likely to consider monetary integration. Furthermore, the euro adoption might have an endogenous, virtuous cycle effect - the euro increases trade, trade increases business cycle synchronization, and the disadvantages of the lack of monetary independence are reduced. Higher trade is also shown to increase income (Frankel and Romer, 1999). These arguments critically depend on how a common currency affects trade, and the euro offers an excellent opportunity to study the issues involved in a more policy relevant context than before.

2.1 Theoretical foundations

To quantify the euro effects on bilateral trade, a gravity model is used, which is based in Newtonian physics where the force of gravity between two planets is directly proportional to the product of their masses and inversely proportional to the squared distance between them (the following theoretical discussion of the gravity model is based on Baldwin et al., 2008):

$$Force\ of\ gravity = G\frac{M_1M_2}{(distance_{12})^2} \tag{1.1}$$

where G is a gravitational constant (equal to $6.67300*10^{-11}$ $m^3kg^{-1}s^{-2}$), the Ms are the planets' masses and $distance_{12}$ is the distance between them.

The analogous "gravity model" for international trade then becomes:

$$Bilateral\ trade = G\frac{GDP_1GDP_2}{(distance_{12})^2} \tag{1.2}$$

This form of the gravity model has produced explained variations of about 90% and has become very popular in empirical international trade (Baldwin, 2006).

Baldwin et al. (2008) argue that a simple OLS estimation based on (1.2), however, almost certainly gives biased results, as G is not a constant – it varies by trading partner, over time and is correlated with many policy variables. Baldwin et al. (2008) develop the theoretical foundation behind the gravity model by modifying the model in Anderson (1979) and Anderson and van Wincoop (2003). Following the theoretical derivation, a "correct" gravity model is arrived at, which might be estimated without a bias. Below are the main features of the theoretical model, which should guide its empirical implementation.

Since trade data are collected in value terms, the model starts with a Constant Elasticity of Substitution (CES) expenditure rather than demand function, where each firm produces a single variety of a unique good. From the solution to the standard utility maximization problem, we find that spending on an imported good produced in the origin nation – *o*, and consumed in the destination nation – *d* is given by:

$$v_{od} = (\frac{p_{od}}{P_d})^{1-\sigma} E_d, \sigma > 1 \tag{1.3}$$

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

Consumer prices are given by:

$$p_{od} = \mu_{od} p_o \tau_{od} \tag{1.4}$$

where p_{od} is the consumer price in nation *d* of a good produced in nation *o*, p_o is nation *o* domestic price, μ_{od} is bilateral price mark-up and τ_{od} is the bilateral trade costs, which might include the exchange rate as well. Combining equation (1.3) and (1.4) and aggregating over all varieties gives us the aggregate bilateral trade:

$$V_{od} = n_{od} (\mu_{od} p_o \tau_{od})^{1-\sigma} \frac{E_d}{P_d^{1-\sigma}} \tag{1.5}$$

where n_{od} indicates the number of nation *o* varieties sold in nation *d*.

This bilateral gravity model tells us that the destination country's Gross Domestic Product (GDP) should enter the empirical specification as it proxies for the income effect in the expenditure function, E_d. Also, bilateral distance should enter the empirical model as it captures part of the effect of bilateral trade costs, τ_{od}. In addition, expenditure functions depend on relative prices, so that a "naive" gravity model is misspecified.

Nation o's market clearing condition, after summing the sales over all markets including its own and making it equal to its production, gives us an expression for nation o price:

$$Y_o = \sum_d n_{od} v_{od} \Leftrightarrow Y_o = \sum_d p_o^{1-\sigma} \left[n_{od} (\mu_{od} \tau_{od})^{1-\sigma} \frac{E_d}{P_d^{1-\sigma}} \right] \tag{1.6}$$

where the second expression comes from substituting the equation for v_{od} in (1.3) and (1.4) in the first expression.

Solving for domestic price gives us:

$$p_o^{1-\sigma} = \frac{Y_o}{\Omega_o}, where\ \Omega_o = \Sigma_d \left[n_{od} (\mu_{od} \tau_{od})^{1-\sigma} \frac{E_d}{P_d^{1-\sigma}} \right] \tag{1.7}$$

This tells us that bigger countries, as given by bigger GDP, will have lower prices ($\sigma > 1$), because they offer goods that are more competitive. This justifies the inclusion of country o's GDP in the gravity equation.

Substituting (1.7) in the expression for aggregate trade volume in equation (1.5) gives us the theoretical gravity equation:

$$V_{od} = n_{od}(\mu_{od}\tau_{od})^{1-\sigma}\frac{E_d Y_o}{\Omega_o P_d^{1-\sigma}} \tag{1.8}$$

It says that the theoretical bilateral trade relationship is for unidirectional exports, that both domestic and foreign GDPs should enter in a multiplicative fashion and that the equation should include the exporting nation market access term Ω_o in order to avoid misspecification. In a cross-sectional environment, one can use pair fixed effects to include the $\Omega_o P_d^{1-\sigma}$ term, but this will not work in a panel data setting, since it is a time-varying variable.

So, Baldwin et al. (2008) take the origin nation's GDP as a proxy for its production of traded goods, the destination country's GDP as a proxy for its expenditure on foreign goods and the bilateral distance between them as a measure for trade costs.

The resulting gravity model is:

$$V_{od} = G_{od}\frac{Y_0 Y_d}{(distance_{od})^{\sigma-1}}\ , where\ G_{od} = n_{od}\mu_{od}{}^{1-\sigma}\frac{1}{\Omega_o P_d^{1-\sigma}} \tag{1.9}$$

Baldwin et al. (2008) argue that failure to recognise that G is not constant leads to econometric problems in estimating a "naive" gravity equation. Taking logs and using panel data, we have an empirical gravity model:

$$lnV_{odt} = (1-\sigma)\ ln(\tau_{odt}) + lnY_o Y_d + lnG_{odt} \tag{1.10}$$

Assuming that the trading costs are due to distance, the euro usage and other factors that vary over time and across partners denoted by Z_{odt} and using the definition of

G_{odt}, the empirical implementation of the theoretical gravity model calls for estimating the following model:

$$\begin{aligned} lnV_{odt} = \beta_0 + \beta_1(\sigma - 1)euro_{odt} + \beta_2(\sigma - 1)lnZ_{odt} + \beta_3(\sigma \\ - 1)\, ln\, distance_{odt} + \beta_4\, lnY_{ot}Y_{dt} \\ + \beta_5 lnn_{odt} + \beta_6(\sigma - 1)ln\mu_{odt} + \beta_7 ln\Omega_{ot} \\ + \beta_8(\sigma - 1)lnP_{dt} + \varepsilon_t \end{aligned} \tag{1.11}$$

What most empirical studies estimate, Baldwin et al. (2008) argue, is the misspecified gravity equation that includes the variables of the first and second row of equation (1.11), but excludes the ones on the third and fourth rows, which are correlated with the euro variable (see (1.7)) and therefore create a biased estimate. Also, Ω_{ot} and $P_{dt}^{1-\sigma}$ are time-varying so they cannot be controlled for by using time-invariant country or pair fixed effects, except in a cross-sectional context. In addition, the effect of the euro shows through *n*, *Z*, and μ as well as through $euro$, so if the third and fourth rows are ignored, they become part of the error term, which will be correlated with the euro term and thus provide biased estimates.

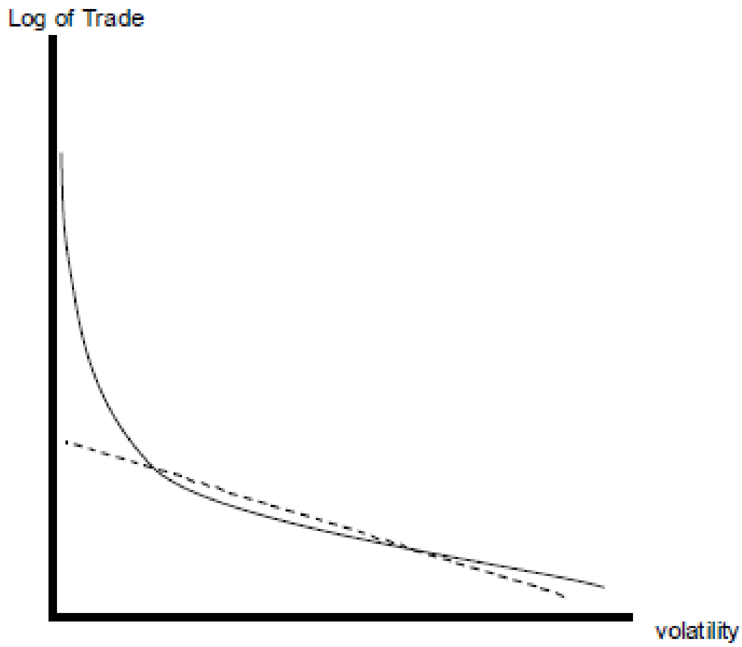
On the other hand, by controlling for those factors, one excludes the euro effect that operates through mark-ups and product variety. Changes in them might be an inseparable part of how the euro affects member economies and thus it might be wise to leave them in the error term. For these reasons, I include estimates of the euro trade effect both when one controls for those factors as Baldwin (2006) suggests, and when they are left uncontrolled for (see Table 1.5).

2.2 Empirical evidence

The empirical literature of the common currencies' effects on trade was launched by Rose (2000). Studies before him have concentrated on the effect of reduced exchange rate volatility on trade and have generally found small, negative effects, suggesting that exchange rate volatility might not be of big importance to trade. Studies using time-series data have had more trouble finding significant trade effects of reduced exchange rate volatility, whereas panel data studies have been more successful (Frankel and Rose, 2002; Rose, 2000).

Using a "naive" version of the standard gravity model, Rose (2000) found that trade approximately triples among countries that share a common currency, even after controlling for exchange rate volatility. That finding suggests that the effect of a monetary union might be quite different from the effect of reducing volatility. In particular, there might be some non-linearity of reducing exchange rate volatility on bilateral trade flows. Because a common currency is a much more robust commitment than a pledge to reduce exchange rate volatility or even a currency board arrangement, it might be expected to have those non-linear effects. To illustrate, consider Figure 1.1 below (Figure 1 from Baldwin, Skudelny et al., 2005):

Figure 1.1: Exchange rate volatility and trade



While the true, non-linear relationship might be illustrated by the solid line, a linear exchange rate volatility term in a regression captures the relationship given by the dashed line. So, adding up a common currency dummy, in addition to the linear volatility term, might be showing up as the "Rose effect".

Baldwin, Skudelny and Taglioni (2005) provide a theoretical foundation of the "Rose effect", or why the true relationship might be convex. They identify two sources of convexities. First, it might be expected that exchange rate volatility affects small firms more than larger ones, as the latter have easier access to hedging opportunities, which might be too expensive for the former. Hence, the marginal impact of reduced volatility will be larger, when there are more small exporting firms to begin with. Second, Europe has a high concentration of small firms, and lower exchange rate volatility will bring an even larger number of small firms willing to export. Thus, a monetary union might be expected to influence trade on the extensive margin, i.e. it lowers the costs of small firms to access export markets and since Europe has a lot of small firms, the effect becomes even larger as exchange rate volatility is reduced to zero.

The gravity model used by Rose (2000) and others after him has had a good track record in empirical international trade and a firm theoretical foundation, as discussed above (Baldwin, 2006; Rose, 2000). It has been estimated in the following general form:

$$ln(Trade_{ij}) = intercept + \gamma CurrencyUnion + controls + error \tag{1.12}$$

The coefficient of interest is γ, which Rose (2000) found to be 1.21. Thus, the "Rose effect" was $e^{1.21} - 1 = 235\%$ increase in bilateral trade. The result was surprisingly robust to various checks performed by Rose.

The magnitude of the estimate generated significant interest and researchers began the "Shrink the Rose Effect" effort (Eicher and Henn, 2009). They pointed to a number of problems - both econometric and sample related - with Rose's original paper. Addressing those critiques, the subsequent studies have generally found a smaller, but still economically and statistically significant result, controlling for a multitude of factors. Table 1.1 summarizes those findings (see Rose, 2008).

Table 1.1: Early studies of the currency union effect

Author	Year	γ	s.e. of γ
Rose	2000	1.21	0.14
Engel-Rose	2002	1.21	0.37
Frankel-Rose	2002	1.36	0.18
Rose-van Wincoop	2001	0.91	0.18
Glick-Rose	2002	0.65	0.05
Persson	2001	0.506	0.257
Rose	2001	0.74	0.05
Honohan	2001	0.921	0.4
Nitsch	2002b	0.82	0.27
Pakko and Wall	2001	-0.38	0.529
Walsh and Thom	2002	0.098	0.2
Melitz	2001	0.7	0.23
López-Córdova, Meissner	2003	0.716	0.186
Tenreyro	2001	0.471	0.316
Levy Yeyati	2003	0.5	0.25
Nitsch	2002a	0.62	0.17
Flandreau and Maurel	2001	1.16	0.07
Klein	2002	0.50	0.27
Estevadeoral, et al	2003	0.293	0.145
Alesina, Barro, Tenreyro	2003	1.56	0.44
Smith	2002	0.38	0.1
Bomberger	2002	0.08	0.05
Melitz	2002	1.38	0.16
Saiki	2002	0.56	0.16
Micco, Stein, Ordonez	2003	0.089	0.025
Kenen	2002	1.222	0.305
Bun and Klaassen	2002	0.33	0.1
de Souza	2002	0.17	0.24
de Sousa and Lochard	2003	1.21	0.12
Flam and Nordström	2003	0.139	0.02
Barr, Breedon and Miles	2003	0.25	0.033
de Nardis and Vicarelli	2003	0.061	0.027
Rose	2004	1.12	0.12
Subramanian-Wei	2003	0.732	0.08

MSO (2003) first based the analysis on the euro. They find a still smaller, but significant effect of between 4% and 16% with data only from the first 4 years. They use two samples – EU-15[1] and a larger one, with several Organization for Economic Co-operation and Development (OECD) countries added. They also use a difference-in-differences, country-pair fixed effects estimator to control for omitted variables as suggested by Anderson and van Wincoop (2003).

Berger and Nitsch (2008) point out to a number of strange results from MSO (2003) - first, the euro effect is too large relative to the EU membership effect; second, it appeared in 1998, a year before the launch of the common currency; third, the effect is heterogeneous among countries, the highest effect being for the Deutsche Mark (DM) bloc of Germany, Austria and the Netherlands and fourth, when removing the DM bloc, the effect disappears. They add another year of data and include observations going back to

[1]Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden, United Kingdom.

1948. They conclude that including a time trend for European integration eliminates the Rose effect completely.

Flam and Nordström (2003) further address a number of issues by using unidirectional exports rather than the average of imports and exports as dependent variable, and partly correcting for omitted variables and reverse causality. Their findings are in line with MSO (2003) or about 15% higher trade. Flam and Nordström (2007) also find evidence that the euro effect is increasing over time – they estimate it to be about 15% in their 1989-2002 dataset and then about 26% with 1995-2005 data. An increasing euro trade effect is corroborated by Frankel and Rose (2002) and MSO (2003) as well. Bun and Klaassen (2002) use a dynamic fixed effects estimator and obtain similar results to MSO (2003), but Bun and Klaassen (2007) find that including time trends reduces the estimates from 51% and 18%, to 3% and 2% for the respective datasets (Baldwin et al., 2008). De Souza (2002) adds a time trend in a gravity model estimated with a sample of EU-15 and finds no "Rose effect" unless the trend is removed.

Frankel (2008) updates MSO's (2003) equations up to 2006, finds similar results and investigates if the gap between the non-euro and euro trade effects can be explained by 1) lags, 2) country size, 3) endogeneity. He concludes that while none of these are responsible for the difference, it seems that the discrepancy comes from the sample size, i.e. when he used the complete data set for 200 countries for 1948-2006, the magnitude and significance of the original "Rose effect" reappear.

Rose (2008) provides a summary of 26 recent studies on the euro trade effects in Table 1.2 (Table 1 in Rose, 2008). He does a meta analysis and rejects the hypothesis that the true effect is zero.

Table 1.2: Recent studies of currency union and trade

			Gamma	SE
1	**Bun and Klaassen**	**2002**	0.33	0.1
2	**de Souza**	**2002**	0.17	0.24
3	**de Nardis and Vicarelli**	**2003**	0.061	0.027
4	**Cabasson**	**2003**	0.63	0.24
5	**Micco, Stein, Ordonez**	**2004**	0.089	0.025
6	**Barr, Breedon and Miles**	**2004**	0.25	0.033
7	**Baldwin and Taglioni**	**2004**	0.034	0.015315
8	**Faruqee**	**2004**	0.082	0.018
9	**de Nardis and Vicarelli**	**2004**	0.093	0.039
10	**Clark, Tamirisa, and Wei**	**2004**	0.22	0.38
11	**Baldwin, Skudelny, and Taglioni**	**2005**	0.72	0.06
12	**Yamarik and Ghosh**	**2005**	1.8285	0.30475
13	**Adam and Cobham**	**2005**	1.029	0.039486
14	**Baxter and Koupritsas**	**2006**	0.47	0.22
15	**Flam and Nordstrom**	**2006b**	0.139	0.02
16	**Berger and Nitsch**	**2006**	-0.001	0.036
17	**Gomes, Graham, Helliwell, Kano, Murray and Schembri**	**2006**	0.069	0.011
18	**Baldwin and Taglioni**	**2006**	-0.02	0.03
19	**Baldwin and Di Nino**	**2006**	0.035	0.01
20	**Flam and Nordstrom**	**2006a**	0.232	0.024
21	**Tenreyro and Barro**	**2007**	1.899	0.351
22	**Bun and Klaassen**	**2007**	0.032	0.016
23	**de Nardis, De Santis and Vicarelli**	**2007**	0.04	0.01278
24	**Brouwer, Paap, and Viaene**	**2007**	0.067	0.025769
25	**Flam and Nordstrom**	**2007**	0.248	0.046
26	**de Nardis, De Santis and Vicarelli**	**2008**	0.09	0.033962

Baldwin (2006) and Baldwin et al. (2008) provide a comprehensive survey of both the theoretical and empirical literature of the effects of common currencies on trade. They also provide a number of critiques, which attempt to explain why Rose (2000) obtained an unrealistically high estimate and why most of the subsequent studies are not relevant for policy implementation. Based on the theoretical literature discussion above, Baldwin (2006) identifies the main critiques as the gold, silver and bronze medal for gravity equation mistakes. The gold medal is for omitted variable bias, and particularly omission of the remoteness variables Ω and P as suggested by Anderson and van Wincoop (2003). The silver is for using the average of exports and imports, while theory suggests the relationship is for unidirectional exports only and also using the log of the sum of exports and imports, instead of the sum of the logs. The silver medal would not create a problem if bilateral trade was balanced. But Baldwin and Taglioni (2007) show that in the case of Europe, there are larger than usual bilateral trade imbalances. Thus, not correcting for the silver medal mistake is likely to overestimate bilateral trade. The bronze medal is for deflating nominal trade variables by US CPI inflation, which can be corrected by including time dummies in the regression.

Baldwin (2006) suggests that to correct for the gold medal of gravity mistakes, one has to include time-varying, country-specific and pair dummies over long samples, or at least, time-invariant, country-pair specific dummies for shorter sample spans. To correct for the silver medal mistake, he suggests using bilateral exports only, but concedes that using just exports or imports might be problematic because of the biased nature of reporting those statistics. In addition, using the log of sums, instead of the sum of logs might not a big problem for a more balanced, North-North trade.

Baldwin and Taglioni (2007) review and analyse the biases in estimation introduced by various specifications and address the Baldwin (2006) critiques. They use a combination of time, country-specific and country-pair fixed effects, as well as nominal trade and GDP values. As a consequence they find that the euro zone dummy actually becomes *negative* and significant, but warn that that might be misleading, since the pair dummies also greatly reduce the impact of EU membership and render it statistically insignificant.

Baranga (2009) argues that gravity equations will always suffer from omitted variable bias and that including fixed effects can never completely control for endogeneity problems, as at least one free dimension of error variation has to be left uncontrolled for in order to estimate the coefficients. To illustrate, he decomposes the error term from a gravity equation as follows (equation (4) in Baranga, 2009):

$$\varepsilon_{ijt} = v_i + v_j + v_t + v_{ij} + v_{it} + v_{jt} + v_{ijt} \qquad (1.13)$$

Here v_i and v_j are country specific factor means over the sample period; v_t represents changes in the trading system over time that affects all countries and includes lower trading costs and tariff reductions; v_{ij} picks up country-pair specific factors. All of these have a time-varying component as denoted by the subscript *t*. If the regressors are correlated with any of these terms then OLS estimates are biased. Inclusion of time, country-specific and pair-specific dummies can help alleviate the bias, but can never completely eliminate it.

Frankel (2008) classifies the main critiques of Rose's (2000) work and offers a rebuttal as follows:

Critique # 1: Cross-sectional inference

The critique is that one cannot infer from cross-sectional evidence what effect a common currency adoption will have. Rather than answering the question: "How much more trade will *joining* a common currency generate?", a cross-sectional analysis answers a different question: "How much more do countries that *are* in a common currency trade with each other?". Frankel's (2008) counter argument is that the effect might only show in the very long run, so that a cross-sectional analysis is relevant. Subsequent research using time-series data suggests that two thirds of the tripling effect can be reached within 30 years.

Critique # 2: Omitted variable bias

Causation might not run from euro to trade. Instead both might be caused by a not-controlled-for third factor, like a history of conflict. That third factor might influence both trade flows and the (un)likelihood of adopting a common currency. Frankel (2008) argues that Rose (2000) has done a pretty good job controlling for such factors, the notable exception being the "multilateral trade resistance" factor, as discussed in Anderson and van Wincoop's (2003) paper.

This factor suggests that, for a given distance between two countries A and B, the further away A is from the rest of the world (more remote), the higher the trade with B. Because this remoteness term is positively associated with bilateral trade and currency union, its omission biases upward the "Rose effect". While finding some substance in this critique, Frankel (2008) also argues that the same theoretical framework predicts trade divergence, which is not supported empirically (see Frankel and Rose, 2002; Flam and Nordström, 2007; MSO, 2003; Baldwin, Skudelny et al., 2005) and thus should not be imposed as a prior constraint. He also argues that including remoteness also wipes out the Free Trade Area (FTA) effect on bilateral trade, a highly counterintuitive result. In addition, even if remoteness is included, the currency union effect is still large and comparable to what MSO (2003) find.

Furthermore, Rose (2000) uses a Hausman test and finds that the currency union variable is exogenous. Also, Rose and Honohan (2001) find that the currency union dummy is not correlated with variables excluded from the equation, suggesting that no bias results from omitting remoteness. Using pair-specific fixed effects to alleviate the problem, Rose and van Wincoop (2001) still find a doubling of trade, instead of tripling.

Critique # 3: Reverse causality

It might be that countries that already trade a lot decide to join a common currency. Thus, it is not adoption that increases trade, but that higher trade leads to adoption. Including time-varying, country-specific fixed effects, alleviates that issue, but wipes out the Rose effect as well. Frankel argues that, while a legitimate concern, efforts of solving it often “throw the baby with the bath water”. Furthermore, several studies use instrumental variable (IV) estimation and find no evidence of reverse causality (Klein and Shambaugh, 2006; Barr, Breedon et al., 2003). Also, in the case of Europe, since the euro was predominantly a political event, trade consideration might have not been as relevant to its formation and thus this problem might be less severe for the euro (Rose, 2000).

Critique # 4: Implausible magnitude of estimate

The critique is that the estimate is just too high to be believable. But the currency union effect is comparable to the FTA effect (Frankel and Rose, 2002). It is also comparable to the border effect as found by McCallum (1995) and others, and probably reflects the “home bias” in international trade.

Critique # 5: Country size

The Rose (2000) results come from either small or very small and poor territories and dependencies in a monetary union before the euro, and thus are not directly relevant to the much more homogeneous, relatively large and rich countries of Europe. Frankel (2008) counters that sensitivity analysis shows the effect of size does not influence the results, and early estimates from the euro from MSO (2003) confirm the presence of the Rose (2000) effect, albeit in a smaller magnitude. Frankel (2008) also finds that for the first 6 to 10 years after a monetary union formation, the euro and other monetary union effects on trade are similar.

Overall, despite the critiques, the prior empirical evidence finds that the euro trade effect exists, but is much smaller than previously thought and smaller than results obtained with non-euro data.

3 Methodology

In this section I analyse the euro trade effects using several variants of the gravity equation. In addition to the theoretical derivation discussed above, it can also be derived from the Ricardian model with continuum of goods, from the Heckscher-Ohlin model with more goods than factors, and from the Chamberlin-Heckscher-Ohlin model with monopolistic competition and increasing returns to scale (see Anderson, 1979; Deardorff, 1998; Helpman and Krugman, 1985; Baldwin, 2006; Flam and Nordström, 2007).

The estimated model and data are closest to MSO (2003), Frankel and Rose (2002) and Baldwin and Taglioni (2007), thus addressing the Baldwin (2006) critiques. I constrict my sample to EU-15 (Greece is excluded from the calculation due to difficulties controlling for its euro entrance, and Belgium and Luxembourg are treated as one country, since their trade statistics were reported that way until 1998 (Flam and Nordström, 2007). The reason for the restricted sample is that the EU-15 includes countries that are similar to each other in many aspects, except that 3 of them – Britain, Sweden and Denmark - are not in the euro. Thus, they provide the cleanest control group to estimate the euro effects. This is the relevant sample as suggested by Baldwin (2006).

My sample starts in 1995 rather than 1992 as in MSO (2003), which has a number of advantages. First, problems with controlling for the EU entrance of Austria, Finland and Sweden in 1995 are eliminated. Second, the Single Market was implemented over several years, starting in 1993. With data starting from 1995, I partly reduce the problem of controlling for its effects. Third, with the introduction of the Single Market, the way trade statistics were recorded changed. In addition, goods entering the Single Market via European ports started to be registered both as trade from the source to the intermediate country and from the intermediate to the final destination country. Starting in 1995, I do not have to control for this so-called “Rotterdam effect”, which was shown to be significant in Flam and Nordström (2003) (see Flam and Nordström, 2007). Finally, the sample is from 1995 to 2008, providing the most up to date results.

While starting in 1995 offers a number of advantages, studies have shown that the euro effects are robust to choosing the sample starting year between 1989 and 1995 (Flam and Nordström, 2007). They also find that the effects become bigger the earlier the sample starts. So by starting in 1995, I can control for a number of factors relevant for the euro trade effects, while not risking obtaining an overestimate.

For results comparability and robustness, I use several estimation methods - OLS as in Frankel and Rose (2002) and a battery of fixed effects, as suggested by Baldwin (2006) and others. This is done to address the gold medal of Baldwin (2006) critiques - the omitted variable bias. In addressing the silver medal critique, I use the average of exports and imports as well as exports only and imports only to control for data misreporting issues and use the log of sums in addition to the sum of logs, for results comparability. To address the bronze medal mistake I use real, bilateral trade data deflated by US CPI with time dummies as suggested by Baldwin (2006), as well as nominal trade and GDP.

I do not include the FTA, EU, and EU trend variables, as MSO (2003) do, because those variables are not significant in their study, and because of the nature of my data set (all of the countries pertain to the same FTA and to the EU). I also drop the real exchange rate (RER) variable as Baldwin (2006) suggests. If anything, without the RER, the MSO

(2006) effect is even smaller. Also, using both exports and imports alleviates some of the valuation effects MSO (2003) use as a justification to include the RER variable. Previous studies suggest that bigger datasets are needed for more significant results, but given my purpose to study the euro effect, I use the EU-15 (excluding Greece and treating Belgium and Luxemburg as one country) sample of more homogeneous countries.

The main equations using OLS and fixed effects estimators are:

$$lnT_{ijt} = \alpha + \gamma_t + \beta_1 Euro_{ijt} + \beta_2 lnY_{it}Y_{jt} + \beta_3 ln\frac{Y_{it}Y_{jt}}{N_{it}N_{jt}} + \beta_4 Land_{ij} + \beta_5 Distance_{ij} + \beta_6 Border_{ij} + \beta_7 Language_{ij} + \varepsilon_{ijt} \qquad (1.14)$$

$$lnT_{ijt} = \gamma_t + \alpha_i + \alpha_j + \alpha_{ij} + \alpha_{it} + \alpha_{jt} + \beta_1 Euro_{ijt} + \beta_2 lnY_{it}Y_{jt} + \beta_3 ln\frac{Y_{it}Y_{jt}}{N_{it}N_{jt}} + \varepsilon_{ijt} \qquad (1.15)$$

where T_{ijt} is bilateral trade between country *i* and country *j* at time *t*, *Euro* is a dummy equal to 1 if both countries are members of the euro at time *t*, $Y_{i(j)t}$ is the real (nominal) GDP of country *i* (*j*) at time *t*, *Land* is 0, 1 or 2, if both countries have access to sea, one of them has, or both have no access to sea, respectively, *Distance* is the log of distance between the principal cities of country *i* and *j*, *Border* is a dummy equal to 1 when the two countries have a common land border, *Language* is a dummy if the two countries share a common official language, *N* is country *i* (*j*) population, α_{ij} are time-invariant, country-pair fixed effects, γ_t are yearly dummies, and α_{it} , α_{jt} , α_i , α_j are time-varying and time-invariant exporter and importer fixed effects, respectively.

Equation (1.14) is closer to Frankel and Rose (2002) and MSO (2003), and equation (1.15) is used to address the issues of omitted variable bias as discussed in the theoretical derivation of the gravity model and for robustness of results.

4 Results and Comparison

For purposes of comparison, in Table 1.3 below I provide the estimates of Frankel and Rose (2002) (Table I, column 2) and MSO (2003) (Table B2, column 1 and 2), together with my results.

Table 1.3: Comparison of results

Variable	F&R (2002, Table I, 2)	MSO (2003, Table B2, 1, 2) OLS	Fixed Effects	OLS	Fixed Effects
Euro	1.78	0.25	0.06	.26	.12
	(.18)***	(0.043)***	(0.013)***	(.02)***	(.01)***
lnYiYj	0.95	0.828	0.996	.71	.33
	(.01)***	(0.013)***	(0.074)***	(.008)***	(.16)**
lnYiYj/NiNj	0.47	0.068		.004	.37
	(.02)***	(0.039)*		(.03)	(.20)*
Distance	-1.11	-0.733		-.73	
	(.03)***	(0.037)***		(.021)***	
Border	0.61	0.275		.297	
	(.13)***	(0.049)***		(.026)***	
Land	-0.36	-0.712		-.59	
	(.04)***	(0.032)***		(.021)***	
Area	-0.17	-0.07			
	(.01)***	(0.013)***			
Language	0.83	0.652		.42	
	(.06)***	(0.073)***		(.039)***	
MU impact	4.93	0.284	0.062	.296	.128
		(0.055)***	(0.014)***		
Observations	31226	1001	1001	2184	2184
R-squared	0.63	0.94	0.64	.93	.99
Country-pair dummies	No	No	Yes	No	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Sample	1970-1995	1992-2002	1992-2002	1995-2008	1995-2008

Note: Dependent variable is LnTijt. Intercepts and yearly dummies used but not reported; robust standard errors in parentheses; *** - significant at 1%, ** - significant at 5%, * - significant at 10%. MU impact calculated as $e^{\hat{\beta}_1} - 1$. Dropping GDP/capita as MSO (2003) do, does not alter results.

The table shows that my results are much closer to MSO (2003) than to Frankel and Rose (2002), which is expected, given Frankel and Rose's (2002) study uses data on non-euro countries. My results also confirm what MSO (2003) find - that the euro trade effects are much smaller than the ones estimated for non-euro countries. While using OLS and time dummies only gives almost identical estimates as MSO (2003), using both time dummies and pair fixed effects I find that in the sample 1995-2008 the effect is .12 (or about 13% increase in trade), while the effect in MSO (2003) is half as much - .06 (or about 6%) in the sample 1992-2002. Both are strongly statistically significant. Thus using the theoretically more relevant pair fixed effects estimator, gives a larger euro trade effect with more data available after the euro adoption. This suggests that the effect is rising over time.

To explore the timing of the euro effect, I interact the time fixed effects with a modified euro dummy variable, which is one if both countries were part of the euro, regardless of the year. Thus, for example, it takes a value of one for Germany and Austria in 1996, although the euro did not exist at the time. This is done to investigate the euro trade effects starting a few years before the actual introduction. Table 1.4 shows the results, together with the findings of MSO (2003) and Frankel (2008). Figures 1.2 and 1.3 further illustrate the trends.

Table 1.4: Timing of the euro effect

Variable	MSO (2003), Table 2, column 3 and 4	Frankel (2008), Table 2, column 3 and 4	Tchinkov (2010)
Euro_1993	-.007 (.035)	-.014 (.035)	
Euro_1994	.025 (.032)	-.006 (.035)	
Euro_1995	.016 (.034)	-.011 (.035)	
Euro_1996	-.000 (.033)	-.013 (.035)	-.015 (.030)
Euro_1997	.018 (.030)	.001 (.035)	-.014 (.029)
Euro_1998	.064 (.032)**	.045 (.035)	.023 (.028)
Euro_1999	.073 (.032)**	.071 (.036)**	.048 (.028)*
Euro_2000	.076 (.035)**	.072 (.036)**	.050 (.028)*
Euro_2001	.166 (.034)***	.162 (.036)***	.103 (.027)***
Euro_2002	.164 (.041)***	.131 (.035)***	.097 (.029)***
Euro_2003		.133 (.035)***	.133 (.027)***
Euro_2004		.151 (.035)***	.159 (.028)***
Euro_2005		.139 (.035)***	.152 (.029)***
Euro_2006		.145 (.035)***	.140 (.030)***
Euro_2007			.177 (.033)***
Euro_2008			.173 (.034)***
Log prod. rGDP	1.06 (.075)***	.409 (.034)***	.086 (.166)
FTA	.045 (.030)	-.067 (.023)***	
EU	-.047 (.053)		
EU trend	-.001 (.004)	-.002 (.002)	
RER 1	-.187 (.060)***	.001 (.003)	
RER2	.374 (.098)***	.007 (.002)***	
Observations	1001	1170	2184
R squared	.783	.93	.99

Figure 1.2: Timing of the euro effect

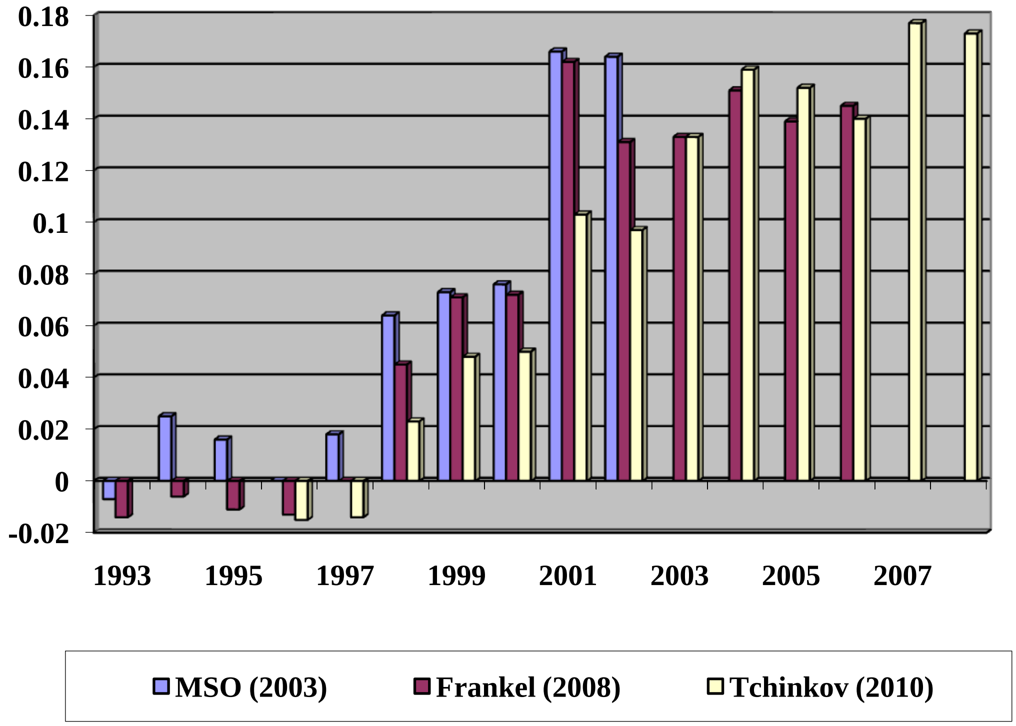
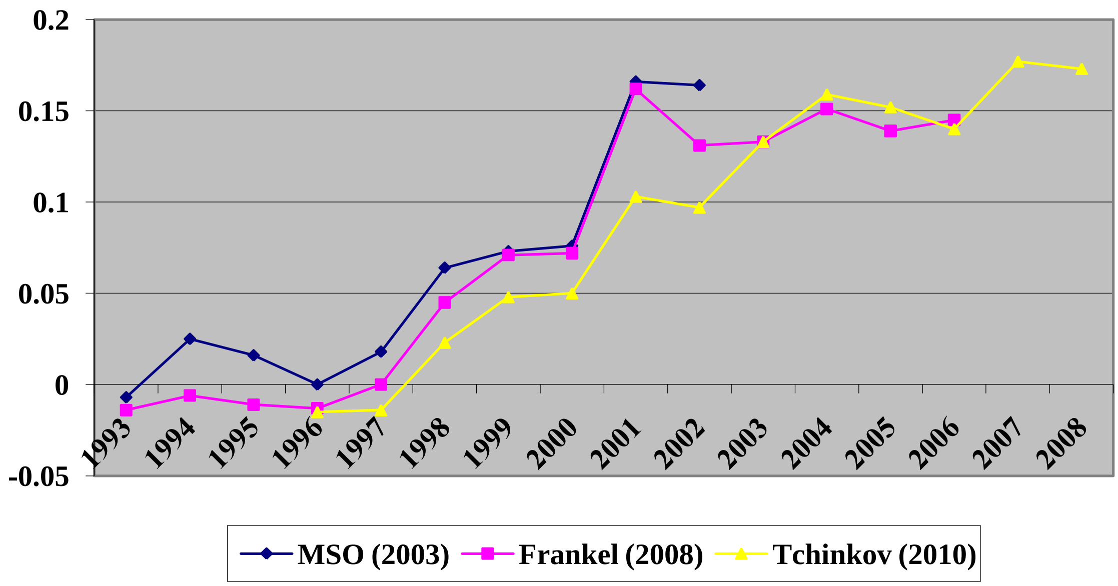


Figure 1.3: Euro effect trend



In addition to confirming Frankel's (2008) finding that the euro effect becomes significant in 1999, as opposed to MSO's (2003) result of 1998, I also find that it becomes higher and very significant after that. And while both MSO (2003) and Frankel (2008) report that the effect levels off at around 16% (coefficient of .15) in 2001, I find a steady increase reaching 19% (coefficient of .17) in 2007 and 2008 (see Figure 1.3). The results are robust to various specifications as in Table 1.5.

So, the major finding is that adding 6 more years of data after the euro and starting in 1995, instead of 1992, doubles the euro trade effect from the one found in MSO (2003) of 6% to about 13%. In addition, the expectation that the effect might change over time, and that it is increasing, is confirmed. The paper also shows that for the euro countries the effect is of much smaller magnitude than for non-euro countries. This finding is obtained from a model specification that is closest to MSO (2003) for results comparability, but it still suffers from the Baldwin (2006) critiques of the gold, silver and bronze medal mistakes. I address this issue in the next section on robustness of results.

In terms of the estimates of the other trade determinants, they are broadly comparable between the two studies, have the expected sign, magnitude and are statistically highly significant. In particular, the effect of GDP is positive and around one, suggesting that bigger countries buy more of each others' goods. This is consistent with the theoretical model, where GDP is a proxy for expenditure. Distance has a negative and significant impact on bilateral trade, with the magnitude of around .7, which is close to the theoretical expectation of about 1. Thus, a one percent increase in the distance between the major cities of two countries lowers bilateral trade by about .7%. A comparable negative and statistically significant effect is found for landlocked countries. In particular, if one of the countries is landlocked, bilateral trade falls by about 82%. Having a common border and sharing a common official language contribute to higher bilateral trade in a significant manner, with the magnitude being about 35%. All of these effects are comparable with estimates from previous studies, have the expected sign, magnitude and are statistically significant.

5 Sensitivity Analysis

Table 1.5 below lists the euro trade effect coefficients obtained from a multitude of different specifications to check for the robustness of the results. In particular, I use 11 different variations of the fixed effects estimation, using time, exporter specific, importer specific, pair, time-varying exporter and importer fixed effects. This is done to address the gold medal mistake as suggested by Baldwin (2006). To alleviate the silver medal mistake, I use real trade and GDP and nominal trade and GDP in the regression. I use the average of exports and imports, the average of exports only, the average of imports only, as well as different ways to average them. Using unidirectional exports or imports does not change the effect. And time dummies are used throughout to address the bronze medal mistake in the gravity equation. In addition, including Greece does not alter the results – effects become marginally smaller, but still highly statistically significant. The coefficients are all significant at 1% and vary from .09 to .32, which is a euro trade effect of between 9% and 38%.

The results are broadly consistent with the evidence shown above. In particular, the bronze, silver and gold medal mistake do not matter much, with the only exception of adding pair fixed effects, which cuts the estimate in half. Otherwise, using all of these different specifications gives little variation in the coefficient – the effect is around .25 without pair fixed effects and around .12 with pair fixed effects.

According to Baldwin and Taglioni's (2007) most preferred specification given in the last column of the table, with time, time-varying exporter, time-varying importer and time-invariant pair fixed effects, the euro trade effect is around .12 or 13%. This estimate, as well as all the estimates in the table, is highly statistically significant at 1%. This is in contrast with the estimate found by Baldwin and Taglioni (2007), which is a statistically significant *negative* effect of .09 or about 9% *decrease* in bilateral trade as a result of the euro.

Table 1.5: Results robustness

		Fixed Effects	No T, 2008 dummy	T only	T,X	T,M	T,X, M	T, pair	T,pair,X, M	T-varying nation	T,nation, pair	T,T-varying nation, pair	T,T-X,M, pair
Real Trade and GDP	Mean X+M	Log sums	.14	.26	.27	.27	.29	.12	.12	.30	.12	.12	.12
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.03)
		Sum logs	.10	.22	.23	.23	.28	.12	.12	.27	.12	.11	.11
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.02)
		Log geom.	.10	.22	.23	.23	.28	.12	.12	.27	.12	.11	.11
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.02)
	Mean X	Log sums	.13	.24	.25	.25	.27	.11	.11	.27	.11	.11	.09
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.03)
		Sum logs	.10	.22	.23	.23	.27	.11	.11	.27	.11	.11	.09
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.03)
		Log geom.	.10	.22	.23	.23	.27	.11	.11	.27	.11	.11	.09
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.03)
	Mean M	Log sums	.15	.27	.28	.28	.30	.13	.13	.32	.13	.13	.15
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.03)
		Sum logs	.11	.22	.24	.24	.28	.12	.12	.27	.12	.12	.14
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.03)
		Log geom.	.10	.22	.24	.24	.28	.12	.12	.27	.12	.12	.14
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.02)	(.01)	(.01)	(.03)
Nominal Trade and GDP	Mean X+M	Log sums	.16	.20	.22	.22	.31	.14	.14	.25	.14	.14	.12
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.03)
		Sum logs	.12	.16	.19	.19	.29	.13	.13	.21	.13	.13	.11
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.02)	(.01)	(.01)	(.02)
		Log geom.	.12	.16	.19	.19	.29	.13	.13	.21	.13	.13	.11
			(.01)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.02)	(.01)	(.01)	(.02)
	Mean X	Log sums	.15	.19	.21	.21	.29	.13	.13	.23	.13	.12	.09
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.03)
		Sum logs	.12	.16	.19	.19	.29	.13	.13	.21	.13	.12	.09
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.03)
		Log geom.	.12	.16	.19	.19	.29	.13	.13	.21	.13	.12	.09
			(.02)	(.03)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.03)
	Mean M	Log sums	.16	.21	.23	.23	.32	.15	.15	.26	.15	.15	.15
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.03)	(.01)	(.01)	(.03)
		Sum logs	.12	.16	.19	.19	.30	.14	.14	.21	.14	.14	.14
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.02)	(.01)	(.01)	(.03)
		Log geom.	.12	.16	.19	.19	.30	.14	.14	.21	.14	.13	.14
			(.02)	(.02)	(.02)	(.02)	(.03)	(.01)	(.01)	(.02)	(.01)	(.01)	(.03)

Note: The table shows regression coefficient β_1 from OLS regression with various fixed effects and trade concept measurement permutations as in (1.15). The euro effect on trade in each case is $e^{\beta}{}_1$-1. All coefficients are significant at 1%. Log of sums is log[(Xij+Xji+Mij+Mji)/4], sum of logs is log(Xij/4)+log(Xji/4)+log(Mij/4)+log(Mji/4), log geom. is log[(Xij*Xji*Mij*Mji)^(1/4)]. X - exports, M – imports, T – time fixed effects.

6 Summary and Conclusions

This paper provides estimates of the euro trade effect, which are found to be between 9% and 38% in an EU-15 sample from 1995 to 2008. It updates previous studies by using the most recent data and corrects for a number of issues that have been raised as critiques of previous work. In particular, it addresses the gold, silver and bronze medal critiques of Baldwin (2006), which are consistent with a theoretically based gravity model. It also uses a multitude of specifications and trade concept measurements to check the robustness of the results. In all specifications, the euro trade effect is strongly statistically significant, with the Baldwin and Taglioni's (2007) most preferred model giving an effect of around 13% increase. This is in contrast with a significant *negative* effect of around 9% *decrease* in trade that Baldwin and Taglioni (2007) found.

The results are comparable and consistent with previous work. In particular, they corroborate the finding that the euro trade effect is much smaller than a common currency effect obtained by Rose (2000) on non-euro data for much smaller and poorer countries. Also, the paper finds that the smaller effect is significant and tends to become larger as more data from the euro zone become available. This result suggests that the effect of common currencies on boosting bilateral trade is not confined to small and poor countries, but is also relevant, albeit in a much smaller magnitude, to the much bigger and richer countries in the euro. It also points to an effect that is likely to become bigger over time.

It thus seems that the main benefit of using a common currency is its trade boosting effect, which works for the euro, despite the already high trade volume that exists in Europe, as the EU has been continuously integrating and abolishing trade barriers for the last 60 years. In a highly integrated world, where global trade and finance become much more important features for the domestic economy, a natural next step after free trade agreements for many countries seems to be the adoption of a common currency. If empirical evidence shows that a common currency increases bilateral trade and trade subsequently increases income and business cycle correlations, then the cost-benefit analysis of common currencies might change in the direction of adopting one so that the country wires itself in a more robust manner to the world economy. Problems in Greece in 2010 notwithstanding, on January 1st, 2011, Estonia is poised to become the next country to take that step and become the 17th member of the euro.

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Appendices

Appendix A: Data Summary Statistics

Table 1.6: Data summary statistics

Variable	Observations	Mean	Std. Dev.	Min.	Max.
Xij	2184	1.09e+10	1.63e+10	9.45e+07	1.40e+11
Mij	2184	1.02e+10	1.49e+10	7.10e+07	1.14e+11
Xji	2184	1.09e+10	1.63e+10	9.45e+07	1.40e+11
Mji	2184	1.02e+10	1.49e+10	7.10e+07	1.14e+11
rXij	2184	1.16e+08	1.67e+08	1211313	1.27e+09
rMij	2184	1.09e+08	1.52e+08	909384.1	1.03e+09
rMji	2184	1.09e+08	1.52e+08	909384.1	1.03e+09
rXji	2184	1.16e+08	1.67e+08	1211313	1.27e+09
GDP1_con	2184	6.17e+11	6.04e+11	6.13e+10	2.09e+12
GDP2_con	2184	6.17e+11	6.04e+11	6.13e+10	2.09e+12
GDP1_conpc	2184	22622.83	5519.738	9197.031	33003.05
GDP2_conpc	2184	22622.83	5519.738	9197.031	33003.05
GDP1_curr	2184	8.12e+11	8.28e+11	6.71e+10	3.65e+12
GDP2_curr	2184	8.12e+11	8.28e+11	6.71e+10	3.65e+12
GDP1_currpc	2184	29922.06	10532.12	11016.22	63184.91
GDP2_currpc	2184	29922.06	10532.12	11016.22	63184.91
Pop1	2184	2.84e+07	2.65e+07	3608850	8.25e+07
Pop2	2184	2.84e+07	2.65e+07	3608850	8.25e+07
US CPI	2184	92.20093	9.68407	78.029	110.247
Euro	2184	.4120879	.4923235	0	1
Border	2184	.1666667	.3727633	0	1
Language	2184	.0897436	.2858795	0	1
Distance	2184	1276.867	679.796	173.0333	3362.978
Land	2184	.1538462	.3608838	0	1

Note: Xij is exports from i to j; Xji is exports from j to i; M is for imports; rXij is exports from i to j deflated by US CPI; GDP1_con is GDP in constant 2000 US dollars; GDP_conpc is GDP in constant 2000 US dollars per capita; GDP1_curr is GDP in current US dollars; GDP_currpc is GDP in current US dollars per capita; Pop1 is the population of exporter; Euro is a dummy equal to 1 if both countries are euro members.

Appendix B: Data Sources and Description

The data are for the EU-15 (excluding Greece; Belgium and Luxembourg are counted together). Of those, 11 were members of the euro as of 1999, and 3 – UK, Sweden and Denmark – were not.

Table 1.7: Variable description and data sources

Variable	Description	Source
Euro	1 if both in euro, zero otherwise	CIA World Factbook
lnYiYj	Natural logarithm of product of real (nominal) GDP in 2000 (current) dollars	World Development Indicators (WDI) 2009
lnYiYj/NiNj	Natural logarithm of product of real (nominal) GDP per capita	World Development Indicators (WDI) 2009
Distance	Log of great-circle distance in kilometers between principle cities	CEP II http://www.cepii.fr/anglaisgraph/bdd/distances.htm
Border	1 if common land border, 0 otherwise	CEP II http://www.cepii.fr/anglaisgraph/bdd/distances.htm
Land	0, 1 or 2 if none, one or both countries are landlocked	CEP II http://www.cepii.fr/anglaisgraph/bdd/distances.htm
Language	1 if common official language, 0 otherwise	CEP II http://www.cepii.fr/anglaisgraph/bdd/distances.htm
lnT	Natural logarithm of exports from i to j plus imports of i from j, deflated by US CPI	OECD STAN Bilateral dataset http://stats.oecd.org/Index.aspx?DatasetCode=BTDNEW&lang=en
US CPI	US Consumer Price Index	International Financial Statistics (IFS), IMF

CHAPTER 2 DOES TRADE CAUSE GROWTH? EVIDENCE FROM EUROPE

1 Introduction and Motivation

Are standards of living affected by trade openness? If a common currency increases trade, and trade is a significant contributor to income, then countries are better off by joining a monetary union, *ceteris paribus*. This paper analyses how trade influences income using European data and finds that higher trade leads to a higher income per capita. The result is consistent with previous studies and is of utmost policy relevance for countries considering adoption of common currencies in general and the euro in particular.

Evaluating the trade impact on income faces a severe endogeneity problem. It may be that rich countries trade more, and not that higher trade increases income per capita. Or, it may be that a third factor, like institutions, causes both higher trade and income. Disentangling the interplay among income, trade, institutions and geography has been a challenging task for economists. Frankel and Romer (1999) attempt to address the problem by instrumenting trade by geographical characteristics and find that trade has a strong positive effect on income. Frankel and Rose (2002) also estimate that a 1% increase in trade relative to GDP raises income per capita by at least .33%. Both studies are based on non-European data. Rodriguez and Rodrik (2000) provide a comprehensive critique of cross-country growth regressions, arguing that the results are not robust to various perturbations. They conclude that there is no strong evidence supporting the hypothesis that trade causes higher income or growth.

This paper's main contribution is to complement existing studies by analysing trade effects on income in Europe, rather than for the world in general. Thus, it provides a critical link in the relationship between the euro, trade and income. For comparison purposes, it uses a similar methodology to Frankel and Rose's (2002), but also attempts to address the Rodriguez and Rodrik's (2000) critiques. It finds that a 1% increase in trade among European countries is associated with between .25% - 1.21% higher income per capita, thus strengthening the argument in favour of adopting the euro.

The remaining of the paper proceeds as follows: section 2 reviews the theoretical and empirical literature; section 3 outlines the empirical model specification; section 4 discusses the results and compares them to previous studies; section 5 checks results' sensitivity to various specifications; section 6 summarises and concludes.

2 Background

There is an ongoing debate in the economic development literature whether trade or institutions are more important for income and growth. Rodrik, Subramanian and Trebbi (2004) argue that institutions are superior in the very long run, whereas Dollar and Kraay (2003) conclude that in the medium run, trade dominates. Since underdeveloped institutions are more characteristic of developing rather than developed European countries and the focus here is on the relationship between the euro, trade and income, this paper focuses on the trade effect, while controlling for institutions. It does not address whether the euro improves (or worsens) institutional quality and thus standards of living, which is a valid, but separate issue.

2.1 Theoretical foundations

The theoretical literature on the influence of trade on income is related to the neoclassical growth literature and in particular to the convergence hypothesis. That hypothesis posits that income at the end of a period depends on initial income, which tends towards a steady state convergence to the long term value. Convergence is conditional if it appears once controls for factor accumulation variables are included (see Frankel and Rose, 2002; Barro and Sala-i-Martin, 1992; Mankiw, Romer et al., 1992).

The Solow model can be used as a starting point to derive the relationship between income, initial income, trade and other income determinants like physical and human capital accumulation. To illustrate, consider an economy i at time t, and let Y_{it} denote output, $L_{it} = L_{i0}e^{n_i t}$ be labour force evolution, where n_i is the constant population growth rate for country i and $A_{it} = A_{i0}e^{g_i t}$ be the evolution of the efficiency level per worker, where g_i is the constant rate of labour augmenting improvement in technology (the discussion here borrows from Johnson, Durlauf et al., 2004). Then two per capita notions are defined as follows: $y_{it}^E = \frac{Y_{it}}{A_{it}L_{it}}$, output per efficient worker and $y_{it} = \frac{Y_{it}}{L_{it}}$, output per worker. The standard result from the Solow model to a first-order approximation implies that:

$$logy_{it}^E = \left(1 - e^{-\lambda_i t}\right)logy_{i\infty}^E + e^{-\lambda_i t}logy_{i0}^E \qquad (2.1)$$

where $y_{i\infty}^E$ is the steady-state value of y_{it}^E and $\lim_{t\to\infty} y_{it}^E = y_{i\infty}^E$. $\lambda_i > 0$ is the speed of convergence of y_{it}^E to its steady-state value. Since the empirical specification will be in output per worker basis, rather than output per effective worker, one can rewrite (2.1) as

$$\begin{aligned} logy_{it} &= gt + \left(1 - e^{-\lambda t}\right)logy_{i\infty}^E \\ &\quad + \left(1 - e^{-\lambda t}\right)logA_{i0} + e^{-\lambda t}logy_{i0} \end{aligned} \qquad (2.2)$$

where the traditional assumption that technological growth and convergence speed are identical across countries, i.e. $g_i = g$ and $\lambda_i = \lambda$ for each i, are reflected in the equation. Thus, income per capita is dependent on two main components – g, the measure of technological progress, and growth due to the gap between initial and steady-state output, measured in efficiency units of labour. Using $-\left(1 - e^{-\lambda t}\right) = \beta$, and allowing for other factors influencing output in a random error term, we have the following as the basis for empirical work on determinants of output per capita:

$$logy_{it} = gt - \beta logy_{i\infty}^{E} - \beta logA_{i0} + (1+\beta)logy_{i0} + \vartheta_i \quad (2.3)$$

where the coefficient$(1+\beta)$ measures convergence. β itself, which is negative, since the convergence parameter λ is positive, implies that countries with a higher initial income have to have a lower *growth* of output – hence convergence. But there is no implication of unconditional convergence, countries can simply converge to their own steady-state growth path.

To implement the equation, Mankiw, Romer and Weil (1992) show how to find empirical analogs of $logy_{i\infty}^{E}$ and $logA_{i0}$. They start with an augmented Cobb-Douglas production function

$$Y_{it} = K_{it}^{\alpha} H_{it}^{\phi} (A_{it}L_{it})^{1-\alpha-\phi} \quad (2.4)$$

where K_{it} is physical capital, H_{it} - human capital, which follow the accumulation equations

$$\dot{K}_{it} = s_{Ki}Y_{it} - \delta K_{it} \quad (2.5)$$

$$\dot{H}_{it} = s_{Hi}Y_{it} - \delta H_{it} \quad (2.6)$$

respectively, where δ is depreciation rate, and s_{Ki}, s_{Hi} are the savings rates to physical and human capital, respectively. Dots above variables denote time derivatives.

Solving for the steady-state value of output per efficient worker, gives

$$y_{i\infty}^{E} = \left(\frac{s_{Ki}^{\alpha} s_{Hi}^{\phi}}{(n_i + g + \delta)^{\alpha+\phi}}\right)^{\frac{1}{1-\alpha-\phi}} \tag{2.7}$$

Mankiw, Romer and Weil (1992) assume that A_{i0} is unobservable and $g + \delta$ is known. Also, they argue that A_{i0} reflects not only technology, but also country specific resource endowments, climate and institutions. They assume these differences vary randomly, i.e. $logA_{i0} = logA + e_i$, where e_i is a country-specific shock, distributed independently of n_i, s_{Ki} and s_{Hi}. Substituting this and (2.7) in (2.3), defining $\varepsilon_i = \vartheta_i - \beta e_i$, and allowing for other determinants of output to enter the equation, denoted by Z_i, we have the following regression relationship

$$\begin{aligned} logy_{it} = {} & gt - \beta logA + (1+\beta) logy_{i0} \\ & + \beta \frac{\alpha + \phi}{1 - \alpha - \phi} log(n_i + g + \delta) \\ & - \beta \frac{\alpha}{1 - \alpha - \phi} logs_{Ki} - \beta \frac{\phi}{1 - \alpha - \phi} logs_{Hi} \\ & + \pi Z_i + \varepsilon_i \end{aligned} \tag{2.8}$$

So, a traditional cross-section regression can be understood as derived from an augmented Solow model and having the following generic form

$$logy_i = \gamma logy_{i0} + \psi X_i + \pi Z_i + \varepsilon_i \tag{2.9}$$

where X_i contains a constant, $log(n_i + g + \delta), logs_{Ki}, logs_{Hi}$. The variables in the first two terms on the right-hand side of (2.9) are those suggested by the augmented Solow model, and the variables in Z_i are those that are not. Regressions in the form of (2.9) have been used extensively in the empirics of economic growth and have been extended in a number of ways. Johnson, Durlauf et al. (2004) provide a summary of 145 different regressors that are used in equation (2.9) as part of Z_i, the majority of which have been found at some point to be significant. One of the problems with empirical growth research has been the lack of consensus which of these variables and what combinations of them are to be included in the equation. So, in practice, most analyses have retained the Solow variables in all specifications in addition to a subset of Z_i variables.

To address these theoretical shortcomings, there has been an effort to empirically determine which variables are robust across various model specifications and have to, therefore, be included in all regression models. Levine and Renelt (1992) find that initial income and investment share of GDP as a proxy for s_{Ki} are the only robust variables, and secondary school enrolment rates, as a proxy for s_{Hi} and population growth are not. This is confirmed by Kalaitzidakis et al. (2000). In addition, Sala-i-Martin (1997) finds that initial income, investment to GDP ratio and secondary schooling are all robust determinants of growth (Johnson, Durlauf et al., 2004). For this reason, in my empirical specification, I include initial income, investment to GDP ratio and secondary schooling and not population growth, in addition to the trade openness measure.

Frankel and Romer (1999), on the other hand, posit that average income in a country is a function of economic interaction among countries, T_i , and within countries, W_i , plus other (possibly including Solow) factors:

$$lnY_i = \alpha + \beta T_i + \gamma W_i + \epsilon_i \quad (2.10)$$

where ϵ_i includes the Solow and some of the Z_i variables discussed above. Thus, this framework is not inconsistent with the Solow theory.

In turn, international trade depends on country proximity factors, and within country trade depends on country size and other factors

$$W_i = \eta + \lambda S_i + \nu_i \tag{2.11}$$

Substituting (2.11) in (2.10), they obtain their main equation to be estimated by proximity instrumental variables

$$lnY_i = (\alpha + \gamma\eta) + \beta T_i + \lambda\gamma S_i + (\epsilon_i + \gamma\nu_i) \tag{2.12}$$

Thus the authors explicitly introduce trade openness and country size as determinants of income, in addition to all the other determinants discussed above, which are left here in the error term.

2.2 Empirical evidence

There are a number of empirical studies that quantify the theory while focusing on trade's effect on income. They generally regress income per capita on a measure of trade and other variables in a cross-country setting and typically find a positive relationship (Edwards, 1997). Trade endogeneity is a problem though - it might not be that trade positively influences income, but rather that richer countries trade more, and have policies that affect both trade and income. Using trade policies as instrument does not solve the problem, since those might be correlated with other domestic free-market policies, which also influence income but are uncontrolled for.

An early influential study on the relationship between outward orientation and growth is by Dollar (1992). He uses estimates of comparative price levels in 95 countries of an identical basket of goods as a measure of trade distortion. He then regresses the average growth in income per capita for the 1976-1985 period on the trade distortion measure, exchange rate volatility and investment rates. Dollar (1992) finds that the higher the trade distortion and exchange rate volatility, the lower the growth. However, Rodriguez and Rodrik (2000) find this result not being robust to alternative specifications. Adding regional dummies, initial income and level of education reduces the significance of trade distortion. When using the latest version of Dollar's (1992) data, even without regional dummies, initial income and education, the sign of the trade distortion coefficient reverses and is not significant (Baldwin, 2003).

Sachs and Warner (1995) construct an index variable of openness for 79 countries, which is zero (closed economy) if any of a number of conditions holds for the country in the sample period – average tariff is above 40%, non-tariff barriers apply to more than 40% of imports, there is a socialist regime, a state monopoly in major export sectors or the black market premium of the official exchange rate is more than 20%. The coefficient estimate of this openness index is positive and significant in explaining the growth rate of GDP, after controlling for schooling, investment, government spending, coups, etc. However, Rodriguez and Rodrik (2000) contend that the result is driven by the state monopoly and black market premiums, which are hardly a measure solely of trade policy (Baldwin, 2003).

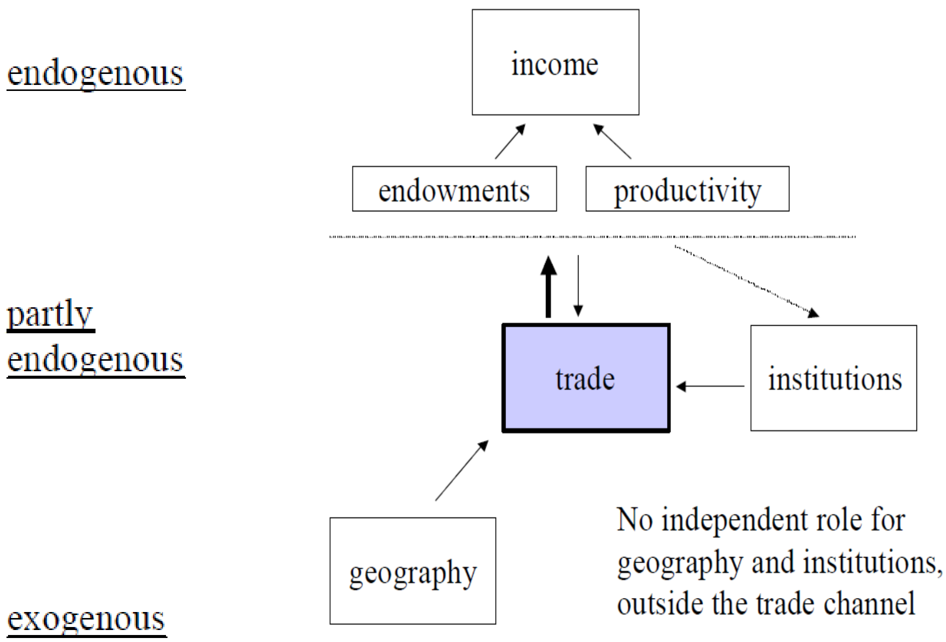
Edwards (1997) is another paper that is critically evaluated by Rodriguez and Rodrik (2000). Edwards (1997) reviews various studies and provides a robustness check with respect to nine measures of trade policy. He finds that six of them are significant and have the expected sign. Rodriguez and Rodrik (2000) argue that these results are dependent on the weighting procedure and that the number of trade measures being significant drops once logs of GDP per capita are used as weights (Baldwin, 2003).

Dollar and Kraay (2003) focus on within country growth and trade flows and thus argue that geographic and institutional variables are less likely to influence the results. They use an instrumental variable regression and find a strong positive effect between changes in trade and changes in growth. Their result is robust to including changes in the share of government expenditure, changes in the inflation rate and revolutions. They conclude, however, “that the available data on trade, growth and other policies may not be sufficiently informative to enable us to isolate the precise partial effect of trade on growth, since our instruments are not sufficiently informative” (Baldwin, 2003).

Frankel and Romer (1999) use geographical variables like distance, common language and border, to instrument for trade and find that a 1% increase in trade share increases income by between 1% and 3%. The coefficient is marginally significant. Frankel and Rose (2002) also estimate that, after controlling for investment in physical and human capital, a 1% increase in trade raises income by at least a third of a percentage point. Both studies find that using instrumental variables actually increases the marginal impact. This result is also corroborated by Noguer and Siscart (2005). They use a richer data set that allows them to estimate the result with much greater precision than before.

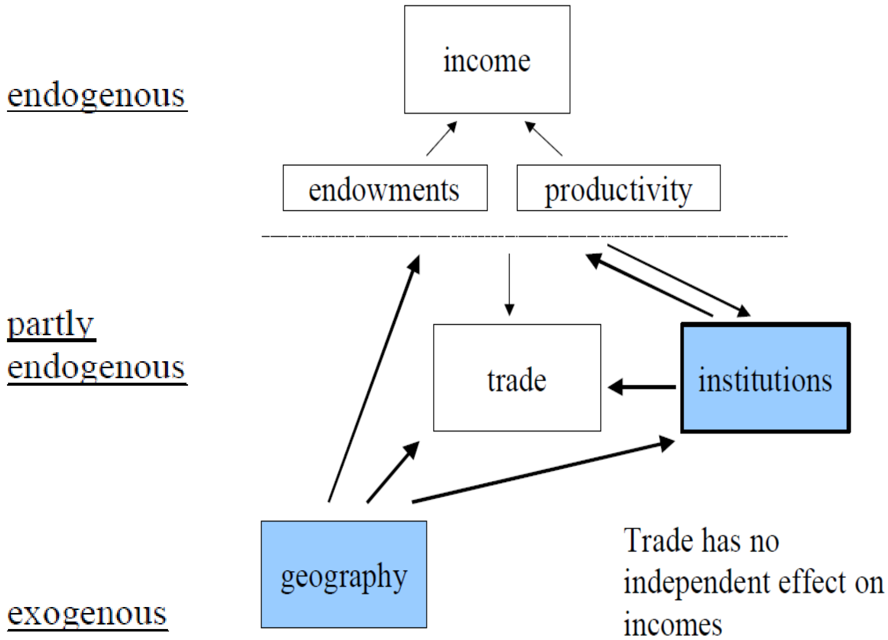
Rodrik (2000) provides a critique of Frankel and Rose (2002). In particular, he argues that the results are driven by outliers like Hong Kong and Singapore. Once these are dropped, the coefficient on trade becomes insignificant. Also, and more importantly, estimates of trade become *negative* once controls for institutional quality and geography are introduced. In other words, they argue that Frankel and Rose (2002) identify the main linkages in development economics literature as there being no independent role for geography and institutions on income, except for their role via trade (see Figure 2.1, Figure 1 from Rodrik (2000) below):

Figure 2.1: No independent role for geography and institutions



Instead, he argues the relationship might have geography and institutions at the core as illustrated in Figure 2.2 below (from Rodrik, 2000):

Figure 2.2: The role of geography and institutions



Thus, the instrument is likely correlated with uncontrolled-for variables (geography, institutions), which affect income via channels different from trade. This attributes to trade income effects of geography through other-than-trade channels. For example, McArthur and Sachs (2001) argue that countries with significant share of population in the tropics suffer from many infectious diseases, which reduces their income. Hall and Jones (1999) suggest that high latitude countries have better institutions, which leads to higher incomes. Both introduce geographic influences on income other than via trade. Rodrik (2000) also finds that although the currency union has an effect on output of about 40%, this is an independent influence and does not operate through trade.

Frankel and Rose (2002) respond by performing a sensitivity analysis in which they address the Rodriguez and Rodrik (2000) critiques. In particular, they control for institutional quality, include distance from the equator, and tropical and regional dummies to control for the effects of geography on income beyond trade. They conclude that in every case, the trade variable retains most of its magnitude and all of its statistical significance. Additionally, they include a currency union dummy along the trade variable and observe that its effect on income is actually *negative*, suggesting that the currency effect on income operates through trade.

Rodrik, Subramanian and Trebbi (2004) use Frankel and Romer's (1999) as well as Acemoglu, Johnson and Robinson's (2001) instruments and find that the coefficient on trade is *negative*, geography is insignificantly positive, whereas institutions consistently come up with a significantly positive effect on growth. Rigobon and Rodrik (2005) use identification through heteroskedasticity instead of instrumental variable approach and confirm that institutions, particularly economic institutions, have a positive effect on growth. Trade *negatively* affects growth, although it has a positive effect on economic institutions such as the rule of law (Rodriguez, 2007).

More recently, Rodriguez (2007) provides a critical assessment of Warner (2003), Dollar and Kraay (2003) and Wacziarg and Welch (2008), which were attempts to address the critiques of Rodriguez and Rodrik (2000). He remains mostly unconvinced about the ways these studies handle the critiques and concludes that it is difficult to "reach definitive conclusions regarding the trade-growth link" (Rodriguez, 2007).

Table 2.1 below provides a summary of the various trade measures and their effects on growth/income in different studies (from Appendix 2 in Johnson, Durlauf et al., 2004).

Table 2.1: Summary of trade effect on growth/income

+/- = sign of coefficient in the corresponding growth regression
? = sign not reported
* = claimed to be significant
_ = claimed to be insignificant

R.H.S. Variables	Studies
Leamer's Intervention Index	• Levine and Renelt (1992) (-,not robust)
Years-Open 1950-1990	• Sachs and Warner (1996) (+,*) • Sala-i-Martin (1997a,b) (+,*)
Openness Indices (growth)	• Harrison (1996) (+,*)
Openness Indices (level)	• Levine and Renelt (1992) (?,not robust) • Sachs and Warner (1995) (+,*) • Harrison (1996) (+,*) • Wacziarg and Welch (2003) (+,*)
Outward Orientation	• Levine and Renelt (1992) (?,not robust) • Sala-i-Martin (1997a,b) (?,_)
Tariff	• Barro and Lee (1994) (-,_) • Sala-i-Martin (1997a,b) (?,_)
Fraction of Export/Import/Total-Trade in GDP	• Levine and Renelt (1992) (+,not robust) • Easterly and Levine (1997a) (?,_) • Frankel and Romer (1999) (+,*) • Dollar and Kraay (2003) (+,_) • Alcala and Ciccone (2004) (+,*) • Rodrik et al. (2004) (+,_)
Fraction of Primary Products in Total Exports	• Sachs and Warner (1996) (-,*) • Sala-i-Martin (1997) (-,*)
Growth in Export-GDP Ratio	• Feder (1982) (+,*) • Kormendi and Meguire (1985) (+,*) • 20+ studies others
FDI inflows relative to GDP	• Blomstrom, et al. (1996)
Machinery and Equipment Import	• Romer (1993) (+,*)

3 Empirical Specification

3.1 General specification

Despite the critiques, this paper seeks to complement existing studies by focusing on the effect of trade on income among European Union (EU) countries. It compares and contrasts how trade affects income in Europe with previous studies, thus providing guidance for future members about expected trade and income changes from euro adoption.

The empirical investigation is based on Frankel and Rose (2002) for which Mankiw, Romer and Weil (1992) provide the theoretical and empirical support as discussed above. Combining it with the empirical evidence on the robust determinants of income and (2.12), the main equation has the following form:

$$\begin{aligned} ln(\frac{Y}{Pop})_{2007,i} = \beta_0 + \beta_1 \left(\frac{X+M}{Y}\right)_{2007,i} + \beta_2 lnPop_i \\ + \beta_3 lnArea_i + \beta_4 ln(\frac{Y}{Pop})_{1990,i} + \beta_5 \left(\frac{I}{Y}\right)_i \\ + \beta_6 School_i + \beta_7 Latitude_i + \varepsilon_i \end{aligned} \tag{2.13}$$

where *Y* is real GDP, *Pop* is population, *X* and *M* are total exports and imports, *Area* is the country area, *I* is gross investment, *School* is secondary school enrolment rate and *Latitude* is the country's latitude. With the exception of real GDP per capita, trade openness, area, and latitude, other variables are computed as averages over the sample period 1990-2007 (see Table 2.10 below in Appendix B for detailed data description and sources).

The first row includes the coefficient of interest, β_1 and also controls for country size, as suggested by Frankel and Romer (1999). If size is uncontrolled for and trade is instrumented by country proximity (i.e. distance, border, etc.), trade estimates are biased. This is because proximity and size are likely to be negatively correlated, i.e. the larger the country, the further away (less proximity) its average citizen is from other countries. Hence the controls for size.

The equation also provides controls for physical and human capital accumulation, as suggested by Mankiw, Romer and Weil (1992) and Sala-i-Martin (1997) (see the theoretical discussion above). Including those might bias the trade effect downwards, as doing so excludes the trade effects on income via physical and human capital accumulation. For this reason, the equation is estimated with and without those controls.

The model also addresses the Rodriguez and Rodrik (2000) critiques – latitude controls for institutions and geography. McArthur and Sachs (2001) suggest that the most important determinants of income are latitude working through institutions and population in the tropics working through diseases and morbidity. Since no EU country has any population or area in the tropics (either in geographic sense as defined by areas limited by latitude of around 23.5 degrees or in climatic sense as defined by the Köppen–Geiger climate classification system in group A (Af, Aw and Am) – tropical and mega thermal countries[2]), that variable is excluded. I run several different specifications which include geography and institutional measures for robustness purposes (see section 5 below).

[2] See http://www.thefreedictionary.com/tropic and http://www.hydrol-earth-syst-sci.net/11/1633/2007/hess-11-1633-2007.html.

My specification differs from Frankel and Rose (2002) in three major ways: first, I have excluded variables that their analysis shows are not significant and others find are not robust like population growth rate, and primary school enrolment rates, to spare degrees of freedom. I also include variables to control for institutional quality and geography for results comparison. Second, given the purpose of this exercise, I have a much smaller sample consisting of European countries only, in particular I look at the EU-25 (all the European Union member countries as of 2006[3]). Thus, the aim is to verify if Frankel and Rose's (2002) result that trade increases income holds for the EU as well. Third, I use GDP per capita in 2007 and a sample period 1990-2007, to provide the most up to date result, before the financial crisis of 2008 and 2009.

3.2 IV specification

In addition to OLS estimates, I also include instrumental variable estimates. I instrument for trade with the help of geographical characteristics, as in Frankel and Romer (1999) and Frankel and Rose (2002). I use a Two-Stage-Least-Squares (2SLS) estimation, where in the first stage log bilateral trade is regressed on log distance, log population in *i* and *j*, log product of areas of *i* and *j*, common language dummy, common border dummy, and the number of landlocked countries. The data I use are bilateral data for the EU-25 as well as the EU's 20 largest trading partners for 2008[4]. The EU as a whole conducts about 75 percent of its outside EU trade with those 20 countries. So, for each EU country, its

[3] Austria, Belgium, Cyprus, the Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, Poland, Portugal, Slovakia, Slovenia, Spain, Sweden, and the United Kingdom.

[4] United States, China, Russia, Switzerland, Norway, Japan, Turkey, South Korea, Brazil, India, Canada, Algeria, South Africa, Saudi Arabia, Libya, Ukraine, Singapore, United Arab Emirates, Australia and Mexico. Source: EUROSTAT (Comext, Statistical regime 4)

bilateral trade with each of the other 24 EU countries and the 20 largest EU partners is considered, which proxies for overall trade.

The exponent of the fitted values of equation (2.14) below is aggregated for each EU country to arrive at an estimate of the country's total trade as a share of its GDP. Following the literature, when constructing the instrument, a variant of the traditional gravity model is used, i.e. only geographical variables (distance, border, area etc.) are included and the countries' incomes are not. The instrumental variable (first-stage) regression has the form:

$$\begin{aligned} ln\left(\frac{Trade_{ij}}{GDP_i}\right) = \alpha_0 + \alpha_1 lnDistance_{ij} + \alpha_2 lnPop_i \\ + \alpha_3 lnPop_j + \alpha_4 ln\,(Area_i Area_j) \\ + \alpha_5 Language_{ij} + \alpha_6 Border_{ij} \\ + \alpha_7 Landlocked_{ij} + \vartheta_j \end{aligned} \qquad (2.14)$$

Table 2.2 below shows the results from this First-Stage regression. It includes bilateral data for the EU 25 and their 19 largest trading partners (bilateral trade data for South Africa were missing) for 2007. There are a total of 25*(24+19) = 1075 observations.

Table 2.2: Gravity (First-Stage) regression

Dependent variable – log of bilateral trade over GDP in country i	Coefficient (robust standard errors in parentheses)
Log distance	-.92 (.05)***
Log Pop1	.15 (.03)***
Log Pop2	.74 (.04)***
Log area product	-.12 (.03)***
Language	.51 (.20)***
Border	.73 (.14)***
Landlocked	-.16 (.07)**
R squared	0.55
F statistic	198.82
Number of observations	1075

Note: Intercepts are not reported; robust standard errors in parentheses; *** - significant at 1%, ** - significant at 5%, * - significant at 10%. The equation is estimated for 2007.

The results are sensible, significant and have the expected effect. In particular, countries that are further away from each other tend to have lower bilateral trade, the higher the size of the trading partner as given by their population, the higher the bilateral trade. Also, a common language and common border enhance bilateral trade. Finally, the higher the number of landlocked countries, the lower their bilateral trade. The results are similar to previous studies both in magnitude and statistical significance.

3.3 IV evaluation

Before I evaluate the instrument, I perform several tests to see if the trade to GDP ratio variable is endogenous and hence justifies the use of IV, which has poor small sample properties (see Nelson and Startz, 1990). This is done by first using the Davidson-

Mackinnon endogeneity test - regressing the trade share on the instrument and other (exogenous) variables from equation (2.13) and saving the residuals. Since the instrument and the other variables in (2.13) are exogenous, the trade variable will also be exogenous if the residuals are uncorrelated with the errors in (2.13). So, equation (2.13) is run by adding the residuals obtained as a regressor. A simple t-test for the significance of the residuals is done. The null hypothesis is that the trade share is exogenous. Results from this test are presented in Table 2.3 below. Several specifications are performed, with similar outcomes. Since the residuals are insignificant, one cannot reject the null hypothesis that the trade share is exogenous. In addition, the Wu-Hausman test statistic is around 1, implying a p-value of around .30 in several specifications (detailed results not reported here). This is confirmed by the Durbin score of around 1.5, and a p-value of around .20. Those suggests that one cannot reject the null hypothesis that trade is exogenous.

So, the endogeneity issue might not be as bad as suggested. This result is conforming with the findings of Frankel and Romer (1999), who also find that OLS and IV estimates are not different. However, I still use both OLS and IV estimates, just to be on the safe side.

Table 2.3: Davidson-MacKinnon test results

Dependent variable – log real GDP per capita 2007	Coefficients (robust standard errors in parentheses)
Trade/GDP	.0007
	(.0018)
Log population	-.09
	(.054)
Log area	.05
	(.07)
Latitude	-.008
	(.009)
Log initial GDP per capita	.82
	(.25)***
Investment rate	.003
	(.02)
Schooling	.004
	(.004)
Residuals	.001
	(.002)
R squared	.82
Number of observations	25

Note: Intercepts are not reported; robust standard errors in parentheses; *** - significant at 1%, ** - significant at 5%, * - significant at 10%.

A valid instrument is one that is correlated with the endogenous variable, but uncorrelated with the excluded variables. Since my instrument is constructed using a variant of the Gravity equation which has been very successful in modelling international trade, intuitively, it should be highly correlated with trade. To evaluate the relevance of the instrument, several tests are performed. First, the correlation between the fitted values from the gravity regression above, summed for each EU country across their trading partners – our instrument – and the actual trade to GDP ratio is .52 (see Table 2.4). Frankel and Rose's (2002) is .72, and the one in Frankel and Romer (1999) is .62. My lower correlation might be due to the fact that I use only the 43 most important trade partners when constructing the instrument, while others use all of the countries trading partners. However, following Frankel and Romer (1999), I run a regression of the endogenous trade to GDP ratio on the instrument and other variables such as population and area. Table 2.5 below suggests that, confirming the intuition, the instrument is strongly statistically significant in explaining trade (results are similar if I add other explanatory variables, such as latitude, initial GDP, investment, schooling, etc.).

In addition, Shea's partial R squared is around .70 in several specifications (detailed results not reported here). Also, the minimum eigenvalue statistic is around 25, suggesting the null of weak instruments is rejected. Thus, as far the explanatory power of the instrument for the trade variable is concerned, the instrument performs well.

Table 2.4: Trade and instrumented trade correlations

	Trade	Trade instrument
Trade	1	
Trade instrument	0.52	1
Log population	-0.6	0.04
Log area	-0.65	-0.26
Log initial GDP/capita	0.17	0.54
Investment ratio	0.32	-0.25
Schooling	-0.12	0.37
Latitude	0.14	0.04
New EU member	0.21	-0.31
Southern country (PIGS + Malta and Cyprus)	-0.38	-0.33
Political risk	0.28	0.38
Euro	-0.04	0.38
Corruption perception index	0.09	0.28
Corruption measure	0.03	0.4

Table 2.5: Regression of actual on constructed trade share

Dependent variable – trade/GDP	Coefficients (robust standard errors in parentheses)
Trade instrument	92.75
	(34.25)**
Log population	-25.53
	(7.88)***
Log area	1.10
	(4.89)
R squared	0.65
F statistic of exclusion of trade instrument	7.33
Number of observations	25

Note: Intercepts are not reported; robust standard errors in parentheses; *** - significant at 1%, ** - significant at 5%, * - significant at 10%.

To test if the instrument is correlated with the error term, I use Sargan's test, which gives a p-value > .10 and thus fails to reject the null hypothesis that the IV is uncorrelated with the errors. Also, since the instrument is constructed using geographical characteristics, I control for geography by using latitude and distance to the equator. This eliminates a possible correlation between the instrument and geography left in the error term. Lastly, the Hansen's J statistic in several specifications has a p-value > .10, implying no overidentifying restrictions.

Table 2.6: OLS and IV results - with and without controls

Variable	F&R, Table II, 1, 2						F&R, Table II, 7, 8					
	OLS	IV	OLS	OLS	IV	IV	OLS	IV	OLS	OLS	IV	IV
Trade	0.79	1.61	.27	.26	.55	.55	0.33	0.43	.0004	.15	.01	.07
	(0.18)***	(0.52)***	(.19)	(.21)	(.15)***	(.16)***	(0.07)***	(0.10)***	(.001)	(.14)	(.13)	(.19)
Log pop.	0.14	0.23	.064	.06	.13	.13	0.07	0.08	-.05	-.09	-.06	-.09
	(0.06)***	(0.08)***	(.05)	(.05)	(.06)**	(.06)**	(0.02)***	(0.02)***	(.04)	(.05)*	(.04)	(.05)
Log area									.01	.08	.01	.05
									(.03)	(.06)	(.03)	(.07)
Log initial							0.71	0.73	.81	.72	.84	.82
income							(0.05)***	(0.06)***	(.25)***	(.24)***	(.27)***	(.26)***
Invest.							0.016	0.013	.003	-.004	.007	.003
ratio							(0.006)***	(0.006)***	(.017)	(.18)	(.019)	(.02)
Sec. school							0.007	0.008	.002	.004	.002	.004
							(0.002)***	(0.003)***	(.003)	(.004)	(.003)	(.004)
Latitude				.18		-.22				-.01		-.008
				(.82)		(1.06)				(.09)		(.009)
Sample	115	110	25	25	25	25	106	102	25	25	25	25
R-sq.	.11		.13	.13			.94	.94	.80	.82		

4 Results and Comparison

4.1 Without controls for initial GDP, investment and schooling

Table 2.6 shows the estimation results. For comparison purposes it shows alongside results from Frankel and Rose (2002), Table II, columns 1, 2, 7 and 8 from both OLS and IV estimation.

Without including initial GDP, investment rate and schooling controls, the OLS effects of trade share on income are about .26% and not statistically significant. This suggests that a 1% increase in the trade to GDP ratio is associated with .26% increase in income per capita. The results are positive although not significant, something that might be due to the small sample size of only 25 EU countries. They are also around 3 times smaller than the ones found by Frankel and Rose (2002), which are .79 and highly significant.

Using the IV estimation, however, not only increases the estimates from .26 to around .55 and .62 (which is the average effect among all specification from Table 2.7 below), but also makes them quite a bit more significant than the OLS estimates. Using the IV estimation to correct for a possible trade endogeneity, a 1% increase in the trade to GDP ratio increases income per capita by a statistically significant .55% (or .62%, the average effect among all specifications in Table 2.7). This result is compatible with a finding of Frankel and Romer (1999) that the estimates increase by using the IV approach. Their results, however, are significant both with OLS and IV, whereas mine become significant only when using the IV approach, possibly due to a much smaller sample size and higher standard errors. Also, the IV estimates are still around 3 times smaller than the ones in Frankel and Rose (2002), .55 vs. 1.61 (see Table 2.6 above).

Using the IV estimates and different specifications which control for latitude, corruption, political risk, new EU member states, share of agriculture in GDP, I find that the effect of a 1% increase in trade to GDP ratio is associated with a statistically significant effect of between .25% and 1.21% increase in income per capita (see Table 2.7 below). This result is compatible with Frankel and Rose's (2002) finding for a broader set of countries, where the effect is between .68% and 1.28% using institutional and geographical controls (Table IIIa in Frankel and Rose, 2002, not reported here) and also compatible with Noguer and Siscart's (2005) Table 3 whose results are between .79% and 1.23%.

So, the results for EU-25 are marginally lower and less significant, but of the same order of magnitude as the effects obtained from a larger set of countries, when controlling for geography and institutions. The trade effect on income per capita is thus not that different for the EU-25 over the sample period 1990-2007 as compared with the world in general for 1970-1990.

Another interesting finding is that geographical controls like latitude and distance from the equator do not seem to be either significant in determining income or important in influencing the trade effect estimates, which retain their magnitude and significance even when controls for latitude and distance from the equator and included. This is running against the Rodrik and Rodriguez (2000) and Noguer and Siscart (2005), but is in accord with Frankel and Rose (2002). Since the EU-25 are pretty similar in latitude, institutions and none of them has any area or population in the tropics, we might expect that these variables should not have a big differential influence on the incomes per capita in EU-25. In the data, the correlation between the trade variable and latitude and distance to equator is .14 (see Table 2.4) and .16 (not reported) respectively, suggesting that even if excluded, they should not bias much the OLS estimates. Also, the correlation between the instrument for trade and latitude and distance from equator are .04 and .08 respectively, suggesting that if excluded they should not be a problem for the IV validity either. On the other hand, the instrumented trade is correlated with institutional measures, suggesting those should be included as controls.

Another result is that when euro membership is included in the regression, its coefficient is significant and the coefficient of trade becomes insignificant at 5% significance level (see Table 2.7, column 9), suggesting that the effect of trade on income per capita might be coming through the euro.

Although latitude and distance from the equator seem insignificant, institutional measures like political risk, and two corruption measures are significant in explaining income per capita and have the expected sign, i.e. the better the institutions, the higher the income per capita, *ceteris paribus* (see Table 2.7, columns 5, 6 and 7). The results are compatible with Acemoglu, Johnson et al. (2001), who argue that once institutions are included, geography plays no independent role for incomes per capita. But, importantly, including institutional controls does not change the trade effect, which retains its magnitude and significance.

In addition, the dummy for new members of the EU as of May 1st, 2004 suggests that those have a statistically significant and negative effect on standard of living, i.e. new member states are poorer even after controlling for initial GDP per capita in 1990 (see Table 2.7 and Table 2.8). There seems to be no negative effect on income for countries like Spain, Greece, Italy, Portugal, Malta and Cyprus, as indicated by the Southern dummy variable. Agriculture is significantly and negatively associated with income per capita, suggesting that countries with higher share of agriculture in GDP have lower incomes per capita.

4.2 With controls for initial GDP, investment and schooling

Once one allows for controls like initial GDP, investment rate and schooling among the EU-25, I find that the trade effect is still positive, but it loses its magnitude and significance (see Table 2.6 and Table 2.8). This is not surprising since I have a small sample as compared to previous studies who find that although the effect of trade when one includes controls diminishes, it is still positive and statistically significant. Also, latitude and distance to equator are still insignificant, new member dummy, one measure of corruption and agricultural share are significant with the expected signs. The investment and schooling variables are positive but not significant. Furthermore, there is a strong effect of initial GDP – it is both positive and statistically significant, indicating that a richer country in 1990 tends to have a higher income per capita in 2007, *ceteris paribus*.

5 Sensitivity Analysis

I perform a number of permutations to investigate the robustness of the results (see Table 2.7 and Table 2.8). First, I consider the impact of outliers who have very high trade to GDP ratio – Luxembourg and Belgium. Excluding them from my analysis does not alter the results, where the trade effects on the remaining 23 EU countries are still relatively high and significant (results not reported here).

Second, I include a dummy for new EU members as of May 1st, 2004 to investigate the effect of new EU members. I find that the new EU member dummy is consistently significant and negative, suggesting that new EU member have a lower income per capita than the richer ones, even after controlling for initial GDP per capita. The inclusion of this variable lowers somewhat the effect of trade on income.

Third, I use distance to equator as well as latitude as different measures of geography. None of them is either significant in affecting income per capita or has an effect of the trade coefficients.

Fourth, a dummy for southern countries is used - Portugal, Spain, Italy, Greece, Malta, Cyprus – to investigate whether they, as a group, have a lower standard of living. I find no such an effect. Inclusion of this variable gives the highest effect of trade on income with no controls for initial GDP, investment and schooling.

Fifth, several different variables are used for institutional quality – International Country Risk Guide's (ICRG's) political risk and corruption measure, as well as

Transparency International's Corruption Perception Index (CPI). I find those significant in affecting incomes per capita. In particular, the better institutional quality and lower corruption measures as given by the indices, the higher the income per capita. So, institutions matter even for the relatively homogeneous countries in Europe. However, their inclusion does not change the trade effect on income.

Sixth, I include a dummy for the euro and agricultural share of GDP as well. The euro turns to have a significant and positive effect on income per capita. Furthermore, when it is included, the trade effect is generally less significant, suggesting that the euro effect on income works through trade. Also, the higher the agricultural share of GDP, the lower the income per capita, *ceteris paribus.* This is compatible with the view that more agricultural countries are less developed and with lower standards of living.

The general conclusion from the sensitivity analysis is that in all instances, the effect of trade on incomes per capita is quite stable and retains its significance with the IV estimation procedure (see Table 2.7 and Table 2.8).

Table 2.7: Robustness check - no controls

	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
Trade	.83	.92	1.21	.25	.54	.55	.37	.63	.30
	(.003)**	(.37)**	(.01)**	(.19)	(.24)**	(.11)***	(.21)	(.35)*	(.15)*
Log population	.04	.03	.11	-.03	.01	-.04	-.002	-.02	.03
	(.13)	(.14)	(.16)	(.06)	(.11)	(.06)	(.07)	(.11)	(.06)
Log area	.17	.21	.20	-.01	.15	.17	.08	.16	
	(.16)	(.19)	(.22)	(.07)	(.15)	(.07)**	(.01)	(.14)	
Latitude	-.02		.01	-.001	-.02	-.03	-.01	-.03	.01
	(.02)		(.03)	(.01)	(.02)	(.01)***	(.01)	(.02)	(.08)
Log equator		-1.14							
		(1.21)							
Southern country			.85						
			(.63)						
New EU member				-.65 (.07)***					
Political risk					.04				
(ICRG Index)					(.01)***				
CPI (Corruption						.2			
Perception Index)						(.03)***			
Corruption							.21		
							(.04)***		
Agricultural share								-.18 (.14)	
Euro member									.41
									(.13)***
Estimation	IV	IV	IV	IV	IV	IV	IV	IV	IV
Sample size	25	25	25	25	25	25	25	25	25

Note: Intercepts are not reported; robust standard errors in parentheses; *** - significant at 1%, ** - significant at 5%, * - significant at 10%. For Political Risk, CPI and Corruption, the higher the measure, the better the countries perform.

Table 2.8: Robustness check - with controls

	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
Trade	.07	.08	.14	.03	.06	.36	.08	.01	-.07
	(.19)	(.20)	(.24)	(.12)	(.18)	(.11)***	(.15)	(.17)	(.10)
Log population	-.09	-.08	-.08	-.07	-.08	-.09	-.07	-.12	-.08
	(.05)	(.05)	(.05)	(.03)	(.06)	(.04)**	(.06)	(.05)**	(.04)
Log area	.05	.05	.06	-.01	.05	.15	.05	.07	
	(.07)	(.07)	(.07)	(.05)	(.07)	(.05)***	(.07)	(.06)	
Latitude	-.01		-.005	.001	(-.01)	-.02	-.01	-.02	.002
	(.01)		(.01)	(.006)	(.01)	(.01)***	(.01)	(.01)**	(.01)
Log initial GDP	.82	.82	.79	.57	.79	.35	.72	.68	.77
	(.26)***	(.26)	(.27)***	(.11)***	(.29)**	(.12)***	(.23)***	(.15)***	(.20)***
Investment ratio	.003	.003	-.001	.01	.006	-.02	.01	-.003	.01
	(.02)	(.02)	(.22)	(.01)	(.02)	(.01)***	(.02)	(.02)	(.01)
School	.004	.004	.003	-.002	.003	-.003	.0001	.003	.001
	(.004)	(.004)	(.004)	(.002)	(.004)	(.01)	(.003)	(.003)	(.003)
Log equator		-.39							
		(.48)							
Southern country			.11						
			(.14)						
New EU member				-.45					
				(.08)***					
Political risk					.01				
(ICRG Index)					(.01)				
CPI (Corruption						.13			
Perception Index)						(.02)***			
Corruption							.08		
							(.04)		
Agricultural share								-.17	
								(.05)***	
Euro member									.21
									(.11)*
Estimation	IV	IV	IV	IV	IV	IV	IV	IV	IV
Sample size	25	25	25	25	25	25	25	25	25

6 Summary and Conclusions

This paper analyses the effect of trade on income per capita based on European data for the period 1990-2007. It seeks to update and corroborate a general result found in a larger set of countries – that an increase in the trade to GDP ratio is associated with an increase in income per capita. It is of a particular importance to countries which intend to adopt the euro (like Estonia, Czech Republic, Poland and Hungary) and countries which plan to join common currencies in general (like the ASEAN group and the Gulf Cooperation Council). If a common currency increases trade, and more trade in turn leads to a higher income per capita, the economic argument for joining a common currency is reinforced.

A critique of the general result has been that the trade variable is endogenous and it is impossible to disentangle the effects of trade, institutions and geography on income per capita. I follow the literature by using a geographically constructed instrument for trade and controlling for other geographical and institutional factors that might have an effect on income per capita via channels other than trade. I do not claim to have resolved those problems, but I attempt to limit them as much as possible while focusing on the trade effects for the EU-25 countries, thus providing a close comparison to the estimates obtained with non-European data.

So, the critiques notwithstanding, I find that the result that more trade is associated with a higher income per capita generally holds for the EU-25 countries. In particular, I find that a 1% increase in the trade to GDP ratio increases income per capita by between

.25% and 1.21%. This result is similar in magnitude and significance to the more general finding using a larger set of countries. This suggests that a country gets to benefit from joining in a monetary union by enjoying more trade with its partners which in turn increases its standard of living.

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Appendices

Appendix A: Data Summary Statistics

Table 2.9: Data summary statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Log real GDP/cap	25	10.21261	0.352859	9.657202	11.19524
Trade/GDP	25	118.0968	59.56027	47.04066	314.443
Instrumented trade/GDP	25	0.587084	0.353735	0.304726	1.831431
Log population	25	15.82647	1.503564	12.88214	18.22502
Log area	25	11.16504	1.721905	5.755742	13.37456
Log initial GDP/cap	25	9.812592	0.387311	9.007508	10.647
Invest./GDP	25	22.82808	3.664313	17.26715	31.65174
School enrolment	25	104.9426	12.85759	90.31319	137.0692
Latitude	25	49.42925	7.716728	35.03115	64.42968
New member	25	0.4	0.5	0	1
Southern dummy	25	0.24	0.43589	0	1
Political risk index	25	81.06	5.858256	70.5	92
Euro	25	0.52	0.509902	0	1
Agricultural share of GDP	25	1.512	0.872888	0.3	3.6
Log distance to equator	25	8.597879	0.161362	8.265768	8.875105
Corruption Perception Index (CPI)	25	6.724	1.672892	4.2	9.4
Corruption measure	25	3.78	1.233896	2	6

from ICRG					
Real GDP/cap 2007 of EU25	1075	29033.32	11500.94	15633.97	72783.16
Area of EU 25	1075	159083	165579.8	316	547026
Pop of EU 25	1075	18600000	23500000	409197	8.2300000
Landlocked EU 25	1075	0.2	0.400186	0	1
Area of EU 25 + trading partners	1075	1861161	3695543	316	17100000
Population of EU 25 +trading partners	1075	94500000	257000000	409197	1320000000
Landlocked dummy	1075	0.134884	0.341758	0	1
Bilateral trade value in US $	1075	8770000000	22900000000	133920	218000000000
Log area product	1075	23.48507	2.939251	12.22654	29.8654
Number landlocked	1075	0.334884	0.519133	0	2
Common border dummy	1075	0.071628	0.257991	0	1
Common official language	1075	0.053954	0.226031	0	1
Bilateral Distance between principal cities in km.	1075	3374.128	3535.407	59.61723	18190.62

Appendix B: Data Sources and Description

The cross-sectional data are for EU-25 (EU members as of 2006). Table 2.10 below provides a brief variable description and data sources.

Table 2.10: Data sources and description

Variable	Description	Source
Log GDP per capita 2007	Natural log of GDP per capita, PPP in 2007	World Development Indicators (WDI) 2009
Log GDP per capita 1990	Natural log of GDP per capita, PPP in 1990	World Development Indicators (WDI) 2009
Log Population	Natural log of average population	World Development Indicators (WDI) 2009
Log bilateral trade in 2007	Natural log of exports from i to j and imports from j to i, in US dollars	UN Comtrade database http://comtrade.un.org/db/default.aspx
Distance	Log of great-circle distance in kilometres between principal cities	CEP II http://www.cepii.fr/anglaisgraph/bdd/distances.htm
Area	Area measured in squared kilometres	CEP II http://www.cepii.fr/anglaisgraph/bdd/distances.htm
Landlocked	0, 1 or 2 if none, one or both countries were landlocked	CEP II http://www.cepii.fr/anglaisgraph/bdd/distances.htm
Trade volume in 2007	Exports plus imports as a share of GDP	World Development Indicators (WDI) 2009

Latitude (distance to equator)	Absolute value of latitude (distance to equator)	Centre for International Development (CID) at Harvard University geography data from www.cid.harvard.edu/ciddata/geographydata.html To get distance to equator, multiply latitude by around 111 km. (see Length of degree calculator - National Geospatial-Intelligence Agency at http://www.nga.mil/MSISiteContent/StaticFiles/Calculators/degree.html)
Investment ratio	Average Investment share of GDP	World Development Indicators (WDI) 2009
Schooling	Average Secondary School enrolment rates, % of gross	World Development Indicators (WDI) 2009
Currency union	1 if a country is part of euro, zero otherwise	CIA Word Factbook
Language	1 if common official language, zero otherwise	CEP II http://www.cepii.fr/anglaisgraph/bdd/distances.htm
Border	1 if common border, zero otherwise	CEP II http://www.cepii.fr/anglaisgraph/bdd/distances.htm
Political risk – institutional measure	Index of country political risk in July, 2009, higher index, better quality	ICRG by Political Risk Services group Table 3B
Agriculture	Agricultural share of GDP	European Commission, Agricultural Statistics Basic data – 2.0.1.2, column 10 http://ec.europa.eu/agriculture/agrista/2008/table_en/en2.htm
Corruption	Corruption Perception Index, 2007, from 1 to 10, 10=least corrupt	Transparency International http://www.transparency.org/policy_research/surveys_indices/cpi/2007

CHAPTER 3 THE EFFECT OF THE EURO ON PRICE FLEXIBILITY

1 Introduction

Does the euro lead to increased price flexibility within its members? In this paper I find that one measure of flexibility - the frequency of price changes - has increased by up to 5 percentage points (p.p.) or about 40% in several euro area countries. This effect has the potential to offset one of the major disadvantages of common currencies - the loss of independent monetary policy and exchange rate adjustment in the face of asymmetric shocks, which would reinforce the argument for joining a monetary union (MU).

Until recently the discussion of the relative merits of flexible versus fixed exchange rates or monetary unions was based on the theoretical developments on Optimal Currency Areas (OCA) of Mundell (1961), McKinnon (1963) and Kenen (1969). The theory suggests that, among other things, if a country experiences similar business cycles with its trade partners and has flexible prices, then it is more likely to gain from monetary unification. The presence of these factors would obviate the need for independent monetary policy and mitigate the impact on economic activity. Hence, if empirical research reveals that a country fulfills those criteria, then the country would be better fit to adopt a common currency, *ceteris paribus*.

Advancing the argument further, Frankel and Rose (1998) introduce a new insight to the OCA criteria. They establish empirically that business cycles synchronization can be endogenous, i.e. even if a country does not fulfill it *ex ante*, it is likely to fulfill it *ex post*. Hence, the more synchronized the business cycles after joining, the less the need for independent monetary policy. The authors conclude that a country should not be judged suitable for MU membership based only on the *ex ante* fulfillment of the OCA criteria.

This paper, in turn, investigates the endogeneity of price flexibility. A country with flexible prices is more suitable for a MU as they can bring the desired adjustment following a shock, even in the absence of a flexible exchange rate. If joining a MU makes prices more flexible, they may generate sufficient adjustment to improve welfare compared to the exogenously sticky prices case with flexible exchange rates. Thus, this OCA criterion might be satisfied *ex post*, even if it is not *ex ante*.

The main contribution of this paper is that it provides an empirical estimate of the effect of the euro introduction on price flexibility within several euro zone countries. The euro has indeed modestly increased price flexibility with the effect ranging from a small negative to a statistically significant 5 p.p. There is also some evidence that the effect becomes larger over time.

The discussion proceeds as follows: section 2 surveys the theoretical and empirical literature; section 3 describes the data and methodology; section 4 presents the results; section 5 offers a discussion; and section 6 concludes.

2 Background

2.1 Theoretical foundations

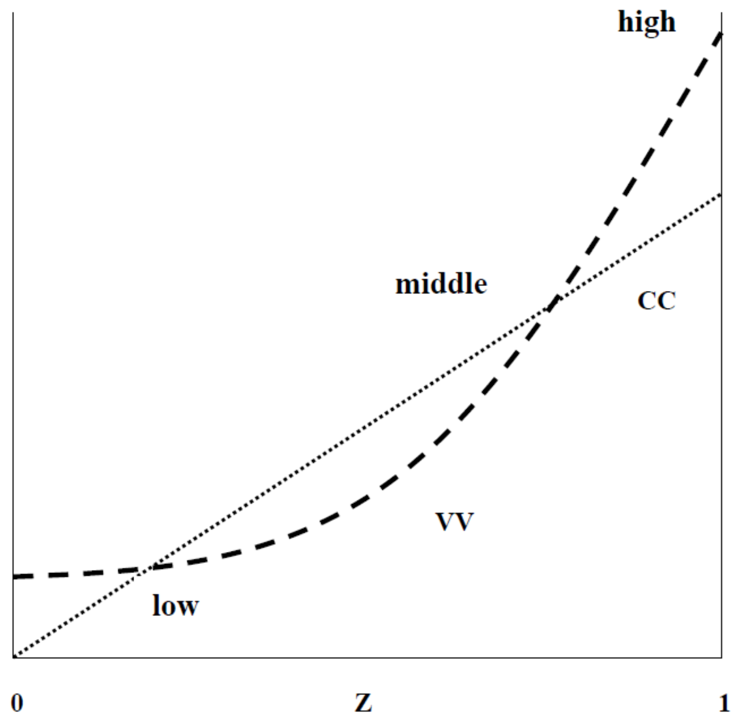
With the advent of the New Open Economy Macroeconomics (NOEM) literature (i.e. Obstfeld and Rogoff, 1995), the issues associated with a country joining a MU can be analyzed in a more consistent manner, which allows a welfare comparison and calibration of results. In particular, the welfare performance of different exchange rate regimes in a general equilibrium, sticky price model are analyzed by Devereux and Engel (2003), Devereux (2000, 2004) and Bachetta and van Wincoop (2000) among others.

All of this work, however, takes prices as exogenously sticky and thus has little to say about the endogenous responses following a change in the exchange rate regime. There are two recent theoretical papers that explicitly endogenize price stickiness in a general equilibrium model and derive theoretical conclusions about price flexibility after the change in monetary policy. Both of them find that fixing the exchange rate could lead to, potentially, large increases in price flexibility.

Devereux (2006) allows monopolistically competitive firms to choose *ex ante* whether to invest in the opportunity to change prices *ex post*. This decision explicitly introduces a menu cost - the trade-off is between the real labour cost and the benefits of flexible prices in the face of fluctuating demand for the firm's products. The author finds potentially large positive effects of fixing the exchange rate on price flexibility. In terms of Figure 3.1 below (Figure 1b from Devereux, 2006), the CC locus represents firms' idiosyncratic labour cost of investing in price flexibility and the VV locus represents the benefit for the marginal firm of doing so (the difference in expected profits of a firm which makes the investment versus the case in which it does not). The vertical axis measures both the costs and benefits, in dollars. *Z* is the fraction of firms that choose to invest.

The CC locus is upward sloping as firms differ in their idiosyncratic cost of investing in price flexibility. This assumption allows only a fraction of firms to choose to invest, leading to an intermediate degree of price stickiness, which is a reasonably realistic case.

Figure 3.1: Determination of *Z*, the fraction of firms investing in price flexibility



If firms are assumed to have identical costs, then a corner solution will imply that either all firms choose to flex or all choose not to flex, which is not that realistic. Intuitively, the real cost of changing prices might include physical costs of printing new price lists, but also the costs of gathering information, reviewing it, etc. Some firms are more efficient in these activities than others, hence the assumption.

The VV is also upward sloping and the author finds it to be convex. Its positive slope comes from the interaction between the decision made by all firms and the incentive of the marginal firm to invest. To see the logic, consider the optimum price setting solution for the flexible price firm, as derived in the paper:

$$\tilde{P}_h = \delta[H^{\psi\alpha}\frac{M}{\chi}(P_h^{\lambda-1}\frac{M}{\chi})^{1-\alpha}]^{w} \tag{3.1}$$

where H is labour supply, M is domestic money stock, χ is random velocity shock, P_h is home price aggregator, the term in small parentheses is market demand derived from the solution to the general equilibrium model and δ, ψ, α, λ, ω are various parameters. Suppose, for example, that there is a positive monetary shock which gives an incentive for the firm to increase its price both because nominal market demand for its product increases (the term in small parentheses, where $\alpha < 1$) and because the real wage increases (the term $H^{\psi}\frac{M}{\chi}$). The extent to which the firm will adjust depends on Z - the fraction of firms investing in price flexibility. Z does not appear explicitly in the equation above, but it appears implicitly via the optimum solution to H and P_h . Z has two opposing effects. First, given other firms raise prices P_h, the market demand for any firm's product rises via the term in the small parentheses ($\lambda - 1 > 0$). Second, as P_h rises, real balances decrease, reducing the home demand for labour and the real wage through the term $H^{\psi}\frac{M}{\chi}$ (as derived in Devereux, 2006). The latter decreases the firm's desire to adjust its price. The author's calibration suggests that the first effect dominates, which gives the upward sloping VV curve, i.e. the more firms choose to invest in price flexibility (higher Z), the higher the benefits of each firm to also do so. This strategic complementarity in firms' pricing decisions gives rise to the possibility of multiple equilibria as shown on the graph. Whenever VV is above CC, the benefit of investing to the marginal firm is higher than the cost, so it invests and increases the proportion of firms who have invested, Z. That process continues until VV=CC, which determines the equilibrium value of Z.

From the optimal solution for the exchange rate

$$S = \frac{(1-\gamma)M\chi^*}{\gamma M^* \chi} \tag{3.2}$$

after a monetary shock to the foreign country - a change in either χ^* or M^* (foreign shock to velocity of foreign money or foreign money stock) - the domestic monetary authority has to react in order to keep S fixed, by changing M (home money stock), assuming γ - the relative preference for home goods - is unchanged. This change in M influences firms' decision to invest in price flexibility as described above. Devereux (2006) shows that the VV locus depends on the variance of M - any increase in the variance shifts the VV locus up and changes the equilibrium Z. The intuition is that fixing the exchange rate or joining a MU will bring in more nominal demand fluctuation for firms' products as the monetary authority has to validate shocks coming from the other members. The benefits of price flexibility increase, more firms invest and the overall price flexibility increases.

But why is there more demand fluctuation in a MU rather than less? Is it not a major reason for joining to stabilize inflation? Indeed, a country with a history of high inflation and unstable monetary policy would find it beneficial to fix its exchange rate as a nominal anchor to inflationary expectations, thus ”importing” stability. To join the euro, however, a country must already possess the necessary stability - it is very unlikely that a high inflation/unstable country can become a member (except unilaterally). More nominal demand fluctuation for this already stable country comes from its exchange rate link. For example, if, say, Sweden joins and Germany slips in a recession, it is likely that the European Central Bank (ECB) lowers interest rates for all euro countries (because Germany has a bigger weight in ECB decisions), including Sweden. Lower rates for Sweden could cause more demand in it than would otherwise occur if Sweden was out. ”Importing” the monetary conditions of other countries in a MU causes more demand fluctuation in an otherwise stable country. With more volatile environment coming from real and nominal shocks to all the members and the unavailability of an exchange rate to cushion the shock, firms find it beneficial to invest in price flexibility, and consequently price flexibility is enhanced.

The theoretical model presented above suggests a testable implication – there should be evidence of more nominal demand fluctuation in a country that joins a monetary union. While this is a relevant question, I leave it for future work to establish the connection. In this paper I use Devereux’s (2006) reasoning to establish *one* way in which a common currency might influence price flexibility and then try to find evidence within several euro countries based on a reduced form equation. Presence or absence of such evidence does not imply the model is correct.

The conclusion from this theoretical work is that in a general equilibrium model with endogenously flexible prices in a two-country framework, strategic complementarity among firms can cause a sufficiently large increase in price flexibility with a fixed exchange rate. In particular, Devereux (2006) finds that with a one-sided peg, like a Currency Board Arrangement (CBA), where the domestic authorities are solely responsible to fix the exchange rate, price flexibility unambiguously increases. By contrast, in a multi-sided peg, like a MU, price flexibility increases if the shocks are real, but it can actually decrease if the shocks are nominal.

Senay and Sutherland (2005) extend Devereux's (2006) analysis to a small open economy. Their model differs from Devereux's (2006) in its fully dynamic specification, and most importantly in its welfare comparison. They use a version of the standard model in the NOEM literature and a Calvo-style pricing structure, but unlike the standard Calvo (1983) with an exogenous probability that prices change in the next period, they endogenize the decision by allowing firms to choose the average frequency of price changes. The trade-off is between the menu costs of price flexibility and the benefits of adjustment in face of demand fluctuation. The more frequent and bigger the shocks to nominal demand, the higher the probability of changing prices, as the benefit of doing so increases.

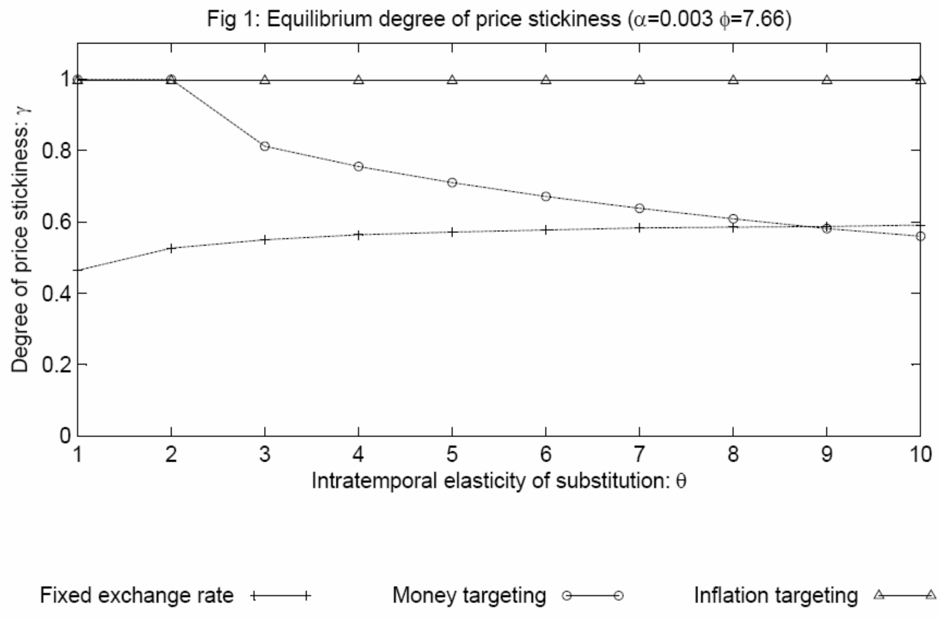
The authors compare price flexibility under three different regimes: inflation targeting, money targeting and fixed exchange rates. They identify situations in which the ranking of regimes is reversed as compared to the exogenous price models. With endogenous price flexibility, fixed exchange rates generate the most price flexibility for a range of values for the intratemporal elasticity of substitution between 1 and 9 (see Figure

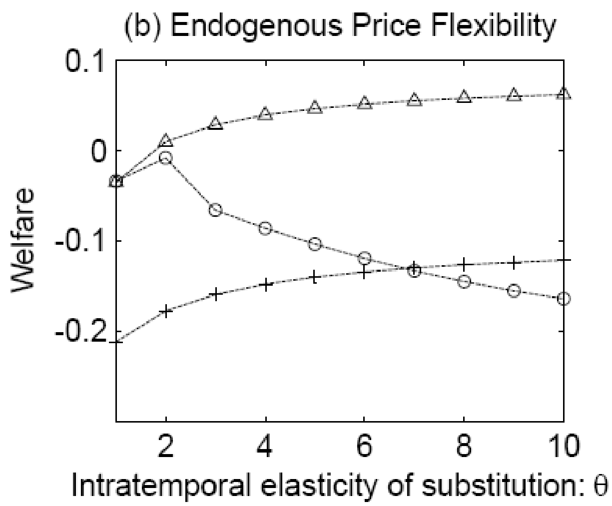
3.2 below - Figure 1 from Senay and Sutherland, 2005). The intuition behind this is as follows: with small values of the intratemporal elasticity of substitution between home and foreign goods, a larger adjustment of the terms of trade is needed for output to respond to a shock. With fixed exchange rates, thus, the larger adjustment comes from larger price flexibility. Put simply, when substitution is low, i.e. expenditure switching is weak, a bigger terms of trade adjustment is needed, which, in the absence of flexible exchange rates, is achieved by bigger price movements.

On the other hand, this enhanced price flexibility cannot compensate the loss of independent monetary policy and tends to lead to lower welfare with fixed exchange rates, relative to, say, inflation targeting (see Figure 3.2 b from Senay and Sutherland, 2005 below). This is because the positive effect of the terms of trade responding to shocks is more than offset by the extra cost of price flexibility, the net result being reduced welfare with fixed exchange rates. The results, however, are sensitive to the parameterization and model used.

Overall, the theoretical literature suggests there should be evidence of enhanced price flexibility in a country, which has fixed its exchange rate or joined a monetary union. The same has been found in the case of wage flexibility (Calmfors and Johansson, 2006).

Figure 3.2: Equilibrium degree of price stickiness and welfare





2.2 Empirical determinants of price flexibility

The empirical literature about the determinants of price flexibility is scarce at best. This is in part due to the relative scarcity of individual item microeconomic data covering many product types, which only recently have been released by statistical institutes for purposes of research. A number of papers analyze empirically price changes in a specific product or market: Cecchetti (1986) on newsstand magazine prices; Lach and Tsiddon (1992) and Eden (2001) on food prices; Kashyap (1995) on catalogue prices; Levy et al. (1997) on supermarket prices; Genesove (2003) on apartment rents (Dhyne, Alvarez et al., 2006). Some studies use micro prices of products covering a larger part of the Consumer Price Index (CPI) to analyze the degree of price flexibility. Examples are Bils and Klenow (2004) for the United States and Baharad and Eden (2004).

The primary source of research on price flexibility in a fixed exchange rate/MU environment is a series of papers for individual euro countries published within the Inflation Persistence Network project of the European Central Bank (ECB). They examine two empirical definitions of price flexibility - the average frequency and the average size of individual items' price changes. A paper that summarizes the results and draws conclusions about the euro level price setting behaviour of firms is by Dhyne et al. (2006). It finds that on average 15.1% of prices change in a given month in Europe, compared with 26.1% found by Bils and Klenow (2004) for the US. Thus, the US has more flexible prices, but it has been in a MU for a longer time.

The paper by Dhyne et al. (2006) uses data on individual item prices at different stores within different countries in the euro-zone and empirically determines the most important factors that influence price flexibility. The authors construct a measure of the average frequency and size of price changes over time and conduct a cross-sectional regression. The individual country studies use seasonal patterns, aggregate inflation rate, sectoral or product-specific inflation, inflation volatility, sales, indirect tax changes, types of outlets, attractive pricing and euro cash changeover[5] to determine the most important factors influencing price changes. They, however, stop short of discussing the effects of the euro introduction, beyond the cash changeover.

Another paper by Angeloni et al. (2006) attempts to shed some light on the effect of the euro on price flexibility and inflation persistence. They draw conclusions based on time-series data aggregated for six euro countries (Austria, Belgium, France, Germany, Italy, and Spain) on the quarterly frequencies of price changes for 50 product categories. They look more closely at two dates as being important for the euro effect - 1996 and 1998. In 1996, it became increasingly clear that both Italy and Spain, whose participation in the MU was considered uncertain, will in fact join as scheduled. In 1998, the publication of the European Commission's convergence report indicated that the countries will go ahead and join in a MU starting in 1999. Thus, the authors concentrate on the effects *expectation* of joining might have had on price changes. They also consider the date Jan.1st, 2002, the date of the euro cash changeover. Based on time plots and summary statistics, the authors conclude that (among other things) there is no effect of the euro on the frequency or the size of price changes. Also, the euro cash changeover has increased price adjustment frequencies, and has decreased price adjustment magnitudes.

The authors, however, do not build a model that incorporates other factors that might have an influence on the variables of interest and thus their conclusions might be seen as a first approach to the data. One of their discussants, William Dickens from the Brookings Institution, writes: "At the very least, I would like to have seen the authors construct estimates of the frequency of price changes at different points in time controlling

[5] The euro was introduced on Jan. 1st, 1999, but the actual coins and notes did not begin circulating until Jan. 1st, 2002. This latter event is referred to as the euro cash changeover, as opposed to euro introduction.

for the rate of inflation" (Angeloni et al., 2006). In contrast, and in accordance with the theoretical foundations above, I concentrate on the effects the euro introduction itself had, not on the expectations of the introduction, and also I control for different aspects of inflation.

3 Data and Methodology

The individual country data sets are described in Dhyne et al. (2006). The paper provides motivation for the choice of explanatory variables, treatment of sales and product replacements, aggregation details as well as harmonization to minimize differences among data collection practices. Put simply, the euro-wide aggregated dataset consists of monthly time-series data on fifty individual item prices belonging to six product groups, sold at various stores around different euro area countries. This approach allows the individual item prices to be followed over time. Dhyne et al. (2006) construct the following statistical measures:

$$X_{ijt} = \begin{cases} 1\ if\ P_{ijt}\ and\ P_{ijt-1}\ are\ observed \\ 0\ if\ at\ least\ one\ of\ them\ is\ not \end{cases} \tag{3.3}$$

$$Y_{ijt} = \begin{cases} 1\ if\ P_{ijt} \neq P_{ijt-1} \\ 0\ otherwise \end{cases} \tag{3.4}$$

$$F_{jt} = \frac{\sum_{i=1}^{n_j} Y_{ijt}}{\sum_{i=1}^{n_j} X_{ij}} \tag{3.5}$$

Here F_{jt} is the average across stores frequency of price changes for product category j in time period t; n_j is the number of stores that sold product category j; P_{ijt} is the individual price of product category j in store i at time t. The average frequency of price changes across stores in a given time period is then available as a time-series measure of price flexibility. These are aggregated across products using CPI weights, and then across countries using the relevant country weights. The aggregation issues are discussed in detail in Angeloni et al. (2006) and Dhyne et al. (2006).

The data I use are for six individual countries and not the euro-wide aggregated data. Those six countries include the four biggest euro area economies - Germany, France, Italy and Spain, and two of the smaller ones - Austria and Belgium, thus they might be considered a representative sample of the whole euro area. The authors of the respective country's study were very generous in providing me with their aggregate price change datasets constructed as in Dhyne et al. (2006) above - monthly and quarterly time-series of the frequency and size of price changes. They cover both aggregate frequency and size, and the equivalent measures for subcomponents of CPI like processed food, unprocessed food, energy, non-energy industrial goods, services. In some cases, data are also broken down to price increases and decreases only[6]. The data for inflation are from the OECD website. Table 3.1 provides a summary of the studies whose data I use and Table 3.3 in Appendix A provides summary statistics for the variables. Figures 3.4 – 3.11 illustrate time plots of the data for the respective countries. There are similar studies about 4 more countries – Finland, Luxembourg, Netherlands and Portugal – but those data were unavailable.

[6] For detailed information about the specifics of data collection, cleaning, truncation, treatment of sales and missing products, etc., please refer to the respective country analysis summarized in Table 3.1.

Table 3.1: Coverage of country data

Country	Paper	% of CPI	Period covered
Austria	Baumgartner et al. (2005)	90 p.c.	Feb. 1996 - June 2006
Belgium	Aucremanne and Dhyne (2004)	68 p.c.	Feb.1989 - Jan. 2001
France	Baudry et al.(2004)	65 p.c.	Aug. 1994 - Feb. 2003
Germany	Hoffmann and Kurz-Kim (2006)	20 p.c.	Jan. 1998 - Jan. 2004
Italy	Veronese et al. (2005)	20 p.c.	Feb.1996 - Dec. 2003
Spain	Alvarez and Hernando (2004)	70 p.c.	Feb.1993 - Dec. 2001

The basic time-series regression model for each of the six countries, which incorporates the euro effect on price flexibility, is the following:

$$F_t = \beta_0 + \beta_1 Inflation_t + \beta_2 Euro99_t + \varepsilon_t \qquad (3.6)$$

F_t is one of the measures of price flexibility - the average price change frequency across products for a country. $Inflation_t$ is the aggregate monthly/yearly inflation rate, $Euro99_t$ is a dummy equal to one if the country was a member of the euro in the specific time period, zero otherwise. I am interested in the coefficient β_2, which reflects the euro effect controlling for aggregate inflation rate.

A critical assumption here is that the euro variable is uncorrelated with any other variable that influences price flexibility but is left uncontrolled for. Those include seasonal patterns, sales, indirect tax changes, types of outlets, attractive pricing, and others found by the individual country studies to explain the frequency and size of price changes. Since none of these variables changed its level with the euro – i.e. firms did not start practicing attractive pricing or offering sales, nor did seasonal effects only start to appear with the euro - the assumption is realistic. The other group of variables which are correlated with the euro but uncontrolled for - like trade and competition - do not pose a problem for the interpretation of results. The influence of more trade, for example, on price flexibility is a result of the euro, i.e. the euro has increased trade, which in turn influenced price flexibility. Since the euro and trade are highly correlated (see Frankel and Rose, 2002), the effect is still traceable to the common currency, hence its coefficient remains unbiased. Including the rate of inflation in the equation is critical as I am interested in the euro effect beyond inflation, which can be influenced with monetary policy and is thus not a unique characteristic of the common currency. Since the euro is correlated with inflation (only low inflation countries are allowed in), if uncontrolled for, inflation will reflect its influence on price flexibility through the euro introduction and bias it downwards.

Whenever the available data cover the period after January 2002, when the actual euro cash changeover was carried forward, I also include a dummy for the euro cash changeover - *Euro02*. When the actual coins and notes were introduced on Jan. 1st, 2002 and both the euro and individual currencies were functioning as legal tender for about two months, anecdotal evidence suggests that some businesses took the opportunity to adjust their prices upwards. For example, in some sectors, the new prices were rounded off upwards in euro, and the sellers used the euro cash changeover as an ”excuse” to change/increase prices. Also, in Austria, there was a period between Oct. 1st, 2001 and March 1st, 2002 of dual pricing, i.e. firms were supposed to display prices in both euro and domestic currency as a way to get consumers accustomed to the ”new” euro numeraire. Because during this period price changes were more apparent, debated and likely to be challenged in front of the authorities, firms might have adjusted prices before that period, or waited after it to incorporate the changes. For these reasons, and in line with other authors, the dummy for euro cash changeover includes the period from July 1st, 2001 until

June 30th, 2002. Alternatively, whenever datasets do not cover post cash changeover period, I exclude the dummy and the data after July 1st, 2001.

A similar regression could be run to explain the other measure of price flexibility - the average size of price changes:

$$S_t = \alpha_0 + \alpha_1 Inflation_t + \alpha_2 Euro99_t + \nu_t \tag{3.7}$$

Here S_t is the average across products of the size of price changes S_{jt} in absolute terms, where S_{jt} is given by:

$$S_{jt} = \frac{\sum_{i=1}^{n_j} Y_{ijt} \, |lnP_{ijt} - lnP_{ijt-1}|}{\sum_{i=1}^{n_j} Y_{ijt}} \tag{3.8}$$

This is the measure of the size of price changes used by Dhyne et al. (2006). A potential drawback of this measure is that it does not include zero price changes, but since data are available in this format, I use it in my analysis.

When analyzing the frequency of price changes, a number between zero and one, a linear model is not always appropriate, thus I will follow Dhyne et al. (2006) in transforming the dependent variable in its log-odds ratio:

$$ln\frac{F}{(1-F)} \tag{3.9}$$

For robustness, several other estimation techniques are used by Dhyne at al. (2006) like the quasi-maximum likelihood (QML) approach, as well as Least Absolute Deviations (LAD). They report similar results.

I pre-test variables for unit root nonstationarity using the Augmented Dickey-Fuller (ADF) and Phillips-Perron (PP) tests. The tests (not reported here) reveal that the frequency, size, frequency of positive and negative changes for all countries are stationary. They also find that the inflation variable for some countries contains a unit root. I include the level of inflation, even if the tests indicate nonstationarity. The low power of the ADF test might have indicated the presence of a unit root, when actually there is none.

The main regression in its log-odds form becomes:

$$ln\frac{F_t}{(1-F_t)} = \beta_0 + \beta_1 Inflation_t + \beta_2 Euro99_t + \beta_3 Euro02_t + \varepsilon_t \qquad (3.10)$$

The effect of the euro on price flexibility is given by the partial derivative[7]:

$$\frac{\partial F_t}{\partial Euro99_t} = \beta_2 F(1-F) \qquad (3.11)$$

Thus, the effect will depend on the actual price flexibility. A common way to deal with this issue is to estimate the effect at the corresponding sample averages. Hence, I multiply the coefficient from the log-odds regression by $\bar{F}(1-\bar{F})$.

Some potential problems with the above approach include the relatively small time dimension of the datasets, which might not have all the "euro effects". Also, before joining the euro, countries spent several years in the Exchange Rate Mechanism (ERM) II

[7] Here I ignore the problems associated with the interpretation of β_2 as the percentage increase in the log-odds ratio, as discussed in Halvorsen and Palmquist (1980) and Kennedy (1981).

where they kept their currencies' fluctuations within narrow bands relative to each other. Thus even before the formal euro introduction, some of the price flexibility effects might have occurred which might bias the euro coefficient downwards. Another problem is that the theoretical thinking requires price changes to be in a certain direction, which I do not account for due to the limited scope of the data. And as far as a potential endogeneity problem between inflation and price changes is concerned, the dependent variable includes only a limited subset of the goods used to calculate inflation – 50 comparable product categories to address heterogeneity across countries. Those constitute a much smaller subset of CPI than reported in Table 3.1. Thus I neglect this potential problem.

4 Results

Tables 3.4 through 3.13 in Appendix C illustrate the detailed results based on three different estimation techniques - OLS using Newey - West standard errors (Newey) or the Cochrane Orcutt method (CORC), OLS with Log-odds as dependent variable and Least Absolute Deviations (LAD). They report different regression specifications, which take into account variables that one can reasonably expect to be correlated with the euro dummy and to influence price flexibility. Those include the size of price changes in the frequencies equation (Size) (as in Dhyne et al., 2006, p.188), the variability of inflation (Infl. var., measured as the predicted values of a GARCH (1,1) model), inflation persistence (Infl. Rho - measured as the autocorrelation function of inflation) and lagged inflation (Lag Infl.).

Theoretically, the size of price changes might be correlated with the euro introduction and the bigger the size of price changes, the lower the frequencies. Investigating this issue requires a structural model, but to check the intuition and in line with Dhyne et al. (2006), I include it in the regression analysis. Also, I run the equations as a Seemingly Unrelated Regression Equations (SURE) system (results not reported here), which does not change the findings. In particular, with SURE, frequency and size in the respective equation are negative and significant and the euro effects are somewhat bigger and more significant.

In addition, the euro might have influenced inflation in several ways - it might have caused inflation variability or inflation persistence to increase. If firms expect higher variability or more persistence, they might adjust the frequency or size of price changes more often. And it might be that firms respond to changes in inflation with some lag.

Table 3.2 provides a summary of the results for the effect of the euro on the frequency and size of price changes and Figure 3.3 provides information of how the magnitude of the effect depends on the number of data points after the euro introduction.

Table 3.2: Summary results for the euro effect on price flexibility

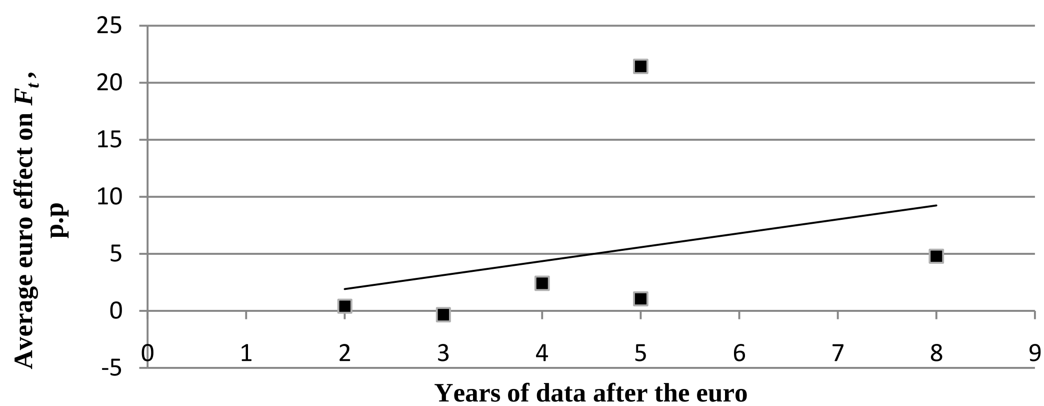
		Frequency OLS	Frequency Log- Odds	Frequency LAD	Size OLS
Austria	total	4.75**	5.07**	4.52**	0.13
Belgium	total	0.34	0.51	0.35	N/A
France	total	2.51**	2.63**	2.08**	N/A
Germany	increases	1.22	1.55**	1.06	1.12
	decreases	0.96**	0.96**	0.55	2.76**
Italy	energy	37.51**	42.24**	47.20**	N/A
	services	0.99	0.24	0.31	N/A
Spain	total	-0.19	-0.17	-0.29	0.00
	unprocess	-1.44	-1.45	-0.79	0.00
	process	0.32	0.24	0.63	0.00

Note: The Euro99 coefficients were averaged across regression specifications and reported in p.p. ** indicates significance at 5%.

The results show that the euro has increased price flexibility in Austria by about 5 p.p. or about 40%, an estimate that is highly significant across specifications. This is the highest euro effect of all countries, and coincidentally, it comes from the country for which data are available for the longest period after the euro, namely 8 years. This is in line with Figure 3.3 which confirms that the longer the country stayed in the euro, the bigger the effect of the euro is (even removing the outlier for Italy, a small positive trend is still present). The effect for Belgium is positive, around .4 p.p., but statistically and economically insignificant. This might be due to the fact that data for only two years after the euro were available for that country, so perhaps the effect has not manifested itself yet.

France's coefficient is about 2.5 p.p and significant, although there are only about 4 years of data after the euro introduction. For Germany, with about 5 years worth of post-euro data, the effect on price increases is about 1.5 p.p., but significant only with the log-odds specification. There is also evidence that the euro has increased the frequency of price decreases, by about 1 p.p. and significant in two out of the three specifications. This is the first country for which data on price decreases are available separately, and the evidence suggests that the euro, although not by much, does increase the frequency of price decreases.

Figure 3.3: Euro effect and data availability



For Italy, data are available for energy products and services only. As expected the euro has increased significantly the frequency of price changes for energy products by about 40 p.p. or about 100% and significant. That might be specific to the energy sector though, as there is no evidence that the euro has significantly increased the frequency of price adjustment in the services sector, although the effect is still positive of about .5 p.p. And Spain is the only country where the effect of the euro is not only insignificant for all specifications for prices in general, and for prices of unprocessed and processed goods, but it is also negative for the first two groups of about .25 p.p. and 1.5 p.p., respectively, while it is modestly positive for processed foods of about .4 p.p. This suggests that the euro might have decreased the frequency of price adjustment in Spain for all the prices in general, and for unprocessed foods in particular. In neither country is the size of price changes significantly influenced by the euro introduction.

Overall, the evidence suggests that, while more data from the period after the euro are needed to evaluate its effect, there seems to be a modest increase in the frequency of price adjustment in several euro countries. At the same time, there is no evidence that the euro has influenced the size of those adjustments in any country.

The findings are not inconsistent with the theoretical discussion above, which suggests strategic complementarity would bring a much more pronounced increase in price flexibility – for standard parameter values Devereux (2006) finds that the fraction of firms investing in price flexibility goes from 10% to 68% with a fixed exchange rate, while Senay and Sutherland (2005) show that the probability of not changing prices falls from 1 to around .5 with fixed exchange rates. The smaller effects of the euro might be due to the very few data points available after it was introduced.

While the main focus of the paper is on the effects summarized in Table 3.2, several other observations are also worth mentioning. Inflation does not seem to have a universal effect on price flexibility, with its effect being significant only in a small number of specifications and countries. This may be due to the inflation data being mostly on a year-on-year basis with significant overlaps. When data are on a month-on-month basis, i.e. for France, inflation turns significant and it increases the frequency of price changes by about 2-3 p.p. (see Table 3.6), which conforms to economic theory, that is, the higher inflation is, the more frequent the price adjustment.

Furthermore, none of the other measures for inflation, like inflation variability, inflation persistence or lagged inflation, seems to consistently show as a significant determinant of price changes. When any of them do, like inflation persistence for Belgium (see Table 3.5), it has the correct sign, i.e. the more persistent inflation becomes measured by its autocorrelation function, the more frequently the prices change. This reflects the theoretical idea that as shocks become more persistent, firms do change prices more often by, in the case of Belgium, about 3-4 p.p. Another example is inflation variance for France - it is significant and it shows that a one point increase in the variability of inflation, increases the frequency of price changes by about 12-13 p.p. Thus, as inflation variability increases, firms tend to change prices more often.

Another result is that the size in the frequency equation and the frequency in the size equation are a significant determinant of the dependent variable, i.e. when firms change prices by a larger size, this reduces the frequency of price changes, and when they change prices more often, they change them by smaller magnitude, which is what one would expect theoretically.

Finally, the effect of the euro cash changeover does not seem to consistently affect the frequency or size of price changes across the six countries.

5 Discussion

The evidence suggests that prices seem to become more flexible[8] with the euro. The effect is not very large, and is not significant for some countries, but it seems that the longer the country is part of the union, the bigger and more significant the effect of the euro is. This result suggests that one of the major disadvantages from fixed exchange rates or MUs - the lack of exchange rate adjustment to asymmetric shocks - might be weaker than commonly thought. A common currency could offer a new channel of adjustment, namely enhanced relative price flexibility at the same time as the relevance of flexible exchange rate to act as a shock absorber is questioned (Devereux, 2006; Devereux and Engel, 2003). Increased price flexibility also eliminates economic inefficiencies caused by sticky prices. And a common currency has also been found to increase business cycle co-movements, trade, income, growth and welfare (Frankel and Rose, 1998; Frankel and Rose, 2002; Frankel and Romer, 1999; Rose and van Wincoop, 2001).

The above evidence suggests a re-evaluation of the debate of fixed versus flexible exchange rates. Currently, flexible exchange rates and inflation targeting seem to be in fashion (see Mihov and Rose, 2008) – no country has been forced to abandon inflation targeting, and there seems to be only a few fixed exchange rate regimes (Hong Kong, Baltic states, Bulgaria) that have not collapsed (yet). However, a MU might substantially decrease the drawbacks of a fixed exchange rate regime by ruling out a speculative attack on one hand, and by increasing relative price adjustment on the other. It also might bring

[8] The results here refer to nominal price flexibility, since I am not able to distinguish between nominal and real rigidity.

in quantitatively important benefits in terms of higher trade, income and welfare. Thus if one compares flexible exchange rate (inflation targeting) with a fixed exchange rate (MU) it is not clear whether the net benefits from the former in terms of smoothing the business cycle outweigh the net benefits of the latter in terms of long run growth and prosperity. While the welfare losses from business cycles have been shown to be modest (i.e. Lucas, 2003), the welfare benefits from increased trade over the long term are quite large. For example, Bank of Canada reckons that the flexible exchange rate of the Canadian dollar has served Canada very well, as the Canadian economy is quite different than the US economy and thus needs different monetary policy, which is only achieved with a flexible exchange rate. However, are these benefits of smoothing business cycles bigger than the potential benefits for Canada of adopting the US dollar in terms of increased price flexibility, trade, income, growth and welfare?

But, even if the euro does increase price flexibility significantly both in a statistical and economic sense, it is still unclear whether increased price flexibility automatically means that prices will adjust efficiently to macroeconomic shocks, and will thus provide the benefit discussed above. A recent paper by Boivin, Giannoni and Mihov (2009) finds that while prices might be flexible in response to sector specific shocks and appear to adjust quickly, they can still be very sticky in the face of macroeconomic shocks, failing to provide efficient economic adjustment in aggregate. Therefore, while this paper presents some evidence that the euro has modestly increased price flexibility, it remains for future work to establish if there is significant euro effect on price flexibility over time and if this effect actually leads to more flexible prices in response to macroeconomic shocks and can thus serve as an adjustment mechanism.

6 Conclusion

Quantitative results from six euro member states - Austria, Belgium, France, Germany, Italy and Spain - show a small positive effect of the introduction of the euro on price flexibility, beyond the cash changeover effect. Using different regression techniques for robustness of the result, I find estimates of the euro effect ranging from a small negative to a statistically significant 5 p.p. or about 40%. The effect of the euro on the size of price changes seems to be neither statistically nor economically important.

The implications of this result for countries weighing the pros and cons of joining the euro (i.e. the former communist, now new EU member countries) are quite important: one of the main disadvantages of a MU - the inability to use discretionary monetary policy for domestic macroeconomic management - might be weaker than previously thought. Even though nominal exchange rate adjustment is inhibited, the advantage from a MU is that prices instead might become more flexible and might facilitate economic adjustment, with the added ”bonus” from a MU of more business cycle correlation (which obviates the need of nominal exchange rate adjustment in first place), increased trade, income and trend growth. This result also strengthens the argument for the endogenous fulfillment of the OCA criteria - even if a MU might not seem beneficial *ex ante*, it becomes optimal *ex post*.

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Appendices

Appendix A: Data Summary Statistics

Table 3.3: Data summary statistics

Country	Variable	Mean		Std. Dev.		Min.		Max.	
		Before	After	Before	After	Before	After	Before	After
Austria	F	12.03	17.25	2.60	4.72	8.73	9.03	21.94	38.99
	S	14.61	14.42	1.02	1.26	12.87	11.11	17.60	16.86
	Inf.	1.35	1.79	0.45	0.80	0.70	0.20	2.30	3.40
Belgium	F	14.98	15.37	3.35	1.74	7.33	12.27	34.16	18.42
	Inf.	2.32	1.47	0.82	0.61	0.66	0.65	4.28	2.74
France	F	18.17	21.07	3.25	3.34	13.46	15.76	27.12	31.56
	Inf.	0.10	0.16	0.19	0.25	-0.30	-0.39	0.72	0.68
Germany	F+	5.34	6.17	2.95	2.10	3.03	2.93	13.89	13.84
	F-	4.26	4.15	0.66	1.50	5.30	7.21	2.78	1.87
	S+	8.14	9.09	2.32	1.94	4.18	6.04	12.80	14.79
	S-	8.69	11.04	1.38	3.05	10.83	19.71	7.02	6.73
	Inf.	0.91	1.17	0.39	0.54	0.44	0.22	1.45	2.71
Italy	F-en.	43.93	81.93	20.19	13.32	18.88	42.67	92.49	97.88
	F-ser.	4.51	5.59	2.53	2.97	1.16	1.38	12.45	13.42
	Inf.-en.	-3.21	4.59	5.54	5.12	-13.27	-4.66	3.74	13.02
	Inf.-ser.	0.22	0.24	0.18	0.20	-0.12	-0.27	0.73	0.82
Spain	F	14.87	14.58	1.53	1.39	13.13	13.03	20.82	18.06
	S	8.94	8.60	0.48	0.38	8.22	8.14	9.83	9.30
	Inf.	3.53	3.09	1.30	0.78	1.50	1.87	5.12	4.14
	F-un	49.90	48.57	4.09	3.63	41.88	41.44	60.69	55.03
	S-un	14.87	15.11	1.26	1.49	12.32	11.80	17.92	17.33
	Inf.-un	0.22	0.38	1.29	0.98	-3.70	-1.80	3.10	2.40
	F-pro	17.81	18.27	2.68	3.65	14.07	13.14	29.13	29.55
	S-pro	7.49	7.16	0.72	0.45	5.97	6.50	9.00	7.94
	Inf.-pro	0.26	0.21	0.41	0.25	-0.40	-0.20	2.00	0.90

Note: F is the average frequency in %, +/- is for price increases/decreases. S is the average of the absolute value of size of price changes in %, as defined in (3.8), +/- is for price increases/decreases. Inf. is the average of monthly observations of year-on-year or month-to-month inflation. ”En” is energy goods, ”ser” is services, ”un” is unprocessed food and ”pro” is processed foods. The summary statistics after the euro introduction exclude the cash changeover period July 1st, 2001 - July 1st, 2002.

Appendix B: Time Plots

Figure 3.4: Time-series plot - Austria

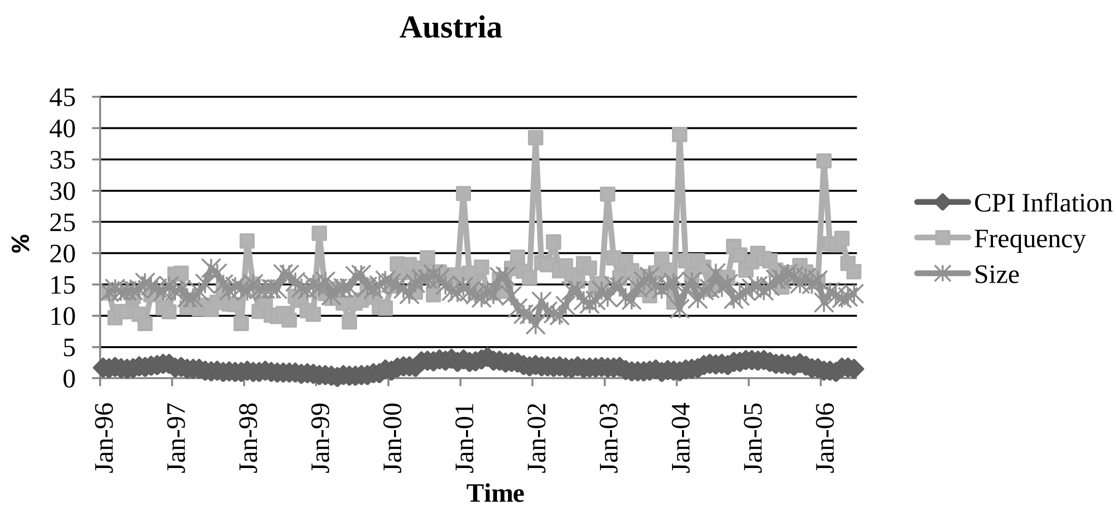


Figure 3.5: Time-series plot - Belgium

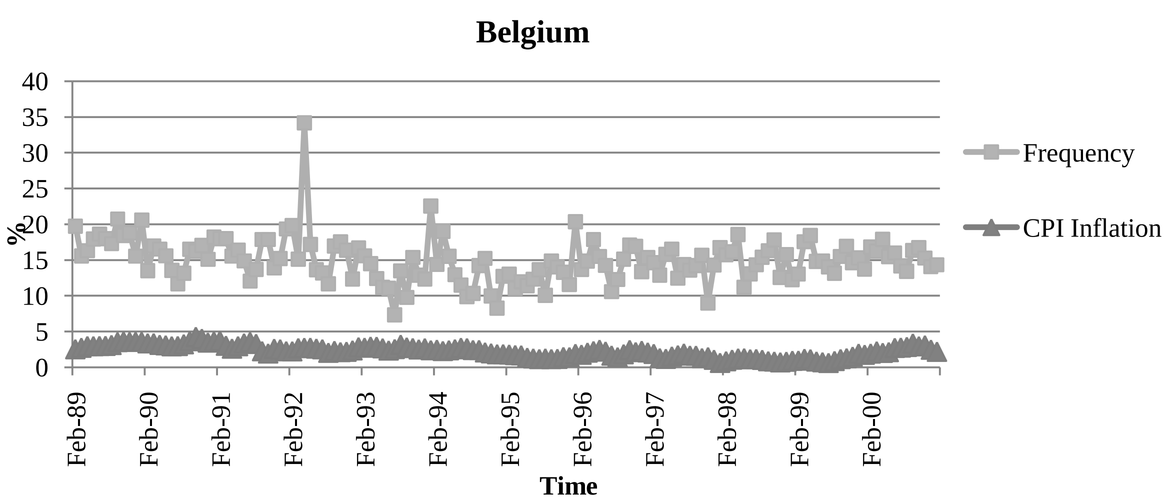
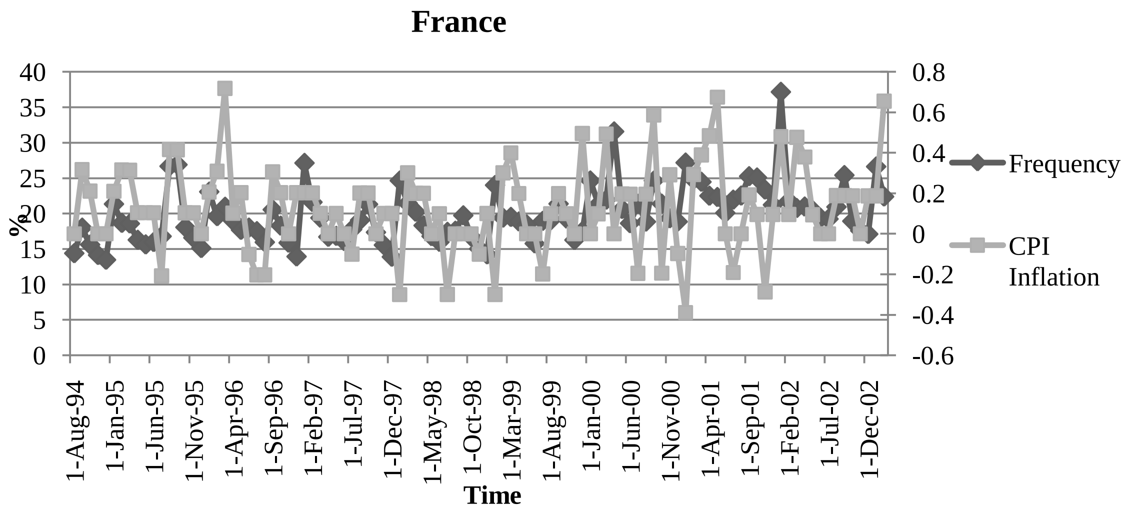


Figure 3.6: Time-series plot - France



Note: CPI inflation for France is measured on the right scale.

Figure 3.7: Time-series plot - Germany

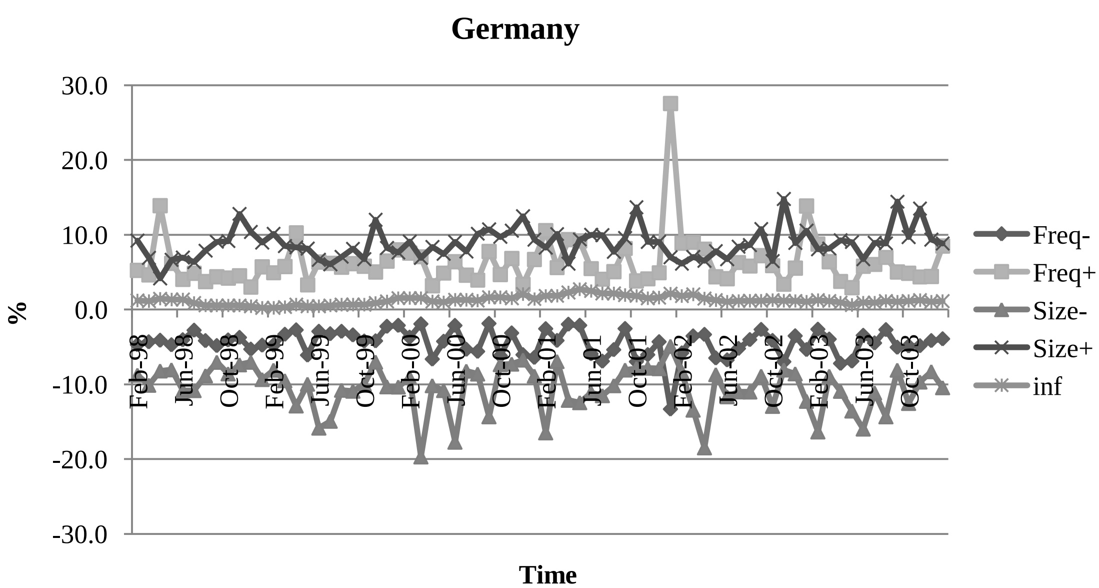
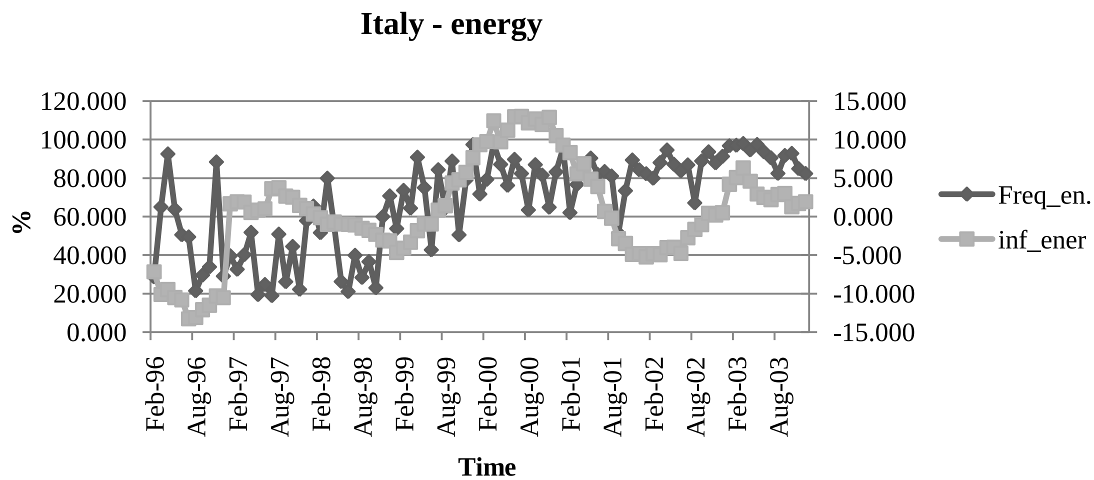


Figure 3.8: Time-series plot - Italy, energy



Note: CPI inflation for Italy is measured on the right scale.

Figure 3.9: Time-series plot - Italy, services

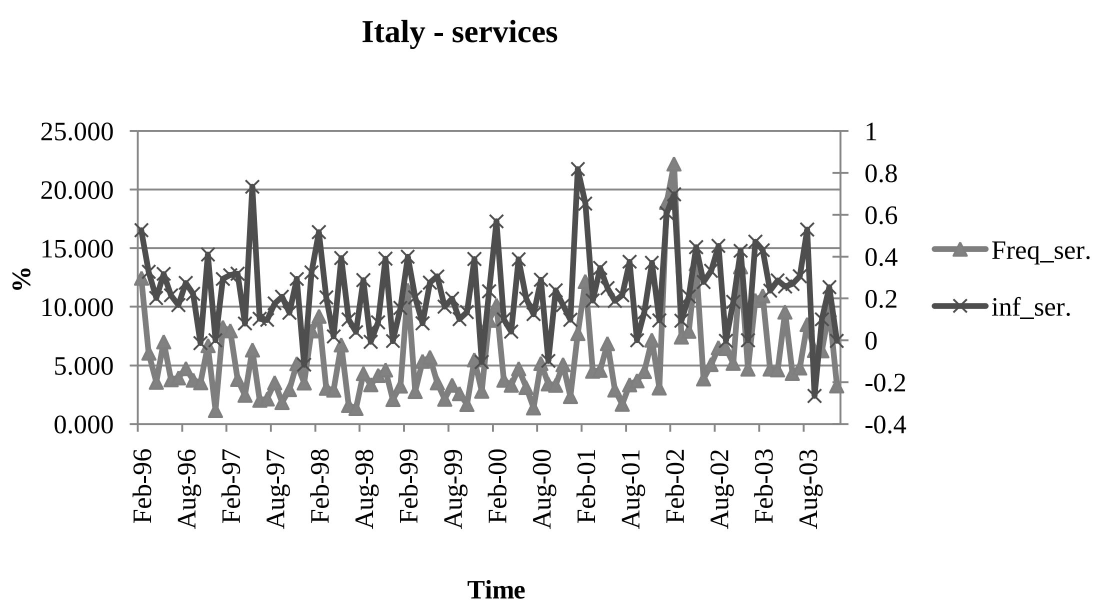


Figure 3.10: Time-series plot - Spain

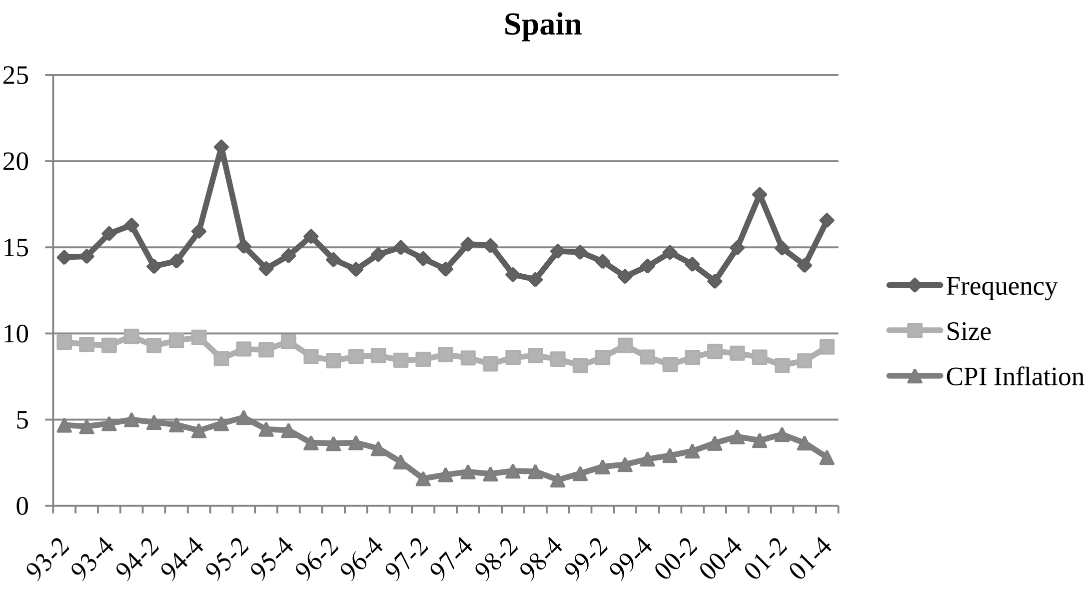
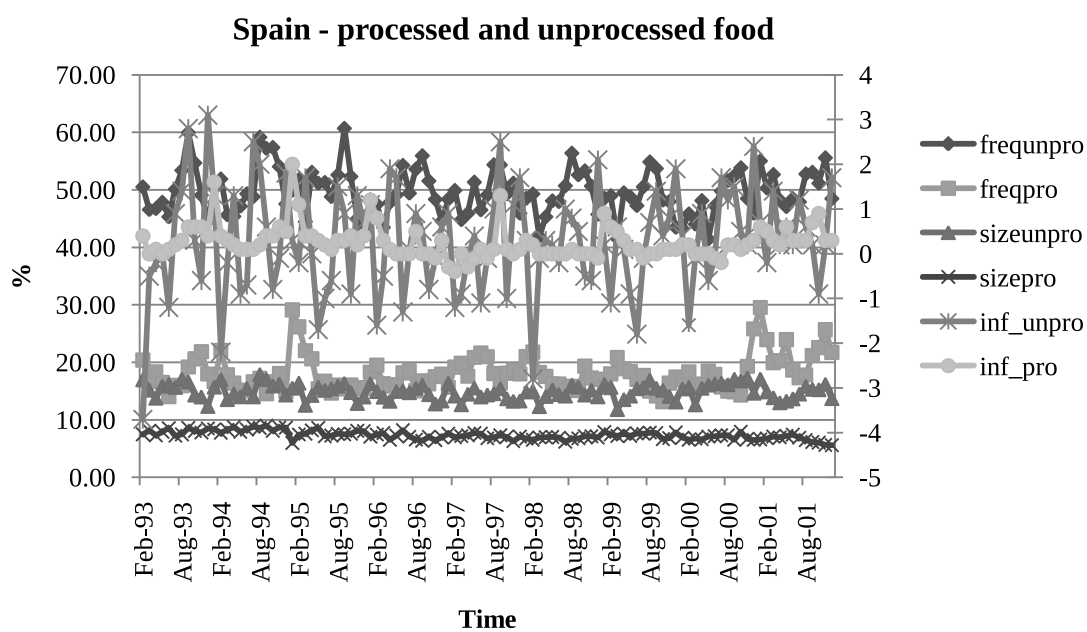


Figure 3.11: Time-series plot - Spain, processed and unprocessed food



Note: (Un) processed food inflation for Spain is measured on the right scale, in %.

Appendix C: Detailed Results

Table 3.4: Results for Austria

Flex. measure	Est. method	Indep. Var.									
Freq.	OLS – CORC	Inf.	0.62	0.67	0.72	1.06	1.40	1.18	0.99	0.87	1.02*
		Euro99	5.03***	4.65***	4.82***	4.71***	4.87***	4.58***	4.53***	4.64***	4.93***
		Euro02	1.67	-2.06	-2.18	-2.16	-2.76*	-2.42	-2.04	-2.50*	-2.84**
		Size		-	-	-	-	-	-	-	-
				1.55***	1.48***	1.49***	1.44***	1.55***	1.56***	1.54***	1.44***
		Infl. Var			-0.61	-0.65	-1.01				-1.01
		Lag infl.				-0.33	-0.41	-0.35	-0.31		
		Infl.					-2.57	-1.86		-2.08	-2.72
		Rho									
		R2 adj.	0.21	0.36	0.37	0.36	0.38	0.36	0.35	0.37	0.39
		DW	1.99	1.97	1.97	1.99	1.99	1.98	1.98	1.97	1.98
		RMSE	4.40	3.93	3.94	3.96	3.96	3.97	3.95	3.94	3.92
		N	124	124	124	123	122	122	123	123	122
	Log-odds	Inflation	0.06	0.06*	0.07*	0.09	0.11	0.09	0.08	0.08**	0.09**
		Euro99	0.40***	0.37***	0.39***	0.37***	0.39***	0.36***	0.36***	0.37***	0.39***
		Euro eff.	5.31	4.99	5.19	5.04	5.20	4.87	4.83	4.96	5.28
		Euro02	0.11	-0.12	-0.13	-0.13	-0.18*	-0.15	-0.12	-0.16	-0.19**
		Size		-	-	-	-	-	-	-	-
				0.10***	0.09***	0.09***	0.09***	0.10***	0.10***	0.09***	0.09***
		Infl. Var			-0.05	-0.06	0.09**				-0.09**
		Lag infl.				-0.02	-0.02	-0.02	-0.01		
		Infl.					-0.20*	-0.14		-0.17	-0.22**
		Rho									
		R2 adj.	0.28	0.41	0.43	0.42	0.45	0.41	0.40	0.42	0.47
		DW	1.98	1.95	1.96	1.98	1.98	1.97	1.98	1.96	1.97
		RMSE	0.28	0.25	0.25	0.25	0.25	0.25	0.25	0.25	0.25
		N	124	124	124	123	122	122	123	123	123
	LAD	Inflation	1.25**	1.43***	1.22***	1.58	1.59**	0.93	0.85	1.46***	1.38***
		Euro99	4.38***	4.28***	4.35***	4.36***	4.73***	4.43***	4.67***	4.78***	4.72***
		Euro02	0.65	-1.16	-0.84	-0.46	-0.99	-1.70	-1.08	-1.72	-0.95
		Size		-	-	-0.49**	-	-	-	-0.67**	-
				0.71***	0.56***		0.52***	0.71***	0.68***		0.50***
		Infl. Var			-1.14**	-1.06*	-				-
							1.29***				1.26***
		Lag infl.				-0.35	-0.25	0.75	0.52		
		Infl.					-	-2.21*		-1.80	-
		Rho	0.27	0.31	0.33	0.33	2.3***	0.32	0.30	0.31	2.23**
		R2 adj.					0.34				0.34
		N	125	125	125	124	123	123	124	124	124

Size	OLS	Inflation	0.05	0.13	0.07	0.24	0.20	0.28	0.31	0.11	0.04
		Euro99	-0.45	0.22	0.18	0.22	0.17	0.22	0.26	0.20	0.15
		Euro02	-1.30**	-0.68	-0.72	-0.68	-0.55	-0.54	-0.64	-0.61	-0.62
		Freq.		-0.12***	-0.12***	-0.12***	-0.12***	-0.12***	-0.12***	-0.12***	-0.12***
		Infl. Var			0.25	0.21	0.24				0.27
		Lag infl.				-0.27	-0.28	-0.33	-0.32		
		Infl. Rho					0.66	0.52		0.33	0.50
		R2 adj.	0.04	0.23	0.23	0.23	0.22	0.22	0.23	0.23	0.23
		DW	1.94	1.82	1.79	1.79	1.79	1.81	1.81	1.82	1.80
		RMSE	0.02	1.05	1.05	1.05	1.06	1.06	1.05	1.06	1.06
		N	124	124	124	123	122	122	123	123	123

Note: Dependent variables are Ft and St in %. Coefficients from log-odds for the euro are multiplied by $\bar{F}(1-\bar{F})$.Intercepts are not reported.

Table 3.5: Results for Belgium

Flex. Measure	Est. Method	Indep. Var.								
Freq.	OLS-CORC	Inflation	0.82**	0.80**	0.85	0.37	0.37	0.94	0.04	0.07
		Euro99	0.59	0.54	0.53	0.06	0.06	0.57	0.20	0.19
		Infl. Var		0.44	0.43	0.03				0.11
		Lag infl.			-0.05	-0.43	-0.44	-0.14		
		Infl. Rho				4.18***	4.22***		3.78***	3.66**
		R2 adj.	0.02	0.02	0.01	0.07	0.08	0.01	0.08	0.07
		DW	2.07	2.06	2.07	2.03	2.03	2.07	2.01	2.01
		RMSE	2.94	2.94	2.96	2.89	2.88	2.96	2.88	2.89
		N	143	143	142	141	141	142	142	142
	Log-odds	Inflation	0.06*	0.06*	0.07	0.03	0.03	0.08	0.00	0.00
		Euro99	0.06	0.06	0.05	0.02	0.02	0.06	0.03	0.03
		Euro eff.	0.76	0.71	0.69	0.23	0.22	0.72	0.37	0.36
		Infl. Var		0.04	0.04	0.01				0.02
		Lag infl.			-0.01	-0.04	-0.04	-0.02		
		Infl. Rho				0.33***	0.34***		0.30***	0.29**
		R2 adj.	0.02	0.02	0.01	0.07	0.08	0.01	0.08	0.07
		DW	2.08	2.07	2.07	2.04	2.04	2.08	2.02	2.02
		RMSE	0.22	0.22	0.22	0.22	0.22	0.22	0.22	0.22
		N	143	143	142	141	141	142	142	142
	LAD	Inflation	0.86**	0.75**	1.82	0.79	0.77	1.09	0.16	0.12
		Euro99	0.66	0.49	0.39	0.16	0.17	0.53	0.19	0.24
		Infl. Var		0.35	0.52	-0.01				0.02
		Lag infl.			-1.13	-0.78	-0.74	-0.25		
		Infl. Rho				3.74***	3.75***		3.67***	3.73***
		R2 adj.	0.04	0.05	0.05	0.10	0.10	0.04	0.11	0.11
		N	144	144	143	142	142	143	143	143

Note: Dependent variable is Ft in %. Coefficients from log-odds for the euro are multiplied by $\bar{F}(1-\bar{F})$.Intercepts are not reported. *** - significant at 1%, ** - significant at 5%, * - significant at 10%.

Table 3.6: Results for France

Flex. measure	Est. Method	Indep. Var								
Freq.	OLS-CORC	Inflation	0.03**	0.03*	0.02	0.03**	0.04**	0.03*	0.04**	0.03**
		Euro99	2.72***	2.38***	2.34***	2.34***	2.59***	2.66***	2.65***	2.39***
		Euro02	0.02	0.02**	0.02**	0.02*	0.02*	0.02*	0.02*	0.02*
		Infl. Var		0.12***	0.12**	0.13***				0.13***
		Lag infl.			0.02	0.02	0.03*	0.02		
		Infl. Rho				0.06*	0.07*		0.06*	0.05
		R2 adj.	0.20	0.25	0.24	0.25	0.21	0.19	0.21	0.26
		DW	1.98	1.96	1.93	1.89	1.93	1.96	1.96	1.92
		RMSE	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03
		N	102	102	101	100	100	101	101	101
	Log-odds	Inflation	0.18**	0.15*	0.15	0.21**	0.23**	0.18*	0.23**	0.21**
		Euro99	0.18***	0.16***	0.15***	0.15***	0.17***	0.17***	0.17***	0.16***
		Euro eff.	2.83	2.50	2.47	2.47	2.71	2.78	2.76	2.51
		Euro02	0.13*	0.14*	0.14*	0.13*	0.13*	0.13*	0.13*	0.13*
		Infl. Var		0.74***	0.71***	0.75***				0.78***
		Lag infl.			0.12	0.16*	0.17*	0.13		
		Infl. Rho				0.41*	0.46**		0.39*	0.33
		R2 adj.	0.21	0.26	0.25	0.26	0.22	0.20	0.22	0.27
		DW	1.97	1.95	1.91	1.87	1.91	1.94	1.95	1.91
		RMSE	0.20	0.19	0.19	0.19	0.20	0.20	0.20	0.19
		N	102	102	101	100	100	101	101	101
	LAD	Inflation	0.04***	0.03*	0.03**	0.04***	0.06***	0.04**	0.06***	0.04***
		Euro99	2.35***	1.98**	2.07***	1.92***	1.92***	2.41***	2.10***	1.91**
		Euro02	0.03***	0.03**	0.02**	0.03***	0.04***	0.03**	0.03***	0.03**
		Infl. Var		0.11**	0.11**	0.10**				0.11**
		Lag infl.			0.01	0.01	0.01	0.01		
		Infl. Rho				0.03	0.06***		0.06**	0.03
		R2 adj.	0.19	0.22	0.22	0.23	0.20	0.19	0.20	0.22
		N	103	103	102	101	101	102	102	102

Note: Dependent variable is Ft as a fraction. Euro99 coefficient is multiplied by 100 to convert to %. Coefficients from log-odds for the euro are multiplied by $\bar{F}(1-\bar{F})$.Intercepts are not reported. *** - significant at 1%, ** - significant at 5%, * - significant at 10%.

Table 3.7: Results for Italy - energy

Flex. measure	Est. method	Indep. Var.								
Freq.	OLS - CORC	Inflation	0.00	0.00	0.01	0.02	0.02	0.01	0.01	0.01
		Euro99	38.05***	38.09***	37.88***	37.05***	37.06***	37.85***	37.04***	37.03***
		Euro02	-0.01	-0.01	0.00	0.01	0.01	0.00	-0.01	-0.01
		Infl. Var		0.00	0.00	0.00				0.00
		Lag infl.			-0.01	-0.01	-0.01	-0.01		
		Infl. Rho				0.27**	0.26**		0.26**	0.27**
		R2 adj.	0.47	0.46	0.44	0.48	0.49	0.44	0.50	0.50
		DW	1.95	1.95	2.01	1.97	1.97	2.02	1.96	1.96
		RMSE	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16
		N	94	94	93	92	92	93	92	92
	Log-odds	Inflation	0.00	0.00	0.08	0.13*	0.12*	0.08	0.04	0.04
		Euro99	1.96***	1.96***	1.95***	1.93***	1.93***	1.95***	1.92***	1.92***
		Euro eff.	42.62	42.61	42.51	42.10	42.09	42.52	41.73	41.74
		Euro02	-0.17	-0.15	-0.03	-0.06	-0.05	-0.04	-0.13	-0.15
		Infl. Var		0.00	0.00	0.00				0.00
		Lag infl.			-0.08	-0.08	-0.08	-0.08		
		Infl. Rho				1.52*	1.48*		1.46*	1.50*
		R2 adj.	0.36	0.35	0.33	0.37	0.37	0.34	0.38	0.37
		DW	1.98	1.99	2.00	1.96	1.96	2.00	1.99	1.98
		RMSE	0.95	0.95	0.95	0.94	0.94	0.94	0.94	0.95
		N	94	94	93	92	92	93	92	92
	LAD	Inflation	0.00	0.00	0.00	0.01	0.01	0.00	0.00	0.00
		Euro99	45.94***	46.06***	44.71***	51.50***	51.10***	45.44***	46.25***	46.62***
		Euro02	-0.04	-0.04	-0.04	-0.07	-0.07	-0.04	-0.05	-0.07
		Infl. Var		0.00	0.00	0.00				0.00
		Lag infl.			0.00	0.00	0.00	0.00		
		Infl. Rho				0.27*	0.25*		0.14	0.19
		R2 adj.	0.41	0.41	0.40	0.41	0.41	0.40	0.41	0.41
		N	95	95	94	93	93	94	93	93

Note: Dependent variable is Ft as a fraction. Euro99 coefficient is multiplied by 100 to convert to %. Coefficients from log-odds for the euro are multiplied by $\bar{F}(1-\bar{F})$.Intercepts are not reported. *** - significant at 1%, ** - significant at 5%, * - significant at 10%.

Table 3.8: Results for Italy - services

Flex. measure	Est. method	Indep. Var.								
Freq.	OLS - CORC	Inflation	0.11***	0.10***	0.11***	0.10***	0.12***	0.11***	0.11***	0.10***
		Euro99	1.09	1.02	0.99	0.91	0.92	1.02	1.01	0.94
		Euro02	0.02	0.02	0.02	0.02	0.02	0.01	0.02	0.02
		Infl. Var		0.11**	0.11**	0.11**				0.12***
		Lag infl.			0.01	0.01	0.02	0.02		
		Infl. Rho				0.00	0.01		0.00	0.00
		R2 adj.	0.46	0.49	0.49	0.48	0.45	0.46	0.45	0.49
		DW	2.01	2.02	2.02	2.01	1.98	2.01	1.99	2.01
		RMSE	0.03	0.02	0.02	0.02	0.03	0.03	0.03	0.02
		N	94	94	93	92	92	93	92	92
	Log-odds	Inflation	1.97***	1.90***	1.87***	1.75***	1.89***	1.98***	1.91***	1.83***
		Euro99	0.24*	0.24*	0.24*	0.24	0.24	0.25*	0.24	0.23
		Euro eff.	1.27	1.23	1.26	1.25	1.26	1.28	1.24	1.20
		Euro02	0.08	0.07	0.08	0.10	0.09	0.07	0.09	0.09
		Infl. Var		1.18	1.25	1.42*				1.28
		Lag infl.			-0.07	-0.15	-0.03	0.02		
		Infl. Rho				-0.18	-0.13		-0.12	-0.13
		R2 adj.	0.47	0.47	0.47	0.46	0.45	0.46	0.46	0.47
		DW	2.01	2.02	2.02	2.02	2.00	2.01	2.00	2.01
		RMSE	0.45	0.44	0.45	0.45	0.45	0.45	0.45	0.45
		N	94	94	93	92	92	93	92	92
	LAD	Inflation	0.09***	0.10***	0.10***	0.10***	0.10***	0.09***	0.10***	0.10***
		Euro99	0.65	0.02	0.11	0.05	0.42	0.74	0.52	-0.05
		Euro02	0.01	0.01**	0.01**	0.01*	0.01	0.01	0.01	0.01*
		Infl. Var		0.25***	0.26***	0.26***				0.25***
		Lag infl.			-0.01	-0.01	0.01	0.00		
		Infl. Rho				0.00	-0.01		0.00	0.00
		R2 adj.	0.22	0.31	0.30	0.31	0.22	0.21	0.22	0.31
		N	95	95	94	93	93	94	93	93

Note: Dependent variable is Ft as a fraction. Euro99 coefficient is multiplied by 100 to convert to %. Coefficients from log-odds for the euro are multiplied by $\bar{F}(1-\bar{F})$.Intercepts are not reported. *** - significant at 1%, ** - significant at 5%, * - significant at 10%.

Table 3.9: Results for Germany - price increases

Flex. measure	Est. method	Indep. Var.								
Freq.	OLS - Newey	Inflation	0.02**	0.02**	0.02***	0.07***	0.07***	0.07**	0.07**	0.02**	0.02***
		Euro99	0.36	0.82*	0.83*	0.31	1.95**	1.95**	0.31	2.23**	2.21**
		Euro02	0.00	0.00	0.00	0.02	0.01	0.01	0.02	0.00	0.00
		Size		-0.01	-0.01***	0.00***	0.00***	0.00***	0.00***	-0.01***	-0.01***
		Infl. Var			0.01	0.00	0.00				0.01
		Lag infl.				-0.06**	-0.06**	-0.06**	-0.06**		
		Infl. Rho					0.02***	0.03***		0.02**	0.02**
		N	72	72	72	71	70	70	71	71	71
	Log-odds	Inflation	0.27***	0.29***	0.28***	0.86***	0.88***	0.87***	0.85***	0.29***	0.29***
		Euro99	0.12	0.20***	0.20**	0.14**	0.37***	0.37***	0.14**	0.41***	0.41***
		Euro eff.	0.72	1.15	1.15	0.84	2.20	2.20	0.85	2.40	2.39
		Euro02	-0.06	-0.13	-0.13	0.06	-0.02	-0.02	0.06	-0.19*	-0.19*
		Size		-0.08***	0.08***	-0.07***	-0.07***	-0.07***	-0.07***	-0.08***	-0.08***
		Infl. Var			0.02	-0.03	-0.03***				0.03
		Lag infl.				-0.69***	-0.70***	-0.69***	-0.68***		
		Infl. Rho					0.35	0.35***		0.318	0.31*
		N	72	72	72	71	70	70	71	71	71
	LAD	Inflation	0.01***	0.01***	0.01**	0.05***	0.05***	0.05***	0.05***	0.01***	0.01***
		Euro99	1.20**	1.01	1.20	0.62	1.50**	1.28	0.35	1.00	1.37
		Euro02	0.02***	-0.02***	-0.02**	0.00	-0.01	-0.01	-0.01	-0.02**	-0.02**
		Size		0.00***	0.00**	0.00***	0.00***	0.00***	0.00***	0.00**	0.00***
		Infl. Var			0.00	0.00	0.00				0.01
		Lag infl.				-0.04***	-0.05***	-0.05***	-0.04***		
		Infl. Rho					0.01	0.02		0.00	0.01
		R2 adj.	0.11	0.16	0.17	0.25	0.27	0.27	0.24	0.16	0.18
		N	72	72	72	71	70	70	71	71	71
Size	OLS - Newey	Inflation	0.20	0.58	0.6	0.86	0.91	0.97	0.92	0.60	0.57
		Euro99	0.89	0.96	0.97	1.04	1.29	1.30	1.03	1.30	1.29
		Euro02	-0.85	-0.77	0.76	-0.65	-0.73	-0.73	-0.65	-0.87	-0.86
		Freq.		-20.4***	-20.5***	-21.54**	-22.01**	-22.01**	-21.5***	-20.7***	-20.9***
		Infl.Var			0.14	0.15	0.15				0.14
		Lag infl.				-0.37	-0.41	-0.44	-0.40		
		Infl. Rho					0.39	0.41		0.47	0.47
		N	72	72	72	71	70	70	71	71	71

Note: Dependent variables are Ft as a fraction and St in %. Euro99 coefficient is multiplied by 100 to convert to %. Coefficients from log-odds for the euro are multiplied by $\bar{F}(1-\bar{F})$.Intercepts are not reported. *** - significant at 1%, ** - significant at 5%, * - significant at 10%.

Table 3.10: Results for Germany - price decreases

Flex. measure	Est. method	Indep. Var.									
Freq.	OLS - Newey	Inflation	0.00	0.00	0.00	-0.01	-0.01	-0.01	-0.01	0.00	0.00
		Euro99	-0.04	0.62**	0.62**	0.75***	1.49**	1.49**	0.74***	1.47**	1.47**
		Euro02	0.02***	0.02***	0.02***	0.01**	0.01**	0.01**	0.01**	0.01***	0.01***
		Size		0.00***	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***
		Infl. Var			0.00	0.00	0.00				0.00
		Lag infl.				0.01	0.01	0.01	0.01		
		Infl. Rho					0.01	0.01		0.01	0.01
		N	72	72	72	71	70	70	71	71	71
	Log-odds	Inflation	-0.12	-0.12*	-0.11*	-0.51**	-0.49**	-0.49*	-0.50**	-0.11*	-0.10*
		Euro99	-0.05	0.11	0.11	0.16**	0.39***	0.39***	0.15**	0.38**	0.38**
		Euro eff.	-0.21	0.48	0.48	0.67	1.66	1.66	0.66	1.63	1.64
		Euro02	0.43***	0.35***	0.35***	0.24***	0.16*	0.16*	0.24***	0.26***	0.26***
		Size		-0.07***	-0.07***	-0.07***	-0.07***	-0.07***	-0.07***	-0.07***	-0.07***
		Infl. Var			-0.03	0.01	0.00				-0.04
		Lag infl.				0.46*	0.45*	0.45*	0.46*		
		Infl. Rho					0.34*	0.34*		0.37*	0.37*
		N	72	72	72	71	70	70	71	71	71
	LAD	Inflation	-0.01*	0.00	0.00	-0.03***	0.03***	-0.03***	-0.02***	0.00	0.00
		Euro99	-0.28	0.24	0.42	0.30	0.92	0.84	0.31	0.99**	1.20
		Euro02	0.02***	0.01***	0.01**	0.01***	0.01*	0.01	0.01*	0.01***	0.01**
		Size		0.00***	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***
		Infl. Var			0.00	0.00	0.00				-0.01
		Lag infl.				0.03***	0.03***	0.03***	0.03***		
		Infl. Rho					0.01	0.01		0.01*	0.01
		R2 adj.	0.08	0.22	0.23	0.28	0.28	0.28	0.28	0.24	0.24
		N	72	72	72	71	70	70	71	71	71
Size	OLS - Newey	Inflation	0.04	-0.16	-0.11	-0.15	-0.08	-0.20	-0.25	-0.14	-0.08
		Euro99	2.34***	2.30***	2.30***	2.36***	3.29**	3.28**	2.37***	3.28**	3.29**
		Euro02	-1.16	0.51	0.50	0.50	0.21	0.21	0.50	0.22	0.20
		Freq.		-86.3***	-86.1***	-86.4***	-87.9***	-88.3***	-86.7***	-88.1***	-87.9***
		Infl. Var			-0.24	-0.22	-0.28				-0.28
		Lag infl.				0.04	-0.01	0.06	0.09		
		Infl. Rho					1.36	1.32		1.35	1.36
		N	72	72	72	71	70	70	71	71	71

Note: Dependent variables are Ft as a fraction and St in %. Euro99 coefficient is multiplied by 100 to convert to %. Coefficients from log-odds for the euro are multiplied by $\bar{F}(1-\bar{F})$.Intercepts are not reported. *** - significant at 1%, ** - significant at 5%, * - significant at 10%.

Table 3.11: Results for Spain - total

Flex. measure	Est. method	Indep. Var.									
Freq.	OLS-Newey	Inflation	0.00***	0.01**	0.01**	0.00	0.01	0.01	0.00	0.01**	0.01***
		Euro99	-0.01	-0.20	0.14	0.38	-0.26	-0.83	-0.13	-0.68	-0.14
		Size		-0.93	-1.08	-1.12	-1.64**	-1.67**	-0.91	-1.64**	-1.61**
		Infl. Var			0.00**	0.00	0.00				0.00**
		Lag infl.				0.01***	0.00	0.00	0.00		
		Infl. Rho					0.00	0.01		0.01	0.00
		N	35	35	35	34	33	33	34	34	34
	Log-odds	Inflation	0.03***	0.04**	0.05***	0.01	0.06	0.06	0.02	0.05***	0.07***
		Euro99	0.00	-0.01	0.01	0.03	-0.02	-0.06	-0.01	-0.05*	-0.01
		Euro eff.	-0.01	-0.18	0.13	0.39	-0.23	-0.76	-0.10	-0.65	-0.14
		Size		-6.65	-7.75	-8.17	-12.12**	-12.33**	-6.57	-12.03**	-11.86**
		Infl. Var			0.02***	0.03***	0.03				0.03**
		Lag infl.				0.05	0.00	-0.01	0.03		
		Infl. Rho					0.02	0.07		0.05	0.00
		N	35	35	35	34	33	33	34	34	34
	LAD	Inflation	0.00	0.00	0.00	-0.01	0.01	0.01	0.00	0.00	0.00**
		Euro99	0.27	-0.46	-0.39	0.60	-0.68	-1.02	-0.06	-0.51	-0.36
		Size		-1.08*	-0.90**	-0.53	-1.11**	-1.09	-0.81	-1.02**	-1.07***
		Infl. Var			0.00	0.00	0.00				0.00
		Lag infl.				0.01**	0.00	0.00	0.00		
		Infl.Rho					0.00	0.01		0.00	0.00
		R2 adj.	0.01	0.06	0.08	0.10	0.12	0.11	0.06	0.10	0.12
		N	35	35	35	34	33	33	34	34	34
Size	OLS	Inflation	0.00***	0.00***	0.00***	0.00	0.00*	0.00**	0.00	0.00***	0.00***
		Euro99	0.00	0.00*	0.00	0.00	0.00*	0.00***	0.00	0.00***	0.00*
		Freq.		-0.06	-0.07	-0.07	-0.09***	-0.09***	-0.06	-0.08***	-0.09***
		Infl. Var			0.00	0.00**	0.00				0.00
		Lag infl.				0.00	0.00	0.00	0.00	0.01***	
		Infl. Rho					0.01**	0.01***			0.00**
		N	35	35	35	34	33	33	34	34	34

Note: Dependent variables are Ft and St as fractions. Euro99 coefficient is multiplied by 100 to convert to %. Coefficients from log-odds for the euro are multiplied by $\overline{\boldsymbol{F}}(\mathbf{1}-\overline{\boldsymbol{F}})$.Intercepts are not reported. *** - significant at 1%, ** - significant at 5%, * - significant at 10%.

Table 3.12: Results for Spain - unprocessed food

Flex. measure	Est. method	Indep. Var.									
Freq.	OLS-Newey	Inflation	0.00	0.01**	0.00**	0.00*	0.00*	0.00*	0.00*	0.01**	0.00**
		Euro99	-0.90	-1.40	-1.46	-1.55	-1.61*	-1.56	-1.50	-1.44	-1.51
		Size		1.90***	1.91***	1.69***	1.73***	1.73***	1.69***	1.91***	1.92***
		Infl. Var			0.00	0.00	0.00				0.00
		Lag infl.				0.01***	0.01***	0.01***	0.01***		
		Infl. Rho					-0.03	-0.04		0.00	0.00
		N	107	107	107	106	105	105	106	106	106
	Log-odds	Inflation	0.01	0.02**	0.02**	0.02*	0.02*	0.02*	0.02*	0.02**	0.02**
		Euro99	-0.04	-0.06	-0.06	-0.06	-0.06*	-0.06*	-0.06	-0.06	-0.06
		Euro eff.	-0.91	-1.42	-1.47	-1.56	-1.62	-1.57	-1.51	-1.46	-1.52
		Size		7.64***	7.69***	6.81***	6.95***	6.96***	6.81***	7.69***	7.73***
		Infl. Var			0.00	0.00	0.00				0.00
		Lag infl.				0.03***	0.04***	0.04***	0.03***		
		Infl. Rho					-0.14	-0.14		0.00	0.00
		N	107	107	107	106	105	105	106	106	106
	LAD	Inflation	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
		Euro99	-0.64	0.09	-0.76	-1.20	-1.11*	-1.15*	-0.72	-0.68	-0.93
		Size		1.92***	1.96***	1.66***	1.53***	1.57***	1.77***	2.11***	2.07***
		Infl. Var			0.00	0.00	0.00				0.00
		Lag infl.				0.01**	0.02***	0.01***	0.01**		
		Infl. Rho					-0.09***	-0.08***		-0.02	-0.02
		R2 adj.	0.01	0.21	0.22	0.25	0.28	0.28	0.24	0.22	0.23
		N	107	107	107	106	105	105	106	106	106
Size	OLS	Inflation	0.00	0.00**	0.00**	0.00**	0.00***	0.00**	0.00***	0.00**	0.00**
		Euro99	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
		Freq.		0.21***	0.21***	0.19***	0.19***	0.19***	0.19***	0.20***	0.20***
		Infl. Var			0.00*	0.00	0.00				0.00
		Lag infl.				0.00	0.00	0.00	0.00		
		Infl. Rho					0.01	0.01		0.01**	0.01*
		N	107	107	107	106	105	105	106	106	106

Note: Dependent variables are Ft and St as fractions. Euro99 coefficient is multiplied by 100 to convert to %. Coefficients from log-odds for the euro are multiplied by $\bar{\boldsymbol{F}}(\mathbf{1}-\bar{\boldsymbol{F}})$.Intercepts are not reported. *** - significant at 1%, ** - significant at 5%, * - significant at 10%.

Table 3.13: Results for Spain - processed food

Flex. measure	Est. method	Indep. Var.									
Freq.	OLS-Newey	Inflation	0.03**	0.04***	0.04**	0.02	0.02	0.02**	0.02**	0.04***	0.04**
		Euro99	0.95	0.19	0.14	0.36	0.49	0.31	0.24	0.15	0.09
		Size		-1.51***	-1.55***	-1.50***	-1.39***	-1.51***	-1.57***	-1.51***	-1.55***
		Infl. Var			0.00	0.01	0.01				0.00
		Lag infl.				0.03***	0.03***	0.02***	0.02***		
		Infl. Rho					-0.02	-0.02		0.01	0.01
		N	107	107	107	106	105	105	106	106	106
	Log-odds	Inflation	0.21**	0.21***	0.24**	0.11	0.10	0.14**	0.14**	0.21***	0.24**
		Euro99	0.06	0.01	0.00	0.02	0.03	0.02	0.01	0.01	0.00
		Euro eff.	0.83	0.13	0.05	0.26	0.39	0.25	0.18	0.09	0.01
		Size		-9.52***	-9.90***	-9.55***	-8.84***	-9.45***	-9.90***	-9.45***	-9.89***
		Infl. Var			-0.04	0.04	0.05				-0.04
		Lag infl.				0.17***	0.18***	0.16***	0.16***		
		Infl. Rho					-0.14	-0.11		0.04	0.05
		N	107	107	107	106	105	105	106	106	106
	LAD	Inflation	0.03***	0.03***	0.03	0.00	-0.01	0.00	0.01	0.04***	0.03*
		Euro99	1.03	0.47	0.66	0.87	0.89	0.58	0.60	0.24	0.39
		Size		-1.02	-1.10*	-1.30***	-1.35***	-1.66***	-1.73***	-1.26*	-1.22
		Infl. Var			0.02	0.03***	0.03**				0.01
		Lag infl.				0.04***	0.04***	0.04***	0.04***		
		Infl. Rho					-0.01	0.00		0.03	0.03
		R2 adj.	0.05	0.10	0.11	0.16	0.15	0.14	0.15	0.10	0.10
		N	107	107	107	106	105	105	106	106	106
Size	OLS - Newey	Inflation	0.00	0.00**	0.01**	0.01**	0.01**	0.00	0.00	0.00**	0.01**
		Euro99	0.00**	0.00**	0.00**	0.00**	-0.01***	0.00**	0.00**	0.00**	0.00***
		Freq.		-0.09***	-0.09***	-0.09***	-0.08***	-0.09***	-0.10***	-0.08***	-0.08***
		Infl. Var			-0.01**	-0.01**	-0.01***				-0.01***
		Lag infl.				0.00	0.00	0.00	0.00***		
		Infl. Rho					0.02***	0.01**		0.01**	0.01**
		N	107	107	107	106	105	105	106	106	106

Note: Dependent variables are Ft and St as fractions. Euro99 coefficient is multiplied by 100 to convert to %. Coefficients from log-odds for the euro are multiplied by $\bar{F}(1-\bar{F})$.Intercepts are not reported. *** - significant at 1%, ** - significant at 5%, * - significant at 10%.