Comments on the paper "The single currency's effects on Eurozone sectoral trade: winners and losers?"

The paper estimates the impact of the introduction of the euro on intra-EA trade. Given the decisions on euro membership in new EU Member States as well as some old members (Den, Swe, UK), the trade benefit of the euro is of policy relevance. Not surprisingly, the issue has attracted a substantial amount of research. The current paper, nevertheless, contributes to the literature by presenting sector-specific and sector-country-specific estimates, while improving on the empirical model that is typically used. The authors study exports from 13 European countries to 23 industrialized countries for 25 2-digit ISIC sectors over 1988-2004. They use a dynamic panel model and estimate it by the GMM estimator of Blundell and Bond (1998). They conclude that (i) the magnitude of the sectoral euro trade effects is lower than in the existing literature, and (ii) the euro impact differs not only by sector, but also by country.

The focus of the paper is clear and it concerns an important and interesting topic. I personally appreciate the idea of the authors to examine where the positive euro effect using aggregate data comes from: is it concentrated in some sectors, or in some countries? From a methodological point of view, I find the allowance for dynamics in export equations interesting.

My main concern with the paper is that I cannot derive a clear and robust interpretation from the large number of estimates of the euro trade impact. In my view, obtaining such an interpretation requires a substantial amount of additional work. I have the following comments and suggestions for improvement:

Main comments:
1. The authors deviate from the existing literature in many directions: different sample, other level of disaggregation, other empirical specification, other estimation method. It is difficult to judge how these steps contribute to the finding that the euro effect is smaller than in other studies. Is the reason indeed the dynamics in their export model, as they claim? Is it the GMM estimation method? I suggest that the authors first try to replicate the existing estimates using a standard model and aggregate data (no dynamics, OLS estimation), and then step-by-step add dynamics, use GMM, and so on. Once the results for the aggregate data are robust, the authors can disaggregate the data.

2. The main value-added of the paper is claimed to be two-fold: (i) the magnitude of the sectoral euro trade effects is lower than in the existing literature, and (ii) the authors disaggregate not only by sector, but also by country. The authors suggest that result (i) is probably due to their modeling strategy (dynamic vs. static). However, in my view the estimates of the contemporaneous euro impacts from a dynamic model cannot be directly compared to the (full) effects estimated using a static model; see below for more details. Regarding (ii), I find the results for the sectoral disaggregation difficult to interpret, so that I do not grasp the outcomes for the even finer sector-country disaggregation. Overall, this makes it unclear to me how the analysis contributes to our economic understanding of trade flows.

3. Model (1) has one sector-specific explanatory variable, that is, value added. Sectoral exports, however, depend not only on income earned within that sector, but also on income earned in other

sectors. Moreover, exports in one sector can be input for producing export goods in another sector. The bottom line is that spillovers from other sectors are ignored. The authors could add nation-wide value added to obtain some preliminary insight into the relevance of this.

4. The EU dummy in model (1) is not sector specific. However, economic integration in Europe has evolved at different speeds across sectors. Using a sector-independent variable cannot control for that.

5. I miss the exchange rate level in model (1).

6. Before the authors can conclude that the euro estimates really vary across sectors (p.9), they should (i) verify that the variation is not just random. I guess a large number of confidence intervals for the euro dummy overlap, so that it is not clear whether the differences are systematic. Moreover, the authors should (ii) check whether the resulting outcomes are robust. Both issues obviously need to be solved before one can spend time on trying to find out what economic features drive the sectoral differences.

7. To investigate these driving forces, I suggest making the euro parameter depend on proxies for openness, comparative and competitive advantage, economies of scale, degree of competition, firm entry leading to new varieties, and so on. This would reduce the number of parameters and facilitate a structural interpretation.

Minor comments:
8. The estimates from a dynamic model are not directly comparable to those from the existing literature, which uses static models. Perhaps a cleaner comparison would be to take the long-run estimates from the dynamic model and compare these to those from the static model (I assume here that the authors currently use the estimates of the immediate/contemporaneous effect and compare these to the ones from the existing literature; if this is incorrect, the authors should make that clear in the paper).

9. The argument of leaving out "country-pair effects" (see p.5) is not convincing. Such effects are constant over time, so they do not "include the impact of the euro effect", because the euro dummy is time varying. I suggest adding "country-pair effects".

10. Including lagged exports presumably picks up a large number of omitted variables. For one, it captures bilateral variation in intercepts as long as country-pair intercepts are not included. Another omitted variable is the exchange rate. All this leads to (presumably upward) bias in the estimate of the lagged dependent variable. The authors stress the dynamics term in their model (which I agree with), but then they should try to estimate it more cleanly by controlling for other effects.

11. The authors refer to Anderson and van Wincoop (2003) to motivate the inclusion of exporter and importer effects so as to capture multilateral trade resistance. This is a nice motivation. However, the Anderson-van Wincoop setting is static, whereas the authors have panel data. It is not clear to what extent exporter and importer effects explain time-varying multilateral trade resistance.

12. I guess the authors use the term "EU countries" to refer to the "old EU countries", because the Czech Republic is an EU country nowadays (see p.7).

13. Dimension of data set: why does the number of exporting countries (13) differ from the number of importing countries (23)? Why not go for a symmetric approach?

14. Bilateral exports are in dollars (p.8). This is another reason to include an exchange rate term in model (1).

15. At the beginning of Section 5 the authors decide not to report all estimation results for all sectors; and I agree. However, somewhat more insight into the plausibility of the results would be preferable. For instance, the authors could provide the average estimate over sectors for each parameter, and provide some number supporting that the estimates "fit well". Furthermore, is the impact of mass around unity? How large is the estimate for lagged exports?