

Response to Referee Report 1

First, I would like to thank the reviewer for the useful comments, which will help to significantly improve the new version of the paper. Below, the remarks of the referee are reproduced in italics.

1. As the author rightly points, many authors and institutions have pointed out that the relative small size of firms might explain the lower amount of exports, as a share of output, in Spain. Hence, a policy recommendation is to remove the barriers that might preclude the expansion of firms in Spain. However, in Melitz-type models, firms success at export is determined by firms' productivity and not by size. In fact, the size of firms is a consequence of firms' productivity. If productivity is the key variable to raise exports, the author should explain why it is still important to focus on the size of firms when analyzing export performance.

There are several market failures (distortions in the tax system, financial constrains...) that are not in Melitz-type models and they affect the firm size for the same distribution of the firm productivity (Garicano et al., 2016). In my paper, the implicit assumption is that the firm productivity is a time invariant variable, so it is controlled with firm fixed effects. It could be a restrictive assumption, but the estimator for panel data with non-separable disturbance proposed by Powell (2015) has the disadvantage that it limits to one treatment variable (in addition to the firm and time fixed effects), while my main aim is the analysis of the relationship between firm size and exports, "which is often considered as a stylized fact" (Wagner, 2001, page 229).

2. Previous studies have pointed out that size explains both the extensive margin of trade (whether the firm exports or not) and the intensive margin of trade (the share of exports in total output). The author only analyzes the intensive margin of trade, and does not explain why the extensive margin is left out of the analysis. This is strange as many policies aimed to increase exports are focused on raising the number of firms that participate in foreign markets.

I agree with the referee's suggestion and I have tested that there is no selection bias estimating the Heckman selection model, that it includes a probit equation for the extensive margin. Unfortunately, I have included it in footnote 5 and I will include it in the body of the text in the new version of the paper.

3. Wagner (2006) already uses quantiles regressions to analyze the relationship between export intensity and size. This paper is not included in the references. If previous work has already addressed this topic, the author should explain the contribution of his paper.

Thank you for the comment. I will include this reference in the new version of the paper. My contributions are:

- a. I use quantile regression estimator for panel data models with firm fixed effects, and Wagner (2006) doesn't. In addition, in comparison with other models with additive

fixed effects, which are already cited in the paper, I use the Powell (2016) estimator for panel data models with non-additive fixed effects, that estimates the impact of exogenous treatment variable on the outcome distribution using within variation in the treatment variables.

- b. I use two dataset: i) The ESEE for the period 1990-2010 is an unbalanced panel of Spanish manufacturing that, in my subsample, includes 3249 firms and 23083 observations, and ii) the EFIGE is a cross-sectional dataset in seven European countries with 7807 firms in my subsample. This allows obtaining more precise estimates. Wagner (2006) uses only a Germany dataset with 458 firms.

4. The author does not make reference to papers that have analyzed the role of size on export status and performance of Spanish firms, neither in manufactures (Mañez et al., 2004; Fariñas and Martí-in-Marcos, 2007) nor in services (Minondo, 2013).

Sorry for the omission. I will also include these references that have analyzed this relationship in Spanish firms. Thank you.

5. The author makes reference to export propensity ratios in Table 1 before explaining how they are calculated. Without knowing the methodology followed by the author, the reader is surprised to find percentages over 100%. The author should also explain in more detail how the export propensity variable is calculated. An example would help the reader to understand the difference between the standard export propensity ratio and the normalized measure proposed by the author.

I agree. I will change the order of the sentences of last paragraph of page 5 (and first of page 6) to explain first how I have calculated the export propensity (percentage of exported sales measured as a percentage of the average value off export propensity in the 20 industries considered and for each 21 years included in the ESEE dataset, and in the 11 NACE-CLIO industries and 166 regions included in EFIGE dataset) and, after, I will show the ratios in Table 1. For example, if the average value in an industry and a year is 30% and a firm has an export propensity equal to 60%, then my measure of export propensity for this firm is 200%.

6. In many cases, Table 1 shows that the export-intensity of medium-size firms is larger than the export-intensity of large firms. The author should try to explain this result. Perhaps, large firms are productive enough so they use direct investment to reach foreign markets, reducing the need for exports.

Thank you, I agree with the potential explanation you provide and will include it in the revised paper.

7. As explained by Wagner (2001), the decision on how much a firm exports is not independent on the decision to export. In footnote 5 the author explains that there is no selection bias. This

result is important, so it would be convenient to present it in the text. It would be interesting as well to estimate a regression following the methodology proposed by Wagner (2001).

This relates to point 2 above. In line with the referee’s suggestion and as already indicated I will include this explanation. The most used alternative to Wagner methodology is the Heckman (1979) model. The results of the estimation of the second stage of Heckman model with ESEE dataset are shown in the next table, and I will include it in the new version of the paper.

Mean estimates of the elasticity of export propensity Heckman model. Second stage	
	ESEE
Number of employees	0.184 (2.64)
Lambda	0.061 (0.74)
Year fixed effects	Yes
Firm fixed effects	Yes
R ²	0.771
Firms	3249
Observations	23083

t-statistics are in brackets. The probit equation include number of employees, age of the firm, year fixed effects and industries fixed effects.

We can see that the inverse Mill’s ratio, Lambda variable, is not statistically significant, and the elasticity of export propensity estimated is very similar to OLS estimates.

8. It would be interesting to test whether the estimated quantile coefficients are statistically different.

Effectively, there are some coefficients at the bottom and at the top of distribution of export propensity in ESEE dataset that they are not statistically different, but the rest of coefficients are statistically different. I will include a statistical test in the new version of the paper.

9. It would be interesting to perform a numerical exercise showing how much Spanish exports would increase if firms raised their size along different quantiles

Thank you, I will do this exercise with the data set used.

10. *There is a typo in the first sentence of the introduction “emphasize this firm heterogeneity...”*

Thanks, I will fix the typo.

References

Garicano, L., LeLarge, C., and Van Reenen, J. (2016): Firm Size Distortions and the Productivity Distribution: Evidence from France. CEP Discussion Paper No 1128.