Dear Editor,

Thank you so much for your email regarding my submission with Ana Abeliansky, originally entitled "The relationship between the Chinese 'going out' strategy and international trade". We are delighted to have the opportunity to revise and resubmit our manuscript to Economics.

We would like to thank you and the referee for the many useful comments and suggestions, which are fully reflected in this new version. We find that, as a result, the paper has greatly improved.

In the submitted 'Response to referee's report' we describe the changes we made in response to each of the points raised by you and the two reviewer. We hope our revision meets your expectations and look forward to hearing from you.

Sincerely

Ana Abeliansky and Inma Martínez

Response to referee's report

We would like to thank the referee for the many useful comments and suggestions, which are fully reflected in this new version. Below we describe the changes we made in response to each of the points raised by you. We hope our revision meets your expectations and look forward to hearing from you.

The paper "The Relationship between the Chinese "Going Out" Strategy and International Trade" (by Ana Lucia Abeliansky and Inmaculada Martinez-Zarzoso) tests whether Chinese exports, imports, and outward foreign direct investment (FDI) are complements. Its innovation is that it considers all three economic flows in a single empirical framework. The focus on China is timely in light of China's rise in the global economy.

However, a couple of decisions would have deserved more explanations: Why does the paper analyze trade in both directions (imports and exports) but looks only at one direction (outward) in the case of FDI? Given their importance for the global economy, both outward and inward FDI flows deserve more attention in the literature as their trade linkages are poorly understood.

-The main motive is that by focusing on Chinese outward FDI and exports we are able to compare Chinese's internationalization strategies. The inclusion of China's imports in the analysis is mainly done to investigate whether China is profiting from trade complementarities in relation to its internationalization strategies.

2- The derivation of the hypotheses is very short. For example, I would have liked to learn why the authors expect that "China exports more to destinations where it is active in FDI." Why don't they expect as well that China imports more from destinations where the country is active in FDI? For example, China may invest to extract natural resources, which would be then registered as imports to China. Would it make sense to extend the hypothesis?

- Sure, it does make sense to extend the hypothesis and we will do so in the revision of the paper. Please refer to the revised manuscript, first paragraph in page 3.

3- In which units do you measure trade and investment? US dollars? Constant or current values? No information is given in the text.

- Trade and FDI are measured in current US dollars, the information has been added in the revised manuscript. See footnote 4 in the revised manuscript.

4- What do the variables colonial relationship, common legal origin, and common language mean in the Chinese case? More details would be helpful since China has never been (completely) colonized and it shares a common language with few countries.

- There are few cases for which these variables take the value of one; hence, we have added a comment on the revised paper to clarify this point. Please see footnotes 1, 2, and 3.

5- Why does the sample start in 2003 and end in 2012?

- The starting and ending years are due to data availability (bilateral FDI statistics).

6- Why does the analysis exclude country fixed effects? What are the consequences of this decision?

- We exclude country fixed effects to be able to estimate the coefficients of the variables that vary by destination/origin, since we have a one-side gravity. The consequence could be that we are not completely accounting for all the unobserved heterogeneity that is time invariant and country specific and it could be

correlated with the error term in the estimated model. We have estimated a feasible generalized least squares model with continental dummies and country RE allowing for panel specific autocorrelation and the results are robust (See Table below). When a fixed-effects regression was estimated the coefficients were inaccurately estimated, surely due to the lack of sufficient within-country variation over time.

	(1)	(2)	(3)
VARIABLES	Ln exports	Ln imports	Ln outward FDI
Lagged dum Nfdi	-0.0843***	-0.0621	
	(0.0248)	(0.0452)	
Lagged .lnoutward FDI	0.0359***	0.0280**	
	(0.00705)	(0.0131)	
Lagged ln exports		0.258***	0.147***
		(0.0340)	(0.0231)
Lagged ln imports	0.0286***		0.0212***
	(0.00594)		(0.00809)
Ln GDP	0.790***	1.148***	0.318***
	(0.0195)	(0.0457)	(0.0471)
Ln population	0.116***	0.126***	0.238***
	(0.0191)	(0.0443)	(0.0435)
Ln distance	-0.284***	0.682***	-0.220
	(0.0837)	(0.170)	(0.196)
Colony	0.956***	3.932***	3.406***
	(0.128)	(0.280)	(0.267)
Com legal origin	0.103**	0.842***	0.528***
	(0.0523)	(0.120)	(0.164)
Com language	1.638***	1.707***	2.308**
	(0.215)	(0.147)	(1.103)
RTA	-0.0417	0.260***	
	(0.0388)	(0.0724)	
BIT			0.204***
			(0.0790)
Constant	1.659*	-21.99***	-7.857***
	(0.864)	(1.778)	(1.952)
Observations	1,481	1,485	1,481
Number of countries	167	167	167

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

7- The statement that "Many have challenged the benefits of the Chinese investments in the local economies" (and similar statements). References would be helpful to guide the reader through the existing literature.

- Thanks for this comment. We have added the corresponding references following the statement. Please see page 16.

8-While I find the results interesting, I have two major concerns about the empirical approach and the interpretation of results: It is not clear to me why your instrumental variable (two-year lag of the endogenous variable) could satisfy the exclusion restriction. Most obviously, past investment could still affect today's trade patterns. This calls for explanations or an alternative causal estimation strategy. That said, I also do not understand how you can run a Hansen test if you have two endogenous variables and two instruments. Maybe I misunderstand your IV approach but fear that other readers might also need more explanations.

-The IV approach consists on using several lags of the endogenous variable and therefore we can run the Hansen test and test for the validity of the instruments. The results of the test indicate that the instruments are valid.

9- I am concerned about the paper's broader conclusion. The paper ends with the statement that "Chinese FDI is not that bad after all - despite being correlated to higher imports from China, it is also associated to higher exports to China." This is a bold claim. The increase in exports to China could be driven Chinese-owned firms that employ mainly Chinese laborers without significant benefits to the local economy.

- Thanks for this comment, we basically agree and will reformulate the conclusions accordingly. See page 16.

10. Finally, a couple of extensions would have increased the reader's knowledge gain. First, it would have been valuable to run separate regressions for services and manufacturing. At minimum, it should be clarified whether the trade and FDI data in the analysis both cover the service sector. Second, as the authors acknowledge in the conclusions, sector-specific regression would have helped the reader to learn more about the dynamics at play.

- We agree with your suggestion. Unfortunately we are using bilateral trade data for manufacturing from COMTRADE. The service sector is very heterogeneous and it is outside the scope of the paper to extend the analysis to this sector. We clarify this in the revised version of the manuscript and leave this issue for further research.

11. Finally, the paper contains a couple of typos (e.g., "seemly unrelated gravity equations" rather than "seemingly unrelated gravity equations"). The section "4.2 Main results" should start in a new line. De Sousa (2012), Head and Mayer (2014), as well as the exact sources of the data obtained from UNCTAD, COMTRADE and WDI are not listed in the bibliography. Johnston et al. (2015), which is listed in the bibliography, is not mentioned in the main text.

-Thanks for noting the typos, which are amended in the new version. Also the lacking references have been included.