RESPONSE TO THE INVITED READER COMMENT

First of all, we would like to thank the invited reader for his/her attention, comments and suggestions. Below we give detailed answers to the questions, comments a suggestions raised by the referee.

General comment: "This paper disentangles the effects of various elements of trade costs (transport costs, time, bureaucratic costs and tariffs) using a gravity equation specification as proposed by Baier and Bergstrand (2009) for cross-section industry-level data of the year 2000. The main finding is that time to trade, technological innovation and number of documents as proxy for trade facilitation are more important determinants in relative terms for trade flows than trade policy measures. Overall I think disentangling the various barriers to trade is a very fruitful area of research. I think the present discussion paper is well motivated. However, I have a couple of comments to the methodologies used and the empirical specification applied.

First, on page 9 the authors motivate that they use the first-order Taylor expansion of Baier and Bergstrand (2009) because this methodology is theoretically founded, simpler than the non-linear methodology suggested by Anderson and van Wincoop (2003) and accounts for asymmetric bilateral trade costs. I have a couple of problems with this motivation:

i) On page 9 the authors state: "The most commonly applied approach to estimate potentially unbiased gravity equation coefficients since Anderson and van Wincoop (2003) is to use region-specific fixed effects, as already suggested by the authors and by Feenstra (2004). Although this method is very simple and avoids the measurement error associated with measuring regions 'internal distances' (as in CNLS), it does not allow direct estimation of the comparative static effects of trade costs." I think this is wrong. You can do comparative statics after estimating the parameters with fixed effects. One has clearly to distinguish the estimation stage (which can include fixed effects) from the counterfactual analysis. One can use the parameter estimates from the fixed effects specification in a counterfactual analysis employing the underlying theoretical structure of the model."

Answer to 1.i):

Following the invited reader's comment, we have modified that sentence accordingly in the paper. A new paragraph has been added in page 9 of the revised paper:

"..Although this method is very simple and avoids the measurement error associated with measuring regions' "internal distances" (as in CNLS), it does not allow direct estimation of the comparative static effects of trade costs. However, an indirect estimation of those effects can be obtained after estimating the parameters with fixed effects. In this case, the estimation stage (which can include fixed effects) has to be distinguished from the counterfactual analysis. The parameter estimates from the fixed effects specification are used in a counterfactual analysis based on the underlying theoretical structure of the model."

ii) Again on page 9 the authors state: "Moreover, the Anderson and van Wincoop (2003) approach is only valid in a world with symmetrical bilateral trade costs (t_{ij} =t_{ji}), whereas the MR approximation terms also work under asymmetrical bilateral trade costs and, in reality, many trade costs are bilaterally asymmetric, such as tariff rates and transport costs."

This statement highly overstates the importance of the symmetric bilateral trade costs in the original contribution of Anderson and vanWincoop (2003). Actually, the Anderson and van Wincoop (2003) methodology can easily be adapted to account for asymmetries (see for example Anderson and Yotov (2010) or Bergstrand, Egger and Larch (2011)).

Answer to 1.ii):

Following the invited reader's comment, we included footnote 7 in the new version of the paper:

"Anderson and van Wincoop (2003) methodology can also be adapted to account for asymmetries (see for example, Anderson and Yotov (2010) or Bergstrand, Egger and Larch (2011)). Since Baier and Bergstrand (2009)'s approximation method is derived allowing for bilaterally asymmetric trade costs, this approach may generate lower biases in comparative statistics than the Anderson and van Wincoop (2003) method when trade costs are indeed asymmetric, as the latter method only addresses "average" border effects."

iii) Another statement on page 9 reads as follows: "Baier and Bergstrand (2009) suggest applying a first-order Taylor expansion to the explanatory variables and then using OLS to estimate the gravity model specified with the transformed variables. The focus in this paper is on estimation (not in comparative statics) and therefore the simple average weights (1/N) are used in the MR construction, instead of the GDP shares used as weights in Baier and Bergstrand (2009)." Given this statement, I am a little bit puzzled why the authors use the Baier and Bergstrand (2009) first-order Taylor expansion. My understanding of the paper of Baier and Bergstrand (2009) is that they wanted to suggest a simple methodology that allows for counterfactual analysis. If you do not focus on the counterfactual analysis, the easiest thing you may want to do is to use a fixed effects specification.

Answer to 1.iii):

We agree with the invited reader. A discussion about the convenience of using Baier and Bergstrand (2009) methodology is provided in the revised version in pages 9 and 10:

"…Baier and Bergstrand (2009) suggest applying a first-order Taylor expansion to the explanatory variables and then using OLS to estimate the gravity model specified with the transformed variables. It could be argued that a simple methodology that allows for counterfactual analysis is a fixed effects specification as proposed by Feenstra (2004). Yet Feenstra (2004)'s gravity specification has some drawbacks in the context of this investigation. Indeed, the use of country dummies or country-and-time dummies to control for the so-called multilateral resistance terms interferes with the inclusion of trade facilitation variables, such as the number of required documents to trade and the number of required days to trade in different countries, which do not usually change much over time (Martínez-Zarzoso and Márquez-Ramos, 2008). Indeed, the use of country fixed effects controls for all country-specific influences and does not permit estimating the impact of a change in trade frictions that are country specific. With regard to this last point, if we estimate the impact of tariffs on bilateral trade with the use of country-fixed effects, and then we control for multilateral trade resistance, the estimate tells us how much tariff barriers deter bilateral trade over the sample, but we would not be able to answer a question such as what is the impact of reducing the number of days required to trade on exports of different countries. To do that, we would need to allow for the resulting changes in multilateral trade resistance in every country, and then changes in the country-fixed effects should be admitted. Baier and Bergstrand (2009) methodology allows both to analyse the impact of exogenous changes and to engage in comparative static exercises. The focus in this paper is on estimation (not in comparative statics) and therefore simple average weights (1/N) are used in the MR construction, instead of the GDP shares used as weights in Baier and Bergstrand (2009)."

iv) Related to point iii): On page 10 the authors write: "Estimating equation (2) by OLS would yield identical coefficients to other estimates used to obtain unbiased gravity equation *coefficients (fixed effects and CNLS), although as with any linear approximation, an approximation error is introduced." Actually, the Baier and Bergstrand (2009) methodology is numerically identical to fixed effects estimation if you use a balanced sample, i.e. one that has the same importers and exporters and includes the x_{ii}-observations. But the CNLS is not. Coefficients will in general not be identical between CNLS and fixed effects.*

Answer to 1.iv):

Following the invited reader's comment, we have modified this paragraph accordingly and we also refer to his/her comment in footnote 10. For comparison purposes, a country-specific fixed-effects regression is added in the revised paper (see equation (3) and Table A.2 in Appendix I).

Second, the specification given in equation (2) raises the following questions:

i) Why does ln (Y_iY_j) appear on the right-hand side? Baier and Bergstrand (2009) include ln (Y_i) and ln (Y_j) as two distinct regressors. Including ln (Y_iY_j) restricts the parameters for income of the both countries to be the same. Additionally, from an econometric point of view including GDPs as regressors may be problematic as they are likely to be endogenous. This also follows from the methodology of Anderson and van Wincoop (2003), where (changes) in GDPs capture general equilibrium effects. This would be a further argument to use importer- and exporter-specific fixed effects, as they would control for GDPs.

Answer to 2.i):

It is right that including ln (YiYj) restricts the parameters for income of the both countries to be the same and that a more flexible specification should include separate regressor, but since this restriction does not affect estimates of tariffs and trade facilitation variables, we opted for a more parsimonious model, following Melitz (2008) who also includes income as a single regressor. Endogeneity issues are discussed in footnote 11, and equation (3) is specified and estimated using importer- and exporter-specific fixed effects, as suggested.

ii) Equation (2) includes ($lnTariffs_{ijk}$ –$lnTariffs_{PiPj}$) in exactly the same way as bilateral distance is included. However, tariffs generate revenues and do not consume any resources. This is different to standard iceberg trade costs which are by definition resource consuming. Hence, I am not sure whether taking the model in Baier and Bergstrand (2009) and extending it for tariffs (which they did not consider) and doing the log-linear first-order Taylor approximation would lead to exactly this term for tariffs. This is something that remains to be shown.

Answer to 2.ii):

The new version of the paper states that we have followed Melitz (2008) when doing this extension.

iii) The authors given no motivation why the easy-to-trade variables (technological innovation, internal transport costs, time and the number of documents required to trade) for the exporting and importing country ET_i and ET_j, respectively, enter the specification as ln (ET_iET_j). Why not additively? Or in some other functional form. Or as separate regressors? This issue pops up again on page 11 where the authors explain that "due to the complementarity of the ET variables considered, models 1-4 include each trade facilitation variable separately, namely technological innovation, transport costs, number of days and number of documents required to trade, respectively. In order to improve the measure of ET, we also computed an average ET that is calculated as the simple average of the variables:

$\sum_{m=1}^{3}\ln(x_i x_j)/3$, *where x denotes time, internal transport costs and number of documents."* *Why not add them as separate regressors? According to the bottom of Table 2 they are* *correlated, but not perfectly. Another question here is why you exclude technological innovation in the constructed average? And is there not the subscript m missing in* $\sum_{m=1}^{3}\ln(x_i x_j)/3$, *i.e. should it not be* $\sum_{m=1}^{3}\ln(x_{im} x_{jm})/3$ *?*

Answer to 2.iii):

We agree with the invited reader, and we explain in the revised paper why we include easy-to-trade variables as the log of the product of ET. The assumption behind is that the effect of trade facilitation variables is of equal magnitude for both the exporter and the importer countries. Technological innovation is excluded from the constructed average because the expected sign for the coefficient of this variable is the opposite of that obtained for the other trade facilitation variables. Whereas technological innovation is expected to have a positive effect on trade; documents, time and internal transport costs are expected to have a negative effect (footnote 13).

iv) You follow the methodology of Baier and Bergstrand (2009) which was developed for aggregate trade flows while using sector-level data. You just add subscripts k for the sector-level, but do not discuss whether the one-sector approach suggested by Baier and Bergstrand (2009) is still applicable if extended to multiple sectors. Actually, work by Anderson and Yotov (2010) suggests that extending the gravity equation to multiple sectors leads to a different specification, where bilateral trade at the sector level no longer depends on overall GDPs and overall multilateral resistance terms but rather on sectoral spendings and sector-specific multilateral resistance terms.

Answer to 2.iv):

Following the invited reader's comments, we add the following a paragraph in the section *Conclusions and policy implications:*

"…recent research suggests that extending the gravity equation to multiple sectors leads to a different model specification, where bilateral trade at sectoral level might depend on sector-specific multilateral resistance terms (Anderson and Yotov, 2010). Indeed, the cross sectional variation of multilateral resistance across regions (analysed for Canadian provinces in Anderson and Yotov, 2010) and commodities could be large. The importance of investigating whether the one-sector approach suggested by Baier and Bergstrand (2009) is still applicable to multiple sectors and intra-national trade flows is unquestionable. Since this research focuses exclusively on the quantification and comparison of the effect of policy and institutional trade barriers on international trade flows, although multilateral resistance may matter for estimation, it is arguably more important to derive comparative statics, and we leave this issue for further research."

3. Third, in the robustness analysis in section 6 the authors account for zero-trade flows using the two-stage estimation procedure suggested by Helpman, Melitz and Rubinstein (2008). Helpman, Melitz and Rubinstein (2008) use additional exclusion restrictions in their estimation as the non-linearity of the heterogeneity-bias term is insufficient to identify the model. I guess this is the motivation of the authors to write in footnote 17: "The selection equation should contain at least one variable that is not in the outcome equation, therefore colony; language and contiguity are not included in equation (4)."

However, one should keep in mind what Helpman, Melitz and Rubinstein also mention in their paper on page 466: "The key is for this variable to be correlated with the z_{ij}*'s but not be correlated with the residual of the second stage equation that has been estimated with the reliable excluded variables (the reliable excluded variables are believed to satisfy the exclusion* *restrictions on theoretical grounds). In our case this means that the residuals from the trade flow equation should be uncorrelated with this variable." However, colony, language and contiguity all turned out to be highly significant in the results given up to section 6. Hence, they are not candidates for excluded variables. I would suggest that either the authors come up with other, more plausible excluded variables or skip section 6 at all.*

Answer to 3):

We follow the invited reader's suggestion and skip section 6 from the new version of the paper.

Fourth, section 7 entitled "Simulations" presents some numbers based on the estimated elasticities. It is not clear to me why these numbers should be helpful. The authors tried to convince us that they base their empirical specification on a sound theoretical basis. But if this is the case, then the estimated elasticities on their own are not telling. Only if you undertake a proper counterfactual analysis the magnitudes can be meaningfully interpreted. For me this is the basic contribution of the Anderson and van Wincoop (2003) paper. Hence, I would suggest dropping section 7 and really stick to the estimation stage as claimed on page 9 by the authors.

Answer to 4):

We follow the invited reader's suggestion and skip section 7 from the new version of the paper.

References

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