**Answers to referee 1 (“SUMMARY - This paper uses data on unit price and transport costs to estimate the relative importance of the additive and multiplicative components of the transport costs…**

***We thank the referee for these insightful comments and suggestions which have helped us revise substantially the paper. Our answers to your questions and suggestions are given in bold.***

*Critique 1: Empirical Strategy*

*My first critique concerns the empirical approach chosen by the authors. Let me elaborate on this critique. (…) In my opinion this choice of strategy is quite sub-optimal, as (i) it relies on the strong assumptions highlighted above, (ii) it is computationally expensive as noted by the authors on multiple occasions, and (iii) it is subject to an endogeneity problem, which the authors disregard with one sentence, but which is rather detrimental in my opinion.*

**The three points raised by the referee are all equally important, and we answer separately to each of them below**

**(i) On the assumptions underlining our empirical approach.**

**Our main empirical equation and its underlying assumptions regarding the separability of transport costs between their country- and product-level components draw on the one proposed by Irarrazabal, Moxnes and Opromolla (*the Review of Economics and Statistics*, 2015) to estimate the share of additive costs in a firm-level context. It relies on a simple theoretical framework with minimal assumptions, and is compatible with most approaches within the so-called category of “New Trade Theories”. More importantly, this formulation allows us recovering explicitly the shares of, respectively, additive and multiplicative costs. Despite its formal elegance and simplicity, this is not possible in such a straightforward manner with the Hummels’ methodology.**

**Nevertheless, to take full account of the referee’s remark, we decided run an additional set of estimations based on the Hummel’s methodogy as proposed by the referee [to be completed with the additional estimates produced with equation A]. Reported in Appendix XX, Results are remarkably consistent with our main estimates etc.**

**(ii) On the use of non-linear least squares (NLS)**

**As highlighted by the referee, our estimated equation imposes to use non-linear estimation methods, such as Non-Linear Least Squares. However, even with another formulation, such as the one referred to as the Hummels methodology, we would still be constrained to resort to non-linear estimators. This is due to the restrictions imposed *ex ante* on parameters, i.e. *τ* ≥ 1 and *t* ≥ 0, the latter meaning simply we constrain both types of trade costs to be non-negative. Without these restrictions, standard linear, least squares estimates often deliver aberrant values with more than mild quality-of-fit [insert examples here]**

**We do acknowledge this is very important point was not made clear enough in the initial version, and did our best to make the pint clear in the revised version [see page XX : XX insert quote XX].**

**(iii) Endogeneity problem**

**Indeed, this is a very important point. The referee states that, based on theoretical insights by Melitz (*Econometrica*, 2003) or Baldwin and Harrigan (*AER*, 2011), 1/~pik is correlated in one direction on another with residuals εik, In other words, more productive firms and/or firms selling high-quality products will charge higher prices, all other things equal – in our case, for a given country-product pair.**

**We obviously do not question this conceptual issue. However, it is worth noting that a good deal of the bias (actually, the part relating to the quality effect) is going to appear identically in the CIF (p) and the FAS (~p) prices. Consequently, since our dependent variable is based on a ratio between the former and the latter, the (reverse causality) bias cancels out. That said, remains the possibility that bigger firms may impact transport costs, due to their ability of bargaining discounts for larger shipped volumes.**

**Following the referee’s advice, we decided therefore to provide some IV-estimates to provide a clean assessment of the size of the potential bias. XXX**

**XXX At some point, explain HS10 duty do not exist, contrary to what the referee asserts. At best, it was HS6 or HS8 XXX**

**The revised version now elaborates more on these considerations in the devoted section, page XX: [Insert quote here].**

*Critique 2: Calculation of unit prices*

**References**

Alfonso Irarrazabal & Andreas Moxnes & Luca David Opromolla, 2015. "The Tip of the Iceberg: A Quantitative Framework for Estimating Trade Costs," *The Review of Economics and Statistics*, MIT Press, vol. 97(4), pages 777-792, October.

ik, in this case, would rely on the across HS10 product and

district-of-entry variation in fik and pik. Estimating the above equation would

obviously require that the authors do not aggregate up the raw Census data

across all districts and all10-digit products pertaining to the same 5-digit category.

The first advantage of this so-called Hummel’s Approach is that the above

regression can be estimated separately for various country-industry pairs, without

imposing Assumptions (a) and (b), outlined above.1 The second advantage

is that there are a handful of previously-proposed instruments (e.g., HS10

product-specific tariff rates or lagged prices), which the authors can use to overcome

the endogeneity problem when estimating the above regression. The third

advantage, is that by adopting this approach, the comparison between the results

attained in this paper and those in Hummels (2007) would become more

transparent.

Now, I understand if the authors strictly prefer to maintain their current

approach; but they should address two issues either way:

1. The endogeneity problem: quoting Footnote 14 of the paper, the authors

are estimating ti and ts(k) as coefficients on the industry or country

dummies times 1=~pik. Namely,

ln

~pik

pik

􀀀 1

= ln

0

@

X

i

i1i

X

s(k)

s(k)1s(k) +

P

i ti1i +

P

s(k) ts(k)1s(k)

~pik

􀀀 1

1

A+ik

Based on the productivity-sorting model in Melitz (2003) or the qualitysorting

model in Baldwin and Harrigan (2010), 1=~pik is either positively

or negatively correlated with ik. So, the NNLS estimates are biased; and

the bias has nothing to with the casual versus accounting interpretation of

the estimates. Accordingly, the one-line justification the authors provide

to not address the endogeneity problem is far from convincing.

2. The aggregation problem: The original annual Census data reports

trade at the origin country-HS10 product-district level of aggregation,

whereas the authors are aggregating up the data even further to the origin

country-HS5 industry-year level. Such an aggregation comes with strong

1The authors do check the robustness of their results w.r.t. the separability assumption,

but this only done for a small sub-sample.

2

implicit assumptions and sacrifices a lot of useful variation in the data.

The authors are motivating the aggregation by stating that the problem

would become computationally expensive without it. But this reasoning

brings us back to my original point that the authors can use the Hummel’s

Methodology to circumvent the computational burden.