Referee report for "International Transport costs: New Findings from modelling additive costs" submitted to the Journal of Economic Geography.

This paper first decomposes transport costs into its respective ad-valorem and specific component. Second, it documents the evolution over time in these two components, finding a persistent downward trend in what the authors calls "pure" transport costs over time, even when taking account of trade decomposition effects.

I have two main concerns with the paper that I outline below.

1. **Implications/usefulness of the exercise**

While reading the paper, I kept asking myself why the exercise performed by the authors is interesting. Of course, it speaks to a large literature (generally well-referenced by the authors) that takes note of the importance transport costs not merely being ad-valorem (as implied by the popular iceberg assumption) but also specific. The fact that transport costs are both specific and ad-valorem is well known. Also, the theoretical implications of having specific, instead of ad-valorem (iceberg), transport cost when it comes to analyzing the consequences of further reductions in transport costs are by now quite well understood; see e.g. papers by Irarrazabal et al. (2015), Hornok and Koren (2015a,b), Alessandria et al. (2010), or Sorensen (2014). The main contribution of this paper thus appears to be to unravel the temporal evolution / relative importance of the ad-valorem and specific transport cost component. However, besides showing this evolution, the paper is basically silent on the implication(s) of the patterns found. In the conclusion, the authors state that this is left for future research. This was quite disappointing, simply documenting the evolution of these two components leaves the reader (at least me) wondering why he/she should care about this. The authors should aim to be much more clear on what the trends in ad-valorem / specific transport costs mean/imply from the perspective of these recent theoretical contributions/insights.

Related to this, it would be very helpful to further unravel the factors that are responsible for the observed temporal patterns in ad-valorem/specific transport costs. But, this is also left for future research by the authors. Again, this was quite disappointing as it left me wondering what is going on. At the very least the authors could/should make use of the fact that the temporal patterns in specific / ad-valorem transport costs is likely to show substantial heterogeneity across goods as well as countries. Exploring this heterogeneity to a fuller extent could already hint at the main drivers (insurance costs / containerization / economies of scale) behind the observed patterns. This could be done by e.g. not only reporting the mean and median of the ad-valorem / specific transport cost components but exploring more fully the heterogeneity in these transport costs components across sectors/exporters. Also, it should be relatively straightforward to provide some analysis of the major correlates with the identified specific and ad-valorem costs (e.g. product value/weight ratio, distance to the exporter, etc, etc).

In sum, the paper now merely documents, based on an empirical strategy that I comment on below, the temporal evolution of the specific and ad-valorem part of international transport costs. It should spend much more time trying to unravel the causes of the patterns found, as well as their consequences.

2. Empirical strategy

The empirical strategy used to unravel the specific and ad-valorem component in international transport costs is quite elaborate. It involves a non-linear estimation technique that precludes the authors from doing their analysis at their desired SITC 5-digit level. (see p.8). Also, it requires two subsequent estimation steps to "filter" out the pure transport cost component.

Both steps appear unnecessarily complicated to me. In fact, and in contrast to what the authors write in footnote 13, I think they could easily use one simple linear regression (using OLS including fixed effects) for their purposes. To be more specific, why not simply estimate equation (1) while plugging in equation (3) and (4) for τ and t respectively? Doing this yields the following estimation equation in levels (I have added a time subscript t as well as an additive error term ε):

$$p_{ikt} = \tau_{it}\tilde{p}_{ikt} + \tau_{kt}\tilde{p}_{ikt} + t_{it} + t_{kt} + \varepsilon_{ikt} \qquad (1)$$

Which can be easily estimated using the CIF and FAS price series in levels, including a full set of country-year fixed effects, a full set of sector-year fixed effects, as well as the FAS prices interacted with the same set of dummies. To be more specific, and using the authors' notation:

$$p_{ikt} = \sum_t \sum_i \alpha_{it}^\tau \mathbb{1}_{it}\tilde{p}_{ikt} + \sum_t \sum_k \alpha_{kt}^\tau \mathbb{1}_{kt}\tilde{p}_{ikt} + \sum_t \sum_i \alpha_{it}^t \mathbb{1}_{it} + \sum_t \sum_k \alpha_{kt}^t \mathbb{1}_{kt} + \varepsilon_{ikt} \qquad (2)$$

I do not see why the authors need the complicated non-linear transformations to do what they aim for. Estimating (2) should be relatively easy, for one it is linear, but it would also not require the inclusion of any country-sector fixed effects.

Based on (a variant of) a linear regression like the one outlined above in (2) the authors should be able to do all their analyses, both those in section 2 as well as those in section 3. Unless I am wrong here, I would urge the authors to make use of this simpler linear specification. In my view, there is nothing in their analysis that requires transforming (1) into the more complicated non-linear specification that they now estimate.