International Transport costs: New Findings from modeling additive costs
Answer to Referee 1

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We would like to thank you for your various insightful comments. They led us to introduce some significant changes to the paper that we hope address your concerns. We first give you an overview of the revision (Section1) before answering in detail each of your comments (Section2). Number of sections, pages and equations mentioned in the text refer to the revised version.

1 Main changes

- Based on your comment 1.2 (*The aggregation problem*), the benchmark set-up now retains $s = 3$ and $k = 10$ for the industry/product aggregation levels respectively. It is true that the original annual Census data reports trade at the origin country-HS 10 product-district level of aggregation. Our initial choice of retaining $s = 3$ at the sectoral level, $k = 5$ at the product level was driven by the use of Hummel's dataset available over 1974-2004 with $k = 5$ the finest degree of aggregation. We yet agree with the referee that jumping from $k = 10$ to $k = 5$ digits at the product level is detrimental to our ability to exploit useful variation in the data. In line with the referee's comment, we now run the estimation by considering k at the HS10 classification level, not 5. **We come back to this point in Section XXX of the letter.**

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This has important consequences in terms of database. In the submitted version, we used the original Hummel's database (available on his webpage from 1974 to 2003, which we completed by the US Bureau of census import database over the period 2005-2013. In Hummels' (2007) database, which we use until 2004, the finest degree of classification is at the 5-degree level until 2004. For comparability reasons over time, we have adopted the same degree of classification $k = 5$ for the following years (based on the original Census dataset). As a result, in the submitted version, transport costs are estimated at the 3-digit sector level as benchmark estimate, exploiting product heterogeneity at the 5-digit level (for a given origin country, and conditional on year and transport mode).

Switching to $k = 10$ as benchmark disaggregation level for the product dimension requires to resort on US Census dataset only. As first step, we have re-run the estimation over the years for which we already had the data (2005-2013). In parallel, we have bought older years from the Census bureau, as well as the more recent years 2014-2019. However, the Census bureau can provide data disaggregated at the $k = 10$ product level only starting in 1989. This deprives us from the very first years 1974-1988. For this period, we can only resort on Hummel's dataset, with $k = 5$ the finest product level category. This notably induces us to maintain $s = 3$ as the benchmark disaggregation level for the sectoral dimension for the whole period. This allows us to preserve (as far as possible) the largest time coverage - even though switching from $s = 3, k = 5$ (1974-1988) to $s = 3, k = 10$) (1989-2019) undoubtedly constitutes a significant break. **What do we do with that?**

- Based on your comment 1.1 (*The endogeneity problem*), the benchmark regression (non-linear least squares) is now completed by a two-stage procedure, where the fas price is instrumented by tax duties and its own lag - see section 4.3 and appendix B.4 in the paper, and section 2.1.2 of this letter.

- Based on your main comment 1, the paper has been rewritten so as to be more in line with Hummel's (2004) methodology. Specifically, we now start from the share of additive costs β, from which we can deduce the additive / ad-valorem components (t, τ) rather than the other way round (as in the submitted version). We thank the referee for pointing this out, as this contributes to a more straightforward and intuitive understanding of our estimation method.

- What about the big picture?

We now answer in detail each of your comments given in italics.

2 Detailed answers

2.1 Critique 1: Empirical Strategy

Using the notation of the authors, they are intreated in identifying the share of the specific cost in the total transport cost. Namely,

$$\frac{\frac{t_{is(k)}}{\tilde{p}_{ik}}}{\tau_{is(k)} - 1 + \frac{t_{is(k)}}{\tilde{p}_{is(k)}}}$$

$$\text{or,} \quad \frac{t_{is(k)}}{(\tau_{is(k)} - 1)\tilde{p}_{is(k)} + t_{is(k)}}$$

The way they approach the problem is that they assume that (a) $\tau_{ik} = \tau_i \tau_k$, (b) $t_{ik} = t_i + t_k$, and (c) t_k and τ_k are uniform across products within industry s. After imposing these assumptions, they estimate the following specification

$$\ln\left(\frac{p_{ik}}{\widetilde{p}_{ik}} - 1\right) = \ln\left(\tau_i \times \tau_{s(k)} - 1 + \frac{t_i + t_{s(k)}}{\widetilde{p}_{ik}}\right) + \epsilon_{ik} \tag{1}$$

in which τ_i, $\tau_{s(k)}$, t_i, and $t_{s(k)}$ are identified as fixed effects coefficients. 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.

Our answer:

The three points raised by the referee indeed deserve careful consideration. We provide separate answers to each of them below, starting with points (i) and (iii), and finishing with point (ii).

2.1.1 On the justifications for our empirical approach

About the key assumption of separability 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 in 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".

Further, we provide a robustness check for this separative assumption, in Section 4.1 of the paper (**Table 2, previously submitted version and revised version**

XXX CHECK AT THE END XXX). We check the robustness of our results by re-running the estimation without the separability assumption. This comes at the cost of a substantial increase in the number of fixed effects to include in the regression. Based on the numbers of countries and sectors reported in **Table 1 of the submitted version and revised version XXX CHECK AT THE ENDXXX** (188 countries, 230 3-digit sectors), it would mean including 86,480 fixed effects ($188 \times 230 \times 2$ for the two additive and multiplicative costs) rather than "only" 836 ($(188 + 230) \times 2$). This is computationally not tractable. For this reason, we have decided to run the robustness check on a reduced sample. If it is smaller in terms of observations (2,381 for Air, 2,798 for Vessel on average over the period 1974-2013, vs around 29,000 on the complete sample, see Table 1), it yet remains quite large in terms of trade coverage, as the selected countries and sectors (at the 3-digit level) constitute 80% of the total value of flows (on a yearly basis). As we conclude at the end of Section 4.1, whatever the transport mode and for both types of transport costs, the trend patterns of international transport costs are very similar whether estimated under the separability assumption or not - see in particular Figures 6 and 7 on pp. 25-26. Given that the restricted sample covers 80% of the total trade flows of the US economy, we hope to convince you that this result can be extended to the whole dataset.

On the use of non-linear least squares (NLS) The referee writes *A more natural approach is what the authors, at some point, refer to as the Hummel's Methodology. That is, one can alternatively estimate the share of the additive component as:*

$$\frac{t_{ik}}{(\tau_{ik} - 1)\tilde{p}_{ik} + t_{ik}} = \beta_{ik}$$

where β_{ik} is the elasticity of transport costs w.r.t. unit price in absolute value, as shown with more details in Appendix A.2 through Equation (7). *Given the authors' objective and the data they are using, β can be separately estimated for each industry-country pair using the following regression:*

$$\ln f_{ikd} = \beta_{is(k)} \ln \tilde{p}_{ikd} + \text{Controls}_{ikd} + \epsilon_{ikd} \tag{2}$$

where d denotes the US district of entry and k denotes an HS10 product (f_{ikd} being the transport costs). The identification of β_{ik}, in this case, would rely on the across HS10 product and district-of-entry variation in f_{ik} and p_{ik}. Estimating the above equation would obviously require that the authors do not aggregate up the raw Census data across all districts and all 10-digit products pertaining to the same 5-digit category.

4

Our answer: As highlighted by the referee, our estimated equation requires non-linear estimation methods, such as Non-Linear Least Squares. However, even with another formulation, such as the one suggested by the referee in Equation (2), we would still be constrained to resort to non-linear estimators. This is due to the necessity of imposing an *ex-ante* restriction on parameters, i.e. $\tau \geq 1$ and $t \geq 0$, or $0 \leq \beta \leq 1$. Should we relax these restrictions, standard linear, least squares estimates often deliver negative, meaningless estimates. In this respect, implementing the referee's method (see below) does not suppress the requirement of resorting on non-linear estimates (and the computational, time-consuming burden it induces). Imposing this parameters constraint was not made clear enough in the initial version, and we did our best to make this very important justification clearer in the revised version - see footnote 16XXX page 8: "One may object that we could preserve the estimation at the 5-digit level by running an OLS estimation on the equation taken in level, i.e.'on the basis of Equation (**??**), specifying the error term additively. This would not solve the problem though, for three main complementary reasons. First, at the 5-digit level the number of fixed effects to include in the estimation would be more than 430,000 (with 216 countries and 2,029 products), making the estimation computationally extremely burdensome, even in OLS. Imposing Equations (**??**) and (**??**) to reduce the number of fixed effects would drive us back to the non-linearity issue for fixed effects. Second, making the error term (specified as following a normal law centered on 0) enter Equation (**??**) additively implies negative values for some of the estimated residuals $\widehat{\epsilon}_{ik}$, which is inconsistent with the constraint that $\frac{p_{ik}}{\tilde{p}_{ik}} > 1$. Third, estimating the equation in level does not eliminate the fact that it is non-linear by nature, as long as there are additive costs to estimate (see Equation (**??**) for $t_{ik} \neq 0$)"

Of course, this does not prevent us for taking full account of your suggestion to adopt an alternative estimation method (summarized in Equation (2). It turns out that this method does not go without a few important limitations, that justify maintaining our initial estimation strategy as the baseline in the building block of the paper.**The referee's method is presented in the online appendix XXX WHERE??? XXX, and we refer to it in Footnote XXX of the paper**. We develop the analysis we held below.

Exploring Referee's alternative functional form The estimation strategy suggested by the referee starts from Equation (7) linking transport costs and unit prices as assumed in Hummels (2007). As shown in Subsection (**??**), the elasticity of transport costs to unit prices β_{ik} (in absolute value) also corresponds to the share of additive costs in total transport costs. The share of additive costs can hence be uncovered by regressing transport costs on unit (ie, fas) prices. Specifically, the referee suggests to run the estimation based on Equation (2).

Precisely, by year and transport mode, this implies running the estimation for each country of origin i and each sector $s(k) = 3$ digit sector, exploiting the variability between sub-sectors at the 10-digit level (k) and between ports of entry in the US (d). We have implemented the referee's method over the years for which we already have the data, i.e. the years 2005-2013 (already used in the submitted version).

Notice that we are aware of the referee's suggestion to run his/her estimation strategy considering sectoral aggregation at the 5-digit level (with $k = 10$). We yet keep considering sectors at the 3-digit aggregation level, in view of being able to compare the pros and cons of the referee's method versus ours, including those years for which the finest disaggregation level is $k = 5$ (i.e., the original Hummel's dataset **over 1974-1988, as the US Bureau of census import database starts in 1989.**)

Despite its interest, implementing the referee's method uncovered some drawbacks, that in our view, outweighs those of our method (in particular associated with *Assumptions (a) $\tau_{ik} = \tau_i \tau_k$, and (b) $t_{ik} = t_i + t_k$.*) We develop our results and conclusion by answering each argument made by the referee at a time.

Our answer can be articulated in two steps. First, we challenge the advantages that the referee's estimation method would bring in comparison with our's. Second, we emphasize the drawbacks induced by this method. XXX HERE XXX

a) The advantages of the referee's method are not as important as suggested

- the referee writes *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) $\tau_{ik} = \tau_i \tau_k$, and (b) $t_{ik} = t_i + t_k$.*

Our answer:

First, the separability assumption does not seem to be a strong assumption. This is based on the conclusion drawn from the robustness check on a reduced sample (in the submitted version, considering $s = 3, k = 5$, and in the revised version, considering $s = 3, k = 10$). If it is smaller in terms of observations,[1] it yet remains quite large in terms of trade coverage, as the selected countries and sectors (at the 3-digit level) constitute 80% of the total value of flows (on a yearly basis). As we conclude at the end of Section XXX, whatever

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[1] In the restricted sample, we have around 25 sectors from 14 countries for Air transport, vs 230 sectors from 190 countries in the large sample; and around 50 sectors from 20 countries for Vessel, vs 600 sectors from 190 countries in the complete sample). Given that the reduced sample makes for 80% of the total value of trade flows, this suggests that the vast majority of US imports comes from a selected range of countries, in a selected range of sectors.

the transport mode and for both types of transport costs, the trend patterns of international transport costs are very similar whether estimated under the separability assumption or not. Given that the restricted sample covers 80% of the total trade flows of the US economy, we hope to convince you that this result can be extended to the whole dataset.

Second, the referee's method imposes a drastic selection of countries / sectors to be included in the estimation. Specifically, since the estimation method is run at the sector-origin country level (on top of being year-mode specific), the number of flows within each country-sector pair should be large enough to more than cover the number of k- product and d- entry port fixed effects. This substantially reduces the number of point estimates, as well as the value of trade flows covered relative to the benchmark method, as summarized in Table .

- the referee writes *The second advantage is that there is a handful of previously-proposed instruments (e.g., HS-10 product-specific tariff rates or lagged prices), which the authors can use to overcome the endogeneity problem.*

  Our answer: We thank the referee for this valuable suggestion. It is worth noticing that we can also handle the instrumentation of fas prices at the HS-10 level with our method - which we do in the revised version.

- the referee writes *The third advantage is that, by adopting this approach, the comparison between the paper [...] and those in Hummels (2007) would become more transparent.*
  Our answer: We agree with the referee that the comparison with the literature (Hummels (2007) in particular) was not straightforward for the reader in the submitted version. We did our best to make the comparison clearer in the revised version, yet in a way consistent with our estimation method. In contrast to the submitted version, the paper now starts from the relation linking transport costs to the unit price specified in Hummels (2007):

$$f_{ikt} = X_{is(k)t}\widetilde{p}_{ikt}^{-\beta_{ikt}} \tag{3}$$

with $f_{ikt} = \frac{p_{ikt}}{\widetilde{p}_{ikt}} - 1$ the transport costs measure, p_{ikt} ($\widetilde{p}_{ikt}$) the cif (fas) price and β_{ik} the price-elasticity of transport costs, which also corresponds to the share of additive costs in total transport costs (as we now show in the paper's appendix):

$$\beta_{ikt} = \frac{\frac{t_{is(k)}}{\widetilde{p}_{ikt}}}{\tau_{is(k)} - 1 + \frac{t_{is(k)}}{\widetilde{p}_{ikt}}} \tag{4}$$

As underlined in the paper (both submitted and revised versions), Equation (7) lies at the root of Hummels's (2007) method, as he estimates Equation (7) on a panel basis and at the sectoral $s(k)$ basis, yielding to estimates of $\beta_{is(k)}$ that are invariant over time (but mode-specific).

Two alternative strategies are yet available. Let us start with the referee's method. It starts from the functional form linking transport costs and the fas price as specified in the Hummels's equation (7). Taken in log (on a mode/yearly basis), it is written as:

$$\ln f_{ikd} = \beta_{is(k)} \ln \widetilde{p}_{ikd} + \text{Controls}_{ikd} + \varepsilon_{ikd}$$

From this, one can then recover the levels of additive / multiplicative transports costs. Denoting $\widehat{\beta}_{is(k)}$ the estimated β for a given sector-country $i, s(k)$, one can indeed solve the following two-equation system:

$$p_{ik} = \tau_{is(k)} \widetilde{p}_{ik} + t_{is(k)} \tag{5}$$

$$\frac{t_{is(k)}}{(\tau_{is(k)} - 1)\widetilde{p}_{ik} + t_{is(k)}} = \widehat{\beta}_{is(k)} \tag{6}$$

with p_{ik} and $\widetilde{p}_{ik}$ the cif and fas prices observed in our dataset (conditional on a given year-transport mode). With 2 equations and 2 endogenous variables, the system might be solved.

Alternatively, our method rather starts from the definition of transport costs as:

$$\ln f_{ik} = \ln \left(\tau_{is(k)} - 1 + \frac{t_{is(k)}}{\widetilde{p}_{ik}} \right) + \varepsilon_{ik}$$

from which we deduce the additive/multiplicative costs $\widehat{t}_{is(k)}, \widehat{\tau}_{is(k)}$ (on a mode/yearly basis). Then, relying on the Hummels' equation, one can deduce the share of additive costs in total transport costs β_{ik} through Equation (4), again on a mode/yearly basis.

In this respect, the two methods are equivalent, in that they both allow to uncover the share of additive costs in total transport costs β, as well as the value of each trade costs component (t, τ), that vary over time and sectors (and transport mode). Put it differently, our method is consistent with Hummel's methodology - provided that it is adequately reported as such. We agree that

8

this was not sufficiently transparent in the submitted version. We hope that the revised version now fits the referee's expectations on this point.

b) The costs are high

Concern 1 The suggested estimation strategy implies having much less data to exploit, for two reasons.

1. Information about the port of entry are only available since 1989. Implementing the referee's method would hence necessarily reduce the time coverage of our analysis by 15 years (1974-1988). In our view, the historical coverage is interesting per se, as it provides useful insights about how transport costs have evolved over time. Eliminating this dimension of the paper would be detrimental to its value-added.

2. As underlined before, the method is run country by country, and 3-digit sector by 3-digit sector, exploiting the variability within each country-3d sector across 10-d sub-sectors and ports of entry. Yet, it appears that for many couples (country, 3-digit sector), there is too few variability across sub-sectors or ports en entry given the number of fixed effects included in the regression, such that estimation can not be run. This can be seen comparing the number of observations by year/ transport mode with our method / with the referee's method reported in Table XXX **TO DO**. Put it differently, this methodology discards countries which export a limited range of goods to the US and/or which arrive in the USA through the same ports of entry. It excludes XXX % of trade flows and XXX % of trade in value. In this respect, the induced selection bias reduces the general scope of the transport costs estimates.

Concern 2 Given that the estimation method is run at the country-sector level (exploiting heterogeneity *within* a given country-sector pair), it implicitly assumes that transport costs are independent *across* sectors and across origin countries. Put it in plain words, it means that transport costs of say, tissues, have nothing in common whether those goods comes from France or from Bangladesh; or that transport costs that apply to imported goods from France have no common component across sectors. In both cases, one might view this as a disputable assumption. By contrast, our method exploits the fact that transport costs have a both a country-specific component and a sector-specific component. **This stands in line with ... FAIRE REFERENCE A DES PAPIERS EN TRADE QUI ONT DES TRADE COSTS Pays-spécifique**

Concern 3 The suggested method features less accuracy in the estimation of the β. If we take the value of the β by itself, there is no criteria to discriminate between

the value estimated with our method and the one obtained with the suggested method (when, of course, run on the same sample). Things are more clear-cut in terms of accuracy of the estimation. Specifically, our method yields a more precise estimation of the β than the referee's method. To develop on this, for each year and transport mode:

- With the referee's method, we estimate one value for the share of additive component *beta* at the i, s level denoted $\hat{\beta}_{is(k)}^{ref}$ associated with a standard deviation SD_{is}, by year and transport mode. From this, we can approximate the 5-95% threshold values through:

$$\hat{\beta}_{is}^{min,ref} = \hat{\beta}_{is(k)}^{ref} - 1.96 SD_{is}, \quad \hat{\beta}_{is}^{max,ref} = \hat{\beta}_{is(k)}^{ref} + 1.96 SD_{is}$$

  From this, we can deduce the confidence interval $CI_{is}^{ref} = \hat{\beta}_{is}^{max,ref} - \hat{\beta}_{is}^{min,ref}$.

- Our method does not yield an estimate of β_{is} and an associated standard deviation, so that we cannot directly compare the precision of the estimation. Rather, our method provides estimates of the underlying trade costs components $(\widehat{\tau}_i, \widehat{\tau}_s, \widehat{t}_i, \widehat{t}_s)$ with an associated matrix of variance-covariance, from which we can rebuild β_{is}. We then deduce the accuracy of the β estimate by relying on bootstrap method. Specifically, on a yearly/mode basis we draw a distribution of trade costs components and associated β_{is} (10,000 random draws) from which we can compute the mean, the median and the 5-95 threshold values for each couple i, s. Noticing β_{is}^{95} and β_{is}^{05} the associated thresholds, we then obtain the confidence interval $CI_{is} = \beta_{is}^{95} - \beta_{is}^{05}$.[2]

- We can then evaluate the accuracy of each estimation method by comparing the size of the confidence intervals of the β, for each couple i, s (by year and transport mode). Figures XXX (for Air) and XXX (for Vessel) thus report the histogram of the log of the ratio $\frac{CI_{is}}{CI_{is}^{ref}}$, for the year 2012. Figure 1 In most cases, the log of the ratio is negative, implying a lower interval confidence of the β estimate with our methodology. Our method undoubtedly yields more accurate estimations of the share of additive components, whatever the transport mode considered. We have checked that a similar conclusion applies on other years (they are not reported here for sake of brevity but they are available upon request).

All these elements drive us to maintain our original estimation as benchmark

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[2]Notice that this should be made on the same sample as the one obtained with the referee's method, since the goal is to compare the accuracy of the β_{is} estimate - implying to have the same sample of countries / sectors at first.

Figure 1: Accuracy of the estimation of the β: Comparison
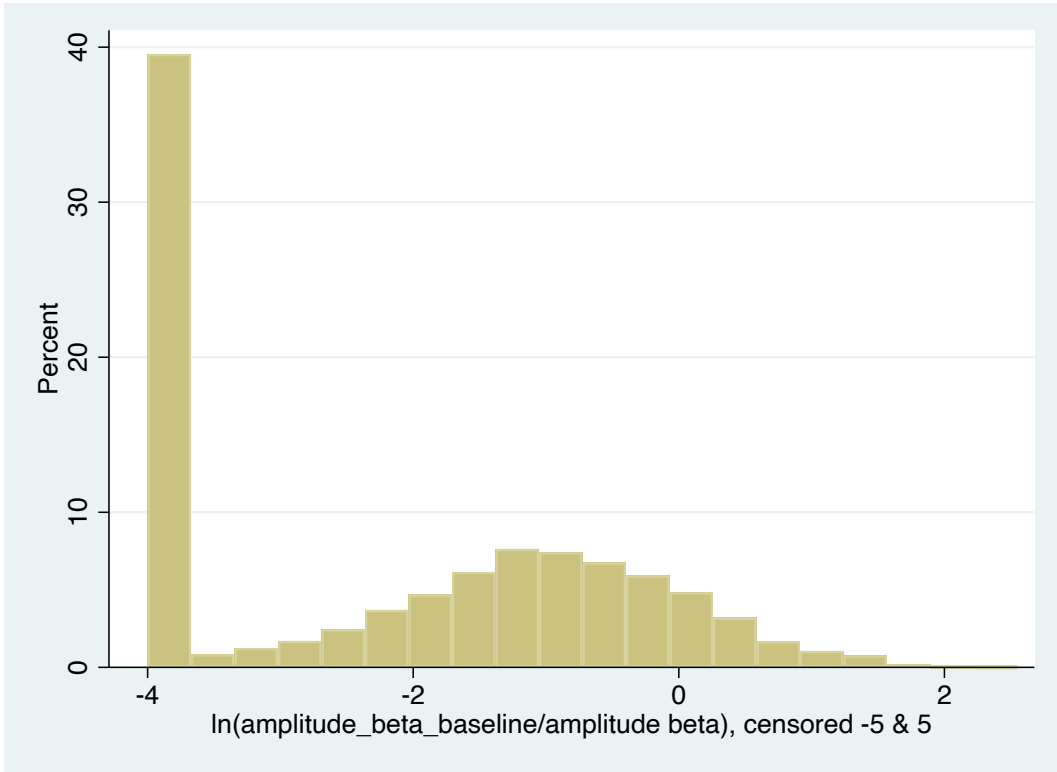
method in the revised version, and we deeply hope to convince you of the relevance of this choice. However, we also want to keep track of this alternative method as further robustness check. Fr this purpose, we have amended the online Appendix with a section devoted to the referee's method. **XXX TO BE DONE XXX CHECK THIS SECTION EXISTS IN THE ONLINE APPENDIX XXX**

2.1.2 Concerns about endogeneity

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 $1/\widetilde{p}_{ik}$. [...] Based on the productivity-sorting model in Melitz (2003) or the quality-sorting model in Baldwin and Harrigan (2010), $1/\widetilde{p}_{ik}$ is either positively or negatively correlated with ϵ_{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.*

Indeed, this is a very important point. The referee states that, based on theoretical insights by Melitz (*Econometrica*, 2003) or Baldwin and Harrigan (*American Economic Review*, 2011), $1/\widetilde{p}_{ik}$ is correlated in one direction on another with residuals ϵ_{ik}. 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 ($\widetilde{p}_{ik}$) 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 a full set of IV estimates to provide a clean assessment of the size of the potential bias. Section 4.3 in the revised version of the paper provides an overview of the results, while section B.4 in Appendix B provides a full presentation of the theoretical basis for the first-stage equation, as well as first-stage estimates. We follow earlier literature (see e.g. Caliendo and Parro, 2015, or Lashkaripour, 2017) by implementing a first-stage equation regressing $\widetilde{p}$ on custom duties coming based on tariffs at the product line, together with one-year lagged fas prices. First-stage estimates reported in section B.4 show that our main instrumental variable displays the right statistical properties, and that we can confidently re-inject the predictions arising from the first-stage equation for $\widetilde{p}_{ik}$ on the right-hand side of Equation 10 to produce a 2SLS-type of estimation. Figure 9 in the revised version of the paper reports our benchmark estimates by transport mode, together with their instrumented counterpart. In all cases, these estimates are very similar, when not identical. This supports that our benchmark estimates do not suffer any substantial biases arising from endogeneity concerns.

Not this check is performed both on our main dataset (SITC 5 digit), and also at the HS 10 level, to handle simultaneously the referee's concern about aggregation issues. In the latter case, second-stage estimates are not reported for the sake of space, but do not change anything to previous conclusion - they are very, very similar to their non-IV, least squares counterparts.

2.1.3 The aggregation problem

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 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.*

It is true that the original annual Census data reports trade at the origin country-HS 10 product-district level of aggregation. Our initial choice of retaining $s = 3$ at the sectoral level, $k = 5$ at the product level was driven by the use of Hummel's dataset available over 1974-2004 with $k = 5$ the finest degree of aggregation. We yet agree with the referee that jumping from $k = 10$ to $k = 5$ digits at the product level is detrimental to our ability to exploit useful variation in the data. In line with the referee's comment, we now run the estimation by considering k at the HS10 classification level, not 5. After checking with the US Bureau of Census, data is only available since 1989, now available to 2019. We have hence asked for all available data at the time of revision (1989-2009), and we now present the results considering $s = 3$ and $k = 10$. This however deprives us from being able to exploit the period 1974-1988 at the aggregation level. **Que faire pour la periode 1974-1989 XXX TO BE DISCUSSED XXX**. Furthermore, the computational burden mentioned by the referee is not attributable to the product classification level $k = 5$ or 10) but rather to the degree of sectoral classification ($s = 3$ or 4) as it conditions the number of fixed effects. This explains why we consider the $s = 4$ digit- sectoral classification level only for some years. We thank the referee for pointing this ambiguity in our paper, which drove us to rewrite the associated paragraph in the revised version of the paper **see XXXX TO BE DONE ??? XXX**

2.2 Critique 2: Calculation of Unit Prices

My second critique concerns the way the authors are calculating the unit prices. The Census data reports the quantity of goods per observation. So, the authors can calculate the unit price as Value/Quantity, which is consistent with how price is modeled in standard trade models. Instead, the authors calculate unit price as Value/Weight. This used to be a common exercise in the past where many datasets did not report Quantity. But, given their data, there is no justification for the authors to calculate the prices this way.

Calculating the unit price as Value/Weight presents the authors with an additional endogenity problem. To elaborate, let $\omega_{ik} = Weight/Quantity$ denote the unit weight of the goods in observation ik. Also let $\widehat{p}_{ik} = Value/Quantity$ (unlike what the authors assume) denote standard definition of price. As a consequence, making the link with the fas price we consider in the paper: $\widetilde{p}_{ik} = \widehat{p}_{ik}/\omega_{ik}$.

This paper is essentially estimating the following equation:

$$\ln\left(\frac{p_{ik}}{\widetilde{p}_{ik}} - 1\right) = \ln\left(\tau_i \times \tau_{s(k)} - 1 + \frac{t_i + t_{s(k)}}{\widehat{p}_{ik}/\omega_{ik}}\right) + \epsilon_{ik}$$

instead of estimating:

13

$$\ln \left(\frac{p_{ik}}{\widetilde{p}_{ik}} - 1 \right) = \ln \left(\tau_i \times \tau_{s(k)} - 1 + \frac{t_i + t_{s(k)}}{\widehat{p}_{ik}} \right) + \epsilon_{ik}$$

There is evidence that (i) ω_{ik} varies significantly within narrowly-defined product categories, and (ii) ω_{ik} is negatively correlated with transport costs. So the way the authors are calculating unit prices and estimating the model creates a new (but avoidable) source of endogeneity.

Our answer: The referee definitely has a point here, in the sense that Census data do report the quantity of goods. In addition, referee's remarks echo recent findings by Lashkaripour (*Journal of International Economics*, 2020): based on US data very similar to ours over the period 1995-2015, the paper highlights, among other results, the unit weight of imported goods is indeed substantially heterogeneous even within narrowly-defined product categories and the cost of transportation increases more rapidly with unit weight than the cost of production. Finally, Lashkaripour (2020) finds accounting for the heterogeneity in export unit weights provides evidence in favor of the iceberg cost assumption regarding transport costs.

We devoted a lot of thought to a potential switch from weight to quantity to get cleaner unit prices. However, the latter appears really uneasy in our context for several, non-trivial reasons:

1. The information on quantity is not mode-specific, which is incompatible with our empirical strategy of estimation transport costs conditional on the transport mode, similarly as in Hummels (2004). We could of course restrict observations to single-mode flows, but that would alter our sample in a non-trivial way, raising some potential selection biases.

2. Second, units are not available for many years over our period, especially in the 1970s and the 1980s **XXX GUILLAUME, TO BE CONFIRMED AND SUPPORTEDXXX**; this is particularly problematic considering that the time length is crucial for the assessment of transport costs long-run dynamics, which is one of the main objectives of the paper.

3. Time length is especially important considering that, in the 1970s, trade composition was significantly different from today, with more goods for which correlation between weight and quality is likely to be less obvious **XXX GUILLAUME: EXAMPLES XXX**. At a more fundamental level, one may argue that considering the unit price as value/weight is relevant in our setting where we seek to identify transport costs. For instance, it makes sense that the shipment costs of cars do depend not only on the quantity of cars exported, but on the weight it makes, which is related to the volume it takes in the plane or the

vessel. **XXX GUILLAUME: Do we have evidence, even anecdotal, on this point?**

These motives altogether highlight that it would be too costly and uncertain in our context to switch from weights to quantities to compute unit prices. That said, do we have reason to believe this measure of unit prices generate significant deviations from the one relying on quantities?

Lashkaripour (2020) does provide important results related to ours. In particular, his result of transport costs close to be totally iceberg is quite different from our own findings of additive costs representing 20 to 40% of total costs. The two stories are not necessarily incompatible, however. First, beyond a longer time span, our database is exhaustive (**XXX GUILLAUME: TO BE CONFIRMED)**, and encompasses all goods and industries, while Lashkaripour (2020) restricts to indivisible/discrete goods representing a bit more 56% of US total imports of goods. In this regard, when comparing Tables 1 and 10 in Lashkaripour (2020), one can see that the share of multiplicative costs tends to decrease with the share of discrete goods.[3] It is therefore reasonable to think that the inclusion of all goods, like in our setting, would move the average share of multiplicative costs far from 100%.

2.3 Critique 3: Big Picture Implications

My third critique concerns the lack of an exciting punchline. The fact that composition effects have not countervailed the reduction in pure transport costs (at least not as much as previously believed) is an interesting but minor observation. Does this observation revise our understanding of say the gains from trade? Does it shed new light on a puzzle many people are thinking about? One crude suggestion is to see how the reduction in the industry-specific cost terms is related to the industry-level trade elasticities. If the composition effects favor low-elasticity industries, the findings in the paper may have first-order implications for the gains from trade. Another suggestion is to dig deeper into the relative rate at which additive and multiplicative transport costs have declined over time. Since additive transport costs favor rich (high-quality exporting) countries, the disproportionally greater reduction in additive costs can perhaps explain the rise of low-income exporter as documented by Hanson (2012, JEP).

Comments about that

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[3]Also, note that the share of multiplicative costs is derived from a formulation - Equation (3) in Lashkaripour (2021) - that may produce noisier estimates compared to ours, as we point in answer 2.1.ii to your Critique 1 above XXX **GUILLAUME : TO BE DISCUSSED AGAIN TOGETHER XXX**

Piste 1 Consider his/her first suggestion. What is the idea? First, the link between gains from trade and trade-elasticity. Am I correct in saying that gains from trade are higher when trade is about low-elasticity goods? The idea being, if national goods are low substitutes, then here are larger gains from trading them. So, if composition effects (trade shifts between sectors) favor low-elasticity sectors, then one might expect high gains from trade. Hummels: trade composition effects matter as they partially offset the reduction in pure transport costs for both air and vessel. Over time, tendency to trade goods more costly to export everything else equal. So, if the referee's assertion is right (trade composition effects favor low-elasticity sectors), then increasing gains from trade. But, we disagree with Hummel's findings (his way of measuring things is inappropriate) and find that trade composition effects do not matter much, at least for air (for vessel, the composition effects matter more, but by amplifying the reduction in transport costs, ie towards goods that are less costly to export). Meaning that gains from trade are purely due to reduction in transport costs at the sector, but not from switch towards low-elasticity sectors. Identify one source of gains from trade, not two.

What to do with this? When composition effects do matter (ocean freight), do they favor low-elasticity industries? In which case, on top of the reduction in transport costs per se (which has welfare consequences as well), one might expect additional gains from these composition effects. When they don't, this mitigates the gains from trade that could be expected from the picture given by Hummels.

GD20200708 Hum... Je crois que je comprends le point dur referee et je suis d'accord avec lui. Le souci, c'est que de toutes les manières, je ne sais pas si cela favorise les high ou les low elasticity. Mais on pourrait voir.

Piste 2 What is at stake? Show that how β = the relative share of additive costs has evolved over time. Good idea as this is really what differentiates us from Hummels, model a varying β over time. Specifically, how methodology lets the β vary over the three time-sector-partner country dimension. Show how β varies over time everything else equal? Hopefully, goes in the referee's direction (decreases over time) with possibly a role in the understanding in the "big picture" of trade patterns?

Piste 3 **GD20200708 On sait que les additive costs sont plus distorsifs que les multiplicative costs, puisqu'ils modifient les prix relatifs. Donc le fait qu'ils sont une partie croissante des costs signifie qu'on sur-estime l'augmentation des gains de welfare liés à la baisse des coûts de transport**

A Clarifying some technical points

A.1 A note on prices

We have three prices: the (US) consumer price, say p^c_{ik}, the "import" or cif price (at the entrance of the US) p_{ik} and the fas price (at the export gate in the origin country) $\widetilde{p}_{ik}$, denoting i the origin country, k the product at the 8 or 5 digit level, and reasoning on a yearly basis, and $s(k)$ the 3-digit classification the product k belongs to. Further, if we denote τ^d_{ik} the duty tax rate paid when the good crosses the US border, then we have:

$$p^c_{ik} = (1 + \tau^d_{is(k)})p_{ik}$$

$$with \quad p_{ik} = \tau_{is(k)}\widetilde{p}_{ik} + t_{is(k)}$$

A.2 Deriving the share of additive costs in total transport costs

With i the origin country, k the product category at the HS6 level (and reasoning at the year- transport mode level), p_{ik} the import (cif) price and $\tilde{p}_{ik}$ the export (fas) price, transport costs $f_{ik} \equiv \frac{p_{ik} - \tilde{p}_{ik}}{\tilde{p}_{ik}}$ are written in Hummel's terminology as:

$$f_{ik} = X_{is(k)}\tilde{p}_{ik}^{-\beta_{ik}} \tag{7}$$

It is trivial to show that:
$$\frac{\partial f_{ik}}{\partial \tilde{p}_{ik}}\frac{\tilde{p}_{ik}}{f_{ik}} = -\beta_{ik}$$

Then

- If $\beta_{ik} = 0$, then $p_{ik} = (1 + X_{is(k)})\tilde{p}_{ik}$: Only ad-valorem transport costs

- If $\beta_{ik} = 1$, then $p_{ik} = \tilde{p}_{ik} + X_{is(k)}$: Only additive costs

This suggests that, the closer β_{ik} to 1, the more prevalent additive costs in total transport costs. Can β_{ik} be related to the share of additive costs in total transport costs? The answer is positive. To show this clearly, start from:

$$f_{ik} = \frac{p_{ik} - \tilde{p_{ik}}}{\tilde{p}_{ik}}$$

$$with: \quad p_{ik} = \tau_{is(k)}\tilde{p}_{ik} + t_{is(k)}$$

assuming that transport costs decompose in two components, additive transport costs $t_{is(k)}$ and multiplicative costs (ad-valorem) $\tau_{is(k)}$, supposed to be identical in the product-dimension for any k product within the 3-digit classification s it belongs to.

This gives:

$$f_{ik} = \frac{(\tau_{is(k)} - 1)\tilde{p}_{ik} + t_{is(k)}}{\tilde{p}_{ik}}$$

From this, we have

$$
\begin{aligned}
\frac{\partial f_{ik}}{\partial \tilde{p}_{ik}} &= \frac{(\tau_{is(k)} - 1)\tilde{p}_{ik} - ((\tau_{is(k)} - 1)\tilde{p}_{ik} + t_{is(k)})}{\tilde{p}_{ik}^2} \\
&= \frac{-t_{is(k)}}{\tilde{p}_{ik}^2}
\end{aligned}
$$

Hence

$$
\begin{aligned}
\frac{\partial f_{ik}}{\partial \tilde{p}_{ik}}\frac{\tilde{p}_{ik}}{f_{ik}} &= \frac{-t_{is(k)}}{(\tau_{is(k)} - 1)\tilde{p}_{ik} + t_{is(k)}} \\
&= \frac{-\frac{t_{is(k)}}{\tilde{p}_{ik}}}{\tau_{is(k)} - 1 + \frac{t_{ik}}{\tilde{p}_{ik}}}
\end{aligned}
$$

This shows that β_{ik} can be interpreted as the share of additive costs in total transport costs:

$$\beta_{ik} = \frac{\frac{t_{is(k)}}{\tilde{p}_{ik}}}{\tau_{is(k)} - 1 + \frac{t_{is(k)}}{\tilde{p}_{is(k)}}}$$

A.3 Share vs level

Starting from the above reasoning, the referee proposes to estimate the elasticity of transport costs with respect to the unit price, equivalently the share of additive costs in total costs. Specifically, the referee suggests to estimate for each industry-country the following regression:

$$\ln f_{ikd} = \beta_{is(k)} \ln \tilde{p}_{ikd} + \text{Controls}_{ikd} + \epsilon_{ikd}$$

The question is then, how to recover the levels of additive / multiplicative transports costs $t_{is(k)}$ and $\tau_{is(k)}$. Denoting $\widehat{\beta}_{is(k)}$ the estimated β from the referee's method for a given sector-country $i, s(k)$, one can solve the following two-equation system:

$$p_{ik} = \tau_{is(k)}\widetilde{p}_{ik} + t_{is(k)} \tag{8}$$

$$\frac{t_{is(k)}}{(\tau_{is(k)} - 1)\widetilde{p}_{ik}} = \widehat{\beta}_{is(k)} \tag{9}$$

with p_{ik} and $\widetilde{p}_{ik}$ the cif and fas prices observed in our dataset (conditional on a given year-transport mode). With 2 equations and 2 endogenous variables, the system might be solved. Specifically, with $\widehat{\beta}$ being estimated at the *sector s-* country i level, and $\widetilde{p}_{ik}$ at the *product k*-country level, this implies that t and τ are product-country i, k specific (not sector-country s, k specific). **from this point of view, this is a finer characterization of transport costs, so better from this point of view...**

To show this properly, start from Equation (9) to express additive costs according to:

$$t_{ik} = \frac{\widehat{\beta}_{is(k)}}{1 - \widehat{\beta}_{is(k)}} \left[\tau_{is(k)} - 1\right]\widetilde{p}_{ik}$$

With $\widetilde{p}_{ik}$ being product-country specific, t_{ik} is necessarily product-country specific $(i, k,$ not $i, s(k))$. Plugging this into Equation (8):

$$p_{ik} = \tau_{is(k)}\widetilde{p}_{ik} + \frac{\widehat{\beta}_{is(k)}}{1 - \widehat{\beta}_{is(k)}} \left[\tau_{is(k)} - 1\right]\widetilde{p}_{ik}$$

Solving for τ, one finally obtains the solutions for the ad-valorem and additive transport costs components:

$$\tau_{ik} = (1 - \widehat{\beta}_{is(k)})\frac{p_{ik}}{\widetilde{p}_{ik}} + \widehat{\beta}_{is(k)} \tag{10}$$

$$t_i k = \frac{\widehat{\beta}_{is(k)}}{1 - \widehat{\beta}_{is(k)}} \left[\tau_{is(k)} - 1\right]\widetilde{p}_{ik} \tag{11}$$

Two points can be made. First, we can check that if $\widehat{\beta}_{is(k)} = 0$, then $p_{ik} = \tau_{ik}\widetilde{p}_{ik}$ (Equation 10) and $t_{ik} = 0$ (Equation **??**), all transport costs are ad-valorem. Conversely, if $\widehat{\beta}_{is(k)} = 1$, then $p_{ik} = \widetilde{p}_{ik} + t_{ik}$ (Equation **??**) and $\tau_{ik} = 0$ (Equation 11). Second, and most importantly, the method allows to recover the levels of both transport costs components at the product-country level i, k, even though the share of additive costs in total costs is estimated at the sector-country level.