

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 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. When necessary, we refer to number of sections, pages and equations from the submitted version. In this case, they are written into brackets.

1 Main changes

The structure of the paper has been modified after taking into account the referees' comments.

- In the submitted version, [Section 2] was devoted to the estimation of international transport costs; specifically, their break-down in two components, additive and ad-valorem. In [Section 3], we investigated the role of additive costs in the decomposition of transport costs time trends, between structural changes and composition effects. [Section 4] was devoted to the robustness analysis relative to the results from both previous sections.
- In the revised version, we have strengthened the robustness checks regarding the estimation results of international transport costs (robustness to aggregation, robustness to IV estimates). Accordingly, the previously-called [Section 2] has been split in two Sections: Section 2, where we present the data and the estimation strategy; and the results, in Section 3, which now includes the robustness checks as final sub-section. For sake of space saving, the robustness analysis related to composition effects has been set to the Appendix (Section C.3).

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- Both referees asked us to put light on the implications of additive costs (the “Big Picture”). We agree that this was not enough emphasized in the submitted version, and we thank the referees for their suggestions on that issue. The revised version answers this concern in two points.

1. We maintain the analysis of the transport costs time trends (previously [Section 3], now Section 4); but we made our best to make this section easier to read, leaving the technical aspects in the Appendix (Section C). As such, we hope that the main message of this Section is better caught.
2. Most importantly, we now emphasize the “Big Picture” implications of additive costs on theoretical grounds. Through the lens of the Melitz’s (2003) model amended to integrate additive costs, we analyze the welfare gains that derive from the reduction in international transport costs that we have estimated (in Section 3 of the revised version), to quantify the extra welfare gains attributable to the reduction of the additive component. This analysis is driven in a new Section 5.

- To keep the paper (both the main text and with appendix included) within a reasonable number of pages, yet to be fully transparent about our results, we have added numerous detailed tables in the Online Appendix, to which the reader can refer to.

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,

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

or,

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

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

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

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.

The three points raised by the referee indeed deserve careful consideration. We provide separate answers to each of them below.

2.1.1 Concern (i): About the 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 et al. (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 XXX 2019 ??? XXX, 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.

WE ALREADY DID THAT BEFORE. SAY THAT WE POUT THAT EARLIER IN THE PAPER AND CLEARER?

2.1.2 Concern (ii): On the use of non-linear least squares (NLS)

As noted by the referee (and in the paper), it is true that relying on the non-linear least squares method is highly demanding computationally when the number of fixed effects increase. This constraint notably drove us to impose the separability assumption and retain $s = 3$ as the relevant sectoral degree of aggregation.

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. Given the authors' objective and the data they are using, β can be separately estimated for each industry-country pair using the following regression:

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

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. [...] 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.

Our answer: First of all, we thank the referee for suggesting to improve the comparison between our method and Hummel's. We took this remark into account by adding a new sub-section in the revised version (Section 2.2, paragraph "Estimation strategy"). As we now show in this 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.

Now coming to the technical side, we thank the referee for his/her relevant suggestion of an alternative estimation method. This drove us to question our own empirical specification deeper. In the end, we yet decided to maintain our empirical strategy in the revised version, because its advantages seem to overweight the possible costs, and the alternative that you propose does not go without a few important limitations. We develop each line of argument in our answer. After reading our answer, we hope to convince you that the choice we made is the most relevant one. **The referee's method is presented in the online appendix XXX WHERE??? XXX, and we refer to it in Footnote XXX of the paper - A FAIRE EN ONLINE APPENDIX? JUSTE DANS LA LETTRE?**

Estimating the share of additive costs: Highlighting some requirements Our estimated equation relies on 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, as it is now exposed in footnote 12 page 8. **LP: A Ai enleve le copie-colle de la footnote dans la lettre; il suffit de dire que c'est dans la version revisee, non?**

Exploring Referee's alternative functional form The estimation strategy suggested by the referee starts from Equation (3) linking transport costs and unit prices as assumed in Hummels (2007), yielding Equation (2) as estimation equation. 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 2005-2019, at the $s = 3, k = 10$ sectoral/product levels.

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)* $t_{ik} = t_i + t_k$.) 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.

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


- *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. 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

¹In 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. **LP: CHECK THE NUMBERS**

3.3.1, 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. 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 XXX**. **Il faut reprendre cette reponse. Reprendre les resultats sur la methode du refere 1** 

- *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 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):

$$f_{ikt} = X_{is(k)t} \tilde{p}_{ikt}^{\beta_{ikt}} \quad (3)$$

with $f_{ikt} = \frac{p_{ikt}}{\tilde{p}_{ikt}} - 1$ the transport costs measure, p_{ikt} (\tilde{p}_{ikt}) the cif (fas) price and β_{ikt} the price-elasticity of transport costs, with i the origin country, k the product and t for time. As we now show in the paper (Section 2.2), this also corresponds to the share of additive costs in total transport costs:²

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

Equation (3) lies at the root of Hummels's (2007) method. In contrast to our method though, Hummels (2007) estimates Equation (3) on a panel basis and at the sectoral

²For sake of expositional purpose, the reasoning is made without any i, k, t specification in the revised paper. We directly report them here to ease the comparison with the referee's method afterwards.

$s(k)$ basis, assuming a constant β invariant over time/sector/origin country (even though mode-specific). Rather than Equation (3), this amounts starting from the functional form: $f_{ikt} = X_{is(k)} \tilde{p}_{ikt}^{-\beta}$.

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 Equation (3). Taken in log (on a mode/yearly basis), it is written as:

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

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

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

$$\frac{t_{is(k)}}{(\tau_{is(k)} - 1) \tilde{p}_{ik} + t_{is(k)}} = \hat{\beta}_{is(k)} \quad (6)$$

with p_{ik} and \tilde{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:

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


from which we deduce the additive/multiplicative costs $\hat{t}_{is(k)}, \hat{\tau}_{is(k)}$ (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 referee's method and our's 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. In comparison with Hummels (2007), one supplementary advantage of our method is that we estimate a β that varies over time, origin country and sector. Section 4 ([Section 3] of the revised version) explores the importance of allowing for such a variability in accounting for the sources of the transport costs time trend. We agree that 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, which is already smaller than our full sample starting in 1974. On top of that, because of the Covid situation, the US Bureau of Census was only able to send them the CDs relative to the years 1997-1999 and 2001-2019. Implementing the referee's method would hence necessarily reduce the time coverage of our analysis by more than 20 years (skipping the 1974-1996 years in particular). 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 of 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. **LP: Il faut le faire**

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, , 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 A FAIRE !!!**

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 β 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:

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

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

- Our method provides estimates of the underlying trade costs components ($\hat{\tau}_i$, $\hat{\tau}_s$, \hat{t}_i , \hat{t}_s) with an associated matrix of variance-covariance, from which we can rebuild β_{is} . **NOTE THAT β is in fact i, s, t because we use individual fas prices to rebuilt it. Should we say tha?.** As such, it does not yield an estimate of β_{is} and an associated standard deviation, so that we cannot directly compare the precision of the estimation. 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 $CI_{is} = \beta_{is}^{95} - \beta_{is}^{05}$.³

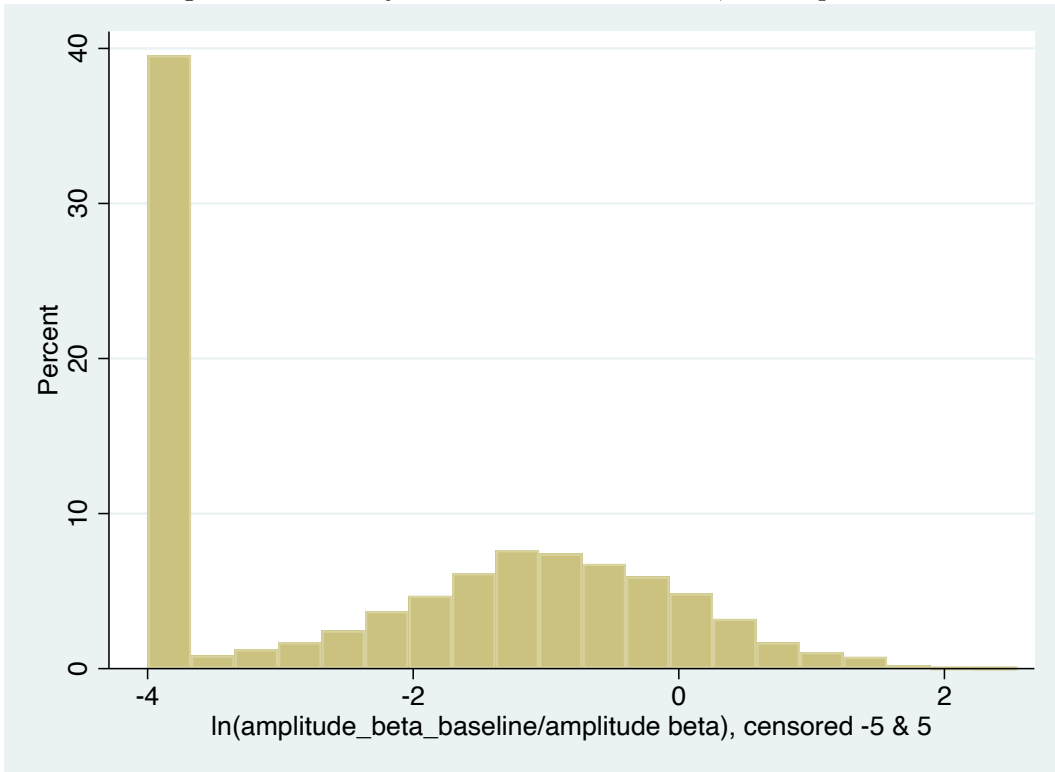
- We can then evaluate the accuracy of each estimation method by comparing the size of the confidence intervals of the β , for each couple i, s (by year and transport mode). Figures XXX (for Air) and XXX (for Vessel) thus report the histogram of the log of the ratio $\frac{CI_{is}}{CI_{is}^{ref}}$, for the year 2012. **IL FAUT ACTUALISER TOUT CA. OU DANS LE FICHIER SORTIE?**

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 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**

³Notice 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



2.1.3 Concerns about endogeneity

***The endogeneity problem:** quoting Footnote 14 of the paper, the authors are estimating t_i and $t_{s(k)}$ as coefficients on the industry or country dummies times $1/\tilde{p}_{ik}$. [...] Based on the productivity-sorting model in Melitz (2003) or the quality-sorting model in Baldwin and Harrigan (2010), $1/\tilde{p}_{ik}$ is either positively or negatively correlated with ϵ_{ik} . So, the NNLS estimates are biased; and the bias has nothing to do 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 (2003) or Baldwin & Harrigan (2011), $1/\tilde{p}_{ik}$ 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 (\tilde{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 \tilde{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 \tilde{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.

It should be noted that we performed this check 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. Needless to say that these results are available upon request from the referee.

2.1.4 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. After checking with the US Bureau of Census, data is only available since 1989, now available to 2019. As previously mentioned, due to the Covid situation, it was further only possible to receive the data over the years 1997-1999 and from 2001 onwards. In front of this arbitrage, we decided to maintain the case $s = 3, k = 5$ as baseline, but running estimation at the $s = 3, k = 10$ in the new Sub-Section 3.3.3 of Section 3.3 devoted to robustness analysis.

Additionally, 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??? LE DIRE DANS LA SECTION DE ROBUSTESSE OU ON FAIT DU 3-10??**

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 data-sets 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 endogeneity problem. To elaborate, let $\omega_{ik} = \text{Weight}/\text{Quantity}$ denote the unit weight of the goods in observation 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.

LP: J'ai enlevé ses équations car c'est totalement faux

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 that 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. **LP: DOCUMENT THAT. SHOW IN THE LETTER THAT WE DID IT?**
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 SUP-**

PORTEDXXX; 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 in our context to switch from weights to quantities to compute unit prices for our baseline regression. That said, do we have reason to believe this measure of unit prices generate significant deviations from the one relying on quantities?

Las (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 30 to 45% of total costs. The two stories are not necessarily incompatible, however. First, beyond a longer time span, our database is exhaustive, and encompasses all goods and industries, while Las (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 Las (2020), one can see that the share of multiplicative costs tends to decrease with the share of discrete goods.⁴ 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%. **LP: EST-ce qu'on a des choses a lui montrer a lui sur la regression en quantite? Est-ce qu'on la met en annexe en ligne?**

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

⁴Also, note that the share of multiplicative costs is derived from a formulation - Equation (3) in Lashkaripour (2020) - 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 A QUELLE CRITIQUE FAIT-ON REFERENCE?**

rate at which additive and multiplicative transport costs have declined over time. Since additive transport costs favor rich (high-quality exporting) countries, the disproportionately greater reduction in additive costs can perhaps explain the rise of low-income exporter as documented by Hanson (2012, JEP).

Our answer: We devoted a lot of thought to this point, which directly echoed a similar concern by the other referee. Taking stock of your suggestion, we decided to offer insights on the welfare implications of our results. In this regard, the new section 5 “The role of additive cost: Theoretical insights” is devoted to a theory-based analysis of the alterations to welfare gains involved by the relative variations of additive and multiplicative transport costs over our period of analysis. To do so, we start from the canonical Melitz’s (2003) model where we add additive trade costs. The inclusion of additive costs in a Melitz (2003) setting has already been performed in Sorensen (2014) and Irarrazabal et al. (2015). However, Sorensen (2014) exclusively performs a theoretical analysis, without any quantitative exercise. In addition to a partial equilibrium extension of Melitz (2003), Irarrazabal et al. (2015) do perform a quantitative simulation to assess the welfare variations induced by the presence of additive costs, but the latter is based on a calibration for transport costs restricted to 2004, based on the case of Norway. In contrast, our own exercise relies on a several decades time span for the US, allowing us to highlight the welfare alterations induced over time by the relative dynamics of additive and multiplicative costs, based on “true” values for the latter - remember that our methodology allows for the identification of both multiplicative and additive costs as a share of total costs, whereas additive costs in Irarrazabal et al. (2015) are expressed relatively to the (median) export price.

More precisely, we use some of the estimates underlying results reported in Section 3 concerning multiplicative and additive costs, more precisely those for the years 1974 and 2019. Based on the latter, we implement several comparative statics exercises to investigate the different welfare consequences of alterations to multiplicative and additive costs. In addition, we also assess how the latter results are distorted by changes in sunk costs of exports, f_x . To that end, we adjust the share of exporting firms in the US: based on ?, we set the latter to 35% in 2019, versus 21% in 1974. In our preferred exercise, we quantify the welfare gains that derive from the reduction in transport costs as documented in Section 3, relying on a combined reduction in each additive/iceberg component, with the reduction in the fixed export cost. We compare this with the case where the total transport costs reduction is solely attributed to a decrease in ad-valorem costs.

Table 5 in Section 5 reports the results of these various comparative statics exercises for Air and Vessel transport modes. Specifically, we report the change in total transport costs decomposed in its two dimensions (additive and ad-valorem); as well as the welfare change, both in absolute and in relative terms. Qualitatively, the conclusions are identical for both transport modes, with two major insights. First, for a given decrease in total transport costs, welfare gains are around 50% higher when this reduction is partly achieved through a reduction in the additive costs. Second, the decrease in export sunk costs (i.e., increase in the share of exporting firms) proportionally amplifies the gains from decreasing variable

costs, with again an additional premium coming from the decrease of additive costs.

Overall, these results appear to add a substantial contribution to the paper : we are able to provide a quantitative assessment of the welfare gains induced by the decrease in both types (additive and multiplicative) of costs over a 45-year period, and to highlight the respective part of each component in the determination of these gains. Note also that not only the inclusion of additive costs in the underlying framework generates large welfare differences, but also that the latter are probably a lower bound of the welfare variations induced by changes in additive *trade* costs, larger than the sole transport costs.

LISE: A FINIR, A REPRENDRE

References

- (2020). Weight-based quality specialization. *Journal of International Economics*, 127, 103380.
- Baldwin, R. & Harrigan, J. (2011). Zeros, Quality, and Space: Trade Theory and Trade Evidence. *American Economic Journal: Microeconomics*, 3(2), 60–88.
- Hummels, D. (2007). Transportation costs and international trade in the second era of globalization. *Journal of Economic Perspectives*, 21(3), 131–154.
- Irrarrazabal, A., Moxnes, A., & Oromolla, L. D. (2015). The tip of the iceberg: A quantitative framework for estimating trade costs. *The Review of Economics and Statistics*, 97(4), 777–792.
- Melitz, M. (2003). The impact of trade on intra-industry reallocations and aggregate industry productivity. *Econometrica*, 71(6), 1695–1725.
- Sorensen, A. (2014). Additive versus multiplicative trade costs and the gains from trade. *Canadian Journal of Economics*, 47(3), 1032–1046.