

## Summary

This paper uses data on unit price and transport costs to estimate the relative importance of the *additive* and *multiplicative* components of the transport costs. The estimated components are then used to re-analyze how much composition effects have contributed to the historical decline in transport costs.

### Critique 1: *Empirical Strategy*

My first critique concerns the empirical approach chosen by the authors. Let me elaborate on this critique. Using the notation of the authors, they are intreated in identifying the share of the specific cost in the total transport cost. Namely,

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

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

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

in which  $\tau_i$ ,  $\tau_{s(k)}$ ,  $t_i$ , 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.

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

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

where  $\beta_{ik} = \frac{\partial \ln f_{ik}}{\partial \ln \tilde{p}_{ik}}$  is the elasticity of transport cost w.r.t. unit price. Given the authors' objective and the data they are using,  $\beta$  can be separately estimated for each *industry-country* pair using the following regression:

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

where  $d$  denotes the US district of entry and  $k$  denotes an HS10 product. The identification of  $\beta_{ik}$ , 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.<sup>1</sup> The second advantage is that there are a handful of previously-proposed instruments (e.g., HS10 product-specific tariff rates or lagged prices), which the authors can use to overcome the endogeneity problem when estimating the above regression. The third advantage, is that by adopting this approach, the comparison between the results attained in this paper and those in Hummels (2007) would become more transparent.

Now, I understand if the authors strictly prefer to maintain their current approach; but they should address two issues either way:

1. **The endogeneity problem:** quoting Footnote 14 of the paper, the authors are estimating  $t_i$  and  $t_{s(k)}$  as coefficients on the industry or country dummies times  $1/\tilde{p}_{ik}$ . Namely,

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

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  $\epsilon_{ik}$ . 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.

2. **The aggregation problem:** The original annual Census data reports trade at the *origin country-HS10 product-district* level of aggregation, whereas the authors are aggregating up the data even further to the *origin country-HS5 industry-year* level. Such an aggregation comes with strong

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<sup>1</sup>The authors do check the robustness of their results w.r.t. the separability assumption, but this only done for a small sub-sample.

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.

## 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*.<sup>2</sup> But, given their data, there is no justification for the authors to calculate the prices this way.

Aside from the obvious discrepancy between theory and measurement highlighted above, calculating the unit price as *Value/Weight* presents the authors with an additional endogeneity problem. To elaborate, let  $w_{ik} = \text{Weight}/\text{Quantity}$  denote the unit weight of the goods in observation  $ik$ . Also let  $p_{ik} = \text{Value}/\text{Quantity}$  (unlike what the authors assume) denote standard definition of price. This paper is essentially estimating the following equation:

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

instead of estimating

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

There is evidence that (i)  $w_{ik}$  varies significantly within narrowly-defined product categories, and (ii)  $w_{ik}$  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.

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<sup>2</sup>This approach is also, for instance, justified in Hummels and Skiba (2004), because they are testing the Washington apples effect as opposed to making claims about the share of additive trade costs in standard trade models like Anderson and Van Wincoop (2004). In particular, Hummels and Skiba (2004) estimate that transport costs increase less-than-proportionally with value-to-weight ratios. As a result, countries facing high transport costs have a tendency to export high value-to-weight ratio goods.

Considering these issues, I see no justification whatsoever for calculating unit price as value per kilogram as opposed to value per quantity.

### **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 favors 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 disproportionately greater reduction in additive costs can perhaps explain the rise of low-income exporter as documented by Hanson (2012, JEP).