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Peer Review

Review of: "Testing Baumol's Cost Disease in Tourism: Productivity, Prices, and Labour Costs in Selected EU Countries Post-COVID"

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Testing Baumol's Cost Disease in Tourism: Productivity, Prices, and Labour Costs in Selected EU Countries Post-COVID

This paper tests the Baumol's Cost Disease hypothesis, based on an econometric analysis of data from 15 selected EU countries from 2011 to 2023 regarding the tourism and manufacturing sectors. The conclusion is that higher productivity is positively associated with both prices and hourly labor costs, which is interpreted as support for the BCD hypothesis.

General comments

Interesting paper focusing on an interesting and complex issue. The authors explore an interesting data set (Tourism). The paper has a logical structure and is comprehensive. The estimated model is based on a familiar specification of the BCD in the literature (Nordhaus, 2008).

Nevertheless, it has some serious flaws. Some of them are quite fundamental.

1. It is not clear what the central research question is. Regarding the title, one may think that the focus is on the outcome of $beta_1$ in the formula on page 6. However, the authors also make an issue of the post-COVID effect $beta_3$ and the interaction effect $beta_4$. The Tourism sector might be interesting, but why choose manufacturing as a counterfactual? Why not some other sector or the whole economy?

2. The structure of the panel is not well described and leads to confusion. At first, it looks like the panel is structured as country x year, but regarding the specification on page 6, the dummy variable

suggests that the structure is country x year x sector. This raises the question of what log_Productivity measures: country-specific productivity or productivity in a sector?

3. Related to 2, the PDID does not make sense here. As explained in the introduction, this approach is applied for evaluating intervention or treatment effects. How does this apply to measuring the difference of effects of two different sectors? If so, this should be thoroughly explained.

4. It is unclear how the proposed PDID relates to the econometric techniques used. See also 3.

5. The paper lacks a table consisting of statistical descriptives of the data and a discussion about the data set, for instance, about the variance in data, measurement issues, outliers, et cetera.

6. Although the authors claim that the technique used is suitable for causal inference, I have my doubts. They just find some correlation between productivity and prices (or wages) and interpret this as a causal relationship. One can also argue that higher wages, for instance, due to labor market conditions (shortages) are a strong incentive for increasing productivity, or higher prices due to a rise in demand implies high productivity due to Verdoorn's Law. These reversed causality relationships are ignored here. Investigations into these relationships probably require a deep research into the lag structure of the system.

The more general academic issue at stake here is whether the original approach by Nordhaus is a suitable approach at all. In a very recent article by Blank & Van Heezik (2024) in *Economics* (https://www.mdpi.com/2227-7099/12/8/215), it is argued that at best one can make a sensible decomposition of cost shares, demand, input prices, and technical change.

The paper needs a thorough revision, in particular on the parts about the usefulness of DID in this context, the structure of the panel data, and the causality issue.

Declarations

Potential competing interests: No potential competing interests to declare.