

This paper aims to study a quite interesting topic, i.e. how residents perceive the development of cruise tourism in their own town, and how their preferences are formed.

The analysis is based on the information arising from an important survey based on personnel interviews.

Though, the importance of the topic and of the survey is not paralleled by the contents of the manuscript, which needs very substantial revision in order to meet the standards of a peer-reviewed international journal.

The main points which should be addressed are the following:

1) There is a mismatch between the idea of externalities and the indicators to which this label is attached. The authors, in fact, talk about "perceived effect" when actually referring to opinions and explanations for why respondents like/dislike cruise activities (page 11). It is difficult for me to realize why these should be considered reliable indicators of external effects.

Moreover, when discussing about externalities throughout the paper, a rigorous attention should be paid to rigorously identify what are these externalities, and distinguishing between true externalities and other economic effects directly affecting residents' activities (so called "pecuniary externalities).

2) The most serious limit of the present version of the paper is the lack of a logical distinction between "determinants" and "motivations of why people supports or does not support" investments in cruise tourism. Incidentally, this drawback causes several superfluous comments which "trivialize" the results' section.

Going to details, only socio-demographic information can be considered as "determinants", whilst this does not apply to residents' perception about economic environmental and socio-cultural impact: they are just motivations. Hence, the description (p. 10-11) of the "variables used to assess the residents' perceptions" is fine up to the sentence <<the main tourism attractions (kmtour)>>. All the remaining variables discussed later on cannot be used as regressors!

Differently from hypothetical scenarios used in contingent valuation or choice modeling analysis, where (to make an example) "job creation" is exogenously set by the analyst, in this case the job creation is an opinion by the respondent, which is actually simultaneously determined together with the approval/disapproval of investments in cruise activities.

Regressing support to investments in the cruise activity can just be considered as a consistency check, but not the estimation of an economic/psychological model!

3) A description of the exact sentences contained in the questionnaire in the questions used for collecting the variables used as regressors is absolutely needed. For example: how the survey collected the information of the variables related to welfare changes?

I also suggest the authors to include all this information in a specific section (maybe changing the title of current Section 4 to something like "The case under scrutiny and the administration of the survey", and of course improving the present contents).

4) The content of a few paragraphs makes the current version of the manuscript more a preliminary draft rather than a paper suitable for peer review submission.

a. This is the case of the comments to equation (1), where for the reader is hard to understand how strictly relate this maximization program (maximization w.r.t. what?), the concept of composite stakeholder and the specific case under scrutiny.

b. Also some wording and the poor quality of the English contribute to the feeling of a not finalized work

c. The regression table should report at least the number of observations and a goodness of fit test.