Manuscript: « Preference and Willingness-to-pay analysis for an ecoengineering technology for floating wind turbines »

General answer

We are deeply grateful for your thorough analysis and review of our work on ecoengineering applied to floating wind turbines. We have carefully considered each of your comments and provide detailed responses below. We are convinced that this article is significantly better thanks to your feedback and perspective. We also did corrections of language and adjustments of the discussion length.

The study design addresses a pertinent and policy-relevant research question, namely, whether eco-engineering, or "nature-inclusive," enhancements included in floating offshore wind technology increase social acceptability and, if so, the willingness to pay for co-benefits by households. The design and research question are strong, the attributes are well-chosen, and the underlying behavioural assumption appears sound, namely, that individuals with very negative attitudes toward offshore wind will tend to select the status quo, regardless of the ecological and economic co-benefits.

The study, however, needs some improvement in its current form for methodological rigor and clarity, which would allow it to pass a peer-review process:

1) *The study, although it is present in a lengthy way, needs more clarifications on the design of the discrete choice experiment, which, in its current form, does not allow for replicability and raises concerns over its validity. For instance, the authors could include a list of the choice cards used in the study, the exact wording of the status quo option, a discussion of the experimental design approach taken, and details on randomization, blocking, and measures taken to prevent respondent fatigue and attribute non-attendance.*

   Response: We have expanded the DCE description to ensure readability and replicability (section 2.2.1). First of all, we have added an example of choice cards (Appendices 3 & 4) and a full list of attribute levels seen by the individuals, following other DCE study on offshore wind farm. By doing so, the reader knows in details how the choice cards were presented and all the different scenarios that were presented to the participants. We also present in more detail the d-efficient design that was employed, as well as blocking structure and random assignment procedure. Finally, we explain that both the length of the questionnaire and the number of cards per individual were voluntary limited to reduce respondent fatigue. To avoid attribute non-attendance, the description of the attribute was non-technical and understandable to general audience.

2) *From an econometric standpoint, the use of a conditional logit model without addressing the issue of repeated choice tasks by respondent is questionable. At the very least, clustered standard errors by respondent should be reported and acknowledged, or a panel mixed logit (random parameters) or latent class model could be used (or at least discussed) to address heterogeneity in preferences and segments of the sample, which would also be consistent with the later discussion of status quo propensity.*

   Response: We agree that accounting for preference heterogeneity is important and goes beyond the basic framework of the conditional logit model. While the conditional logit provides a useful benchmark, it relies on restrictive assumptions such as homogeneous preferences and the independence of irrelevant alternatives (IIA). Given the repeated choice structure of our data, these assumptions may be too limiting. In the revised version of the manuscript, we therefore estimate a panel mixed logit model (sections 2.3.3, 3.6 and Appendices 6 & 7), which allows for unobserved heterogeneity in preferences and accounts for the panel nature of the data, since each respondent completes several choice tasks. To further enrich the analysis, we also explore observed heterogeneity by including interaction terms between the *recycle* attribute and the geographic department of residence, in order to assess whether the importance attached to this attribute varies across locations. Finally, we opted for the panel mixed logit

specification rather than a latent class model. While latent class models identify discrete preference segments, they generally require larger samples to reliably estimate class membership and class-specific parameters. Given our sample size, the mixed logit approach provides a more parsimonious and robust way to capture preference heterogeneity. We chose to include the results and the method used in the body of the manuscript, but given the similarity of the results and trends, we added the tables in the Appendices.

3) *Communication of the results, specifically the WTP, could be clarified. Since the biodiversity and fisheries impacts are expressed as percentage changes, the WTP values are technically expressed as percentage points. Consider reporting the WTP values corresponding to policy-relevant changes (e.g., +10 percent biodiversity, +5 percent fisheries income) and a range of bundles, along with confidence intervals.*

Response: We substantially revised the WTP reporting.
   a. We now report WTP for policy-relevant increments for two different and pertinent scenarios and for bundled packages combining multiple co-benefits (sections 3.3, 4.1).
   b. We compute 95% confidence intervals using the delta method and report them alongside WTP estimates (Table 6).
   c. We explicitly clarify in the text and table notes that biodiversity and fisheries attributes enter as percentage changes, so the marginal WTP is expressed per percentage point; we therefore focus reporting on meaningful increments for interpretability (Table 6).

4) *Finally, there are some internal inconsistencies in the results that affect the overall credibility of the findings. Table 6 has an error in the ">4 times status quo" column, which incorrectly sums to 49 instead of 449. The results in the Spearman correlation table are questionable and should be tightened or removed. If the table is retained, the issue of multiple testing should be addressed. The ZINB model, given the bounded range of 0 to 8, is questionable and not ideal. Consider a different model, perhaps a bounded model, or integrating status quo behaviour into the choice*

Response: We agree and have addressed each point.

   a. We corrected the error in Table 6 (">4 times status quo" in Table 7).
   b. We removed correlation analysis and associated plots because these results were not anymore central.
   c. We appreciate the reviewer's remark regarding the modelling of *status quo* behaviour. While the ZINB specification was initially used to explore the frequency of *status quo* choices, we now complement this analysis by integrating status quo behaviour directly within the discrete choice framework. Specifically, the revised panel mixed logit model includes an interaction between the *status quo* constant (ASC) and respondents' attitudes toward floating offshore wind. This specification allows the attractiveness of the *status quo* alternative to vary systematically with respondents' prior attitudes while remaining fully consistent with the random utility framework. This additional specification provides a robustness check and ensures that systematic differences in the propensity to select the status quo are captured within the choice model itself. (see point 2.b).