<u>Manuscript</u>: *"Preference and Willingness-to-pay analysis for an ecoengineering technology for floating wind turbines"*

General Comments

The manuscript investigates public preferences and willingness-to-pay for an eco-engineering solution integrated into floating offshore wind farms in France, using a discrete choice experiment across five coastal departments. The topic is timely, relevant, and clearly within the scope of Wind Energy Science, particularly given the accelerating deployment of floating wind and the rising emphasis on social acceptability, marine ecosystem impacts, and nature-inclusive design. The study makes a meaningful contribution by combining a specific engineering concept with a socio-economic valuation, and by comparing preference heterogeneity across distinct French coastal regions.

The manuscript is generally well written, well structured, and rich in contextual detail. The presentation of background literature, methodological steps, and results is thorough. The econometric analysis is appropriate and the discussion links findings to broader questions of public acceptance and environmental integration of offshore wind. However, some aspects require clarification or tightening before publication.

Specific Comments

1) The abstract is informative and complete, but overly detailed and thus somewhat lengthy. I believe that a tighter structure and more quantitative reporting would improve its scientific impact. Please consider shortening the context, add one clear sentence identifying the paper's contribution, summarize the method succinctly, and include one or two key quantitative findings (e.g., mean WTP) to make the results more impactful.

2) The description of the socio-demographic survey is thorough. However, the authors should specify whether quotas or weighting were applied to approximate regional populations. Otherwise, sample representativeness is uncertain. *"As representative as possible"* is not sufficient. Please specify sampling weights or quotas used (e.g., age, gender, income).

3) The use of "marine biodiversity" and "local fisheries revenue" as attributes may involve correlated perceptions (both positive ecosystem services).

4) The design's experimental realism should be discussed. Were visualizations or images used to communicate the concept? If so, please show them in the Appendix for clarity and reproducibility.

5) There is no mention of an attention or dominance test (e.g., a consistency check). Without it, internal validity of responses cannot be assessed.

6) Discuss potential self-selection bias: Did environmentally concerned individuals over-respond? This can systematically raise WTP.

7) The ZINB model is innovative for analyzing status-quo choices. However, variable scaling and coding should be presented in more detail (e.g., attitude scale, income normalization).

8) Table 3 is useful, but it could be placed in the Appendix. The paragraph at the begin of the Section suffices to summarize the descriptive statistics.

9) The Wald tests are appropriate but under-explained. Please clarify the null hypothesis (pages 14-15, 15-16).

10) Figure 6 and Table 6 are informative but repetitive. Please consider keeping in the main text only the Table.

11) Cross-references: some tables/figures are cited before their introduction. Please reorder.

12) Section 4.1 correctly notes the relative homogeneity of preferences. However, the claim that this *"challenges the dominant view of fragmented acceptability"* is overstated given the small sample sizes per region.

13) The interpretation of recycled-steel preferences as *"territorial sensitivity to circular economy"* is plausible but speculative. Consider testing correlations with education or employment in industrial sectors.