Response to Associate Editor

Editor's comments appear in italics, **our responses appear in boldface blue text**.

Dear authors,

Thanks for the revised version of the manuscript as well as for answering all the reviewers' comments and the interactive public comment. I would like to address that I do agree with the reviewers in some if not all of the main aspects that make the manuscript unpublishable at this stage. Particularly, after looking at your responses and the track-changes version, I find that:

1. The manuscript is still quite focused on describing the response of the model by looking at specific variables but I cannot see that clear specific research questions are being asked. An example of this is the abstract: more than half (the second half) of it addresses the specific results of your findings (lines 9-17) but then what? Can we trust these results or how uncertain they are? Is local meteorology really being affected?

Thank you for clearly expressing this concern. In light of this and other questions raised by the associate editor and reviewers, we have reformulated the manuscript to focus on the sensitivity of micrometeorological impacts of wakes to the amount of added turbulence kinetic energy in the Fitch wind farm parameterization and included new figures showing how the results are sensitive to the amount of added TKE, to hopefully guide future research efforts. This focused research question is reflected in the new title, "Simulated Meteorological Impacts of Offshore Wind Turbines and Sensitivity to the Amount of Added Turbulence Kinetic Energy", a revised introduction and discussion of the results, as well as revised and/or additional Figures (9 through 22).

We have highlighted the research questions being investigated in our analysis in the Introduction:
"Given the scenario above, the following research questions guide this study:

- **What are the year-long impacts of offshore turbines on simulated local meteorology?**
- **How does atmospheric stability influence the results?**
- **How does varying the amount of added TKE in the WRF wind farm parameterization (WFP) affect the results of the above questions?**
- **Can a reliable method be developed to automatically estimate wake characteristics from WRF WFP simulations?**
- **What is the relationship between simulated boundary-layer height and the extent of simulated wind plant wakes?"**

Finally, the question "Is local meteorology really being affected?" is a question that simply cannot be answered with a simulation-based study. However, several simulation-

based studies have been published on different aspects of wakes, demonstrating the broad interest in simulation-based assessments of local meteorology. For example, Golbazi et al. (2022) carry out a simulation-based study in this area over three months. To assess possible (entirely model-based) impacts of wakes on upwelling, Raghukumar et al. (2022, 2023) carry out simulation-based studies.

2. The choice of the setup. I understand this was the best setup from the 16 setups compared in Bodini et al. (2024) but I guess that that study only looked at setups with one WFP under one WFP setting? The challenge here is that your focus is on the effect of this WFP and it becomes really difficult to judge whether the impact of the wind turbines are much different when using other WFP or other WFP settings. So the question is also whether the title of the manuscript does really reflect what you describe in your work. I think both reviewers are really concerned about this issue.

We have changed the title of the paper to "Simulated Meteorological Impacts of Offshore Wind Turbines and Sensitivity to the Amount of Added Turbulence Kinetic Energy" to more accurately reflect the new focus on the impact of the amount of added TKE. Of course other model setups may have slightly different results, but it would not make sense to carry out large simulations for a setup that, in the no-wind-farm scenario, did not agree well with the available observations.

I also agree with you in that, e.g., 25% TKE is not a settle number, but that is perhaps not the main point. The main point is whether the impact on the variables you study will be significantly different if you use 33% or 25% or similar. I am not sure how to address this challenge (if you should do other simulations or similar) but it is a strong concern.

The original version of our manuscript used only the results with 100% TKE. To address this concern about the impact of a range of TKE values, our revised version now includes the same assessments with 0% TKE. These two values represent the bounds of possible values, and so bound the range of possible impacts.

3. You add some results of Rosencrans et al. (2024) to justify your choice of setup. First, I think that somewhere in the introduction you should briefly tell what are the differences between this work and that of Rosencrans et al. (2024).

We have added a sentence to the introduction:
"This focus on meteorology distinguishes this contribution from that of Rosencrans et al. (2024), who focus on hub-height wind speed and power production impacts."

Second, the sensitivity study seems very short and difficult to scale to one year.

We are puzzled at this suggestion that one year is a short time for a sensitivity study considering that Golbazi et al.'s sensitivity study (2022) uses three months, Pryor and

Barthelmie (2021, 2024) use 55 days. Our one year significantly extends the record of consideration.

Third, and related to the community comment, could the subtle impacts you mentioned this latter study found be related to the way the simulations are performed? By “fixing” SSTs, impacts of close to surface variables might be minimal given this type of setup. I understand the challenge of coupling with a wave model, but then what if it does have a strong influence? Can we perhaps use a slab surface model over water instead and not necessarily coupling if we need to study effects close to surface? The point here, again, is that you are examining the local and close to surface impacts of your simulations but this could potentially be greatly impacted by the way you simulate them. Perhaps for analyzing wake areas and the extend of wakes, the simulations as they were carried out can be of great use but can they really be used to study these local/surface effects?

We agree that the possible impact of the surface is an interesting area of future research, and we are in fact working on coupled simulations. (In general, the coupled simulations minimize wake effects even more than what has been shown here.) We appreciate that in other correspondence the editor has agreed that coupled models are not necessary at this stage of the work. This present work advances the science by assessing seasonality (beyond the one season or 55 days covered in previous studies), and future work should assess the impact of ocean-atmosphere coupling. Such an advance is beyond the scope of this paper, and we have added a full paragraph to the Conclusions to suggest this area as future work.

References

Golbazi, M., Archer, C.L. and Alessandrini, S., 2022. Surface impacts of large offshore wind farms. *Environmental Research Letters*, *17*(6), p.064021.

Pryor, S.C., Barthelmie, R.J. and Shepherd, T.J., 2021. Wind power production from very large offshore wind farms. *Joule*, *5*(10), pp.2663-2686.

Pryor, S.C. and Barthelmie, R.J., 2024. Wind shadows impact planning of large offshore wind farms. *Applied Energy*, *359*, p.122755.

Raghukumar, K., Chartrand, C., Chang, G., Cheung, L. and Roberts, J., 2022. Effect of floating offshore wind turbines on atmospheric circulation in California. *Frontiers in Energy Research*, *10*, p.863995.

Raghukumar, K., Nelson, T., Jacox, M., Chartrand, C., Fiechter, J., Chang, G., Cheung, L. and Roberts, J., 2023. Projected cross-shore changes in upwelling induced by offshore wind farm development along the California coast. *Communications Earth & Environment*, *4*(1), p.116.