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

Dear authors,
I have two comments regarding your manuscript:

1 Possible feedbacks from the ocean surface

In my opinion, the manuscript overlooks a crucial conceptual point that could significantly impact the manuscript results and conclusions. In the WRF model, the state of the land surface is controlled by a land surface model. Over the ocean, such a model is not often included. In your simulations, the namelist.input file shows that sea surface temperatures (SSTs) are specified from an input file. Using slowly varying but fixed SSTs throughout the simulation could lead to inaccuracies in calculating changes in heat fluxes and 2-meter temperatures and possibly other derived quantities. These inaccuracies result from omitting essential thermodynamic feedback processes from the ocean to the surface layer above. This factor must be included in your manuscript to ensure a fair discussion of your findings.
Thank you for emphasizing this point. While we did mention the important role of coupling in our original conclusion, we have now expanded this discussion to directly address ocean mediation of wake effects. Our preliminary (unpublished) work with coupling WRF with a wave model and an ocean model indicates that, depending on the depth of the ocean mixed layer in the ocean model, the ocean may or may not react to minimize the changes caused by wind farms.
Our new text states:
"Further, because these present simulations are not coupled with a wave model and ocean model, other feedbacks between the ocean and atmosphere may be relevant. Over water, wind plant wakes may influence ocean dynamics (Raghukumar et al., 2022, 2023; Liu et al., 2023), including upwelling. Therefore, coupling with wave and ocean models could provide insight into potential wake impacts on the ocean. Daewel et al. (2022) considers the impact of offshore wakes on primary production, but additional analysis on surface currents would provide a more complete picture of wake impacts. The ocean's response may also mediate these effects of wakes on surface meteorology, as suggested by the simulations of Fischereit et al. (2022b) in the North Sea. Extended simulations, such as those shown here, with a coupled atmosphere-ocean-wave model, could provide more accurate insight into the ocean's role in modulating wake impacts. Such work is ongoing."

A possible mechanism in nature will be the following. In the stable case with strong winds, your results show a change in the heat from the air to the water of the order of 3 W m−2. This change in flux is positive downward, thus possibly increasing the skin temperature of the sea surface. This temperature change will decrease the vertical temperature gradient, which results in a reduced heat flux. The reduced heat flux could also alter the 2-meter temperature and the surface layer's stability. Thus, in nature, the ocean could respond to minimize the changes caused by wind farms. Or not. It could be argued, however, that the ocean has a large heat capacity and, thus, an excess of 3 W m−2 will quickly be mixed in the water column without altering the ocean's temperature. This will be linked to the stability of the ocean's surface layer. Knowing which process will dominate is only possible with measurements and accurate simulations, including the thermodynamic effect on the ocean surface. Similar processes exist in the

simulation of tropical cyclones, which often consider the possible ocean surface changes.
Thank you for the nice example demonstrating that the depth of the ocean mixed layer will play a large role in mediating wake response. We are also looking forward to having observations and/or coupled results to potentially confirm this!

In your article, you cite the work of Golbazi et al. (2022), which carried out similar shorter WRF model simulations with various sizes of wind turbines. From their methods and namelist, I can also see that their simulations were done with fixed (but spatially more precise) SSTs. So, their results and discussion also disregard the possible effects of the surface ocean's response to changes in surface fluxes and temperatures. So, the article cannot be used to substantiate your results.
We do not cite Golbazi et al. in our conclusion paragraph regarding ocean-wave-atmosphere coupling.

Through the results and discussion section of your manuscript, I often find that you mix land and offshore publications. The processes are different in these two environments in nature and the models and should not be mixed.
While we agree that many processes offshore are different from those onshore (i.e., the seasonal cycle of stability is stronger offshore while the diurnal cycle of stability is stronger onshore), we think that the fundamental physics governing these processes are the same. For example, Hogstrom and Smedman did very nice work drawing an analogy between offshore and onshore winds, in their explanation of offshore low-level jets that develop from decoupling of flow from onshore to offshore similar to the evening decoupling of the atmospheric boundary layer over land (Smedman et al., 1993, 1995, 1996). If there are specific locations where Dr. Hahmann finds inappropriate references, we will be happy to respond to these specific instances.

The issue of the possible impact of fixed SSTs should be discussed and addressed in your manuscript. This discussion is crucial for the reliability of your findings. It is outside the scope of your manuscript, but WRF model simulations, including the effects of a slab ocean, are possible. Running with a fully coupled ocean will be even better but expensive.
We certainly agree that coupled simulations are needed and this work is ongoing but outside of the scope of this particular paper. This particular contribution is envisioned as a first step. A planned follow-on paper would investigate this same set of simulations with coupled modeling. We have expanded a paragraph in the conclusions to more thoroughly discuss coupled modeling.

2 Dependence of your results on PBL and wind farm parameterization

While your results are exciting, they represent only one possible scenario with one WRF PBL scheme and one wind farm parameterization. Recent publications have shown that the impact of wind farms on the atmosphere is highly dependent on the PBL and wind farm parameterization used. The paper should emphasize this point and acknowledge that observations have yet to verify most aspects of the simulated impacts of large wind farms on the atmospheric flow.
We have added a paragraph in the conclusions to point out the dependence on PBL scheme:

"Further, simulated winds (Draxl et al., 2014; Bodini et al., 2024a; Liu et al., 2024) and simulated wakes (Rybchuk et al., 2022) show dependence on the PBL scheme chosen for the model simulations. At the moment, the Fitch wind farm parameterization is coupled only with the MYNN PBL scheme used here and with the 3DPBL scheme (Kosović et al., 2020; Juliano et al., 2021). Future work could assess how micrometeorological responses to wind farm wakes depend on the choice of PBL scheme."

Statements like the one in the abstract, "Offshore wind energy projects are cur- rently in development off the east coast of the United States and will likely influence the local meteorology of the region." should be avoided. It is all a question of degrees and assessment of significance.

Respectfully, we have received many many specific questions about micrometeorological impacts of offshore wind projects from industry and from the public, so it is important to acknowledge that impacts may occur. We have softened the sentence from "will likely" to "may".

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