REVIEWER 1

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

The manuscript presents an additional analysis of a subset of the simulations conducted previously by the team and published in Rosencrans et al. (2024). The current analysis uses only the simulations that were conducted with 100% of the added TKE by the turbines available, which was the default value in the Fitch parameterization in older versions of the WRF. The default value in new versions of the WRF is 25%, not 100%. There is no justification in the paper as to why the value of 25% was not used.
By contrast, it is known that using 100% causes an overestimation of added TKE at the grid cells with the turbines. For example:

1. *Eriksson et al. (2015) report excessive TKE by about a factor of 2 (their Figures 15 and 19);*
2. *Abkar and Porté-Agel (2015) find that TKE is overestimated by 50%-200% using the 100% factor (their Figure 5);*
3. *Pan and Archer (2018) find that using the 100% value causes an overestimation of at least a factor of two in several WRF simulations of commercial-scale offshore wind farms (their Fig. 6);*
4. *Archer et al. (2022) propose the 25% value because using 100% causes an overestimation by up to 300% in TKE (their Figure 6, case 4).*

As the current manuscript uses the 100% value, which is unjustified and likely to cause incorrect distributions of heat and momentum fluxes, the manuscript as is should not be published. The team needs to rerun the simulations using the 25% value or, at the very least, rerun the simulations with the 25% value for selected months (say one per season) and present a sensitivity analysis of the results.

We thank the reviewer for their time and consideration in reviewing our manuscript. The issue of the correct amount of turbine-added TKE is an area of active research and scientific discussion, so we are surprised at the reviewer's stance that 25% is a settled issue, especially since the first three papers they cite predate the TKE bug fix which may have influenced the conclusions. The Archer et al. (2020) recommendation of 25% was based on large-eddy simulations of one turbine in neutrally-stratified conditions and is not likely to be representative of all circumstances in which wind farm parameterizations will be used.

Further, several papers (not from our research group) have recently concluded that the 25% value recommended by Archer et al. (2022) does not perform optimally:

1. Ali et al. (2023) state that "Overall, wind speed and TKE magnitudes predicted by Fitch's parameterization were close to the measured values. This result does not necessarily contradict other studies that found excessive turbulence generation using Fitch's parameterization, because the current comparison is local to the FINO-1 mast and the transect

flights over the Gode Wind farms. However, setting α = 0.25 was not optimal in the conducted experiments."

2. Larsén and Fischereit (2021) state that "It also remains inconclusive which correction factor should be used in connection with the turbine-induced TKE generation in the Fitch scheme: we only tested two factors (1 and 0.25) here and we observe a better performance when using α = 1 than α = 0.25, which does not support the conclusion from Archer et al. (2020)."

Further, other groups that have implemented versions of the Fitch wind farm parameterization into other mesoscale numerical weather prediction simulation codes (Akhtar et al., 2024; Chatterjee et al., 2016; van Stratum et al., 2022), and have also chosen to use 100%. Our approach is not an outlier in the scientific community and is justified.

Finally, the reviewer requests an expensive sensitivity study. A short sensitivity for a range of wind speeds was already conducted and presented in Rosencrans et al. (2024), Supplemental Figure 1, which showed subtle impacts on the heat and moisture fluxes. We reproduce that figure here for the reviewer's convenience:

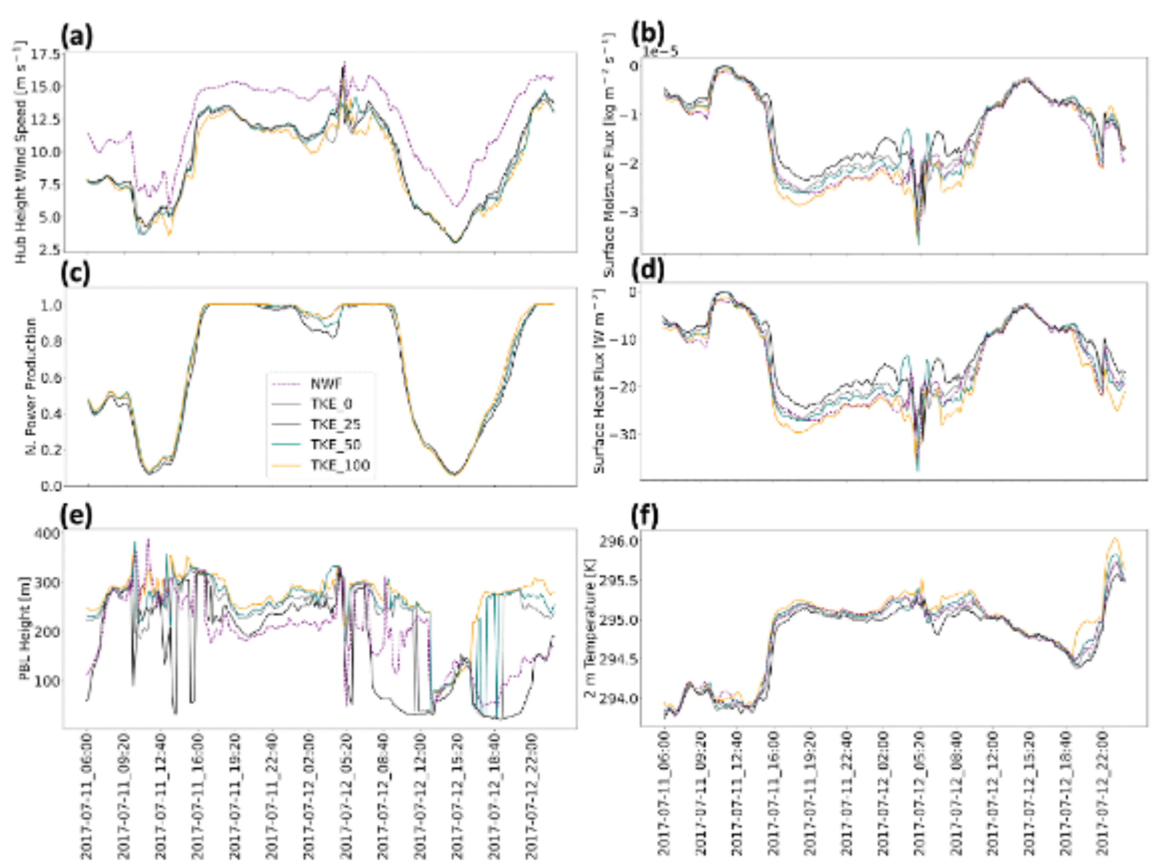


Figure A1 The effects of modifying the amount of turbulent kinetic energy (TKE) during test runs. Panels show (a) hub-height wind speed, (b) surface moisture flux, (c) normalized power productionv (d) surface *heat flux, (e) planetary boundary layer (PBL) height, and (f) 2 m temperature. Values are collected from a point centered on the RIMA block. Power production is the sum of all cells containing wind turbines. TKE_100 is shown in orange, TKE_50 in blue, TKE_25 in gray, TKE_0 in black, and NWF in purple dashes.*

Given these very small differences, we find the considerable computational expense of such a sensitivity study to be unjustified.

Regardless, we should have included some discussion of the fact that this topic is an active area of research. Therefore, we include the following text in the manuscript starting at the end of Section 2, just before Section 2.1:

> **The topic of how much turbine-generated turbulence to incorporate into mesoscale wind farm parameterizations is an active area of research. The initial default value recommended by Fitch et al. (2012) was 100%. In later work, Archer et al. (2020) recommended a value of 25% based on their large-eddy simulations of an individual turbine in neutrally-stratified conditions. More recent comparison with field observations, using the bug fix and corrected TKE advection (Larsén and Fischereit, 2021; Ali et al., 2023) both suggest that 100% added TKE yields better agreement with observations. Given this uncertainty in the literature and the experience of the sensitivity study of Rosencrans et al. (2024) (their Supplemental Figure A1b and d) that the amount of added TKE makes a very subtle impact on surface fluxes, we return to the original default value of 100% added TKE for these simulations.**

Minor issues

1. *It is unclear why the runs named "d" were separated out and studied here. The wind speed (0-3 m/s) is below the cut-in wind speed of the turbines, thus there is no power extraction in these runs. The results should look identical to those of the NoWF, but they do not, possibly because some turbines may be experiencing wind speeds locally that are above the cut-in. Nonetheless, I do not see any value in the analysis of these results.*

In fact, the results for wind speeds between 0-3 m/s should not look identical to the NWF runs because there is a standing thrust coefficient included in the WRF wind farm parameterization that is still activated even at wind speeds below cut-in. See lines 304/305 of the WRF code at https://github.com/wrf-model/WRF/blob/master/phys/module_wind_fitch.F. The value of this analysis is in assessing the effect of the drag of the inactive blades as represented by the standing thrust coefficient.

2. *Figure 4: do not show QKE but TKE. QKE is only used inside the WRF model but TKE is the well-known, physically-meaningful variable of interest. Same for the discussion in Section 4.4, focus on the real variable TKE, not on the WRF-specific QKE.*

QKE and TKE differ by a factor of two as explained in section 2.1. Regardless, the revised version of the manuscript will modify the figures and discussion to correct for this factor of two to focus on TKE.

3. *Eq. 1: The flux should be the* ***virtual*** *heat flux, not just the heat flux, typo perhaps. Also, I assume this is calculated from the WRF output fields, but it is unclear which level(s) was chosen for the mean theta_v.*

We have corrected the equation and the text to note that we calculated the mean value of virtual potential temperature from the 2-m temperature, surface pressure, and surface water vapor mixing ratio. We use heat flux (which is available from WRF), not virtual heat flux (which is not available).

4. *Remove the sentence at line 225: your results are indeed the same as those of Rosencrans et al. (2024), you can't say that they are confirming them.*

The reviewer is correct, we have revised the sentence to:

> **During unstable conditions, wind speeds are replenished faster due to increased mixing from aloft, which reduces the extent of the wake, as also reported in Figure 12 of Rosencrans et al. (2024).**

5. *Explain why there is a slight increase in 10-m wind speed at the wind farms in stable conditions. What mechanism can cause this unusual finding? Is it possible that it has to do with the excessive TKE from the 100% coefficient?*

This increase in 10-m wind speed in stable conditions is not "unusual", but has been observed in long-term measurements of wake effects in stable conditions as reported in Bodini et al., (2021), their Figure 4 and in the measurements of Rajewski et al. (2013), their Table 2. It has also been seen in stably-stratified large-eddy simulations (Vanderwende et al., 2016). When stably-stratified flow encounters an obstacle (such as a wind turbine, which is a porous obstacle but an obstacle nonetheless), flow is diverted around the obstacle and accelerates to fulfill mass conservation. In this case, the flow is diverted not just around the obstacle of the wind turbine rotor disk but also under it.

6. *Line 294: if the downward heat flux is reduced, thus less heat comes down to the surface from the layer above it, how come "more heat is transferred to the surface"?*

Thank you for catching this typo and unclear statement. We have corrected the text to

> **"During stable conditions, heat flux is negative. When heat flux is moderately reduced within the wind plant, it becomes MORE negative implying that the cooling typical of stable conditions slows, as is also seen in the T2 changes of Figure 11. We also observe an increase in heat flux downwind of the wind plant of around 1.5 W m-2 during stable conditions, implying that the cooling typical of stable conditions accelerates. No downwind effect on heat flux occurs during neutral and unstable conditions."**

7. *Figure 13: the square patterns are concerning … if they are truly due to the fact that there are more turbines in the grid cells along the lines (line 321), then why are we not seeing a similar pattern in the wind speed deficits, only in the added TKE?*

The difference between the effect of TKE and the effect of the wind speed deficits is related to the magnitude of the changes. The added TKE at hub-height as seen in Figure 13 is more than a factor of two greater than the values of TKE in the NWF simulations, and so the addition (especially for cells with multiple turbines) is a factor of two larger than the ambient background values. In contrast, the wind speed deficit introduced by the wind farm effect is smaller than the ambient wind speed background values, and so the grid pattern is not clearly visible. The grids are slightly visible in other quantities that have smaller ambient background values compared to the wind-farm-induced differences, like the heat flux in stable conditions for fast winds (Figure 12f).

8. *All figures: add units*

We have double-checked all figures for units and realized they were inadvertently omitted. The revised manuscript has them added to Figure 6 (within the wind roses), and to the color bars in Figures 9, 10, 11, 12, 13, 14, 15.

References

Akhtar, N., Geyer, B., and Schrum, C.: Larger wind turbines as a solution to reduce environmental impacts, Sci. Rep., 14, 6608, https://doi.org/10.1038/s41598-024-56731-w, 2024.

Ali, K., Schultz, D. M., Revell, A., Stallard, T., and Ouro, P.: Assessment of Five Wind-Farm Parameterizations in the Weather Research and Forecasting Model: A Case Study of Wind Farms in the North Sea, Mon. Weather Rev., 151, 2333–2359, https://doi.org/10.1175/MWR-D-23-0006.1, 2023.

Archer, C. L., Wu, S., Ma, Y., and Jiménez, P. A.: Two Corrections for Turbulent Kinetic Energy Generated by Wind Farms in the WRF Model, Mon. Weather Rev., 148, 4823–4835, https://doi.org/10.1175/MWR-D-20-0097.1, 2020.

Bodini, N., Lundquist, J. K., and Moriarty, P.: Wind plants can impact long-term local atmospheric conditions, Sci. Rep., 11, 22939, https://doi.org/10.1038/s41598-021-02089-2, 2021.

Chatterjee, F., Allaerts, D., Blahak, U., Meyers, J., and van Lipzig, N. p. m.: Evaluation of a wind-farm parametrization in a regional climate model using large eddy simulations, Q. J. R. Meteorol. Soc., 142, 3152–3161, https://doi.org/10.1002/qj.2896, 2016.

Larsén, X. G. and Fischereit, J.: A case study of wind farm effects using two wake parameterizations in the Weather Research and Forecasting (WRF) model (V3.7.1) in the presence of low-level jets, Geosci. Model Dev., 14, 3141–3158, https://doi.org/10.5194/gmd-14-3141-2021, 2021.

Rajewski, D. A., Takle, E. S., Lundquist, J. K., Oncley, S., Prueger, J. H., Horst, T. W., Rhodes, M. E., Pfeiffer, R., Hatfield, J. L., Spoth, K. K., and Doorenbos, R. K.: Crop Wind Energy Experiment (CWEX):

Observations of Surface-Layer, Boundary Layer, and Mesoscale Interactions with a Wind Farm, Bull. Am. Meteorol. Soc., 94, 655–672, https://doi.org/10.1175/BAMS-D-11-00240.1, 2013.

Rosencrans, D., Lundquist, J. K., Optis, M., Rybchuk, A., Bodini, N., and Rossol, M.: Seasonal variability of wake impacts on US mid-Atlantic offshore wind plant power production, Wind Energy Sci., 9, 555–583, https://doi.org/10.5194/wes-9-555-2024, 2024.

van Stratum, B., Theeuwes, N., Barkmeijer, J., van Ulft, B., and Wijnant, I.: A One-Year-Long Evaluation of a Wind-Farm Parameterization in HARMONIE-AROME, J. Adv. Model. Earth Syst., 14, e2021MS002947, https://doi.org/10.1029/2021MS002947, 2022.

Vanderwende, B. J., Kosović, B., Lundquist, J. K., and Mirocha, J. D.: Simulating effects of a wind-turbine array using LES and RANS, J. Adv. Model. Earth Syst., 8, 1376–1390, https://doi.org/10.1002/2016MS000652, 2016.

UPDATE TO REVIEWER 1 RESPONSE:

We have noticed that our previous answer to the reviewer's minor issue #6 had an error. The correct answer is below, and we have updated the text in the draft accordingly:

Line 294: if the downward heat flux is reduced, thus less heat comes down to the surface from the layer above it, how come "more heat is transferred to the surface"?

Thank you for catching this unclear statement. We have corrected the text to

"During stable conditions, heat flux is negative (downward). When heat flux is moderately reduced within the wind plant, it becomes more negative implying that more heat is transferred to the surface, which is consistent with the T2 changes of Figure 11. We also observe an increase in heat flux downwind of the wind plant of around 1.5 W m-2 during stable conditions, implying that the cooling typical of stable conditions accelerates. No downwind effect on heat flux occurs during neutral and unstable conditions."

REVIEWER 2

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

General comments:

This article explores the impact on a range of meteorological fields by the presence of large wind farm cluster off the USA east coasts using the WRF mesoscale model and using the WFP wind farm parameterization, using the 100% added TKE option.

Overall I find the article outlines a repeatable methodical approach and describes results, but lacks clarity on the motivation for the investigation and lacks discussion on the limitations of the method. What conclusions are to be drawn beyond describing the response of a model (WRF) in these "no wind farm" and "with wind farm" simulations? What research question is being asked? What is the hypothesis being tested? Please revise to address this.
We thank the reviewer for their time and consideration in reviewing our manuscript. We have added the following paragraph to the Introduction:
"Given the scarcity of comprehensive offshore observations along the U.S. East Coast, this study aims to complete the first year-long assessment of how modeled offshore wind plants influence the modeled local environment. We achieve this by comparing WRF model (Skamarock et al. 2021) simulations with and without wind plants included. Our analysis focuses on the Massachusetts-Rhode Island offshore wind lease area, where we quantify the difference in hub-height and 10-m wind speed, boundary-layer height, 2-m temperature, surface heat flux, and TKE at the surface and at hub height. Our expectation is to demonstrate that different stability conditions are a key driver of the simulated micrometeorological impacts, and that these impacts also vary with different wind speeds, as wind turbine operation changes. Furthermore, we aim to assess the relationship between boundary-layer height and the extent of wind plant wakes, hypothesizing that deeper boundary layers will limit the extension of these wakes."
As detailed in our other responses to the reviewer's comments, we have also added more discussion throughout the manuscript, including the conclusions.

The choice of one WRF set-up, one WRF wind farm parameterization, and one setting for the added TKE option, is a severe limitation of the article. It means that the whole paper becomes a description of model results, rather than focussing on what might actually happen in nature itself.
While we agree that the question of what actually happens in nature itself is extremely interesting, a very limited set of observations are available to quantify the real-world impact of wakes. In the absence of extensive observations, modeling studies such as the one presented here are needed, and typically used, to fill the knowledge gap. The modeling tools used here have repeatedly demonstrated results consistent with available observations in other locations (i.e., the comparison of the modeling studies of Xia et al. with the observations of Zhou et al., the intercomparison of aircraft observations

with modeling studies of Siedersleben et al. 2018, 2020, and the validation studies of Larsén and Fischerei 2021 and Ali et al. 2023). However, the micrometeorological impacts of wakes over a complete annual cycle in this region with intensively planned offshore wind development has not yet been investigated, hence the effort here.
Regarding the choice of one WRF set-up: this set-up was based on a comparison of 16 set-ups in this region (Bodini et al. 2024 ESSD) and was the best-performing set-up. We have added a sentence to the beginning of Section 2:
"We note that the WRF setup used here resulted from a comparison of 16 different WRF setups against an observational dataset; this setup was the best performer (Bodini et al., 2024)."

The manuscript needs to be revised to include a comprehensive discussion of the limitations of WRF-WFP, and what that might mean for the given results.
We have added a discussion to the conclusion about the existing validation of WRF-WFP and other ongoing research efforts that might affect the given results.
"Of course, this study relies on the accurate representation of wakes in the Fitch WRF wind farm parameterization. While wakes simulated with this parameterization compare reasonably well with the limited sets of observations available (Lee and Lundquist, 2017; Siedersleben et al., 2018b, a, 2020; Ali et al., 2023; Larsén and Fischereit, 2021), the availability of observations of wake effects at multiple distances and heights from wind farms, especially offshore, is limited. Ongoing experiments such as AWAKEN (Moriarty et al., 2024) may provide more extensive datasets to support modifications to wind farm parameterizations in mesoscale models. Additionally, comparisons of these mesoscale representations to more finely resolved large-eddy simulations of wind farms (Vanderwende et al., 2016; Peña et al., 2022) may suggest other improvements, although these comparisons should be carried out for a range of atmospheric stability conditions and wind farm geometries. Particular attention should be paid to effects on surface meteorology as well as dynamics directly relevant to wind turbine power production".

Please include more justification for the model set-up, for example, why only a one year simulation? How might a longer period or different year impact the results?
While of course a longer set of simulations would be interesting, the work presented here includes a complete annual cycle, which goes far beyond other wake studies in this region, i.e., 55 days (Pryor et al. 2021) or three months (Golbazi et al. 2022) investigated in previous work. This particular year was chosen, as discussed in Rosencrans et al. (2024), because of the availability of lidar data for validation of the no-wind-farm simulation.
Because this year includes a range of stability conditions and wind speeds typical for this region these results are not particularly sensitive to the choice of this particular year.

I think there is a lack of physical mechanisms, and where mechanisms are conjectured, no model fields are used to back these up (see specific comments).
We list below the main physical explanations we provide in the paper about the (modeled) changes in atmospheric variables. We have added some and expanded most of the existing ones, on top of what already discussed in the specific comments below:

- **Hub-height wind speed:**
  "Within the wind plant, wind speeds are reduced by up to 2.7 m s-1 in stable conditions, and up 1.5 m s-1 in unstable conditions, as turbines extract momentum from the flow."
  "During unstable conditions, wind speeds are replenished faster due to increased mixing from aloft (Abkar et al. 2013), which reduces the extent of the wake"
- **10-m wind speed:**
  "The acceleration in stable conditions can be understood as acceleration around an obstacle in stably-stratified flow: when the flow cannot pass through the rotor disk and cannot rise above the rotor disk due to stable stratification, it must pass through a more confined region (under the rotor disk) and therefore accelerates. In neutral or unstable conditions, there is no such constraint and so the flow does not need to accelerate under the rotor disk."
  "The spatial extent of the wake is smaller for unstable conditions than for neutral and stable conditions: during unstable conditions, wind speeds are replenished faster due to increased mixing from turbines above, which reduces the extent of the wake."
- **Temperature:**
  "During stable conditions, turbines mix warmer air from aloft down to the surface, resulting in what appears to be a temperature increase but is really just redistribution of heat (as also discussed in Fitch et al. 2013, Siedersleben et al. 2018, among others). In unstable conditions, the boundary layer is already well mixed, so that any mixing by wind turbines is simply remixing a well-mixed layer."
  "For stably stratified mesoscale simulations onshore (Texas), Xia et al. 2019 find that the turbine-added turbulence drives the surface warming signal by enhancing vertical mixing. In contrast, the turbine drag component causes the remote downwind surface cooling by reducing shear and promoting near-surface thermal stratification. A similar process occurs here, most visible in the stably stratified conditions"
- **Surface sensible heat flux:**
  "Heat fluxes are positive during unstable conditions, so a reduction in heat flux corresponds to less heat being transferred upwards, which is consistent with the (slight) reduction in 2-m temperature during unstable conditions. During stable conditions, heat flux is negative (downward). When heat flux is moderately reduced within the wind plant, it becomes more negative implying that more heat is transferred to the surface, which is consistent with the T2 changes of Figure 11. We also observe an increase in heat flux downwind of the wind plant of around 1.5 W m-2 during stable conditions, implying that the cooling typical of stable conditions accelerates. No downwind effect on heat flux occurs during neutral and unstable conditions."
- **Hub-height TKE:**
  "The largest increases in TKE occur in the grid cells populated by turbines, where the turbulence is directly introduced by the WRF wind farm parameterization. This hub-height TKE increase rapidly erodes downwind of the wind plants. The amount of added turbulence directly relates to the number of turbines in each grid cell, resulting in a grid pattern of larger TKE values corresponding to cells with more turbines."
  "Atmospheric stability does not seem to impact the magnitude of TKE increase at hub height

(as the amount of added TKE is not a function of stability but rather of wind speed and the number of turbines)."

- **Surface TKE:**
  "During stable conditions, TKE at the surface is largely unaffected by the presence of wind turbines. Vertical mixing is suppressed during stable conditions, making it unlikely that turbulence from the turbines, injected at the rotor disk altitudes, can reach the surface. Surface TKE increases within the wind plant during neutral conditions, although changes are limited to areas close to turbines. During unstable conditions, TKE increases throughout the entire lease area, albeit by a factor of four less than at hub height, thanks to enhanced vertical mixing that causes the TKE injected at hub height to also reach the surface."
  "Downwind of the wind plant, wind speeds are reduced in the wake, which results in less TKE producing wind shear."
- **Boundary layer height:**
  "During stable conditions, PBL heights are generally lower and often within the rotor region, which results in a larger overall change in PBL height. During neutral and unstable conditions, PBL heights are generally deeper; thus, turbines are less likely to interact with air in the free atmosphere."
  "Distant from the wind plant, PBL heights are reduced by up to 45 m during stable conditions as compared to the no-wind-farm, likely due to the decreased shear in the wake of the wind plant."

The paper several times states where results confirm what is already published, as a reader I would like more clarity on what are the most novel parts of the study and what led to these novel parts being of interest for investigation. Please revise to address this.
As noted above, we have introduced a section in the introduction clearly stating the novelty of the study in looking at variability of wake impacts over an entire annual cycle. We have also revised the conclusions to emphasize the annual variability as well as the machine-learning approach demonstrated here for wake area and wake length characterization:
"We also develop and demonstrate a machine-learning approach to identify wind plant wakes, and use this method to demonstrate the relationship between boundary-layer height and both the area and length of the wind plant wake."

Latter sections seem a bit rushed.
If the reviewer is requesting additions to the conclusion, we have expanded the conclusions and discussion of the results therein considerably.

Adding to the limitation discussion, it would be good to include what would be good further studies to pursue, and what might be an approach to the difficult question of validation. Please revise to address this.
As noted above, we have added a discussion to the conclusion about the existing validation of WRF-WFP and other ongoing research efforts that might affect the given results.

"Of course, this study relies on the accurate representation of wakes in the Fitch WRF wind farm parameterization. While wakes simulated with this parameterization compares reasonably well with the limited sets of observations available (Lee and Lundquist, 2017; Siedersleben et al., 2018b, a, 2020; Ali et al., 2023; Larsén and Fischereit, 2021), the availability of observations of wake effects at multiple distances and heights from wind farms, especially offshore, is limited. Ongoing experiments such as AWAKEN (Moriarty et al., 2024) may provide more extensive datasets to support modifications to wind farm parameterizations in mesoscale models. Additionally, comparisons of these mesoscale representations to more finely resolved large-eddy simulations of wind farms (Vanderwende et al., 2016; Peña et al., 2022) may suggest other improvements, although these comparisons should be carried out for a range of atmospheric stability conditions and wind farm geometries. Particular attention should be paid to effects on surface meteorology as well as dynamics directly relevant to wind turbine power production."

Specific comments:

L16: "exceeding 100 m" -> "exceeding 1000 m"?
We have removed numbers from this sentence to avoid ambiguities.

L52: "extreme scale" , suggest changing this term. "Extreme" 10 years ago is not "extreme" today.
We have replaced it with "bigger".

L81: The sentence "determine … how .. influence the local environment", it should be reformulated to say this is modelled local environment being investigated, not the actual environment in nature.
We have changed it to "modeled environment".

L107: Please detail more about what is meant by "the model produced unrealistic wind speeds, … ". Please describe and state what it is that is unrealistic.
We have expanded this discussion of the literature:
"Vanderwende et al. (2016) suggest that added TKE is critical. In that study, when TKE generation within the wind farm parameterization is disabled, the model produced unrealistic wind speeds, wind directions, and turbulence as compared to large-eddy simulations, with too-small of values of turbulence and too large of decreases in wind speed."

L96 and Table 1: Why was this period chosen?
We have rephrased as "NOW-WAKES covers from 1 September 2019 00:00 UTC - 31 August 2020 23:50 UTC (chosen to overlap with lidar data availability in the region) at 10- minute resolution".

L94 and Figure 1: Why was the domain chosen as it is? What is the reason for the far eastward extent?
We have added a sentence explaining:
"The Rosencrans et al. (2024) domain is consistent with other datasets for this region (Xia et al., 2022; Redfern et al., 2021; Bodini et al., 2024) and was initially chosen to optimize processor partitioning for the WRF simulations."

Figure 3: It is strange to have a caption referring to a later caption.
We have changed the captions of Figures 3, 4, and 5 accordingly.

L136: In the description of the BLH definitions, what happens in transitions from one stability condition to another, is there a discontinuity in the BLH? Could the authors use a sentence or to to justify the use of the approach of Olson et al (2019) for this analysis. What is the most relevant BLH determination for a wind farm do the authors think or recommend?
Yes, during stability transitions, there may be discontinuities in the estimation of BLH by the WRF model because of the transition from one approach to another. Because these simulations use the MYNN PBL scheme, the authors recommend using the PBLH estimation approach included in that scheme (the Olson et al. (2019) approach) for consistency. This approach has performed well in comparison to observations for some case studies (Bauer et al. 2023).

Table 2: It might be better to have "region 1" and "region 2" also part of the wind speed column in this table, to remind the reader of the reasoning behind the wind speed partitioning.
We have added references to Region 1, 2, and 3 in the Table.

L162: I am a bit wary about this statement about the "tight coupling" because it suggests that everything can be explained by atmospheric stability, but there may be very important other aspects of the profile, and these might be overlooked by this approach. Please expand on the justification of the approach.
We have replaced it as "correlation" to soften the message of the sentence.

L166: "a leveling of the power production", I think a better term here would be "the rated power production being reached and not increasing further".
Changed.

L167: "To isolate" , again similar to the L162 comment. It is not just wind speed that is varying, even though you keep stability and direction within a certain band. Please discuss other things in the profile that might vary, given this constraint on stability and direction.
We have rephrased it as "To more clearly identify".

L175: Why is 1 m/s deficit chosen as the measure of a wake? Why not other measures, such as relative deficit? What are the advantages of this measure, what is the impact of different wind speeds (NWF simulations) on this wake definition?
We chose an absolute (rather than relative) definition of the wake threshold to be consistent with previous work. A relative deficit requires comparison with spatially heterogeneous unwaked fields which can make the assessment of the wake even noisier than an absolute definition. In the text, we have added an explanation:

“This wake definition is stronger than the 0.5 m s-1 threshold used in Golbazi et al. (2022); Rybchuk et al. (2022); and Rosencrans et al. (2024), and was chosen to aid in identifying contiguous wakes. A relative wake definition proved problematic by making the wake field even noisier.”

L178: Please explain why there are “not contiguous” wind speed perturbations, could they be related to the wake? How do you discount that there may be a distant response to the wind farm, perhaps oscillation in wake above and below the 1 m/s threshold that has been chosen.

The WRF wind farm parameterization is known to produce noise in wind fields similar to these remote patterns. We have added a sentence:

“The deficits at these remote locations are presumed to be numerical noise as identified in Ancell et al. (2018); Lauridsen and Ancell (2018) and discussed in Appendix F of Rosencrans et al. (2024).”

L192: “ill defined” wakes. This seems a bit subjective to me, perhaps wakes are not neat and tidy as we might expect. Please justify. And is the 15.2% of hours with “ill defined” wakes not quite a significant share of the time?

Of course we do acknowledge that this definition is by necessity somewhat subjective. To address this subjectivity and to enable the analysis to be replicated by other research groups, we have clearly defined the criteria used to make a distinction between the 85% clearly defined wakes and the 15% ill defined wakes. While 15% of the wakes is a not trivial share of the time, it is clearly a minority of the time. Further, it is consistent with other machine learning approaches used to identify wind turbine wakes in heterogeneous fields, such as 87.18% in Aird et al. (2021), although in Aird et al. they are identifying wakes from individual turbines and not wind farm wakes.

L211: Please can the authors explain why the wake is compared in wind speed across the different stability classes? Is the mean wind speed the same for the different stability classes, if not, the difference in wake deficit can be partly due to this effect.

We have addressed the roles of wind speed and stability by partitioning our results by both stability and by wind speed within the stable stratification class. We first emphasize stability classes because of the long history of observations that wakes are stronger in stably stratified conditions (e.g., see the summary in section 2.3 of Porté-Agel et al. 2020, with a sample of over 20 investigations documenting wake variability with atmospheric stability). In Figure 6 of the current manuscript we already demonstrate that the wind speed and direction distributions are different for stable vs neutral vs unstable conditions, demonstrating that faster winds occur in stable conditions. By further partitioning the stable results into the different wind speed regimes, we identify the differences in wakes due just to wind speed variation.

L214: The authors write “”due to increased mixing from aloft”, but this statement is not argued with data from the model, but appears to be more like a hypothesis for a possible, and plausible mechanism. Please justify the statement or rephrase it.

We justified the statement by including a reference to the sensitivity of wake replenishment from above (Abkar and Porte-Agel 2013).

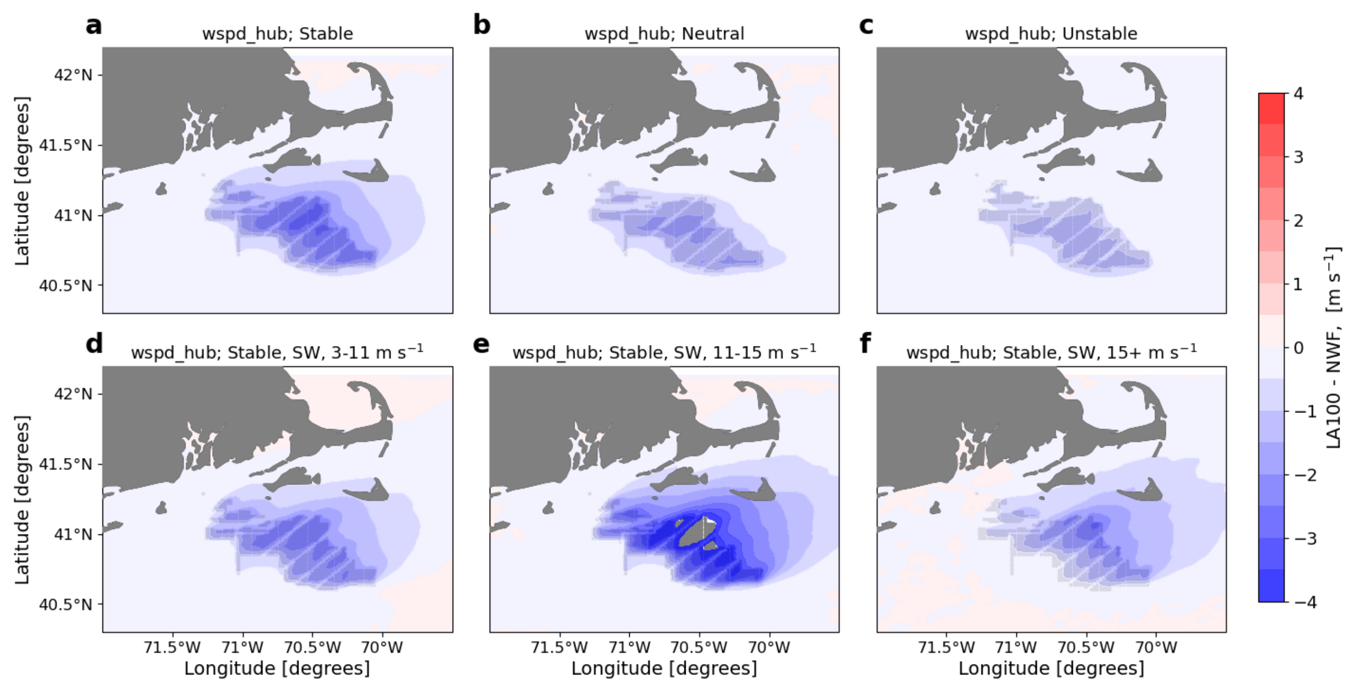
L219: It would help the reader to refer to region 3 next to the "above 11 m/s".

We have modified the sentence to read "*Hub-height wind speeds are reduced by up to 2.5 m s-1 for wind speeds in Region 2 of the turbine power curve, and up to 3.6 m s-1 for wind speeds above 11 m s-1 in Region 3 of the power curve.*"

L225-228: Does this effect also show when wind speeds are in the range 15 m/s - 25 m/s where the thrust is dropping significantly? See Fig 2b.

Thank you for this suggestion. As seen in the comparison of e) and f) below, the magnitude of the wind speed deficit decreases for wind speeds faster than 15 m s-1. We have added this figure to the appendix and expanded the discussion in the text:

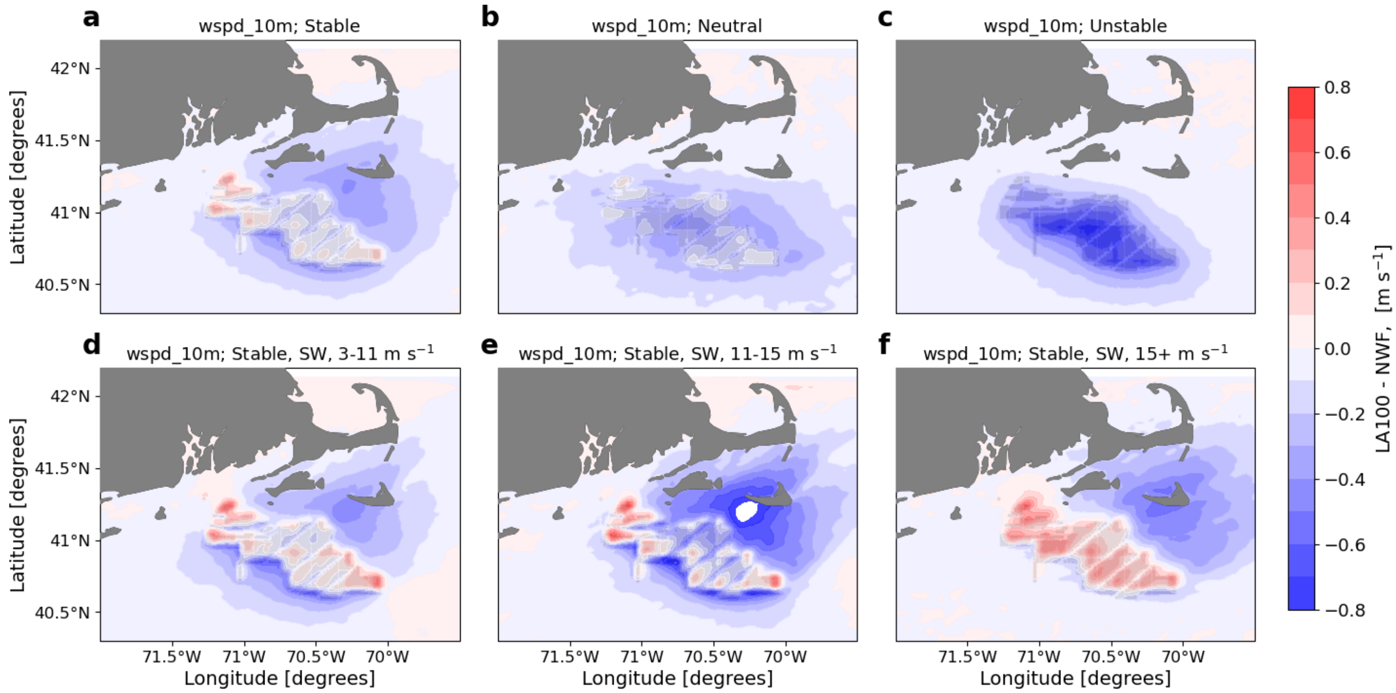
"Of note, when the wind speeds exceed 15 m s-1 when the thrust coefficient is very small, the wind speed deficit starts to decrease again (see Appendix Fig. B1)."



L233: Same question as above.

Thank you also for this suggestion. As seen in the comparison of e) and f) below, the magnitude of the 10-m wind speed acceleration increases for wind speeds faster than 15 m s-1. We have added this figure to the appendix and expanded the discussion in the text:

"For wind speeds faster than 15 m s-1, the accelerations are more widespread within the wind plant but the maximum accelerations are not faster than those in the range of 11 - 15 m s-1 (see Fig. B2)."



L240: "reduced more", more than what? Does the deficit increase, or does the absolute wind speed reduce? It reads more like the latter, but I think it is the former.

We have rephrased it to "Under stable conditions and southwesterly winds, the deficit in 10~m wind speeds increases more with increasing ambient wind speeds".

L251: Please quantify "increase slightly".

We have rephrased to "At 10 m, wind speeds accelerate slightly (less than 1 m s-1 within the wind plant during…"

L260: Temperature increases by "around 0.05 degrees". Is this significant?

As the topic sentence of this paragraph suggests, these changes are "small". We have added "only" to the sentence including "0.05 degrees".

L261-263: Are these statements conjecture or justified by model fields of fluxes? Please reformulate so it is clearer.

These statements are not conjecture but are rather consistent with a wide body of literature discussing mixing mechanisms dating back to Baidya Roy et al. 2004 and demonstrated in Fitch et al. (2012). We have reformulated the sentences as follows:

"During stable conditions, turbines mix warmer air from aloft down to the surface, resulting in a temperature increase (as also discussed in Fitch et al. (2013) and Siedersleben et al. (2018a), among others). In unstable conditions, the boundary layer is already well mixed, so that any mixing by wind turbines is simply remixing a well-mixed layer."

L289: The heading "heat flux", please clarify what kind of heat flux is being looked at. Surface heat flux, vertical heat flux, sensible heat flux, etc, etc.
We have renamed the heading to "Wind plant wake impacts on surface sensible heat flux".

L407: The use of the word "promote" infers a causal relationship, is that what is meant?
Yes, we intend to suggest a causal relationship.

References

- **Abkar, M. and Porté-Agel, F.: The Effect of Free-Atmosphere Stratification on Boundary-Layer Flow and Power Output from Very Large Wind Farms, Energies, 6, 2338–2361, https://doi.org/10.3390/en6052338, number: 5 Publisher: Multidisciplinary Digital Publishing In- stitute, 2013.**
- **Aird, J.A., Barthelmie, R.J., Shepherd, T.J. and Pryor, S.C., 2022. Occurrence of low-level jets over the eastern US coastal zone at heights relevant to wind energy. Energies, 15(2), p.445.**
- **Ali, K., Schultz, D.M., Revell, A., Stallard, T. and Ouro, P., 2023. Assessment of five wind-farm parameterizations in the Weather Research and Forecasting model: A case study of wind farms in the North Sea. Monthly Weather Review, 151(9), pp.2333-2359.**
- **Ancell, B. C., Bogusz, A., Lauridsen, M. J., and Nauert, C. J.: Seeding Chaos: The Dire Consequences of Numerical Noise in NWP Perturbation Experiments, Bulletin of the American Meteorological Society, 99, 615–628, https://doi.org/10.1175/BAMS-D-17-0129.1, 2018.**
- **Baidya Roy, S., Pacala, S.W. and Walko, R.L., 2004. Can large wind farms affect local meteorology?. Journal of Geophysical Research: Atmospheres, 109(D19).**
- **Bauer, H.S., Späth, F., Lange, D., Thundathil, R., Ingwersen, J., Behrendt, A. and Wulfmeyer, V., 2023. Evolution of the Convective Boundary Layer in a WRF Simulation Nested Down to 100 m Resolution During a Cloud-Free Case of LAFE, 2017 and Comparison to Observations. Journal of Geophysical Research: Atmospheres, 128(8), p.e2022JD037212.**
- **Bodini, N., Optis, M., Redfern, S., Rosencrans, D., Rybchuk, A., Lundquist, J.K., Pronk, V., Castagneri, S., Purkayastha, A., Draxl, C. and Krishnamurthy, R., 2023. The 2023 National Offshore Wind data set (NOW-23). Earth System Science Data Discussions, 2023, pp.1-57.**
- **Fitch, A.C., Olson, J.B., Lundquist, J.K., Dudhia, J., Gupta, A.K., Michalakes, J. and Barstad, I., 2012. Local and mesoscale impacts of wind farms as parameterized in a mesoscale NWP model. Monthly Weather Review, 140(9), pp.3017-3038.**
- **Golbazi, M., Archer, C.L. and Alessandrini, S., 2022. Surface impacts of large offshore wind farms. Environmental Research Letters, 17(6), p.064021.**
- **Larsén, X. G. and Fischereit, J.: A case study of wind farm effects using two wake parameterizations in the Weather Research and Forecasting (WRF) model (V3.7.1) in the presence of low-level jets, Geosci. Model Dev., 14, 3141–3158, https://doi.org/10.5194/gmd-14-3141-2021, 2021.**
- **Lauridsen, M. J. and Ancell, B. C.: Nonlocal Inadvertent Weather Modification Associated with Wind Farms in the Central United States,**

https://doi.org/https://doi.org/10.1155/2018/2469683, iSSN: 1687-9309 Pages: e2469683 Publisher: Hindawi Volume: 2018, 2018.

- Lee, J.C. and Lundquist, J.K., 2017. Evaluation of the wind farm parameterization in the Weather Research and Forecasting model (version 3.8. 1) with meteorological and turbine power data. Geoscientific Model Development, 10(11), pp.4229-4244.
- Moriarty, P. J., Bodini, N., Letizia, S., Abraham, A., ashley, t., Bärfuss, K., Barthelmie, R. J., Brewer, A., Brugger, P., Feuerle, T., Frère, A., Goldberger, L., Gottschall, J., Hamilton, N., Herges, T., Hirth, B., Hung, L.-Y. L., Iungo, G. V., Ivanov, H., Kaul, C. M., Kern, S., Klein, P., Krishnamurthy, R., Lampert, A., Lundquist, J. K., Morris, V. R., Newsom, R., Pekour, M., Pichugina, Y., Porté-Agel, F., Pryor, S. C., Scholbrock, A., Schroeder, J., Shartzer, S., Simley, E., Vöhringer, L., Wharton, S., and Zalkind, D.: Overview of Preparation for the American Wake Experiment (AWAKEN), Journal of Renewable and Sustainable Energy, accepted for publication, 2024.
- Olson, J. B., Kenyon, J. S., Angevine, W. M., Brown, J. M., Pagowski, M., and Sušelj, K.: A Description of the MYNN-EDMF Scheme and the Coupling to Other Components in WRF–ARW, Tech. Rep. OAR GSD-61, NOAA, https://repository.library.noaa.gov/view/noaa/19837, publisher: Earth System Research Laboratory (U.S.), Global Systems Division, 2019.
- Peña, A., Mirocha, J. D., and Laan, M. P. v. d.: Evaluation of the Fitch Wind-Farm Wake Parameterization with Large-Eddy Simulations of Wakes Using the Weather Research and Forecasting Model, Monthly Weather Review, 150, 3051–3064, https://doi.org/10.1175/MWR- D-22-0118.1, publisher: American Meteorological Society Section: Monthly Weather Review, 2022.
- Porté-Agel, Fernando, Majid Bastankhah, and Sina Shamsoddin. "Wind-turbine and wind-farm flows: a review." Boundary-layer meteorology 174.1 (2020): 1-59.
- 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.
- Redfern, S., Optis, M., Xia, G. and Draxl, C., 2021. Offshore wind energy forecasting sensitivity to sea surface temperature input in the Mid-Atlantic. Wind Energy Science Discussions, 2021, pp.1-31.
- Rosencrans, D., Lundquist, J. K., Optis, M., Rybchuk, A., Bodini, N., and Rossol, M.: Seasonal variability of wake impacts on US mid-Atlantic offshore wind plant power production, Wind Energy Sci., 9, 555–583, https://doi.org/10.5194/wes-9-555-2024, 2024.
- Rybchuk, A., Juliano, T. W., Lundquist, J. K., Rosencrans, D., Bodini, N., and Optis, M.: The sensitivity of the fitch wind farm parameteriza- tion to a three-dimensional planetary boundary layer scheme, Wind Energy Science, 7, 2085–2098, https://doi.org/10.5194/wes-7-2085- 2022, publisher: Copernicus GmbH, 2022.
- Siedersleben, S.K., Lundquist, J.K., Platis, A., Bange, J., Bärfuss, K., Lampert, A., Cañadillas, B., Neumann, T. and Emeis, S., 2018b. Micrometeorological impacts of offshore wind farms as seen in observations and simulations. Environmental Research Letters, 13(12), p.124012.
- Siedersleben, S.K., Platis, A., Lundquist, J.K., Lampert, A., Bärfuss, K., Cañadillas, B., Djath, B., Schulz-Stellenfleth, J., Bange, J., Neumann, T. and Emeis, S., 2018a. Evaluation of a wind farm

parametrization for mesoscale atmospheric flow models with aircraft measurements. Meteorologische Zeitschrift (Berlin), 27(NREL/JA-5000-73670).

- Siedersleben, S.K., Platis, A., Lundquist, J.K., Djath, B., Lampert, A., Bärfuss, K., Cañadillas, B., Schulz-Stellenfleth, J., Bange, J., Neumann, T. and Emeis, S., 2020. Turbulent kinetic energy over large offshore wind farms observed and simulated by the mesoscale model WRF (3.8. 1). Geoscientific Model Development, 13(1), pp.249-268.
- Vanderwende, B. J., Kosović, B., Lundquist, J. K., and Mirocha, J. D.: Simulating effects of a wind-turbine array using LES and RANS, Journal of Advances in Modeling Earth Systems, 8, 1376–1390, https://doi.org/10.1002/2016MS000652, 2016.
- Xia, G., Cervarich, M.C., Roy, S.B., Zhou, L., Minder, J.R., Jimenez, P.A. and Freedman, J.M., 2017. Simulating impacts of real-world wind farms on land surface temperature using the WRF Model: Validation with observations. Monthly Weather Review, 145(12), pp.4813-4836.
- Zhou, L., Tian, Y., Baidya Roy, S., Thorncroft, C., Bosart, L.F. and Hu, Y., 2012. Impacts of wind farms on land surface temperature. Nature Climate Change, 2(7), pp.539-543.

PUBLIC COMMENT 1

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".

References

- **Bodini, N., Optis, M., Liu, Y., Gaudet, B., Krishnamurthy, R., Kumler, A., Rosencrans, D., Rybchuk, A., Tai, S.-L., Berg, L., Musial, W., Lundquist, J., Purkayastha, A., Young, E., and Draxl, C.: Causes of and Solutions to Wind Speed Bias in NREL's 2020 Offshore Wind Resource Assessment for the California Pacific Outer Continental Shelf, Tech. Rep. NREL/TP–5000-88215, 2318705, MainId:88990, https://doi.org/10.2172/2318705, 2024a.**
- **Daewel, U., Akhtar, N., Christiansen, N., and Schrum, C.: Offshore wind farms are projected to impact primary production and bottom water deoxygenation in the North Sea, Communications Earth & Environment, 3, 1–8, https://doi.org/10.1038/s43247-022-00625-0, number: 1 Publisher: Nature Publishing Group, 2022.**
- **Draxl, C., Hahmann, A. N., Peña, A., and Giebel, G.: Evaluating winds and vertical wind shear from Weather Research and Forecast- ing model forecasts using seven planetary boundary layer schemes, Wind Energy, 17, 39–55, https://doi.org/10.1002/we.1555, _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1002/we.1555, 2014.**
- **Fischereit, J., Larsén, X. G., and Hahmann, A. N.: Climatic Impacts of Wind-Wave-Wake Interactions in Offshore Wind Farms, Frontiers in Energy Research, 10, https://doi.org/10.3389/fenrg.2022.881459, publisher: Frontiers, 2022b.**
- **Juliano, T. W., Kosović , B., Jiménez, P. A., Eghdami, M., Haupt, S. E., and Martilli, A.: "Gray Zone" Simulations using a Three- Dimensional Planetary Boundary Layer Parameterization in the Weather Research and Forecasting Model, Monthly Weather Review, -1, https://doi.org/10.1175/MWR-D-21-0164.1, publisher: American Meteorological Society Section: Monthly Weather Review, 2021.**
- **Kosović , B., Jimenez Munoz, P., Juliano, T. W., Martilli, A., Eghdami, M., Barros, A. P., and Haupt, S. E.: Three-Dimensional Planetary Boundary Layer Parameterization for High-**

Resolution Mesoscale Simulations, Journal of Physics: Conference Series, 1452, 012 080, https://doi.org/10.1088/1742-6596/1452/1/012080, 2020.

- Liu, K., Du, J., Larsén, X. G., and Lian, Z.: Spatiotemporal Variations of Ocean Upwelling and Downwelling Induced by Wind Wakes of Off- shore Wind Farms, Journal of Marine Science and Engineering, 11, 2020, https://doi.org/10.3390/jmse11102020, number: 10 Publisher: Multidisciplinary Digital Publishing Institute, 2023.
- Liu, Y., Gaudet, B., Krishnamurthy, R., Tai, S.-L., Berg, L. K., Bodini, N., Rybchuk, A., and Kumler, A.: Identifying Meteorological Drivers for Errors in Modeled Winds along the Northern California Coast, Monthly Weather Review, 152, 455–469, https://doi.org/10.1175/MWR-D-23-0030.1, publisher: American Meteorological Society Section: Monthly Weather Review, 2024.
- Raghukumar, K., Chartrand, C., Chang, G., Cheung, L., and Roberts, J.: Effect of Floating Offshore Wind Turbines on Atmospheric Circu- lation in California, Frontiers in Energy Research, 10, https://doi.org/10.3389/fenrg.2022.863995, publisher: Frontiers, 2022.
- Raghukumar, K., Nelson, T., Jacox, M., Chartrand, C., Fiechter, J., Chang, G., Cheung, L., and Roberts, J.: Projected cross-shore changes in upwelling induced by offshore wind farm development along the California coast, Communications Earth & Environment, 4, 1–12, https://doi.org/10.1038/s43247-023-00780-y, publisher: Nature Publishing Group, 2023.
- Rybchuk, A., Juliano, T. W., Lundquist, J. K., Rosencrans, D., Bodini, N., and Optis, M.: The sensitivity of the fitch wind farm parameteriza- tion to a three-dimensional planetary boundary layer scheme, Wind Energy Science, 7, 2085–2098, https://doi.org/10.5194/wes-7-2085- 2022, publisher: Copernicus GmbH, 2022.
- Smedman, A.S., Tjernström, M. and Högström, U., 1993. Analysis of the turbulence structure of a marine low-level jet. Boundary-layer meteorology, 66, pp.105-126.
- Smedman, A.S., Bergström, H. and Högström, U., 1995. Spectra, variances and length scales in a marine stable boundary layer dominated by a low level jet. Boundary-Layer Meteorology, 76(3), pp.211-232.
- Smedman, A.S., Högström, U. and Bergström, H., 1996. Low level jets–a decisive factor for off-shore wind energy siting in the Baltic Sea. Wind Engineering, pp.137-147