*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).**

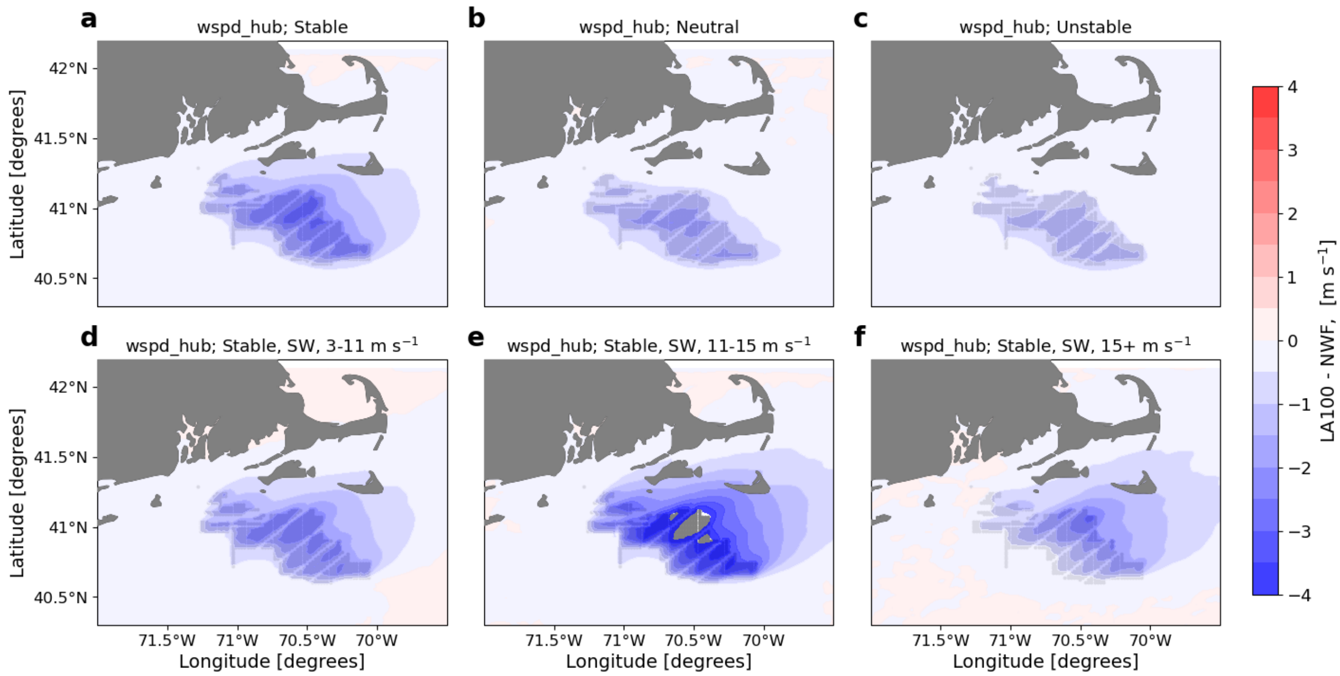
*L219: It would help the reader to refer to region 3 next to the "above 11 m/s".*

**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:**

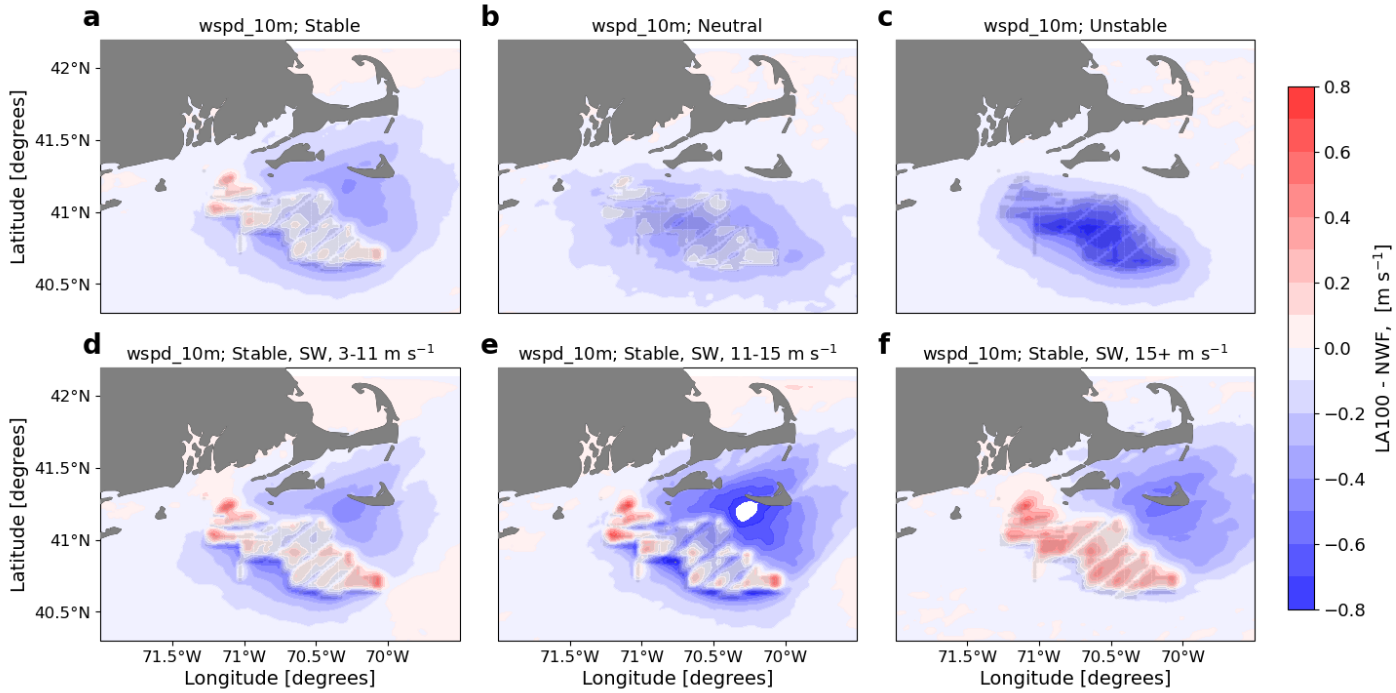
***"Of note, when the wind speeds exceed 15 m s-1 when the thrust coefficient is very small, the wind speed deficit starts to decrease again (see Appendix Fig. B1)."***




*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.**

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