

Second review of “Simulated Meteorological Impacts of Offshore Wind Turbines and Sensitivity to the Amount of Added Turbulence Kinetic Energy” by Quint, Lundquist, Bodini, and Rosencrans, submitted to Wind Energy Science

The authors are praised for their excellent job at improving the paper. I appreciate their efforts in doing additional simulations with 0% added TKE, although I would argue that 0% added TKE is unrealistic. The revised paper now provides upper and lower bounds to the effects of TKE and even interesting insights to what is going on in the ABL. As such, I recommend that the manuscript be published after the minor revisions below.

Minor Remarks

1. L. 45: The authors state that the maximum wind speed deficit is “generally occurring in the top half of the rotor disk”, while it is generally near or at hub height. For example, all analytical wake loss models, like Jansen or Gaussian, predict it at hub height. The literature is rich of such evidence (e.g., Abkar and Porte-Agel 2015, Fig. 5; Xie and Archer 2017 Fig. 4). The paper cited to support the statement about the upper rotor peak (Bodini et al. 2021) refers to a figure for one location at two instantaneous times during which the maximum was at about 100 m, for a hub height of, I believe, 91 m. This is not a big deal by any means, but I would prefer to read that the maximum wind speed deficit is near hub height.
2. L. 50: By contrast, here the authors mention that the peak in TKE occurs “in the upper part of the rotor” with no reference. Generally the peak in TKE is actually near the rotor tip (e.g., Abkar and Porte-Agel 2015, Fig. 8; Lu and Porte-Agel 2015, Fig. 12). No big deal, but it would be more accurate to state that the peak in TKE is generally near the rotor tip.
3. L. 139: How can the temporal resolution of the WRF model be 10 minutes? Perhaps you mean the resolution of the output, not that of the NOW_WAKES simulations?
4. L. 150: How are the wind turbines distributed in the grid cells (i.e., one or two per grid cell)? What layout was used (if regular, along which directions)? Please add a few sentences to clarify this, since it becomes important later in the discussion of the added TKE spatial patterns.
5. L. 163: There appears to be confusion between C_{TKE} and α . It is α that is 1.0 in the case of 100% added TKE, not C_{TKE} , as C_{TKE} is not constant and not equal to one.
6. L. 164: unfinished sentence? Perhaps it should be: “default, since comparisons with large-eddy simulations by Vanderwende et al. (2016) and Garcia-Santiago et al. (2024) suggest ...”.
7. L. 185: How do you determine stability here? One value for all lease areas or one value per grid cell? I believe you talk about it later (Eq. 4); if it’s the same approach, then perhaps tell the reader that the info about stability will be discussed shortly.

8. Eq. 4: I am confused about the heat flux here. Since it is based on θ' , it is a potential temperature flux, thus not the same as the heat flux that is discussed, for example, in Section 4.4. How do you calculate it? Or, is L a direct output of the WRF? If so, please state it.
9. L. 250: also Golbazi et al. (2022) reported and discussed the same numerical noise issue.
10. Figure 7: How do you ultimately exclude the light-blue numerical patterns in c)?
11. Figures 9–22: It is difficult to discern the boundaries of the wind farms when they are grey-shaded but hidden below the shades of whichever field is being plotted. Would it be possible to replace the grey-shaded areas with black contours? This is not a requirement, just a suggestion.
12. Figure 10: What is the small grey shape in the middle of e)?
13. L. 309: I would also conclude that the hub-height wind speed deficit is basically insensitive to the amount of added TKE, consistent with Archer et al. (2020), Fig. 7, as long as TKE advection is on.
14. L. 341: The sentence starting with “At 10 m” is a repetition of what was already discussed at L. 316–324. Consider removing it.
15. L. 356: Similarly, the sentence starting with “Increases in TKE” is redundant.
16. Section 4.2.2: An important finding to add is that TKE at the surface is reduced in neutral and stable (fast-wind) conditions in the wakes regardless of the amount of TKE added.
17. General: The manuscript now provides excellent evidence of the links between TKE and 2-m temperature through the heat flux changes. Figures 15–20 now tell a coherent story. Well done.
18. L. 408: I think that the discussion could be simplified and easier to explain and understand if the authors used the magnitude of the heat fluxes, rather than expressions such as “more negative heat flux”. Here is what I mean. The magnitude of the heat flux is directly related to the amount of added TKE near the surface. Regardless of the sign of the heat flux, with more TKE the magnitude increases and with less TKE the magnitude decreases. Thus, in stable conditions, we know from Fig. 15 that TKE at the surface increases in the wind farms and decreases downwind in their wakes; therefore the magnitude of the heat flux is larger inside the farms and smaller in the wakes. Therefore, in stable conditions with negative heat fluxes, this causes warming inside the wind farms (large negative minus small negative equals small negative, thus downward and warming) and cooling downstream (small negative minus large negative equals small positive upward, thus cooling). This is now beautifully demonstrated in the paper. Since added TKE is 100%, the warming inside the farm is somewhat strong, because there is a lot of added TKE there.

With 0% added TKE, we do not see the warming inside the wind farms because there is no added TKE there, but the reduction in the wake remains, thus the cooling and the positive heat flux difference (i.e., reduction of the heat flux magnitude, thus less warming coming down, thus cooling).

19. L. 434–439: Can we conclude that 0% added TKE is unphysical?

Typos and style

20. L. 3: The first sentences of the abstract are about **potential** impacts of the future wind farms, because they are not built yet and therefore we cannot know for sure. As such, terms like “may” (L. 1) and “potential” (L. 4) are correctly used to indicate uncertainty. But on L. 3, the strong future tense “will” is used (“how future plants will change local meteorology”). I recommend the use of “may” instead of “will”, given that any single sentence, out of context, can potentially be used to exaggerate such impacts.
21. L. 10 (and many other instances): one should write either “temperature at 2 m” or “2-m temperature”, with a dash “-” between 2 and m. Same for “2-km resolution” at L. 137 or “10-m wind speed” at L. 173.
22. General: acronyms are sometimes defined but not used, or not needed, or not defined. For example, WRF is not defined but used at L. 122 (it was originally defined at L. 92 but then erased for some reason). PBLH is defined at L. 81 but not used in the next two sentences. IBL is not really needed (L. 84).
23. General: Remove the space between 100 and %, thus “100%” (like at L. 108), not “100 %” (like at L. 111). Same for 0% and 25%.
24. L. 167: weird use of “of”. Perhaps rephrase as: “too-small values of turbulence and too large decreases in wind speed”.
25. L. 217: misspelling of “calculated”.