Wind Energ. Sci. Discuss., referee comment RC2
https://doi.org/10.5194/wes-2021-127-RC2, 2022

Comment on wes-2021-127

Anonymous Referee #2

Referee comment on "The Sensitivity of the Fitch Wind Farm Parameterization to a Three-Dimensional Planetary Boundary Layer Scheme" by Alex Rybchuk et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-127-RC2, 2022

The paper inserts for the first time the Fitch wind farm parameterization in the newly developed 3DPBL scheme in the WRF model. This is important and innovative because, as far as I know, the Fitch parameterization has only been coupled with the MYNN PBL scheme. It is therefore valuable to see how it would work with a different PBL scheme. However, the paper consists of pages and pages and pages of detailed and rather pointless differences between the results obtained with the two schemes, first with idealized cases, then with a series of simulations of a few offshore wind energy areas in the US Northeast, leaving however no useful information on which would be better for which cases and why. I am afraid that, in order for the paper to be acceptable in its current format, too much additional work would be required (i.e., redo all idealized runs and simulate a different real farm), as discussed next.

An alternative would be to remove Section 4 entirely. Adding a real case would be valuable if it allowed the authors to validate the 3DPBL+Fitch coupling, but it has no value in this manuscript unfortunately, it just adds pages and pages of minutia and repetition.

Major points

- Although some of the co-authors have access and/or have participated in field campaigns that have collected plenty of data on wind farm wakes, inland and offshore, and on observed wind farm power production (e.g., Siedersleben et al. 2020 just to mention one), no comparison against any type of observations is offered in this study. Why did the authors choose to simulate the Vineyard Wind and the other U.S. wind energy areas, for which no data are available yet, when so many other farms with data are available? At a very minimum, high-resolution simulations (like HRRR) could have been used for the wind speed profiles for August 2020 for Figure 9. But, better yet, a different farm with actual wake observations should have been simulated instead.

- The authors state that TKE advection is turned on (see l. 243), but it does not seem to be true. Figure 6 shows without doubt that all the added TKE is confined within the boundaries of the wind farm and above it, but not advected downwind at all. With the MYNN scheme in particular (top two rows), one can even see the individual positions of the turbines, one every other grid cell, with the added TKE at their grid cells and above, but none in the next adjacent cells downwind. This proves that no advection is actually operating. The authors need to double check that bl_mynn_tkeadvect is indeed set to true in the inner domain. Since TKE advection appears to be wrongfully turned off in all the simulations, all the conclusions of the paper are potentially invalid.

Minor points

- 55: There is another wind farm parameterization for WRF in the literature: the hybrid model by Pan and Archer (2018).
- 64 and 133 and 164: Missing citation "?"
- 101: I think I know what you are trying to say, but it needs to be defined better because an external wake cannot be defined as a "distance". Also, here you suse 0.2 m/s as the threshold, but in the rest of the paper it seems to be 0.5 m/s (e.g., Figure 3 and 11, dashed blue line).
- Table 1: the same label here is used to indicate three different runs. Please use unique labels for each run, like "S-NWF" for stable, "U-NWF" for unstable etc.
- 203: not OK to cite a manuscript in preparation, please remove Rosencrans et al.
- 206: type for "pseudo"
- 209: How many turbines are there in total? 25 perhaps?
- 322: Why 0.5 m/s deficit if 0.2 m/s was stated earlier?
- 322: I cannot understand what the e-folding distance is. Please include an equation. To be honest, I do not even understand why this variable is even introduced, it does not add much, it is overly sensitive to the stability and choice of the scheme, and it is no longer used in the real simulations later. Consider dropping it since it does not add much.
- Figure 3: I am surprised that the maximum deficit possible is 1 m/s (note that the maximum deficit is 4 m/s in Figure 11). This must be the most efficient ideal wind farm ever designed. Why is the flow from the west-southwest? I would recommend using white for the range -0.25 – 0.25 m/s.
- 345-350: I find it **very** difficult to believe that the addition of TKE causes a longer wake. Also very confusing that the weird decrease in TKE in one specific case (Figure 6g) can be used to explain this general and counter-intuitive finding. To me this is another flag that suggests that advection of TKE was **not** turned on.
- 384: not OK to cite a manuscript under review. Please remove Bodini et al.
- 409: the authors themselves note that there is no advection of TKE! This is not a realistic result. Flag bl_mynn_tkeadvect must be true for TKE to be advected, at least with the MYNN scheme.
- Figure 8: please use one color scheme! You can intervals that are variable to better emphasize features, but using two colorbars like that is not OK.
- 476: Are these results with 0% TKE or 100% TKE? Why not 25% TKE as

recommended?

- 484: define “centroid”
- Figure 12: as in #14, not OK to have 4 colorbars.
- 28: by this point, I could not force myself to read the manuscript anymore. Too boring and pointless. This section on the real cases is rather useless without observations and does not add anything to the discussion of the idealized cases. The paper would be better off without Section 4.