Comment on wes-2021-127

Anonymous Referee #1

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-RC1, 2021

Dear authors,

Your paper tries to assess the sensitivity of the Fitch scheme in wake simulations by implementing it on a newly developed PBL scheme. I am certain that this manuscript is interesting and has some value but I am not sure whether in its actual state displays correctly this value, since I think the reader cannot gain much from all the numbers you reach with your simulations. So in the following, you find a list of major and minor/specific comments.

Major comments:

- My main comment is that the reader does not gain much when reading your work (this sounds harsh but I will try to explain). You compare results from Fitch using two PBL schemes. You do not conclude which PBL scheme is better. You do not conclude which PBL scheme is better with Fitch either. You basically simulate wakes and report the results and basically claim that since the results can be quite different then you suggest to use many different PBL schemes when simulating mesoscale wakes to get an idea of the uncertainty. This is of course valid but then what? One could then say you need to try all wind farm parametrization to get an idea of the uncertainty (and all PBL schemes available). I would think this would be a waste of resources. What I think it would have been nice to see here is some arguments/analysis in which given the differences between the wake simulations one could already say something about the ability of the Fitch scheme to model wakes. Or, better, that the authors have developed a methodology which could use the results of the simulations to gain knowledge about the accuracy of the Fitch scheme. All the quantitative results with regards to the differences under the idealized and the mid-Atlantic case are not really used to anything (I mean the actual numbers) and will not really help anybody to discern

anything about the Fitch scheme and/or PBL schemes used. Maybe, one could also simply say: what should be the maximum differences in wind speeds and turbulence deficits when simulating wakes with different PBL schemes when using the same farm parametrization?

- Your introduction is way too long. Particularly Section 1.2 is not really needed and it falls very much into kind of the same comment I just made about many numbers (from a number of previous wake works) without much meaning. The last paragraph of that subsection can be kept and will be sufficient. Also all the Fitch studies before Archer et al. (2020) are wrong (due to the bug in the model) and so they should not be mentioned. Lastly, what the introduction really lacks is why trying now Fitch with the 3DTKE PBL scheme? Would it be better? More realistic? Totally wrong? I guess the reader would tend to think that a 3D PBL scheme is better than a "2D" one such as MYNN

- Second paragraph in Sect. 2.1: these lines should be complemented with the formulations so that the reader can get an idea of the advantages/extensions of the 3D TKE PBL.

- Text between lines 198 and 208: due to the use of a new PBL scheme, it would be interesting to see the development with time of the turbine-free simulations and find out why 3 days are indeed needed to develop the ABL and reach quasi-steady state (it just reads as a quite extremely long spin up period). By the way, you do not mention (I think) the type of boundary conditions during spin up and wake simulations.

- Idealized simulations: to compare fairly the Fitch scheme with the two PBL schemes, you should aim to get the same wind speed and direction at hub height. Therefore you should not use the same geostrophic wind for all simulations The problem is clear for the unstable simulation where it does not make sense at all to compare the wake results given the large differences in wind speed

- Conclusions: the manuscript is already quite long and so such a long conclusion (which is not really concluding statements) is not needed. Can the reader get some nearly like bullet-points from your work? Also, and in relation to my comment 1, there are way too many sentences with a number of values that do not mean much if you do not have a reference or measurements. This is very clear between lines 657 and 674

Specific comments:

- Line 4: "were only compatible with one PBL scheme" this is a general comment but I guess you mean the specific case of the WRF model, which is not mentioned at that point.
- Line 8: "internal" the reader does not know what you mean by internal so maybe drop the work and be specific in the abstract
- Line 10: add "atmospheric" before "stability"
- Line 27 "their impacts in numerical" I guess you mean "their impacts on atmospheric variables when implemented in numerical weather…" or so
- Line 31: you provide some low and high losses… what are the cases for this? I mean these are because of the size of wind farms?
- Line 57: maybe delete "generation"
- Line 64: you have some ? signs when making a particular reference… this is not the first time

- Line 155: remove "to" before "behave"
- Line 164: again a ? in a reference
- Line 165: not sure whether you define "Sq"
- Line 180: why not using the value suggested in Archer et al. (2020)?
- Line 198: why not using the roughness of the sea? These parameterizations are mostly used offshore and it will be more straightforward to compare to the mid-Atlantic case
- Line 199: the sentence kind of suggests that the hub height wind speed is very close to the geostrophic wind speed but that is not necessarily the case. So why 10 m/s?
- Figure 1. This figure can be changed to show the model domains and maybe an inset with a zoom of the vineyard wind 1 with the turbine arrangement would be nice
- Line 238 and similar: all these references to manuscripts in preparation are not useful. Particularly the one at this line is not needed (also that in line 252)
- Sentences in lines 264-265 and 286 are redundant given each of the sentences before them
- Figure 2: do you say somewhere whether these profiles are instantaneous output at some time? Are they spatial averages over the whole domain? Are they time-averaged? Also the profiles should be somehow smoother; however they show some weird peaks, e.g., the highest wind speed of the stable MYNN or those below the lowest farm boundary in the stable TKE
- Also about the result in Fig 2 for the unstable TKE: why is MYNN 3 times lower than 3DTKE? You mention this is related to the empirical constants but the stable and neutral ones seem fine
- Line 300 maybe you can add after "values" whether these are from instantaneous values at a given time
- Figure 3 and related results: why not aligning the wind with x so that when you make the cross (side) analyses the plots are easy to digest
- Line 327 "wakes erode" not sure how general is the erode term in wakes, could you replace it by recovery? I think you have different instances with this
- Line 384 the ref. to your not published work can be changed. Some others have seen this as well
- Line 432 delete one "of the"
- Line 480 Not really true as TKE was quite different for the unstable case
- Line 491: I think you need to add "on land" after "near-neutral stratification"
- Line 491-494: a figure with the frequency distributions of these surface heat fluxes would be nice
- Figure 9 how are the winds above the boundary layer? Are they close? Maybe an inset showing the full profile would be nice
- Line 516 replace "off" by "on"
- Line 600 "amplification" I am not sure what you actually mean
- There are way too many avoidable references and also many references to your work (where any of the coauthors are involved)