Review of *Effect of different source terms in atmospheric boundary modelling over the complex terrain site of Perdigao* by K. Venkatraman et al.

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The authors compare and validate results of different atmospheric RANS turbulence models applied to a complex terrain site.

I like the idea to investigate the effect of different atmospheric physics and the effect of a forest representation on complex terrain using two-equation RANS turbulence models. The topic is also well suited for Wind Energy Science. However, the article is not entirely clear because the RANS turbulence/inflow models and input parameters are not fully described. I think it is essential that you do describe them completely in order to be able to understand the article and provide the reader an opportunity to redo the simulations. More detailed comments are listed below; they need to be addressed before the article can be considered for publication in Wind Energy Science.

Main comments

1. Koblitz et al. (2013) used a range of turbulence models that differ in complexity and it is not entirely clear which elements you have adopted. For example, do you use an active temperature equation or do you only use the global turbulence length scale limiter of Apsley and Castro (1997) [1]? Do you use ambient source terms to avoid zero turbulence values above the ABL (see for example van der Laan (2020))? I propose that you write down the full model description of the momentum equations including possible source terms, the Boussinesq hypothesis and the k-ε (and ω) turbulence transport equations (also including all possible source terms). Then you can write in Table 2 which source terms are active by referring to the variable name ($S_?$). You could have a look at a recent article of my own where I tried to do this [3]. In addition, I strongly recommend to add a table including the chosen values of all turbulence model constants. Furthermore, not all parameters are defined. For example, what are the chosen values of G, f_c, β_B, $\alpha_t 0$, etc? What was the set inflow wind direction (at a certain reference height)?

2. You use sources of buoyancy in the BBSF1 model while you are only considering a neutral case. Are these sources then set to zero? If this is the case, wouldn't it make sense to remove them from the article and also remove the word buoyancy in the abstract and elsewhere, since you have not yet investigate its effect?

3. Are you aware that the global turbulence length scale limiter of Apsley and Castro (1997) [1] can have problems when it is applied to complex terrain where the turbulence length scales of the hills are in the order of the maximum value that is set by turbulence model? When this is case one could observe non-physical large hill wakes or even numerical convergence problems. I think this is worth mentioning in the article. (Unfortunately, I couldn't find a good reference for it other then a brief discussion in an article of my own [4]).

4. Abstract, Line 10: I would rewrite the following sentence: *The inclusion of a canopy model is shown to improve predictions close to the ground for most of the towers, while reducing prediction accuracy on top of the ridges, illustrating the need to represent terrain heterogeneity.*, because the second point is not in favor of representing terrain heterogeneity.

5. Section 3: You mention that a grid refinement study was performed, but I could not find any results in the article. Also note that a reference to the grid refinement study of Laginha Palma (2020) is not sufficient because they have used a different solver and setup. (A grid refinement study is solver dependent.) In addition, the chosen turbulence model might also influence the grid study; you could show results of a grid refinement study using the most demanding turbulence model.

6. You forgot to add results of the inflow (precursor). It would be useful to compare the two inflow models in a plot for wind wind speed, wind direction, TKE, turbulence length scale and temperature.

7. Do you have an idea how much the wind direction varies in the observations? If this is significant you might need to account for it in the models by running a set of wind directions and then you can average the results (profiles) using a weighted averaged following a Gaussian distribution with a standard deviation representing wind direction uncertainty. A typical value for wind farm wake studies is 5° but it depends on the site and the distance between the location at which the reference wind direction was measured and the location at which the profile was measured. For more info you can have a look at Gaumond et al. (2013) [2] and a work of my own [5] (Section 3.1.3). For complex terrain, the effect of wind direction is often quite significant and a small difference in wind direction (distribution) between the measurements and models can result in large differences.

8. Line 115: You mention that you use a loglaw inflow with TKE profile that varies with height following a modification of Yang et al. (2009). If you want a varying TKE profile with height then you could just model a pressure-driven boundary layer with a constant pressure gradient, which should result in logarithmic wind speed profile near ground and a varying TKE profile. Such a model would require a precursor simulation to generate the inflow. In addition, I am not convinced that the model of Yang et al. (2009) is a solution of the standard k-ε model, meaning that the inflow will most likely develop downstream (especially of the domain is large). Have you checked this?

9. Line 134: You write *The direction of the flux automatically determines the inlet and outlet regions.* I thought that the set wind direction would determine the inlet and outlet regions, or this is a misunderstanding from my side? How do you handle wind veer for determining the inlet and outlet boundaries?

10. Section 3.2, Line 144: What is the reason that you use a constant leaf area density? You could easily use a varying leaf area density based on the forest point cloud data.

11. Section 3.2, eq. (3): Is this really how the Coriolis force source term is implemented in your model? If you follow the ABL model of Koblitz et al. (2013) I would expect that you include U and V-momentum source terms that represent a balance between a (constant) geostrophic wind speed and the Coriolis force (see for example van der Laan (2020)), as you also briefly discuss.

12. Section 3.2, eqns (2) and (4): I think you need to define different source terms, for example $S_{p,m}$, $S_{p,k}$ and $S_{p,\varepsilon}$.

13. Table 2: The skipped entries (–””—) are unclear to me. For example, does the BBSF1 case include forest source terms or not? I would just fill in the entire table for clarity. In addition, what do you mean by using SKE for BBSF1? I though that you use a global length scale limiter in the epsilon equation, which is different from a standard model.

14. What do the error bars on the measurements represent? Is is the standard deviation of the uncertainty of the mean? I would recommend to use the latter.

15. You could group the profile plots in the results into three figures, where each row of sub plots represents a mast: SW ridge (combine Figs. 6-8), valley (combine Figs. 9-12), NE ridge (combine Figs. 13-14). I think this make sense because you also discuss them as met mast groups in the text. In addition, you could consider to plot normalized results of wind

speed (U/U_{ref}), turbulence intensity ($\sqrt{2/3k}/U_{\text{ref}}$) and wind direction ($wd - wd_{\text{ref}}$) instead of dimensional results. Finally, you could zoom the x-axis of the wind direction, since it is hard to see the difference between the models and measurements in Figs. 6c, 7c, 8c ad 14c. This also applies to some of the wind speed and TKE plots.

16. Line 194: You mention: *A good match is obtained in between the measured and computed velocity, turbulent kinetic energy and wind direction profiles for the calibration Tower 20 as seen in Fig. 6.* However, the results of the model including forest are not matching well, especially for the TKE and the wind speed near the ground.

17. Figure 13: Nice plot. You could add the relevant mast location(s) in the plot, so the reader can better understand the results of Figs. 9-12.

18. I am missing information on code and data availability, which is normally added at the end of the article. In addition, I was wondering if is possible to provide the numerical setup using a DOI of a git hub repository through Zenodo or something similar. By proving the numerical setup/ run scripts, one could easily redo the work since the numerical solver (OpenFOAM) is publicly available.

Minor comments

1. Introduction, Line 13: You could rewrite the following *Lack of terrain availability in flat terrain pushes wind-farm developers to look for alternative sites along complex terrains.*, since you use the word terrain three times and I think complex terrains could be rewritten as complex terrain sites. The latter also applies elsewhere in the paper.

2. Line 117: log-low → log-law.

3. There are quite a lot of other typos in the article but these can be fixed in the proof reading process.

References

[1] Apsley, D. D. and Castro, I. P. A limited-length-scale k-ε model for the neutral and stably-stratified atmospheric boundary layer. *Boundary-Layer Meteorology*, 83:75, 1997.

[2] Gaumond, M., Réthoré, P.-E., Ott, S., Peña, A., Bechmann, A., and Hansen, K. S. Evaluation of the wind direction uncertainty and its impact on wake modeling at the Horns Rev offshore wind farm. *Wind Energy*, 17(8):1169, 2014.

[3] van der Laan, M. P., Baungaard, M., and Kelly, M. Inflow modeling for wind farm flows in RANS. *Journal of Physics: Conference Series*, (1):1–11, 2021.

[4] van der Laan, M. P., Kelly, M., and Baungaard, M. A pressure-driven atmospheric boundary layer model satisfying rossby and reynolds number similarity. *Wind Energy Science*, 6(3):777–790, 2021.

[5] van der Laan, M. P., Sørensen, N. N., Réthoré, P.-E., Mann, J., Kelly, M. C., Troldborg, N., Hansen, K. S., and Murcia, J. P. The k-ε-f_p model applied to wind farms. *Wind Energy*, 18(12):2065, December 2015.