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

José Carlos P. Lopes da Costa, ISEP

April 7, 2022

This article present the results of different simulations of the wind behaviour on Perdigão site, comparing it with the results of a field measuring campaign on this region of complex terrain. The authors used several combinations of two-equation RANS turbulence models with a forest canopy model and atmospheric stratification conditions. I think that this is a quite interesting subject and very adequate to be published on a wind energy publication.

However, in order to be considered for publication, I think that this article lacks of a more detailed description of the implementation of the different models used; furthermore, it will be necessary to better specify the parameters used in the different numerical models, such as the forest canopy model, so that they can be eventually mimicked in other studies.

Comments

1. From reading the article, one can not know precisely what TKE models parameters were used in the simulations. As an example, the $k-\varepsilon$ model can use different model constant values such as C_μ used in the calculation of the turbulent viscosity $\mu_T = \rho C_\mu \frac{k^2}{\varepsilon}$. Is is usually used the standard value $C_\mu = 0.090$; however, for atmospheric flows $C_\mu = 0.033$ seams to be more adequate. Is that the case? There are also other parameters that are not revealed, such as the Prandtl number for the turbulent dissipation and others. Do you use the default values in OpenFOAM? Even if it is the case, it should be enumerated.

2. A similar problem appear in the canopy model description; the model parameters aren't completely defined. It is said that the model is based in [Lopes da Costa, 2007], but this model uses several parameters that are not fully addressed in this article. It is presented the source term for the velocity and one can not know what is the α parameter in it or what that represents. The source/sink terms for the turbulent kinetic energy and its dissipation rate are not also mentioned. I think that you might be more clear in this subject.

3. In chapter 3, though the definition of the domain volume is well explained, the description of the computational mesh is vague. It

is said that it consists in 12.7 million cells, but it would be more useful to define it by the number of cells per main direction, such as $n_x \times n_y \times n_z$. It is said that the horizontal mesh resolution was set to 33 m, but is this resolution observed only in the center of the domain (close to the masts location) expanding itself to the boundaries or is it a regular mesh? (...which is probably not the case, as the domain is not square or rectangular, but cylindrical.) It is also said that an "uniform stretching is applied to the vertical direction" with no more details. However, one of the most important mesh parameters in an atmospheric flow simulation is the minimal mesh hight Δz next to the ground (along with the vertical number of cells), which is not presented in the article.

4. It is said (line 145) that you have choose a mean tree heigh of 3 m. I suppose that it is all over the domain (or, at least in the $7,5$ km $\times$ 7.5 km squared area - it is not clear in the article. Wouldn't it be more correct to use different patches (eventually with different heights), as you have that information in figure 5? Or, at least remove the trees from the higher zones of the ridges, as it is common to happen in this kind of topography and cam be observed in figure 5?

5. I suppose that you use in the canopy model an uniform area density. However, forests have a higher foliage density at the half top than at the bottom zones - [Lalic and Mihailovic, 2004]. I think a non-uniform leaf area density could be easily implemented in that model.

6. I find the Figure 13 very interesting. The extension of the slice could be wider, in order to include the whole top of the ridges and clearly show the positions of the masts 10 and 7 (and approximately the zone of mast 37). This small change could complement and be more enlightening in the interpretation of the results obtained for these masts and presented in the other figures. (Also could be useful for the reader if the caption of the figure refer that the location of slice is defined in Figure 2.)

Minor Comments

1. Line 111: "...homogeneous atmospheric boundary layer (ABL). either..." should be "...homogeneous atmospheric boundary layer (ABL) either...".

2. Figure 5: The scale in the figure is strange; the sequence of the d (m) values is $0, 2, 6, 4, 8$; shouldn't it be $0, 2, 4, 6, 8$?

3. For example: In Figure 5, m for meters shouldn't be written in italic as it is not a variable, but a length unit. In many other figures there is also the variables (U, TKE,...) not in italic, but with the units in italic; it should be the opposite.

4. Table 2: I would prefer the description of each case to be done in a more clear way, instead of using " –"– " all over the table.

5. Line 111: "predictions" is two times written.

6. Line 205 to 206: I think that the order of the towers and the figures is somehow messed up. It should be "...towers 25, 7, 27 and 22.", and further, "...shown in Figs. 9, 10, 11 and 12 respectively.".

7. ...and a small detail: in the references, "Costa, J. L. C. (2007)" should be "Costa, J. C. L. (2007)", or even "Lopes da Costa, J. C. (2007)". ;)

References

Branislava Lalic and Dragutin Mihailovic. An empirical relation describing leaf-area density inside the forest for environmental modeling. *Journal of Applied Meteorology - J APPL METEOROL*, 43: 641–645, 04 2004.

J. C. Lopes da Costa. *Atmospheric flow over forested and non-forested complex terrain*. PhD thesis, University of Porto, 2007.