

Wind Energ. Sci. Discuss., referee comment RC1
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Comment on wes-2021-36

Dries Allaerts (Referee)

Referee comment on "Meso- to micro-scale modeling of atmospheric stability effects on wind turbine wake behavior in complex terrain" by Adam S. Wise et al., Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2021-36-RC1>, 2021

This paper studies the interplay between complex terrain, atmospheric stability, and a wind turbine wake at the Perdigão site by means of large-eddy simulations driven by realistic mesoscale forcing conditions. The paper focuses on two particular case studies with distinct atmospheric stability conditions: a case with stable atmospheric stability conditions in which a mountain wave occurs, and one during convective conditions where a recirculation zone forms in the lee of the ridge with the turbine. The authors used the WRF-LES-GAD multi-scale modelling suite, which couples mesoscale WRF to microscale WRF-LES including a generalized actuator disk model, and they validated the simulations by comparing to field observations with in-situ and remote sensing instrumentation. The paper provides a very detailed description of the terrain-induced flow features and an extensive validation with field data. Furthermore, the interaction between the terrain-induced flow features and the wind-turbine wake is also studied in depth. I believe the paper is well written and will be of interest to the wind energy community, both for demonstrating the viability of WRF-LES-GAD and for explaining the interplay between complex terrain, atmospheric stability and wind-turbine wakes. I do have some scientific questions and a number of minor or and technical comments as listed below.

Scientific questions:

- Can you comment on how representative the specific case studies (stable in combination with mountain waves, unstable in combination with a recirculation zone) are for the wind conditions at Perdigão and for complex terrain in general? Do you think the conclusions in terms of wind-turbine wake behavior (terrain following in stable, deflecting upwards due to recirculation) will hold for all stable/unstable wind conditions at this site and for complex terrain in general?
- The inverse of the Froude number as defined in Equation 1 is often called the non-dimensional mountain height and it represents the ratio of the mountain height to the (vertical) wave length of the mountain wave. When discussing the case where the

mountain wave length is shorter than the width of the mountain (line 133-134), wouldn't it be more appropriate to use a Froude number based on the width rather than the height of the mountain like for example in the book of Stull (1988, section 14.2.3 Flow over hills). Obviously, this also affects figure 3 and later calculations of the Froude number in section 4.1.

- It is not entirely clear how you setup the semi-idealized simulations. What pressure gradient force do you impose, or alternatively what wind speed and direction do you enforce (and at what height)? Do you apply a negative surface heat flux in the stable case? What is the domain height of these simulations? Do you use any damping layers at the top? For how long did you simulate these idealized cases?
- Line 182-183: What do you mean with the stratification is self-destructive? Do you mean the stable stratification turns into a constant temperature profile because of turbulent mixing? Does the simulation become unstable due to inertial oscillations? Something else?
- Figure 12: Is there any averaging of model results or measurements? Under higher turbulent conditions, does it make sense to compare instantaneous velocity fields with the point measurements of the met towers given the chaotic nature of turbulence?
- Line 361 "Differences in 80m mean wind speeds ... are less than 1 m/s ...": Is this the difference averaged over the entire time horizon, or the root-mean square, or something else? When I look at figure 13b around 13:55, I obviously see differences of more than 1m/s. Similar for the discussion of the differences in the wind direction and also for the plots for stable conditions. You seem to be talking about a certain average difference being below a certain value, but the instantaneous differences are clearly higher than this value and it is not clear how the averaging is done. Please clarify.
- Line 445: The blockage addressed in the papers by Meyer-Forsting, Bleeg and Segalini refers to the global blockage effect far upstream of large wind farms that arises due to the combination of many wind turbines. Here, you only have one turbine and you are only looking at 2D upstream, so I would say that turbine induction might be playing a role, not blockage. I don't think it is appropriate to refer to these wind-farm blockage studies here, so consider removing these references.

Minor questions and technical comments:

- Line 148: Please mention at least once how UTC relates to local time at this particular site. Maybe it is even more interesting to mention what time corresponds to sunrise.
- Line 148-149: Please mention what value of the wind direction corresponds to a direction perpendicular to the ridge such that the statement can actually be assessed by the reader.
- Line 149 reference to Fig 2a and 2b: I don't think this reference is correct. Figures 2a and b show wind speed in stable and convective conditions, while the sentence was talking about the wind direction.
- Line 155-156 and figure 2d: Wouldn't it make more sense here to use an actual stability parameter like the Obukhov length L or z/L to assess the atmospheric stability? Especially when talking about moderately convective conditions, how can you assess that based solely on the potential temperature gradient?
- Line 158 reference to Fig 2d: Again this reference seems to be a bit misplaced as it refers to the wind direction plot, not the wind speed plot.
- Fig2: Please mention hub height of 80m (?) and measurement points for the temperature gradient (10m and 100m?) in the caption for clarity.
- End of line 188: surface roughness length should be in units of meters.
- Line 291, 356, and many others : You often start a new paragraph without clearly

indicating that you will be talking about a new figure. For instance on line 291, are you still referring to figure 7, or are you rather referring to figure 8 or table 2?

- Table 2 and 3: the results in these tables are not really discussed it seems. Can you somehow use these results when assessing/discussing the results of related figures?
- Line 312: How high above the actual surface is the nose of the jet which you use to calculate the Froude number? The Brunt-Vaisala frequency is based on the temperature gradient between 10m and 100m, but it is not clear whether the nose of the jet is at about the same height above the surface.
- Line 320: How high is the inversion layer above the valley floor (600m asl but at what altitude is the valley floor)?
- Line 323: When talking about the striation of lower wind speed close to 600 asl in fig 6b and d, in what ranges of x can this be seen?
- Line 357 "there is less variability in the potential temperature gradient for the model compared with observations": I'd say there is more variability in the model output compared with the observations (this is also mentioned later in the text). The statement seems to contradict the results shown in figure 13 g,h,i.
- Line 427: The year of the publication by Churchfield and Srinivas is missing (also in the list of references).