

Interactive comment on “An intercomparison of mesoscale models at simple sites for wind energy applications” by Bjarke Tobias Olsen et al.

Anonymous Referee #1

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The manuscript provides a valuable comparison of NWP models against wind observations from tall towers. The article is well written and it should deserve publication. One aspect that the authors should consider is the inclusion of dataless sites. The comparisons at these sites do not provide much information and could be removed from the manuscript. A more important aspect is the relative little attention that the authors pay to the effects of atmospheric stability. According to Table A3 Ri and L are provided by the different teams so there is not a clear reason for not analyzing in more detail the important effects of atmospheric stratification. The behavior of the models could be very different under stable/unstable situations. Another relevant aspect for wind energy is how well the models represent the annual evolution and the diurnal cycle. More specific comments are provided below.

SPECIFIC COMMENTS

C1

1. Page 1, Line 10. Clarify what is “average wind speed distribution”.
2. Page 2, Line 13. Can you quantify instead of saying “does a much better job”?
3. Page 2, line 15-16. The open statement of the paragraph says “many different climates and terrains” but all the examples are for northern Europe. It is better to change the opening sentence or enlarge the number of examples.
4. Page 2, line 32. Clarify what do you mean by “the observed mean wind speed”. Do you mean simulated wind speed?
5. Page 3, line 8. An important conclusion of Gomez-Navarro et al. is to account for the effects of unresolved topography in the WRF model.
6. Page 3, lines 32-34. Clarify what do you mean by “little knowledge has been derived from assessing the operational NWP models run by the community”.
7. Page 7, line 30. What is the distribution of the vertical levels near the surface?
8. Page 8, line 20. Why do you want to remove outliers? In the case of observations you may question the validity of the data but in the case of the simulations you do not question this so you should not remove them.
9. Page 11, line 7. Jimenez et al. (2016) compared 10 years of observations and WRF simulations at Cabauw. They already pointed out the reduction of the bias with height at this site. You should probably mention this previous work to construct on its findings.
10. Page 16, line 2. Do you think the temporal interpolation is also responsible for the poor results?
11. Fig. 10: Is it correct that some models have a bias of about 20 m/s at Cabauw? That's a very large bias, something looks wrong with that model(s).
12. Page 21, line 1. Two consecutive “used”.
13. Page 22, line 7. Tow consecutive “submitted”.

C2

14. Table A.5. The fifth row should be the third one according to the horizontal grid spacing.

References:

Jimenez, PA, J Vila-Guerau de Arellano, J. Dudhia, F. Bosveld, 2016: Role of synoptic- and meso-scales on the evolution of the boundary-layer wind profile over a coastal region: the near-coast diurnal acceleration. *Meteorol. Atmos. Phys.*, 128, 39-56.

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