

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

Bjarke Tobias Olsen et al.

btol@dtu.dk

Received and published: 1 March 2017

C1

Comments to anonymous reviewer #1

Bjarke Tobias Olsen, Andrea N. Hahmann, Anna Maria Sempreviva,
Jake Badger, Hans E. Jørgensen

Reviewer comments in **bold text**. Author comments in plain text.

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.

Thank you for the feedback.

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.

Your comment, and that of the other reviewers, suggests that the comparisons at the dataless sites add more noise than value to the manuscript. We agree with your suggestion and propose to remove them from the future 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

C2

the important effects of atmospheric stratification. The behavior of the models could be very different under stable/unstable situations.

We agree, and have further analyzed the data with respect to stability. We propose to add a new section on this topic to the manuscript.

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.

With respect to the annual cycle we agree, and propose to add a section about that. Regarding the diurnal cycle, the effects related to changes in the atmospheric stratification, which occurs during the diurnal cycle, are represented well by the new results related to stratification. We propose to add a statement in the new manuscript about this, without adding additional figures.

SPECIFIC COMMENTS

1. **Page 1, Line 10. Clarify what is "average wind speed distribution".**
We agree that clarification is needed, and propose to rephrase the abstract. To be clear, what we ment was the mean of all the modelled wind speed distributions.
2. **Page 2, Line 13. Can you quantify instead of saying "does a much better job"?**
We suggest changing it to "provides a better representation"
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.**

C3

We propose to rephrase the opening sentence.

4. **Page 2, line 32. Clarify what do you mean by "the observed mean wind speed". Do you mean simulated wind speed?**
Yes, we agree that this should be corrected.
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.**
We agree that this should be clarified.
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".**
We agree that it needs clarification, and propose to rephrase the opening of the paragraph to: "Community-driven model intercomparison projects provide an opportunity to study both model uncertainties, and sensitivities to model components."
7. **Page 7, line 30. What is the distribution of the vertical levels near the surface?**
Approximately 10, 34, 69, 118, 187 and 275 m. we agree that detail should be added to the manuscript.
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.**
We would like to present the general performance of the models with aggregated statistics. We chose the intermodel mean and standard deviation for this. In some cases, the output from one or two model(s) is very different from the other models (> 3.5 intermodel standard deviations away from the intermodel mean), which would heavily skew the intermodel mean and standard deviation if included.

C4

Since it is so few models we are talking about, we decided to leave them out of the aggregate. The models that are left out are still shown, and the methods we use to calculate the intermodel mean and standard deviation are clearly defined, which makes it completely transparent for the reader.

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.**

Thank you for mentioning this paper, we agree that a reference in the manuscript is appropriate.

10. **Page 16, line 2. Do you think the temporal interpolation is also responsible for the poor results?**

That is an excellent point. The poor results are, as you say, to a large degree a result of the vertical and temporal interpolation. This should be stressed in the new version of the manuscript.

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).**

Thank you for catching this. The unit was wrong, and should have been % not m/s. However, we suggest removing this section from the manuscript, as per the reviewer responses.

12. **Page 21, line 1. Two consecutive "used".**

Thank you.

13. **Page 22, line 7. Two consecutive "submitted".**

Thank you.

C5

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

Thanks. Fixed.

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.

Interactive comment on Wind Energ. Sci. Discuss., doi:10.5194/wes-2016-43, 2016.

C6