

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

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Comments to anonymous reviewer #2

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

The manuscript provides a comparison of 25 atmospheric forecasts with mast observations for three different locations with a focus on wind energy related parameters. While the undertaking itself is very important for the wind energy community given the collected data especially from multiple commercial sources. In my opinion, however, there are several issues that need to be addressed before publication.

In general, the language of the manuscript needs some improvement. I gave several corrections in my detailed comments but I suggest a native speaker or a professional editing service to correct all of the numerous (small) errors. Further, I recommend to use present tense instead of past tense for most of the manuscript.

Thank you. We are reviewing the paper accordingly.

The section "Introduction" is too long and needs to be much more concise. Of-

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ten, the authors do not only cite the essence of a referenced paper, but also provide additional detail about it which does not add value to the actual message. An example for this can be found on page 2 line 28ff: The authors cite Hahmann et al. (2014b) with an explanation on what was done in the study before adding the sentence "A year long wind climatology simulation was used as the test variable". This information is too detailed and can easily be omitted without lessening the message itself. Further, the introduction contains a lot of abbreviations. Some of these are even not used later in the manuscript, e.g. LCOE.

We agree with both points made, and propose to improve the text in both respects.

The use of the three comparison sites without measurements seems to be unnecessary. First, I would disagree that the data-less sites resemble the mast sites from a climatological perspective (e.g., wind climatology). Second, at horizontal resolutions down to 1km, comparable sites with a focus on near-surface PBL will be very hard to find. Third, the authors themselves do not provide much detail about the comparison. I suggest to omit this part of the comparison.

Agreed. We propose to remove this from the revised manuscript.

Most of my concerns with the manuscript are with the section "Individual model performance" which provides the results for the major objective of model inter-comparison: The authors show that the models differ, but they fail to show why. In my opinion, in a comparison study of model simulations, the attribution of differences among the data sets with respect to the representation of the simulated parameters to the characteristics of the simulation systems is most important. While the authors list multiple such characteristics as potentially crucial to the quality of the simulations, e.g., model, physical process schemes, they fail to

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show a dependence of the single model results to these characteristics with the exception of showing the dependence of wind speed error to grid spacing in a very simplistic way. I think the reader as well as the quality of the manuscript would profit from more details, e.g., how do longer forecast lead times or smaller grid spacing reflect on the performance of the models presented in a plot similar to Figure 3.

We tend to agree, and suggest a revision of the section. We propose to remove much of the old content, and to add new results to the revised manuscript that provide an analysis of the model results related to three specific model options: PBL scheme, grid spacing, and simulation time.

I suggest to merge sections 4 and 5 into a "Conclusions"-Section which can contain a summary.

We agree with this.

Detailed comments:

Thank you for catching all these!

1. **Page 2 Line 4: "... as ensemble members ..."**
Thanks!
2. **Page 3 Line 9: "... sensitivities of the WRF"**
Thanks!
3. **Page 3 Line 23: "... assessment exist."**
Thanks!

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4. **Page 3 Line 25: "... near surface winds were ..."**
Thanks!
5. **Page 3 Line 26: "... the WRF model was in better ..."**
Thanks!
6. **Page 3 Line 32: "... to initial conditions, ..."**
Thanks!
7. **Page 3 Line 34: What community?**
We agree, it should be clarified that it is the wind energy community.
8. **Page 4 Line 8: "for a number of reasons: ..."**
Thanks!
9. **Page 4 Line 10: "... who rely ..."**
Thanks!
10. **Page 4 Line 17: "... of the simplest terrains ..."**
Thanks!
11. **Page 6 Line 4f: Can the authors provide a reference for this approach. Why not use the data at 50 and 70 meters?**
Comparison of the (single anemometer) measurements at 40 and 60 meters to the extrapolated/interpolated measurements indicated that the errors due to flow distortion were much larger than the errors from extrapolation/interpolation. Peña et al. (2016) and Fabre et al. (2014) show that the impact from flow distortion due to the mast can be large. Pena et al. shows a discrepancy of more than 10% between two anemometers at the same height, in the case where one is located upstream and one is downstream from the mast, at Høvsøre.
12. **Page 7 Line 16: Nudging is an assimilation method.**
We agree, it is redundant.

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13. **Page 8 Line 2: "This study is ..."**
Thanks!
14. **Page 8 Line 22: Tilde is shifted**
Thanks!
15. **Page 9 Line 5: "... between two levels ..."**
Thanks!
16. **Page 9 Line 12: "... the model output data were ..."**
Thanks!
17. **Page 10 Line 7: The variance is given in % but there is no reference to what the numbers refer.**
Thanks, we should clarify that it is % deviation (relative to the observation).
18. **Page 11 Line 2: "... and the intermodel variance is ..."**
Thanks!
19. **Page 11 Line 9: "... mesoscale datasets and ERA-Interim show a significant ..."**
Thanks!
20. **Page 11 Line 11: "... varies between ..."**
Thanks!
21. **Page 11 Line 13: "... clear that the correlation ..."**
Thanks!
22. **Page 11 Line 16: "by at"?**
Thanks!

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23. **Page 12 Line 6: "... instead shows an ..."**
Thanks!
24. **Page 13 Line 2: "... dataset does not ..."**
Thanks!
25. **Page 13 Line 3: "... and tends to ..."**
Thanks!
26. **Page 14 Line 9: "... dataset captures the ..., but shows a ..."**
Thanks!
27. **Page 14 Line 14: "... dataset, however, does not ..."**
Thanks!
28. **Page 14 Line 18: "... roughness varies a lot ..."**
Thanks!
29. **Page 16 Line 6: "... Fig. 5), two of the sites are investigated."**
Thanks!
30. **Page 16 Line 7: "... with a strong dependency of surface roughness on the wind direction."**
Thanks!
31. **Page 16 Line 8: "... variation were/are binned ..."**
Thanks!
32. **Page 18 Line 1: "The hypothesis of this study is that ..."**
Thanks!
33. **Page 18 Line 3: "... factors are expected to ..."**
Thanks!

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34. **Page 18 Line 4: Please provide more detail: What is meant by "source of orography"?**
Elevation data set. We agree, it should be clarified.
35. **Page 18 Line 7: I would expect that the model itself, initial boundary layer conditions and simulation time aka forecast lead time have a large impact on the model estimates. I wonder why the authors hypothesise that the impact of these factors will be of a lesser degree.**
Initial results did not show any significant impact of these factors. However, we suggest adding new results to the paper, which looks at the model performances related to the PBL scheme, grid spacing, and simulation time.
36. **Page 18 Line 10: "... significant correlations were ..."**
Thanks!
37. **Page 18 Line 18ff: When calculating correlations for wind speed over such distances (up to 500km), large correlation coefficients are to be expected given the data set used. A better approach would be to filter-out low frequency (e.g. days, weeks, months) variations in the time series in order to retrieve the intra-day wind speed variations. Then these can be used in an analysis to remove the obvious correlations between the mast sites.**
We would like to omit this part of the manuscript completely. We do not believe that it adds enough value to the study. We propose revising this part of the paper.
38. **Page 20 Line ": "... by an underestimation ..."**
Thanks!
39. **Page 21 Line 1: "... schemes used in ..."**
Thanks!
40. **Page 21 Line 11: "... largest biases are/were observed ..."**
Thanks!

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41. Page 21 Line 23: "... to accurately estimate ..."

Thanks!

42. Figures 3 to 9: Why is the MM variance plotted when every single MMi is shown in the diagram?

It can be tricky to estimate the spread of 20+ lines that are near each other, and the standard deviation adds a simple metric to show the spread, while not hiding the lines for each model.

43. Figure 10: The dashed diagonal is misleading as it suggests that there is meaning to it which is not as far as I understand (Model resolution in km against wind speed bias in m/s). Please correct me if I am wrong.

We suggest omitting the figure completely, as it does not add enough value to the study. However, we propose to revise this part of the study, and add new results that goes into details about the impact of the grid spacing in the modeling results.

References:

Peña, Alfredo et al. "Ten Years of Boundary-Layer and Wind-Power Meteorology at Høvsøre, Denmark", *Boundary-Layer Meteorol* (2016) 158: 1. doi:10.1007/s10546-015-0079-8

Fabre, Sylvie, et al. "Measurement and simulation of the flow field around the FINO 3 triangular lattice meteorological mast." *Journal of Wind Engineering and Industrial Aerodynamics* 130 (2014): 99-107.

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