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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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Final author comments

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The comments from the reviewers highlighted important issues that we would like to adress. We propose to make a number of changes to the manuscript. The main change we propose is to section 3. We intend to completely remove section 3.4, and replace it by a new section that go into details with the performance of the models related to three specific model options: the PBL scheme, the grid spacing, and the simulation time. We also intend to divide section 3 into just three subsections: 3.1 "Mean quantities and distributions", 3.2 "Relating performance to model setup", and 3.3 "Wind energy application".

We also propose to omit the dataless sites from the paper, since all three reviewers made this suggestion.

Suggested changes

Sections removed: 3.4, 3.4.1
Figures removed: 4, 10, 11, 12
Tables removed: A2, A3

New figure: A figure with two subplots: one that shows the distributions of modelled and observed mean wind speed for each month of the year at the three sites, and one that shows the corresponding distributions of Mean Absolute Error (MAE) for wind speed, for the models, for each month of the year, at the three sites.
New figure: Figure that shows the distributions of MAE for wind speed, for the models, for five classes of atmospheric stabilities.
New figure: Figure that shows the Root Mean Squared Error (RMSE) for the shear exponent vs the normalized RMSE (NRMSE) for wind speed for the modes at the three sites. This is used in the new section "Relating performance to model setup".
Three new tables: Aggregate statistics (mean, median, std, min, max) of RMSE for the shear exponent, and NRMSE for wind speed, grouped according to PBL scheme used, the grid spacing, and the simulation time. This is used in the new section "Relating performance to model setup".

Other changes we suggest:

1. The introduction will be shortened and improved, as per reviewer comments.
2. The language will be improved and typos removed, as per reviewer comments.
3. Dataless sites will be omitted.
4. We would like to add results and discussion of the ability of the models to capture the annual cycle.
5. We would like to add new results and discussion of the model errors in different atmospheric stability regimes.
6. We would like to combine section 2.2 and 2.3.

7. We would like to shorten, improve, and combine section 4 and 5, as per reviewer comments.

Comments to reviewers

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

Comments to anonymous reviewer #1

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 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 manucscript 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 reprase 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.**
   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. Since it is so few models we are talking about, we decied 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. Tow consecutive "submitted".**
    Thank you.

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 synop-ticand 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.

Comments to anonymous reviewer #2

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. Often, 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 unneces- sary. 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 intercomparison: 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 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!
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 discrepency of more than 10% between two anemometors 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.
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!

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!

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!

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 eachother, 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 itwhich 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.

Comments to anonymous reviewer #3

This paper presents an interesting comparison of mesoscale models at sites with flat orography. While the study is of relevance for the community, I believe it lacks in some aspects which could be easily fixed. First of all, the introduction seems too long. It could benefit of a condensation of some of the informations reported.

We agree with the comment about the length of the introduction. We propose to shorten and improve it.

Also, one could argue about the need of including the dataless sites in the comparison, since they don't add much value to the study. I would consider of removing them.

We agree, and so did the other reviewers. We propose to entirely omit the dataless sites from the paper.

As the authors state in the conclusions, "While it was a key objective of this study to determine the model setup choices that have a large impact on the models ability to estimate the wind climate accurately in the lowest part of the PBL, only weak indications were found.". I suggest putting more emphasis in trying to describe the differences and advantages/disadvantages of using different model configurations.

We tend to agree, and suggest a revision of the section, leaving out much of the old content, and adding new results that go into more detail with three specific model options: PBL scheme, grid spacing, and simulation time.

Typos:

Thanks alot for finding these.

1. **-page 1 line 3: replace "a" with "an"**
   Thanks!
2. **-page 1 line 15: unnecessary "-"**
   Thanks!
3. **-page 2 line 27: replace "Meller" with "Mellor"**
   Thanks!
4. **-page 3 line 11: replace "spacial" with "spatial"**
   Thanks!
5. **-page 4 line 28: replace "is shown" with "as shown"**
   Thanks!
6. **-page 6 line 1: replace "Cabuaw" with "Cabauw"**
   Thanks!
7. **-page 13 line 24: replace "srpead" with "spread"**
   Thanks!
8. **-page 16 line 6: "exists" is repeated**
   Thanks!
9. **-page 16 line 7: replace "represeting" with "representing"**
   Thanks!

10. **-page 21 line 16: replace "used assess" with "used to assess"**
Thanks!

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