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Comment on wes-2022-21

Anonymous Referee #1

Referee comment on "An investigation of spatial wind direction variability and its consideration in engineering models" by Anna von Brandis et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2022-21-RC1, 2022

A review of

An investigation of mesoscale wind direction changes and their

consideration in engineering models

By Anna von Brandis et al

General comments:

This article has two main elements. The first element is a quite comprehensive

examination of the spatial behaviour of wind direction change of German Bight region of the North Sea. The second is an assessment of a method to incorporate the spatial variation of wind direction in engineering wake models.

Both elements are in my opinion very interesting to the readership, and worthy of investigation.

The first element examining the behaviour I would say is the stronger part of the article. Several novel and, I imagine, "discussion starting" maps are figures are shown. Although it may be beyond the scope of the paper, it would be interesting to further examine some of the root causes for these interesting direction change properties and phenomena. The article also applies nicely the NEWA datasets, in this research work.

The second element assessing the improvement gained by using the curvature of streamlines in engineering models, is the weaker part of the article. And I address several aspects of it in the specific comments. Overall the criticism that I think needs to be addressed concerns, i) some more guidance about the implementation of the direction change in the models, ii) some more discussion of the detail about the differences in the estimates coming from BLM and SWM (Fig 9) (for example is the sign of the difference telling us anything about the behaviour? And if we expect around 14% of the time long wakes that have curvature, is that consistent with the no significant difference in yield estimates), and iii) the term uncertainty is used a fair amount but I don't think it is uncertainty that is discussed, no SCADA data is used in validating the models, and it is therefore difficult to estimate model errors. Instead it is differences in estimates being discussed, and since the sign of the difference and the sign of the expected error is not really discussed the reader is left unsure whether the differences are likely to lead to improvement or not.

Another issue is the assumption of stationarity of the streamlines for each application of the wake modelling. I have suggested some more discussion on this and thinking of advection timescales in the specific comments. For me, although this is an obvious disadvantage of the method, I think the method is certainly worthy of investigation, as it

may provide good results within its range of validity. So I see this article as providing a good stepping stone in the research.

Overall, I'd say the article is suitable for publication if the general and specific comments are all addressed adequately in a revised manuscript.

Specific comments.

L9 "Pressure systems" is used quite frequently, I'd suggest "synoptic pressure systems" is a better term.

L15 About "good agreement if the underlying mesoscale " … in the main part of the article it would be nice to support this a statement a bit more. For example show examples where the underlying mesoscale situation is not represented well.

L19 When comparing the computer total energy yield it was found no significant differences using the two methods. In the main text it would be good to learn more about the difference, the sign of the difference and discussion about how good the estimates are in the first place, i.e. since an engineering model is used over longer scales.

L30 Important correction. The Agora study did not show that yield decreases with

increased installed capacity. It did show the efficiency decreased, but yield still increases with additional capacity. Please correct.

L57 Do the authors make a distinction between wind veering and backing. I suppose veering is the usual direction of wind direction with increase height from surface in an Ekman spiral setting.

L64 About "Despite the higher uncertainty… " what is the context of this statement. For resource assessment application of CFD may not necessarily give better results in practice because of the limitation of domain size, modelling of atmospheric stability conditions, number of simulations and so on. Please clarify.

L72 "... benefits of grid-less wake models", this might be a new term to the reader, please can you explain a little more about grid-less models, and their benefits.

L83 About "investigate", perhaps a better word is assess or evaluate. Investigate is a bit too general.

L120 About "normed", please can this process be clarified. I think it refers to how many degrees of turning there is per 100 km.

Fig 1: Nice map, I suggest the different scales and the lines are given different colours to make them stand out a bit more.

L126 Heading, consider adding "*synoptic* pressure systems"

L129 "drop in *mean sea level* pressure " or "drop in *surface* pressure

L155 Eq 4 and 5. I think it would be nice with more guidance to the reader about this coordinate system part. For example a diagram or two, showing how the coordinate system looks in relation the streamlines and perhaps a visualisation of the wake getting curvature in some way.

L85 What is "REWS"? It is not explained anywhere. Please clarify.

L196 "emblematic" is an odd word choice, I think "indicating" would be better.

L218 It is a bit unclear here what was chosen for the model coefficients. In some places low k_TI and k_b is used to make wakes deeper and prolonger, those also more realistic values are used too.

L223 About the calibration of coefficients, did that use the BLM SWM set-up? Would it make a difference which was used?

L237 and Eq 8 “REWS” is used again. What is it?

Fig 3: Small thing. In the main text and Table 1 <14 m/s is referred to, but in the figure the binning uses 15 m/s.

L290 and thereabouts: can the authors say anything about the asymmetry of the turning characteristics. I think this is pretty interesting.

L299 Again the “normed” term again, please correct. Also in caption of Fig. 4.

Fig 4: I like these graphs. Not seen anything like this before.

Fig 5: The colour scale has “KDE” please explain what this means, and the caption spells it

out but how does the reader understand it. Does it have a unit?

L332 “exemplary” is not the correct word I’d say. “Example” is better.

L393 From what is shown in the article I think it is difficult to claim the extent is significantly greater in models. The figure 8 has different scales and not even the plotted quantities are the same. For SAR it’s the NRCS and not the wind speed.

L425 “fade away” replace with “reduce”.

L440 Why is it the “uncertainty” that is reduced here? Isn’t it just the difference in the two estimates? Why is that uncertainty?

L447 and onwards: This is a good point for discussion. I think the authors could consider some scale of interest here. For example 100 km takes 167 minutes (nearly 3 hours) at 10 m/s. While the update time from NEWA is 30 minutes. At 10 m/s the advection would be 18 km. In the future it would be nice if there was validation against SCADA data so the stationarity assumption of the streamlines could be tested over different time and length scales. One hypothesis could be that for intra cluster scales it may be ok, while for German Bight scales the assumption is not so suitable. In this case the wake effects may be much smaller anyway.

L486 About “far greater fidelity”, I think this was the case in the examples shown with large amounts of streamline turning. One could add the comment that the BLM showed wakes crossing each other, and this was, I’d argue, a non-physical situation.

L488 I’d say the “accuracy” can only truly be assessed with comparison against SCADA data. The conclusion about the accuracy depending on the accuracy of the underlying NEWA data, I say is actually only at this stage a reasonable hypothesis, because it is not actually tested in the article, as far as I can see.

L495 About “reduction in uncertainty”, again this reasonable hypothesis, but not tested again SCADA data. The article describes differences in the two approaches, however it is not clear to me that one is absolutely shown to better than the other. This would require validation against SCADA data.