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Reply on RC2

Anna von Brandis et al.

Author 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-AC2, 2022

Reply to Reviewer 2 by Anna von Brandis et al

This paper covers an important topic, namely how spatial variability in wind direction affect the evolution of wind farm cluster wakes and how this can be implemented in engineering wake modelling tools. I do however have some substantial concerns that need to be address before I can recommend the paper for acceptation in wind energy science. The improvements necessary are detailed below:

☐ *We thank the reviewer for the careful evaluation of our manuscript and the comments to the work presented. We address all the comments of the reviewer separately below.*

Major comments:

1. "spatial variability in wind direction" covers better the content of the paper than "mesoscale wind direction changes" and should in my opinion be used throughout the paper. First of all, the systems that are discussed here are often larger than what is traditionally described as mesoscale systems. Moreover, the paper focuses on variability in space and not in time (streamline instead of lagrangian approach).
☐ *We partly disagree with this comment. We think that in the main part of the manuscript the methodology, underlying datasets and figures clearly enough describe and demonstrate the associated scales of our investigations. We agree however to change the title to "An investigation of spatial wind direction variability and its consideration in engineering models".*

2. The qualitative comparison with synthetic aperture radar (SAR) is the center of the paper, but the setup of the mode experiments and comparison with data should be improved. To produce this figure, the wake model parameters were adjusted to minimise wake recovered and to achieve slowly decaying wakes. However, the authors argue that this does not yield a goods agreement with real production data and they propose other parameters for realistic simulations. It would therefore be much fairer to produce the maps and the comparison with SAR for realistic coefficients of the wake models. Moreover, the current comparison is not straightforeward to interpret since SAR data in figure 8 are displayed as the backscatter value of the normalised radar cross-section. It would be much better to use here the geophysical quantity wind speed which also improves the ability to compare the observed versus the modelled far wakes.
☐ *Although the comparison with SAR is important for our conclusions, we do not see it as*

the focal part of the paper. The main purpose of Fig. 9 [initial version: Fig. 8] is to provide evidence of the wake turning due to mesoscale wind distribution through the SAR data and to demonstrate that such turning can be represented reasonably well only with the SWM algorithm. Hence, the focus is on the trajectory that the cluster wakes have rather than their extent. For this purpose, we believe that the best way to let the reader focus on the wake trajectory predicted by the models is to avoid vanishing wakes. Thus, we decided to tailor the wake model to have a negligible wake recovery. Furthermore, we wanted to avoid discussing cluster wake extent and velocity deficit. Therefore, we don't find necessary to convert the SAR data into wind speed fields, as this would also add further uncertainty. We have however reformulated the paragraph about the definition of the wake model coefficient to describe our intention in more detail: "For the results of Sect. 5.2.1, where the focus it on showcasing just the computed wake trajectory, the wake model parameters were adjusted to minimize wake recovery and to achieve slowly decaying wakes. The near wake length was set to zero, kTI = 0.05 and kb = 0.0. Such a set of parameters exacerbates unrealistically the power loss due to wake effects within the farm, and it is not advisable for computing a meaningful energy yield of the two clusters. Therefore, they are not suited for a meaningful quantitative analysis. The parameter used for the quantitative evaluation in sect. 5.2.3 were calibrated using an optimization procedure involving the supervisory control and data acquisition (SCADA) data of one of the wind farms in the wind farm clusters considered. The procedure, here not presented for brevity, determined the coefficients kTI = 0.23, kb = 0.003, α = 1.4, and β = 0.077 to guarantee the best agreement between the engineering model and the real production data."

3. I am not convinced with the use of the term "reduction in uncertainty" in the paper. In my opinion what the authors propose here is rather an attempt to include a process, namely the curved propagation of cluster wakes as a result of spatial variability in wind direction and thereby improve existing wake models. Even though this improvements cannot be quantitatively demonstrated in the current framework. For example, in line 440 where you state that applying the proposed model reduces uncertainty by up to 0.7% when estimating the interaction between neighboring wind farm clusters. This is just referring to a sensitivity where curvature of the path is included versus a situation where this is not included and is not a metric for reduced uncertainty. Please go through the entire paper to critically reflect on the use of the term uncertainty and reduction of uncertainty.
☐ *Thank you for this comment. The term was indeed used with too much liberty in the context of the paper. We are convinced that considering wake turning is fundamental for a correct estimation of how neighboring wind farms hamper each other performances. Therefore, we were eager to quantify how including this phenomenon would have affected the engineering model results. We now made very clear that only in the case the SWM model is validated and demonstrated better than the BLM, the differences across each other are a step forward in the engineering model accuracy. We modified several parts of the manuscript to reflect this in prticular in Section 5 and on the Conclusions.*

4. In Section 5.1.1, I do not understand the logic of taking the mean of the medians. If I understand it correctly, you first take for every time frame the difference in wind direction compared to FINO. Then you take for each pixel the yearly median of this difference. Subsequently, you average over a 30 year period. Why making it so complex and not just taking the median of all the timeframes for each pixel (or the mean if you prefer)?
☐ *We didn't take the mean as the wind direction changes can be varying quite significantly into and the median provides a better statistical representation of what we want to show. In particular, in Figure 5 [formerly Figure 4], we want to show how the upper 25% and lower 25% that contribute in a significant share to the long-term conditions. The median (aka 50th percentile) is the corresponding value to 25th and 75th percentile. The mean then reflects averaging over a thirty year climatology.*

5. In the same section, I do not understand why your value does not become zero when looking at the FINO grid point for 25th percentile and the 75th percentile. Per definition, the direction difference with reference to FINO should always be zero at FINO. Moreover, there is little discussion anyway on the 25 and 75th percentile, so it is perhaps better to remove this.
☐ *You are right, we described this in the text but not in the Figure caption that the data are not based on a single point but an average over a larger area (gray area in Figure 2). We added this information to the caption as well. In addition, we added another sentence to interpret the results of Table 2: "Table 2 also reflects that while the mean wind direction change can be quite low, a large share of situations in particular for wind speeds in the partial load range (< 10.0 ms−1) exist with significant wind direction changes."*

6. In the same section, I am not sure if it does make sense to take the median (or the mean) of a direction (which is a circular metric). The average wind direction of -179 deg and +179 deg is not 0 deg. Am I missing something on how you exactly did the data processing (and then you can perhaps explain it better). Of course if the difference in the direction is in between -90deg and +90deg it is okay, but I would expect that larger differences can occur over a distance of 200 km or so? Even if you scale it back to 100 km there might be similar issues. Please check.
☐ *Of course, we did account for the circular properties of the wind direction variable. We didn't calculate the average wind direction, but the average/median of the CHANGE of the wind direction. The average wind direction of e.g. of 359 degrees and 1 degrees would be 180 arithmetically but in reality a wind from North (i.e. 360 degrees). In our case, we reflected this by subtracting/adding 360 degrees in case a direction change is larger/smaller than +/-180 degree. To give an example: When we calculate the wind direction difference of a wind from 359 and 1 the resulting difference in our case is 2 degrees. Larger differences approaching +/- 180 degrees over 200 km do occur, but very rarely. This could be caused by e.g. winds at a front / strong convection. This is by the way reflected in the scatter plot in Fig. 6 [formerly Fig. 5], where some cases of wind direction changes of > +/− 100 degree exist. We added one sentence to the Method section (2.1.1): "In all cases where wind direction differences were calculated, we accounted for the circular nature of the wind direction variable."*

7. Figure 7 and 8: add the positions of the wind farms to this figure. Moreover, make a map using a lat/lon grid similarly to the other figures that you created. Make Figure 8 so that you can directly compare the model with SAR by using the same map and also by plotting wind speed in both the model and SAR.
☐ *We don't understand why the reviewer suggests adding the wind farms to these figures. The individual wakes of the turbines and their origin are visible in Figures 7 and 8 [revised version: Figures 8 and 9]. In addition, the names of the wind farm clusters are given in the figures. The actual positions of the individual turbines are given in Figures 1 and 2. If we add those to Figures 7 and 8 again, we risk showing too much information and distract the reader from the wakes that we want to show in these figures.*

8. Figure 9: you plot the standard deviation of the relative difference. I am a bit puzzled why this is an interesting metric.
Why not simply plot the relative absolute difference, which is a much easier metric to intuitively interpret. The standard deviation will also never be a negative number, so what you plot in the figure cannot be a standard deviation, since negative numbers appear in the figure.
☐ *The caption of the figure has been updated to better clarify that, in the heatmap, only the color of each bin represents the standard deviation, for which there is indeed no negative value. The numbers annotated within each bin are instead the relative absolute difference (or mean of the relative difference across all the observations in a bin). We think that it is important to show the standard deviation because it suggests that the differences between the two considered models are very high on a single situation basis.*

9. Overall after reading your paper, although I'm not an expert on wake modelling, your results seem to imply that turning of the wakes in relation to spatial variability in wind direction is relevant, but that the current engineer models of the wind farm wakes are perhaps not appropriate yet to firmly demonstrate this. Your conclusion on line 486 is that "it is concluded that the new model can represent the flow with the fact greater fidelity than becoming baseline approach." This statement is much too strong given the limitations of the analysis currently done and discussed above.
☐ *The statement has been changed to "in a way more consistent to physics". Such claim refers to current Fig. 8, where we demonstrate the BLM to predict two wakes crossing each other, a rather non-physical situation. Concerning the conclusion that engineering models are not suitable to demonstrate the effect of wake turning, it is important to consider that accounting for this phenomena can easily either produce benefits or malus to the power production of the farm considered when we sum over all the differences to compute yield they tend to balance out. However, it is reasonable that shortening the period of yield evaluation or changing the location and the number of wind farms could provide a different result.*

10. Also the line 488 doesn't clearly follow from the results where you state that "the new model should consistently outperform the baseline approach that neglects wake turning." Well perhaps in principle it should (because in reality the wakes move with the flow) but the paper does not firmly demonstrate that.
☐ *Thanks. We changed the sentence to: "in no occasion the BLM provided a better representation of the wake propagation than the SWM."*

11. A related question: are there better ways to model the wake of a wind farm than adding up the wakes of the individual
turbines? In figure 7, the wakes of the individual turbines remain visible over a long distance and I wonder whether there is observational evidence for this: Individual streaks remain visible in the model as far as 50 km downstream. Is this realistic? I therefore would like to see a brief discussion on this together with a discussion on how wakes of farms are implemented in current engineering models, how this model relates to other approaches and whether the approach presented here is accurate enough to study wind farm wakes and interaction between wind farms.
☐ *In Fig. 7 we wanted to demonstrate the full path followed by the wakes within the domain according to the different models. For this reason, we tweak the wake model to reduce wake recovery to a minimum. With more pertinent wake model coefficient, such as the one used in the quantitative comparison of SWM and BLM the single wake streaks will decay much faster into a wind farm wake. So far, despite the superposition of single turbine wakes presents many flaws, it is still the computationally cheapest way to compute wind farm yield with acceptable level of confidence. The comparison of different large-scale wake modelling approaches of different complexity is a very recent topic in wind energy science. We added reference to a very recent study that compares mesoscale and engineering wake models in the outlook "Future development, partly already initiated, encompasses the attempt of validating the model with a combination of lidar measurements and wind farm production data, and the comparison to other large-scale cluster wake modelling approaches such as mesoscale wake modeling (Fischereit et al., 2022a). The study by Fischereit et al. (2022b) provides an attempt of comparing different wake models of different complexity although on a smaller scale than the German Bight."*

Minor comments:

1. It would be good to add a short description how the wake expansion is related to the atmospheric conditions. From the equation 6, it is clear that the wake deficit is in turn a function of ambient turbulent intensity, which is a function of meteorological conditions. A

paragraph explaining how this relation is exactly formulated would add strong value to the paper. Later in the paper, this can be used to describe how the wake expansion relates to meteorological conditions in the cases that are presented (low pressure system).
☐ *Thanks for this comment, we added a couple of sentences to section 3.2 of the revised manuscript to reflect this point: "The wake expansion coefficient, k☐, determines the length of the wakes, and it should be connected to the meteorological conditions of the atmosphere, i.e. atmospheric stratification. However, it still remains unclear in the framework of engineering modelling how to express such a dependency. In this work, we adopted a linear relation on the turbulence intensity that should at least partially capture how different atmospheric stratification affects the wake recovery."*

2. Section 2.2 on the implementation of direction changes into engineering models should be improved. It is unclear what x exactly means. Moreover, r(t) is the path as a function of time whereas s is distance along a path. The distinction should be better explained . More details should be given on the determination of the coefficients of the engineering model for the realistic situation. This is not a detail of the paper, because the extent of the wake and consequently the turning of the wake, is very much determined by these parameters therefore the potential impact of spatial variability in wind direction is very much dependent on these parameters.
☐ *We added a new figre (now: Figure 3) to Section 2.2, showing the relation of the piecewise-linear streamline and the local coordinate systems. Also, a visualization for the example of the Jensen wake model in heterogeneous background flow with wind rotation has been added. Hopefully this clarifies Eqs. (4) and (5). In steady-state flow, any point p along the streamline curve can either be described as a function of the travelling time t, or, equivalently, as a function of the path length s, counting from the wake-causing rotor. The text in Section 2.2 has been extended and now describes the method in more detail.*

3. In Section 5.1.1 it is shown and discussed that wind direction are large for very high wind bins. Can you analyse the cause of this? Are these situations where the location is close to the central point of a deep low pressure system with strong wind speed but also strong curvature?
☐ *Thanks for this comment. We are not showing particular situations here, but it is very likely that these large changes in wind direction are associated with e.g. the passage of cold fronts of low pressure systems that typically are associated of with a strong change in wind direction from southerly to more westerly or even northwesterly winds. We added one sentence to discuss this:*
"Very high wind speeds, despite less frequent, come along with larger positive direction changes. One explanation for this could be the passage of cold fronts of low pressure systems that typically are associated of with a strong change in wind direction from southerly to more westerly or even northwesterly winds."

4. Please look again figure 5 on what you plot on the x-axis. You take the logarithm of a variable and then you plot this on a logarithmic scale.
☐ *Thanks for spotting this. The log was removed from the axis label.*

Further Changes:
1. The results in this paper were obtained using the Fraunhofer IWES in-house code flappy, which has recently been released as open-source software under the new name FOXES - Farm Optimization and eXtended yield Evaluation Software. This has been mentioned in the manuscript under Section 3.1 and Code and data availability.