Wind Energ. Sci. Discuss., referee comment RC1
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Comment on wes-2021-7

Anonymous Referee #1

Referee comment on "Recovery processes in a large offshore wind farm" by Tanvi Gupta and Somnath Baidya Roy, Wind Energ. Sci. Discuss.,
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The study presents numerical simulation runs by the WRF model to investigate recovery processes from an hypothetical 50x50 km2 offshore wind farm. The WRF is driven by real weather data. The experiments quantify the recovery processes under different wind speeds and wind turbine spacing.
The study is well structured and written, although the language can be improved at some parts. The results are presented clearly, but some figures need be improved to be more accessible to the reader. The discussion picks up on the novel results but also remains very superficial at some points. My main criticism of the study is the neglection of the stability, which is a main control parameter for wake recovery. This is further explained in the comment sections. With the consideration of this aspect and further improvement, the study can be a very valuable contribution to the offshore wind energy community.

General comments

The abstract gives a detailed description of the study, but remains very vague on the results. The results need to be presented more precisely and concretely, (e.g how is high inter turbine spacing defined, densely packed etc…). For more details please see the specific comments.

My main criticism is that the study does not take into account the stability and boundary-layer height regarding wake recovery. For the description of offshore wind farm impacts on the atmosphere (such as far wake effects/blockage effect or the influence of the farm on vertical turbulent moment flux, as mentioned on p.13 l.24), a consideration of the stability and ABL top (as mentioned in p.10 l.25) as major parameter, along with the park layout and wind speed, have been identified in several recent studies (e.g. Djath et al . 2018, Siedesleben et al . 2018, Cañadillas et al. 2019, Platis et al. 2020 etc…). However, this study only takes turbine spacing and the wind speed into account.
In addition, the results from these mentioned studies and further recent studies about the ongoing investigation of the far field effects of offshore wind farms are not addressed in the study.

Also on p.13 l.24: Turbulent vertical mixing depends on the thermal and dynamic stability. Also for strong horizontal wind speed during strong convective conditions vertical recovery may remain the main contributor. Therefore, I highly suggest at least to take the stability (e.g. lapse rate or Richardson bulk number) for the investigated cases into account and include them in the discussion.

Figures have to be re-worked including units to the scales. For some figures I also recommend to enlarge them to make them more readable. Also the range of the scale hat to be rearranged as e.g in figure 6 a) the variation is seen hardly.

To make things easier for the reader, I suggest to define the mean wind direction as positive x. This will help the reader to compare easier cases A with B and C. This will also help to better distinguished between flow effect parallel and perpendicular to the flow e.g., the interpretation of Figures 3 b) , 4, 5 and 6.

Why is the recovery only presented for the wind farm domain (Fig. 6). I expect also far field effects similar to the wake effects to be seen in the vicinity of the wind farm. Also this will give a broader picture of the upwind and downwind effects.

Specific comments

p.1 l.14. How is high defined? Narrow spacing? Please be more precise.

p.1 l.16: What is meant by can be quantified using low-order empirical equations? Please be more more concrete.

p. l.17. What is meant by high wind speed. Which range are you referring to?

p.2. l.15: What version of the WRF model? Which wind turbine parameterization ? Please rephrase the sentence in the abstract or in the introduction as they are identical.

p.4. l14ff: I suggest to add a figure showing the relation between grid cell and turbine spacing in order to makes things more clear for the reader

p. 5 l.15: This is still a simulation, so the term ‘realistic’ is not appropriate.

p.6. l.5: Please introduce here what k,i,j is referring to.

p.6. l.18:Over which domain are the horizontally averaged? Over the wind farm domain?

Eq. 8 Please describe what is defined by î and Äμ ?

p.9. l15: Please give a broader description about the statistical analysis.

Eq. 15: The denominator on the right-hand side of Eq. 9 should contain the unit. I also suggest to write 140m – 28m

p.7 l.25 Why small v ?

Fig. 3. The figure is hard to read, especially 3c). Please enlarge the plots and the labels. Please add a unit to the scales. For case A the resolution is way to small to be able to follow the analysis on page. 9. l. 15-16.

p.10. l.5: Not true several other studies such as Platis et al. 2018, 2020, Siedersleben et al. 2018 reported a deceleration of up to 40 % in the wake of offshore wind farms.

p.10 l.25. What is the height of the ABL top?

Fig. 4. I recommend to mark the area where the wind farm is located.

Fig. 4: Case A III. Why is there a deceleration and then an acceleration of the flow between 0-1000 m and at x= 780-850 km?

Fig. 4 The upwind deceleration seem very impressive. I am wondering whether a too small simulation domain is causing an intensification by boundary reflections? Is there a way to assess this influence ?

Fig. 4 b. Why is a streak pattern visible? Can this be also attributed to artifacts caused by

the simulation?

p.11.9 ff. I do not understand how this argument contributes to case C-I. Please describe more clearly.

Fig. 5 I don ot understand the meaning of the legend at the second left figure in the first row.

Fig .5. The description and argumentation of the results is at some points not very precise.

p.11. l.17. It would be helpful for the reader just to mention again very briefly the difference between synoptic and micro scale.

p.11 l.8-10. "It is possible that this meso-scale momentum transport aids in the wind farm recovery
by making more momentum available for downward mixing by turbulence." This is very speculative. Is there a way to justify it ? Can the variations at different heights be explained?

Fig. 6: Why does the plots in case of III look much coarser then for I and II ?

Fig. 7a) Why is there such a sharp boundary (jump) at about x= 4x10-3 ? Because of the different cases? This could be mentioned in the text.

p. 17 l. 11. Please refer to the equation or describe the integration by an seperate equation.

p.18 l. 31. What do you mean exactly by synoptic scale effects?

Technical corrections

p.2. l.1 ff: Please put the citations in chronological order.

p.4 l.9: Please correct: boundary-layer scheme

p.4 l.10: Please correct: second-order moments

p.6 l.22 typo error 'that'

Fig. 7 b)-d) Please title the figures with its specific cases.

References:

Djath, B., Schulz-Stellenfleth, J., and Cañadillas, B.: Impact of atmospheric stability on X-band and C-band synthetic aperture radar imageryof offshore windpark wakes, Journal of Renewable and Sustainable Energy, 10, 043 301, 2018

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