

Interactive comment on “Observed and simulated turbulent kinetic energy (WRF 3.8.1) overlarge offshore wind farms” by Simon K. Siedersleben et al.

Anonymous Referee #2

Received and published: 10 September 2019

General Comment: The manuscript by Siedersleben et al provides useful and interesting results about large scale wind farm cluster wakes and the modelling of their impact by means of mesoscale simulations. The focus on comparing turbulent kinetic energy and its sensitivity in the modelling is an important and actual research topic and has not been studied extensively. In general, the manuscript is also quite well written and the figures are easy to read. However, I have four major and a number of minor comments and thus recommend the publication of the manuscript after dealing with major revisions.

Major Points: 1) The reviewer had experienced problems with the data of the Bun-

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desnetzagentur (coordinates and information such as hub heights and diameters) and knows that these data need to be taken into account with great caution. For instance, the wind farm Meerwind Südost consist of 80 turbines according to the operator [1]. The authors write that only 74 turbines were accounted for! The authors will thus should double check the coordinates and all other information that were taken from the Bundesnetzagentur data and correct all the wrong information and rerun all cases that used wrong specifications.

2) A quite big difference between the surface measurements at FINO1 and the air temperature above is reported in two cases that does not physically fit to the measured stability above the sea. The reviewer suggests to compare several other sea surface stations (e.g. BSH buoys) in the surroundings and also a high resolution sea surface dataset such as OSTIA to verify the measurements at FINO1. These could also be affected by algae growth and thus have a bias.

3) When measuring wakes it is important to know the operational state of the wind farms that are responsible for the wakes. This data is mostly confidential. However, public data sources (such as e.g. the Energy charts [2]) allow for estimating if the energy production at the respective wind speed is realistic and the majority of the turbines were actually operating. This should be done by the authors for the cases presented here.

4) Between WRF 3.7.X and WRF 4.X a change of a constant in the MYNN model (COARE OPT from 3.0 to 3.5) led to significant differences and biases in the wind (see e.g. [3]). Thus, this parameter was changed back later by the model developers (see e.g. [4] and [5]) in the WRF standard version. The reviewer suggests to test the impact of this on the results by comparing a different WRF version or changing it manually in the version used by the authors.

Minor Points:

Title: The reviewer suggest to rewrite the title to something like: Turbulent kinetic en-

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ergy over large wind farms observed and simulated by the mesoscale model WRF

Abstract: Because. . . -> You should not start a sentence with . . . because. . . .

Page 1 – Line 15: Offshore wind energy. . . -> the reviewer suggest to also give a time frame since when. Maybe something like “in recent years” or “in the last decade”

Page 2 – Line 9: Given generally low mean vertical velocities -> Yes, this is of course true but only for the ambient flow. The vertical velocities close to the rotor can be very high due to the rotational speed. Thus, either you should add the scales you are looking at (on mesoscales) or add the information that this is about the ambient flow.

Page 2 – Line 11: Global and regional climate simulations . . . -> This term is used a few times in the paper but WRF is in its first place a weather model. Thus, the reviewer suggest to add “weather models” here.

Page 2 – Line 17: . . . both WFPs delivers. . . -> deliver (no s)

Page 2 – Line 28: . . . we want to know -> the reviewer suggests to rewrite: “. . . we try to find answers to the following research questions.”

Page 3 – Line 8: Unique. . . -> Do you mean “only”?

Page 3 – Line 9-10: As we have. . . -> This sentence is difficult to understand. Maybe rewrite: We have a SAR satellite image for one of the cases investigated in this study, a brief. . .

Page 3 – Line 13: It would be good to have an introductory sentence here, something like: In the framework of the WIPAFF project, the research aircraft Dornier 128-6 operated by TU Braunschweig was used for measuring the wakes of several large offshore wind farms within several campaigns. In these campaigns, the aircraft flew with a true air speed of.....

Page 3 – Line 15: . . . have a resolution of 0.66m -> do you mean .. provide values ever 0,66m of a flight leg?....

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Page 3 – Line 24: . . . have the same pattern. -> Suggestion: . . . were conducted using the same pattern.

Page 4 – Figure 1 (Caption): . . . are in use. -> in operation

Page 4 – Figure 1 (Caption): . . . in 2017 -> I suggest to rewrite: “. . . during the time period investigated in this study.

Page 5 – Line 2: . . . such as Synthetic Aperture Radar (SAR) . . . -> I suggest to add: ‘sattelite-borne Synthetic Aperture Radar’

Page 5 – Line 8: . . . repeat cycle of 6 days, -> Do you mean return frequency/period?

Page 5 – Line 9: . . . Sentinel 1 passes in the German bight. . . -> This sentence is hard to understand. I suggest something like: Sentinel 1 passes the German Bight at 5am or 5pm

Page 5 – Line 17: 16 km in the outermost domain followed by two domains with 5 km and 1.67 km,. . . -> Usally a factor of 3 or 5 is used in WRF simulations. Do you mean 5.3 km? (if yes, please correct throughout the paper)

Page 5 Line 20-22: The authors reference the study by Hahmann which shows that a spin-up time of more than 12 hours should be used and then don't do this in the third case. Why? If there is no good reason for this, the third case should be re-run with a spinup-time of 12 hours or larger.

Page 7 – Figure 4: It is quite difficult to distinguish wind farms and islands especially close to the coast (e.g. Borkum Riffgat). The reviewer suggests to change the wind farm shapes to some greyish color.

Page 8 – Line 1: A summary of our sensitivity tests appears in Table 2. -> The reviewer suggests to rewrite: A summary of the setups of our sensitivity tests appears in Table 2.

Page 9 – Table 3: Meerwind Südost consists of 80 turbines (see Major Point 1). Also,

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the reviewer suggests to double check all other information (hub heights/diameters) with the official information from the wind farm operators or turbine manufacturers!

Page 10 – Line 1: “stably stratified” → The reviewer suggests to rewrite: “slightly stably stratified” because the vertical change of the potential temperature with height over the rotor area is very small.

Page 10 – Line 5 (Fig 1 – black thick line). It took me really long to find the black thick line. Maybe add a marker to the line? Or Markers at start and end? (also for the other lines)

Page 10 –Line 6-7: approx. 2 K warmer than the air temperature → This is really a lot and thus should be explained. The sensors at FINO1 might be impacted by algae growths etc. You should check with other BSH buoys in the German Bight and a high-resolution sea surface dataset e.g. the OSTIA data if this measurement at FINO1 is really valid. This is also true for case III (see below).

Page 10 – Line 14-15:Further to the north. . . -> This is true but Sandbank is also very “thin” for westerly winds (only 2 rows of turbines). This point should be added to the discussion.

Page 10 – Line 21-22: However an SST of 288.5 K was measured at FINO1. . . (see second last comment above). If it holds to be correct, a discussion should be added that the stability could also be an advective feature coming from another location.

Page 10 Line 25: Wind speed over. . . → The reviewer suggests to correct to “above”. This occurs several times in the document.

Page 11 – Line 3-4: We suggest that this acceleration emerges due to an enhanced flow around the wind farm due to the stably stratified atmosphere → This is of course correct but needs further explanation. The reviewer suggests to add something like: This stability hinders the flow to extend vertically above the wind farm and forces it to flow around the wind farm leading to a speed up at the wind farm edges.

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Page 11 – Line 12: TKE over and next to the wind farm -> Suggestion: TKE above and next to the wind farm

Page 11 – Line 18: . . . where the shear was greatest (shown in Fig. 8a) → The reviewer suggests to change to “largest”

Page 12 – Line 6: . . . where the horizontal wind shear was greatest →The reviewer suggests to change to “largest”

Page 13 – Line 3: In every case -> The reviewer suggests to change to “In summary, in every case . . .”

Page 13 – Line 21: “interpolating the wind speed to a height of 10 m is difficult.” → The WRF model provides the output of U10/V10 which are of course not from a model level but should give comparable results to the 10m wind speed. The authors thus should use these data

Page 13 – Line 28: “of the atmosphere in the model did not change with time.” → better: over time

Page 14 – Line 2: “at 200 m AMSL over and next to the wind farms” → better: above and next to the wind farm

Page 14 – Line 4: “A weak increase in wind speed similar. . .” → What this the physical explanation for this increase?

Page 15 – Figure 8 (also 10,12) → The reviewer suggests to change the x- axis to kilometres from wind farm center and define a point in the center of the wind farm. This way it is very difficult to estimate what the extension of effects such as the speed up are.

Page 15 – Line 6: “showed an increased impact on the wind speed” → Do you mean “showed an increase of the wind speed above the wind farm”?

Page 16 – Line 5: “for the simulations conducted for 14 October 2017” → The reviewer

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suggests: “for the simulations of 14 October 2017”.

Page 17 – Line 6: It is very confusing that the CNTRns are not related to the cases here. The reviewer suggests to add this where needed. E.g. change to ‘CNTRb – case II’

Page 17 – Line 10: “active” → do you mean activated?

Page 19 – Line 6: “resolution with a disabled TKE source” → The reviewer suggests to rewrite to “without a TKE source” to make it more general.

Page 19 – Line 7: “without an explicit turbulence source (Volker et al., 2015)” → I think it would be really nice and strengthen the work if another case simulated with the model by Volker would be added but I also see that this would probably be a lot of additional work. So maybe add this to the outlook at the end of the paper?

Page 19 – Line 21: “. . . produced too small of TKE compared. . .” → Do you mean “. . . produced too small amounts of TKE compared. . .”?

Page 19 – Line 24: “in a greatly reduced TKE” → The reviewer suggests to write “largely reduced TKE”.

Page 19 – Line 26: “over the wind farm - 2 m s⁻¹” → Better: “over the wind farm of – 2 ms⁻¹”

Page 22 – Line 10: “Strong shear lines at the edge of a wind farm or wake. . .” → The reviewer suggests to add: that originate in the flow around the wind farm.

Page 22 –Line 16: “In model the wind farms GW” → In the model the wind farms GW

Page 23 – Line 24: “but feasible for regional climate simulations.” → The reviewer suggests to add “weather simulations”

Page 24 – Line 8: The reviewer suggests to change this last sentence of the Conclusions: Thus, future work on wake effects of large offshore wind farms should primarily

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focus on.

Page 28 – Line 23-24: Svensson et al → doi missing

References:

[1] <https://www.windmw.de/meerwind.html>

[2] <https://www.energy-charts.de/>

[3] <https://zenodo.org/record/2682604>

[4] https://github.com/NCAR/WRFV3/blob/master/phys/module_sf_mynn.F

[5] https://github.com/wrf-model/WRF/blob/master/phys/module_sf_mynn.F

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2019-100>, 2019.

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