

Wind Energ. Sci. Discuss., referee comment RC2
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Comment on wes-2021-7

Anonymous Referee #2

Referee comment on "Recovery processes in a large offshore wind farm" by Tanvi Gupta and Somnath Baidya Roy, Wind Energ. Sci. Discuss.,
<https://doi.org/10.5194/wes-2021-7-RC2>, 2021

This study explores different recovery processes in a large hypothetical offshore wind farm in the Arabian Sea for 3 different meteorological conditions using the mesoscale model WRF and the Fitch WFP. The manuscript is well written in general. Some figures could be improved to facilitate the understanding of the reader. I think it is an interesting study that should potentially be published in WES, after carefully addressing the comments below.

General comments

My main criticism of this study is that, while the three different cases certainly consider different wind speed ranges, they might also have different stability ranges. However, the stability for the cases is not discussed (see specific comments below), although various studies have shown an impact of stability on wakes (Lee and Lundquist 2017, Cañadillas et al. (2020) to mention just a few).

My second concern is the sensitivity of the model results with respect to the vertical resolution. The vertical resolution is rather coarse (>20 m) compared to the resolution, which is suggested to be necessary to capture wind farm effects (e.g. Siedersleben et al 2020, Tomaszewski and Lundquist (2020) or Pryor et al. (2020)). In addition Pryor et al. (2020) also pointed out that the TKE magnitude depends on the vertical resolution. One of your conclusions concerns the effect of TKE advection on the recovery process. This conclusions could be faulty, if the resolution is too coarse. Please elaborate.

Specific comments

p 1, line 27ff: Is the citation of so many studies necessary here?

P 2, line 8: "... also confirmed by LES...", in line 5 you write that the recovery process was previously investigated by LES simulations. Why is the study by Calaf et al. (2010) listed separately from the others?

P 2, line 27: Here you write that TKE advection is deactivated in your study, while later on Page 5, line 14-17 you write that both activated and deactivated scenarios are used. Please make these statements consistent.

P 3, line 3: Please provide the version number of the WRF version that you have used. The bug found by Archer et al. (2020) showed, how important it is to document clearly, which version number has been used.

P3, line 19: How transferable are your results to other regions? This farm is quite far offshore. Existing offshore farms are closer to the coast. How could coastal effects change your results?

P3, line 24: A vertical resolution of > 20 m is at the upper limit of the necessary resolution to capture wind farm effects correctly as studies by Siedersleben et al 2020, Tomaszewski and Lundquist (2020) or Pryor et al. (2020) have indicated. In addition Pryor et al. (2020) also pointed out that the TKE magnitude depends on the vertical resolution. How sensitive are your results to vertical resolution?

P4, line 4: Was the Sea Surface Temperature also taken from that source?

P4, line 4-11: A tabular overview could provide a better overview on the set-up

P4, line 14: This is a rather small turbine compared to turbines that are currently being installed offshore. Why have you used such a small turbine and how representative are your results for more modern turbines?

P5, line 7: Atmospheric stability is another relevant parameter for wind energy. Is the stability similar in all three cases? Otherwise this will affect the conclusions as well.

P 6, Eq 6: Define what \hat{u}^i and \hat{u}^j mean. Consider to add subindex "h" to indicate that hub-height winds are used here

P 8, line 18: How do these values compare with Volker et al. (2017)

Figure 3, 4, 6: Please add variables and units to the colorbar.

Figure 3: Are averages over the entire two or three day period shown in that figure? Or is it a snapshot? Please clarify. Consider to add the "plots depict the wind farm only, that is the black square in (c), if appropriate". In figure (c), please add: every xth vector is shown

P 9, line 13: here it says averaged over the rotor depth, while in the caption of figure 3 "hub-height wind" is mentioned. Please clarify.

P 9, line 17: The wake length depends also strongly on stability. Again, have you checked that the stability is the same in all simulations?

P 10, line 3: Wake lengths can be defined in many ways. You mention a wake length of 521 km, which is very long. Please add your definition of a wake length

P 10, line 13 – 15 and Figure 4: The figure could be easier understandable, if Case A would be turned into the mean wind direction and thus the colours would be the same in all cases. Please consider doing so.

Figure 7 (a): Most points follow the linear trend. However there are also quite some points with relatively high momentum loss rate but small vertical recovery (especially around $10 \cdot 10^3 \text{ ms}^{-2}$). During which situations does this behaviour occur? Also there is a sharp drop at about $3 \cdot 10^3 \text{ ms}^{-2}$, where does this come from?

Figure 8: Why is only the U-momentum flux considered here, even though the wind direction is south-west / north-east?

Section 3.7: I think this section deserves a bit more elaboration and possibly a figure. As you mention in your introduction on page 2, there has been a debate, whether TKE advection needs to be activated or not. With that in mind, it would be good to give more evidence for your conclusion on TKE advection.

Technical corrections

P5, line 21: Consider to add "the method by" in using Avissar and Chen (1993).

P6, line 22: "that" is used before and after the citation

P8, line 7: "As" should not be capitalized

P8, line 23-24: Fig. 3b is referenced twice within one sentence

Figure 3: sub-figures are usually referenced only with (b) not with Fig. 3 (b). Please check with the journal guidelines

P 13, line 22: consider to add "side" or the like after "at the upwind"

P 18, line 28: "till" is rather colloquial. Consider to use "until" instead of "till"

P 19, line 24 – 28: I agree with the authors that it is an interesting study, but how significant a contribution is cannot be known before publishing. I would suggest to advertise it a bit less.

P 19, line 31: Code availability: Please add the URL to the WRF github repository (or where ever you downloaded WRF)

P 19, line 32: Code availability: Please also add namelist.wps to the the repository. Why are only the example files for 0.5 km included in the repository, even if they are modified within the code it would be good to have them all. Please also add a description of model changes in the 5 files that you mention in the README.

P 20, line 1-2: Please add a link to the NCEP data.

P 21, line 10: Chou et al: How can this publication be accessed? Please provide a doi / url

P 21, line 30-31: Hong et al: How can this publication be accessed? Please provide a doi / url

P 22, line 22: line 1-2: IRENA: How can this publication be accessed? Please add a url.

P. 22, line 7: Li et al: How can this publication be accessed? Please provide a doi / url

P. 22, line 16: Noppel et al: How can this publication be accessed? Please provide a doi / url

P 22, line 26: Skamarock et al: How can this publication be accessed? Please provide a doi / url. In addition: your zenodo repository indicates that you have been using WRF 4.2.1, why do you cite version 3 here?

P. 23 line 5: Zeng et al: How can this publication be accessed? Please provide a doi / url

References

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