Response to Reviewer 2 – WES-2021-7
April 15, 2021

We are thankful to the reviewer for providing the insightful feedback. Please find our response to the comments in blue in the section below.

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 faciliate the undestanding 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).

This is a very important point. While designing the experiments to study recovery, we wanted to look at wind farm design, specifically, the variability of recovery process under different wind farm spacing and wind speeds. But we agree with the reviewer that the stability question is extremely important. As suggested by the reviewer, we will take an extensive look at the static and dynamic stability patterns of the 3 case studies using lapse rate, Richardson number and other appropriate metrics.

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.

Thank you for bringing these studies to our notice.

Tomaszewski and Lundquist (2020) suggested a low-level vertical resolution of ~10 m and horizontal of either 1 km or 3 km in order to achieve the appropriate mixing required to match the expected surface warming and drying. Siedersleben et al. (2020) clearly recommended a vertical level on the order of ~12m. However, they also mentioned that in case of limited computational resources, a horizontal resolution of 5 km and vertical resolution of 35 m below 100 m also captures the most important features over the wind farm. Also, Pryor et al. (2020) showed an impact of vertical resolution on TKE added in the wind farm and its vertical distribution.

Our simulations are computationally very expensive with each simulation costing ~16000 computational hours on a high-performance computer with 200 processors. We have conducted 24 simulations leading to a total of 384000 computing hours. Each simulation requires extensively large amount of computation resources because we have a very large 1500 km X 1500 km horizontal domain discretised with a high horizontal grid spacing (1 km for the finest grid). This configuration is essential for our simulations. We need the large horizontal domain to avoid wake boundary reflections and the fine horizontal resolution to capture the intra-wind farm wake effects for different inter-turbine spacings. We have used a vertical resolution of ~21.4 m in the lowest 150 m of model domain. This resolution is comparable to Pryor et al., 2020 (~16.7 m) and is appropriate for capturing the important features including TKE over wind farms in case of limited computational resources (Siedersleben et al., 2020).

Lastly, we have used total 61 vertical levels till top of the atmosphere. It is necessary to keep a fine resolution beyond the rotor depth (lowest 150 m) as well because it will help us capture the effect of windfarm on mesoscale and synoptic fluxes also.

Specific comments

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

We have tried to provide an exhaustive list of studies that have explicitly discussed how recovery happens in the wind farms through microscale and mesoscale processes. Some of these studies have only mentioned the issue in passing while others have explored it using rigorous quantitative approaches. We will reduce the number of citations to more quantitative studies.

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?

Sorry for missing the reference of Calaf et al. (2010) in P2L6 with other citations. We will add the mentioned reference with others in P2L6.

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.

We agree that this sentence should be rephrased for clarity. We conducted two sets of simulations: the first set with TKE advection on and the second with TKE advection off. All results shown in sections 3.1

to 3.6 include TKE advection. In order to study the effect of TKE advection like Archer et al. (2020), we conducted sensitivity simulations with the TKE advection turned off. The comparison of these two sets of simulations is discussed in section 3.7. We will make this clearer in the revised manuscript.

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.

We have used WRF Vr 4.2.1 for our simulations. We will add this in the updated manuscript.

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?

The results are valid for all deep offshore regions if the basic meteorology is same. However, in case of some extreme phenomena such as cyclones, we anticipate that results may vary. Coastal wind farms are often affected by sea breezes and can have different recovery patterns. We will mention this is the discussion section. We are currently conducting a study for coastal wind farms but we do not have final results yet.

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?

Please see the justification given above in the general comments section.

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

Yes, the SST was also taken from the National Centers for Environmental Prediction Final Operational Global Analyses dataset (NCEP, 2015). We will mention this in the revised manuscript.

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

We will provide the physics parameterization that are used in the simulations in a tabular form in revised manuscript.

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?

We understand the reviewer’s concern. The absolute magnitude of the power generation and recovery will depend on the turbine rating. However, we expect that the spatial patterns and relative magnitudes of the recovery will be valid for any of the installed turbine size provided that the inter-turbine distances and meteorological conditions are comparable. Please note that we have presented the recovery terms as a function of power generated in Figure 6 and Table 2.

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.

We understand the reviewer's concern. We will analyse the stability conditions for all the three cases and add a sub-section in the revised manuscript.

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

The symbols î and j are the unit vectors in the zonal and meridional directions, respectively. We will explain this in the manuscript. We will add a sub-index "h" to indicate the hub-height wind speeds in order to avoid any confusions with Eq. (4) & (5) for the readers.

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

We cannot directly compare Volker et al. (2017) with our study because the wind farm size and number of wind turbines are different than ours. However, the central tendencies of wind speeds are somewhat comparable. Here we present some very approximate comparisons by visual interpolation:

1) In our case, we are getting 31% efficiency for 4.5D and 11.3 m/s mean CTRL case wind speed. In case of Volker et al. 47.7% (interpolated) efficiency for 5.25 D and at a wind speed of 9.1 m/s.
2) In our case, we are getting 81% efficiency for 4.5D and 15.6 m/s mean CTRL case wind speed. In case of Volker et al. 60.3% (interpolated) efficiency for 5.25 D and at a wind speed of 13.1 m/s.
3) In our case, we are getting 78% efficiency for 8.9D and 11.3 m/s mean CTRL case wind speed. In case of Volker et al. 77.5% (interpolated) efficiency for 10.5 D and at a wind speed of 9.1 m/s.
4) In our case, we are getting 100% efficiency for 8.9D and 15.6 m/s mean CTRL case wind speed. In case of Volker et al. 85% (interpolated) efficiency for 10.5 D and at a wind speed of 13.1 m/s.

These comparisons are too approximate to be presented in the paper.

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

We will add variables and units to the colorbars in the mentioned figures.

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

In Fig. 3 (b) and (c), averages over the entire two day period is shown. We will add the clarification in manuscript and modify the captions as per the reviewer’s suggestion.

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.

We apologize for the mistake. The plot shows differences in wind speeds averaged over the rotor depth, not the hub-height wind. We will correct the error in the Fig. 3 (c) caption.

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?

We will analyse the stability for the different cases and address the concern in revised manuscript.

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.

We take the difference the hub-height wind speeds between the control and wind farm cases and estimate the statistical significance using the Wilcoxon sign rank test. The wake lengths are calculated as distance from the wind farm to the grid cell where the difference falls below the 99% level of significance . We will add this in the manuscript.

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.

We understand that this could be confusing for the readers. But we prefer to keep the figures unchanged because we want to give the readers a flavor of the fact that these simulations are constrained by observed boundary conditions where the wind directions vary. We have taken the components of meteorological variables along the dominant direction. To help readers understand these figures better, we have added arrows showing the prominent wind directions. We will make these arrows bolder so that the difference in wind direction between the different cases clearly discernible.

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*10â□»³ msâ□»²). During which situations does this behaviour occur? Also there is a sharp drop at about 3*10â□»³ msâ□»², where does this come from?

There are also some points where though the momentum loss rate is high, the vertical recovery is low. These are the points on the upwind edges of the wind farm where the horizontal recovery dominates leading to lower values of vertical recovery. The sharp change at momentum loss rate ~ $3.5X10^{-3}$ in Fig. 7 (a), is because of change in cases. We will explain this in the revised manuscript.

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

Figure 8 presented here is for Case C-I. This is the only case where the mesoscale fluxes are of the same order of magnitude as turbulent fluxes. The wind direction for case C is west-south-westerly with stronger u components than v. This is the reason the u momentum flux is much higher than the v momentum flux (P13L12) and hence, only the u-momentum flux is considered in the paper. We will add this explanation in the section 3.7.

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.

We will add a table in the section 3.7 with values of difference in horizontal and recovery for the different cases analysed. We will also add a figure showing spatial patterns of vertical and horizontal recovery with TKE advection off.

Technical corrections

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

We will add “the method by” in P5 L21.

P6, line 22: “that” is used before and after the citation.

Sorry for the mistake, we will correct it.

P8, line 7: “As” should not be capitalized.

Sorry for typo, we will correct it.

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

We will correct the reference of Fig. 3b in P8L23-34.

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

We thank the reviewer for pointing this out. Though we cannot find specific instruction in the journal guidelines, we checked the papers published in the journal and they follow the same style of figure referencing in the caption as mentioned by the reviewer. Therefore, we will revise the figure captions as per the reviewer's suggestion.

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

We will add the word "side" after upwind.

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

We will replace till by until in P18L28.

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.

We will rewrite this paragraph as per the reviewer's suggestion.

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

We had downloaded WRF version 4.2.1 from github. We will add a URL (https://github.com/wrf-model/WRF/releases/tag/v4.2.1) for the same.

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.

We will add all the files suggested by the reviewer to the repository. We will list all changes made in the model code in the README file.

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

We will add a link to NCEP data in the revised manuscript.

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

We will add a url (https://ntrs.nasa.gov/citations/20010072848) for Chou et al. in the revised manuscript.

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

We will add the url (https://www.kci.go.kr/kciportal/ci/sereArticleSearch/ciSereArtiView.kci?sereArticleSearchBean.artiId=ART001017491) for Hong et al. in the revised manuscript.

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

We will add a url (https://www.irena.org/publications/2019/Oct/Future-of-wind) for the IRENA technical report.

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

We will add the url (https://doi.org/10.1007/s00376-010-0041-0) for this publication in the paper.

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

We will add the url (https://doi.org/10.1023/A:1015556228119) for this publication in the paper.

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?

We apologize for this mistake. The correct citation should be of: Skamarock, W. C., Klemp, J. B., Dudhia, J., Gill, D. O., Liu, Z., Berner, J., … Huang, X. -yu. (2019). A Description of the Advanced Research WRF Model Version 4 (No. NCAR/TN-556+STR). doi:10.5065/1dfh-6p97. We will correct this in the manuscript.

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

We will add the url (https://doi.org/10.1175/1520-442(1995)008<1156:LIAFAI>2.0.CO;2) for this publication in the paper.

References

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Volker PJH, Hahmann AN, Badger J, Jørgensen HE (2017) Prospects for generating electricity by large onshore and offshore wind farms. Environmental Research Letters 12(3), DOI 10.1088/1748-9326/aa5d86