Response to the comments of Andrea Hahmann for WES-2023-033

June 18, 2023

General feedback from the referee

The manuscript presents an excellent contribution to assessing wind resources in the North Sea, which could be limited by extracting kinetic energy from the atmosphere by large wind farms offshore. The work is well embedded in the existing literature and brings enough novelty. The design of the study is robust, and results soundly support the conclusions, and I recommend the publication with two 'medium' and a few minor concerns, as follows:

General reply from the authors

We thank the referee for taking the time to assess our work and for proposing several adaptations to make the work more robust and clear. In the following we have formulated replies to each of the suggestions after consultation amongst the co-authors.

Medium comments

Comment 1: It will be nice to get an indication of the accuracy of the simulated model stability classes. Since you are using these frequencies to split the wake losses among stability classes, it would be good to know if they relate to reality. We know the SSTs are input to the model simulation, so could we verify the temperature above the sea using buoy data? For the NEWA project, we estimated that the stability classes could be different by as much as 10% when a different PBL was used in the simulations (see Figures 11-13 in NEWA D4.3 report).

Reply: This is a valid concern. As our stability classification relies on vertical temperature profiles, it would be interesting to get a better feel for how accurately COSMO-CLM models these temperature profiles. However, since the stability classification considers data between 50 m and 150 m MSL, a validation over this height range might be more suitable than with buoy data closer to the surface. We propose to add a comparison in terms of the temperature gradient with air temperature measurements at meteomast Ijmuiden for the period 2012-2015 for which we have data at two height levels (21 m and 90 m), hence allowing us to compute a gradient. This can give a first indication of how accurately the temperature profiles are simulated by COSMO-CLM.

Comment 2: I am missing a discussion on validating the wake farm parameterisation used. It is challenging to validate these parameterisations in terms of far wakes due to the lack of wind farm data and the fact that current wind farms are not yet as large as the ones you simulate. Please indicate an uncertainty based on the literature. Volker et al. (2015), Fischereit et al. (2022) and other publications show considerable differences between Fitch and EWP schemes with limited validation data, which are relatively close to the wind farm. How much would this uncertainty affect your CF reductions for the North Sea?

Reply: We agree that the sensitivity to the wind farm parametrization used deserves more attention. As the mentioned publications point out (in addition to several presentations at WESC2023 around this topic) both the wake development and estimates of wind farm power production are highly sensitive to the parametrization used so we will discuss this uncertainty in our work and include the mentioned publications for this.

Minor comments

Comment 1: Please follow the WES guidelines for units (e.g., m/s is not acceptable)

Reply: Unit formatting will be adapted according the WES guidelines in a revised version.

Comment 2-3: textual improvements

Reply: We will include these suggestions in the revised version.

Comment 4: L94. I would add, "However, wind farms have increasingly affected some of the masts used in the validation. " This is the case for FINO1 and FINO3.

Reply: This is indeed only mentioned implicitly in this part in the text. However, on line 164 we go into more detail about filtering of observations for wind farm disturbance and refer to the corresponding supplementary table which summarizes filtered angles or periods per station.

Comment 5: Some of your symbols are sometimes italics and sometimes not. e.g. R in 104. Also, after the equations. All symbols should be in italics.

Reply: This will definitely be streamlined towards the new version.

Comment 6: Is there a direct relationship between the PSS and the EMD? I have a feeling it does. It could be good to mention and thus be able to compare your statistics with those of the NEWA simulations.

Reply: We will have a look what the relationship is, but these seem to be applicable to test the same type of agreement indeed. It will be mentioned in the new text.

Comment 7: L211: This is not an extrapolation, right? The values above and below the sensor height are known. BTW, this is analogous to a log interpolation of the wind speed between the levels. Too much text confuses people. Also, we want to move away from using power law relationships when data for interpolation is available. The way you write this method could give the wrong impression.

Reply: Extrapolation might not be the correct word here, indeed, since information from below and above the sensor height is used. We tested the difference between our method and the log interpolation with a representative dummy example with strong shear: 9.5 m/s at 90 m, 11 m/s at 120 m. The inferred wind speed at 105 m differs with 0.05 m/s between both methods (higher value with our method). Our method also gives 11 m/s at 120 m when inferred from 90 m, so it is a slightly different profile shape going through the two points. In this example, the target level is right in between the two model levels, but in general it will be closer to one of them and we interpolate from the nearest level, further reducing any error. This dummy example is also very strong shear (e.g. compare with average profiles Fig. 6), so in general the differences between the methods will be smaller. To conclude, the differences between both methods introduces some additional uncertainty but since differences are very small, it is expected that it does not impact the findings of our study. It is probably also difficult to determine which method is better for our use case given the other uncertainties on measurement data.

Comment 8-9: Textual improvements.

Reply: We will include these suggestions in the revised version.

Comment 10: How are the turbines located in each grid cell? That should be mentioned.

Reply: This will be added to the text. In the used implementation of the Fitch WFP, no subgrid-scale layout effects are included. All turbine rotors are assumed perpendicular to the wind and there are no subgrid-scale turbine wake interactions.

Comment 11: Extrapolation of each measurement has been shown to work poorly. See Badger et al. (2016) in JAMC, DOI:10.1175/JAMC-D-15-0197.1

Reply: We assume that this comment refers to the vertical extrapolation of in situ data to the 10 meters MSL of the ASCAT data. It is true that this constant coefficient extrapolation is too simplistic as pointed out by the referenced publication, so that stratification effects are not taken into account. A mention of this limitation will be added to the text.

Comment 12: L258-259: Please be more explicit on what data is used to compute the static stability

Reply: We will add additional information on the number of height levels in the considered range (see line 245), about the temporal resolution and more detailed information on the grid cell used to select timesteps in the observational dataset in the revised version.

Comment 13: Figure 2. Please explicitly name the period used.

Reply: We will make more explicit that it refers to the measurement period of each individual measurement station. Adding the individual time periods to the figure might become messy? Instead we have added this information to Appendix A1 and A2 and in visual form to Fig. S4.

Comment 14: Figure 7. The height at which the maps are computed and the time period details are also missing.

Reply: This will be made more explicit towards the next version.

Comment 15: L360-361. It is not clear what you mean by "data points". Are these in space or in time? I guess in time, but maybe a time match will be best or a frequency.

Reply: This is indeed not very clear. It refers to time points, but adopting a frequency instead will be better. We will adapt this.

Comment 16: I like the transects in Fig. 8. Especially because they help assess the necessary spacing between wind farm clusters regarding recovery distances. But again, you are missing the height in Fig 8 caption. Please make sure that the captions are complete.

Reply: It is the hub-height of the considered scenario, but should indeed be made more explicit for the reader to avoid confusion.