Wind Energ. Sci. Discuss., referee comment RC1
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Comment on wes-2021-153

Anonymous Referee #1

Referee comment on "Comparison of large eddy simulations against measurements from the Lillgrund offshore wind farm" by Ishaan Sood et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-153-RC1, 2022

This paper is an excellent piece of research, full of innovative ideas and techniques and very well written. It should definitely be published in WES. My suggestions below are not even minor revisions, but addressing them will clarify a few issues and hopefully improve readability.

I have one somewhat sad comment. To be honest, I was expecting a better performance of the entire modeling system and perhaps more wake analyses (only two turbine wakes were analyzed, qualitatively). I guess I should not be surprised because the data are never what we hope them to be. There were so many mismatches between the real wind farm and the simulated one, from pitch angles to yaw misalignment etc. But unfortunately, because of such data issues, I am not convinced of the goodness of either the SP-Wind solver with the AASM or the initialization technique using TotalControl Flow data. This is really sad because both are innovative and appear to be robust. Maybe a more in-depth data cleaning procedure could be applied to the dataset?

- Either “dataset” or “data set” should be used, but not “data-set”. Please replace it throughout.
- Line 20: you should expand your literature review because citing only two studies about LES applications to wind farms is just not acceptable. There are so many more such studies, including several about Lillgrund. Here is an incomplete list:

Archer et al. (2013)

Bhaganagar and Debnath (2015)

Calaf et al. (2010, 2011)

Chaudhari et al. (2017)

Churchfield et al. (2012)

Fleming et al. (2014)

Ghaisas and Archer (2016)

Ghaisas et al. (2017)

Han et al. (2016)

Lu and Porte-Agel (2011)

Martínez-Tossas et al. (2015)

Meyers and Meneveau (2011)

Xie and Archer (2017)

- Lines 22-23: you should not list a series of issues that LES has apparently helped address without citing the literature studies that you think did it. You need to add at least one citation for each of these terms: gusts, atmospheric stratification, and turbine-wake interactions. I assume that Mehta at al. (2014) was about local wind climate only.
- Around line 190: What exactly is a "Pressure-driven" boundary layer? Is it neutral? It seems that there is no thermal inversion in those. What is a "conventionally neutral" BL? Please add some more description about the two datasets used (PDBL and CNBL).
- Related to question 2 above, it would really help is the same information could be provided for all 5 cases (PDBL and CNBL). In Figure 6, it would be great if you could add the same profiles but for CNBL. In Figure 7, it would be even better if you could add the profiles for the PDBL, especially the potential temperature profiles.

- Figure 6a: is the value of z0 reported for PDkhi correct? It is inconsistent with the value in Table 1.
- Lines 200-205: It is brilliant to re-scale the precursor runs from Total Control Flow via appropriate z0 and u*. In the text, it sounds like u* is "imposed" in the Total Control Flow fields (together with z0). Is that true? Can u* be imposed during initialization? I am just curious. If so, please add the values of imposed u* in Table 1 in another column.
- (5): what is vector e1?
- Table 1: Is z0 for case PDkhi really 2 x 10^-5 m? In Figure 6a, it appears to be 2 x 10^-3 m. There is an inconsistency.
- Page 13: This idea is really cool, but the notation is tough. For example, u and v are usually the two horizontal components of the wind vector. Here, they are used to indicate wind speed, but u for LES and v for Lidar. A bit confusing. X and Y are both wind speeds, but usually x and y are Cartesian coordinates. The overbar indicates what exactly? A 75-minute time average at each point? Or is there some sort of horizontal average first to obtain "time average profiles at range gate locations", which are then weighted with w? Why is it so important to minimize the covariance distance? Given the relatively large errors in wind direction (Figure 8b, especially PDk3), I was thinking that maybe a wind direction distance could be minimized instead? And finally why is the simple sum of the two distances chosen? Isn't wind speed more important? These are all requests to add some clarifications in the text.
- Table 3 is not cited anywhere in the manuscript. Perhaps at line 253?
- Line 2054: This domain is huge! Why was such a large domain chosen? I thought that perhaps that was the domain size in the PDBL and CNBL datasets (if so, please mention it). If not, then how do you "extract" a portion of the data from those datasets to match your domain?
- Line 277: this phrase does not make sense … What does "made to advance" mean? Who advances what? What do you mean by "the inflows"? I thought you took initial and boundary conditions from TotalControl Flow, after correcting them with the scaling parameters. Please rewrite to clarify.
- Line 301: It sounds like you should have cleaned up the SCADA data and removed high-pitch cases. Why did you not do it?
- Figures 16 and 17: I assume that the orange lines correspond to the location of the vertical profiles. Did you also average along those lines or did you pick the wind speed at the exact points downstream at 0.5D, 1D etc?
- I did not review the Appendices due to lack of time.