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Interactive comment on "Improving mid-altitude mesoscale wind speed forecasts using LiDAR-based observation nudging for AirborneWind Energy Systems" *by* Markus Sommerfeld et al.

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1 General comments

The paper is a useful contribution to a better understanding of the winds at larger heights, which is not only relevant for the AWES applications for which the paper is written, but also in general for large wind turbines. Nudging with wind observations within the boundary layer has not been done a lot, so it is interesting to see how the

WRF model behaves. I have two major issues with the paper:

1) In the abstract it is stated that: "Observation nudging improves the overall accuracy of WRF". This cannot be concluded based on this study, because the observations are assimilated and then also used for evaluation. This will obviously result in the model being closer to the measurements, but this has nothing to do with WRF being more accurate 'overall'. If you want to draw this conclusion you would have to compare with measurements that are not assimilated in the model, preferably at some distance away from the point where the observations are nudged. Otherwise it should be more clearly written that the nudging is only valid at the lidar point: as it can be seen from Fig. 6 the modelled wind speed is just bias-corrected with approx. 1 m/s over a 180 km area, but it might well be that this detoriates wind speed comparison at other locations. For example, it could be that the bias at this point is caused by a wrong surface roughness or other local flow properties, which means the bias does not exists in other places. Also the nudging is likely only valid over land, because over sea the physical processes that determine the wind profile at a given time are different. All this should be written more clearly throughout the abstract/results/discussion/conclusion. Figures 2-6 all show the same message: nudging brings the model closer to the observations, so they can be combined into one or perhaps two figures. Figure 11 and 12 also show the same thing and can be combined.

2) The definition of the Obukhov length in Eq. 4 is not clear or wrong: to classify stability one should take into account the effect of the *virtual* kinematic sensible heat flux and not the dynamic sensible heat flux directly from WRF (W/m^2), which seems to be implied in Eq. 4 (although H_sfc is not defined anywhere). In the WRF model surface layer fluxes are split up in a sensible and latent heat flux. Sensible and latent heat flux are equally important in a fairly moist areas as Germany (see for example Stull (2017) or Floors et al. (2013)), so they should both be used when computing the Obukhov length.

2 Specific comments

p3l8: It would be useful to give the opening angle of the lidar.

p4l2: What CNR threshold is used for filtering the data? What is the defintion of an 'available' measurement?

p4l6-9: I would remove this, because it has nothing to with the measurements, which is what the section is about. It is also discussing some of the results which have not yet been presented.

p4l13-17: All brackets make this section difficult to read. Please rewrite.

p4: Please mention the land-surface, radiation and surface-layer scheme that were used in the WRF model. p6l2: 180 km is a very large distance. See major comment 1.

p7l2: I assume the wind direction is not calculated like this because it would lead to discontinuities when crossing 360 degrees. Please add more details.

Section 4.1-4.3: see major comment 1;

p14l10-12: I think this is an important conclusion from this work and I agree that this is a potential application of using nudged WRF simulations. Perhaps it is useful to relate this to the discussion in Gryning et al. (2019) regarding the wind speed bias from lidars as a function of CNR threshold and data availability, to show that this issue is not specific for the site studied in your paper.

p16l9-11: The wind speed in summer is mostly lower due to the lower synoptic pressure gradients in that time of the year, not so much due to the stratification (particularly at greater heights).

p19 table 2: Maybe better to also express this as percentage instead of number of obs.

p19l7: It is not clear to me how the lidar measurements are normalized: with the friction velocity from the OBS run?

p26: Remove Appendix A, it is not discussed anywhere.

3 Technical corrections

p5l20: "(see equation: 2)" –> "(see Eq. 2)"

p9 Fig 4 label: Abbreviation HWS is not defined

p14l2: 100m –> 100 m (and m not in italics).

p17l2: to (Sommerfeld et al.) –> to Sommerfeld et al. Also I don't know the journal policy but usually you can only include references that are 'accepted' and not those that are 'in review'.

p17l4: ? –> ref

p20l3-4: These two lines repeat the same thing.

p20l5: ?? –> ref

p21l7: Please split equation and units.

p21l10: drag coefficient and drag coefficient? Also equal sign is not enclosed in '$'.

p22 Fig. 13 caption: there is mention of a),b),c) here but they are not in the figure.

p23l14: decreases –> decreases.

4 References

Floors, R., Vincent, C. L., Gryning, S.-E., Peña, A., Batchvarova, E. (2013). The Wind Profile in the Coastal Boundary Layer: Wind Lidar Measurements and Numerical Modelling. Boundary-Layer Meteorol., 147(3), 469–491. http://doi.org/10.1007/s10546-012-9791-9

Gryning, S.-E., Floors, R. (2019). Carrier-to-Noise-Threshold Filtering on Off-Shore Wind Lidar Measurements. Sensors (Basel, Switzerland), 19(3). http://doi.org/10.3390/s19030592

Stull, R. (2017). Practical Meteorology: An Algebra-based Survey of Atmospheric Science. (Nina Horne, Ed.) (version 1.). Brooks/Cole

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