RC2: 'Comment on amt-2024-186', Anonymous Referee #2, 24 Jan 2025

Review for AMT-2024-186

Title: “Atmospheric stability from microwave radiometer observations for on/offshore wind energy applications”

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General comments:

The study presented in this manuscript first reviews stability metrics derived by NWP models and reanalysis, useful for wind energy. Second, it quantifies the performances of different microwave radiometers at estimating stability metrics on land and offshore. The manuscript is relevant, well written and easy to read. I think it fits well in the scope of AMT. I suggest only some minor modifications and to include some additional information. For instance, some more info on retrievals’ and calibration’s procedures, a-priori or radiosonde dataset used in the retrievals, radiosonde type, should be provided.

We thank the reviewer for the positive feedback and useful suggestions. We received all the suggestions as detailed below (in red font).

The title of the manuscript does not mention the first part of the study. The authors might want to consider if to mention this as well.

Agreed, thanks for the suggestion. The title now reads: “Atmospheric stability from numerical weather prediction models and microwave radiometer observations for on/offshore wind energy applications”

Specific comments:

Page 1, Abstract, line 32: Include units for RMS.

Agreed. Thanks!

Page 5, line 138: “Measurement data came from the FINO1, FINO2 and FINO3 met masts”. How are these data used in the before mentioned NWP models? Are they assimilated?

We confirm that tower measurements are not assimilated into NWP models and thus they are independent from each other. This is now clarified in the revised manuscript (Section 2.2).

Page 6, Figure 1 caption: Describe the pink line in the caption of the figure. Also, in figures with multiple panels, a), b) c)… would be useful.

Agreed. Thanks!

Page 7, line 194: How id the boundary-layer height determined during stable vs unstable conditions?

The boundary layer height is parameterized using friction velocity and latitude, as proposed by Gryning et al. (2007), independently of atmospheric stability. This is now explicitly stated in Section 2.1.

Page 8, Figure 2 caption: Panels are 'top' and 'bottom', not 'left' and 'right'. Also, in the caption you say '(8 and 12 m/s, 207 respectively)', but in the title of the panels you say '4 +/- 0.5' and 8 +/- 0.5'.

Agreed. Figure caption has been revised accordingly. Thanks!

Page 11, Figure 4 caption: This is another figure with multiple panels, where a), b) c)… would be useful.

Agreed. Figure 4 panels have been labelled.

Page 11, Figure 5: Do the models have hard limits on e lower values of dT/dz?

We confirm we have not applied hard limits to lower dT/dz values. However, the models (NEWA or DOWA) do seem unable to capture temperature differences larger than approximately -0.5°C between the model output at 100 and 50 m levels (corresponding to -20K/km), e.g., the minimum value is -0.6°C for the entire NEWA time series. This may be due to the treatment of surface stability in these models, but we have not been able to identify the root cause for these. Such instances occur for very unstable conditions with very small wind speeds (less than 4 m/s), i.e. not significant for the most engineering analyses discussed in the paper (wind turbines have cut-in wind speeds around 4 m/s). We added this consideration to Section 2.2.

Page 13, Figure 6: Please include a), b) c)… to the panels. Also, include x- and y-labels to the upper left panel, and y-labels to the lower two left panels.

Agreed. Missing panel and axis labels have been added. Thanks!

Page 14, Figure 7: Please include a), b) c)… to the panels. This figure really needs them (same for all the figures in the Supplemental material). For the figures in the supplemental material, it is interesting to see the jumpy behavior of the zi time series, particularly for the FINO1 site. This is why it might be interesting to know how are these zi values obtained.

Agreed. Panels have been labelled. Here, the ABL height comes from model (NEWA). The jumpy behavior in the time series may be due to the way the underlying NWP model (WRF MYNN) computes the ABL height, i.e by selecting a discrete model level (as seen in the subroutine GETPBLH within the following fortran module: https://github.com/wrf-model/WRF/blob/master/phys/module_bl_mynn.F).

Page 16, Table 1: I think it'd be useful to list somewhere the retrieval techniques utilized for each of the MWRs? Also, what a-priori were used?

Agreed. Information on the retrieval method and the a-priori used within each dataset have been added to the revised manuscript (e.g., Table 2 and Section 3.2).

Page 17, after line 368: Could you describe what type/schedule of maintenance, calibration were performed on these MWRs?

Agreed. Information on MWR calibration/maintenance have been added to the revised manuscript (Section 3.2).

Page 19, line 435: How many daily radiosondes per day and at what time?

ARM launches two daily radiosondes from ENA at 11:30 and 23:30 UTC. This is now stated in Section 3.2.

Page 20, line 473: What type of MWRs?

The MWR is a 14-channel RPG HATPRO. This is now stated while introducing THAAO. Thanks for noticing we missed that!

Page 23, Section 4: Would it make more sense to add the info on Radiosonde/MWR matchups, radiosonde launch times, radiosonde type, and so on in a table, rater than in the text for each dataset? In this way, you could also remove some of these details from the individual dataset descriptions in Section 3.2.

Agreed. Information on radiosonde/MWR matchups and radiosonde launch times has been added to Table 2.

Page 30, line 655: 'mostly driven by only one point'. I agree.

As noted by the reviewer, we admit that the improved correlation is mostly driven by only one point (at 27 K/km). If that point is removed, then the correlation would not be improved substantially. However, we decided to leave it in, as it is the only remaining case in very stable conditions where both zenith and scanning retrievals are available, supporting theoretical expectations that scanning shall provide more accurate vertical gradients.

Page 32, lines 710-711: No mention to the different method used for the retrievals (and the a-priori used for these) is given up to this point, nor about calibration performed, but I think it would be an useful information to add to the Section with the different dataset descriptions. Good calibrations and adequate a-priori datasets are crucial for MWRs.

We concur that adequate a-priori information and calibration procedures are crucial for accurate MWR retrievals. In this analysis we rely on the calibration procedures and quality control applied by the data provider. These are trustable national weather services (e.g., DWD, MeteoFrance), scientific programmes (ARM), or research campaigns (SOFOG3D, SVAAP, EUREC^{4}A). This information has been added at the end of Section 3.2. In addition, information on retrieval methods, a-priori, and calibration/maintenance have been added to the revised manuscript (e.g., Table 2 and Section 3.2).