AMT-2022-12: Response to reviewers

03 May 2022

The co-authors would like to thank all reviewers for their feedback and thoughtful suggestions. We have responded to each comment below. All line numbers refer to lines in the revised document unless otherwise stated.

In addition to edits made in response to reviewer comments, we have made the following additional minor corrections which have no further implications on the results:

1) We have corrected a typo on line 201, the number of verified clear sky hours between June and September 2019 is 179 not 236.
2) We have corrected an error in Fig. 4 panel (d): The original plot showed the temperature standard deviation ratio on the x-axis rather than the water vapor standard deviation ratio.

Original reviewer comments are included in *italics* below. Co-author responses are in simple text, and quotations from the revised manuscript are enclosed in square brackets ["..."].

Response to RC1:

General comments

- Here we only look at cases were we know fog develops. How many false alarms for fog onset to expect? Can you please comment on this?

Quantifying a false alarm rate depends on the definition of fog, which might be different for different purposes. For example, a horizontal visibility threshold of <1,000 m is a typical definition of fog for transport safety and nowcasting, but from a radiative perspective, much thinner fogs might still have a large impact on the surface energy budget.

We have added the following lines to emphasize this point:

Lines: 184 to 192: ["For forecasting and nowcasting purposes, fog is usually defined by a threshold in horizontal visibility (typically < 1,000 m) which has important implications from a safety perspective (Gultepe et al., 2007). However, limiting the definition of fogs to those that reduce visibility to < 1,000 m encourages thinner fogs (or mists) to be ignored or incorrectly classified as clear sky events. Being able to accurately measure thinner fogs is extremely important because (a) they form the precursor to thick fog, (b) they modify the surface moisture, aerosol, temperature and radiative structure which might impact fog development further down the line (Haeffelin et al., 2013) and (c) they can have important radiative and climatological impacts even without developing into a thick fog (Cox et al., 2019, Hachfeld et al., 2000). Because both the MWR and AERI are directly sensitive to the radiative impact of fog (as opposed to visibility), for the purpose of this study, we define fog as the presence of near surface liquid water that has a detectable radiative impact."]

We have added an additional method of fog detection using the ceilometer for independent verification (see lines 404 to 409 for the description of this). Using the ceilometer as truth, there would be one case out of 12 (case ID 7) where the AERIoe fog detection was a false alarm (i.e. the AERI detected fog when the ceilometer did not). However, even for this case, the onsite observer recorded that a fog bow was present between 07:15 and 08:30, indicating that there were in fact liquid droplets present. This was a marginal case (max LWP 2 g m^{-2}) that demonstrates the ability of the AERI to detect miniscule amounts of liquid water when even the ceilometer cannot. Since we do not have access to an instrument that is more sensitive to liquid water than the AERI, it is not possible to determine a false alarm rate for the detection of liquid water presence versus absence.

Of course, for fog nowcasting, the real parameter of interest is the reduction in horizontal visibility. Neither the MWR nor the AERI are suitable for direct measurements of horizontal visibility (which depends on fog depth and droplet size distribution as well as liquid water path). To determine a false alarm rate for fog detection based on a visibility threshold, it would be necessary to develop an algorithm to estimate horizontal visibility from the AERI and MWR measurements, and then use independent measurements of horizontal visibility to determine a false alarm rate for fog detection based on a visibility threshold, this is outside the scope of the present study.

- The commercial MWR have a surface temperature sensor incorporated. With this cheap addtion, the retrieval can be constrained at the lowermost levels. Excluding these is scientifically possible, however, it does distort reality if we consider the instrument as a whole (MWR plus meteo station). Please account for this in your manuscript, not just in the appendix. You have done all of this in appendix B, and I found it relevant enough to include Fig B1 and B2 to the main manuscript in place of Fig 5 and 7.

Based on this suggestion, we have integrated appendix B into the main text and replaced figures 5 and 7 with figures B1 and B2. Note that small changes in the RMSE values are due to the removal of one of the case studies, the reasons for which are discussed in the response to Reviewer 3.

Specific comments

- l. 35: on clear evenings with light winds -> on clear evenings with no or light winds (otherwise one might think that wind is needed)

Our preference is to not make this change, it's not possible to measure 'no wind' absolutely which makes the distinction somewhat meaningless. Also, some very light wind (> 0.5 m/s) can aid the formation and development of radiation fog by increasing the depth of the layer that experiences surface-driven cooling (Oke, 2002; Haeffelin et al., 2013).

- l. 54: The DIAL by Vaisala claims to have a lowest usable range gate at 50 m (see e.g. Mariani et al. 2021). Anyway, I agree that in the end this does not change a big deal for the usability for fog.

Thanks for pointing this out, we have changed this to 50 m to reflect the claim by Vaisala. Although I note that none of these papers assess the performance of the Vaisala DIAL below 100 m, this would be nice to see!

- l. 66: I don't like the +/- here as the direction of the sign of the change has opposite influence on the melting potential.

Bennartz et al., (2013) show that, for this case, either an increase or a decrease in the LWP would have reduced the surface melt potential. This is because a thicker cloud (higher LWP) would have reduced the amount of shortwave solar radiation reaching the surface, but a thinner cloud (lower LWP) would have reduced the amount of downwelling longwave, with both effects resulting in a decrease in surface melt potential overall.

We have changed this line to the following to avoid confusion here:

Lines 67-71:
["Cloud LWP was a critical control on the exceptional Greenland Ice Sheet melt event of 2012 (Bennartz et al., 2013). At the highest point on the ice sheet, had the cloud LWP been 20 g m-2 higher than observed, the reduction in downwelling shortwave radiation would have prevented surface melt. Equally, had the LWP been 20 g m-2 lower, the reduction in downwelling longwave radiation would have prevented surface melt (Bennartz et al., 2013). "]

- l. 68: It is an over-simplification to reduce MWR to K- and V-band. What about "... that measure downwelling radiation. Commercial sensors for temperature and water vapour profiling typically operate 14-35 spectral channels at 22-31 GHz and 51-58 GHz.

We have changed this sentence to the following per your suggestion:

Line 72-73;
["Ground-based microwave radiometers (MWRs) are passive sensors that measure downwelling radiation. Commercial MWRs for temperature and water vapor profiling typically operate 14-35 spectral channels between 22-31 GHz and 51-58 GHz and are sensitive to the lowest 6 km of the atmosphere (Lohnert and Maier 2012; Blumberg et al., 2015)."]

- l. 77: "Despite the promise of MWRs to improve fog forecasts". Please re-word the beginning of this sentence. I judge that the above-cited references (Martinet, ...) demonstrated that the improvement is more than just a "promise" that might not be fulfilled.

We have reworded this sentence to the following:

Lines 81-83:
["However, the maximum vertical resolution of boundary layer temperature profile retrievals from the MWR is 50 m at the surface, decreasing to 1.7 km at 1 km a.g.l (Rose et al., 2005, Cadeddu et al., 2013), which is insufficient to resolve the shallow surface-based temperature inversions that often portend the onset of radiation fog (Price et al., 2011, Izett et al., 2019)."]

- l. 104: your study only assess the situation of supercooled radiation fog. Would you expect your results to also be representative for fog>0°C? Why (not)? Please elaborate in the

manuscript. Ok, see you mention it is not guaranteed to be representative in Sect. 4, but maybe you want to give an indication here already.

We have moved the reference to the discussion of this issue in section 4 up to the end of the introduction section.

Lines 119-120: ["The applicability of the results of this study to other (less extreme) environments, and different types of fog, is discussed in section 4."]

- l. 173: here you study only cases without cloud. Please comment on what effect clouds above fog would have on AERI observations.

We are assuming that you mean line 177 in the original manuscript here. Typically, radiation fog forms under clear skies, so it makes sense to use this criterion to identify radiation fog events.
As described in section 3.1, the temperature profile retrievals from the AERI are highly sensitive to the cloud base height assumption, and so an accurate cloud height (i.e. from a ceilometer) would be required to retrieve thermodynamic profiles in the presence of cloud. Also, as pointed out in the introduction, if the total optical depth of the cloud is so thick that the cloud is opaque in the infrared, then the AERI is not sensitive to the atmosphere above the cloud. For the liquid water path retrieval, both instruments would be sensitive to another cloud layer above an optically thin fog, and so we wouldn't be able to separate the fog liquid water path from the cloud liquid water path in this case.

We added a line in section 3.2 to clarify this point:

Lines 192-195: ["Radiation fogs typically form under clear skies and as such we only consider cases of fog under otherwise clear skies, this allows us to be certain that the LWP retrievals are a measure of fog LWP alone."]

- l. 249: I undertand that you are limited to max altitude 10m by the height of the tower. However, this comparison is most probably artificially penalising MWR which, with its lower vertical resolution, might perform better when comparing to 20 or 50 m. And having a somwhat thicker layer might also make sense from a physics point of view. Please comment on this.

One of the key points that we are trying to emphasize here is that the low vertical resolution of the MWR means that it is unable to retrieve the strong surface-based temperature inversions associated with radiation fog formation. The surface inversions associated with radiation fog initiate in the lowest 10 m above the surface at Summit, and greatest part of the temperature inversion associated with radiation fogs is often concentrated in this layer (Price 2011; Izett et al., 2019). So, we believe that this comparison is important to emphasize that the resolution of the MWR is insufficient to capture these events. However, w have added an additional panel to Fig. 6 showing the comparison of the 100 m – 10 m inversion strength with the 14 coincident radiosonde profiles, unfortunately the limited number of radiosondes available limit the conclusions we can draw from this. We have added the following text discussing the results of this comparison:

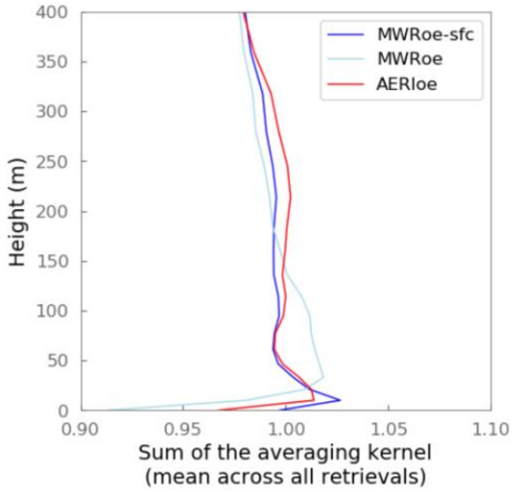
Lines 362 to 369: ["The radiosonde profiles provide an alternative independent measure of surface inversion strength, allowing the comparison of the ability of each retrieval configuration to capture surface temperature inversions over a deeper layer. Fig 6b compares the 100 m - 10 m retrieved inversion strength with that measured by the 14 coincident radiosonde profiles. Over this depth the RMSE of the AERIoe and the MWRoe-sfc are comparable to the values for the 10 m - 0 m comparison (1.65 and 1.83 C m-1 respectively), but the MWRoe RMSE remains much larger (2.22 C m-1), demonstrating that the MWRoe alone is not capable of accurate retrievals of surface temperature inversions even in this deeper layer. Only the AERIoe retrievals in this case are significantly correlated (r=0.46) with the radiosonde measurements, although the small number of radiosondes available for comparison makes it difficult to draw robust conclusions from this result."]

- l. 317: "This suggests that most of the information ... comes from the prior" -> It would be interesting to see the measurement response (i.e. the sum of the averaging kernels). This would allow us to judge how much info comes from the prior rather than just guessing.

Thank you for mentioning this – after examining the averaging kernel I think that my assumption that most of the information comes from the prior is actually incorrect. The reason for the constant difference is that the averaging kernel for perturbations in temperature at '0 m' and '10 m' is almost identical for the MWRoe – it simply cannot see a difference between the two levels, hence the constant difference between the two.

Below we've included plots of the sum of the averaging kernel as you recommended, and also the averaging kernel function at selected heights in the boundary layer for each retrieval methodology.

We have rephrased the line in question to the following:
Lines 353 to 354: [" In fact, the 10 m - 0 m lapse rate is essentially constant in the MWRoe, implying that retrieved temperatures at 0 m and 10 m are highly correlated."]



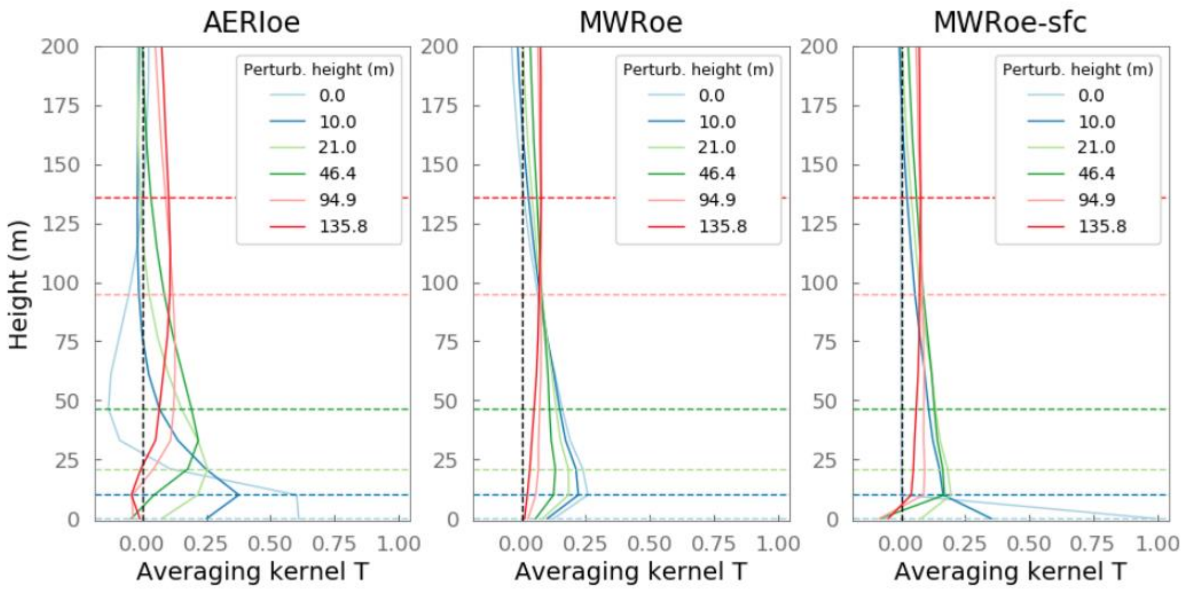


Fig. 8: Please specify the extent of the central box in the legend

The box plot whiskers in figure 8 extend to include 90% of all data points. The central part of the box plot shows the interquartile range (25th-75th percentile). We have included this in the figure caption.

Appendix B: Can you also show the corresponding figure for the 0-10m lapse rate with and without surface temperature. I would suggest to use this instead of Fig. 6

We have added this to figure 6. Although we would like to highlight that this is not really a fair comparison since we used the surface tower measurement to constrain the retrievals for the MWRoe-sfc, so the two variables are not independent but, based on your suggestions, we have included it for reference and I make a note of this in the text:

Lines 357 to 361: ["When the in-situ surface temperatures are used to constrain the MWR retrieval (in the MWRoe-sfc), the ability of the retrieval to capture the shallow temperature inversions is considerably improved (Fig. 6a). Note that the correlation between the MWRoe-sfc near surface temperature inversion and the in-situ measurements in Fig. 6a is not a fair assessment of performance since the retrieval results are not independent from the in-situ measurements. Nonetheless, it highlights the importance of using accurate surface temperature measurements to constrain MWR temperature retrievals."]

- l. 388: ... Results of Appendix B... --> you probably wanted to add a reference to Appendix B

This line has been deleted since we have integrated appendix B into the main text.

- l. 427: "AERI contain more information about the temperature near the surface than the MWR measurements". you might add something like "what indicates the importance of including surface observations to the retrieval if available from the MWR's weather station"

Thank you, we have added this in line 495:

[“This highlights the importance of using accurate surface temperature measurements to constrain MWR thermodynamic profile retrievals.”]