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Interactive comment on "Pan-Arctic measurements of wintertime water vapour column using a satellite-borne microwave radiometer" *by* Christopher Perro et al.

Anonymous Referee #2

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Review of "Pan-Arctic measurements of wintertime water vapour column using a satellite-borne microwave radiometer" by C. Perro et al.

General comments:

This study builds upon a previous AMT paper's water vapour retrieval by adding further validation/comparison, improved reflectances, and some tunable parameters into the equation relating TBs, reflectances, and transmittances. The authors implicitly argue that the retrieval is to be trusted on virtue of the good validation against two ground-based sensors, which in turn means that all the reanalyses examined might have a systematic bias.

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Discussion paper

While it is an interesting study and well written, there are too many pieces missing here for it to be recommended for publication. The methodology is quite light, relying heavily on the Perro et al. (2016) study and a submitted manuscript to IEEE TGRS (therefore not reviewable at this time) on the reflectances methods that is one of the key novelties of this updated study. It is also disconcerting that an undefined tuning parameter is introduced at the end of the appendix (delta W) but described in no detail. In addition there is no discussion of the retrieval's error sources or uncertainties, including the influence of supercooled cloud water or ice clouds, which can certainly impact TBs at these frequencies.

The study's conclusions are fittingly light as well, but few of these are significant because of the study's limited scope. Why are all comparisons limited to 6kg of water vapour or less? Because all comparisons are from this subset, it is hard to judge the retrieval's performance for different conditions. And if the retrieved quantities do not permit strong conclusions, then assessment of the radiative transfer underpinning the retrieval could yield more significant conclusions. But instead the authors shy away from giving explanations for some of these aspects, such as the possible cause for elevation changes causing errors or why such systematic errors exist that necessitate the tuning parameters outlined in the appendix.

My recommendation is to reject the current manuscript but encourage resubmission once the study has been fleshed out by addressing the following major criticisms. A selection of minor/textual comments follows the major points.

Major:

1. It is unacceptable to rely upon unpublished work as a foundational part of the retrieval methodology, and it is not correct to list a submitted manuscript as 'Perro et al. (2018)' in the text when it is not published as this is misleading. It is difficult for authors when coincident manuscripts are in the review process, but it means that a reviewer cannot judge the methods fully because the information is just not available. If this

were a very minor part of the methodology then it would not be as big a deal, but it is cited numerous times.

2. A tuning parameter, delta W, is confusingly introduced at the end of the appendix. It is not defined in any equation that I saw, and then it's admitted that the "source of the error requiring this correction is unknown" (P17 L13). A quick glance at Table 4 shows that this parameter can be as big as 2kg? It's unacceptable to have this separate from the main methodology when its magnitude is so large. It would also be good to have some discussion of the magnitude of the other tuning parameters in Table 4 and what these mean; differences in the bias coefficients and reflectances of up to 3.5K and 0.15 are quite significant indeed, but yet gets no discussion. To me these represent large enough tuning to signal the inadequacy of the forward model in some regimes.

3. Clouds are only mentioned in the manuscript when referring to the IR instrument, as it only performs retrieval in clear-sky. They are not treated in the forward model, which would be fine if they were entirely radiatively insignificant, but both liquid and ice clouds have a non-negligible impact on microwave radiances. Even in the Arctic wintertime, supercooled water can exist in clouds (Cesana et al. 2012 (https://doi.org/10.1029/2012GL053385)), and thicker ice clouds can exert a significant scattering signal on radiances near 183GHz. If this were admitted as an error source and quantified that would be better, but instead it seems these errors are just folded into the myriad tuning parameters of Table 4.

4. All of the comparison and validation is limited to cases of <6kg water vapour. This seems arbitrary, and was never justified. Why are higher values not included, or the results stratified? This was especially confusing because of the separation of the forward model parameters into the low/mid/extended water vapour regimes.

5. The validation against E-AERI left a few unanswered questions. Firstly, the bias as reported in Table 3 of '+0.0002' should not be written that way as surely the measurement accuracy of either retrieval does not approach that many significant figures.

Secondly, the zero bias and very low RMS error seems unlikely, given the different fields of view, sensor noise, and imperfect space-time matchups. Do the E-AERI retrievals within some time window, let alone over some spatial domain, vary more or less than the reported RMS deviation of 0.12kg? Some context here would be very useful for readers to judge how good this result is, and more importantly its statistical significance. Because there is no discussion of retrieval errors, or possible errors from E-AERI itself, it is hard to judge such questions.

6. Inclusion of Figure 6 does not seem to be justified. It is a speculative assertion that is not underpinned by any statistics (say correlation between the two) and there is no physical mechanism hypothesized to be behind it (P13 L18), so its inclusion caused more confusion than insight.

Minor/textual issues:

P2 L25: This paragraph contains results of a kind, comparing ERA5 to other reanalyses, so it seems inappropriate in the introduction. Further, none of the reanalysis acronyms are defined.

P5 L7: It's speculative to say that these 'are not expected to impact the conclusions' so this should be removed.

P5 L27: "A formula was fit" is vague and should be explained.

P6 L7: What does "increased retrieval noise" mean here?

P7 L5: Why would more water vapour over the ocean cause reflectance ratio errors? This requires physical explanation.

P8 L14: I don't think this is the correct use of "comparator" though it may be a usage I am unfamiliar with.

P8 L16: Improved relative to what?

P10 Fig3: There should be units given for A and B.

P10 L1: “scientists” should be “sondes”?

P11 L3: What kind of “regular grid” was chosen? And are the 711 locations all of the points on this grid or were they chosen in some way?

P14 L9: There should be at least one citation to back this up.

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