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Atmospheric Measurement Techniques Discussions

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

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1 Response to Referee 2:

General comments:

Referee 2: 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 groundbased sensors, which in turn means that all the reanalyses examined might

have a systematic bias.

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.

Authors: Thank you for your feedback on our manuscript. We will respond to each of the above points below, where they are repeated in more detail. We believe that we have a good handle on each of the major points owing to the work put into the previously published Ph.D. thesis by Perro (2017; https://dalspace.library.dal.ca/handle/10222/73353). We therefore hope that the Referee will be open to considering a revised manuscript.

Major:

Referee 2: 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.

Authors: The paper by Perro et al. (2018) remains under review, and represents an updated analysis of that given in the Ph. D. thesis by Perro (2017; https://dalspace.library.dal.ca/handle/10222/73353). We have emailed a pdf of the draft paper to the editor to share with both Referees.

While it would have been better to have had the surface emissivity paper published prior to submitting this paper on water vapour measurements, this was not a practical possibility given the time, funding, and career development constraints on a PostDoctoral fellow (Perro).

Referee 2: 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.

Authors: It is not possible to include delta W in an equation because the water vapour column is an implicit parameter of Eq. 9 and determined using an optimization

procedure. The delta W correction of 2 kg/m2 is for the extended regime only. The choice of limiting our comparisons to columns less than 6 kg/m2 means that only the low and mid regimes – where the corrections are small – are used. We agree that this is not obvious and will add clarifying points to the revised manuscript.

Despite much effort, we still do not know why the delta W correction for the extended regime is so large. We made that point clear in the manuscript in the interests of transparency.

Referee 2: 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.

Authors: The numbers quoted above are for the extended regime, which is not included in the comparative work in this manuscript. We agree that there may be some inadequacy in the forward model (RTTOV, the radiative transfer model published and used by ECMWF). To this we would add the following additional sources of error:

1) Issue with the calibration of the satellite instrument brightness temperatures.

2) Auxiliary profile information having a consistent bias in all or part of the temperature or water vapour shape profiles.

We will make these points in the revised manuscript. To determine which of these is causing the issue with the retrievals is difficult – we must rely on the output of other groups in this work – and so we determined it best to use the empirical corrections included in this manuscript.

Referee 2: 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.

Authors: We agree that there is a potential cloud effect, and that it should be discussed. In particular, we will refer to the paper of Cesana et al. (2012).

The impact of clouds on water vapour retrievals was considered by Perro (Ph. D. thesis, 2017). He found that the statistical impact of clouds on comparisons between radiosonde and satellite water vapour columns was small. This point was discussed briefly in Perro et al. (2016).

A paper suggested by Referee 1 (doi:10.1109/JSTARS.2015.2499083) is helpful in assessing the potential impact on our conclusions. Because clouds are thought to cause a dry bias in microwave water vapour retrievals, they cannot explain the fact that our measurements are relatively moist compared to ERA5. We will make this point in our revised manuscript.

A detailed analysis of how clouds impact our retrieval will require a separate publication. The analysis would be too extensive to include here. Fully assessing the impact of clouds is in our near-term plans.

Referee 2: 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.

Authors: We agree that the reasons for this choice need to be made clear, and will do so in the revised manuscript. We find that the extended regime retrievals are noisier, and the tuneable correction parameters are larger, than in either the low or mid regimes. We therefore have less confidence in the extended regime data. Limiting our analysis to 6 kg/m2 and below eliminates the extended regime from comparisons and is consistent with the earlier analysis of Perro et al. (2016).

Referee 2: 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.

Authors:

We appreciate the concern here: the comparison between the two data sets indeed looks very good, but there is no uncertainty associated with the measured bias. We will obtain an uncertainty estimate by determining the bias implied by errors in linear least-squares fits to our data.

We note that the E-AERI data in our paper represented 7-minute integrations. Assuming an average wind speed of 20 m/s leads to an equivalent spatial resolution of about 8.4 km. The ATMS has a spatial resolution of 15 km at nadir. Thus, the resolutions of the two instruments are reasonably compatible. Higher resolution E-AERI data are not available, and so it is not possible for us to look at variations in a smaller time window. Variations in a longer time window would need to be compared to larger spatial integrations of the ATMS measurements.

Errors in the E-AERI operating at Eureka and in ancillary data used in the retrievals were characterized and estimated as described in Knuteson et al (2004a,b), Rowe et al (2008) and Weaver et al (2017). These errors were propagated to uncertainties in retrieved water vapour (Weaver et al 2017). In previous work (Weaver et al 2017), uncertainties were found to be 3 to 11% for summer to winter at Eureka and water vapour retrievals from the E-AERI were found to agree with several other instruments to within combined measurement errors, with large biases (1 to 3%) and comparably small standard deviations (0.1 to 0.3%) between instruments. For this work, uncertainties in retrieved water vapour are typically 8 to 11% for December-March cases at Eureka.

We will address the above issues in the revised manuscript.

Referee 2: 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.

Authors: We will remove figure 6 and the discussion surrounding the sloping terrain.

Minor/textual issues:

Referee 2: 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.

Authors: We will remove this paragraph and will expand the acronyms for the various data products (and provide references as appropriate) where they are first introduced.

Referee 2: P5 L7: It's speculative to say that these 'are not expected to impact the conclusions' so this should be removed.

Authors: We will remove this statement, as requested.

Referee 2: P5 L27: "A formula was fit" is vague and should be explained.

Authors: We can explain this more simply by saying that the intermediate values were interpolated to higher resolution.

Referee 2: P6 L7: What does "increased retrieval noise" mean here?

Authors:

This issue was also raised by Referee 1. Equation 6, which forms the basis for the water vapour retrieval in our work (and that of Miao et al. (2001) and Melsheimer and Heyster (2008)), require brightness temperature difference measurements. Small differences in temperature lead to large relative errors – what we called "retreival noise" in the original manuscript. All immediately adjacent 183 GHz channels have similar brightness temperatures. We therefore excluded certain channels from our analysis to avoid this difficulty. We will replace the confusing sentence in our original manuscript with an explanation based on this discussion.

Referee 2: P7 L5: Why would more water vapour over the ocean cause reflectance ratio errors? This requires physical explanation.

Authors: Measured radiances have components due to both the surface and atmospheric water vapour. Removal of the atmospheric water vapour contribution using auxiliary data is more uncertain (owing to errors in the auxiliary data) when the water vapour columns are greater, as is climatologically the case over ocean. We will provide this physical explanation in the revised manuscript.

Referee 2: P8 L14: I don't think this is the correct use of "comparator" though it may be a usage I am unfamiliar with.

Authors: Referee 1 also raised this point. We will replace "comparator data product" with "data product being compared".

Referee 2: P8 L16: Improved relative to what?

Authors: We mean improved relative to our 2016 analysis, and will make that clear in the revised manuscript.

Referee 2: P10 Fig3: There should be units given for A and B.

Authors: The change is a relative change so A and B are unitess. In the revised manuscript we will represent these as percentages so that the units are clear.

Referee 2: P10 L1: "scientists" should be "sondes"?

Authors: Agreed. We will fix this typo.

Referee 2: 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?

Authors: The latitudinal grid resolution is constant at 2.5 degrees. Below 80 N the longitudinal grid resolution is 5 degrees, between 80 and 85 N it is 10 degrees, and above 85 N it is 20 degrees. Yes, the 711 locations represent all of the points on the grid. We will clarify these points in the revised manuscript.

Referee 2: P14 L9: There should be at least one citation to back this up.

Authors: We will cite Scarlat et al. (2018) and Melsheimer and Heygster (2008).

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