

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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We thank Reviewer 1 for their constructive comments, and agree that a better understanding of polar water vapour is important. While the revisions proposed were deemed as “substantial”, we think that they can be addressed in the time frame allowed by this review. We note that Reviewer 2 raised some of the same points.

Rather than respond point by point at this time, we will address a selection of issues in what we view as their order of significance. Remaining issues and revision details will be provided in our final response to the review after discussion has closed.

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1) Reviewer #1: “The effect of clouds has been neglected in this study.”

Authors: We agree that there is a potential cloud effect, and that it should be discussed. The impact of clouds on water vapour retrievals is discussed in detail by C. Perro in his Ph.D. thesis (Dalhousie University, 2017, Chapter 3, <https://dalspace.library.dal.ca/handle/10222/73353>). 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). We are presently revisiting this analysis, with a view toward publication.

2) Reviewer #1: “. . . the authors take into account retrieved values up to only 6 kg/m² (low and mid range regimes only) - the reason or motivation for this is not given.”

We find that the extended regime retrievals are noisier, and the tuneable correction parameters are larger. The noise in the extended regime would unduly affect the statistics of the low and mid regimes. Limiting our analysis to 6 kg/m² and below eliminates these problems and is consistent with the earlier analysis of Perro et al. (2016).

3) Reviewer #1: “Reference is made to a still unpublished study of the same first author. . . .”

Authors: This paper is still under review, and represents an updated analysis of that given in the thesis of C. Perro. We expect to post the paper on archive.org for the next stage of review.

4) Reviewer #1: “Do the authors assume here that all land is snow/ice covered?”

Authors: As a first analysis, we have divided the surface into four categories: first-year ice, multi-year ice, land and ocean. We agree that subdividing land into further categories would be useful, and expect to pursue that in future work. It seems reasonable to assume that most land surfaces in this winter study are snow covered.

In the thesis of C. Perro, the surface reflection assumptions were tested against actual water vapour retrievals. The same result for land was obtained as was found in the

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above-mentioned submitted paper. We propose to include these results in an appendix to a revised manuscript.

5) Reviewer #1: “Wouldn’t it be easier to determine b12 and b23 empirically right away, bypassing the need for model/reanalysis data?”

Authors: The approach advocated is effectively the same as that of Melsheimer and Heygster (2008). As discussed by Perro et al. (2016), our new approach is more accurate at the cost of computational complexity. The tuneable parameters create consistency between and within regimes, without removing the advantages of computing bias coefficients from auxiliary profiles that vary in time and space.

6) Reviewer #1: “1c) Internal calibration? The authors state that the calibration (determination of the adjustment parameters) “does not depend on outside parameters” (P.5, L.3). I disagree: As we see in Appendix A, all curves used for the parameter determination are plotted with reanalysis WVC values as x-axis.”

Authors: The point we are trying to make is that the data are not calibrated to match those of any external source. For example, we calibrate oblique satellite measurements against nadir satellite measurements, but not against radiosondes or other external measurements. We will find a better way to express this in the revised manuscript.

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