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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.

Anonymous Referee #1

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1 General comments

The manuscript presents a method for the retrieval of the water vapour column (WVC) in the Arctic using satellite microwave radiometers. It builds on the algorithm published by the same first author a few years ago (Perro et al., 2016). The novelty here is that it uses brightness temperatures measured by a newer instrument (AMTS on Suomi NPP instead of MHS on NOAA-POES and MetOp satellites), and that it takes into account the different reflection and emission properties of the various ground surface types occurring in the Arctic, such as open water, sea ice (first-year ice and multiyear ice) and land. The results of this retrieval for several winter seasons are compared with

ground-based WVC measurements and with meteorological reanalysis data. While the satellite retrieval results compare well with the ground-based measurements, they generally show higher column water vapour than the reanalysis data.

Water vapour is an essential component in most weather and climate related processes. Monitoring water vapour in polar regions is therefore a very relevant topic as such data are sparse, and this study is a useful contribution to this field. There are, however, a number of issues that need clarification, further discussion or analyses. I therefore suggest acceptance after substantial revision.

2 Specific comments

1) P.4, L18ff.:
Here, the authors first introduce the "tuneable parameters" δb_{23}, $\delta \frac{r_1}{r_2}$, $\delta \frac{r_2}{r_3}$ and δW. There are several issues here:

   1a) A general one:
   The algorithm presented here (and the one by Perry et al., 2016) is more analytical than the related algorithms by Miao (1999) and Melsheimer and Heygster (2008) because here, the parameters b_{12} and b_{23} are actually calculated using model profiles of the atmosphere, instead of just deriving them empirically from fits with data. The cost for this is, of course, that one needs model or reanalysis data. However, then, the authors still introduce further empirical parameters to adjust the retrieval algorithm. Wouldn't it be easier to determine b_{12} and b_{23} empirically right away, bypassing the need for model/reanalysis data?

   1b) Specifically about b_{23}:
   There are actually 3 distinct parameters b_{23}, one for each regime, because the numbers 1, 2 and 3 represent different channels in each regime (see

Table 2). To avoid confusion, the parameter names should be different - I suggest a superscript for the regime (L - low, M - mid, X - extended). Therefore, there are also three distinct tuneable parameters b_{23}. See also items 9) and 10) further below. The same applies, by the way, for b_{12}, but then note that $b_{12}^L = b_{23}^M$ and $b_{12}^M = b_{23}^X$. I suggest to add a small section explaining all this earlier in the manuscript, probably in section 2.3 "Regimes".

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.

2) P.5, L.8ff. ("2.2. Surface Reflection Mixtures"):
Reference is made to a still unpublished study of the same first author (Perry et al., 2018, submitted) about the emissivity of the different surface types. This is unfortunate as the main feature that distinguishes the retrieval method in the present manuscript from the method published earlier (Perry et al., 2016), namely, the accounting for varying surface properties, relies on that unpublished study.

3) P.5, L.9/10:
Land is treated as a Lambertian reflector. This is surprising as in the microwave range land is usually treated as specular reflector, unless covered by snow or ice. Do the authors assume here that all land is snow/ice covered? This is probably a reasonable assumption as the study is restricted to the winter months, but this should be mentioned here explicitly.

4) P.6, L.6/7. "... due to the increased retrieval noise with small differences in frequency"
I do not understand this explanation - is the retrieval noise higher for the two channels left out in this study? What do you mean by "small differences in fre-

quency"? The spacing of the sidebands is at 1, 1.8, 3, 4.5 and 7 GHz from the central frequency, the extra channels at 1.8 and 4.5 are not particularly close to the others, at least at first sight. And in which channels are brightness temperatures therefore similar?

5) P.9, L.9/10: "...the range of water vapour values encountered is ... smaller"
- Why are water vapour values in Eureka so much smaller? If this is simply the climatology, that should be briefly mentioned, if not, it should be discussed.

6) P.9, L.11ff. "... sloping terrain", and P.13, L.1-13, and Fig.6:
Why should the topography, or the terrain slope, have an influence on the satellite retrieval or its agreement with ground-based measurements? The physical reasons/mechanisms should be explained and discussed (at least qualitatively). Is it just the effect of the "shorter" air column above elevated ground? But are the elevation variations near the measurement stations large enough to cause the observed effect?

7) Sections 4 (Radiosonde Comparison) and 5 (Reanalyses Comparison):
The algorithm, using the three regimes, can retrieve up to 14 kg/m2 WVC. In all the comparisons, 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. This should be explained and discussed, or else the whole range should be used. (Note also that the WVC range shown in the plot in Fig. 4 is actually 0 to 10 kg/m2, although RMSD and bias are calculated only for WVC < 6 kg/m2, which is confusing)

8) P.15, L.2-4:
Why are oblique measurements drier than nadir measurements? Is there a physical reason for that? Maybe some saturation effect? This should be discussed.

9) P.15, L.6-8, about the adjustment parameters:

As mentioned above in item 1b), the authors must state clearly that there are three separate adjustment parameters δb_{23}, one for each regime (see above the suggestion with the superscripts).

10) P.15. Section A.1 ("Bias Coefficient Adjustment Parameters")
The authors should state more clearly that they show a plot for the determination of one of the three parameters only, or they should rather state that the adjustment parameters for the mid and extended regime have been determined in a similar way.

11) The effect of clouds has been neglected in this study. However, in particular ice clouds have a strong effect on the 183 GHz channels because of scattering. These channels are even used for the detection of strong convection associated with, e.g., polar lows. The effect of ice clouds on this kind of algorithm are erroneously low water vapour retrievals (see, e.g., doi:10.1109/JSTARS.2015.2499083)

3 Technical corrections

P.8, L.4: constaint -> constraint

P.8, L.14 and P.12, caption of Fig. 5, and elsewhere: "comparator data product": 'comparator' is the wrong word here. Rather "data product with which the comparison was done"

Fig.3 and Fig.5: Maybe express the relative RMS deviation and relative bias in per cent. If plain number are given, they might be misunderstood as per cent and will be much too low. In addition, in the text, the authors use per cent.

4 Acronyms used here

WVC: water vapour column

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