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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.

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1 Response to Referee 1:

1 General comments

Referee 1: 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.

Authors: Thank you for your comments. We agree that more attention needs to be paid to Arctic water vapour, and appreciate your recommendation to publish after substantial revision. We take your concerns seriously and have addressed all of the points your raised in your review.

2 Specific comments

Referee 1: 1) P.4, L18ff.: Here, the authors first introduce the "tuneable parameters" δb23, δr1r2 , δr2r3 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 b12 and b23 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 b12 and b23 empirically right away, bypassing the need for model/reanalysis data?

Authors: The approach advocated above is effectively the same as that of Melsheimer and Heygster (2008), which has fixed values for the bias coefficients b_{12} and b_{23} for a given viewing angle. Our bias coefficients vary in time and space with the atmospheric conditions. As discussed by Perro et al. (2016), our new approach is more accurate at the cost of computational complexity. We will clarify these points in Sec. 2.1 of the revised manuscript.

In our analysis we find that there is a dependence on viewing angle and discontinuities between regimes. These issues are eliminated in Melsheimer and Heyster's (2008) analysis by calibrating their retrievals against radiosonde measurements. Because we wish for our retrieval to be independent of radiosonde measurements, we have introduced correction factors to allow for an internal calibration. Effectively, the correction factors force measurements at different viewing angles to agree. This point will also be clarified in the revised manuscript.

Referee 1: 1b) Specifically about b23: There are actually 3 distinct parameters b23, 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 b23. See also items 9) and 10) further below. The same applies, by the way, for b12, but then note that bL12 = bM23 and bM12 = bX23. I suggest to add a small section explaining all this earlier in the manuscript, probably in section 2.3 "Regimes".

Authors: Thank you for this suggestion, which will indeed help us to better explain a complex issue. This will also make our work more consistent with MH2008's since they use a similar notation to what is suggested.

Referee 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 our oblique satellite measurements against our nadir satellite measurements, but not against radiosondes or other external measurements. We will clarify this point in the revised manuscript.

Referee 1: 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.

Authors: This paper is 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 1: 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.

Authors: For the present analysis, we divided the surface into four categories: first-year ice, multi-year ice, land and ocean. 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, and we will state that explicitly in the revised manuscript.

We presented evidence in the surface emissivity paper currently under review that shows convincingly that land should be treated as a Lambertian reflector. We will emphasize this point, and that it is different from what is typically done, in the revised manuscript.

Referee 1: 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 frequency"? 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?

Authors:

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

Referee 1: 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.

Authors: Yes, the smaller range of water vapour values at Eureka is climatological. We will indicate this in the updated manuscript and refer to the climatology at Eureka of Lesins et al. (Atmosphere-Ocean, 2010, https://www.tandfonline.com/doi/abs/10.3137/AO1103.2010) for support.

Referee 1: 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?

Authors: Referee 2 also remarked on this section, and found it too speculative. We agree with their assessment and have decided to remove it from the paper.

In answers to your questions, you are correct, the shorter air column is the effect we considered that could influence the retrieved water vapour column. The effect is not large enough to significantly influence our comparison.

Referee 1: 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)

Authors: 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). We will address this in the revised manuscript, and provide statistics to indicate what percentage of the measurements are eliminated by this choice.

We believe it is still valuable to plot measurements up to 10 kg/m2, because it makes readily apparent the larger error at higher values and that most columns measured are below 6 kg/m2. The plot also shows that the trend of relative dryness in ECMWF extends to the higher water vapour columns. We will make this point in the revised manuscript.

Referee 1: 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.

Authors:

We have performed simulations to test the saturation hypothesis, and found it could not explain relatively dry retrievals at oblique angles. We will make this point in the revised manuscript.

There are a few possible reasons that could introduce the errors we are seeing:

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.

3) Errors in the RTTOV radiative transfer scheme.

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. We will address these points in the revised manuscript.

Referee 1: 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 δb23, one for each regime (see above the suggestion with the superscripts).

Authors: Agreed. We will make this recommended change to the revised manuscript.

Referee 1: 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.

Authors: Agreed. We will include both points in the revised manuscript.

Referee 1: 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)

Authors: We agree that there is a potential cloud effect, and that it should be discussed. 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).

The paper provided by the referee 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.

3 Technical corrections

Referee 1: P.8, L.4: constaint -> constraint

Authors: We will make this change.

Referee 1: 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"

Authors: We will make this change.

Referee 1: 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.

Authors: We will make this change.

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