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Atmospheric Measurement Techniques Discussions

Interactive comment on "A microwave satellite water vapour column retrieval for polar winter conditions" *by* C. Perro et al.

Anonymous Referee #2

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The manuscript presents a new algorithm (PLDC15) for the retrieval of Arctic column water vapor from measurements by microwave instruments deployed on operational meteorological weather satellites. Monitoring water vapor in this climate sensitive region is both challenging and important.

The algorithm is only a slight update of previous ones, i.e. Melsheimer and Heygster (2008; called MH08) retrieval. However, the incorporation of a priori information is shown to provide a major benefit in a synthetic study and in the assessment using ground-based observations (GVR) at Barrow as a reference. Several other water vapor products are also evaluated providing an interesting add-on. In general the paper provides an interesting contribution though the relation of the study to previous work (see below point 1) and the uncertainty analysis (2) need to be improved. The latter

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also includes the formulation of statements in respect to algorithm quality.

1. In the introduction I am missing background information on the need of algorithm, e.g. see Serreze et al. (2012), motivating the evaluation of the new algorithm and its relative performance to various water vapor products from reanalysis and satellite. Further, the introduction should include an overview of other sources of information/ground truth for water vapor like the classical radiosonde profiling and many more – see also the GEWEX water vapor assessment (G-vap; http://gewex-vap.org/) to motivate that the GVR is especially suited. In my view the introduction should already name the challenges of satellite retrievals and intercomparisons, namely the problem of varying surface emissivity (nowhere the variation with surface type is quantified) and spatio-temporal variability of water vapor (Bühler et al., 2012, Tobin et al., 2006).

2. The authors are sometimes to optimistic with their statements (see detailed description in my minor comments). In its current form I can (yet) not share the view of the authors on their algorithm as expressed in the abstract: "The errors are shown to be significantly less than for other satellite measurement systems." or on p9970, l17 "The results indicate that the PLDC15 retrieval is more accurate." In section 4 the performance of the algorithm (PLDC15) in comparison to MH08 is tested on a synthetic data set. Here Case 1 (ideal) and Case 2 (addition of noise) represent self tests of the PLDC15 algorithm and only Case 3 is a realistic application. For Case 1 and 2 MH08 can not perform better as the coefficients are not tuned to the specific situation For the realistic application (Case 3) for most regimes MH08 is superior to PLDC15 (see Table 3)? This proves that the major benefit of PLDC15 arises from the incorporation of the a priori. Therefore the question is: how good does the apriori have to be that PLDC15 becomes superior to MH08 and other methods? BTW, it is irritating that for Case 3 – in contrast to Cases 1 and 2 the scatter plot is shown only for PLDC15 and not for MH08. This synthetic study (Case 2) should be extended to not only test the effect of noise but also to test the assumptions in respect to emissivity and cloud liquid/ice to estimate the associated uncertainties. I appreciate section 5.3 which includes a comprehensive list

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of uncertainties (but I would like to see more in depth attempts to estimate the relative contribution (see above). There are also additional points of uncertainty: âĂć Both GVR and MHS use the same spectral range and therefore any uncertainties in the spectroscopy are cancelled. Also both would suffer from liquid water emission and/or scattering. To address this issue a comparison of satellite estimates with Barrow radiosoundings could further validate the algorithm. With the strongly overlapping orbits there are hopefully sufficient data point.

âĂć Supercooled liquid cloud layers occur frequently in the Arctic. They absorb microwave radiation strongly increasing in strength with frequency. Thus, I am not convinced by the statement on p 9964, l19. Here the authors refer to Miao et al. (2001) but since this time more detailed measurements on Arctic mixed phase clouds have revealed their complicated nature. As shown by Xie et al. (2015) liquid and snow significantly effect microwave brigthness temperatures for ground- and spaceborne geometry and also the relative layering of liquid and ice leads plays a role. The authors should at least provide a quantitative estimation of the effect of hydrometeors.

3. For potential users of the algorithm detailed information how to apply it would be highly desirable – for example in an appendix ?

Minor points

Abstract: “The errors are shown to be significantly less than for other satellite measurement systems.” Here you need to be more specific. I guess it concerns Table 5 so it is important to name the ones used there and say which satellite products perform better/worse? And are you sure that this is true everywhere in the Arctic?

P9961, line 20: explain acronym “ARM”

Introduction: In fact I would have liked a separate section on the GVR rather than having processing details in the introduction. This could also address some more details on the intercomparison with radiosondes. Of that period? relative error? Humidity

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correction of radiosondes?

P9962, 1st paragraph need to explain acronyms, make clear that MIRS is not an instrument!

Section 2: The title of section 2 is misleading as “Microwave signal formulation” also includes the GVR. I suggest to reformulate or have the GVR as a separate subsection.

P9962, Eq(1): It is very important to mention that this is already a reduced form of the RT equation! The sentence “.. satelliteborne microwave instrument is given ..” needs to be changed as it includes simplifications and assumptions. For example the detailed atmospheric vertical structure is parameterized in the factor m. What is exactly T0 – Tatm at the surface or at 2m? Please change the word “given” to “parameterized” or “simplified” and provide the reference (Guissard and Sobiewski, 1994) right in the beginning. What do you mean with “solar contribution to the microwave background”? Is this for the rather rare occasions that the sun is in the field of view?

P9963, l19: “Notice that the dependence on surface emissivity is eliminated due to the assumption constant conditions across the spectrum.”

P9967,l1: M08 -> MH08

P9969,l24: The sentence “The results indicate that the error for PLDC15 is lower than for MH08. The PLDC15 results also demonstrate the utility of using a priori information for regime selection.” should be eliminated. Case 1 is just a self test for the algorithm. Something would be wrong if it moves away from the a priori. As mentioned above (l 10) there is no chance for MH08 to be superior. What might be mentioned instead is that here only cloud free cases are tested and perfect knowledge of emissivity is assumed. I wonder

p9970, l17: “The results indicate that the PLDC15 retrieval is more accurate.“ see the previous point. This should be eliminated. Why don’t you use same regime classification or quantify the effect?

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p9970, l19: Exchange “worst-case“ scenario to “realistic”.

P9971, l2-2: “The results show that when the a priori is degraded our retrieval can be expected to perform comparably to MH08.” In my opinion this is only true for the low and mid regime.

Section 4.4: Why don’t you use case 2 which is much more suitable as the effect of measurement noise is included? How much do the results of changing emissivity ratios depend on the atmospheric situation?

Section 5: Suggest to change title to “Assessment of water vapor column using GVR”

Section 5.1: What about the elevation difference between ERA-Interim, MHS pixels, Barrow and other products? What is its impact on water vapor column? How does surface emissivity / land surface type vary around Barrow?

P9973,l15: To make you point ypu could also plot the standard deviation (or rel. error) as a function of water vapor column (binned).

P9974, l15: Noise in AMSU-B in Table 1 might be underestimated as in flight degradation probably has occurred for some instruments. Did you see dependence on satellite?

P9974, l16: I would move the following paragraph to 5.3 as it is a study on the uncertainty due to the choice of a priori.

P9977, l3: Note the strong meridional convergence of the oribits. In fact in 6 hours the whole Arctic is covered – therefore it is important to know how good PLDC15 performs elsewhere in the Arctic.

P9977, l7: Typically microwave brightness temperatures are directly assimilated into NWP models and reanalysis. Therefore I see the production of water vapor climatologies and budget studies as most Important aplications. Please formulate a bit more careful. argets application in

General: A single sentence can not be a paragraph.

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Table 5: The specification of the relative error would also be helpful

Table 6: say that comparison is with GVR

Figure 5: PLD15 -> PLDC15

Serreze, M. C., A. P. Barrett, and J. Stroeve (2012), Recent changes in tropospheric water vapor over the Arctic as assessed from radiosondes and atmospheric reanalyses, J. Geophys. Res., 117, D10104, doi:10.1029/2011JD017421.

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