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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of Total Water Vapour in the Arctic Using Microwave Humidity Sounders" *by* Raul Cristian Scarlat et al.

Anonymous Referee #2

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The manuscript "Retrieval of Total Water Vapour in the Arctic Using Microwave Humidity Sounders" by Scarlat et al. (amt-2017-219) builds upon previously published algorithms for retrieving total (integrated) water vapour in the Arctic from TOA brightness temperature measurements by passive microwave radiometers on satellite platforms. Where the previous work was constrained to areas of high sea ice concentrations, the main purpose of the extended algorithm is to include areas over open ocean and variable sea ice. Such development is particularly important in light of decreasing sea ice extent in the Arctic. The manuscript is straightforward and well-organized, and the new approach generally well-documented and well-integrated into the previous work. However, I had difficultly interpreting the cross-validations in Section 3 and recommend that this be clarified or revisited in a revision. Also, there are many cumbersome sentences

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and grammatical errors throughout the text (in particular near the beginning). I have flagged some examples under “Editorial Comments” and recommend the authors do a thorough edit for language before submitting the revision. The study is suitable for publication in Atmospheric Measurement Techniques, but my comments below should be addressed before publication.

General Comments:

I am struggling to interpret Section 3 for a number of reasons:

1) It was not clear that Fig. 5 was not actually a pixel count of the bottom panels on Fig 4, which according to the caption is just an example from 1 day. I think Figs 4 and 5 should be comparable. Perhaps keep the contents of Fig. 4 (the example is nice) and add additional panels for the “new” and “original” algorithms where instead of TWV, the pixels show the frequency of time that a retrieval is possible (perhaps for both January and July 2007-2009). This will provide the spatial context for Fig. 5.

2) You have not explained the domain over which you counted the pixels shown in Fig. 5. I’m assuming that this is the same as was plotted in Fig. 4 ($\sim$ north of 50°N) and if so it includes large regions of the north Atlantic and Pacific. It is likely then that a large number of newly-retrievable pixels are found outside the Arctic, which artificially inflates the percent increases reported in Section 3.1. Are you recommending that your method be applied in the north Atlantic and north Pacific too? If I understand correctly, the new algorithm is actually fairly limited in the more southerly parts of the map when averaged over time and these limitations are apparent also in Fig. 4. In addition to quantifying the improvement in coverage from the new algorithm, it would be interesting to know how close to total coverage is represented by the new algorithm. I recommend carefully defining the domain over which you recommend that algorithm be applied, explain this, and then use this domain to calculate all the results in Section 3.

3) I am confused about how to interpret the validation of the retrievals in comparison to

the ECMWF data. This is for two main reasons:

(3a) The ECMWF data are highly dependent on the model, especially at high latitudes, and are thus normally the type of data that is being validated. Therefore, it seems odd to conclude that the retrieval with the smallest systematic difference compared to ECMWF is the best retrieval. This is further confused by the fact that the ECMWF product likely assimilated the same/or similar data to that used in the retrievals. I realize you need a benchmark for comparison, so perhaps this can be resolved by tweaking the wording. What can you conclude through such a comparison?

(3b) AMSU-B new and AMSU-B orig are valid over different spatial areas and I assume the same is true for NN and RSS AMSR-E. I don't see where it is explained what spatial areas are averaged for the ECMWF data, but I know that it cannot apply to all four satellite algorithms at the same time. Therefore, it is not possible to isolate systematic differences in retrievals (which is interesting) to biases tied to spatial gaps (which was already established earlier in the section).

4) If the conclusion is that the new algorithm performs poorly compared to existing algorithms, what advantages are there to using it? If biases associated with spatial coverage could be separated from biases in the retrieval perhaps the advantages of the proposed algorithm would become clearer.

Specific Comments:

P4L19: AMSU-B is introduced here for the first time without explanation. I thought SSM/T2 was the data set being discussed.

P5L1-10: Can you more clearly articulate your motivation? While I learn later, it is not clear in the introduction why the Melsheimer and Heygster (2008) algorithm is unable to retrieve over the open ocean and marginal ice zone.

Section 2.8: What data set do you use to find the sea ice concentration?

P17L5-13: You have addressed the emissivity difference between 183 and 150 GHz

for the ocean component. Why not also develop an analogous correction for the sea ice so that this bias is corrected across the whole Arctic domain?

Editorial Comments:

P3L7-9: “Within this scenario . . .” is a very cumbersome sentence. Consider revising.

P3L11: no comma needed

P4L5: “Satellite retrievals also face. . .”

P4L22: “This assumption is false when switching” to “This is a poor assumption when using”. The sentence is a bit odd anyway. Do you mean that the 183 triplet is used with 150 up to 2 kg/m2 after which one of the 183 bands is saturated and uncertainty increases?

P5L9: “allows for application of”

Fig 2: It would be better to label the panels as 2a and 2b and refer to them in the text accordingly.

P16L16: You mean for the channels used for retrievals in the mid-range of TWV?

P23L11: “method-specific”

P25L3: “improve the retrieval”

Figs 8 and 9: Please add “new” and “old” to the AMSU-B label in the legends.

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