Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
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Comment on amt-2020-491

Anonymous Referee #1

Referee comment on "A systematic assessment of water vapor products in the Arctic: from instantaneous measurements to monthly means" by Susanne Crewell et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2020-491-RC1, 2021

General Comments:

Susanne Crewell1, Kerstin Ebell1, Patrick Konjari1, Mario Mech1, Tatiana Nomokonova1, Ana Radovan1, David Strack1, Arantxa M. Triana-Gómez2, Stefan Noël2, Raul Scarlat2, Gunnar Spreen2, Marion Maturilli3, Annette Rinke3, Irina Gorodetskaya4, Carolina Viceto4, Thomas August5, and Marc Schröder6

"A systematic assessment of water vapor products in the Arctic: from instantaneous measurements to monthly means"

This paper provides a useful comparison of several satellite products to reanalysis estimates over daily, monthly, and decadel time scales.

I enjoyed reading this paper and am interested in the results. I think a little more care should be given to how the vertical layering is defined for each product since past experience has shown that methodology differences among the products is a significant contributor to IWV variations among products. With the possible exception of AMSR2, none of these IWV estimates are direct measurements, they are all derived through integrating the atmosphere vertical column. The problem with that is of course the number density of water vapor molecules decreases exponentially with height. That means that the largest fraction of IWV is within the lowest 100's of meters of the atmosphere. Even small differences in the vertical layering used can change the estimated IWV. This issue may have been addressed in the project but since it is hardly mentioned in the paper I have my doubts that it was done carefully enough. However, despite that caveat I feel the results warrant publication as is.

Specific Comments:

1. This paper uses the word IASI for three different meanings; the sensor, the radiances, and the EUMETSAT L2 product. Strongly recommend the following:

"IASI" for the sensor

"IASI radiances" for the infrared spectrum subset assimilated into reanalysis products

"IASI L2 PPFv6" for the EUMETSAT L2 v6 product

2. In a similar way the AIRS product should be referred to as "AIRS L2 v6 IR-Only" to distiguish it from many AIRS products commonly in the past and in the future.

It is not really fair to compare what you call "AIRS" to "IASI" when the Aqua satellite was in it's 15th year of operation in 2017 after both the AMSU and MHS have failed. A fair comparison of AQUA to METOP products can be found in Roman et al. (2016) when all sensors were working correctly. While there were differences between the NASA and EUMETSAT products, they are not as serious as those in this paper which is using the AIRS IR-only product. I just want you to appreciate that it's not a fair comparison and that if you don't want to remove the AIRS results you should change what you say about them. For example I think you can use AIRS IR-Only as an example of how METOP can expect to degrade with time, or at least avoid implying negative connotations about "AIRS" by replacing "AIRS" with "degraded AQUA sensors" in the conclusions. As you should know the AIRS sensor itself is actually working normally after 19 years.

Line 201. "as well By" Punctuation.

Line 213 (Figure 2). Can the reanalysis difference be explained by a difference in the reanalysis surface pressure compared to the station surface pressure? Any surface pressure difference should be documented.

Line 220. Reanalysis.
Can you include a sentence that describes how the IWV was computed using NWP profiles? The details of that are important to document.

Line 253. I don't see the GNSS symbols in the lower panel of Figure 2. Can you include some explanation? Perhaps there is no GNSS at the Polarstern location? If true please clarify in the caption.

Line 286. add ", with a station elevation of 30m."

Line 286. Should estimate the PWV of the 30m Ny-Alesund station elevation and include that number in the text of the paper since it would contribute to a small bias relative to the ocean satellite observations.

Line 345. Did you compare the AIRS and IASI surface pressure to the station pressure? Since AIRS and IASI retrievals use NWP surface pressure as input the derived IWV can often contain an error due to a bias in assumed surface pressure in the satellite retrieval.

This issue has been known for a long time so I am surprised it is not mentioned as a contributing factor in this paper.
"For both the land and sea cases, uncertainties in the specification of surface pressure for the retrievals through interpolation of NCEP_GFS surface pressure (the only ancillary parameter in the AIRS APS) might be causing an error term, which requires further investigation. "
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2005JD006116

Line 503. This is nice. I like how you convert to surface IR flux. I agree this is the relevant issue.

Line 533. MHS is repeated
"from MHS and MHS"

Line 556. This statement about ERA5 and IASI is incorrect and should be modified or removed. ERA5 does not assimilate L2 sounding products it only assimilates a radiances. Also it is nearly impossible to attribute ERA5 changes to any specific data inputs. Since this is assertion is not proven in the paper it should not be in the conclusions.