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Atmospheric Measurement Techniques Discussions

Interactive comment on "Intercomparison of atmospheric water vapour measurements in the Canadian high Arctic" *by* Dan Weaver et al.

Anonymous Referee #3

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The paper investigates the accuracy of water vapour measurements in the Canadian Arctic (Eureka) over the time span from 2006 to 2014. This is a highly important topic as the role of water vapour for Arctic Amplification is uncertain and the detection of water vapour trends and their spatio-temporal patterns is a major challenge. As such the paper provides an important contribution to the field. However, there are some major issues in terms of focus and technical aspects that require major revisions.

General Comments:

The manuscript reports on the intercomparison of water vapour column and profile measurements observed by a suite of different instruments operated at two sites in the Eureka area:

1) The paper reports a "good agreement between all datasets with correlation coeffi-

cients > 0.9". I have two issues with this statement: i) How good is good enough? ii) Is the correlation coefficient the right quantity to judge good agreement? For i), the authors should look at the requirements for essential climate variables, the accuracies required to detect trends (see Serreze et al., 2012, JGR) and/or importance in terms of the radiative budget (see for example Cox et al., 2015 Nat. Commun.)? Are the data good enough to evaluate atmospheric reanalysis (see Perro et al., 2016, AMT)? The requirements feed back to the question which measure is the right one to judge agreement among data sets. Often a combination of absolute and relative values is used value. This is especially important for low column water vapour (PWV) values there percentwise differences can become rather large. For instruments that have a certain detection limit, e.g. microwave radiometer, sun photometer, very large relative errors can occur. Another reason why in addition to the relative error and the correlation coefficient also the root mean square difference (RMS) should be used is the sensitivity of the correlation coefficient to the PWV spread. As the sample size and thus PWV spread varies in pairwise comparisons, the values achieved for different instrument comparisons are not comparable.

2) The focus of the paper is sometimes confusing. First, I thought the paper should introduce water vapour measurements in the Arctic in general but became puzzled in the introduction with a rather limited list of satellites omitting the most used microwave sounders that can also penetrate clouds. Next, emphasis was put on the MUSICA FTIR processing not even mentioning the measurement principle of solar absorption. As a side note I find the branding of MUSICA a bit overdone and would prefer a more generic naming convention as it is done for the other instruments. Then at the end of the introduction the new thermal infrared measurements are mentioned as a novelty in the outline. Therefore, the introduction section (p3l17 to P4l2) needs to clarify the overall goal.

3) The readability of the paper is poor due its structure that goes three times through the series of instruments (2. Instrumentation, 3. Comparison, 4. Discussion) and switches

between the two sites and column & profile. I strongly recommend the authors to move the profile comparison into an appendix and concentrate on PWV. Anyhow only FTIR and radiosonde profiles are compared although also AERI has profiling capability. Profiling might be even an opportunity for a second paper that also investigates the interesting question is to what degree remote sensing can observe moisture inversions in the Arctic (see Serezze et al., 2012).

4) The paper does not explicitly mention the problem of cloud sensitivity that prevents measurements from several instruments during long time periods. What are the implications then trying to derive trends? What is the percentage of cloudy cases? Sometimes only 64 coincidences where found. Will this give a significant result?

5) Altitude difference: Measurements at two different sites 15 km and 610 altitude meters apart are used. On page 5 it is mentioned that this offers "an opportunity to investigate the effectiveness of different measurement techniques" but this is not addressed properly and no conclusions can be found in the manuscript. In fact, Fig. 17 shows the high correlation of 0.97 between sun photometers at the two sites indicating no strong differences. The authors need correct the altitude difference before doing any comparisons. This can easily be done by taking the mean of the absolute humidity (ground measurement) at both sites and calculating the hypothetical column to be added to the higher altitude site.

6) The microwave radiometer time series needs to be stopped in 2009 as the last calibration (inspection) was performed in 2008. A calibration needs to be performed roughly every 6 months – maybe up to a year. The comparisons with other instruments clearly show that the data are completely off in later years. No conclusions can be drawn from these data.

7) I am not convinced that all 20 figures are really needed to convey the message of the paper (see specific comments). In fact the conclusion section is rather weak. What are the major results? The paper started showing some climatology from radiosondes

– so which additional information is provided by the ground-based remote sensing measurements? The AERI data are recommend due to their sampling – the data set can be used to study sampling issues. What products do you recommend a potential user except "to be explored"?Are the data good enough to look at longwave downward feedback? How does the water vapor climatology compare to Barrow (cf. Perro et al., 2016)?

8) Several minor error like missing references occur. I also hope that it is a typo on page 12, line 20 that says that relative humidity is converted to mixing ratio (dimensionless) and then integrated and converted to PWV. PWV is the vertical integral of absolute humidity (kg/mˆ3) so mixing ratio needs to multiplied with air density to give absolute humidity and then integrated.

Specific Comments:

Title: Mention Eureka as the regional focus is limited

P3l4 Reference Soden et al missing in reference list

P3l14: say that WV trend from Lesins is derived by radiosonde. I am a bit reluctant of the quality of 1960's 1970's radiosonde. Would be good to also refer with more detail to the work by Serreze et al. (2012)

P3l23, Say clearly that Eureka is no GRUAN station

P4l15: Reference Adams et al., 2012 is missing

P5l20: This paragraph is disconnected. Why is there a need to mention water isotopes if nothing is done with them within the paper.

P6l5: Nothing has been said about the emission FTIR (AERI) before

P6l18: The single sentence as no meaning and should be omitted.

P7l3: Paper by Rodgers (2000) not mentioned

P8l16: What is the standard software? Manufacture, reference?

P9l14: It is not true that 10 km is the border when radiosondes can't measure humidity accurately any more. Probably you mean the stratosphere but the tropopause height varies quite a bit so this level should depend on the actual sounding.

P9l14: Because radiosondings are also in cloudy sky conditions you're a priori will have a moist bias.

P9l22: After developing atmospheric profiles sounds odd: better stick to constructing P10l2- how large are the uncertainties in percent?

P10l20-29: I think I understand what you want to say but it is really difficult to follow. Somehow the issue of vertical (layers) and temporal interpolation become mixed. I suggest to revise this carefully and also use paragraphs to separate different thoughts, for example before the "forward model". Here a sentence to introduce its purpose would be helpful. Add spectral before resolution when you talk about matching.

P11l14 and all instruments: you give a bias error but what about random uncertainty?

P11l20: "in most conditions" -> during non-precipitating clouds. You mention short-term variability but you us e 30 min averages – see Steinke et al. 2015 ACP.

P11l27: There are also radiosondes which measure dewpoint temperature. P12l5: Before you said that the radiosonde can't measure humidity above 10 km but now you say that 99.4 is below 15 km. Better give value for 10 km.

P12l10: Better give uncertainty due to rounding in absolute humidity to relate it to humidity.

P12l19: One sentence does not make a paragraph!

P12l19: Important: One sentence does not make a paragraph!

P12l22: No single subsection 2.4.1 without 2.4.2. Please say how many radiosondes

are processed according to GRUAN. Why not more?

P13l1: What does close mean? If you say that 64 km are close when I argue that the difference between both sites (15 km) is negligible.

P13l3: section 2.5. Very short: Maybe add some information on WV conditions. What is the impact on sampling due to instrument limitations.

P14l10: In order to decide what coincidence criteria to use one should not look at quality of the intercomparison but at the time scales of water vapour variability at the site (e.g. autocorrelation), e.g. from high resolution, continuous time series or reanalysis. Twelve hours for sure is too long. . .cf Steinke et al., 2015, www.atmos-chem-phys.net/15/2675/2015)

P14l25: warm atmospheric temperatures do not "hold much greater quantities".. With same relative humidity the atmosphere contains more water vapour.

The pages 15-17 are rather lengthy descriptions listing number from figures and tables.

P18l9: What is excellent correlation?

P18l18: "demonstrates significant influence of altitude " : you need to correct for that! The correlation is 0.97 for the sun photometer – so not too bad.

P19l26: You argue that the bias is not to bad for a global data set but this depends what you want to do with it. . .

P19l29: Seasonal pattern? I guess it is more a dependence on PWV itself and temperature.

P20l17: The radiosondes drifts away and the MWR looks at zenith!

P21l9: The statement does not make sense. Without an altitude correction you can't compare measurements at two different altitude sites.

P26l5: what does cited 2011 mean?

P28l24: latex commands

Table1: Make clear that GRUAN is a subset of the radiosndes. Why not more?

Table2: What type of accuracy, e.g. absolute, relative, bias? Use same order as in the ordering of the sections. Why here MUSICA and not 125HR?

Table3: Why did you use the smoothed RS for calculating columns? Wha is it not mentioned in the text but in the table caption? Explanation for the entroes in the table is missing. I can only guess what is what? Otherwise I like the table – why not do the same for the other site?

Table4 – does not make any sense without correction for altitude difference. Here you describe the different values but not in table 3.

Fig2: horizontal lines could be integrated in Fig.1 and then the figure could be omitted.

Fig3: add mean and std of each data set

Fig4. Shows typical PWV variability – what are you aiming at? If you want to perform a climatological analysis you should use all the different instruments. You could for example also show the mean values for interannual variability. How does the frequency of occurrence look like. For what is currently shown in the paper the figure is not necessary.

Fig. 5 but especially Fig. 6 are not necessary

Fig. 10: I do not see the value of that graph – the table already contains all the data and the temporal distribution is already shown in Fig. 9.

Fig.14: What happens in the lowest kilometre? This is not described in the text. Maybe I don't understand the most right figure as sigma is not explained but a standard deviation of more than 100% - actually above 2000% in the plot is not tolerable. . .or di you mean ppmv? ..also some other figures not necessary ..

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