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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Significant decline of mesospheric water vapor at the NDACC site Bern in the period 2007 to 2018" *by* Martin Lainer et al.

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Dear authors,

please find below the comments and suggestions for improvements by referee 1:

The study presents a trend analysis of the 10-year data set of middle atmospheric water vapor measured by a ground based microwave radiometer. It is emphasized that the measurement does not show any drifts despite some hardware upgrades and changes in the calibration cycle. Significant trends are found in the mesosphere and upper stratosphere.

The paper addresses an important topic and makes potentially a valuable contribution. However, major revisions are needed to present the necessary evidence, that the measurement does not show significant drifts. The analysis of the baseline is not convincing since it is rather an analysis of the noise instead of the baseline. Changes in the noise level are evident and its consequences on the measurement not sufficiently discussed. The data set does not seem to be homogenized. Despite a dynamic integration scheme to keep the noise level constant there are important variations in the measurement response. The test of the stability of the averaging kernels is over stressed and does not prove that there is no drift in the measurement.

Specific comments:
The Introduction is focused on troposphere and stratosphere but the main results are in the mesosphere. Please adapt the focus of the introduction.

P4/l6: this is not the SNR but the noise.

P4/l14: with 80 MHz bandwith there is no sensitivity at 10 hPa. Hence the difference simply refers to the difference between MLS climatology (a priori) and MLS measurements. Further, it would be interesting to know, if the bias of 10
P4/l16: Figure 1 does not show anything about the stability of the baseline but only about the evolution of the measurement noise. To demonstrate the stability, please show the annual averages of the residuals instead.

The annual cycle that is visible in Figure 1 contradicts the statement on p4/l6 that the variable integration time ensures a constant noise level. The explanation given on p5/l1 do not apply since the variable integration time should account for all such effects. Further, it is not precise to say the SNR is constant, since if the noise is kept constant with dynamic integration, the line strength can still vary and modify the SNR. The change in pattern after 2016 have to be discussed as well.

P5/l2: I do not agree with the statement that such changes do not affect the retrieval. Changes in the measurement noise affect the sensitivity of the retrieval and can in turn affect the trend analysis.

P5/l14: Why does the white line show such a pronounced seasonal cycle if the measurement error is supposed to be kept constant?

P5/l21: if the observational error is essentially a statistical error, should it not decrease when calculating the monthly mean? Hence why is not $\sigma_{obs}/sqrt(N)$ the correct value?

P5/l29: For dataset shorter than one solar cycle, the solar cycle (SC) term can be highly correlated with the linear term and should be avoided. Are the SC and the linear term correlated in this trend study? What would be the trend results without the SC proxy?

P6/l29: the test with the AVK is much appreciated and a very good indication for the good quality of the data set. However, it does not prove, that there is no shift/steps in the data set which could arise for instance form a drift in frequency. Please comment.

P8/l5: positive drift detected by Hurst et al on MLS are at a pressure levels of greater than 20 hPa. The authors cannot justify the difference between MIAWARA and MLS trends in the low mesosphere by this drift

P8/l11: I do not agree:
- Stability of the baseline has not been shown, see comment above.
- It has been shown, that the actual AVKs do not introduce a drift. It has not been shown that the instrument has no drift. More evidence would be helpful to convince

the reader.

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