Review of revised manuscript by Lainer et al. on “Significant decline of mesospheric water vapor at the NDACC site Bern in the period 2007 to 2018”

Neither the response nor the revisions in the manuscript are satisfying. The response is subjective and lacks scientific argumentation in various places (see examples below). The revisions are minimal and do not respond to my main criticism. I recommend to consider acceptance of the manuscript after a major revisions addressing properly the points raised in my first and second review. I came to this recommendation for two reasons: as laid out in the introduction, only few studies exist on mesospheric water vapor trends. Second, I believe the authors are capable to address the open points properly and to provide the necessary sound discussion.

General comments:

The authors present the residuals from the retrievals which show a step and then a periodic pattern. The authors only speculate where the oscillations could come from (p5/l14 The pattern is likely …). Further the speculative explanation is obviously wrong: Neither temperature fluctuations of the absorbers nor tropospheric attenuation could introduce a change in the noise level of the residuals, since the noise level is kept constant with a dynamic integration scheme (p4/l10).

The 80% measurement response contour shows significant variability, which is not a good sign if trends shall be analyzed. Despite my criticism in the first review, this issue is not discussed in the revised paper.

I consider these two points crucial for a trend study and both must be fully addressed and explained by the authors.

Specific comments on the authors response:

“The analysis of the baseline as stated in the manuscript is indeed at first instance an analysis of the measurement noise. However, indirectly we show the good stability of the baseline fitting in the retrieval algorithms.
If you want to discuss the baseline, why do you present results that are at first instance an analysis of the noise and only indirectly show the stability of the baseline? My suggestion to show annually averaged residuals is completely ignored.

The changes in noise patterns are visible in the 3-dimensional view, but very tiny and would not be recognizable in a 2-dimensional plot looking from above.
Are you really telling me, that I should not worry about all the structure in the residuals because I would not recognize them if plotted in 2D? This is an outstanding lack of scientific argumentation. The phrase on p5/l14 can impossibly appear in a scientific publication.

We do not see any severe changes in noise levels, only small patterns originating either from temperature fluctuations or changes in tropospheric attenuation of the line signal or a combination of both.”
“not severe” and “only small” is subjective and qualitative and not convincing.

Homogenization would have been necessary if for example a replacement of the spectrometer would have taken place. But did not. Only adjusting the measurement cycle and installing a faster mirror motor does not imply to do a homogenization.

This is again very subjective. The answer whether or not a homogenization is required is given by the data itself. Numerous tests can be found in the literature.

The periodically variations of the measurement response (Fig. 1) originate from the seasonal variability of the H2O line strength. We note that these changes in the measurement response do not seem to be important for the trend, because the a priori (MLS) information does not have any trend.

The variations in measurement response are not addressed in the revised paper. If the line strength was the origin of the variations in measurement response, wouldn't we expect to have a higher measurement response in summer when there is more water vapor, always keeping in mind, that the noise level is kept constant by dynamic integration? But Fig 2 shows the opposite.

We think the AVK test is the best way to show the stability of the water vapor measurements. Any important drift of the measurements would be reflected in the AVK development.

This is lacking scientific argumentation. I acknowledge your expertise in the field, but to simply tell me that you think this is the best way and to claim all kind of measurement drifts would be seen in the AVK does not convince me. The only ingredients of the AVK are the Jacobian and the covariance matrices and all except the Jacobian are constant in time, since you apply a dynamic integration scheme and keep the noise at 0.01 K. What about a time series of the receiver temperature, monthly or annual averages of the residuals (would show frequency shifts), …

We think that some kind of broader introduction to the topic is useful for the reader. Therefor we would like to keep the information on the upper troposphere and stratosphere.

I did not criticize the broadness of the introduction but the fact that a discussion of the mesosphere is missing and I still think I have a point. I am astonished by the extent to which the authors ignore my comments.