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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climatology of middle atmospheric water vapour above the ALOMAR observatory in northern Norway" *by* K. Hallgren et al.

Anonymous Referee #3

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This manuscript addresses 3 topics on water vapor over ALOMAR. 1) As indicated in the title, there is discussion of a climatology. 2) There is a discussion of trends. 3) There is a short discussion of Sudden Stratospheric Warmings. The authors have an interesting and unique set of measurements, and my hope is that they will in the future submit manuscripts addressing one or more of these topics. Unfortunately in this manuscripts all 3 topics are very inadequately addressed, and I think that the further work that is required is beyond that normally performed under a major revision.

1) It is certainly good that this dataset is made available, but the statement [31534; line 25]: "The authors have identified a lack of accessible and updated reference profiles

of middle atmospheric water vapour, especially concerning the polar latitudes." is quite strange. The Aura MLS instrument has provided twice daily measurements at the latitude and altitude of these measurements since 2004 [Lambert et al., 2007], and this publicly available dataset can certainly be used by anyone who wishes to establish a reference atmosphere. Given the authors concern about a lack of reference profiles, it is surprising that the MLS instrument is never mentioned in this manuscript.

2) The authors do have some short discussion of instrumental changes, but make no effort to link this to the large observed geophysical variations. If the authors wish to discuss trends, then they must make some effort to show that the large geophysical variations observed are not related to instrumental changes (Table 1). Again, some effort to validate against publicly available satellite (MLS, HALOE) data sources would be invaluable here.

3) The discussion of SSW is too short and lacking in figures to make much sense to me, but there is certainly interesting material here.

More detailed, but in some cases still quite serious, comments are below:

The authors quote their resolution as [31573; line 1]: "7–10 km (FWHM of the averaging kernels) (Hartogh et al., 2010; Hallgren and Hartogh, 2012)". This number is consistent with that quoted in the 2010 paper, but is inconsistent with the averaging kernels shown in the 2012 paper. Measuring these kernels by hand shows them to be typically $\sim$14km FWHM.

"It is therefore troubling that no conclusive results of the long-term evolution of water vapour exist." – I don't understand exactly what this statement means. This whole paragraph is extremely confusing and poorly written. There are certainly studies of the long-term evolution of water vapor, e.g. Hurst et al. [2011] which analyzes 30 years of data. What Garcia et al. [2007] point out that: "water vapor has various sources of low-frequency variability (heating due to volcanic eruptions, the quasi-biennial oscillation and El NinËIJo–Southern Oscillation) that can confound the determination of secular

trends." I don't think that Garcia et al. make any statement which [31534; line 10]: "suggest possible solutions to the problem".

It is not clear from Figure 1 which of the instruments mentioned in Table 1 is providing the data. This makes it extremely difficult to interpret the jumps in the data, some of which are larger than one would expect from geophysical variations.

The large drop which occurs after a gap in measurements in late 2000 is associated with the drop in water vapor observed at lower latitudes and lower altitudes during this period. This is seemingly inconsistent with the much smaller decrease observed at these altitudes but at lower latitudes [Nedoluha et al., 2009]. If the authors do think that this is a real geophysical drop in water vapor they need to explain the gap in the data that occurs before this drop. They should also validate the result by comparing with nearby HALOE measurements.

Presumably the solar cycle is relevant to these measurements, yet no attempt is made here to explain which of the variations in Figure 1 are related to the solar cycle.

The discussion of CH4 effects is completely confused. Anthropogenic CH4 variations, even during periods of relatively rapid increase such as the early 1990s, are too small to have any noticeable effect on Figure 1. This discussion [31540; line 2 onwards] should either be eliminated, or the authors must make some quantitative statements comparing these CH4 variations to the variations in H2O which they measure. There are changes in transport that can affect the CH4 oxidation which a parcel has experienced, and therefore will affect H2O in that parcel, but this discussion is presently convoluted with the surface level variations in a way which makes it completely unhelpful.

[31541] The discussion of SSW here is very hard to follow given that the reader is asked to compare, on a plot showing $\sim$14 years of data, 2 rather short timescale events. This discussion must either be accompanied by some figures (including perhaps one showing temperature), or dropped altogether.

References:

Hurst, D. F., et al. (2011), Stratospheric water vapor trends over Boulder, Colorado: Analysis of the 30 year Boulder record, J. Geophys. Res., 116, D02306, doi:10.1029/2010JD015065.

Lambert, A., et al. (2007), Validation of the Aura Microwave Limb Sounder middle atmosphere water vapor and nitrous oxide measurements, J. Geophys. Res., 112, D24S36, doi:10.1029/2007JD008724.

Nedoluha, G. E., et al. (2009), Water vapor measurements in the mesosphere from Mauna Loa over solar cycle 23, J. Geophys. Res., 114, D23303, doi:10.1029/2009JD012504.

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