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Atmospheric Measurement Techniques Discussions

Interactive comment on "A new microwave spectrometer for ground-based observations of water vapour" *by* K. Hallgren et al.

Anonymous Referee #1

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Comments on "A new microwave spectrometer for ground-based observations of water vapour" by Hallgren et al.

This paper gives a description of a microwave radiometer called cWASPAM3. This instrument is cooled in order to reduce the noise contribution of the microwave amplifiers at 22 GHz what leads to a reduced receiver noise temperature. The authors claim that "the low receiver temperature ensures a time resolution of an order of magnitude better than what has been achieved by earlier instruments". Error sources are discussed and a set of 70 measured spectra of the ground-based instrument are compared to MLS data for a time period in 2009 during the ARIS campaign. The authors say that the results "are in good agreement with each other".

I have two main problems with this paper:

A) Most of the content of this paper has already been published by the same authors as part of a paper in AMT by Straub et al., 2009 where a description of the ARIS campaign is given and where cWASPAM3 was part of. Table 1 of the present paper is just a subset of Table 1 in Straub et al. Figure 4 is part of Figure 1 in Straub et al. Figure 5 is the same as Figure 5 in Straub et al., with the exception that averaging kernels are given on a kilometer grid in stead of pressure altitude. Figure 6 gives the same information as Figure 8 in Straub et al. Figure 7 gives the same information as Figure 10 in Straub et al. Only Figure 1 giving a block diagram is new and the two photographs of the system in Figure 2 and 3 are new. Also some text blocks are more or less identical to Straub et al.

B) I could not find any proof of the statement made in the abstract that "the low receiver temperature ensures a time resolution of an order of magnitude better than what has been achieved by earlier instruments". Worldwide approx. half a dozen microwave radiometers are operated on a regular basis for the detection of water vapour in the stratosphere and mesosphere. Many of them are operated in the frame of NDACC. All these instruments have been discussed in the literature e.g. by Forkman et al. 2003, Nedoluha et al., 1997, 1998, Deuber et al., 2004, DeWachter et al. 2011, Gomez et al., 2012. If the authors claim that their instrument is an order of magnitude better than earlier instruments they at least should give a summary of what other instruments have achieved. But unfortunately no such information is given that would justify the statement made.

For these two reasons I cannot recommend this paper for publication. It has to be completely revised and resubmitted.

In addition to the two main concerns given above I would like to make a few more comments: I have no doubts that cWASPAM is a very sensitive and sophisticated instrument. Previous work by the team showed excellent results of atmospheric studies

by similar instruments. But as this paper is aimed at showing the high potential of cWASPAM3 a lot more information is needed.

- The authors should state in much more detail what they mean by high sensitivity. Further they should be in a position to show how stable the instrument is (stability and bias of calibration loads, long term drifts, etc.) after some years of operation. By now the authors should have a data set extending over several years since the ARIS campaign. How is this data set comparing with other observations from MLS (which version?). There should be many more spectra available, not only 70. Much more weight has to be put on such an inter comparison including a proper statistics. (Figure 7 does not even show a standard deviation). It is not sufficient just to say that data are in good agreement with each other.

- In a new paper it further is recommended that the authors are more specific about the retrieval process (covariance, measurement response, altitude sensitivity etc.), give more details about the used spectroscopic parameters and give a description of the tropospheric correction. In the present paper this is not discussed at all.

- The authors say that the Backus-Gilbert retrieval might be better than the Optimal Estimation Technique in case minimal noise is a criterion but give no justification or explanation of this.

- In a future paper the authors also might like to give some more details about the measuring technique and give more details about the specific aspects of this new instrument, e.g. what is the effect of a cooled horn, difference between silver-rhodium coated mirror to aluminum mirror, details about the cooled calibration loads such as material, temperature monitoring, emissivity etc.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 4677, 2013.