

Interactive comment on “Investigation of ground-based microwave radiometer calibration techniques at 530 hPa” by G. Maschwitz et al.

Anonymous Referee #1

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The manuscript presents a comprehensive error analysis of two commonly used calibration techniques for microwave radiometers, that is the calibration against liquid Nitrogen and the tipping curve calibration. Theoretical considerations are then applied to measurements that have been obtained by the HATPRO-G2 radiometer as part of a campaign in Atacama between August and October 2009.

While the error assessment is rather extensive and definitely appropriate for the measurements it is not novel by itself so that it would justify a publication. The interesting part therefore would be to apply this analysis on a number of measurements under different conditions. The authors present one liquid Nitrogen calibration measurement of one day and compare it to a tipping curve calibration of another single day. This is a pity since much more calibrations have been performed during the campaign under

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a three months' period, covering a wider range of tropospheric conditions that would affect the tipping curve calibrations. This is even described in the outlook paragraph and leaves me to wonder why the authors did not use more, if not all, of the calibration measurements from the campaign. The authors neither considered the Jülich LN2 measurements which could at least contribute some more evidence of repeatability of their calibration study, especially when looking at the effects on the calibration caused by uncertainties in the LN2 refractive index, the resonance effect due to LN2 levels, and trumental non-linearity.

Here follow some more detailed comments on the manuscript:

- Under paragraph 2 probably a numbering is missing for the subsection 'Microwave Radiometer. Here the authors mention observations at elevation angles of 9.6 and even 4.8 degrees. At least 4.8 degrees seems to be pretty low with respect to the antenna opening angle (2 HPBW) of 7 degrees. For good reasons they performed their tipping curve calibration measurements at elevation angles 30 degrees and higher up. An opening angle of 7 degrees would strongly affect low elevation measurements because of the increasing variability of the atmosphere. Different elevation angles for tipping curve calibration are given in section 3.2 and 4.2.

- I suggest the authors describe the 4-point calibration using the internal noise diode a bit more in detail (or present a reference). Unfortunately they present only one such LN2 calibration for this study and leave it to the reader to interpret this as either 'one-fits-all' or 'no further LN2 calibrations could be performed'. I understand that the calibration measurements during the campaign have been performed with the internal noise diode after the initial LN2 calibration instead of using LN2 calibrations continuously, which would have given a much larger base for this study.

- The authors refer to Table 4 frequently, however, I cannot find the numbers in the table as stated in the text. This needs clarification. When giving a number in the text with a reference to a table then I would expect the number to show up in the table. See

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for instance line 664 ff where neither of the mentioned numbers 0.2 or 0.5 shows up in Table 4.

- Furthermore the caption of Table 4 mentions a variable 'air' which does not show up in the table. The same seems to be the case with the variable TN mentioned in the caption but missing in the table in Table 2.

- In Figure 8 the authors argue that the larger spread of the opacity-air mass-correlations for frequencies closer to the water vapor line center is due to the stronger inhomogeneities of atmospheric water vapor closer to the line center. I would agree with that. However, this is not reflected in the V-band cases being far away from any line center. If one would expect a difference there according to the argument above, then the 51.26 GHz channel should have less spread than the 52.28 GHz channel. What could be the reason for the larger spread in the 51.26 GHz channel?

- In the final comparative assessment the authors compare TB measurements deduced from tipping curve measurements with those TB values deduced from basically the noise diode measurements. They assume a stable behavior of the noise diodes over the time between the LN2 calibration on August 11 and the tipping curve measurements on August 16. Without presenting the Jülich LN2 calibration measurements regarding the stability of this method I would have my doubts on whether this comparison of calibration methods is justified.

- Since the authors mention one principle problem for radiometers, standing waves, I would like to know how they deal with it. Using discontinuous channels in a filterbank always contains the risk of undiscovered standing waves that affect the measurements. I don't understand their comment in this context '... the assessed uncertainty within this study is too small'. Standing waves might also contribute to the observed resonance effect discussed in section 4.1.2 (if not a numbering for this subsection is forgotten here). For me this would be more convincing than that the horn antennas and amplifiers are optimized for the center frequencies in the respective bands and therefore

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produce some kind of higher amplitude there.

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