

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

Anonymous Referee #3

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This manuscript provides a very detailed, mostly theoretical error analysis for the HATPRO-G2 instrument while deployed in Atacama. The measurements were conducted as part of a campaign conducted between August and October 2009.

The results and conclusions of the entire paper hinge upon a single calibration performed at Atacama on 11 August 2009 and a comparison to a series of tipping curves from a single day on 16 August 2009. Why are the tipping curves only shown for a single day? Repeatability under different conditions (presumably there is some variation τ over the 3 month campaign) is a key here as the authors admit, yet they fail to address this issue. Was there a problem with the instrument on all other days?

While I understand that there are probably difficulties with returning to Atacama to perform additional calibrations, I do not understand why the authors do not make full use

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of 28 calibrations which were performed at Julich and which would give some indication of the repeatability of their calibration technique. Again, without some indication of repeatability it is not clear what to make of this study.

The error analysis on its own is reasonable and certainly appropriate for a measurement study. If there were more measurements shown this could be an interesting publication. But, given the small number of measurements shown, the error analysis is not sufficiently novel to warrant publication. Almost all of these points have already been addressed in previous studies such as Han and Westwater (which the authors do appropriately repeatedly reference).

Without some evidence of repeatability, the results will either be ignored or they might be used to form incorrect conclusions.

Some more detailed comments, separated by Section number, are given below.

3.3 “TN is stable enough to be used as a secondary calibration standard for several months.” Is there a plot that shows this? Or a reference?

4.1 There is a lengthy theoretical discussion of possible problems with the cold calibration. Reflections from the surface of the liquid nitrogen may cause large errors, but there are other issue not discussed such as the condensation which can typically occur over a cold load. While the discussion is perhaps not unreasonable, all of the results (with the exception of some discussion on non-linearities) seem to be based upon a single calibration. Without any evidence of repeatability this whole section is deeply flawed. The authors themselves do present a very nice idea for a study of calibrations as the LN2 evaporates, but instead of actually doing the study they merely state: “Therefore, a more practical solution is to determine the calibration parameters from repeated calibrations while the LN2 evaporates.” This is a very nice idea, and if the results from this were shown in this paper I would not hesitate to recommend publication.

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Surprisingly, in 4.1.4, the authors state “The variability in alpha is investigated by 28 LN2 calibrations that were performed with HATPRO-G2 at the Research Center J  lich, Germany between July 2010 and November 2011.” These calibrations need to be studied to address the issues brought up in 4.1.1, 4.1.2., and 4.1.3? There is no need to do those parts of the study at 530hPa.

“For both receiver bands, amplitudes in the band’s center show higher amplitude, because the horn antennas and amplifiers are optimized to the central frequency.” – It seems unlikely that the either the antenna or the amplifier optimized over such a narrow range as to cause this effect.

4.2 The problems listed in this section are problems which any MWR study has had to address, and almost every subsection begins with a reference to Han and Westwater. It might be justifiable to publish these sections if it were presented in such a way as to be of some use to others. However, the errors calculated in Section 4.2 and the various subsections (4.2.1, 4.2.2, 4.2.3, 4.2.4) are all given as T_b for a very specific case, which makes it impossible to extrapolate the results to anything other than measurements at precisely these frequencies for precisely this atmospheric state. Can’t all of these errors just be expressed as $x \cdot T_{mr}(1 - \exp(-\tau))$, where x is some calculated error. This would also help to eliminate the repeated statement “has no effect on K-band and XXK effect on V-band”, which, I think, is really just a statement of the relative optical depths in those 2 bands.

Figure 6 – Isn’t this slope just given by $(1 - \exp(-\tau))$? I don’t think that there is any need to plot this.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 989, 2013.

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