Atmos. Meas. Tech. Discuss., 6, C506–C510, 2013
www.atmos-meas-tech-discuss.net/6/C506/2013/

Atmospheric Measurement Techniques Discussions

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

Anonymous Referee #2

Received and published: 5 April 2013

The paper "Investigation of Ground-Based Microwave Radiometer Calibration Techniques at 530hPa" is a detailed discussion of errors in different calibration methods for microwave radiometers. The paper is relevant for the microwave remote sensing community and fits well to the scope of AMT. For these reasons I can recommend to publish it after some corrections.

There is one major error in Section 3, Equation (1): The temperature in the Planck equation is not the "black body equivalent brightness temperature Tb", but the thermodynamic temperature T. See e.g. Eq. (3) in [Han+Westwater 2000]. The statement "Note that in case of a black body, Tb is equal to the physical temperature of the object" is also incorrect. Even for an ideal black body this is just an approximation, and its validity depends on temperature and frequency. The paper of Han+Westwater gives an

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

in depth discussion of different definitions of the brightness temperature, i.e. Rayleigh-Jeans or thermodynamic Tb. The authors should mention which approximation they will follow in the rest of the paper, and double check that they use it consistently.

Further comments: The abstract states estimated uncertainties of 0.2-0.4K for the tipping calibration and 0.5-0.9K for the LN2 calibration, and that "Systematic offsets, which may cause the disagreement of both methods within their estimated uncertainties, are discussed". However, one of the main findings of this study seems to be that the two calibration schemes do not agree within their uncertainties. This disagreement should be more clearly in the abstract, i.e. by giving the remaining bias after the obvious corrections of LN2 boiling point and pointing.

Section 3.1: The LN2 target is described as "...mounted on the radiometer for calibration. The load is filled with egg carton shaped styrofoam to improve the target's black body properties." Since this mounting can have some effect on the observed calibration bias it should be described in more detail, e.g whether the LN2 target is placed alongside of HATPRO and viewed via a 45deg reflector as described in the HATPRO manual. The load is presumably not filled with "egg carton shaped styrofoam", but with an microwave absorber (e.g. carbon loaded PU foam)

Section 3.2: According to section 3.2 tipping curve calibrations have been made every 6h with the manufacturer's default angles (30,33.3, 38.4, 45.6 56.4 and 90deg) towards 50deg N azimuth. In section 4.2, however, the continuous tipping observations at 90, 45, 30, 15 deg elevation were used for this study at 70 and 250deg azimuth were used for calibration. Which statement is correct, and if both scanning schemes were used, how well do they agree with each other?

Section 3.3: It is stated that "TN is stable enough to be used as a secondary calibration standard for several months". It might be useful to provide some numbers for the TN stability from repeated calibrations over extended periods (even if this was outside of the RHUBC campaign).

AMTD

6, C506–C510, 2013

Is the the sentence "Using TR from a previous calibration gives a corrected detector gain g" correct, or should it read TN? It is not made very clear in the text that g is determined from switching the noise diode on and off,

Section 4.1 It should be made clear that Eq. 10 refers only to the normal incidence reflections at the LN2 interface of the cold load. The current wording of this paragraph is more general ("when pointing to a calibration target"), but e.g. for the convoluted foam absorber Eq. 10 cannot be used.

Section 4.1.1 Misplaced semicolon after Eq. 12. At the end of the section is an empty pair of brackets (), to be either removed or replaced by (NIST).

Section 4.1.2 Erroneous citations in the sentence "The results are in agreement with several other experiments at frequencies between 0.5GHz (Shitov et al., 2011) and 516GHz (Vinogradov et al., 1967)". The Vinogradov provides measurement data at lambda=2.3mm. Shitov's experiment operates at 800GHz, but he did not determine n of LN2 and only cites [Hosking et al 1993] with the 0.5-10GHz data.

The theory behind the standing wave errors is not explained very well, and the meaning of the equation " res(s)(res(s(t))=0)" before Eq. 15 is not clear to me. I'd recommend to read and cite e.g. J. Randa et al, "Errors resulting from the reflectivity of calibration targets," IEEE Transactions on Geoscience and Remote Sensing, vol. 43, no. 1, pp. 50–58, Jan. 2005.

The time varying standing wave error is estimated with 0.6K at 23.04GHz, and less for other channels. However, the authors state that they cannot integrate over one or more oscillation periods of the resonance because the load was not observed long enough. They should clarify whether this estimate is based on further measurements outside of the RHUBC campaign, or whether the LN2 target was observed long enough during the campaign to see the variability.

It is a pity that the authors do not show more results from LN2 calibrations over ex-

AMTD

6, C506–C510, 2013

tended time periods (e.g. during the tests at Juelich). This would help to give a better understanding of the errors which occurred in the single short-term LN2 calibration of 11.8.2009.

Section 4.1.3 Apparently an uncertainty of alpha affects the error on Tb. It should be made clear whether this error depends on the type of calibration, i.e. whether it is the same for LN2, tipping curve or noise diode calibrated data.

Section 4.2.2: Probably the first sentence "The tipping curve procedure uses Tb observations to derive opacities at different air mass values" is worded misleadingly. I understood that only the detector voltages are measured at different airmasses, and that this information is used to derive opacities and Tb.

Section 5.1 Line 816: "In general, for different channels the overall uncertainty is between 0:6K and 2:7K". These values do not correspond to the ones in Tab. 4, and also Fig. 4 is inconsistent with them and Tab. 4. There is also missing ")" in that line.

References: Currently the references appear in random order, and not alphabetically.

Fig 1: The V-Band spectra at 530hPa show spectral line features which are not resolved by the broadband filters at the higher frequencies. At normal pressure where the atmosphere becomes opaque this will not be an issue, but at 530hPa it should be necessary to convolve the precise channel response function with the spectrum. How has this been achieved in the present analysis?

Table 2: The caption refers also to the noise diode temperature TN, which is not shown in the table.

Table 3: The columns with yes/yes and no/no are a bit confusing, I assume they indicate whether beam width and air mass correction was applied to the data. It is not clear, however, why this correction affects the number of samples which pass the quality check. Also missing ")" in the caption after "Sec. 4.2.5".

AMTD

6, C506–C510, 2013

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

AMTD

6, C506–C510, 2013

Interactive Comment