Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-455-RC1, 2020

Atmospheric Measurement Techniques Discussions
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Interactive comment

Interactive comment on "Measurement Characteristics of an airborne Microwave Temperature Profiler (MTP)" *by* Mareike Kenntner et al.

Anonymous Referee #1

Received and published: 1 March 2020

The paper presents a characterisation of the airborne microwave temperature profiler (MTP) which is flying on the HALO research aircraft. It briefly investigates the spectral and antenna characteristics while the core of the paper is dedicated to identify the best calibration technique. In this way it addresses questions on instrument performance important for users of the airborne measurements and fits well to AMT. However, I see several weaknesses which need to be addressed before publication especially since the authors seem to have not much experiences with microwave radiometry. My main concern is that the different lab measurements/ analyses show little structure and are not clearly connected to the overall uncertainty assessment (see below for details) of the measured brightness temperatures.

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Discussion paper

I agree with the authors that the limitation to measurements space (brightness temperatures, TB) – leaving retrievals out – is sensible. Section 5 which is addressing an optimized scanning strategy for future flights seems unnecessary at this point as the rest of the paper deals with past measurements especially as it neglects several). In light of the already rather lengthy manuscript I suggest to leave this aspect out and instead relate the lab measurements to TB uncertainty using radiative transfer calculations. Furthermore, the authors should use the well established Allan variance technique for assessing the noise characteristics. My concerns are detailed below as well as many instances where the paper needs clarification.

General Comments:

1. The implications of the instrument characterisation for the subsequent interpretation of TB and temperature retrieval are not thoroughly assessed. Section 5 should have a clear outcome on the questions: (i) spectral characteristics: Which are the representative frequencies of the three channels? Which frequencies shall be assumed for the retrieval algorithm? Does the RT have to consider the full bandpass characteristics? (ii) Which is the effect of the antenna bandwidth? Is a pencil beam approach justified? (III) What noise characteristics have to be assumed in the retrieval, e.g. in the measurement covariance matrix?

2. Accurate calibration is the most important task in microwave radiometry. As all components are strongly temperature dependent besides temperature stabilisation a periodic calibration is needed. The calibration might only update the gain of the system (relative calibration) or make an absolute calibration in which all parameters of the raw measurement (count) to TB model are derived. In the simple linear case (as it is used in this manuscript) these are gain and receiver noise temperature Tr which can be derived by pointing the antenna successively to two reference targets. The authors seem to be not aware of this classical microwave formalism which is also apparent as they hardly cite any literature microwave radiometry (list in the back) and some flaws in the radiometer formula application. The major questions which would need to be ad-

dressed are: How good are the reference targets (blackbodies)? How frequently does a calibration need to be made? Why have the measurements in the cold chamber with view on a stable target not been used for such an analysis? The next step would then be the in flight calibration. Assuming that the laboratory calibration (strategy 1) would work is a bit naïve. However, there are good approaches later on using the horizontally pointing measurements but a motivation and explanation why this procedure was chosen needs to come first.

3. The information on the MTP measurement principle is not clearly provided in the beginning of the manuscript making it difficult for the reader to follow. Bits and pieces come together at different instances, e.g. scanning is explained on page 14 and especially the discussion on the use of different oxygen lines is confusing. For better understanding the authors should include a thorough description of the MTP measurement principle in the beginning and add an absorption spectrum (preferably even for different pressure levels as in Fig. 16) to illustrate the frequency channels (and their potential tuning range). This also serves to introduce the double sideband principle. Further, it could be explained why the LO is typically set at center frequency for mitigating problems due to frequency drifts, and how non-resonant emission (water vapor-continuum, hydrometeors) affects the measurement. This would also demonstrate that the LO frequency is not the frequency for which the measured TB is representative (passband averaged – see Fig. 16).

4. Section 5 address future measurement strategies in terms of frequency selection and elevation scanning. This is an important study but is not done as thoroughly as it is needed especially in light of vertical resolution of the retrieved temperature profiles for different types of atmosphere. It also does not take into account the findings of their laboratory measurements in respect to the spectral and spatial sensitivities. As the paper is already very lengthy it should be taken out.

5. At several instances it seems that the authors have gravity wave detection as application in their mind – this is ok but needs to be clearly stated (only abstract). Many

readers might not know which requirements in TB are needed for this purpose. Other users might be more interested in vertical resolution for stability assessment.

6. The readability of the paper needs to be improved - sometimes it is more a technical report than a paper. No clear goals are provided, the structure is not always clear, the text is written rather lengthy and many basic informations only appear rather late in a middle of a section where you would not expect it. Short paragraphs sometimes even only one sentence long occur and the text frequently repeats (unnecessarily) the figure captions, e.g. “Plotted is also a..”. The paper could be shortened by reducing number of figures or using an appendix. I would recommend to concentrate only on the past measurements. The optimized scanning strategy In case but the future – which I think would be an own study if done carefully could go in an appendix.

Specific Comments: Why are brightness temperatures referred to as BT in the text (and Fig. 11) and TB in the equations. Historically the satellite community uses BT and the ground-based community TB. I don’t think it matters which one is chosen but it should be consistent.

P1l8: “records radiances”, no it records counts which are calibrated to brightness temperatures - it is ok to say TB here

P1l9: “state of the atmosphere can be derived” this indicates much more information than the temperature profile which was stated already – what else?

P1l22: “weaker oxygen lines” better write ‘frequency channel’. The LO frequency of the channel does not necessarily need to be at a line center. Also and it seems to me that it is not clear to authors: the LO frequency is not the representative frequency of the channel – and the “representative frequency” can be extracted from their laboratory measurements. I anyway suggest to modify section 5 such that it can provide the necessary input for the retrieval algorithm

P1l22: “calibration parameters do clearly depend on the state of the instrument”. This is the key in microwave radiometry for astronomy, atmospheric, planetary science etc. ever since and for all instruments there is the question how frequently one has to calibrate, e.g. Dicke switching for short-term fluctuations. Unfortunately, even slight vibrations and temperature changes can cause transmission characteristics to change thus calibration parameters. So this sounds a bit naive – I recommend the authors to look more in basic microwave radiometry books, e.g. Janssen, 1994, Vowinkel, 2013, Woodhouse, 2017,

P1l26: Here it should be said that precision is determined for TB which closely relates to the atmospheric temperature when the instrument is pointed horizontally – otherwise it is confusing

P2l16. What is meant by structures?

P3l9-12 and P3l14: There is a very long list of applications of past studies using older versions of the MTP (is that really necessary?) and then it is claimed that instrument characteristics need to be known for correct interpretation. This is true and that's why this study is valid but it somehow implies that the work here also helps with data from old campaigns. This needs to be clarified.

Introduction: the whole introduction is dedicated to the MTP but there is no reference to other studies on the characterisation of other microwave airborne instruments is made, e.g. Blackwell et al, 2001 describing NAST with frequencies 50-57 GHz, McGrawth and Hewison, 2001, Wang et al, 2007 etc. which might also check different instrumental parameters. The introduction clearly needs to mention the goals of the lab investigations.

P4l2: Not all radiometers for temperature profiling measure at the oxygen absorption complex around 60 GHz - also 118 GHz is used. In general, it is surprising that no reference is made to the fact that operational meteorological satellite instruments, e.g. AMSU-A, do temperature sounding since decades. These sounders exploit only the frequency information for profiling while the MTP aims at improving the resolution by angular information. It is necessary to explain the measurement principle here thoroughly, showing a spectrum (ideally for different altitudes) and the considered frequency channels. On a side note: The accuracy of the oxygen spectroscopy is still under debate which is, however, more important for retrievals, Caddedu et al, 2007; Cimini et al, 2018, Maschwitz et al 2013.

P4l2: Why don't you explain the heterodyne principle and talk about a double side band receiver. This is very important to clearly define the frequencies for the radiative transfer used for retrieval development.

P4l18: "making the retrieval of temperature profiles possible" Most instruments only use information on frequency dependence. Make clear that the MTP can achieve higher vertical resolution by adding the angular information.

P4l24: Thermal stabilisation is the most important part in a microwave radiometer the performance of all microwave components strongly depends on temperature. Therefore more details on that are needed.

P4l229: What about temperature stability, homogeneity, spill over of the target, cf. Mc Grawth and Hewison (2001).

P5, l14-15: the discussion on the oxygen spectrum and LO needs further explanation and should come before not in the section on wing-canister, same for the information on the frequency range (l25) below.

P5l22: how large is the gap, x MHz?

P6l 6 "investigation OF calibration"

Section 3. The frequency response of the bandpass is investigated but there is no discussion on the stability of the LO frequency – does this have any potential effect on measured TB?

P6l27: The authors mention the periodicity of the signal first. I understand that for gravity wave detection this is important but in terms of radiometer performance the most important question is whether the instrument follows the radiometer formulae (Eq. 4.8), i.e. noise reduces with increasing integration time. For this purpose typically the Allan variance is used. This characterizes the noise and determines how long measurements can be integrated in time and how frequently a calibration needs to be performed.

Section 3.1: The name is irritating as it could mean much more. The measurements of the bandpass characteristics and the antenna diagram (section 3.1) are important and interesting but are presented rather briefly without any implications for the subsequent retrieval. Even the exact measured bandwidth and beamwidth are not given. For the analysis or implications RT calculation would play a major role. As for example shown in Crewell et al. (2012, their figure 10) the bandpass characteristics can cause the effectively measured TB being representative for a frequency deviating significantly from the specified channel frequency. In fact in the double side band approach this anyway takes place and needs to be handled in the RT underlying the retrieval process. Similarly, the antenna pattern smears out atmospheric features especially at low deviations from the horizontal in a vertically stratified atmosphere (Meunier et al., 2013). To appreciate this laboratory measurements and their impact on the measured TB further analysis is required which would fit well into section 5.

P7l12: "A certain 'waviness' is visible next to this" ripples are typical in any microwave component due to EM wave theory propagation – reducing the amplitude is key.

P7l23: how stable is the noise diode, how much does it depend on temperature (stabilization)?

P8l14: "takes some time to stabilize".. needs to be more quantitative – later it is mentioned but not here

Section 3.2: Information on the accuracy of the target temperatures is missing. P9l14 mentions the "hot" target – should be explained before.

P8l30: I find the term “at all LOs” confusing – also at other instances. Why not write for all frequency channels?

P9l7: Why do the authors not use the classical microwave notation using the gain (cf, Janzen, Mc Grawth and Hewison, ? The difference between receiver and system noise temperature needs to be made clear.

P9l17: Radiometers are never completely stable which is why periodic calibrations have to be made. In between this calibrations the TB could be corrected assuming a linear trend as shown in Fig. 6. The following paragraph describes this for the airborne measurements bit it is unclear for me that for these linear fits segments of 5 min without calibration are used?

P10l1 and following: The spectral analysis is interesting and similar to the Allan variance but is unclear to me why it is applied to atmospheric measurements and not to the cold chamber measurements where the real instrument performance could be tested. The concatenation eliminates real temporal signals. Does the analysis differ between in flight and laboratory measurements .

P10l20-27: “line parameters” is irritating as it could be interpreted in spectral lines: it is about the updating your calibration model, basically, gain and receiver noise temperature. It looks like the authors are not too familiar with typical microwave calibration techniques which is reflected by the lack of citation of microwave radiometer basics and studies. In operational receivers many strategies for that exist (Maschwitz et al., 2013) as typically gain needs to be adjusted more frequently than TR, relative/absolute calibration.

P11lEquation: Why so complicated Trˆ CCh(C_hot) and not simply Tr – explain the meaning of the different indices.

P11l19: Give values to underline the statement

P12l4: The calibration strategies might serve different purposes. That the first strategy leads to comparable results seems astonishing.

P12l12: The cause for the standing waves is the refractive index of the LN2 – here Küchler et al., 2016 should be cited for details. Here it sounds that just the evaporation is the reason

P12l25: Of course the calibration parameters change with changing environmental conditions if the temperature stabilization of the instrument is not perfect. The question to ask if this is repeatable. Would the same parameters be measured if the instrument had been moved and electrically disconnected in between?

P13l29: Why is the temperature unknown – more discussion is needed – see Mc Grawth and Hewison, 2001.

P16l8: why do you explain this only here and not at the beginning of the calibration section

P16l13: Nobody remembers counts better give the atmospheric temperatures and notate the counts with c_min and c_mac or later c_ref instead of 18500.

P16l24: "The vertical, grey shaded.." this is not paper style. The figure should be only a reference for the text.

P17l9: "In literature" then give a reference

P17l9 to 29: This paragraph shows that the authors have not much experience with microwave radiometry. It is weird to present the well established radiometer formula at the end and not in the beginning. The formula describes the internal noise of an ideal radiometer and typically one just writes a proportionality and not an equal sign as other losses occur (e.g. factor 2 for Dicke switching). Further, the authors put in 400 MHz as bandwidth but the double sideband receiver only has 200 MHz in the IF. The most important think to look at the radiometer formula is to check if the noise decreases with longer integration time which is basically what the Allan Variance technique does – it finds out at which point gain fluctuations dominate. This should be checked by the laboratory measurements in the beginning and not in this section. Note, it is strange to only now to provide the integration time for atmospheric measurements.

P18l14: If the dominant uncertainty is the noise couldn't it be reduced by longer integration times?

P18l30: LO frequency

Table 1 does not include all instrument characteristics of interest, e.g. receiver noise temperatures, integration time, polarization. I am missing information on microwave window transmission

Fig. 8 could be combined with Fig. 10

Fig 11: Different calibrations need to be explained in figure caption. Caption does not say how the difference is calculated (what is the reference – the overall mean?). As I do not see significant temporal development mean and standard deviation could be just added as last lines in Table 6.

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Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-455, 2020.