COMMENT 1:

Ground-based microwave radiometers (MWRs) are used by many to provide measurements of the temperature profile and its evolution in the boundary layer. This paper builds upon work by other authors, exploring some important aspects that could lead to errors in the retrieved profiles. In particular, explore the role of horizontal inhomogeneities near the instrument, tilt of the radiometer, physical obstructions, and radio frequency interference (RFI).

My main question to the authors is simply: how does this paper add to the already extensive number of papers that address many of these topics? They reference the Meunier et al 2013 paper (but don't include any of those findings in their discussion here -- indeed, they ignore the possible uncertainties associated with beamwidth totally). There were also good papers by Han and Westwater (2000) and Liljegren (2000) that should have been referenced, and from which this paper should build. Regardless, the authors need to clearly state the new knowledge this paper is contributing to the field.

- First of all, thanks for your insightful comments. This paper tries to give a comprehensive overview of the measurement uncertainties of the newest state-of-the-art Gen5 HATPROs. Older papers do not describe all of these uncertainties (e.g. impact of obstacles or tilt) and when they do, it is often outdated information from older instruments. For network operation, these uncertainties are very important, that is why ACTRIS, E-PROFILE and the DWD need these up-to-date information.
  A passage has been added in Section 1 to make that more clear:
    - "For all that it is crucial to assess the uncertainties of state-of-the-art MWRs, as there is no comprehensive analysis and overview of uncertainties of these newest instruments yet. This study analyses measurement uncertainties by external sources, but uncertainties also encompass instrument uncertainties like biases, drifts, random noise and calibration errors which have been partly discussed in previous studies (Liljegren, 2002; Crewell & Löhnert, 2003; Maschwitz et al., 2013; Küchler et al., 2016) but are in need of updating."
- Liljegren (2002) and Han & Westwater (2000) references have been added to Section 1 and 2.
- Possible uncertainties associated with beam width are negligible small in the V-band with elevation angles >4° (<0.05K), according to Meunier et al. (2013). This is clearly stated at the end of Section 2. As there are no notable uncertainties associated with beam width in our case, there is no need to discuss this further in the paper.

I also found that this paper has a very informal tone about it; i.e., the language is a bit "loose". Many of the statements are repeated multiple times and could be better organized. There were multiple radiosonde (and retrieval coefficient) datasets used and at least two different MWRs; I suspect this does not impact the overall results at all, but it does add confusion.

- Repetition of certain important aspects are repeated multiple times on purpose to remind the reader, when needed. Nevertheless, we tried to make the language more precise in various sections.
- Section 3 already tries to make it clear where the data comes from and why.
- A new passage in Section 5 has been added to summarize all that:
    - "On one hand, the pointing errors and obstacles have been simulated with the help of a line-by-line RT model. The forward model implements radiosondes from RAO as input and the T-profile retrievals utilize coefficients from RAO as well. On the other hand, the instrument misalignments, horizontal inhomogeneities and an example of RFI have been analyzed through measurements on site at JOYCE, and T-profile re-

trievals from those measurements utilize coefficients from nearby De Bilt, Netherlands."

Also, the focus of the paper (from the title and abstract) was on the impact to the retrieved temperature profiles, and thus the inclusion of the discussion on the K band channels is distracting from the main message. And in many ways, it seemed that the K-band results were included as an afterthought, and not well organized. I would recommend that either the title changed and the K-band results be separated into their own (sub)sections, or that the K-band results be removed.

- Indeed, the main focus of the paper is the impact on the T-profiles (and hence the V-band channels), as it is stated multiple times. Nevertheless, the impact of RFI in the water vapor channels are briefly mentioned at the end of the abstract. The inclusion of the K-band channels throughout the paper is very brief and only complements our findings and is important for future research in how uncertainties could affect measurements on humidity advection/horizontal inhomogeneity of water vapor.
- Most radiometers (especially HATPRO-G5) do measure within the K-band, that is why it should be shortly introduced.
- We decided against changing the title of the paper, because it describes the content of the paper very well. We also decided to not get rid of all K-band results, as they are needed to paint a full picture. Removing the K-band would also result in losing our example we have of RFI. Some lines have been tweaked in Section 5 to make it more clear, that K-band findings do not play a role for T-profiling:
    - "The impact of RFI on T-profiling – at least in our example – is nonexistent, when they occur around or near commonly used frequencies for communication links, which are usually situated within the K-band (mostly between 20–30 GHz). These frequencies are not utilized in T-profile retrievals. However, RFI can negatively affect the analysis of observed TBs within the K-band in off-zenith directions, which bear the potential for deriving horizontal water vapor inhomogeneities."

  In the other Sections (4.4. and 4.2.) it is already clearly stated that K-band channels are not a focus and are therefore only briefly mentioned.

- We removed the description of the phase shift between K- and V-band in Section 4.4. though (line 479-486), as it is not really necessary within the scope of this paper, which hopefully reduces possible confusion for the reader.

I think that a key point that is being made here, but not explicitly stated, is the importance of always collecting elevation scans on both sides of the MWR. This allows the analyst to determine (from a sufficiently long dataset) the possible tilt of the instrument and the frequency of horizontal inhomogeneities. I think this point should be stated strongly, as I've observed many groups who believe it is sufficient to only collect elevation angles along one side of the radiometer.

- It is indeed better to collect elevation angles scans from both sides of the instrument during measurements, if this is possible. Oftentimes though it can be very difficult to scan down to 5° on both sides.
- That is why it is important to do 360° azimuth scans before doing regular measurements (which is stated in the paper in Section 4.4. and 5), in order to figure out possible tilts and inhomogeneities.
- Statements have been added (Section 4.2.2. and 5.) that collection of scans on both sides of the radiometer is recommended when possible, but that this also introduces some other challenges (see Section 4.2.2.):

- “In real-world scenarios, scanning on both sides of the radiometer – if possible – can mitigate pointing uncertainties to a certain degree, when T-profiles are retrieved from the average of such scans. However, this approach comes with problems, such as longer measurement times, the assumption of horizontal homogeneity of the atmosphere, and the assumption that elevation scans on both sides even out linearly.”
- “If full azimuth scans are not feasible, at least elevation scans on both sides of the MWR are recommended, when there is the possibility to scan down to 5° elevation in both directions. Scanning in only one direction is sufficient though to retrieve accurate T-profiles, when the instrument is set up properly.”

- For accurate T-profiling, scanning on one side of the instrument is sufficient though, if the instrument is set up properly. Possible (small) tilts of the instrument only have a very small impact (<0.1K for 1°) on T-profiles, as stated in Section 4.2.2.

The retrievals performed in this paper were done using a statistical method. Would have the results changed if a more accurate physical retrieval method was used instead? Line 79 indicates that the method of retrieval could matter (it might be useful to reference the Maahn et al. BAMS 2020 paper here for context -- Loehnert is a coauthor of that paper).

- For the purpose in this paper – a sensitivity study – it does not matter whether you use a statistical retrieval or a more accurate physical retrieval, as long as you use the same sort of retrieval all the time. Our results would be the same. We’re mainly looking at the differences of the retrieved profiles, not at the absolute temperatures of the retrieved profiles. For this study, the use of a statistical retrieval was faster and easier to implement.
- Additionally, most operators do not use physical retrievals.
- A statement in the beginning of Section 5 has been added to address this issue:
  - “Regarding the retrieval method, a statistical approach has been employed. The utilization of alternative retrieval methods, such as a physically based one, would not yield a different outcome for this study.”

Regarding obstructions: one of the more common setups that could affect these observations are power lines that are in the field of view of the radiometer. These lines clearly don't fill the entire field-of-view. Would the authors be able to provide any guidance on how far power lines would need to be away from the radiometer as to not impact the V-band observations?

- As the power lines are thin and would only fill out a very small portion of the beam, it is hard to say what impact they would have on V-band observations. It would depend on the thickness and number of powerlines in the field of view, and also on their temperature, which probably is higher than the ambient temperature. It is safe to say that avoiding power lines altogether or being as far away as possible (at least a few hundred meters) is the best bet. We cannot easily simulate power lines at different distances. Azimuth scans would reveal possible RFIs though.
- Statement for non beam-filling obstacles has been added in Section 4.3.:
  - “When encountering a small or slim obstacle that does not fill the entire beam width of the instrument, the resulting impact is generally less significant compared to larger obstacles. Simulating obstacles that do not completely fill the beam width of the MWR (such as power lines or lightning rods) poses challenges. Therefore, in our simulations, we focus on beam-filling obstacles for which a minimum distance can be determined at which they do not interfere with the measurements anymore. Our simulations of such beam-filling obstacles…”

Equations 2 and 3: why is the retrieved temperature a function of both frequency and height? I think you can remove the nu from the left side of both of those equations.

- Has been changed.

Line 230: Aren't rapid changes in zenith radiance observations also a measure of the horizontal inhomogeneity? Is there a way to look at the variability of the zenith radiance observations over time (and the trend of the magnitude of these radiance obs over time) to estimate the possible level of horizontal inhomogeneity?

- Yes, changes in zenith radiance are also a measure of horizontal inhomogeneities. In order to estimate the level of horizontal inhomogeneity reliably, elevations scans are needed though. Estimates on horizontal inhomogeneity only using zenith measurements are rather unprecise, as it only concerns the variability over the instrument, but does not include horizontal gradients.

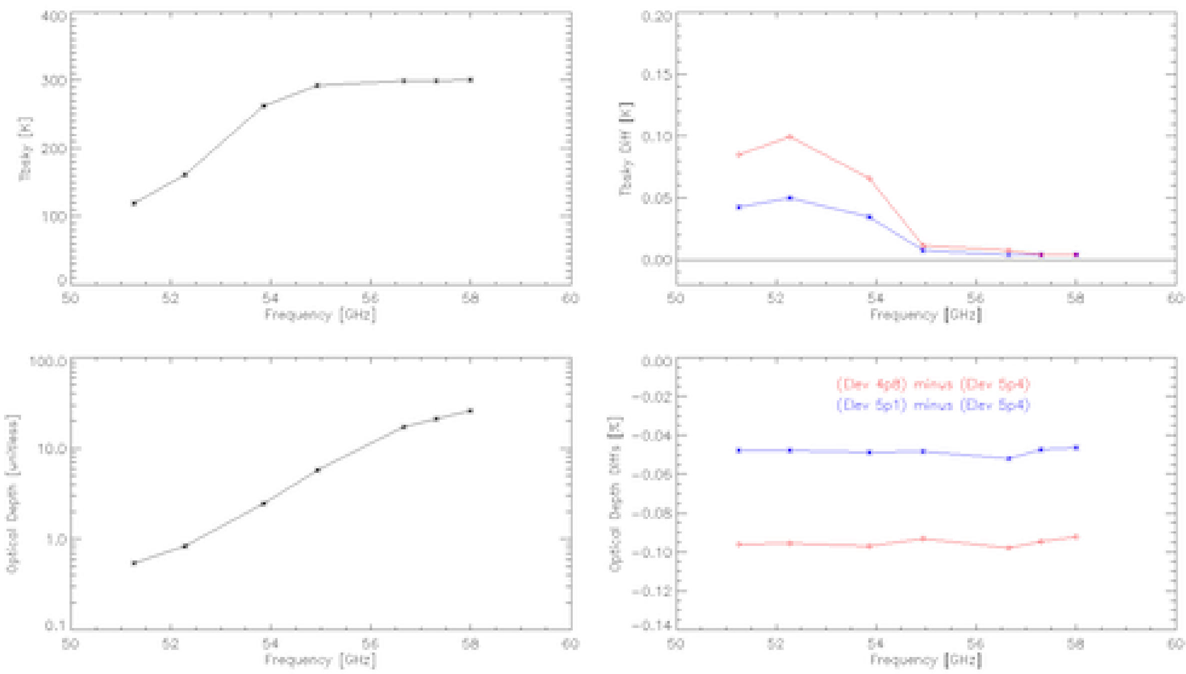
Lines 315-320: (if you decide to keep the K-band material and reorganize it): the magnitude of the impact on the K-band as described in the text does not seem to match with the magnitudes shown in Fig 4.

- Has been checked. Description in the text does match with the magnitudes shown in Fig. 4.

You make the point a few times in different places that the impact of obstacles depends on the vertical structure of the temperature profile; namely, that there is a smaller impact when there is an inversion. This really should be discussed a bit more to explain why this is.

- Explanation has been slightly extended in Section 4.3.1:
    - "For an ambient temperature obstacle, the ΔTB can even become negative when there is an inversion, meaning that the ambient temperature obstacle near the colder surface blocks the MWR from observing warmer atmospheric layers above and beyond the obstacle."

This paper would be markedly stronger if a plot showing the changes in Tb and the associated changes in total optical depth for small elevation differences was shown. I've included such a figure here for a mid-day, midcontinental radiosonde with IWV=43 kg/m2, for small elevation angle changed around 5.4 deg. In particular, note that the change in the optical depth is constant, in a fraction changed sense, as the elevation angle changes. This also demonstrates why there is little impact for channels 12, 13, and 14 -- the optical depth is already very large.



- The plots you show are interesting, and the information in them is complementary to our results. But we think that they do not show anything new. In our opinion, the information already given in the paper is enough, as the analysis of changes in optical depth due to elevation is not a focus in this paper, but rather the impact of that for sensing obstacles.
- It is stated in the paper multiple times (Section 2, 4.1., 4.3.) that channels 8-10 are optically thinner than channels 11-14, which are optically thick and therefore do not penetrate the atmosphere very far. The results in Section 4.1. and 4.2. also make clear that as a result the optically thick channels 11-14 do not sense horizontal inhomogeneities.
- Table 2 also demonstrates the various penetration depths for different channels depending on the elevation angles.

Line 440: Clear sky scenes also have very little temporal variation in the observed Tb values. So not only should the mean LWP be small, but the standard deviation of the LWP should be small also.

- Indeed. New filtering has been added in Section 4.4.:
  - "If the mean LWP of a 30 minute interval before and after one scan is below 10 g m^{-2} and its standard deviation below 4 g m^{-2}, then it is most likely clear-sky."

Line 452: Earlier, you made the point that obstacles needed to been many hundreds of meters away; yet this lightning rod is only 5 m away and has no impact. I suspect this is because the beamwidth, together with the 10-deg azimuth sampling, resulted in this rod not being in the V-band's field-of-view. Is that correct? Regardless, the way this section is worded conflicts with what you wrote earlier in the paper about obstructions.

- Paragraph has been added in Section 4.4. which addresses this phenomenon and explains better why the obstacle can only be seen in the K-band but not in the V-band:
  - "The lightning rod is not detected in the V-band channels primarily due to the narrower beam width of the V-band receiver. This is mostly attributed to the combination of the 10° azimuth step and the rod's slim profile, which results in the rod being predominantly positioned outside the field of view. Although it is possible that parts

of the rod may be partially within the field of view, the limited coverage within the field of view is not substantial enough to have a noticeable impact on the measurements."

Line 475: here is an example of the loose writing. You've already stated that off-zenith observations from channels 8 and 9 are not used in the temperature retrievals, so RFI disturbances in these channels at off-zenith angles should ZERO impact on the temperature retrievals (not negligible).

- Has been fixed and shortened:
    - "As already discussed in Section 4.1, off-zenith disturbances in channel 8 and 9 have no impact on T-profiling."

Line 485: I really don't understand how internal uncertainties / misalignments would cause these results. This needs to be explained, or (as is my preference) the K-band results removed.

- We are also not sure what causes this or how it is caused, it's only speculation. But the error source has to be "inside" the instrument, as external factors can be excluded. We removed this paragraph (line 479-486 in the original manuscript), as it is not crucial for T-profiling.

Line 500: this is a great place to emphasize the need to have matching elevation scans on both sides of the radiometer

- Initial azimuth scans, as suggested a few lines earlier, are a good method to detect disturbances, when feasible. Statement has been added, if not:
    - "If full azimuth scans are not feasible, at least elevation scans on both sides of the MWR are recommended, when there is the possibility to scan down to 5° elevation in both directions. During regular operation, scanning in only one direction is sufficient though to retrieve accurate T-profiles, when the instrument is set up properly."

Line 511: Are the errors in retrieved temperature profile small because the errors in the observed Tbs are somewhat offsetting because you are using data from both sides of the radiometer? I think yes.

- We're talking about simulated pointing errors here, so there's no data from both sides of the radiometer. Just a simulation at a certain elevation and another simulation at a slightly different elevation. Then the differences of these simulations and their influence on T-profiles are described. So there is no "offsetting" taking place.