

Atmos. Meas. Tech. Discuss., referee comment RC1
<https://doi.org/10.5194/amt-2021-293-RC1>, 2021
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Comment on amt-2021-293

Anonymous Referee #1

Referee comment on "Quantification and mitigation of the instrument effects and uncertainties of the airborne limb imaging FTIR GLORIA" by Jörn Ungermann et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-293-RC1>, 2021

General Comments:

This paper presents an updated instrument calibration and characterization approach for the GLORIA imaging FTS instrument. The paper builds on earlier work that has been presented in several other papers, namely, Kleinert et al (2014) and Guggenmoser et al., (2015). The paper provides a detailed description of the updated techniques that were developed to characterize and mitigate instrument artefacts using numerous inflight measurements. The improved approach is then used to characterize error sources and examine the impact of these errors on the level 2 temperature and ozone products.

The authors point out that the current paper "collects all relevant processing information for GLORIA in one place, thus being a reference for further geophysical interpretation of the data or derivative satellite-borne instruments". Indeed, the authors provide a good review of the overall measurement technique, the general calibration approach, and the level 0 to level 2 processing steps. This end-to-end examination of the updated calibration approach, error estimates and analysis of the impact on level 2 products will be a good addition to the literature. In many ways this paper can be viewed as a companion paper to the earlier works.

The only general comment that I have is that it isn't clear that there has been an improvement to the level 2 products. Since only ozone and temperature are examined, it's also not clear what the broader impact is on other GLORIA products (H₂O, HNO₃ etc.). However, the paper does provide an excellent basis to estimate and understand the impact of the instrument artefacts in a manner that likely wasn't possible for the earlier works. My only suggestion would be to explicitly identify any improvement (or not) to the level 2 products if possible.

All other questions, comments and suggestions are minor and are listed below.

Specific Questions & Comments

- **Section 4.1:** The first step of the process of removing the atmospheric signatures from the deep space measurements is the calibration using the two (hot and cold) on-board blackbodies and the subsequent removal of a residual broadband offset. The authors demonstrate that the shape of the offset can be corrected by fitting a Plank function to several points that are identified in Table 2 and in Figure A1.

- Can the authors comment on the possible cause of this residual offset?
- It appears that the fit points are close to regions that are devoid of atmospheric signal for the first 4 points (left to right); however, this is less clear with the 5th. Can the authors clarify how the micro-windows used in the fit to the Plank function were chosen?
- A spectral shift is also derived from the shaved deep space measurements. This is calculated separately in each of the micro-windows in Table 2. How large are the individual shifts and how much do they vary between the micro-windows? Is this the same spectral shift that is characterized in Section 5.5?

- **Line 176:** Why not just remove the bad pixels and use the mean of the central row to provide a high SNR spectrum for the central pixel as opposed to the median?
- **Line 204:** The authors refer the reader to Figure 2 regarding the linear interpolation between rows. However, only the median spectrum is shown in Figure 2. Can the authors clarify what was being referred to here?
- **Figure A4:** The data plotted in Figure A4 is used to characterize the quality of the removal of the atmospheric signatures from the deep space measurements. The authors note large deviations near 830 cm⁻¹ that were attributed to the germanium window emission that wasn't corrected until after the early TACTS campaigns. However, from Figure A4, there are also enhanced features between 750 cm⁻¹ to 800 cm⁻¹, as well as, near 1050 cm⁻¹ and 1300 cm⁻¹ that don't appear linked to variations in the window emission. Can the authors provide clarity on the cause of these features?
- **Line 741:** What is the reasoning behind using different profiles for the same species in the different micro windows? For example, for ozone, v2 is used for 850 cm⁻¹ to 1065 cm⁻¹ while the other spectral ranges use v0.
- **Line 319:** Why was the beam splitter turned by 90 degrees? During which campaign does Figure 8 correspond? In that case, the parasitic images are still distributed horizontally.
- **Line 365:** The wording here suggests that the Gaussian was fit only to the left portion of the distribution in Figure 10. If that is the case, then what criteria was used to reject certain data from the fit?
- **Figure 17 (c):** There is clear "band" structure in the noise estimate that is most likely associated with the readout electronics. The variation in the vertical and impact on retrievals could potentially be minimized by rotating the camera. Was this considered?
- **Line 526:** Isn't the correction presented in Section 4.3 supposed to correct for the changing temperature of the window?
- **Line 623:** Was the PSF characterized in the lab or simulated using optical design

software? What is the expected PSF and how well does it match the chosen profile?

- **Line 658:** I think the vertical structure in these errors could be quite important since the retrieved profiles are vertically resolved. It would be interesting to know which ones have the largest vertical structures. Can you comment on the impact of these structures on the retrieved profiles?

Technical corrections:

Line 65-60: Sentence is repeated.

Line 207: The wording isn't clear here. Suggestion to replace "The subtraction....." with "The removal...." ?

Line 236: "and are thus behave" should be "and thus behave"?

Line 655: do you mean CO₂ emission lines?

Line 762: I assume the term "sled position" corresponds to the interferometer mirror position. This should be defined or reworded to be clearer.

Eq. B5, Line 795: Does the "x" in Eq. B5 refer to the non-linearity scaling factor? If so, it could get confused with the variable x in Eq. B1.

Figure A5(a): What wavenumber does this figure correspond to?