

Interactive comment on “IMK/IAA MIPAS temperature retrieval version 8: nominal measurements” by Michael Kiefer et al.

Michael Kiefer et al.

michael.kiefer@kit.edu

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Replies to Anonymous Referee #2

We thank the referee for his corrections and suggestions which we much appreciate, and we are confident that their implementation is beneficial for the readability and quality of our manuscript.

Questions/comments of the referee are marked by **RC:** and set in *slanted font*.

The suggested corrections of lines 154, 182, 310, 371, 461, 556 have been imple-

mented.

RC: *Line 44: “.. degraded spectral resolution reduced resolution” Perhaps “reduced resolution” can be italicized or put in quotation marks to make it easier to read. I had to read the sentence, particularly “degraded spectral resolution reduced resolution” several times.*

Reply: We changed the sentence to: For this second operation phase with degraded spectral resolution we shall use the designation “reduced spectral resolution” (RR) period.

RC: *Line 71: “correction led to too small values” Unclear, what “values” refers to here: correction values, radiances or temperatures (probably radiances)? Please clarify.*

Reply: It is the temperature values, which is stated in the text now. In the cited report of Hubert et al. temperature (among other quantities) from several MIPAS data versions is compared with correlative measurements.

RC: *Line 137: “and then fitting a linear regression function to the shift values, which are calculated for the single microwindows.” It would be good to mention how well the frequency shift values for the different microwindows can be approximated by a straight line.*

Reply: Text with appropriate numbers has been added after the cited sentence.

RC: *Line 196: “is obtained by linear interpolation along with hydrostatic correction of*

pressures at the given geometric altitudes.” I don’t really understand what was done here. Can you rephrase or add an additional sentence?

Reply: The text has been expanded with a description of the temperature calculation in the transition region and of the pressure integration.

RC: *Line 203: “Since limb measurements used for one profile retrieval cover, depending on the measurement mode, about 1600 to 2200 km in the horizontal,” I wonder, why this distance is so large. What does it refer to exactly? What is duration of a limb scan?*

Reply: The sentence has been split in two and partly rewritten. We now mention the horizontal ranges 260 to 440 km from which 95% of the radiance information comes from, and include a reference for these values. The numbers in the original version were simply the length of the line of sight between the top of the atmosphere (assumed to be about 100 km) and the tangent point, multiplied by two because there is a path segment behind the tangent point and another one in front of the tangent point

RC: *Section 3.5: Horizontal variability The approach you used to consider horizontal variability seems very good. I suggest mentioning the horizontal resolution of your model atmosphere. This is not mentioned, as far as I can tell.*

Reply: The horizontal resolution of the used ECMWF ERA-Interim data set is given now in the text.

RC: *Section 3.7: Is the background continuum spectrally neutral? Probably yes, but it should perhaps be mentioned explicitly.*

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Reply: It is not clear to us what “spectrally neutral” means. The background continuum is wavenumber-independent within a microwindow in terms of the absorption cross-section, which implies, via the Planck function, a slight wavenumber-dependence within the microwindow.

RC: *Line 248: “The cause of the continuum signal from high altitudes is presumably meteoric dust” Just out of interest: Is there any chance the measurements can be used to identify meteoric dust? Or has this been attempted already?*

Reply: The only information we have is the paper by Neely II et al (2011), cited in the paper. We too are curious whether this hypothesis can be independently corroborated. To make more clear that we consider it a hypothesis, we changed “presumably” to “possibly” in the text.

RC: *Section 3.8: Does the offset have a constant value for all wavenumbers of a microwindow?*

Reply: Yes. The text has slightly been changed to make this more clear.

RC: *Section 3.11: I suggest mentioning which process/reaction leads to vibrational populations being removed from LTE. If it's several processes, perhaps the most important one can be mentioned.*

Reply: A brief description of the cause of the population of the NLTE states at 15 μm has been added to the text.

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RC: *Line 328: “The atmospheric conditions under consideration are northern and southern polar winter, polar summer.. ” This is only a minor issue, but does polar summer include both hemispheres? I tried to count, whether it is nine scenarios and was a bit confused.*

Reply: We meanwhile have error estimates for a larger number of atmospheric conditions available (now also distinguishing N and S polar summer, and many more). We will adjust the text and the presentation of data accordingly.

RC: *Section 4: I Suggest mentioning how the individual error sources were added to determine the total error.*

Reply: Since we consider all error sources as independent from each other they are added quadratically to give the total error. This is explained now in Section 4.1.

RC: *Figure 3: Please explain the meaning of the red crosses and plus signs at the bottom of the figure.*

Reply: These symbols indicate the position of the measurement and give the lighting conditions (cross: night, plus: day). This is stated in the caption now.

RC: *Line 366: Please explain or spell out “IF16”*

Reply: The term “IF16” has been cancelled. Instead we have added some details about what kind of data is used and why (only) these data could be used for this

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specific method to determine gain uncertainties.

RC: *Line 487–498: It would be good to provide more quantitative information here. How large was the drift before, compared to other sources (which sources) and how large is it now?*

Reply: Section 5.2.1 has been completely rewritten and the figure has been exchanged. We now present drifts in temperature of MIPAS V5 wrt two reference instruments, as extracted from a paper by McLandress et al. (2015), and then use the accordingly calculated drift between MIPAS V5 and V8 temperatures to infer that there indeed has been an improvement in the data quality.

RC: *Figure 8: Suggest to mention the years in the caption of this figure, too*

Reply: OK, done.

RC: *Figure 9 and related discussion: One can see the differences, but one doesn't know which product agrees better with the true T-field. The discussions of the differences between versions 8 and 5 should be complemented by more quantitative comparisons with independent measurements. Perhaps you can simply refer to existing validation studies for V5.*

Reply: Since this deficiency in V5 occurs only in specific situations, it was not detected by temperature validation studies of nominal mode data. However, we now use the V5 middle atmosphere data (which has been validated by García-Comas et al., 2014) as a reference to show that V8 nominal mode data indeed has improved over V5 nominal

data. Figure 9 and caption, as well as the text of Section 5.1.3, have been extended accordingly.

RC: *Appendix A: The tables A1 to A9 differ in the altitude range shown. I guess this was done on purpose? If yes, it would be good to mention it and mention the reasons for the different altitude ranges.*

Reply: There are two reasons: First the lowest altitude of MIPAS nominal mode observations varies along the orbit/latitude. However, the second, and more important, factor is that the errors are only defined at altitudes where the spectra are not contaminated by IR-emission of clouds. The cloud altitude strongly depends on latitude and season. This is now explained in the text of Appendix A.

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