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Atmospheric Measurement Techniques Discussions

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

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Replies to Referee #1

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*.

All minor edits and wording suggestions of the referee have been implemented.

RC: *I would have liked to see a few more words on how pressure is handled. The comment on page 8 ["Between 43 and 53 km, a smooth transition between ECMWF and bias-corrected WACCM temperatures is obtained by linear interpolation along with hydrostatic correction of pressures at the given geometric altitudes."] seems to imply that pressure is fixed to a priori information in the analysis and makes me wonder if perhaps the source of pressure information is different below 43 km versus above 53 km. The significance of this question comes from the following comment: Page 24, line 553: V8 engineering tangent altitudes are, on average, lower than the retrieved ones by about 200 m below 40 km and by about 50 m above*

Reply: The method which leads to the a priori temperature and pressure values above 43 km is now described in more detail in the text. Only the pressure at 20 km altitude (or at the lowest tangent altitude above 20 km with valid spectral information) is taken from the apriori profile for the calculation of the pressure profile during each iteration step, making use of the hydostatic equation. This is described in some detail in the 2003 paper by von Clarmann et al., cited in the text. Sentences that shortly state this and which contain a citation of the paper where details of the method can be found have been added to the text of Secs. 3, 3.1, and 3.4. From this it should be clear that one would not expect a visible influence of the transition region pressure on the line-of-sight retrieval. We realize that the referee's suspicion that there might be an influence probably is based upon our maybe too sloppy description of the course with altitude of the differences between engineering tangent altitudes and retrieved tangent altitudes under item 3 of Sec. 5.4. The keywords here are "on average". Actually it is not true that there is a jump at 40 km in the differences, but only in the averages from 0-40 km and 40-70 km. However, we unfortunately had to realize that our presentation in the manuscript was erroneously based on FR data alone. Hence, the text has been reworked to also take into account the RR period. We try to be more specific about the altitude course of the differences of quantities to avoid the (wrong) impression that there are steps or jumps.

RC: *I can imagine that the onset of refraction effects below 40 km might contribute to larger discrepancies in this altitude region, but I would perhaps naively expect such errors to increase with decreasing altitude.*

Reply: Right. Actually these errors kick in below 20 km. This can be nicely seen in comparisons between the engineering tangent altitudes of V5 and V8, since in the L1b-processor refraction has been implemented after V5 processing. We briefly mention this in the reworked Section 5.4.

RC: *As described here, the errors seem to be more of a step function, possibly suggesting something in the analysis that generates a* < 150 *m discrepancy below 40 versus above 40 km. Obvious candidates would be pressure (from page 8, the described hydrostatic correction of pressures between 43 and 53 km), or from Table 2, there is a set of three microwindows containing strong CO2 lines where the lower altitude limit is 42 km. Discrepancies between these microwindows and other microwindows used in the analysis below 42 km could give rise to an apparent step in pointing. The latter possibility could be tested by adjusting the lower altitude limit of the three microwindows in question from 42 km up to 55 or 60 km and see if the discrepancy from engineering information changes for the region just above 40 km.*

Reply: As already mentioned above, there actually is no step function. There are no retrieved quantities which change (more or less) abruptly at an altitude of 40 km, neither at any other altitude. However, we much appreciate the Referee's comments, obviously motivated by the wish to help us to overcome an error, which, if it actually

existed, would be an unpleasant one.

RC: *It would be good to say a few words on the determination of instrument pointing (tangent heights). I assume it basically amounts to ensuring hydrostatic equilibrium is maintained for the combination of assumed pressure profile and retrieved temperature profile, but it is not clear from the text.*

Reply: We have added text to clarify this to Sections 3, 3.1, and 3.4. Since the method has already been described by von Clarmann et al. (2003) we kept these additional text items short and cite this work where appropriate.

RC: *The apparent step function in the discrepancy from engineering information below 40 km versus above 40 km is not reflected in the error estimates. If the source of the step function is a problem in the assumed pressure profile, the retrieved tangent height will mostly compensate, but there might be a "second order" contribution to the retrieved temperature error, along with a 150 m altitude registration offset in the two altitude regions. Even if there is a 150 m step, though, things are still better off than the previous processing version. I am not advocating further investigations at this time or significant changes to the paper. Just a few words on how pressure is handled and a brief description of the nature of the tangent height determination (e.g., ensuring hydrostatic equilibrium) would suffice.*

Reply: The error estimates do not include the step because there is none.

RC: *Page 10, line 248: "The cause of the continuum signal from high altitudes is presumably meteoric dust." I do not know what magnitude of continuum levels are being discussed, but it would surprise me if that were the case. I can see there being* *measurable scattering effects for lidars operating in the visible, but we are talking about the thermal infrared here. Contributions to the spectrum from background sulfate aerosols could extend up to about 40 km. However, my first inclination would be far wing contributions from the nearby strong CO2 Q-branch that are missing in the calculation (assuming your calculation does not extend that far in wavenumber), or the shape of the far wing contribution isn't quite right (e.g., it should be sub-Lorentzian or line mixing contributions are missing or not quite right). The shape (as a function of wavenumber) for the retrieved continuum parameters might give a clue. If the far wing lines are missing in the calculation for a set of microwindows, if the apparent continuum is larger for microwindows closer to the CO2 Q-branch, that could suggest far wing effects are the source. Note that I cannot say with certainty that the continuum is not associated with meteoric dust. Perhaps it is, but it would surprise me.*

Reply: We would be surprised if pressure broadening and related effects played such a prominent role particularly at these high altitudes where Doppler broadening becomes the dominating mechanism. However, nothing in our retrieval scheme depends on this since we use the empirically fitted continuum. Our remark on the meteoric dust is not more than a speculative, tentative explanation. To make the speculative nature of our statement clearer, we replaced "presumably" with "possibly" in the text.

RC: *Page 10, line 266: the offset can vary independently between microwindows The sources of offsets that I can imagine would all at least vary smoothly with wavenumber. Self-emission of the instrument, which would provide an offset with a blackbody curve appropriate to the instrument temperature. Deficiencies in the detector non-linearity correction. Hard to say what the shape might be, but would it be random? Channeling artifacts, which would have a sinusoidal variation with wavenumber (or a superposition of sine waves if there are multiple contributions), but if your microwindows sample the*

sine pattern (or the beating pattern from multiple overlapping sine waves) at various locations, I suppose it might look vaguely random. Is there typically a lot a scatter as a function of wavenumber in the retrieved offset values? Is there a physical explanation attributed to the offsets?

Reply: Since MIPAS spectra are calibrated, the offset accounts only for the residual differences after calibration. Self-emission of the instrument should first order be calibrated away. Since the calibration measurements themselves are susceptible to noise, we suspect that the offset may well have a random component. Since ideally everything that can cause an offset should be calibrated away, discussions on the sources of the residual offset must remain speculative. Thus we prefer not to discuss this issue at any depth.

RC: *Page 17: "Random errors are errors which explain the standard deviation of the differences between measurements of the same state variable by two different instruments." I know what you are saying, but the wording implies a narrower definition of random error than I would like. How about the following: When comparing measurements of the same state variable by two different instruments, random errors are errors that contribute to the intrinsic variability (standard deviation) of the differences.*

Reply: The text is changed accordingly.

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