

Interactive comment on “On the quality of MIPAS kinetic temperature in the middle atmosphere” by M. García-Comas et al.

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The paper "On the quality of MIPAS kinetic temperature in the middle atmosphere" is well written, interesting and scientifically sound. It may be accepted for publication in ACP after addressing a few points:

page 24235, line 8: spectral range of MIPAS is up to 14.6 um (not 15.6 um)

page 24237, line 11: CRISTA was mounted on the free-flying ASTRO-SPAS satellite. It was launched with the U.S. Space Shuttle in November 1994 and August 1997, yielding about one week of atmospheric measurements, each.

page 24237, line 13ff: You may cite Gusev et al. [2006] (doi:10.1016/j.jastp.2005.12.010) for the CRISTA non-LTE T-retrieval and Gross-

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mann et al. [2002] for the CRISTA-1,2 experiments. I would not call the non-LTE retrieval a 'reviewed processing'. The LTE T-retrievals up to 85 km are presented by Riese et al. [1999] and non-LTE retrievals (up to 110 km) by Gusev et al. [2006]

page 24237, line 14: 74 deg S - 74 deg N

page 24242, line 22: Can you comment (shortly, a few words) on the accuracy of the statistical band methods used here?

page 24243, line 12ff: Please clarify, why you chose a different VV scheme than Lopez-Puertas et al. [2009a]

page 24247, line 22: Since laboratory and atmospheric measurements give rate constants differing by a factor of four (for CO₂-O VT collisions), why do you assume a factor of two uncertainty for this rate constant, only?

page 24247, line 11: An uncertainty of 15% for CO₂ vmr in the UMLT is not adequate. Several publications (e.g., Lopez Puertas, 2000; Kaufmann et al., 2002, Beagley et al., 2010) exhibit much larger uncertainties, which are larger than 100% at 100 km. Please revise this source of uncertainty.

page 24247, line 23: Figure 3 in your paper demonstrates, that tidal signatures are visible down to 35 km.

page 24248, line 16: Since uncertainties in the spectroscopic data are based on personal communication with J.M. Flaud, they should be given explicitly in the paper.

page 24249, line 11: As far as I remember, 6-2 rotational temperatures may be affected by rotational non-LTE. If you agree, I suggest to use a different wording than 'non-LTE free measurements'.

page 24249, line 20: time difference is in UT? Please clarify.

page 24250, line 19: To my opinion, it is not appropriate to add systematic uncertainties of two instruments quadratically, since this is not a random variable. In particular,

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MIPAS and SABER T data exhibit the same sources of uncertainty (and even the same values for certain rate constants) in many cases. This type of uncertainty should be considered in the comparison with T data from other measurement techniques, but it should not in the SABER-MIPAS comparison. I propose to calculate T uncertainties for the MIPAS-SABER comparison comprising of only those components, which differ between the two datasets, such as radiance uncertainties, utilization of different atomic oxygen profiles, etc.

page 24252, line 4: Please specify 'un-physical retrievals' quantitatively.

page 24256, line 2 and page 24273ff: You mentioned on page 242532 that you use atomic oxygen data from the NRLMSIS-00 model. Comparison with SABER data and also your T data comparisons suggest, that the MSIS atomic oxygen is too low. Is this (more general) statement correct? If yes, I suggest to add (this more general statement) in the paper as well. However, Smith et al. [2010, JGR] pointed out, that SABER atomic oxygen data is a factor of 2–5 larger than MSIS and other measurements, which may be mentioned in this context as well.

page 24286: I suggest to define 'Non-LTE' (including atomic oxygen uncertainty?) and 'Total Sys' (root sum square?) uncertainties in the table caption

page 24289: I would define the acronyms (such as MLO) in the table caption.

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