Atmos. Chem. Phys. Discuss., 11, C16386–C16393, 2012
www.atmos-chem-phys-discuss.net/11/C16386/2012/

Atmospheric Chemistry and Physics Discussions

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

M. García-Comas et al.

maya@iaa.es

Received and published: 12 April 2012

We thank Dr. Martin Kaufmann for reviewing our manuscript and for his useful comments. We think we addressed all issues he raised. The answers (AA) to his comments (RC) are given below.

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

AA: Done.

RC: 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.

AA: The sentence has been changed accordingly.

RC: 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 Grossmann 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]

AA: Done.

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

AA: Done.

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

AA: We included the following: "Above 20 km, radiative transfer is calculated using statistical band methods (i.e. "equivalent line" approach) in the Curtis matrix formalism, except for the v2 and v3 fundamental bands, which are calculated line-by-line. The accuracy of the statistical band method has been assessed to be better than 1% in terms of resulting populations."

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

AA: We thank the referee for this comment since the scheme has not change with respect to López-Puertas et al. (2009) but with respect to López-Puertas and Taylor (2001). Anyhow, the differences in the scheme used follow. Our VV scheme is more sophisticated than that used in López-Puertas and Taylor. Dang et al. (1983) provide values for each vibrational level in the 020 triad. Lopez-Puertas and Taylor assumed the same rate for the three levels. Using the harmonic oscillator law and an energy gap law, we also scaled those values for the more energetic levels and for the inter-isotopic V-V exchange. That leaded to a large and complete set of rates for all levels included in the non-LTE model, instead of the constant value used in López-Puertas and Taylor for all levels. As an example, for the 020 triad (net) and for the inter-isotopic exchange, the values we use double the ones used in López-Puertas and Taylor. We rewrote the sentence: “A new V-V v2 collisional scheme has been included, in which we have used the values of Dang et al. (1983) for the V-V exchange between the three v2=2 levels and the v2=1 level, and have scaled these values using the harmonic oscillator law and an energy gap law for collisions in which more energetic levels or different isotopes take part. In particular, for the 020 triad and for inter-isotopic V-V exchange, that results in a 2 times faster net exchange rate of v2 quanta in CO2-CO2 V-V collisions (kvv) than that used in Lopez-Puertas and Taylor (2001).“

RC: 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?

AA: Because we used the uncertainty reported by Sharma and Wintersteiner (1990), which coincides with that coming from atmospheric measurements alone (when not considering the case of the rate obtained from the SISSI data analysis of Vollman and Grossmann (1997), which was too low probably due to a more than a factor of 4 too large CO2 used (Gusev et al., 2006)). This consideration was also used in García-Comas et al. (2008). In other words, no satellite 15-μm measurements have been found so far which can be explained using a rate constant a factor of 4 smaller than the rate of Sharma and Wintersteiner (1990) and, therefore, we think that an uncertainty of ±75% is too pessimistic. We would like to stress here that the uncertainty assumed does not include the uncertainty in the atomic oxygen, which doubles the overall uncertainty involving these VT collisions.

RC: 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.

AA: As stated in the manuscript, MIPAS temperature retrievals are done using zonal mean CO2 abundance taken from WACMM. There are three reasons why we chose an uncertainty of 15%: 1) Examination of the variability with longitude in WACMM CO2 at 90 and 100 km for different latitudes and seasons is smaller than 10%, being maximum during the solstices. 2) Averaged WACCM CO2 vmr at 100 km lies within ±15% of the measurements of ATMOS, ISAMS, rocket-borne (López-Puertas et al., 2000) and ACE (Beagley et al., 2010) (once the profiles are scaled to the WACCM value at 60 km). Only CRISTA measurements (Kaufmann et al., 2002) are 40-50% smaller. At 90 km, ATMOS, ISAMS and ACE measurements are also within 15% of WACCM simulations. Rocket measurements are about 20% larger and CRISTA is here about 20% smaller. 3) A 15% uncertainty was the one used in Remsberg et al. (2008) and, since both instruments use the same technique, we decided that, for consistency, it was better to use the same value.

Nevertheless, we now also mention in the manuscript estimates of the error inferred by a CO2 uncertainty of 20%. We write now: “According to considerations in Remsberg et al. (2008) and comparisons with ACE (Beagley et al., 2010), ATMOS and ISAMS (Lopez-Puertas et al., 2000) measurements, we have assumed a 15% uncertainty. The induced Tk error is smaller than 0.1K below 70 km, 2K at 85 km, 3K at 90 km and 2K at 100 km. Nevertheless, comparisons of WACCM CO2 with rocket-borne measurements at 90 km and, particularly, with CRISTA (Kaufmann et al., 2002) at 100 km point to a slightly larger uncertainty (20% at 90 km and 40-50% at 100 km), which would lead to a Tk error smaller than 0.1 K below 70 km, 3K at 85 km, 4K at 90 km and 5K at 100 km.”

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

AA: That’s correct and tidal signatures at 35-60 km are discussed at lines 23ff. However, theses tidal signatures are small (i.e. a few K) compared to the mesosphere and are quite localized. Therefore, we state that the assumption that “the atmosphere does not change significantly within 12 h" does only hold at altitudes below approx. 60 km.

RC: 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.

AA: We included the following sentence: "Based on estimates supplied by J.M. Flaud (personal communication, 2008), we have assumed uncertainties of 3 to 5% in the intensity (depending on the vibrational band and the rotational quantum numbers) and 6% in the broadening coefficients."

RC: 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'.

AA: Even if the vibrational levels involved may be in vibrational non-LTE and high J rotational levels are in rotational non-LTE, the temperature from the OH spectrometers is the rotational temperature derived from selected 6-2 band lines originated from ro-vibrational levels which are not affected by rotational non-LTE. In the case of SATI, the temperature is derived from the Q1(1), Q1(2) and Q1(3) lines, which according to López-González et al. (2007), is similar to that obtained with P1(2) and P1(4). In the case of the Davis spectrometer, temperature is derived from P1(2), P1(4) and P1(5) (French et al. 2010). According to Sijvee (1992), those P1 lines are in rotational LTE. It is perhaps worth mentioning that the low-J levels may be in spin non-LTE (Dodd et al., 1994) but this does not affect the derived temperatures because the lines selected are taken from the same spin-orbit branch. Thus, we agree with the referee that it is not precise to say 'non-LTE free' and, therefore, we change the sentence slightly and we now wrote in the manuscript: "Temperatures derived from the ground measurements are not significantly affected by non-LTE." And also re-wrote the sentence after: "The temperature retrieved from the Davis and SATI spectrometers measurements use the information from the rotational structure from OH or from O2 emissions that are in rotational LTE.

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

AA: Time difference is indicated in hours. No action taken.

RC: 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, 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.

AA: We agree with the reviewer. The combined systematic error should not include errors coming from non-LTE parameters that have the same value in both datasets because, to first order, the sensitivity of MIPAS retrieved temperature to the non-LTE parameters is similar to that of SABER. We acted accordingly and recalculated the SABER-MIPAS combined errors and changed Figs. 5-8. We also included a paragraph in section 3.1 in the manuscript explaining it: "SABER and MIPAS temperatures are both derived from the 15 μm CO2 non-LTE emissions. Both use a non-LTE code to calculate the population of the emitting vibrational states. Both include in the code the same collisional processes. In order to estimate the combined systematic error, we have not taken into account the errors coming from those collisional rate values that are the same in both retrievals. Thus, we only included in the non-LTE error budget the contribution from the kvv rate and the atomic oxygen concentration uncertainties, and, for polar summer, also that of the kair rate."

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

AA: These are SABER retrievals for which the pressure was too large. We selected those retrieved profiles with pressures exceeding typical upper-mesospheric pressures by more than 50%. We now include this in the manuscript.

RC: 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.

AA: MSIS atomic oxygen is too low around the mesopause, but not in the polar summer. As we mention in (old) page 24256, "MIPAS O daytime abundance in the mesopause region is on average 50% smaller than the values used in SABER Tk retrievals, except for polar summer, where it is 50-70% larger". We now stress that the difference is with the NRLMSIS-00 model and changed the beginning of that sentence to: "The atomic oxygen daytime abundance used in MIPAS retrievals (taken from the MSIS model) in the mesopause region (. . .)". We have also included the reference to the Smith et al. paper in that sentence. Additionally, MIPAS comparisons with other instruments also suggest that the atomic oxygen used (from MSIS) is too small in the thermosphere. We pointed that out several times in the manuscript and, following the reviewer suggestion, we now mention explicitly in several occasions that the O source is NRLMSIS-00 (end of sections 3.2, 3.3 and the summary).

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

AA: Done.

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

AA: Done.

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