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Atmospheric Chemistry and Physics Discussions

Interactive comment on “$CO_2(\nu_2) - Oquenchingratecoefficientderivedfromcoincidental SABE$

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We thank Dr. Stephen Bougher for his analysis and comments on the paper. The responses to major and minor comments are given below. We marked the reviewer's and the author's comments by "**RC:**" and "**AC:**", respectively.

General comments

RC: . . .However, one additional reference is suggested. A few grammar issues exist; they are quite small and easily corrected.
AC: The suggested reference has been added and necessary grammar corrections have been made. Please, see the corresponding sections below.

Specific Comments

RC: The most important component for the CO_2-O k_{VT} rate retrieval in your method is

the average [O] density that you use! How do numerical calculations of climatological [O] densities (at the same location and season) compare with those derived from averaging the SABER observations?
AC: We fully agree with the statement above. That is why we provide the γ(z) profile so the k_{VT}(z) profile may be re-estimated in the future if the atomic oxygen profile is revised. By "numerical calculations" we assume that the calculations in numerical chemical models are meant. There are not many published values but the ones we looked at indicate a large range. O values from the TIME-GCM family of models (see for example Roble, 1995) are similar to the SABER values used here. O from the WACCM model (e.g. Marsh et al., 2007) are lower than SABER profiles whereas the O density maximum from the extended version (WACCM-X, see Liu et al, 2011) is as large as that seen by SABER. One difference in the two versions of WACCM is the treatment of molecular diffusion of gases. Experiments with the ROSE model (see Smith and Marsh, 2005) indicate that the O distribution in the upper mesosphere is sensitive to both eddy and molecular diffusion and that molecular diffusion acts to increase O. This suggests that the higher O in the MLT seen in the TIME-GCM and in WACCM-X, as opposed to the standard version of WACCM, have some contribution from the more accurate representation of molecular diffusion processes in those models. The WACCM group is currently investigating the discrepancy and its impact on trace gas distributions. Please, also see the answer to the first general comment of Reviewer 2 regarding the quality of current SABER V1.07 atomic oxygen product.

RC: Future work should also include similar studies utilizing SABER measured [O] averaged at other locations at different times and seasonal conditions in association with overlapping lidar measurements. This may be difficult, but it is necessary to confirm that the averaging technique you use yields similar k_{VT} rates regardless of location and season.
AC: Indeed, the suggested study is feasible and the corresponding activity was proposed to NSF. As the following table shows (http://dl.dropbox.com/u/44230060/lidars.pdf), there is a whole set of lidars all

over the world, which could be used for this study. As one can see, both high and low latitudes in the Northern and Southern hemispheres are covered. The mesopause temperature changes from 110 K for the polar summer to 220 K for the polar winter conditions (Lübken et al., 2009). Including the polar summer temperature profiles in MLT will help to expand k_{VT} atmospheric retrievals to low temperature values that has never been done before. Two rightmost columns in the table give an average number of SABER measurements with a tangent point within 2(4) degrees away from the corresponding lidar's location. The values in the table are, of course, the maximal estimates since weather conditions and operation of lidars do not provide round-the-clock coverage 365 days a year. For example, some of lidars can operate only in nocturnal mode that automatically reduces the number of available scans by half. Estimates for the Fort Collins lidar show that the ratio of selected SABER scans to total number of overflights in the "two degrees column" is 0.2. Assuming that the ratio for other locations is of the same order of magnitude one might expect about 90(350) scans per location for 2(4) degrees overlap for high-latitude lidars and twice as much for mid-latitude lidars for the whole period of SABER observations (2002–now). The total number of scans to be analyzed is, therefore, about 700(3000).

RC: The only detailed calculations to date related to α(z) come from Kharchenko et al. (2005). Quenching of hot [O(^{1}D)] largely by ambient O, O_2 and N_2 collisions likely produces hot [O(^{3}P)] in the 80-110 km region. The key issue here is the thermalization timescale for these hot [O(^{3}P)] atoms (about 1eV from the [O(^{1}D)] quenching) in both elastic and inelastic collisions with the ambient atmosphere (see Figure 8 of Balakrishnan et al., 1998). Very short thermalization timescales are likely in this 80-110 km region. Therefore, detailed photochemical/energy calculations of α(z) for Earth are needed to confirm whether your suggested mechanism (i.e. hot O collisions as a source of $CO_2(\nu_2)$ level excitation over 80-110 km) is valid or not. Some discussion of this is needed.
AC: We agree with this point and in the revised version of the manuscript we perform a simple estimate of the rate coefficient required to explain the observed behavior of

k_{VT}(z), and the fraction of hot oxygen atoms compared to the concentration of "thermal" atomic oxygen. We write that we consider the suggested excitation mechanism to be probable but it is not the "ultimate truth".

RC: Summary section. The significant impacts of this CO_2-O k_{VT} rate upon heat balance calculations of other planets and their dayside temperatures (e.g. Venus and Mars) is not discussed, but should be.
AC: We think that this information is more appropriate for the introductory section so we have added there a corresponding sentence and the reference suggested above.

RC: These planetary "laboratories" (particularly Venus) can provide insight into the solution of this problem at Earth. A self-consistent treatment of this CO_2-O VT rate across Earth, Venus and Mars upper atmospheres is needed. In short, your call for additional studies should include these planetary upper atmospheres, and the radiative cooling calculations of all three of these upper atmospheres in general circulation models (i.e. Bougher et al., 1999) should be performed in accordance with the fractionizing you define in equation 4. The fraction (α) of total O(3P) density which corresponds to hot atoms will need to be re-evaluated for each planetary upper atmosphere, especially in those regions where CO_2 cooling serves as the primary IR radiator (see Bougher et al., 1999).
AC: We agree with this comment. This will be a natural course of study of this phenomenon. The number of satellite missions increases and more information about planetary atmospheres becomes available. However, there are certain technical difficulties, which will complicate the study for the atmospheres of Mars and Venus. Examining the vertical behavior of k_{VT}(z) requires knowledge of the vertical distributions of O(z), T(z), and $I_{15\mu m}$(z) for different latitudes and seasons measured simultaneously in the area where the non-LTE effects are noticeable (>80 km for Mars). At the same time, T(z) used in the analysis must not be retrieved from the $I_{15\mu m}$(z) emissions and should represent an independent "reference" data set like the one provided by the lidar in the current study. From this point of view, stellar occultation instruments (e.g. SPICAM)

are good candidates since these instruments observe the absorption from the ground state, which is not affected by non-LTE effects.

Technical Corrections

RC: Pg. 32586, line 5: ".. frequency of collisions is lower and the vibrational level populations...."
AC: Fixed

RC: Pg. 32587, line 25: "...uncertainties in the k_{VT} coefficient."
AC: Fixed

RC: Pg. 32588, line 22: "...It is important to choose...."
AC: Fixed

RC: Pg. 32590, line 14: "..shown in Fig. 2c fit well..."
AC: Fixed

RC: Pg. 32591, line 22:"...which seems justified usage of..."
AC: Fixed

RC: Pg. 32592, line 2: Do you instead really want (1-α) in the first term of equation 4? Or is there an embedded sign in the term that I do not see?
AC: This was a misprint, thanks.

RC: Pg. 32592; line 12: Same as above regarding the factor of (1-α) in term 1 of equation 5?
AC: Same as above. Fixed.

RC: Missing key reference that is needed: Bougher, S. W., S. Engel, R. G. Roble, and B. Foster, Comparative Terrestrial Planet Thermospheres : 2. Solar Cycle Variation of Global Structure and Winds at Equinox, J. Geophys. Res., 104, 16591-16611, (1999).
AC: We have added the suggested reference.

Additional references

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Marsh, D. R., R. R. Garcia, D. E. Kinnison, B. A. Boville, F. Sassi, S. C. Solomon, and K. Matthes (2007), Modeling the whole atmosphere response to solar cycle changes in radiative and geomagnetic forcing, J. Geophys. Res., 112, D23306, doi:10.1029/2006JD008306.
Roble, R. G. (1995), Energetics of the mesosphere and thermosphere, in The Upper Mesosphere and Lower Thermosphere: A Review of Experiment and Theory, Geophys. Monogr. Ser., vol. 87, edited by R. M. Johnson and T. L. Killeen, pp. 1–21, AGU, Washington, D. C.
Smith, A. K., and D. R. Marsh (2005), Processes that account for the ozone maximum at the mesopause, J. Geophys. Res., 110, D23305, doi:10.1029/2005JD006298.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 32583, 2011.