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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "$CO_2(\nu_2) - O quenching rate coefficient derived from coincidental SABER/TIMED and Fo$

Anonymous Referee #4

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The paper "CO2(v2)-O quenching rate coefficient derived from coincidental SABER/TIMED and Fort Collins lidar observations of the mesosphere and lower thermosphere" by Feofilov et al. addresses one of the mysteries in remote sensing of the upper mesosphere and lower thermosphere region: the noticeable disagreement between NLTE model calculations of carbon dioxide utilizing collisional rate constants measured in laboratory and limb observations of carbon dioxide 15 micron radiation. There is an ongoing discussion about the reasons for this discrepancy, such as the rate constant (kvt) and its temperature dependence for collisions of carbon dioxide with atomic oxygen, the atomic oxygen abundance itself, or the carbon dioxide volume mixing ratio.

Several studies have been published addressing this problem in terms of temperature deviation between measurements and different parameter choices. This study utilizes coincident measurements of carbon dioxide 15 micron radiation and Lidar tempera-

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Interactive Discussion

Discussion Paper

tures to derive the kvt rate constant itself, which is a different approach. To my opinion, the advantage of this approach is the possibility to obtain kvt along with its error margins - at least for model assumptions made in this study.

My major objection to this work is the omission of several systematic uncertainties in this study. These are the atomic oxygen abundance, the carbon dioxide volume mixing ratio, radiance uncertainties (affecting the upwelling radiation), the temperature dependence of the rate constant, or uncertainties of the Einstein coefficients. These uncertainties should be addressed in much more detail!

Another point not addressed in the paper is the temperature dependence of of the SABER atomic oxygen retrieval. The k(O+O2+M) rate constant utilized in the atomic oxygen retrieval has a noticeable temperature dependence, such that the choice of the temperature profile in this retrieval has some impact / feedback on the atomic oxygen profile. Was this effect estimated or considered in your work?

The carbon dioxide volume mixing ratio profile is another quantity relevant in the context of this paper. The authors should show the profile utilized in this study, since the reference (Rezak, 2011) is a PhD thesis, only. What are the uncertainties of the carbon dioxide volume mixing ratio profile utilized here, such as the NLTE processes involving O(1D) and its production, day-night extrapolations, or radiance uncertainties in the SABER 4.3 micron channel?

As stated above, all of the these uncertainties should be considered in the non-linear fitting of the rate constants and expressed in the confidence limit of the fitting parameter.

Another, more formal point, is the neglect of some references, such as Remsberg et al. [2008, JGR] or Carcia Comas et al. [2008, JGR]. The authors should put their results in the context of this work as well.

Finally, the introduction of a new quenching process involving highly excited oxygen

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atoms lacks a discussion about its relevance. The authors should estimate its impact quantitatively or shorten this paragraph significantly.

In summary, this article is acceptable for publication after major revisions, as indicated above.

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

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