

Dear Franz-Josef,

please see below our replies to referees' reports.

We marked there precisely the lines in the revised manuscript where we address each particular comment.

In the revised manuscript edited or new text is given in bold. This includes three new sub-sections as well as those sections from the previous manuscript version, where edited text exceeded 70%. Since the manuscript is prepared in latex it was technically not possible to mark any new word or any small sentence part changed.

With warmest regards,  
Alex Kutepov

### **Replies to the report of Anonymous Referee #3**

We thank this reviewer for comments to your manuscript. Below these comments are given in italic followed by our replies.

*The authors have addressed my comments and suggestions but very little changes have been taken into account in the revised version, eluding the most important calculations. Also, I do not like the tone of the response, e.g. sentences like "We suggest the referee read more carefully the LP04 paper ..." or "... his/her strange way of thinking..." or "We invite the referee and his/her group to make their own research contributions, given the new knowledge" are completely out of place and should be omitted. That said, I have tried to be as objective as possible.*

We are very sorry if our replies to this reviewer comments looked inappropriate.

*I think the paper deserves to be published. I am making the less possible comments and are divided in: a) essentials (without which I would not recommend the publication) and b) important (highly recommendable). I also include a comment on the authors' comment at the end that do not affect the manuscript but, since this report will be public, I feel I should reply.*

We changed the title and the text of paper to satisfy all *essential* and *important* comments of this reviewer except of comments 4 and 5. We believe that the requirements in Comment 5 are in contradiction with those in Comment 4 (please see detailed replies to comments 4 and 5 below).

*One of the major points comes from the interpretation of the sentence in LP04 of "...We have investigated the SABER 4.3  $\mu\text{m}$  radiances with the help of a non-LTE radiative transfer model for CO<sub>2</sub> and found that the large radiances can be explained by a fast and efficient energy transfer rate from OH(v) to N<sub>2</sub>(1) to CO<sub>2</sub>(v<sub>3</sub>), whereby, on average, 2.8–3 N<sub>2</sub>(1) vibrational quanta are excited after quenching of one OH(v) molecule." We may question if this mechanism is realistic or not but it is a mechanism and LP04 were able to reproduce the radiances for essentially all conditions (e.g. for 4 orbits of 4 days covering equinox and solstice (Figs. 12-14)) within +/-20%, that is, equally or better than with the new mechanism.*

In the new manuscript version we removed any discussions of which model and how well it **fits**

SABER measurements. The paper deals now only with the comparison of various model calculations using WACCM self-consistent inputs of all atmospheric parameters. Although SABER measures emissions in three channels, and now also ground and space observations of OH(v) distributions are involved in the discussion, they are given only as reference data to illustrate how well various model reproduce basic features of measurements.

*True that so far there is no evidence of such a "direct" energy transfer process and this is why this manuscript is relevant, because it gives a plausible "indirect" way of such an energy transfer, even though the actual reaction rates have not been measured (only estimated) and the efficiency of OH(v) to O(1D) has not been measured either. Hence, I do appreciate this qualitative new mechanism.*

We are puzzled with this statement. The comment 4 below shows, that reviewer is aware of the works by Sharma et al 2015 and Kalogerakis et al 2016. In the latter paper reaction rates for both OH(v)+O3P and OH(v)+O1D were measured. We use results of this measurement in our calculations.

*That is why I suggested to focus (title, abstract, etc.) on the new pathway more than on if it is able to reproduce better or not the measured SABER CO2 4.3 μm radiances. Then, my comments:*

We followed this suggestion and show now the work of new pathway in comparison with other models. Therefore, we show observation results only for illustration of how well various models reproduce basic features of different measurements.

*1) Title: (important). I would still recommend changing the part "New model calculations improve agreement with SABER observations".*

We changed the second part of the title. It is now neutral saying only that we compared various models with observations: **Comparison of the CO2(v3) and OH(v) emission models with space and ground based observations**

*2) Abstract (important): If with the "previous study" the authors refer to "Kumer et al." it is correct. However if to LP04, it is also correct but not complete (see above). My recommendation would be to give the whole story not only part of it.*

It is barely possible to tell the whole story of previous studies in the abstract. We do this in Introduction. However, both in Abstract (lines 1-5) and Introduction (lines 16-23) we underscore an importance of the LP04 study who, for the first time, quantified required efficiency of the OH(v) energy transfer to N2 to fit 4.3 μm observations.

*3) Motivation of the work (clarification, not relevant for the manuscript). I tried to say that this work is important itself and does not need the additional justification of retrieving CO2 at night. Of course, in no way I meant to discourage the authors in pursuing such research.*

We removed from the text any mentioning of our motivation for this study. The paper deals now only with comparison of various models.

*4) (Essential). Since there have been already two papers dealing with this new mechanism (Sharma et al. (2015) and Kalogerakis et al. (2016)), I think this work should make the most accurate and*

*consistent calculations as possible, making use all available SABER data in a consistent and proper way. That it, in my opinion is not valid the argument that the purpose of this work is to make "estimates" and further work will be done later. Hence:*

This comments is addressed below together with comment 5)

*4a) They should use the retrieved CO<sub>2</sub> from SABER (contrary to their reply, CO<sub>2</sub> is publically available, ([ftp://saber.gats-inc.com/Version2\\_0/Level2C/](ftp://saber.gats-inc.com/Version2_0/Level2C/)) and two of the co-authors are co-authors of the CO<sub>2</sub> retrieval papers (Rezac et al., 2015a,b).*

We would like to remind here that Rezac et al, 2015 retrievals were performed only for daytime SABER observations. In this study we modeled nighttime conditions. Additionally, retrievals of Rezac et al, 2015 use the SABER retrieved O<sub>3</sub>P which is considered to be too high compared to other observations (see also comment 5). To avoid a risk of using inconsistent inputs we switched to WACCM based inputs.

*4b) My previous major comment on the OH SABER radiances (see below) has not been addressed adequately. "As the new proposed mechanism affects also to the population of OH(v) and the emissions from these levels were measured SABER in two different channels, I think it is essential that the authors demonstrate that the new OH(v) model explain very well the measured SABER OH radiances, as LP04 did. Thus, figures for different conditions with the SABER observations and modelled radiances for the two OH SABER channels should be presented in this work."*

We followed this request and show now comparison of our model with SABER OH(v) emissions (new Figure 3, lower row, the discussion is given in the new Section 3.4 “ **The OH 1.6 and 2.0 um emissions**”).

*Thus, their replies of: "... but this is clearly out of the scope of this report." or "The goal of our study was to estimate, ..." I think it is crucial for this manuscript (not report), and I think it should be something more than an estimate, the title of the work reads "New model calculations improve agreement with SABER observations"*

We are not sure we understand this recommendation: in one of the previous comments we were suggested to change the title, while here it is suggested to improve the paper to make it more consistent with the original title.

As we replied above, the title was changed to be consistent with the revised paper which deals exceptionally (as it was suggested by this reviewer) with comparing new mechanism with other models.

*5) (Essential, in the same line as point 4). Atomic oxygen is key for the new excitation mechanism, therefore all models inputs and SABER radiances should be consistent. Thus, my previous point on this topic has not been adequately addressed. Taking from my previous report:*

*"About the O(3P) abundance and the OH(v) model, the authors state that they used the O(3P) retrieved from SABER measurements. The SABER O(3P) is derived from the SABER OH radiances but a photochemical OH(v) model is required for such inversion (Mlynczak et al., 2013)." The authors should use the same photochemical OH(v) model or prove (with calculations and figures) that they are consistent. Further on this topic, several works have shown that SABER atomic oxygen might be overestimated in a ~30%. It would very important to comment, how would this affect to the simulations of SABER CO<sub>2</sub> 4.3 μm nighttime radiances with this new mechanism?*

This comment is in contradiction to comment 4, which requires to “*make the **most accurate and consistent calculations as possible, making use all(!) available SABER data in a consistent and proper way.***” On the other hand, comment 5 is about an internal inconsistency of SABER retrieval products: O3P is supposed to be too high, indicating that the OH( $\nu$ ) model currently applied for its operational retrieval may require update. In this study, we apply new research for the OH( $\nu$ ) model, which utilizes new OH( $\nu$ )+O3P mechanism. The latter is missing in the current operational model. Therefore, using any SABER inputs to test this new model would be not logical. To be on a safe side, we show the calculations based exceptionally on inputs for night-time atmospheres obtained from the WACCM model.

*Further on this topic, several works have shown that SABER atomic oxygen might be overestimated in a ~30%. It would very important to comment, how would this affect to the simulations of SABER CO2 4.3  $\mu\text{m}$  nighttime radiances with this new mechanism?*

Actually, this topic was discussed in the initial version of this manuscript. Since results we present now are based exceptionally on the WACCM inputs we do not address this point.

*6) Sec. 3.2 and Fig. 2 (Essential). I cannot see the reasoning of why using only the partial result of LP04. Do the authors want to validate their model? I see a high risk to misleading the reader as giving the impression that LP04 were not able to explain the SABER radiances when they did (see Figs. 10 and 11 in LP04). I strongly recommend to either show the two LP04 simulations or none.*

In the revised version of Fig.2 we show how well we reproduce both initial and final (with the 3 times higher efficiency) results of LP04. We discuss in detail **both** results and compare them with other models and measurements:

- in new section 2.3, where we describe in detail all models we used in our calculations, p4, lines 25-33, p5, lines 11-13,
- in section 3.1, p5, lines 26-29, p5, lines 1-5
- in section 3.2 which discusses revised Fig.2 and 3, nearly a half of text is about results of LP04 and their comparison with other models.
- In Conclusion p10, lines 30-34

In all these discussions we stress several times that LP04 were able to reproduce SABER 4.3  $\mu\text{m}$  measurements with their final model as good as we do it with our new model based on the new “indirect” mechanism.

*7) (Essential) About the OH densities. If the authors calculate OH( $\nu$ ) (does this include also  $\nu=0$ ?) from SABER O3 and H, why they need OH (ground state? or total (i.e. OH( $\nu$ ) including  $\nu=0$ ) from WACCM? If this is important, my previous argument whereby WACCM should sub-estimate OH still applies. As mentioned above, the authors should use a consistent model and inputs for all quantities.*

As it was already discussed above we use in a new version of manuscript exceptionally WACCM inputs.

*8) (important) Conclusions. As mentioned at the beginning I would focus more on the mechanism itself (find the way to transfer so much energy from OH( $\nu$ ) to N2( $\nu$ ) rather than on the "improve agreement" of the SABER radiances.*

Again, following this recommendation we focused now on the study of new mechanism and its comparison with other models of OH(v) relaxation. However, this theoretical study without any comparison with reference measurements would be quite useless.

Therefore, we show SABER and other measurements, compare calculations with them, and discuss how various models reproduce main features of observations.

*I do not fully agree with your statement that "(b) everything else that follows in our conclusions is rather measured ..." You need to make "estimates" of key parameters as how much energy is transferred from OH(v) to O(1D), both on the collisional rates and their temperature dependences.*

This comment is appropriate (as a short note publication, for instance) to the papers by Sharma et al, 2015, and Kalogerakis et al 2016, where "estimates" of these key parameters was performed. It can be hardly addressed to this work where we use results of these studies to show how strong they may influence the 4.3 um emission modeling.

*Just one clarification to your statement:*

*• LP04 also showed that  $f \sim 3$  (possible multi-quantum process, but mechanism is not explained) removes about 40% of differences for some selected scans studied. In our very extensive study we found that these differences can reach as high as 80%. Following the LP04 logic it would require  $f=6$  and higher to remove these differences, which is absolutely unrealistic. In other words, LP04 quantified the energy transfer efficiency that would be required for model calculations and observations to agree (for some limited set of scans), but no detailed mechanism was described, let alone validated by referring to theoretical or experimental investigations. This is not fully true. LP04 showed, not only for some limited scans but for 4 orbits of 4 different days covering equinox and solstice conditions, that SABER measurement with  $f=2.8-3$  could be reproduced within +/-20% (see Figs. 12-14). I would not be surprised to need larger  $f$ -values for the polar regions, where auroral excitation is prone and frequent. Also, if you analysed so many data, it would be very useful to the reader to show more than just only 2 days (Fig. 3).*

In the new version we removed any discussion regarding **fitting** SABER measurements, what efficiency needed for this, etc. The paper deals now only with comparison of various models using WACCM self-consistent inputs of all atmospheric parameters. Although SABER measurements in three channels, as well now also ground and space observations of OH(v) distributions are involved in the discussion, they are given only as reference data to discuss how well different models reproduce main observational features.

## **Reply to the report of Anonymous Referee #2**

Below the reviewer's comment is reproduced in italic and is followed by our reply.

*The authors have addressed satisfactorily most of my comments, however, I still think that the paper would benefit substantially from a figure that shows the modeled and observed OH(v=8,9) radiances (SABER channel 8), similarly as Fig. 2 does for CO<sub>2</sub> 4.3 um (SABER channel 7). The point is that with the proposed mechanism the excitation of CO<sub>2</sub> is ruled by the number density of the excited OH states and hence a rigorous test would be to compare both OH\* and CO<sub>2</sub> radiances with the SABER observations. The authors state in their reply that such a consistency test has already been performed, so why not including it in the manuscript?*

We are very grateful to this reviewer for his friendly comments, which helped to improve this manuscript.

In revised version of manuscript we completely accounted for suggestions above and compare in new Fig.3 both CO<sub>2</sub> and OH emission calculations with SABER measurements. Detailed discussion of these results is given

- in p.7, lines 13-23 (CO<sub>2</sub> emissions)
- in new section 3.4 p.9, lines 20-33 and p.10, lines 1-12. (OH emissions)