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## ***Interactive comment on “Implications of the O + OH reaction in hydroxyl nightglow modeling” by P. J. S. B. Caridade et al.***

### **Anonymous Referee #1**

Received and published: 18 April 2012

The discussion paper on de-excitation of vibrationally excited OH by Caridade et al. is interesting and fits into ACP, although not situated in the “thematic barycenter” of this journal. The paper is, as far as I can judge, scientifically sound, and the contents are well presented. Numerous approximations are made but these are all explicitly stated and critically discussed. I recommend publication of this article in ACP after consideration of the following specific and technical comments.

Specific and technical comments:

On the whole the work is well placed into the context of existing work, with a lot of references. However, some more recent articles may also be relevant in this context, e.g. Xu et al., J. Geophys. Res. 117, D02301, 22 pp., 2012 doi:10.1029/2011JD016342, and possibly references therein.

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p 6486 line 9: This statement is a bit vague. Please specify the “Other limiting cases”. If this refers to the “sudden death” and “collisional cascade” limiting cases: These technical terms are well enough known that they can (to my judgement) be used in the abstract without definition.

p 6484 line 16: I suggest to delete “that are critical for the sustainability of life on Earth”. While this certainly is true, this sounds a bit lurid and does not really help the paper. I guess that more than 99.9 percent of ACP articles treat atmospheric regions which are in some way critical for the sustainability of life on Earth.

p 6486 line 22: Is the Earth’s “upper atmosphere” meant here (“...low pressures found in the upper atmosphere”)? I first understood it this way, but the next sentence makes a statement on the first application to the terrestrial atmosphere, suggesting that in line 22 still something else is meant, and I was left confused. I guess that indeed the Earth’s atmosphere is meant (because otherwise the restriction to the upper atmosphere would make no sense, e.g. on Mars the pressure is already low near to the surface). To make a long story short, I suggest to modify line 22 to “...found in the Earth’s upper atmosphere”.

p 6487 line 1: “physical chemistry properties” sounds a bit funny to me. While pressure and temperature certainly have influence on the chemistry, these are physical properties.

p 6487 lines 7/8: In this context the statement is a bit too general. It should be restricted to atmospheric chemistry models (chemistry-transport models and chemistry-climate models. Other models, like radiative transfer models, often include non-LTE.

p 6487 line 12/13: I do not quite understand this: If we had accurate knowledge on the excitation and de-excitation rate coefficients, couldn’t we accurately model the required vertical profiles? Is this really “another reason” or is this just another view on the same reason? Or does the statement refer to other species? Please clarify.

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p 6488 line 5: “that any ... does not”. The grammar of this sounds a bit funny to me and I would say “that no relaxation process contributes...”. However, since I am not a native English speaker, all my language recommendations should be used with care.

p 6488 line 14: I am not familiar with the term “rationalization” in this context (and elsewhere in this paper). If this is a technical term I am just not familiar with, then it is ok; otherwise, I have seen the term “fractionation” to describe which fraction is in which state.

p 6489 line 4-7: I do not find that the use of the AG data is criticized by von Clarmann et al. They just discuss what would happen if the AG data would be used. They draw no conclusion which data set is superior.

p 6489 line 23: Not sure if “masterpiece” is the correct term here. “key issue”?

p 6491 line 19: I suggest to remove the comma after QCT.

p 6492 line 9: adverb: roughly T-independent

p 6492 lines 12 and 14: As said above, I have some problems with the term “rationalize”.

p 6492 line 27: reword: theoretical data of this work.

p 6494 line 6: Not sure if the expression  $HO_{y+3}$  is widely known, and the reader might not wish to consult the related reference. Could you state in a few words what  $HO_{y+3}$  is about?

p 6494 Eq 3: It took a while until I figured out that the subscript CHV are the initials of the authors of the discussion paper, and that it is used to distinguish the assumption made for Eq. (3) from the other assumptions. I suggest to make clear in the text that the subscript refers to the assumptions made. It becomes more obvious in the context of Eqs 4 ff but clarification already in the context of Eq 3 would be helpful.

p 6502 line 1 “rationalization” see above.

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p 6502 around l13: I have a problem to understand the message of these lines: I assume that these tests have been made to demonstrate that the assumed steady-state concentrations are reasonable. Thus, they are fed into a time-dependent model. I do not quite understand why the steady-state assumption is considered justified if the concentrations change their values at all. If steady state is valid, I would expect that integration of the reactions over time would not change anything. I would expect flat lines. Or is this, because you initialize with  $[\text{OH}(v)]_{\text{CHV}}$  but integrate for  $[\text{OH}(v)]_{\text{CC}}$ , which results in another set of steady-state concentrations? Perhaps the use of the term “steady state” in this manuscript is ambiguous: Sometimes it is used to specify the  $[\text{OH}(v)]_{\text{CHV}}$  distribution (probably p6502, line 11), and elsewhere it is used in a more generic sense, whenever steady-state theory is used. Perhaps it is this ambiguous use of the term which confuses me. Or have I misunderstood anything?

p 6512 Fig 2: Please use larger legends; also the legends of the other figures may be a bit too small when the figures are shrunk to one-column ACP format. Or make sure that the other figures are reproduced as 2-column figures.

p 6517 Fig 7: The axis caption  $10^4 t/s$  is certainly correct but this way to report the magnitude is not very common. Could you use milliseconds or microseconds for the time axis?

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 6485, 2012.

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