

## ***Interactive comment on* “Sensitivity studies of the recent new data on O(<sup>1</sup>D) quantum yields in O<sub>3</sub> Hartley band photolysis in the stratosphere” by N. Taniguchi et al.**

### **Anonymous Referee #1**

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### **General comments**

The paper investigates the impact of new data on O(<sup>1</sup>D) quantum yields on stratospheric chemistry. This topic is of interest to the readership of *ACP* and quantitative results are presented. The paper could therefore make a contribution to current problems in (upper) stratospheric chemistry. However, in the present form of the paper, the presentation makes it difficult to clearly understand the points being made. Moreover, incomplete information on the studies and model runs that have been conducted makes it difficult to assess the quality of the presented results. I believe this could be an interesting paper if the presentation is reworked and more information on the model

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studies would be provided.

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## Specific comments

- The paper cites the recommendation of JPL 2003 once in the introduction but otherwise ignores the existence of the new compilation. It is of course not easy to be confronted with a new compilation in the middle of a study of that kind. Nonetheless, the reader needs to know if the comparison presented in the paper between JPL 2000 and the 'new data on  $O(^1D)$  quantum yields' is essentially a comparison of JPL 2000 vs. JPL 2003 (where the new data are adopted). This is at least how I understand the paper. If this is *not* the case, the reader needs to be told this as well. I guess such a clarification could be achieved without further model runs.

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- I read the paper as making the statement that the ‘new data’ are of no importance for tropospheric chemistry. If this is the case, this should be a) clearly stated and b) the discussion of tropospheric chemistry (introduction!) in the paper be removed. If this is not the case, the impact on tropospheric chemistry should be roughly quantified.
- Much of the scientific content of the paper rests on the model runs that have been conducted. Yet the model is neither described in a reference nor is it at all adequately described here. Apparently modules of the Garcia and Solomon model have been used to build a new model. Is this correct? – So which parts are ‘new’? The actinic flux calculations? Or any transport formalism? No feature of the new model is described in the paper as it stands and the only references are to the Solomon et al. paper and JPL compilations. Of course it is completely acceptable to just use an established model such as the Garcia and Solomon model. However, if this is the case, it needs to be clearly stated in the paper.
- Similarly, the actual model runs are not well described: Only from the figure captions we learn that they are for March at 40° – presumably northern hemisphere?. But for which year are the initial Cl<sub>y</sub> values? How appropriate are the initial values that were chosen. What is their impact on the results? And what is meant with ‘day-night average condition’?

### Some further detailed comments

- Introduction: the major impact of the new data what regards a scientific issue seems to be the ozone deficit problem. Thus the ozone deficit problem should be the focus of the discussion in the introduction and not the oxidation capacity of the troposphere for which the new data are not of major interest.

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- P. 2333, l. 11: ‘lower and upper stratosphere’ could be understood as ‘throughout the stratosphere’. This is not correct.
- P. 2334, l. 21: remove ‘the’ in front of Matsumi.
- P. 2336, l. 10: Do the Malicet data correspond to standard recommendations? If yes, it should be stated, if no it should be explained why the recommendation was not followed.
- P. 2340, l. 25: ‘probably activated’ and similar statements in this paragraph sound rather speculative. It should be possible to find out what definitely the relevant chemical mechanisms are that act – at least in the model.
- P. 2341, l. 14: It should be stated here which JPL recommendations were used by Crutzen et al. (1995) and by Grooß et al. (1999).
- Conclusions: The last sentence is not very clear; more importantly, I believe that this is the place to clearly state the field(s) where the new data are of relevance. From my reading of the paper I think it is the ‘ozone deficit problem’.

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Interactive comment on Atmos. Chem. Phys. Discuss., 3, 2331, 2003.

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