

## ***Interactive comment on “Chemical contribution to future tropical ozone change in the lower stratosphere” by S. Meul et al.***

### **Anonymous Referee #2**

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General Comments. . . This paper addresses the chemical contribution to future tropical ozone change in the lower stratosphere. The authors have done an exceptional job detailing the chemical processes that impact odd-oxygen loss due to climate change and reduced ODS abundances in the future. They have used the EMAC chemistry climate model and two methods for deriving ozone budget information (i.e., ProdLoss and StratO3Bud). This work is acceptable as is from publication in ACP. I have a few comments below that the authors may want to consider.

Abstract: General – I really like this paper, but the abstract needs to be rewritten. Please tell the reader why this is something new and worth their time to read.

Lines 5-10. “The dominant role of transport for future ozone changes is confirmed, but

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it is found that changes in ozone destruction and especially changes in the production of ozone do contribute to the relative ozone changes in the tropical lower stratosphere.” The authors should be more quantitative in what “do contribute” means.

Lines 12-16 “It is shown that changes in the production are mainly due to changes in the overlying ozone column which are determined by both chemistry and transport. Changes in the ozone destruction can be attributed to a modified efficiency of the ClOx and NOx loss cycles in the lower and middle stratosphere and of the HOx loss cycle in the lower- most tropical stratosphere.” Before reading the paper I could have stated this sentence. If you are going to state the obvious, at least back it up with the paper results.

Lines 16-19. “The role of ozone transport in determining the ozone trend in this region is found to depend on the changes in net production, with a smaller contribution from transport to the ozone changes if the net production decreases.” If I hadn’t read the paper, I would have no idea what you are talking about. You need to expand on this sentence.

Main text. . . Line 25 – if you are going to mention, “super recovery” – please define it.

Line 62 – typo. . . “carbon dioxide”

Line 68 – “The impact of a temperature decrease on ozone is found to result primarily from the acceleration of the three-body reaction  $O + O_2 + M \rightarrow O_3 + M$  which leads to a changed partitioning of odd oxygen in favor of ozone instead of atomic oxygen (Jonsson et al., 2004).” I believe Rosenfeld et al. discussed this issue first. See JGR, VOL. 107, NO. D6, 4049, 10.1029/2001JD000824, 2002.

Lines 79-81. “Additionally, a change in the ozone column above a certain layer will modify locally the downward flux of shortwave (SW) solar radiation and will therefore affect the ozone production via photolysis in the underlying layers (e.g., Revell et al., 2012).” You are talking about the self-healing process (or reverse self-healing). This

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has been discussed in the ozone perturbation literature for decades. I'm not exactly sure who first discussed this process, but Revell et al., would not be my choice for a reference.

Line 125-128. "Natural forcings such as solar variability (e.g. the 11-year solar cycle), the QBO, ENSO or volcanic eruptions are not included in the simulations." I understand why you would leave out solar variability, volcanic eruptions, and even ENSO. However, in model simulations I've seen that include a QBO, the secondary circulation does strongly modify the tropical vertical velocity and affect the isolation of the tropics from the mid-latitudes; essentially modifying the base transport of the tropical stratosphere. Since you mention later (line 264) that... "a significant enhancement of the tropical upwelling in the lower stratosphere between 2000 and 2095 can also be identified in the timeslice simulations with EMAC used in this study (not shown). This leads to a stronger net export of ozone in the tropical lower stratosphere." I wonder if your results would be significantly affected if you ran timeslices with a repeating QBO. I realize this is most likely beyond the scope of this paper, but if you have any idea on how NOT including a QBO would impact this study it would be interesting to mention.

Line 188. "Therefore, the reactions of  $\text{NO} + \text{HO}_2$  and  $\text{NO} + \text{CH}_3\text{O}_2$  are considered for ozone production in StratO3Bud." Just a note: I believe that Johnston and Kinnison also showed that  $\text{NO} + \text{HO}_2 \rightleftharpoons \text{OH} + \text{NO}_2$  has a null cycle component; that is, not the entire rate for this reaction goes to odd-oxygen production. That is if the OH reacts with CO, odd-oxygen is produced. If OH reacts with O<sub>3</sub>, it is a null process for odd-oxygen production.

Page 8 and Figure 3. I really like how you broke down the R\_T, R\_P, and R\_D in figure 3 using the three time slice simulations. Why not bring some of the quantitative details forward from discussion of this figure into the abstract? This is clearly what is new about this study.

Line 366, "As in the lower stratosphere..." – did you mean "In the lower

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stratosphere..."? Or are you talking about 30-10hPa region, which you mention in the next sentence? Please clarify.

Page 11, line 369. There is no Figure 4c, you must mean Figure 4b.

Line 370, "Here, two processes might balance each other: on the one hand the increased tropical upwelling reduces the release rate of NO<sub>y</sub> (see above). On the other hand a reduced formation of the reservoir species chlorine nitrate due to the decline in chlorine will increase the NO<sub>x</sub> abundance (not shown) and compensate the effect of a reduced release." This seems too speculative for this paper. Can you not be more quantitative – I never like reading words like "might".

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 27855, 2013.

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