We thanks a lot for your comments, the manuscript is revised accordingly and following is the reply (in black) to your questions (in blue).

The manuscript investigates the influence of stratospheric processes on tropospheric OH. The authors apply a chemistry-climate model with an ozone tagging approach and a perturbation experiment to quantify the contribution of stratospheric ozone to tropospheric OH. The manuscript presents an interesting and potentially valuable approach. However, revisions are needed to strengthen the quantitative interpretation and provide more detailed chemical mechanism analyses. I recommend publication after major revision.

Major comments

(1) The current approach quantifies the impact from stratospheric ozone intrusion, rather than the full influence of stratospheric intrusion processes. Other chemically relevant stratospheric transported species (e.g., NOy) are not considered. I suggest revising the manuscript title and relevant descriptions accordingly. For example, “stratospheric ozone intrusion contribution” would more accurately describe the calculated quantity than “stratospheric intrusion contribution.”

R: We agree with this comment and the term “stratospheric intrusion” is replaced by “stratospheric ozone intrusion” in the revised manuscript.

(2) The manuscript would benefit from a more quantitative analysis of OH chemical budgets, including the relative contributions of OH production and loss pathways. At present, many interpretations are based on qualitative comparisons among StrOH, ozone, humidity, and radiation. However, these relationships alone are insufficient to establish the dominant controlling mechanisms. Additional diagnostics, such as OH production/loss budgets would strengthen the conclusions.

R: In the revision, the latitudinal variations in zonal-mean, mass-weighted tropospheric mean StrOH production and loss rates along various pathways are presented. According to the results, StrOH production is dominated by the first production reaction, $StrO_3$+hv, in the tropics, which accounts for about 73%. At latitudes beyond 75°, the $StrO_3$+HO_2 pathway is more important and is about twice that of the first reaction. On global average, the two production pathways contribute about 79% and 21%, respectively, while the loss pathways via reactions with CO, CH_4, CH_2O, and others contribute about 32%, 20%, 5%, and 43%, respectively. These descriptions are included in the revised manuscript.

Specific comments

(1) Line 214: The authors calculate mass-weighted tropospheric OH concentrations, while Figure 2 presents volume-weighted OH concentrations. I recommend applying the same weighting method for both model evaluation and subsequent analyses.

R: In the revision, the mass weighting method is applied in all analysis and figures.

(2) Line 233: The authors attribute the secondary maximum of OH below the tropical tropopause in EAC4 to lightning NOx emissions. The authors should provide the corresponding vertical distribution of lightning NOx emissions or NOx concentrations to support this explanation.

R: EAC4 does not provide lightning NOx emissions. The following figure shows the zonal mean EAC4 NOx distribution averaged over the period 2006-2010. It is clear that there is a maximum in the tropical upper troposphere, which should originate from lightning emissions. However, this figure is not included in the revised manuscript because it is not the focus of this study. If you think this is necessary, we can include it in a future revision. The corresponding sentences are revised to

"A second maximum occurs in EAC4 OH concentration just below the tropical tropopause, which could be related to lightning NO_x emissions (there is a maximum in the zonal mean EAC4 NO_x distribution in the similar region, not shown).".

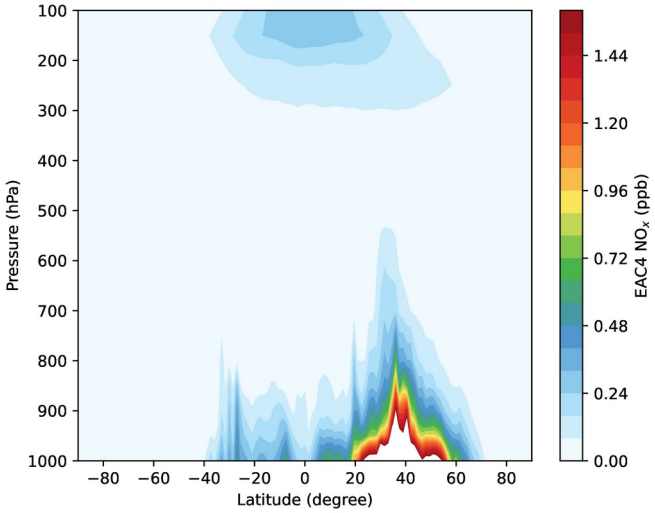


Figure R1. Zonal mean distribution of EAC4 NOx averaged over the period 2006-2010.

(3) Before discussing seasonal variations of StrOH, the authors should first quantify the overall contribution of stratospheric ozone to global tropospheric OH. This information is essential for evaluating the importance of this process for atmospheric oxidation capacity and the lifetime of short-lived greenhouse gases.

R: In the revision, we present the global distribution of mass-weighted tropospheric mean StrOH and OH. The results reveal that the mass-weighted tropospheric mean contribution of StrOH to OH is high in the southern midlatitudes (up to 30%) and in the northern subtropics and midlatitudes (up to 12%). On a global average, StrOH accounts for about 8% of tropospheric OH.

(4) Line 237: Why was 400 hPa selected for the analysis? Besides the analyses at 400 hPa and the surface, the impact on tropospheric mean OH would be more relevant for atmospheric oxidation studies. In addition, the authors present StrOH in ppt, which makes it difficult to evaluate its relative importance compared with background OH concentrations. I suggest presenting StrOH in

units of molecules cm^{-3} and the relative contribution (%) of StrOH to total OH, which are more commonly used quantities in OH studies.

R: In the revision, both StrOH and its relative contribution are presented as mass-weighted tropospheric means, with StrOH expressed in molec/cm^3. In addition, zonal-mean pressure-latitude cross sections of StrOH and its relative contribution are presented, also with StrOH expressed in molec/cm^3 (Figure 5 in the revision).

(5) Line 245: The spatial and temporal distributions of StrO3 should also be presented. This would help clarify the pathway through which stratospheric ozone transport influences tropospheric OH.

R: In the revision, the global distribution of mass-weighted tropospheric mean StrO3 is presented.

(6) Line 246: The discussion of factors controlling StrOH variations is currently insufficiently supported. For example, the authors discuss the roles of solar radiation and humidity, but quantitative evidence is lacking. Additional model diagnostics, such as OH chemical budgets, should be provided to support these conclusions.

R: In the revision, the production and loss rates of StrOH along various pathways are calculated, and their zonal-mean, mass-weighted tropospheric means are presented (Figure 7 in the revision).

(7) The negative StrOH contribution is interesting. The authors attribute this effect mainly to the OH loss reaction of OH +O3. However, more evidence is needed to support this conclusion. For example, the authors should compare OH production from ozone photolysis with OH loss through O3 reactions. A comparison of OH chemical budgets between the original and perturbed simulations would also strengthen this discussion.

R: In the revision, we compare the OH chemical budgets between the original and perturbed simulations. It is found that the mass-weighted tropospheric mean perturbation of the net OH production rate due to the presence of $StrO_3$ shows negative values in region similar to those where negative StrOH occurs. These results are presented in Figure 6 of the revised manuscript.

(8) Figure 3: The meaning of the black contour lines is not clearly described. Please clarify.

R: The black contour lines indicate the zonal wind speed at 200 hPa. However, these plots have been removed from the revised manuscript.

(9) Section 3.3: The selection criteria for the four analyzed regions (Tibetan Plateau, subtropical ocean, northern South America, and South Africa) should be explained. Currently, this section mainly presents seasonal cycles for individual regions, but the broader implications remain unclear. For example, what general conclusions can be obtained from these regional analyses? Are different regions controlled by different chemical regimes? What are the implications for regional oxidation capacity? Additional quantitative analyses are needed to identify the dominant controlling factors.

R: In the revision, detailed analyses for the selected regions are no longer included. Instead, we concentrate on the global variations of StrOH, specific humidity and $StrO_3$, as well as the possible reasons why negative StrOH occurs.

(10) Line 305: The authors state that the relative contribution of stratospheric intrusion could be doubled if EAC4 and CESM2-WACCM OH values are used as references. I do not think this inference is justified. The higher OH concentrations in these datasets may result from differences in other factors, such as surface emissions, chemical mechanisms, and model parameterizations. Since the authors did not calculate StrOH using these models, they cannot directly infer that the stratospheric contribution would be doubled. This statement should be removed or revised.

R: Yes, higher OH possibly indicates higher StrOH as well. These sentences are removed in the revision.