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

Review of “Contribution of stratospheric intrusion to tropospheric hydroxyl radical” by Wang et al.

Wang et al. present a discussion of the role of stratospheric ozone intrusions on tropospheric OH production. They use the GFDL AM3 model and compare the results to HIRDLS and output from other CCMs. My two main comments are: 1) it’s clear what we learn from this work that hasn’t been shown before and 2) the presentation of the work needs to be cleaned up. It’s clear the authors did a lot of work with many comparisons, but in my opinion they need to do a better job of detailing how this goes beyond previous work (including some uncited papers) mentioned below. Additionally, the figures need to be redone. They are not in a publishable state. I would recommend major revisions for the manuscript.

Major Comments
1) Not entirely clear what we learn

The authors need to do a better job of explaining what is actually new here. There is quite a bit of work discussing the role of STE for OH going back 20+ years (e.g., Crutzen et al., Telus 1999). Other papers describing the controls on OH seem to be missing such as Murray et al. (2014; https://doi.org/10.5194/acp-14-3589-2014), Murray et al. (2021; https://doi.org/10.1073/pnas.2115204118), Bourgeois et al., (2021; https://doi.org/10.1073/pnas.2109628118), and Zhang et al., (2024; https://doi.org/10.1126/science.adn2961). Many of the conclusions stated in the abstract are things that have been known for a while.

The text does a decent job of describing differences, but many of the differences they do describe have already been seen for many years in these models.

R: All these studies are cited in the revised version except Bourgeois et al., (2021), which appears to be less relevant to our work.

Here, we focus on quantifying the contribution of stratospheric ozone intrusion to tropospheric OH, particularly its three-dimensional distribution. Previous study have mostly discussed the impact of STE on global mean OH, with exception of Kentarchos et al. (2003). However, the model they used does not represent chemical reactions specific for the stratosphere.

In addition, our understanding of OH chemistry, particularly the recycling mechanism, has undergone significant revision (Lelieveld, et al., 2008, 2016; Taraborrelli et al., 2012; Bates et al., 2019). This study apply AM3 that includes interactive tropospheric and stratospheric chemistry and has a more detailed OH chemistry compared to the previous study. In addition, the contribution of stratospheric ozone to tropospheric OH is quantified by perturbing the tropospheric chemical rather than by tracing stratospheric ozone and its sink processes in the troposphere, some of which produce OH.

These descriptions are inserted into the third and forth paragraphs of the Introduction in the revision.

2) Cleaned up
The manuscript needs to be cleaned up. All of the figures need to be improved and/or cleaned up.

It's not totally clear which figures go with each caption. For example, it seems like Figure 1 is split across 3 pages and is made up of 12 separate panels but only labeled a-d. I initially thought the text on Page 9 was the caption for 2 separate figures. I eventually realized that the paragraph is part of the main text, not a caption. The current panel "(a)" should be its own figure. This is showing the same thing. The authors should group figures together that are showing similar results instead of stringing 12 panels together.

R: In the revision, all figures have been recreated. Figure 1 from the original version has been split into three figures (Figure 1-3) in the revised version. In addition, extra space has been inserted between each figure caption and the main text.

Figure 2 is similar. It is comprised of 6 panels and there is only one label (b). The (a) label seems to be missing. Although each subpanel has a label, so there are technical 3 panel "(b)".

R: In addition, Figure 2 has also been split into two figures, Figure 4 and 5, in the revised version.

Figure 3 is similarly missing a label (a), the caption is on a separate page, and it is comprised of 24 panels.

This may result in far more figures than the authors would like. If so, they should consider moving some to an SI, particularly if they are not important to the story.
R: In the revised manuscript, the content of the original Figure 3 has been replaced by Figure 6, which provides a different presentation of the model simulations. This should now be clearer and more compact. These figures are currently not included in the SI, but they can be moved there if deemed necessary.

Minor Comments
1) Abstract contradicts their numbers
The abstract seems to contradict their own numbers. The abstract says the "contribution of stratospheric intrusion can reach the 12% over the Tibetan Plateau, northern South America and South Africa, and 20% over the subtropical oceans". This seems to disagree with the Conclusions (Section 4.1):
• Tibetan Plateau: -11% to 7.5%
• Subtropical oceans: 22%

R: There could be some errors in the writing. In the revised version, the presentation of the model simulations has been largely recreated and the corresponding descriptions have been significantly modified. Care has been taken to keep the important numbers consistent throughout the manuscript.

2) Simulation timeline doesn't make sense

Section 2.1 (Lines 142-145) state that the simulation ran from January 1, 1979 to November 31, 2004. It then ran four more years with daily output. First, November should only have 30 days. Second, this would take their simulations to the end of 2008. The analysis is supposedly from July 2005 to December 2010 (Lines 143-145). This doesn't make sense to me.

R: Yes, these are writing errors. In the revised version, the corresponding sentences are modified to

"The simulation began on January 1, 1979, and ended on December 31, 2004. Then, it continued for six years with daily output. The analysis is based on the simulation's daily output from July 2005 to December 2010.".

3) Figure contradicts their numbers

The text states that the tropospheric OH from AM3 is 10.10x105 molec/cm3. However the text on Figure 2a (top left panel) lists a value of 9.06x105 molec/cm3.

R: The value of 10.10×10^5 molec cm^{-3} is the mass-weighted tropospheric mean OH concentration for 2010. The value of 9.06×10^5 molec cm^{-3} is the volume-weighted tropospheric mean OH concentration averaged over 2006-2010. The value of 10.10×10^5 molec cm^{-3} is used because Zhao et al. (2023) present their results as a mass-weighted tropospheric mean for 2010. It is intended for a comparison only. In the revised version, only the mass-weighted tropospheric mean is used.

Specific comments

Abstract (Lines 11-12): Typo, should be "revealed" and "variability". Confusing sentence using "and" 3 times.

R: The corresponding sentenece is revised to "Previous studies have revealed a significant impact of lightning and air pollution on the interannual variability and long-term trends of tropospheric OH" in the revision now.

Lines 34-43: Should include Murray et al., (2014; https://doi.org/10.5194/acp-14-3589-2014) and Murray et al., (2021; https://doi.org/10.1073/pnas.2115204118)

R: The two refences are cited as follows in the revision,

"Murray et al. (2014) proposed a simple prediction equation for tropospheric mean OH, which includes tropospheric ozone photolysis frequency, water vapor concentration, and the ratio of total NO_x emissons to the source of reactive carbon. This equation can explains 72% of the simulated variability in tropospheric mean OH." in the first graph of Introduction, and

"However, Murray et al. (2021) showed that the treatment of reactive oxidized nitrogen species (NO_y) and the fraction of emitted carbon lost within each pathway can explain inter-model variations in global mean OH." in the second graph of Introduction.

Lines 46-50: Citations needed.

R: The sentences are revised to “For instance, indirect estimations generally indicate a peak in OH abundance during 2000-2004, which can account for the nearly constant mixing ratios of tropospheric methane from 1997 to 2006 and the increasing ratios before and after this period (Turner et al., 2017).”.

Lines 59-75: Other recent papers that should be mentioned:
• Bourgeois et al., (2021): https://doi.org/10.1073/pnas.2109628118
• Zhang et al., (2024): https://doi.org/10.1126/science.adn2961

R: Bourgeois et al., (2021) appears to focus on the relative contributions of biomass burning and urban emission to tropospheric ozone, which is less relevant to this study. Zhang et al. (2024) has already been cited.

Line 98: Weird reference style. Mao, Paulot, et al. (2013) is not in reference list.
R: The corresponding sentenece is revised to “The updated chemistry include heterogeneous reactions of HO_2, NO_2, N_2O_5, and NO_3 on the surfaces of all simulated aerosol types, as described in Mao et al. (2013a, 2013b).”.

Lines 104-105: Typo, missing “convection”

R: It is revised to “The original deep convection scheme produces … ”.

Line 116: Is this the stratospheric ozone tracer from Michael Prather that was implemented by Meiyun Lin in AM3? Needs citations if so. In either case, these need to be defined better. It’s not clear what the subscripts mean or what each is supposed to represent.

R: Here, $O_{3,s}$ is a tracer that is identical to O_3 in the stratosphere and is used to correct for the stratospheric intrusion contribution to tropospheric O_3; it is then set to zero at each time step in the troposphere. $O_{3,rm}$ is tropospheric O_3 with the stratospheric intrusion contribution removed. The $O_{3,s}$ tracer defined in Michael Prather and Meiyun Lin is identical to O_3 in the stratosphere and undergoes chemical loss and deposition in the troposphere. They are different artificial tracer. In the revision, a sentence has been inserted to clarify this difference,

“Therefore, $O_{3,rm}$ represents tropospheric O_3 with stratospheric ozone intrusion removed, and OH_{rm} represents tropospheric OH with the contribution of stratospheric ozone intrusion removed.”.

Figure 2: Citations for the models used?

R: These models are well known, and references for them are therefore not included. However, they can be added if deemed necessary.