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Comment on acp-2022-718

Anonymous Referee #2

Referee comment on "Surface ozone over the Tibetan Plateau controlled by stratospheric intrusion" by Xiufeng Yin et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-718-RC2, 2022

The manuscript by Yin et al. presented an interesting topic related to the influence of stratospheric ozone input on tropospheric surface ozone variability. Through analyses of monthly surface ozone variability, modeled stratospheric O3 tracer (unjustified), and weather structure of the upper troposphere/lower stratosphere, the authors concluded that stratospheric ozone contributed ~ 65 % of surface ozone across the high mountain Asia. Although in general I agree that stratospheric ozone inputs may be important for surface ozone budget at midlatitudes & high mountain regions, ~ 65% contribution is too overwhelmed, and which is inadequately supported by evidence and assessments presented in the manuscript. I think there are sufficiently more analyses that the authors need to conduct to support the conclusion, and to promote the manuscript to be potentially publishable.

Major comments:

- sections 3.2, the rejection of the role of in-situ chemistry on surface ozone variations is a rush. Simply analyzing the seasonal patterns of O3 with NO2 and CO cannot rule out the role of in-situ chemistry, especially the author didn't explain why such correlated or uncorrelated data can reveal their causal relationships. In general, NO2 and VOCs, as well as CO are precursors of O3, but sunlight (photochemistry) is also necessary. In the plateau, soil emissions would be important sources of NOx and VOCs, which in general depends on temperature. How this precursors and local actinic flux varied with O3? And how was transport affects surface O3? All these need to be assessed. Meanwhile, don't forget CO is also an indicator of stratospheric air incursion as which is characterized as low CO and high O3.
- Section 3.3, it is entirely unclear how the contribution of stratospheric O3 to surface O3 was estimated, by the ratio of stratospheric O3 tracer (O3s) to the **modeled** or the **observed** surface O3? How was the modeled surface O3 vs. the observed surface O3 in the model?
- section 3.4, the CAM model is partially driven meteorological parameters, where the

interactions between the troposphere and the stratosphere are determined by jet stream and/or tropopause folding. Thus it is kind of a loop to compare the modeled O3s with such synoptic events as it is that these events transport air out and in the stratosphere (and vice versa) at the boundary of stratosphere and troposphere. This should put before or right after analyses on the effects of in-situ chemistry, but before the model analysis. The formers can be qualitative, but the latter should be quantitatively conducted.

In fact, why not used the CAM model to also assess contributions of tropospheric processes to the observed surface O3? In general, most atmospheric chemistry model (online or offline coupled) has a better performance on tropospheric chemistry simulations than stratospheric chemistry simulations. Why don't show the modeled surface O3 concentrations as well as the fractions of tropospheric contributions by simply deducting O3s from total model surface O3.Without such comparisons, both section 3.2 and 3.3 are incomplete and are insufficient to support the conclusion.

In addition, I suggest the authors to move SI materials to the main text, overall, the length of the main text is short and lacks of sufficient details in results and discussions, and putting related materials in SI makes further difficulties in understanding the arguments/assessments the authors stated.