Atmospheric Chemistry and Physics Discussions

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Comment on acp-2022-718

Anonymous Referee #1

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-RC1, 2022

This paper by Yin et al. presents an interesting topic , i.e. the influence of stratosphere-to-troposphere transport to the surface ozone over high altitudes in the Tibetan-Himalayas region. The concept of using modelling analysis (i.e. the "Luo et al. 2019" approach) dealing with the identification of tropopause fold together with observations at multiple sites is intriguing. Also based on the available literature, I agree that there is a significant contribution from the stratosphere and that the meridional excursion of the jet stream can play a pivotal role in triggering this process. However, I think that the authors failed in support their hypothesis (i.e. the influx form the stratosphere is the main driver of the ozone variability in this region).

The analysis suffers by several caveats, much of the discussion is qualitative and some tools are not well characterised or documented, indeed.

Moreover, other minor issues are present along the paper.

Thus, I cannot recommend publication on ACP in this current form. Below you can find a list of the major points that, in my opinion, prevent the publication of this work.

- The authors used the outputs O3S from the CAM-chem model to infer an estimate of the quantity of stratospheric O3 which is contributing to the overall O3 variability. How was this specific product validated? The authors must convince the reader that this model product is able to provide accurate and reliable quantification of the O3S/O3 ratio for the investigated region and sites. Unfortunately, I was not able to find any usable information on the cited paper by Tilmes et al. (2016). Before using it for this assessment, you should have shown how the CAM-cam model compares with observations at the considered sites (Mt. Waliguan, NCO-P, Nam-Co) and document

how well the model is able to diagnose transport from the stratosphere and to quantify the contribution to the tropospheric ozone.

- The period of investigation is not clear and not consistent. Section 3 discussed analyses based on 2012 and 2016 data. There is not discussion about how much this inconsistent data frame can affect the results. How much are these single years representative of a longer temporal period?
- The analysis of the variability of monthly mean O3 values with monthly mean CO and NO2 values at the urban sites is too basic. Honestly, I cannot understand how this could support a dominant role of stratospheric transport to the appearance of the yearly O3 peak. Moreover, no discussion about the robustness of the correlation analysis was provided.
- The authors implicitly assumed that the variability of ozone in the region is only due to local photochemistry and input from the stratosphere. They completely neglected the role of the transport (from the regional to the large scale). As an instance, despite what the authors concluded, a notable fraction of the highest O3 values observed at NCO-P during spring (pre-monsoon) were mostly related to transport of pollution often related with wildfires. They cited the paper by Putero et al. (2014) who nicely assessed this contribution.
- The quality of the figures is really poor. In some cases (e.g. Figure 1), it was not possible to understand labels and numbers: this prevents a serious evaluation of their scientific content.
- I don't think that the use of data from external data originator was well recognised/acknowledged. As an instance the paper by Putero et al. (2014) did not report any usable dataset for NCO-P. How the data were obtained? Why the authors did not take the data from traceable data repository like https://ebas-data.nilu.no/ ? I suppose that the same can apply for Nam-Co and Mt. Waliguan.

Minor points:

Line 43-46: In the Introduction, more recent bibliographic references should be provided besides than Crutzen (1988). You can consider the valuable papers produced within the TOAR initiative. Bracci et al. (2012) is specifically devoted to the analysis of synoptic-scale processes leading to stratospheric intrusions over the southern Himalayas: it should be better contextualised. In the global troposphere, ozone burden is not only affected by photochemical production and STE but also chemical loss and dry deposition. When discussing ozone variability over specific regions or sites other processes must be considered (among the others PBL dynamic, air-mass transport occurring at different scales).

Line 61: here you should also mention the long-range transport as well as the transport of polluted air-masses from the neighbour regions.

Line 67: please introduce the background sites. You should give to the readers, at least, some basic descriptions of the sites in terms of geographical locations and already documented processes that can affect ozone.

Line 71: the description of the measurement methodology was unsatisfactory. No information are provided about uncertainties related to the measurements, data coverage, reference to metrological standards. Why two different year are considered 2012 and 2016?

Line 83: 2106 should be 2016. Data for NCO-P cannot be obtained by the web site that you indicated.

Line 92: the identification methodology should be described.

Line 101: According with Tilmes et al. (2016) the O3S product is not affected by dry deposition in CAM-chem. How much this can create bias on your estimate if compared to the real world? The ability of the model to reproduce the O3 variability at the considered sites must be demonstrated before using it (e.g. no comparison with the real observations are provided and discussed).

Figure 2. Please express PV in pvu and Ozone in ppb. The scale of the plate (a) should be increased to values higher than 10%. It looks that the 2 pvu surface is well detached from the ground. I agree that there are signals of stratospheric transport occurring (the relatively high PV – but well lower than 2 pvu -stretching down from the stratospheric "reservoir"), but it's difficult to use this plot to support a dominant role of stratospheric transport for surface ozone variability. Do you have a similar cross section for the tropopause fold frequency?

Figure S4 did not report the measurement unit for O3S.