The authors have provided a detailed response to the original two referees and in particular the error budget, underlying DOAS fits, and TUV model set up to determine OH production are now much more clear. They have also provided further support for near surface ozone and transport of ozone and NO_x to the site. These provide good support for some of their key results near the surface which are most results reported in the abstract. However, questions particular to the vertical profiles remain which could indirectly still impact the key results.

I have two major comments:

1) What is the information content of the retrieved profiles? Can the authors provide representative or average values for averaging kernels (AVK) or degrees of freedom (DoF) for specific altitudes? The authors report that results with less than 1 DoF are filtered, but this will typically be concentrated near the surface. In Fig. 4 results are shown as high as 1.8 km agl and in in Figs. 5 and 7 to 4 km agl. Are the results at higher altitudes significant or simply conforming to the a priori? Information is needed to assess this.
2) More information is still needed regarding the calculation of and reporting of results related to OH production.
    a. Firstly, the authors state in the abstract "O_3 and HONO were the main contributors to OH on the TP" and have similar language to this effect in the main text, however, they do not appear to consider other sources. This is therefore not a finding and the language should reflect that.
    b. Related to major comment 1, O_3 and H_2O appear to be sometimes or always elevated at high altitudes relative to other retrieved species potentially driving the resulting OH source.
        i. O_3 appears to never drop below 48 ppb, is this based on the a priori or is it retrieved? If it is from the a priori why is O_3 given a non-zero concentration it decays to when other species are not. Is it based on TROPOMI or lidar data in Fig. S4 if so that needs to be explicit? These would seem to leave room for substantial variability in the free tropospheric background. I will also note that the various traces in Fig. S4 are not explained and units are not the same across panels.
        ii. H_2O appears to frequently increase above ~3 km agl, sometimes to concentrations greater than at the surface despite presumably lower temperatures and pressures at those altitudes. Is this allocation of the remaining column being placed at high altitudes or actually localized at the higher altitudes?
        iii. The TP is a frequent site of stratospheric intrusions (Škerlak et al., 2015) as has been observed at Nam Co in particular (Yin et al., 2017) one would expect this to drive greater profile variability for O_3

and H_2O (and possibly other gases) is this hiding in the averages or is there a reason it is not detected? If there are high-O_3 and low-H_2O air masses descending over the site will that impact the retrieval?

c. The authors have provided a detailed response regarding their implementation of TUV already, but can they address how uncertainty in the retrieved profiles might impact the TUV calculations?

References:

Škerlak, B., Sprenger, M., Pfahl, S., Tyrlis, E., and Wernli, H.: Tropopause folds in ERA-Interim: Global climatology and relation to extreme weather events, Journal of Geophysical Research: Atmospheres, 120, 4860–4877, https://doi.org/10.1002/2014JD022787, 2015.

Yin, X., Kang, S., De Foy, B., Cong, Z., Luo, J., Zhang, L., Ma, Y., Zhang, G., Rupakheti, D., and Zhang, Q.: Surface ozone at Nam Co in the inland Tibetan Plateau: Variation, synthesis comparison and regional representativeness, Atmos Chem Phys, 17, 11293–11311, https://doi.org/10.5194/ACP-17-11293-2017, 2017.