

Review of the paper **“Surface ozone at Nam Co in the inland Tibetan Plateau: variation, synthesis comparison and regional representativeness”** by Yin et al.

This is my second review of the paper. I would like to congrats the authors since the manuscript is really improved since its first submission. Most of my suggestions have been accepted by the authors and most of my concerns cleared. A few minor points must be considered at this point, thus I support publication on ACP after there are considered/fixed.

Abstract, line 19: please, substitute “long-term” with “continuous”

Abstract, line 23: here the low anthropogenic contribution from China in summer and from South Asia in spring must be cited.

Pag 7, line 182: 47.6 +/- 11.6 ppb. Is it +/- 1-sigma? Please, specify.

It is noteworthy that July 2015 appeared much more higher that other July months. Maybe an interesting anomaly to investigate in a future paper!

Pag 9, line 233: “...because stratospheric intrusion contribution is characterised by a maximum in spring...”

Figure S4 is rather obscure to me. I cannot be able to understand which is the message that the authors aim to provide by this figure and what information is actually provided. What these histograms and scatterplots represent? Please, better explain in the text and in the figure caption. Moreover, even in the SM, I would like to see some explanation about this block-bootstrapped method. I’m not used with this method (and I guess other potential readers) and a simple explanation can help in better evaluating the obtained results without searching in other papers!

Line 256: to consider the PV at 350 hPa has much more sense that considering PV at the surface, actually!

Line 273: actually these structures over Himalayas are similar to those reported by Bracci et al. (JAMC, 2012). However, looking at figure 5, the 1-2 PV isolines do not appeared much more different in spring than in winter, please check it!

Line 283: in my opinion, by this analysis hardly you can affirm that STE “occurred right above Nam Co”. Please remove.

Line 285-290: It is not clear which is the “take home message” of the SWD analysis. It is the local photochemistry which is supposed to play the most important role or the hemispheric-scale contribution? Please, clarify.

Line 294: please correct “Der”.

Line 295: “the height of the folding is higher than those in the spring and summer”. I do not think that this info can be retrieved by Fig. 5. Maybe some works can be cite to support (e.g. Ojha, N., Pozzer, A., Akritidis,

D., and Lelieveld, J.: Secondary ozone peaks in the troposphere over the Himalayas, Atmos. Chem. Phys., 17, 6743-6757, <https://doi.org/10.5194/acp-17-6743-2017>, 2017.)

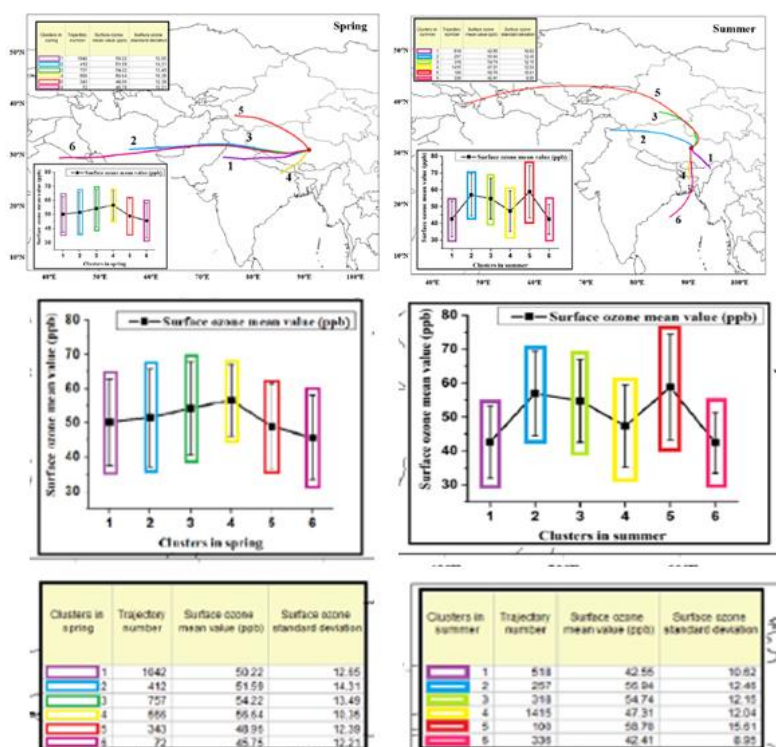
Line 312: and high PBLH, thus suggesting an important role of thermal transport regimes of PBL air-masses to diurnal ozone variability". Once again: it is not completely clear which is (for the authors) the driving processes for diurnal variability: (local?) photochemistry or transport?

Line 346: "...on the urban scale, due to NO titration under ambient conditions not favourable to photochemical production."

Section 5.3: I must say that I'm still convinced that your clustering is not so effective. Looks for instance to the winter season: the single trajectories spanned over a wide latitudinal range, but the cluster centroids are not able to catch this variability. This must be stressed in the discussion...

Line 378: please modify as following: "...may also reflect this possibility."

Figure 11: it is almost impossible to read the inserts. I would suggest to rearrange figure 11 (see my attachment)



Line 390: probably PSCF also picked up the contribution of the transport of pollution from the Indo-gangetic plains and Himalaya foothills...

Line 395 – 397: Actually, you showed that Mt. Waliguan, NCOP and other stations have very different seasonal and diurnal cycles. So I do not agree that they are representative of the entire TP! Please, check!

Line 425: I think that the term “aged” is confusing in this context. As shown by other works (Putero et al., 2016; Ojha et al., 2017...), STE affecting Himalayas can occur very far from the region (e.g. Mediterranean basin). So I wouldn’t call STE occurring over TP as “aged”...

Line 428: ...”is similar” (please remove “most”)

Line 429: please quantify “infrequent”: which is the percentage of occurrence?