

## ***Interactive comment on “Secondary ozone peaks in the troposphere over the Himalayas” by Narendra Ojha et al.***

### **Anonymous Referee #1**

Received and published: 2 December 2016

#### GENERAL COMMENTS

This paper by Ojha et al. presents an interesting study about the occurrence of secondary ozone peaks (SOPs) in the troposphere over the Himalaya/Indian region.

The work is mainly based on the combined use of a limited set of vertical ozone soundings available at a single measurement site (Naintal in the Himalayan region) and on the outputs from the EMAC model, with the purposes of elucidating the processes leading to the occurrence of SOPs, characterizing their temporal variability and assessing their contribution to tropospheric ozone. The measurement data at Naintal were also used to evaluate the capacity of EMAC in reproducing SOPs.

The paper is well within the scopes of ACP and , potentially, it can seriously help in better clarifying this specific (but rather frequent) tropospheric phenomenon and its

C1

implication on tropospheric O<sub>3</sub> budget over the region.

However, I think that the authors should better take advantage of the long-term (2000 – 2014) ECAM data-set to provide a more robust characterization of this class of events both in terms of their origin, dynamical features and impact on long-term O<sub>3</sub> variability.

Thus, I recommend publication, but after that some major efforts will be implemented towards this direction. I'm rather confident that the authors can implement the requested changes in a reasonable small amount of time.

#### SPECIFIC COMMENTS

In the introduction you should better describe the paramount importance of clarifying processes affecting tropospheric ozone variability in the Southern Asia and Himalayas, two global hot spot for climate, atmospheric composition and anthropogenic pressures (see e.g. <http://www.unep.org/pdf/ABCsummaryFinal.pdf>).

The verification of EMAC model capacity in reproducing SOP is based on a limited number of vertical soundings (only 6). The comparison provided by Figure 2 is encouraging about the ability of EMAC (despite the relatively coarse horizontal resolution 2.8 x 2.8 deg) in reproducing the SOPs. However, you should mention that this very limited amount of data prevent a systematic assessment. Did you try to inspect soundings at other locations in the same region (e.g. New Delhi, see <http://woudc.org/data/explore.php>) to make the data-set for verification larger? Can you provide some references of earlier works showing comparison of EMAC vertical ozone profiles with measurements (maybe Jöckel et al, 2016 can be profitably cited )?

Pag 6, line 169: I would like to see the bias expressed as %. This would better help in understanding the deviation of the model from the measurements.

Pag 6 line 176: “However, these...for completeness”. I cannot understand this sentence. Do you mean that the selection by Ojha et al. (2014) is not accurate? Please, rephrase!

C2

It is a pity that the “core” Section “3.2 Origin of SOPs” is discussing only the results from the six selected profiles at Naintal! I strongly encourage the authors to use the 15-year ECAM outputs to investigate in a more systematic way and for a long-term perspective this point. Also the back-trajectories investigation can be carried out for the whole 2000-2014 period by using NCEP re-analysis. I would suggest to use the SOP events identified over the period 2000 – 2014 and aggregate them on a seasonal basis to provide indication about the amount of ozone transported from the stratosphere during SOP (by comparing average O3 , O3s and PV vertical profiles).

Figure 5. Basing on the Figure caption, the TF locations 5 days before the events are reported in the maps. However, all the back-trajectories showed very fast transport: 5 days before the arrival to Naintal the air-masses were (at least) off of the north Africa western coast-lines. Thus, which is the relationship with the identified TFs? I suppose the authors would say that the TF DURING the air-mass transport were reported. . . Moreover, how long the back-trajectories are? No information are provided along the manuscript. . . Also seasonal composites over the period 2000 – 2014 about the spatial locations of tropopause folding related to SOP events can be presented (see e.g. Figure 4 by Putero et al., 2016 but for tropopause crossing). What about days without SOPs? I guess that no (or fewer) tropopause foldings were crossed by back-trajectories for these cases. . . To provide a “climatological” long-term perspective, you should also consider the possibility to present a composite for Fig. 7 and Fig. 8 as a function of the seasons for the period 2000 – 2014.

Pag 7, line 228: “This variability in LRT. . .in Fig.5”. It is not clear to me. Please, explain better this kind of association. . .

Pag 7, line 232: please define “medium”.

Pag. 8, line 241: please provide in the text longitude boundaries for these regions.

Pag 8, line 243: despite your statement at pag 8 line 236, basing on that plot, it looks that a STE is actually occurring also for the June event (a tongue of air-mass rich in O3

C3

extended down to 500 hPa southward than 30N)!

Section 3.4 The authors must provide some information about the long-term SOP trend over the region of interest: this information is very valuable also taking into account the current debate about the occurrence and attribution of tropospheric ozone trends (see e.g. <http://www.igacproject.org/TOAR>) . Trends in seasonal/yearly frequencies or physical features (e.g. altitude) of SOP and the related O3 contribution are detected? Also the information that no long-term trends were detected is nevertheless valuable. . .

Figure 10: I would add the percentage contributions of SOPs to monthly TCO values. What the error bars represent?

Conclusions In general this Section reports very important general statements about SOP but which are mostly based on the analysis of just 6 case studies (see lines 323-220). I would recommend to try to increase the robustness of these interesting hints by adopting a long-term perspective basing on EMAC simulation.

Line 335: “The minimum in the. . .mixing”. I would also mention the northward displacement of subtropical jet stream during summer monsoon.

Line 339: are you able to provide any indication about the impact of this increase in terms of radiative forcing over the region?

#### TECHNICALS

Figure 1. I would skip the typical event plot since it is also reported in Figure 2.

Figure 6: x-axis and y-axis. I suppose the black line is the back-trajectory pressure level: it should be reported in the caption.

Figure 7 (Figure 8): please indicate in the caption the latitude (longitude) value for which the cross section is produced.

Figure 9-10: what the error bars represent?

C4

