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Atmospheric Chemistry and Physics
Discussions

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

Anonymous Referee #2

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Review of Ohja et al., Secondary ozone peaks in the troposphere over the Himalayas

The authors use soundings from an Indian station (Nainital) sampled over the course of one year to identify 'secondary ozone peaks' (SOP). According to the authors 3-4 profiles are available per month. Six profiles are presented showing an SOP. A comparison with the EMAC model at T42L90 is used to extend the limited data set over a time period of 15 years (2000-2014) to assess the impact of such events on the ozone column over the Himalayan region. During the monsoon season they find virtually no SOPs over the region of interest. According to the authors such SOPs contribute 7-9DU of ozone to the tropospheric ozone columns during SOP occurrence. They also show, that the SOPs are only a minor effect and do not significantly enhance the ozone column over the whole year. The quantification of ozone transport across the tropopause is important and as such this study could in principle add to this.

Overall the paper is well written and the graphics are clear and appropriate. However, the paper needs some major clarifications: I missed clear descriptions of terms and definitions given the central topic SOP: How do the authors define an SOP? They only provide a definition for the model analysis later in the manuscript, but does this also work for the soundings, which have a much higher resolution? How do they distinguish an SOP from a tropopause fold or do they imply folds as SOPs? This is not clearly stated at all also in the introduction. Directly linked to this they don't discuss the transience or irreversibility of the phenomena, which are however crucial for the irreversibility of ozone flux and the persistence of the effect. I also missed a careful analysis of the transport and mixing process, as stated in the abstract. The authors should and could provide this, but currently they show coincident fields, but not a process. Given these points there I recommend the paper for publication after the following points have been addressed.

Major points: 1) In section 2 the authors should provide a clear definition for SOPs, which have been applied to the soundings. Further: What is the vertical resolution of the soundings and which role does the resolution of the sounding play for definition and the final column ozone estimate? The authors also do not discuss the effect of the limited vertical and horizontal resolution of the model. How many layers do they miss compared to high resolution sonde profile and how would this affect the number of peaks and the ozone column?

2) The authors should pay more attention to the reversibility of the SOPs. As long as the SOPs keep their high PV values as indicated in Figures 2-4, the ozone peaks will not permanently contribute to the tropospheric ozone budget, since they do not mix as shown in Fig.4 by the O3s. Figures 7-9 show O3s structures in the troposphere which are collocated to the tropopause (i.e. PV structure). The authors could e.g. diagnose the evolution of O3S on an isentropic surface relative to the evolution of PV to diagnose a persistent effect of the SOPs on tropospheric ozone. Maybe an additional plot of wind gradients or Richardson number would give some further indication for the process.

3) I suggest to calculate a statistical amount of trajectories in the model and evaluate the evolution of O3s, O3 and PV along the trajectories? I can't see, how the current Lagrangian analysis provides a robust view on any exchange on the basis of one trajectory per case and I suggest to remove Fig.5 and 6. At least the authors could show plots of ozone timeseries along the trajectories in Fig.6. Instead of the current Fig.6 the authors could plot the ratio of O3S/O3 to illustrate the stratospheric entry (with PV as contour to differentiate between transience versus irreversibility). This would much more strengthen the paper. Alternatively the authors could use the ERA Interim data, which drive the EMAC to perform trajectory calculations with a statistical amount of data. This would also much better help to identify the process of ozone transport and mixing into the troposphere by diagnosing PV change.

4) For the estimate of the effect of the SOPs on the tropospheric ozone column the authors should extend their analysis. As long as they don't account for the PV change, their results are not related to the tropospheric ozone budget. I suggest to compare in addition to Fig. 10 O3 and O3s for PV < 2 only for periods with and without SOPs. This would give the ozone which stays in the troposphere and leads to an enhancement during periods of SOPs, which would strengthen the importance of the results.

Minor comments: l.53: If SOPs occur in the lower stratosphere, how are these defined? They can't be the result of the same mechanism as tropospheric SOPs, are they comparable? l.100: Whats teh output frequency of the model?

l.117: "Tropopause folds are identified..." : How do the results compare to Sprenger et al,2003 or Skerlak, 2014 (over the Himalayas)?

l.146: Why don't you use a larger number of trajectories and perform a robust analysis?

l.155,156: Why is the model interpolated and not simply evaluated at the model levels, which would avoid interpolation errors particularly in the vertical? Is the output interpolated in time?

l.167-172: How do the relative ozone enhancements of compare to the observations instead of the absolute values?

l.285-287: 285-287: Clearify: What is meant with" PV structures and subtropical jetstreams"? Do you mean tropopause folds below the jet?

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