

Interactive comment on “High-ozone layers in the middle and upper troposphere above Central Europe: potential import from the stratosphere along the subtropical jet stream” by T. Trickl et al.

Anonymous Referee #2

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GENERAL COMMENTS

This work by Trickl et al., presents and describes three case studies of ozone-rich air-masses to Central Europe, with the aim of investigating the possibility that STE occurring along the sub-tropical jet stream (SJS) can influence tropospheric ozone budget.

The paper is a newer and improved version in respect to the paper already published on ACPD by the same authors in 2009 (“High-ozone layers in the middle and upper troposphere above Central Europe: strong import from the stratosphere over the Pacific Ocean”). In respect to this previous version, the paper goal is more clear, new analyses

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were added and the paper structure was notably improved. The role of SJS in triggering STE leading to stratospheric air-mass transport over Europe has been recognized in this work. This permitted to clarify some high-ozone events already presented in previous work (e.g. Trickl et al., 2003) but not well understood.

In general, I think that the authors should work a little bit more in making the reading more linear and, possibly, omitting some of the huge amount of detailed information provided in the paper. To this aim the authors should consider the possibility to move some of the figures to the electronic auxiliary materials.

On the contrary, also basing on the long experience of these authors in studying STE, I would like to see in the “Discussion” section more sentences (even speculative) about the dynamical processes (CAT? Tropopause foldings? Differential advection?) favoring the STE in connection with the SJS: this would represent a valuable add-on for other scientists working in the field.

Concerning the analyses, my principal concern is related to the fact that (for all the presented case studies) the authors made a direct comparison of the in-situ LIDAR profiles with profiles of modeled quantities (i.e. the stratospheric air-mass fraction) at 10 - 20 days upstream to the measurement site. As I will explain in the specific comments, I'm not convinced about the reliability of this comparison ...

Basing on that, I recommend publication on ACP after revisions (mainly related with the paper structure) because in my opinion the paper is scientific-sounding and covers a potentially important and relatively not already extensively investigated topic (i.e. influence of sub-tropical jet stream on STE and O3 budget). The manuscript focus is within the ACP scope clearly demonstrate the effectiveness of merging high-quality measurements with model analyses.

SPECIFIC COMMENTS

-Introduction:

Pag 30475, line 1: basing on the number here provided (six 4-day episode from 1996 to 2001) it seems that these events can only play a minor role in determining the yearly ozone variability over central Europe: please comment.

Pag 30476, line 3: please indicate a reference (Trickl et al., 2003)

Pag 30476, line 4: also the works by Langford (1999) and Zachariasse et al. (2000) should be cited:

Langford, A. O. (1999), Stratosphere-troposphere exchange at the subtropical jet: Contribution to the tropospheric ozone budget at midlatitudes, *Geophysical Research Letters*, 26, 2449-2452

Zachariasse, M., van Velthoven, P.F.J., Smit, H,G,J, Lelieveld, J., Mandal, T.K., Kelder H., Influence of stratosphere-troposphere exchange on tropospheric ozone over the tropical Indian Ocean during the winter monsoon, *J. Geophys. Res.*, 105, D12, 15403-15416, 2000.

Pag 30478, line 7: please add a reference.

-2.1 Measurements:

Page 30479, line 10: please can you specify “lower” and “upper” troposphere?

Page 30480, line 14: please can you add a reference where details on in-situ measurements (e.g. experimental set-up) were presented before?

Page 30480, line 15: the description of the experimental set-up is very detailed for the ozone sounding. Maybe you can shorten it and referring to earlier work for more detailed description.

-2.2 Models

Pag 30482, line 6-19: From these sentences is it not clear what the “Column” plates presented in Fig.2, 3, 9 and 18 are representing. I suppose these plates indicated the

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“emission sensitivity” described from line 6 to 11. Certainly, one can read the paper by Seibert and Frank (2004) and Stohl et al. (2003), but using the same nomenclature between the FLEXPART outputs and the figures presented in this work (as you did for the “footprint” product) will help!

Pag 30482, line 20: For simplifying the reading, you can provide here the list of cases (or simulations) for which the “Frost et al” inventory have been used. Did you note some differences by using the two inventories?

Page 30482, line 27 – pag 30483 line 2. If correctly understood, the fraction of stratospheric particle was only calculated for the 20-day simulation. Thus I suggest to move this sentence to page 30483 at line 25.

Pag 30483, line 2 to 10: I suggest to move this paragraph (which guide the reader to the interpretation of retroplume cluster plots) to page 38488 (line 15) where this output is presented for the first time: this probably would further help the reader.

Pag 30483, line 25: with the purpose of better helping the reader, you should indicate in the FLEXPART figures (by simply adding a code on the figure top) if these are referred to the 15-day or the 20-day simulations.

Pag 30483, line 11-25: I would shorten this paragraph. I would only explain the “hard” differences between 15-day and 20-day runs.

-3. Results:

Pag 30485, line 3-5: I would skip these sentences.

-3.1 Case 1:

Pag 30486, line 11: basing on table 1, there was 2 layers from 4.5 to 7.5 km and from 7.5 to 11 km. Please update.

Pag 30846, line 11: please substitute “in our 2003 study” to “Trickl et al (2003)”

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Pag 30487, line 17-22: this sentence can be skipped as the FLEXPART outputs have been already described in section 2.2.

Pag 30488, line 6: For increase the reading simplicity I would specify that this is referred to the 7 – 10 km layer. Moreover, how did you diagnose STT by trajectories? Did you analyze PV along the trajectories? Did you use FLEXTRA or LAGRANTO? You should be a little bit more specific Moreover, from this paragraph it seems that this STE (between Kamchakta and 40N, 180E) was the leading processes determining the observed ozone increase. However on the following (line 27) it was reported that: “there is little influence from the intrusions over Kamchatka”. Please, clarify.

Pag 30488 line 10- 12: I would skip this sentence.

Pag 30488, line 14. As already noted in the general comments I’m quite skeptical about the possibility of directly compare in-situ results with simulation profiles from 15-day earlier!!!

Pag 30488, line 24: I’m not completely sure about these numbers...I don’t think that you can extrapolate the amount of ozone transported over the measurement site simply multiplying the stratospheric air-mass fraction (calculated for air-mass 15 days before the arriving at the measurement site) for the average amount of ozone in the lower stratosphere (0.12 * 500 ppb) ! Surely, dilution and mixing processes can act in decreasing the concentration of stratospheric ozone. I’m wondering why you don’t show the profile of the stratospheric fraction at the time of air-mass arrival over the measurement site?

Pag 30489, line 24: The map showing the jet streams (Fig. 7) show interesting hints, nevertheless I had some difficult to link the different maps with the backward movement of air-masses. You may add a number (from 1 to 4) to each jet stream plate and then superimpose these number on the retroplume maps. How the authors decided that the important STE were those over the Strait of Gibraltar, Caucaso, China, Japan and not (for instance) the green dots over West USA on May ,27? Pag 30490, line 5. probably

the work by Langford et al. (1999) can be profitably cited in this context.

Pag 30490: line 14: this sentence is not clear to me.

-3.2 Case 2

Pag 30490, line 16: Please add the starting day (as you did for the previous paragraph title)

Pag 30491, line 14: please remove the word “final”.

Pag 30491, line 15: in my opinion these last sentences can be removed. They did not add nothing of vital to the paper.

Pag 30492, line 10: I supposed that the 60-80 ppb of ozone here reported, were referred to air-quality measurement in the US. In this case 80 ppbv of ozone would imply not-negligible photochemical production that in case of sufficient precursor emissions could have sustain photochemical production also during the air-mass travel!

Pag 30492, line 25: did you rule out any influence from NOx thunderstorm production?

Pag 30493, line 1. I suggest to eliminate the sentences starting from “Most trajectories...”. A more deep analysis will be shown just in the following.

Pag 30493, line 25: please rephrase: “We, thus, show results only for days from -20 to -15”

Pag 30493, line 28 – page 30494, line 6: Again, I’m not convinced about the direct comparison of LIDAR data with simulated profile referring to 20-15 days earlier! I suggest to remove these sentences and, if not used for other purposes, Fig 10 (the same can be applied to Fig. 4 and Fog. 19).

Pag 30494, line 8: please substitute “in Figs. 8c and 10b” with “on 27 May at 11:00 (Fig. 8c)”

Pag 30494, line 9: please add “(7000 – 7250 m)” after “in the aerosol profiles”

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Pag 30494, line 13: from Fig. 9 (plate 12000 m) I was not able to see any retroplume over Canada. Probably did you mean Alaska? Paragraph 3.2.4: in the paragraph 3.1.4 you was very specific in identifying the location where STE could have been occurred. I suggest to be a little more specific also in this case.

Pag 30496, line 11 to 21: I will skip or significantly reduce this very detailed paragraph.

-3.3 Case 5

Paragraph 3.3.2. It was quite difficult for me to follow this paragraph that appears a little bit confused. Contribution to layer L1 and L2 are explicitly described, but not for L3. Moreover, it is almost not possible to follow the single back-trajectories on figures 16 and 17. Is it possible to show only back-trajectories for layers L1 and L3? Please check also the figure numbers!

Pag 30497, line 13: “In addition, for L1...”, this sentence is not clear. I suggest to remove it also considering that in the following only L3 layer will be analyzed.

Pag 30497, line 19: please substitute “It follows...” with “The “12000 m” modeled backward plume is very similar to the...”.

Pag 30498: once again, I’m skeptical about the effectiveness about the comparison of model results concerning vertical profile five to twenty days before the observations! The sentence about EURAD results: this not adds important information for the paper. I think that the time-altitude plot you didn’t show can provide more indications about the evolutions of the observed layers: I suggest to show it in place of Figure 19!. Paragraph 3.3.4: you should add some details on this key paragraph. For instance it is not clear when the STE over East Asia and South of Alaska has been occurred. This information is important to understand the aging of air-mass observed at the measurement site.

-4. Discussion:

Sometimes the case studies are referred following the numeration in table 1 and sometimes by using the date of occurrence, please homogenize.

Pag. 30500, line 7: the paragraph citing EURAD simulation and discussing the complexity of modelling ozone input seems a little bit outside of the paper focus.

Pag 30501, line 14: STE can occur also at the exit region of sub-tropical jet stream (see e.g. Langford, 1999).

-Figures:

Fig.2,3, 9,18: Please rename the third plate: “NO_x PBL source contribution”

Figures 4, 10, 19: Is it not clear to me if this stratospheric fraction is the average or the highest values over the considered time periods (e.g.,. from -20 to -15 days in fig. 4)

Figures 5,11: what do the bubble diameters represent? (I suppose the number of particles). Please, specify in figure captions.

Figure 15: please substitute the x-axis labels with dates and hours.

Figure 16,17: is it possible to show only the back-trajectories related to layers L1, L2, L3?

TECHNICAL COMMENTS

There are several typesetting errors (some are listed below). I suggest the authors to careful check the manuscript.

Pag 30476, line 7: please, delete one of the right brackets.

Pag 30484, line 26: please, delete one of the right brackets.

Pag 30486, line 11: please, substitute “our 2003 study” with “Trickl et al. (2003)”.

Pag 30490, line 27: please, check the brackets!

Pag 30491, line 13: please, modify in “Trickl et al. (2009)”

Pag 30497, line 1: substitute “Fig. 15” by “Fig. 16”

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Pag 30497, line 6: substitute “Fig. 16” by “Fig. 17”

Pag 30504, line 4: please, delete one of the right brackets.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 30473, 2010.

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