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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climatologies of streamer events derived from a transport model and a coupled chemistry-climate model" *by* V. Eyring et al.

Anonymous Referee #1

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General comments:

This paper addresses topics of highly relevance to atmospheric chemistry and dynamics research. A simple but efficient algorithm is developed to quantify the occurrence of tropical tongues within the surfzone across the subtropical transport barrier within the stratosphere. The technique is applied to a chemical transport model (KASIMA) and an ECHAM4 simulation. The seasonal and zonal variability is assessed with a climatological perspective.

Despite the high scientific value, the paper must undergo a significant revision to make its content valuable:

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- The term "streamer" is used in this paper for a feature (i.e. filaments of tropical air in the extra-tropics within the stratosphere) that does not correspond to the feature it is used usually (filaments of stratospheric air within the troposphere in the mid-latitudes, see e.g. Appenzeller, 1992). The authors are urged not to use this term for this paper, but describe the feature as other studies do, e.g. tropical "tongues" (Chen et al., 1994) or "filaments" (e.g. Waugh, 1996) or "laminae". In particular, the title of the paper must be changed accordingly.

- Most parts of the text are not as clearly written as they could be. For example the "introduction" and "method and model description" sections should cover only the corresponding content. Further, repetitions should be circumvented. The better order of the story ("roter Faden") would not only make the paper better understandable but also shorten the text. Also, the literature review, technical specifications, methodology, scientific results and interpretation / conclusions / speculations should be separated more clearly from each other by using corresponding Sections (see also specific comments).

- Speculations should be clearly marked.

- Section 5 is not very well integrated into the rest of the paper. A different technique and a different chemical constituent are used. The reader is not prepared to this change of perspective. Could some of the technical part be shifted to Section 2? The nature of the material (e.g. the vertical structure of filamentation, the sensitivity experiment) would deserve a much more extended text. This Section could be the basis of an additional paper (e.g. extending the inspection to the tropospheric and extra-tropical stratospheric O3 sensitivity and the horizontal distributions).

- If the Section 5 is kept in this paper, the results of the other sections should be used to interpret and discuss the results of Section 5 (e.g. the seasonal variation in reliability of E39/C simulations)

Specific comments and technical corrections:

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Both kind of comments are taken together, since several points are not attributable to a specific kind of comment. Position numbers correspond to the "full text print version" PDF document as it is available from the ACP website.

2297, (title):

- see general comment

- The applied technique does not count events, but is applied to each time step individually. Therefore, the term "events" should be omitted or replaced by "structures".

2298:

3: "single" layers: of what thickness?

2299:

14-18: what about the polar vortex?

15: ...variety of dynamic processes...

15-16: In what respect do the 2 references (Reed, 1950 and Dameris, 1995) relate tropospheric dynamics to stratospheric ozone concentrations? Don't they compare tropopause variations to total ozone column?

18: ...within the stratosphere...

21-27: not optimal order of arguments. Suggested: (i): observations (agreement when and where), (ii): theses/indications (forced from troposphere?), (iii): conclusions (model deficiencies in respect to tropospheric dynamics).

27: what is meant by "remaining"?

23-(2300)1: please give reference(s) for irreversibility of transport. What about the Pacific? Are there no observations of tongues?

2300:

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15: from the tropopause up to the middle stratosphere

16: Waugh and Polvani (2000): this paper discusses tongues from the tropical troposphere across the subtropical tropopause into the subtropical stratosphere (within the middleworld). Are the tongues investigated in their study really of the same kind? To the reviewers understanding, the present study focuses on the overworld.

17-18: Offermann et al (1999) and Kouker et al (1999a) both investigate the same case, observed with the same instrument. Please give more references or rephrase the sentence.

23-24: in what way differ Waugh (1996) and Orsolini and Grant (2000)?

17-23: the whole paragraph should be rewritten, differing more clearly between observations and simulations of filaments. Focus on the statements: the filaments are not a numerical feature, but can be observed in reality / the instrument used for the observations is reliable.

24-(2301)20: The overview of the paper should be one paragraph at the end of the introduction section. There should be no discussion, and no abstract like comments, usually one 1 sentence per section should suffice.

2302:

6: latitudinal gradients: Where? At the same location for all tracers? What about the vertical extent?

7-14: could be shortened by circumventing repetitions

14-15: is it justified to use pressure levels?

16 / 20: NH: background has negative gradients and the threshold is defined as $>$10ppbv/rad. Is this not a criterion less strict than a change in sign?

2303:

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6-10: unclear formulation. What is special about the polar vortex?

13-15: the description of the whole method is disrupted. It would ease the understanding, if the whole subsection would be restructured and the technique itself would be explained in one paragraph. The structure of the N2O distribution could be described in the Introduction. The reasons for the reduction of area could be discussed afterwards.

18: what levels? Integrated in pressure, potential temperature or in Z?

21-(2305)1: This subsection must be rephrased, since it is highly unstructured. Since no results are shown, the title of the subsection could be adjusted (e.g. discussion of...). The whole subsection could also be placed in the Introduction section, since it has a literature review character.

2304:

1-2: the comparison seems to be only qualitatively and not of "in detail". The zonal criterion is not really discussed. What are the drawbacks of the meridional technique?

1 / 28: Contradiction: this study makes use of the meridional criterion only.

22: Are the normalized gradients used in Appenzeller and Holton (1997)?

23-25: Argument of horizontal transport is unclear.

2305:

6: "logarithm of pressure as vertical coordinate": inappropriate formulation

9: is this ERA-15? Are the operational analyses used with the full truncation or with the same spectral resolution as the ERA data? Or does the model uses only T42 as input anyway?

16: what is "10 / 120 km pressure altitude"?

2306:

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7-14: the parameterization description could be shortened. Correspondingly, the number of references could be reduced.

24: What time slices are used? What boundary conditions?

25: Table 1 can be skipped; the whole information is within the text.

2307:

1: the title should reflect the focus on present climate

3-17: there are several needless repetitions from Section 2.3 and statements that should be moved to Section 2.3 (e.g. 14-16 about the time slices).

2308:

29: ...than indicated by the KASIMA...

2309:

11: ... (Fig. 4d). Again, the region of ...

12: ...KASIMA. As discussed...

21: ...for the 20 - 70 deg latitude band...

2310:

10: ... hemisphere). The shape of ...

24: Thus, the annual cycle of laminae frequency seems to...

24-(2311)19: The reasons for the seasonal variability of laminae are a crucial point of this paper. Would it be possible to further illustrate this point by showing a figure? E.g.: showing the polar vortex strength and the laminae frequency over the years (only corresponding winter months or all months)?

2311:

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1: does the standard deviation refers to the interannual variability of monthly mean values, or are all individual time steps considered separately?

12-19: it is very well possible, that the underestimation of planetary waves in the SH is related to the too small frequency in winter and spring. Nevertheless, the resolution (T30) is probably not the main reason, since it is the same as on the NH (where the frequency seems to be ok). It would be more appropriate to place the discussion into the following subsection.

18: "extra-tropical" could be misleading, subtropical would be more appropriate.

24-28: In the reviewers view, there is qualitatively good agreement to older studies, what speaks for the quality and value of this study and should be stressed. "partly" (21), "For example, " (22) can be deleted.

2312:

22: Since this study operates on pressure levels, the pressure values (and an estimate of potential temperature values) would be very helpful instead of km values. (as done at 2(2313)).

10-24: To the reviewers knowledge the referenced studies compare the transport based on horizontal patterns (as this study does). The density (and thus the mass) is not considered. It is likely, that the absolute mass transport decreases with height.

2313:

4: ...one of Orsolini and Grant (2000), we ...

13-14: "Therefore... follows:" can be skipped.

2314:

2-23: most of this paragraph could be shifted to Section 2.

2315:

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3: from the inspection of figure 7, it seems that the most significant increase is not during summer months but rather spring, in particular at the SH.

6-29: results should be better separated from interpretation and speculations.

15-16: ...different. More synoptic-scale ...

2316:

3-13: the formulations are very vague. It is recommended to rephrase it and move it, in particular the speculative parts, to Section 6.

4: ...some hints...

21: point (2) is unclear. What is meant by "long range transport".

2317:

11: What is the difference to the method applied n the earlier sections? Obviously, this technique does not make use of a vertical integration. Further, the regions depend on altitude, whereas for the earlier technique, the latitude band is not dependent on altitude.

28: What is the difference between 8 left and right? Region C seems to be the same. Wouldn't Figure 8 right be enough for the (present) discussion?

2318:

28-29: ...released by northern hemispheric tongues than by southern hemispheric ones.

2319:

2: Figure 10 deserves much longer discussion. It shows also the ozone sensitivity in the troposphere that seems to be influenced to the seasonal variation of cross tropopause transport in mid-latitudes.

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14-27: the paragraphs should be rephrased with a clearer language.

21: how is the tropopause defined?

Tables:

Table 1: could be skipped

Table 3: Is the average taken of area weighted values? Or rather grid point weighted?

Figures:

Fig. 1: Is this figure really needed?

Fig. 2:

- What is shown in Figure? Caption should not describe the criterion, but give information about what is shown in figure (e.g. Schematic longitude - latitude distribution of N2O concentration...)

- Why is there a shading of the anomalies? A shading of the region of negative resp. positive gradients would be more appropriate.

- The labels I and II are not explained.

- The labels 1 and 2 are not explained.

- The labels "High values" and "Low values" do correspond only to N2O, but not to O3. This could be reflected by changing the labels to "High N2O concentrations" and "Low N2O concentrations".

Fig. 5: Is the meridional average taken of area weighted values? Or rather grid point weighted? The reviewer strongly recommends the use of area weighted values. If the current figures do not take the area weighting into account, that could explain a part of the seasonal dependent differences between KASIMA and E39/C on the Southern Hemisphere (SH), since the distributions are not meridionally collocated during the SH summer months.

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Fig. 6: as Fig. 5.

Fig. 9: The second sentence of the caption should be rephrased: interpretation should not be given within the caption.

Fig. 10:

- as Fig. 9:

- Interpretation within captions should be omitted. What is the bold line? If it denotes the tropopause, how is it defined?

- ...The superimposed ozone source is shown...

- There seems a technical problem with the shading in the JJA panel: the white area in the center of the shaded area should presumably be shaded.

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 2297, 2002.