Atmos. Chem. Phys. Discuss., 2, S1118–S1128, 2002
www.atmos-chem-phys.org/acpd/2/S1118/

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.

V. Eyring et al.

Received and published: 23 April 2003

We thank the referees for all their helpful comments and suggestions. Nearly all comments have been considered in the revised version of the paper. In some points the authors do not agree with the suggested ideas. We have then explained why.

1. REFEREE 1

1.1. General Comments: Referee 1

- **General Comment 1:** We have removed the term "streamer" in the title. We have

Full Screen / Esc
Print Version
Interactive Discussion
Discussion Paper

clearly defined the term "streamer" in the abstract and have mentioned that in this paper those structures are called "streamers". We are using the term "streamer" in the following parts of the paper.

- **General Comment 2 and 3:** We have marked speculations and have furthermore separated speculations from results by moving them to the *Conclusion-Section*.

- **General Comment 4:** We have integrated Section 5 in that way, that we have highlighted the structure of the paper in earlier sections: In section 3 and 4 the main focus is on the comparison between the two models KASIMA and E39/C. In section 5 the focus is on interpretation and impact of horizontal transport processes on mid-latitude ozone. In this section we additionally look at other altitude ranges. We have not shifted the technical part to section 2, because we think that this would confuse the reader. We have instead pointed out in Section 5 , that until now the special focus was on the validation of the model and that we now in Section 5 like to look closer. In order to do that, we need to slightly redefine the criterion.

- **General Comment 5:** The idea of the paper is to validate the simulated horizontal subtropical mixing, estimating its strength, future changes and its impact on the ozone budget, which, in our opinion, belongs all together. However, the referee's comment shows that the link is not clear enough. We emphasised it more. The criterion had to be adapted to the new question, i.e. (1) it had to be strictly conservative and any misinterpretation had to be strictly avoided, since those effects could accumulate in the ozone field (excluding regions A and B). (2) The criterion has to identify an area, with a concentration deviation with respect to the surroundings. (3) The criterion has to be applied to N2O and ozone. In the third paragraph of section 5 we discussed interhemispheric differences and seasonal cycles and showed that the results are mainly the same as for the previous

ACPD

2, S1118–S1128, 2002

Interactive Comment

section. We emphazised this more to better link the sections. The paragraphs now include a discussion of the hemispheric differences and seasonal cycle of the additional ozone source in comparison to the findings in section 3. And in addition the simulated changes are more interpreted in comparison to the streamer frequencies, i.e. section 3, as suggested by the referee (see last paragraph).

1.2. Specific Comments: Referee 1:

- **Specific Comment 1, Referee 1, page 2297:** The title has been changed.
- **Specific Comment 2, Referee 1, page 2298, line 3:** single layers has been changed to horizontal plane
- **Specific Comment 3, Referee 1, page 2299, line 14-18:** and **Specific Comment 4, Referee 1, page 2299, line 15:** included: stratospheric ozone distributions in mid-latitudes
- **Specific Comment 5, Referee 1, page 2299, line 15-16:** other references included
- **Specific Comment 8, Referee 1, page 2299, line 27:** cut "remaining "
- **Specific Comment 9, Referee 1, page 2299, line 23-(2300)1:** Pacific included
- **Specific Comment 11, Referee 1, page 2300, line 16:** Waugh and Polvani 2000 canceled
- **Specific Comment 12, Referee 1, page 2300, line 17-18:** e.g. included
- **Specific Comment 13, Referee 1, page 2300, line 23-24:** cancelled *which are different in detail:*. At that time we want to focus on the maximum of 3 years of data records and not on the seasonal variations.

ACPD

2, S1118–S1128, 2002

- **Specific Comment 14, Referee 1, page 2300, line 17-23:** included observed
- **Specific Comment 15, Referee 1, page 2300, line 24-(2301)20:** shortened to one sentence per section
- **Specific Comment 16, Referee 1, page 2302, line 6:** almost at the same location included
- **Specific Comment 17, Referee 1, page 2302, line 7-14:** has been shortened as recommended
- **Specific Comment 18, Referee 1, page 2302, line 14-15:** The newly formulated meridional criterion detects a change in the meridional gradient of a tracer distribution in a horizontal plane.
- **Specific Comment 19, Referee 1, page 2302, line 16-20:** Yes, it is less strict than a change in sign, but this is exactly what the text says.
- **Specific Comment 20, Referee 1, page 2303, line 6-10:** We changed the text and hope it is clearer now.
- **Specific Comment 21, Referee 1, page 2303, line 13-15:** We have moved the example to the end of that subsection and hope that it is clearer now.
- **Specific Comment 22, Referee 1, page 2303, line 18:** The vertical layers between 21 and 25 km are now explicitly given for both models.
- **Specific Comment 23, Referee 1, page 2303, line 21-(2305)1:** should be clearer now
- **Specific Comment 24, Referee 1, page 2304, line 1-2:** removed *in detail:*
- **Specific Comment 25, Referee 1, page 2304, line 1/28:** We have included not shown: The study has used other criterions, but the results are not shown.

ACPD

2, S1118–S1128, 2002

- **Specific Comment 26, Referee 1, page 2304, line 22:** sentence has been removed
- **Specific Comment 27, Referee 1, page 2304, line 23-25:** We changed the text slightly and hope it is clearer now.
- **Specific Comment 28, Referee 1, page 2305, line 6:** The vertical coordinate is now described more detailed.
- **Specific Comment 29, Referee 1, page 2305, line 9:** In 1994 the ECMWF changed their operational analyses scheme. In order to provide the scientists with a consistent set of analyses the ECMWF re-analyses former years (1979-1994) with the 1994 operational analyses model (the ERA-15). Thus the ERA-15 plus the operational analyses 1994-1999 are a consistent dataset based on the same analyses method. Regarding the spectral truncation: Only the 1st 42 triangular coefficients are used from the spectral ECMWF data. This provides a non-aliasing horizontal field for the corresponding 64x128 horizontal grid. Since we believe this is obvious for the reader, no text changes are made.
- **Specific Comment 30, Referee 1, page 2305, line 16:** Since pressure altitude is now defined, this should be clear. Sorry for weak precision.
- **Specific Comment 31, Referee 1, page 2306, line 7-14:** has been shortened as recommended
- **Specific Comment 32, Referee 1, page 2306, line 24:** SSTs differ between the different model experiments. The details can be found in the references where the time slice experiments are described in detail.
- **Specific Comment 33, Referee 1, page 2306, line 25:** The authors think table 1 is important to get an overview about the two different model systems.

ACPD

2, S1118–S1128, 2002

- **Specific Comment 34, Referee 1, page 2307, line 1:** We are also addressing future and past conditions. Therefore we have not included *present climate* within the title.
- **Specific Comment 35, Referee 1, page 2307, line 3-17:** The authors do not agree that there are repetitions. In section 2.3 the model E39/C is described, whereas in section 3.1 the setup of the model experiment is described.
- **Specific Comment 42, Referee 1, page 2310, line 24-(2311)19:** We have these plots, but including all those figures (and if you want to show what we have said in the text, we would have to include many of those figures) would go beyond the focus of the paper. It is furthermore already mentioned in the paper via the small standard deviations.
- **Specific Comment 43, Referee 1, page 2311, line 1:** The sentence: *The standard deviation refers to the interannual variability of monthly mean.* has been included in the text.
- **Specific Comment 44, Referee 1, page 2311, line 12-19:** The whole paragraph has been moved to the section *Discussion*.
- **Specific Comment 46, Referee 1, page 2311, line 24-28:** the agreement to older studies has now been pointed out.
- **Specific Comment 48, Referee 1, page 2312, line 22:** the corresponding altitude has been included
- **Specific Comment 49, Referee 1, page 2312, line 10-24:** Agreed and reformulated.
- **Specific Comment 52, Referee 1, page 2314, line 2-23:** The authors do not agree this part should be moved to section2. In section 2.3 the model E39/C is described, whereas in section 4 the setup of the model experiments is described

ACPD

2, S1118–S1128, 2002

- **Specific Comment 53, Referee 1, page 2315, line 3:** text has been changed
- **Specific Comment 54, Referee 1, page 2315, line 6-29:** Text has been changed and the results have been seperated from speculations. Speculations have been moved to chapter 6.
- **Specific Comment 56, Referee 1, page 2316, line 3-13:** Text has been changed and the results have been separated from speculations. Speculations have been moved to chapter 6.
- **Specific Comment 58, Referee 1, page 2316, line 21:** Since 'long range' seems to be confusing, we changed it into 'large-scale transport, e.g. into the troposphere'..
- **Specific Comment 59, Referee 1, page 2317, line 11:** The differences are more discussed now. See also major remark 5.
- **Specific Comment 62, Referee 1, page 2319, line 2:** Unfortunately, much more effort would be necessary to estimate those impacts. The reason is that too many variables are changing with the season. I.e. the ozone source varies, transport varies and chemistry varies significantly during one year. From the concentration changes one cannot conclude on either of them. The only further interesting point is that the seasonal cycle in the troposphere is the same on both hemispheres, which peaks in winter. That means that the seasonal cycle of the chemistry dominates. See last par. of section 5.
- **Specific Comment 63, Referee 1, page 2319, line 14-27:** done
- **Specific Comment 64, Referee 1, page 2319, line 21:** thermally (WMO) definition. Added in Fig. caption.
- **Specific Comment 65, Referee 1, Table 1:** The authors think table 1 is important to get an overview about the two different model systems.

ACPD

2, S1118–S1128, 2002

- **Specific Comment 66, Referee 1, Table 3:** Grid point weighted
- **Specific Comment 67, Referee 1, Figure 1:** The authors think that this figure is needed, in order to show the reliability of observations. The authors think that it is important to have a measurement of streamers as an example in the paper. Therefore we have not cut that figure.
- **Specific Comment 68, Referee 1, Figure 2:** all comments have been considered and changed as recommended
- **Specific Comment 69, Referee 1, Figure 5:** The authors agree that the absolute value might change if area weighted, instead of grid weighted values would be used. But the grid point weighted values compare well with the results presented in Fig. 4, where good agreement in the NH and discrepancies in the southern hemisphere have been found.
- **Specific Comment 70, Referee 1, Figure 6:** same as Fig.5
- **Specific Comment 71, Referee 1, Figure 9:** done.
- **Specific Comment 72, Referee 1, Figure 10:** We added the definition of the tropopause. And rephrased the sentences. It was not meant as an interpretation, but as an explanation of the figure and to avoid misunderstandings.

2. REFEREE 2:

2.1. General Comments: Referee 2:

- **General Comment 1:** The paper has been read and corrected by a native speaker.

ACPD
2, S1118–S1128, 2002

- **General Comment 2:** We have now highlighted, that the horizontal resolution **might** be a reason, why KASIMA detects more streamers than E39/C. Of course the two models are different in their horizontal resolution, but they also differ in e.g. the way they treat N_2O, the transport etc. It is not the aim of that paper to make the two models as "identical" as possible, but the question was, whether those two very different models will derive climatologies of streamers which lead to the same conclusions . We think that this has been demonstrated in the paper.

- **General Comment 3:** The text was misleading and we re-phrased it. In general, it is of interest whether changes in streamer frequencies lead to changes in mid-latitutde ozone. Although CTMS are driven by meteorological data the transport characteristics differ (e.g. Rogers et al., 2002, Met. Z.) moreover, CCMs may even have different meteorology. We just want to point out the possibility of an impact.

- **General Comment 4:** The analysis has not been restricted to 21-25 km. In section 3 and 4 the main focus is on the comparison between the two models KASIMA and E39/C. In section 5 the focus is on interpretation and impact of horizontal transport processes on mid-latitude ozone. In this section we also look at lower altitudes. The upper boundary of E39/C is centered at 10 hPa (roughly 30 km). The layer between 26 and 30 km is therefore already influenced by the upper boundary, why we have not considered that layer in our study.

- **General Comment 5:** The authors think, that including test-studies would be away from the focus of the paper and would be too technical.

- **General Comment 6:** Even if we apply the vertical criteria to KASIMA we would not expect that we could compare those studies directly, because there are also differences in how the two CTM models treat certain processes. The authors think that including the climatology of E39/C derived with the vertical criterion

ACPD

2, S1118–S1128, 2002

(which has been done, but which is not shown) would go beyond the focus of the paper.

- **General Comment 7:** The sentence *The shape of the annual cycle of streamer frequencies seems to indicate a link to the seasonality of the stratospheric polar vortex.* has been removed.
- **General Comment 8:** The changes of the winds are small between the different time slice experiments (not shown).
- **General Comment 9:** The two questions are "What is the source?" and "How does it change the ozone distribution?". So the referee's suggestion addresses point two. We added short version in paraphrases. At the regions were we are looking for streamers, the ozone concentration is smaller in the tropics than at mid-latitudes. Anyway, ozone is not the criterion, but N2O. Whenever we find an N2O streamer we look whether we also find an ozone streamer. The streamer criterion is therefore not affected.
- **General Comment 10:** The text has been changed and speculations have been removed.

2.2. Specific Comments: Referee 2:

- **Specific Comment 1, Referee 2, page 2298, line 2:** Abstract has been changed.
- **Specific Comment 2, Referee 2, page 2298, line 9:** For the first time, the seasonal and the geographical distribution of streamer frequencies has been determined on the basis of 9 years of observations. The focus is on the basis of 9 years. This is the first study, which looks at a longer time period.

ACPD
2, S1118–S1128, 2002

- **Specific Comment 3, Referee 2, page 2298, line 11:** observations changed to meteorological analyses.
- **Specific Comment 4, Referee 2, page 2298, line 15:** validation changed to comparison
- **Specific Comment 7, Referee 2, page 2299, line 22-23:** Paragraph has been rephrased.
- **Specific Comment 10, Referee 2, page 2301, line 2:** cut respective
- **Specific Comment 15, Referee 2, page 2307:** The authors do not agree that there are repetitions. In section 2.3 the model E39/C is described, whereas in section 3.1 the setup of the model experiment is described.
- **Specific Comment 20, Referee 2, page 2322, line 2:** partly removed, statistically inserted

The other specific comments have been changed as suggested by the reviewers.

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