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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climatological features of stratospheric streamers in the FUB-CMAM with increased horizontal resolution" *by* K. Krüger et al.

K. Krüger et al.

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General response:

We thank the two anonymous reviewers and H. Wernli for their valuable comments which helped to improve the manuscript, the results and the discussion of it! There are three points which we want to address in particular in our general response. 1. The detection method and criterion of polar vortex streamers has raised several questions by the two anonymous reviewers, concerning the reliability and the limitations of them. The method has been carefully checked as is presented in the case study and showed interesting new results. However problems using this method for the entire time period cannot be ruled out. Taking into account the comments of the two reviewers we agreed to shorten the results of polar vortex streamers and to discuss the limitations of

the method as suggested by the reviewers. See also the specific comments below for more details. 2. To provide a "deeper insight in the relationship between streamer generation and overall wave dynamics" (reviewer 2), a wave climatology and a correlation analysis for the case study between these quantities has been added in sections 2 and 3. The abstract and the summary are changed taken these new results into account. 3. Concerning the remark of H. Wernli on the role of stratospheric streamers on the mid-latitude ozone trend, we decided to leave this part. The main purpose of this paper lies on the variability of stratospheric streamer events during 10 Arctic winters using a global data set with a consistent spatial and temporal time series to derive a statistical basis.

Specific response to:

Reviewer 1:

The paper is now more focussed as was suggested (see the specific comments below also from reviewer 2). The context of the paper within the literature is discussed in more detail (see the reply on the other two reviews). Most of the suggested changes have been taken into account.

Specific comments:

1-2) Text changed in the ms.

3) The method: The comparison with other methods and the discussion of them have been added in section 2.2 and 5 and left out in the abstract.

4) Inspecting Fig. 3 shows that there are less polar vortex streamers simulated than tropical-subtropical streamers in this time period. This behaviour is clearly resolved in the zonal anomaly field (Fig.4) and is also presented in the detection map in Fig. 5. As it is stated in the ms, a visual inspection of the streamer events corresponds very well with the objective method used for this case study (see also comment in the general response including the comments from reviewer 2 as well.) The text has been

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changed e.g. polar vortex streamers are weaker and are detected less readily (see also response on comment 5).

5) Detection of polar vortex streamers: Fig. 7b is cut out to focus more on subtropical streamers as was suggested by the reviewer (see general response). The discussion of the results is changed in the ms.

6) Constant threshold: the tracer mixing behaviour during the winter season was checked (added in the ms) and the discussion was changed according to the reviewer's suggestions.

7) PV on Eql.: As the PV fields do not show the same fine horizontal transport structures as the advected passive tracer field, it doesn't seem to bring benefit into the ms to detect polar vortex streamers with PV on Eql. Fig. 2 and Fig. 3 show the same time sequence for both fields. Comparing these two fields also shows that subtropical streamers are resolved in quiet good correspondence with the passive tracer fields, whereas "polar vortex streamers" do not show up (that regularly) in the PV field, possibly due to the "weaker" phenomena itself. Therefore a detection of polar vortex streamers with PV would not improve the results of this study. Reasons for this difference might be due to the way of initialization of the tracer field and the different vertical grid and time steps used in the SLT advection scheme compared to the model physics.

8) Technical point: Fig. 3 and 4 have now the same latitude range. The numbers of the colour bars are bigger in the print version.

Reviewer 2:

Comment 1): The mentioned text has been changed. The "very qualitative results" are now discussed at the end of the ms regarding reversible and irreversible transport processes (see also comment of H. Wernli).

Comment 2) Text has been changed to "confirm earlier studies". Randel et al., 1993 and Waugh et al., 1993 are already cited in the paper. We will add the two last points

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("frequent feature" and "improved statistical basis...") in the ms.

Comment 3) Each criterion has been tested carefully and separate from each other. The case study presented at the beginning of the paper clearly demonstrates the good behaviour of the two chosen thresholds. The ratio between the two phenomena can be indeed in certain time periods almost 1 (e.g. year 11 in Fig. 6), but Fig. 6 does not show simultaneous maxima at high latitudes in each year. The two mentioned examples during CRISTA-1 and -2 seem to be cases with dominant wave 2 dynamics in particular with a dominant eastward travelling wavenumber-2 event which frequently occurs during SH winter and can be also present weaker and less often during November-December in the Arctic (for references see e.g. Naujokat et al., 2002 GRL; Krueger et al., 2005 JAS). It would be very interesting to investigate this phenomenon in more detail, but this is beyond the scope of this paper (see also comment to reviewer 1 and reply on comment 5).

(Comment 4 doesn't exist)

Comment 5: To avoid a too lengthy paper this point was left out so far. A plot showing the vertical distribution of wave 1 and 2 for the 10-year climatology is added and also the relation with the occurrence of streamer events. For the presented case study a correlation analysis is added in the text to highlight the physical processes of such streamer events.

Comment 6: See the general response and the reply on H. Wernli comment. The role of a possible trend in planetary wave activity affecting such streamer events is added in the summary.

Minor 1: Change "new result" to "new approach" is done.

Minor 2: The reference Riese et al. 1999 has been changed.

Minor 3: This is certainly true and the effects of the QBO on transport processes are now added in the discussion of the ms.

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Minor 4, Fig.6: In the case study presented the strong maximum in the tropical-subtropical area is on most days related to the developing stadium of a tropical-subtropical streamer. From this region, the streamer is advected north eastward. In some cases the detection of a tropical-subtropical streamer might be related only to a strong meridional gradient in the tracer field without an associated transport phenomenon. We agree that in many cases this maximum might be more related with reversible transport and that the maxima at higher latitudes are more indicative for irreversible transport processes. This is added in the description of Fig. 6 and in the discussion.

Minor 5: Thanks for this hint; we will add this agreement with CRISTA in the description of Fig. 9a, 10a.

Minor 6: We will add this in the ms.

Minor 7: Text will be changed.

Minor 8: Reference will be changed.

Heini Wernli:

We have already replied earlier on the 3 critical remarks. Here is just a short summary concerning the changes in the ms:

1) The text has been changed. Fig. 7 and 8 show the altitude range from 15-40km altitude to investigate where "stratospheric streamers" maximize (see general response). Later on in the ms, Fig. 9, concentrates on the lower stratosphere to show a possible influence on mid-latitude ozone dilution.

2) The "trend" part has been cut out and only been left in the introduction, discussion/ summary part of the paper citing other relevant papers in this context. The purpose of this paper lies on a climatology of streamer events using the advantage of a global data set with a regular spatial and temporal time series for 10 years. This advantage was not available in the studies of Calisesi et al. and Koch et al. This is clearly written

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in the ms. Using a purely Langrangian technique was not in the purpose of this paper, but would be a very interesting study in a next step. The two above mentioned papers are now cited in the ms, thanks for the suggestion.

3) Terminology: see our earlier reply on ACP. Text and papers are added.

3a) Exchange has been cut out.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 6789, 2004.