

***Interactive comment on “The water vapour distribution in the Arctic lowermost stratosphere during LAUTLOS campaign and related transport processes including stratosphere-troposphere exchange” by A. Karpechko et al.***

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We thank the Reviewers for their comments. We found the comments very helpful for improving the manuscript.

Mainly, corrections are applied to Section 5. Following recommendations of Reviewer 2, the link between the PV changes and the turbulence index (TI) values along the trajectories is described and illustrated by two new figures in the revised manuscript. The time step important for STE is considered in Section 4 as Reviewer 1 suggested. Hopefully, these corrections make the link between different parts of the paper more clear. The changes are also reflected in the Abstract and in the Conclusions.

We agree with Reviewer 2 that a simple coincidence of CTF and TI is not an ultimate proof of the link between CAT and STE. This is now explicitly stated in Section 5. However, we still believe that our results are suggestive of the link and valuable since they might stimulate more detailed research in this important field.

In Section 2 additional information about the trajectory model is added and calibration procedure of the hygrometer is described as asked by Reviewer 1 and Reviewer 3 correspondingly. The table with some related information about the flights is provided in Section 3. We also appreciate numerous structure and grammar corrections made by Reviewers. These are implemented as well.

The specific comments by Reviewers 2 and 3 are answered earlier. Below are answers to those comments by Reviewer 1 which do not duplicate the previously answered comments.

Reviewer1: "(1) The description of the measurements is done carefully, but leaves the reader confused about the actual tropopause definition referred to. Different definitions (WMO, two dynamical tropopauses) are applied, but not carefully enough depicted in the text."

Answer: Actually, only one dynamical tropopause is used throughout the text of the manuscript, which is the 3.5PVU isosurface of PV. 2PVU is used only in Figs. 4 and 5 together with 3.5 PVU. Since it causes confusion, the 2PVU tropopause is taken away from the revised figures. Also, in the revised manuscript, it is specified to which tropopause (dynamical or thermal) we are referring to wherever the distinguishing is important.

Reviewer 1: "(2) Part 1 is closing with the identification of two possible STE events that should be investigated. The description of some aspects of the meteorological situation (part 2) and drawn conclusions hereof (in particular the leading paragraph on p. 4736) is confusing. In both chosen cases, the application of the trajectory model does not provide substantial evidence of STE events or, if so, the significant timesteps

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of the trajectory calculations are not shown. Additionally, the second case is discussed only very briefly and the conclusions drawn remain speculative."

Answer: Some sentences in the leading paragraph which, as we think, could cause the confusion are rewritten. Figure 4a is replaced in the revised manuscript so that the time step significant for STE is shown. The 24 February case is discussed only to show that the 17 February case is not unique. Some analysis was however performed and the results were demonstrated in our poster at EGU General Assembly 2006. It was found that this filament was similar to the 17 February one in that sense that it was also associated with a cut-off anticyclone. Since this case is used only as a supportive material for the 17 February case, only a short report is given. In this context we think that it is possible to skip a more detailed discussion.

Reviewer 1: "(3) The description of the tools used for the diagnosis of STE (part 3, in particular the trajectory model and the CAT index) is too short, e.g. it remains unclear on which surface or for which layer the CAT index is shown in Figure 6."

Answer: Additional information about the trajectory model is provided in the revised manuscript. The formula for TI is given. The TI in Figure 6 (new Figure 8) is shown at the dynamical tropopause (3.5PVU).

Reviewer 1: "(4) Dealing with stratosphere-troposphere exchange fluxes, the changing of nomenclature between CTF and STE leads to a confusion of the reader."

Answer: The following nomenclature is adopted in the revised manuscript: we refer to CTF when speaking about calculations made with Eq.2 (new numeration) and to STE when speaking about the process. The abbreviation "TST" is used when only the upward part of STE is referred to.

Reviewer 1: "(5) The reason for the calculation of the STE fluxes both with ECMWF analysis and forecasts is not well discussed either."

Answer: The reason for using forecasts in addition to analyses is that subsequent anal-

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ysis fields are dynamically not consistent with each other, because they are produced in independent assimilation cycles that lack dynamical constraints (Stohl et al, 2004). This leads, in particular, to spurious changes in PV and hence to additional CTF. In general, the analysis fluxes are 20-25% higher than corresponding forecast fluxes (see Fig. 7 in the original manuscript). We found it useful to compare the results and to assess the effect introduced by the spurious PV changes.

Reviewer 1: "The coincidence of the STE fluxes and the CAT index that the authors use as a basis for the correlation approach is not clear to me from Figure 6, as the high values of the CAT index especially inside the anticyclones cannot be found in the STE flux patterns."

Answer: CAT is not the only cause of STE and therefore exceptionally high correlation should not be expected. However, a statistically significant correlation between CTF and TI does exist as can be seen in Fig. 7 in the original manuscript.

Reviewer 1: "(6) I appreciate that the authors discuss their findings very critically on top of p. 4741, but this leaves the reader unclear about (a) why the different diagnostic tools are applied for the whole area and (b) how they are linked to the measurements. My suggestion would be that the authors link the diagnostic tools to the measurements more closely or, if the tools are not able to provide relevant information, they should not be discussed at all."

Answer: The diagnostic tools (namely the CAT diagnostics) are linked to the measurements more closely in the revised manuscript. Analysis of the backward trajectories run for the 17 February case suggests that surroundings of the cut-off anticyclone are important for STE and also that STE is linked to CAT. Consideration of the whole area is done to verify these findings with more data.

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