

Interactive  
Comment

***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.***

**Anonymous Referee #1**

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The paper presents and discusses a set of high-resolution balloon-borne water vapour measurements with different sensors obtained during the LAUTLOS campaign at Sodankylä, Finland. As the primary interest of the authors is to assess the water vapour distribution in the upper troposphere/lower stratosphere (UTLS) region and transport mechanisms here into, the paper addresses relevant questions within the scope of ACP.

The paper is divided into three parts, (1) a description and general discussion of the measured water vapour profiles in the wintertime arctic UTLS region, (2) a description of some meteorological features during selected periods of interest within the LAUT-

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LOS campaign and finally (3) the calculation of stratosphere-troposphere exchange (STE) fluxes based both on analysis and forecast data from the ECWMF model version T511L60 and the discussion of the processes leading to this exchange.

So far, the paper presents novel data for the wintertime arctic UTLS water vapour distribution, but it does not introduce novel concepts, methods or tools. The possible strength of the paper might be a clearly drawn combination of the measurements and the tools used to derive STE fluxes, but currently the parts are not closely enough linked.

Specific parts that need to be improved are:

(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.

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

(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.

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

(5) The reason for the calculation of the STE fluxes both with ECMWF analysis and forecasts is not well discussed either. The coincidence of the STE fluxes and the CAT

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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.

(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.

(7) Finally, the abstract leaves the reader in expectation of a consideration of the processes leading to STE that in the end produces the observed water vapour laminae, but from the applied diagnostic tools, the authors only arrive at speculative conclusions in this context.

(8) A table giving overall informations about the flight dates and payload carried might be helpful.

(9) The figures 4-6 should be revised in the sense of: (a) using the same map projection for easier comparison, (b) annotation of the grid latitudes and longitudes as well as the time as difference to the regarded timestep of interest. Additionally, the contours in Figure 4 cannot be distinguished (i.e. one cannot clearly identify the 2 PVU and 3.5 PVU contour).

Some specific comments concerning language improvements and content clarification are:

p.4729/l.10: misspelling: Fischer instead of Fisher (also in references)

p.4729/l.21: reduplication: TST transport (cancel "transport" [second T in TST is for "transport"])

p.4729/l.28: acronym LAPBIAT not explained

p.4729/l.29: and repeatedly following: date: Jan. 29th instead of 29 January

p.4730/l. 3: ... statistical basis ... (instead of statistics)

p.4731/l.10: misspelling: photodisso\_\_c\_\_iation (c instead of s)

p.4731/l.20: ...to correct for this ... (cancel "for")

p.4731/l.24: ...delay the signal... (insert "the")

p.4732/l. 3: ...from the ECMWF model version T511L60 (currently operational is T799L91)

p.4732/l.16: you refer to levels above 360K, but your figure ends at 360K !

p.4732/l.22: ...mixing across the tropopause... (and repeatedly in the following): avoid confusion about the terms mixing and transport!

p.4733/l.22: ... Fig. 1c and 1a...: "1a" instead of just "a" is less confusing

p.4734/l.5: as above: "1c" instead of just "c"

p.4734/l.6: ...than the value... (insert "the")

p.4734/l.23ff: note that Hoor et al. use a value of 2 PVU for their dynamical tropopause whereas you use 3.5 PVU. This is an additional source for differences!

p.4738/l.25: ...ECMWF model version T511L60... (instead of "T511 ECMWF model")

p.4740/l.7 and 20: (also Figure annotations): label longitudes from 0° to 360° (than cancel "E" and "W") or from 180°E to 180°W, or from -180° to 180° (than cancel "E" and "W"), but do not mix!

p.4742/l.18: you talk of levels above 360K, but your figure does not show them!

p.4742/l.20: you refer to the hygro-pause in the conclusions, but never in the text before

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