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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Transport of Antarctic stratospheric strongly dehydrated air into the troposphere observed during the HALO-ESMVal campaign 2012" *by* C. Rolf et al.

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This is a rather detailed analysis of a very interesting set of in-situ and remote sensing observations in the Antarctic lower stratosphere. The observations are unique and relevant and show very dry air (< 2 ppmv) close to the tropopause. I appreciate it a lot that the authors combined different observations and output from diagnostic tools and I am fully convinced that this analysis can become a very good paper. However, in its current form, the paper is difficult to read and some important conceptual aspects (what is the tropopause, what is STE?) are unclear. Major revisions are required to address the points below, which are hopefully useful to better identify the key aspects

of the study and to more clearly present the nice observations and their implications.

Major comments:

1) In several places the writing should be more precise, for instance in the abstract: - line 3: "in-situ measurements of dehydration", I think you rather mean "of dehydrated air masses" (which is not the same)

- line 8: what is meant by "which has never been observed by satellites"? The statement could mean "which has never been observed before (and before all observations have been made by satellites)" or it could mean "which is in contradiction to satellite observations, which never show such low values". Similarly, I don't understand the intention of the statement on p. 7899 line 16: do you want to emphasize that satellite measurements are not good enough to see dehydrated air masses or that this process is so rare that it has not been seen before??

- p. 7899 line 27: "frequent" should read "frequently", then the rest of the sentence and the next sentence must be rephrased. It is not clear whether the Khosrawi study is relevant for the Arctic or mid-latitudes. Then why do you know that the transport across the thermal tropopause occurs "vertically" (see also comment 2), it can also be along isentropes. Then "directly" is not needed, and "dry the troposphere down to the surface" sounds strange to me - do you mean that a dry tongue of originally stratospheric air is reaching down to the surface? (OK with this, but this is not the same as "drying the troposphere").

- p. 7905 line 18: this is a misleading statement: PV is conserved in the stratosphere along the flow, not in an Eulerian sense (as implied by your sentence). And in the troposphere, it is not small-scale mixing that primarily alters the PV of air parcels but diabatic processes in clouds.

- p. 7915 line 22: "katabatic surface winds"?? Why should they influence your air parcels at an altitude of 10 km? Katabatic winds are typically very shallow and directly

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located above the topography.

- p. 7915 line 23: this is rather speculation than a description of your figure. It might be right that RWB events occurred but you never show this. I suggest that you more strictly separate the parts shown by your data and analyses and the more speculative parts.

2) The concept of what the authors regard as strat-trop exchange should be reconsidered. There are (to me) some irritating statements already in the abstract:

- line 17: "the irrelevant role of the Antarctic thermal tropopause as a transport barrier is confirmed" is a strange statement because the thermal tropopause is never (not only not in Antarctica) a transport barrier, because its definition is based on a lapse rate criterion and the lapse rate is not a materially conserved quantity. Therefore air parcels can without any problems cross the thermal tropopause. A PV-based tropopause is already a bit more a "transport barrier", because of PV conservation for adiabatic flow - so for an adiabatic flow the dynamic tropopause acts as a transport barrier, and because the real flow is not perfectly adiabatic, there is STE. I therefore don't think that this particular "finding" is a key result of this study (which, I think, has many other important things to show!)

- line 20: what is a "weak tropopause"? A tropopause with a weak PV gradient? A tropopause where STE occurs? Per se, the term "weak tropopause" does not make sense.

- line 21: This sounds like a very general statement, but it is well known that the transport of STE air parcels down to surface can occur much faster (within 1-5 days, see, e.g., Skerlak et al. 2014, ACP, and references therein). For this fast downward transport the large-scale flow along tilted isentropes is then much more important than radiative cooling.

- p. 7900 line 14: you should write "can descend ..." instead of "will descend" because

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many air parcels, after crossing the tropopause, will never reach the surface! See, e.g., Stohl et al. 2003 (BAMS) for a discussion of deep STT vs. total STT (deep STT reaching down to the surface is only a small fraction).

- p. 7905 (Fig. 1): why do you show equivalent latitude? On line 20 you write that high values of eq. lat. indicate the polar vortex. Is this really true? Even without a polar vortex you would get high values of eq. lat. somewhere by definition. And the really high eq. lat. values are only south of 70S (which would be a more normal position of the vortex). Then on line 22 you use the high eq. lat. values in the troposphere to infer about STT. Again I am not convinced that this works. If you define eq. lat. separately on every isentrope then you must get high values somewhere, but this does not necessarily point to a stratospheric origin. In the troposphere PV is strongly altered by diabatic processes and therefore PV (and the PV-based eq. lat.) use some of its qualities as a tracer of origin.

- p. 7905 line 26: the three times crossing of the thermal tropopause and the 320 K isentrope is maybe not too meaningful. The two surfaces are rather parallel and they might change in time. Also it is not clear that the flow is along the particular vertical section you are showing, therefore simply from looking at the intersections you cannot infer about STE.

- p. 7915 line 19: even if the vertical PV gradient is relatively weak, a diabatic process is required to change the PV of an air parcel and to make it move across the dynamic tropopause. I think that the argument that the a weak vertical gradient (in one particular cross section!) implies strong STE (i.e., a weak barrier) is too simplistic. We clearly also know of the reverse case where STE occurs due to clear air turbulence near the jet stream (i.e., in a region where the PV gradient is particularly strong). Similarly, the statement on line 22 "... can be transported ... without strong resistance" is very fuzzy.

3) At the end of the introduction I am missing a clear outline of research questions addressed in this paper. The reader is therefore constantly unclear about where the story

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goes and it is difficult to follow the presentation of the results. Having a set of specific questions at the end of section 1 would be very helpful. The same problem occurs at the beginning of section 4 - here it would be very helpful if the reader was presented with a brief outline of what she/he can expect/learn from the trajectory analysis. As presented now, it is difficult for the reader to follow the story.

4) Trajectories are so essential for this study that you should give a better explanation of the input data. Why did you use ERA-Interim reanalyses and not operational analyses (which have a better resolution)? And what diabatic heating rates did you use? Do they only include radiative heating or also latent heating in clouds?

5) The meteorological description of the event is too brief and makes it difficult to put the detailed analyses into context. For instance, on p. 7905 the vortex edge is mentioned to be at 47S, which I think is quite unusual(?). Has the entire vortex been shifted far away from the pole? Also is it fully justified to speak about the vortex edge when looking at PV and winds on 360-400 K? I assume that this is OK and that you have checked that in this specific situation the vortex really reaches so far down into the lower stratosphere, but I think that this deserves a better description (additional figures showing the entire vortex, discussion of how typical/unusual this situation is, etc.

6) Related to 5): on p. 7908 line 22 you write that "the dynamic tropopause ... is somewhat lower than the thermal tropopause", which I think strongly downplays the huge difference between the two tropopauses in this situation. The GLORIA derived thermal tropopause is always above the -4 pvu contour and the -2 pvu contour is up to 4 km(!) lower than the thermal tropopause. Clearly there is exciting dynamics going on with a -2 pvu tropopause reaching below 7 km, but this is not properly discussed. The implications for STE are that crossing the thermal tropopause brings an air mass to a region with PV < -4 pvu, which is not yet the "real troposphere". I think it should be emphasized that the low H2O values observed by GLORIA are mainly/all above the -2 pvu tropopause. This questions then somehow whether you really observed dehydrated air in the troposphere or just in the lowermost stratosphere. To me this

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would be (almost) equally exciting - but I think that this ambiguity (what is the relevant tropopause in this situation? The thermal tropopause appears to be very high, etc.) should be much more carefully discussed.

7) p. 7914, beginning of section 4.2.2: I suggest that this general discussion of the Antarctic tropopause is moved to the introduction and slightly extended. A highly relevant paper to reference is by Zängl and Hoinka, 2001, The tropopause in Polar regions, where they show that in winter the thermal tropopause definition is not very meaningful.

8) The end of the story (Fig. 8 and its description) is a bit weak because of the shift of perspective from a very detailed analysis of the measurements (which I like) to the very coarse analysis of the trajectories over several months (which is very general and does not provide too much insight). It would be very interesting to understand what happens to the observed dry air masses during the following hours and few days (with the one month perspective we always get into the question of do we belive the trajectories? What does it mean that the gray area in Fig. 8c covers everything from 30 to 80S?). Do they enter the folded tropopause structure? Do they move to low/high latitudes (Fig. 8c indicates that they move poleward during the days after the observations: why? There is not much descent during this time period ...)? How does PV change along these air parcels? When do they cross the -2 pvu tropopause and where?

Minor comments:

- p. 7897 line 25: references should be in chronological order

- p. 7898 line 3: "... dehydration extends down ..."

- p. 7898 line 7: "which lie around" is translated from German, maybe "ratios of about 4-5 ppmv"

- p. 7898 line 9: I think this is not really correct, for sublimation temperature is not directly relevant but rather relative humidity.

- p. 7899 line 14: "quite far north up to" sounds odd, maybe "were measured in-situ

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between XdegS and YdegS"

- p. 7900 line 12: the James et al. paper is not really about tropopause folds, maybe Sprenger et al., Tropopause folds and cross-tropopause exchange: A global investigation based upon ECMWF analyses for the time period March 2000 to February 2001, J. Geophys. Res., 108(D12), 8518, doi:10.1029/2002JD002587, 2003, would be a better reference (which also shows that some folds occur along the Antarctic coast).

- p. 7901 line 21 (and in several other places): I don't understand the notation "X% +/- Y ppmv", how can you add ppmv to %? Do you mean that the 6% correspond to about 0.4 ppmv?

- p. 7903 lines 1 and 8: why do define precision with 2 sigma for one instrument and 1 sigma for another?

- p. 7903 lines 21ff: here I have the impression that the text is very general (a most general description of GLORIA), however a description that focuses more on the relevant aspects for this study would be more useful. Similarly, I don't think that you need to mention PSCs for this study (p. 7904 lines 16ff).

- p. 7904 line 2: "quantities at lower altitudes are several ... hundreds of kilometers away" is very unclear.

- p. 7908 line 2: "to focus on air masses where ..." sounds odd, maybe better "to focus on a time period when GLORIA observed vortex air"

- p. 7908 line 10: note that "westerly" is used only for winds (a westerly wind is from W to E), what you mean is probably simply "measured ... west of the flight path"

- p. 7908 line 13: why "seem"?

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