

Interactive
Comment

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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The authors thank Heini Wernli for reviewing the paper and for the many fruitful comments which were helpful to improve the paper. All changes of the paper are highlighted with red colors. Point by point answers to your comments are reported below.

Major comments

- *In several places the writing should be more precise, for instance in the abstract:*

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We revised the text according to your points below.

- - line 3: *"in-situ measurements of dehydration", I think you rather mean "of dehydrated air masses" (which is not the same)*

You are right, measurement of "dehydration" and "dehydrated air masses" is not the same, we changed the text to "dehydrated air masses", because that is what we actually observed.

- - 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??*

We want to state that satellite measurements in the UT/LS do not offer the necessary horizontal and vertical resolution to see dehydrated airmasses down to the tropopause as written on page 7898 l16. But you are right especially in the abstract it is not clear what is really meant "which has never been observed by satellites". We removed this part from the abstract and we added an explanation on page (see page 4 ll 67-71) to make it more clearer.

- - 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").*

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We followed your suggestion and changed it to "frequently" and revised the sentence with the Khosrawi study (see page 5 ll 85-87). We assume a transport along isentropes, but the transport has a vertical geometrical component due to the tilt of isentropes. We agree with the third part of your comment and rephrased it to "Nevertheless, Antarctic stratospheric air masses can be transported through the thermal tropopause into the troposphere and can create a dry tongue reaching down to the Earth's surface." (see page 5 ll 87-90)

- - 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.*

You are right, the PV is only conserved along the flow. But this is actually what we wanted to say. To make it more clearer we revised the text as you suggested: "The PV and equivalent latitude are nearly conserved along the flow in the stratosphere but less strongly in the troposphere due to diabatic processes in clouds (Joos and Wernli, 2012) and small-scale mixing." (see page 11 ll 264-266)

- - 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 located above the topography.*

You are right, katabatic winds are shallow. But nevertheless the mass flux associated with these katabatic winds is significant and results in a general subsidence over Antarctic continent (see Introduction, Van de Berg et al., 2007, Stohl & Sode-mann, 2010). Some of the air descending into the Antarctic boundary layer may originate in the stratosphere as described by Roscoe (2004). Indeed, we have no prove that the airmasses described by the trajectories descend caused by the katabatic winds, but it is one possible explanation for the subsidence visible in Fig. 8 a,b. Therefore we would like to keep the line of argument as it is, but denote it as more speculative.

- - 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.*

We replaced Figure 8 (now Figure 9) by a more detailed analysis (2 weeks before and 2 weeks after the observation) where now 2 RWB events are visible. The text is revised according to the new Figure 8 and the speculative parts are more separated to the end. (see pages 21-23)

- 2) *The concept of what the authors regard as strat-trop exchange should be re-considered. There are (to me) some irritating statements already in the abstract: We revised the text and give answers to your points below.*
- - 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!)*

You are right, the thermal tropopause is based on a lapse rate and is not a materially conserved quantity and much less a transport barrier. Nevertheless, the change in the temperature lapse rate coincides with a change in the static stability. The static stability in the troposphere is low (i.e. unstable with strong vertical mixing), while in the stratosphere it is higher (i.e. stable with weak vertical mixing). The thermal tropopause separates both regimes. Therefore, we think that

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it is justified to speak about a thermal tropopause with enhanced permeability, if the tropopause is only poorly defined. Nevertheless, we changed the text to focus more on the adiabatic transport of dehydrated air masses within the folded PV structure. (see Introduction p.5 ll 92-102)

- - 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.*

We agree, the term "weak tropopause" is a bit awkward/unclear. We changed the wording in the whole text to thermal tropopause with enhanced permeability. With more explanation in the text according to the thermal tropopause and static stability. (see Introduction p.5 ll 92-102)

- - 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.*

We changed the statement to several days. Because with the new Figure 8 (now 9) it becomes obvious that the air masses represented by the green line descent from 8.5 km down to 3 km within 4 days. In the old Figure, this behavior was smoothed due to averaging over all 1400 trajectories. Also the downward transport of air masses represented by the red line is fast (3 km within 4 days) but started later. We also mentioned the fast transport along tilted isentropes in addition to the radiative cooling in the introduction. (see Introduction p.5 ll 108-113)

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

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We agree on this point. The text is revised already relating to previous point (see Introduction p.5 ll 92-102).

- 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.*

We used the equivalent latitude because it visualize the stratospheric intrusion in a nice way. You are right, using equivalent latitude especially in vertical cross section might lead to false conclusion. But in this case we get the same conclusion, if we use the PV to show the stratospheric origin. We therefore exchanged Figure 1b showing the PV instead of equivalent latitude. The new Figure 9b displays the PV along the trajectories showing a rather constant median value of -4.3 PVU in the two weeks before the measurement time. Another point is that the structures of air masses below thermal tropopause in Figure 2b and vortex air in the equivalent latitude and PV fields are connected and coherent, so that there is a strong evidence that these air masses down to 5 km in the latitude range from 60-45°S have a stratospheric origin. In addition, the observed dry air below the thermal tropopause is also a strong indicator for a stratospheric origin of these air masses. (see page 11 ll 262-275)

- 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.

We are very sure that the surfaces of the thermal tropopause and isentropes will change in time. But this snapshot in time indicates the region of potential transport. We remove the specific locations of the intersections and write it in a more general way (see p.11 ll 276-282).

- *- 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.*

The text is not contradictory to your comment. For sure the PV has to change in order to cross the dynamical tropopause. However, if the PV gradient within the stratospheric intrusion is weak, the air masses can more easily detach the PV structure and become more tropospheric compared to a case with a strong PV gradient. From the new Figure 9 we see that the PV changes in the following days after the observations (green line) imply a decrease of PV that air masses become more tropospheric. Due to the extreme dryness of these air masses, we think that most likely no cloud processes but rather diabatic cooling is the reason for changing the PV. The statement "... can be transported ... without strong resistance" is removed.

- *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 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*

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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.

This is a good suggestion. We introduced three specific questions in the introduction (see page 6 ll 115-122), which will be answered in the trajectory section and summarized in the conclusion section (see page 23-24).

- *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?*

We used ERA-Interim reanalyses because they provide full diabatic heating rates in the UT/LS region in contrast to the operational analyses. We include the following text into the trajectory section to give more information about the trajectory calculations and the used input data (see page 15 ll 373-383):

"To resolve transport processes in the troposphere influenced by the orography and transport processes in the stratosphere where adiabatic horizontal transport dominates, the hybrid σ - θ coordinate ζ is used (Mahowald et al., 2002). Therefore in the stratosphere and in the UTLS, potential temperature θ is employed as the vertical coordinate of the model and the cross-isentropic velocity $\dot{\theta} = Q$ is deduced from the ERA-Interim forecast total diabatic heating rates Q , including the effects of all-sky radiative heating, latent heat release and diffusive heating as described by Ploeger et al. (2010). In the tropospheric region defined by the condition $\sigma = 0.3$, the vertical model coordinate smoothly transforms into an orography-following $\sigma = p/p_s$ -coordinate (p - pressure, p_s - surface pressure), with the vertical velocity transforming into the corresponding $\dot{\sigma}$ Pommrich et al. (2014)."

- 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.*

The vortex edge is derived with the Nash criterion, which is fulfilled down to the 340K level (see Figure 2b). The vortex has typically a concave outer shape, where the bottom is broader. In addition, the bottom of the vortex is stronger disturbed by Rossy waves propagating from the troposphere. So we think that this situation occurs regular, but we cannot state numbers or rates. This would need additional analyses, which would be out of scope of the paper. But we included two additional Figures where we show the potential vorticity on two theta levels (310, 350 K) to give more insights how the entire vortex behave (see page 10 || 239-250).

- 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*

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then somehow whether you really observed dehydrated air in the troposphere or just in the lowermost stratosphere. To me this 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.

We agree to this point, that we cannot state that the observed dry air masses down to 7km are already in the troposphere. We revised the text (also according to your point 2), which contain a better description of the dynamic tropopause and a more diminished statement to the thermal tropopause. In the revised Figure 8 (now 9), we show that parts of the air masses between the thermal tropopause and the -2 PVU isoline are transported into the troposphere since they change their PV to values between 0 and -2 PVU.

- *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.*

The part is shifted to the introduction with citing the paper of Zängl and Hoinka, 2001. (see page 5 ll 92-102)

- *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 believe 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*

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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?

You are right, the rather long perspective is not very meaningful. We therefore replaced the Fig. 8 by a more detailed / shorter timescale analysis where we show the trajectories from all GLORIA observations (< 3ppmv) below the thermal tropopause in a two week perspective. We separated the trajectories every time a RWB event happened and a subset of air masses (green and red line) were detached and separated from the rest. With this analysis it becomes better obvious, that one Rossby wave event is not the sole reason for the large downward transport. The separation in different branches reduces also the gray area and makes the trajectory analysis more meaningful. The text is revised according to the new Figure (see page 21 || 544-584).

Minor comments

- - p. 7897 line 25: *references should be in chronological order*
Changed.
- - p. 7898 line 3: *"... dehydration extends down ..."*
Changed.
- - p. 7898 line 7: *"which lie around" is translated from German, maybe "ratios of about 4-5 ppmv"*
Changed.
- - p. 7898 line 9: *I think this is not really correct, for sublimation temperature is not directly relevant but rather relative humidity.*
Changed to "sublimate caused by sub-saturation of water vapor at higher temperatures".

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

- 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).

We exchanged the reference to Sprenger et al., where it can be more clearly seen that tropopause folds occur more often in midlatitudes (see page 5 ll 113-115).

- 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?

This is a notation which is often used for instruments to specify the relative uncertainty (%) with a constant uncertainty due to the precision (see e.g. Fahey et al., AMT, 2014). We changed it to $\pm(X\% + Y \text{ ppmv})$ to make it more clearer.

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

There is no hard definition for the precision of an instrument. So typically each instrument group state its precision with specifying the way how it is determined (1σ , 2σ , etc.).

- 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).

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The GLORIA instrument is relatively new, thus it is rather unknown for most people. Therefore, we kept the description simple and referenced to the corresponding papers. Nevertheless, we give few information more about the H₂O product used in this study. Regarding the second part of your comment: We used the CALIPSO PSC cloud product to identify freezing events along the trajectories in order to show that dehydration is present in the lowermost stratosphere. The two sentence describe the data product. So we keep this description here.

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

The vertical location of retrieved quantities approximately follows the tangent points of the measurement geometry (parabola curve through the atmosphere). With the following additional sentence we try to make the measurement geometry more clearer: "Assuming a flight altitude of 13 km the water vapor observation at 12 km is horizontally 113 km away from the aircraft while at 8 km it is already 250 km." (see page 9 ll 207-213)

- - 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"*

Changed.

- - 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"*

Changed.

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

We see it east and west of the flight path in a distance of up to 250 km but cannot ensure 100% that it is below the flight path / aircraft. But to avoid any confusion we skip the the word "seem".

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