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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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The authors thank Howard Roscoe 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

- *First major comment: As author of a paper speculating about descent of Antarctic stratospheric air into the troposphere (Roscoe 2004, referenced by Rolf et al.), I was greatly looking forward to reading this work in detail. My earlier speculation was that some air should be descending over a broad area within the stratospheric vortex for a large part of the winter and early spring, based on various lines of circumstantial evidence. It would have been very pleasing for the speculation to be proven. If so, the implications for the water vapour budget in the troposphere over the Antarctic Plateau, and for the isotopic composition of snow in ice cores there, would have been very significant. Unfortunately, Rolf et al. do not indisputably show descent over a broad area, nor for a large part of the winter and early spring. Instead they show transport over a limited area (a few degrees latitude and longitude) on one day. They show how there is a strong probability that it is caused by a breaking Rossby wave, which suggests that the transport identified is unlikely to be widespread or frequent. Although their Figure 8 is interpreted as showing a widespread occurrence of such episodes, Figure 8 is based on trajectories of such long duration that their validity must be considered doubtful (see D below). Hence, while the title, abstract and arguments within the bulk of the paper are factually correct, the importance of the discovery to the wider questions of water budget and isotopes in ice cores is probably rather limited.*
  You are right, we don't see a decent of air masses from the stratosphere into troposphere over a broad area or the entire Antarctic. We see also no evidence for changing the isotope ratio in ice cores. Nevertheless, transport and exchange processes in the Antarctic UT/LS region are insufficiently investigated due to the lack of adequate observations in this region. We cited your paper (Roscoe 2004) due to the description of the downward transport mechanisms which might facilitate the transport from the stratosphere into troposphere. Even if this transport is caused by Rossy wave activity, we think it occurs more often and permanent in the Antarctic lowermost stratosphere (see page 22 ll 555-580). Regarding your comment on the trajectory length in Figure 8, we included a new Figure 8 (now it

is Figure 9), where we give more detailed insights in the behavior of the trajectories (i.e. shorter timescales of 2 weeks before and after the observations).

- *Second major comment: The authors consistently describe air at 8 to 9 km as though it were obviously tropospheric. This is often not the case in Antarctic winter and spring within the vortex. In the example shown in Figure 3 of Roscoe et al. (2004) of temperature and ozone structure at Halley (76S) on 27 August 1987, the lapse rate becomes stable and the ozone amount increases sharply above about 290 hPa, which was at 8.4 km. On 1 September 1987 the ozone increase started at 300 hPa, which was at 8.2 km (Gardiner & Farman 1988). There are many more examples of such a low tropopause in Gardiner & Farman (1988), which contains convenient plots and tables for examining this issue. One difficulty with the "thermal tropopause" calculation, from ECMWF and GLORIA data in Figures 1 and 3 is that such a calculation would normally define the tropopause as the height where the lapse rate changes sign. In the example in my Figure 3 (Roscoe et al. 2004) this would be at 30 hPa, clearly a long way into the stratosphere. (What matters is the height at which the atmosphere becomes ultra-stable, with a lapse rate less than, say, 3C/km, which would always be at a much lower altitude than where the lapse rate changes sign. A tropopause defined by the discontinuity in ozone amount would also often be at a much lower altitude than where the lapse rate changes sign Finally, most authors would assert that a potential temperature of 310 K in Antarctic winter and spring is in the lowest stratosphere, and 290 K is probably in the troposphere. In Figure 3, the 300 K contour would be at about 8.5 km within the vortex on the earlier flight leg, and down to 7.8 km during the dehydration event in the later flight leg. Again, much of the discussion about the dehydration in the later flight leg is about air that is very likely still in the stratosphere. I do not argue that the premise of the paper is wrong because of these factors - Figure 3 shows clear transport of dehydrated air down to 7 km - merely that some of the analysis of data in the paper contains*

  statements about tropospheric air that are contestable or over-emphasised.
  We agree to the point, that we cannot attribute the air masses below the thermal tropopause to be tropospheric at the time of the observation. However, the observation shows clearly the transport of dehydrated airmasses down to 7 km. Even if the air masses are still in the lowermost stratosphere, it is very likely and shown by Figure 9 (revised Figure) that these low air masses would be transported further and become really tropospheric. The thermal tropopause definition in this study based on the World Meteorological Organization (WMO) using the lowest height at which the temperature lapse rate decreases to $\leq 2\,$K/km and remains below this value for a range of at least 2 km. So we don't chose the height were the lapse rate changes sign. We include the description of the tropopause calculation in Section "Instrumentation and meteorological data". We revised the text and describe the air masses as tropospheric or stratospheric more carefully.

- *Third major comment: The transport event described in the paper is very reminiscent of a tropopause fold. These normally occur on the poleward side of the mid-latitude or the sub-tropical jet, and take stratospheric air sideways and equator-ward as they descend. But for Figure 1b, which shows no sign of the mid-latitude jet near the event at 47S, we might easily have characterised the event described as a conventional tropopause fold. Surely, this aspect of the discovery deserves comment, and perhaps some analysis of why it is more significant than a conventional tropopause fold?*
  We actually don't state that it is completely different compared to a conventional tropopause fold. You are right, the mechanism are similar/same as a conventional tropopause fold. The point is compared to a tropopause fold at the mid-latitude jet that the air masses originate directly from the vortex and were strongly dehydrated. These air masses have a different chemical composition than the "normal" air masses in the mid-latitude lowermost stratosphere. This point is included in the description of the meteorological situation (see P. 12 ll 271-275).

Minor comments

- *A. The last paragraph on page 7899 misses an important point identified by Lee et al. (2002): the lowest stratosphere below the vortex, from about 300 K to about 330 K, is (significantly mixed with mid-latitude air – the vortex barely exists at these altitudes as an isolating mechanism.*
  We agree on this point. The vortex edge derived from the Nash criterion visible in Figure 1b reaches down to 340 K. Below the 340 K filaments of enhanced H2O in the lowermost stratosphere are visible in the GLORIA observations (Figure 3). With the trajectory analysis, we show that the origin is due to mixing from outside the vortex. We include the reference with an additional sentence in the introduction (see page 4 ll 80-84).

- *B. Page 7900 lines 6-8 describes my idea that katabatic winds provide a downward force over the Plateau, because of the expected paucity of re-supply to be expected as a result of the Coriolis force, as though it were a proven fact. It is not proven.*
  We revised the text and state the idea of katabatic winds more as a possible suggestion for downward transport (see page 5 ll 103-106).

- *C. Possible rehydration after ice particles have fallen is an important topic. It would be very helpful to see the water measurements during the dive, which the authors say (page 7912 lines 3-4) confirm the absence of any rehydration features.*
  Figure 1 shows the vertical profiles from the dive. In the stratosphere no rehydration layers are visible. The increase at 10km is associated to mixing of tropospheric air, which is indicated by a decrease in O3 and an increase of CO. The peaks 9.3-9.5 km can be attributed to clouds in the troposphere below the thermal tropopause (blue dashed line), as the saturation mixing ratio is lower than that of the total water mixing ratio. We don't want to include this Figure into the

manuscript, because it fit not very well into the scope of this study and the dive has a different location as the stratospheric intrusion.

- *D. Most workers would regard back trajectories calculated for 50 days (Figure 4) or 150 days (Figure 8) as being of doubtful validity. Surely there should be some discussion about this?*
  You are right, that a trajectory length with 150 days has to be discussed and treated carefully. We actually don't need the trajectory length of 150 days to explain the mechanism. We decided to have a more detailed look into the trajectory properties 2 weeks before and 2 weeks after the observations. Therefore, we replaced Figure 8 where now 2 RWB events are visible and revised the text accordingly (see p. 21-23).

- *E. The confusion about heights and potential temperatures discussed in the second major comment above continues on page 7915 line 27, where 5 km altitude is reported as being at 300 K. During the ozonesonde flight of 27 August 1987 (Figure 3 of Roscoe 2004), 5 km was at 286 K, and 300 K was at 8.9 km.*
  You are right. In the text the median potential temperature and height of all trajectories are described. Some of the air masses are transported to lower latitudes over time and therefore have a different potential temperature/altitude dependence. We revised the text and describe the air masses and potential temperatures more carefully (see p. 21-23).

Editorial comments

- *1. p7898 l3 – replace "extend" by "extends".*
  Changed.

- *2. p7898 l3 – replace "changing" by "changes".*
  Changed.

- *3. Fig1 caption l5 – replace “indicate” by “indicates”.*
  Changed.
- *4. Fig1 caption l6 – surely the dots are blue-green (turquoise if you prefer), not blue?*
  Changed.

figure-1.png

Fig. 1. In-situ measurements during the dive at the most southern point during the ESMVal Flight on 13 September 2012: blue profile in the left panel during decent, red profile in the left during ascent. The measured thermal tropopauses during decent and ascent are marked with dashed lines. Tracer measurements during the decent of CO and 03 are shown in the right panel.