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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying transport into the Arctic lowermost stratosphere" *by* A. Werner et al.

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to Anonymous Referee 3:

Main concerns 1. The Lack of rigor in the description of the lowermost stratosphere and related processes. This is mostly reflected in the introduction. Take an example from the last sentence of the first paragraph (L3-8, p1409). A long list of processes, phenomena and locations are mis-matched and lumped together to describe the isentropic transport processes, some are not isentropic/adiabatic (cu-off low, for example).Several sentences in the 2nd paragraph are either ambiguous or wrong. For examples, "STE processes are periodically occurring events, associated with synoptic and mesoscale processes, and do not have a constant influence on the

LMS." (L10-12, p1409). And "Due to the vertical stability of the stratosphere, the tropospheric influence cannot penetrate deeper into the stratosphere and is mixed with the downwelling air from the overworld." (L14-16 p1409). Both reviewers 1 and 2 have already commented on some of the problems here.

1. Critically review the statements you made in the introduction. Check each time you mention STE, if you intend to describe two way exchange or only troposphere to stratosphere transport. STE processes are not "periodically" but "episodically" occurring events and they do have a "constant" influence to the LMS in the sense they always influence but may not be the same amount. Be aware of the convective influence to the LMS. You can justify the effect of that is neglegible in winter. Check into Appenzeller et al., 1996 for seasonality of STE based on mass balance.

The introduction has been partly re-written.

The main point here is that the assumption of isentropic transport is largely unnecessary and hardly used in the analyses presented, why insist on it in the introduction?

I agree, the assumption is not necessary as the analysis does not separate between different transport processes across the tropopause. However, the mostly relevant process will be quasi-isentropic transport across the tropopause an further into the LMS.

In fact, the analysis presented is opposite to the assumptions of isentropic transport. If you assume isentropic, the tropospheric boundary conditions should be taken from those isentropes connecting troposphere and lowermost stratosphere in the same levels you report your mass balance analyses, typically from the lower latitudes. On the contrary, you derived your boundary conditions using the lowest part of you *profiles, taken at high latitudes and likely below the isentropes of your mass balance analyses. What are the actual isentropes and latitudes of the data that went in the boundary condition calculations?*

3. Examine the assumptions of your entry point values/boundary conditions. How does the values you use from your measurements at delta theta <-10K represent the tropical and subtropical upper troposphere, where the isentropic TST would have dictated the airmass? What about the latitudinal gradient of the tracers? What are the known climatology of some of the tracers?

The temperature dependence of H2O is accounted for by a theta-dependent boundary function. The analysis does not opposite the assumptions of isentropic transport. The tracers N2O, CFC11, H-1211 the troposphere can be regarded as homogeneously mixed. For existent inhomogenities and latitudinal gradients of O3 and CH4 accounted for with a rather large error.

2. Ambiguities in the data description. Part of the above-mentioned problems is due to the ambiguity in the data description. After reading twice, I am still confused of what are the campaigns, time period, number of flights, latitude covered by the data. What are the dates and fractions of sampling shown in Fig 2., Fig 8 and Fig 9? These made the discussion session difficult to follow. 2. Improving the data description. Make a table to list the time period covered and the number of flights from each campaign that entered data analyses. What are the sampling rate and corresponding representation of LMS air mass?

A table will be inserted with the description of the used data. Concerning the time/spatial resolution of the data: A sample is taken every 90 seconds; The sam-

ple size is 2 ml at a minimum sample flow of 60 ml/min. Thus the sampling time is 2s or faster. At a cruising speed of 750 km/h (200 m/s) the horizontal resolution is about 400 m. At average ascent and descent rates of the aircraft of 10m/s the vertical resolution is therefore 20 m.

This information has been inserted in the data section.

3. Physical meanings of the negative contributions from the troposphere. Both Figs.8 and 9 show negative fractions of tropospheric contribution, especially at high isentropes (380-400K). Little discussion is made to explain the physical meaning of that. Given that the analyses is based on a 3 part mass balance (a triangular box of three sides/boundaries), the results of the first two fractions are not believable if the results of the 3rd one is non-physical. If we look into the regions of negative fractions, they are most likely not troposphere. To what extend these are results of using a tropospheric boundary condition for tropical lower stratosphere, or physically represents equatorward transport to balance the large inflow from the vortex?

As pointed out in section 5.2., p. 1422, the boundaries especially at 400 K exhibit rather large error bars and the available tracers do not provide sufficient constraints to properly solve the equation system. However, the results are zero within their error bars.

But you are absolutely right, the negative fractions indicate a problem with the boundary conditions. The tropospheric boundary is definitely a big problem for H2O, where I might not really capture the correct theta adjustment. Here, cross-isentropic transport or mixing provides another source of error. Additionally, the input of water vapour into the LMS is highly variable. However, I account for these errors with large error margins for the tropospheric H2O boundary. N2O, CFC11 and H1211 have such a long lifetime and are well mixed in the troposphere and show no significant latitudinal gradient.

Yes, O3 and CH4 have a spatial variability. However, in case of O3 this variability can be neglected compared to the large differences of severely ppm between tropospheric and stratospheric O3. For CH4 a representative value for the free troposphere (with an error of 2%) was chosen as well, as HAGAR CH4 measurements between mid latitudes (down to 30°N) in autumn 2002 and the high latitudes in winter 2003 match quite well within this 2% and thus describes the relevant tropospheric boundary for a large latitude interval quite well (see attached PIC).

The bigger problem or error arises, in my point of view, from the dynamic vortex boundary where I assume that horizontal inhomogenities are neglected. In some cases the measurements in the LMS lie below (above for O3) their referring vortex boundary value. Thus, the equation system can only solve the problem by adding a negative fraction. And the equation system reacts very sensitive to errors in the boundary conditions (see section:5.2). I played around with the boundary values and by this you can of course influence the results. However, the reaction of the equation system to changes in the boundary is not always straight forward, as the tracers do not strictly provide sufficient constraints for the solution. Maybe the results could be improved by adding other tracer data, providing more constraints to the equation system. Other tracers were not available from these flights.

However, justifying an adjustment of the vortex boundary (i.e. intensifying the descent, regarding different) in order to "fit" the data into the expected frame feels quite arbitrary to me. And the determination of the vortex boundary like it was done here is straight forward: Taking descent from the CLAMS model, calculating the time when a certain air mass had crossed the 400 K isentrope and determining the mixing ratio of a tracer at 400 K at this point of time.

4. Examine the tropical tropopause height for the season of your data analyses. Is the average higher or lower than 380K? That will provide a fact if 380-400 belong to the lowermost stratosphere for the measurement period. If not, how many terms should

you use in the mass balance equation for this region?

Generally you can surely talk of at least 4 source regions (Vortex, mid-lat stratosphere, Troposphere, Tropical Lower Stratosphere) but the here respected tracer mostly do not provide the corresponding information. And the main idea was to stay as close as possible to highly accurate tracer data from the season autumn 2002 March 2003.

5. Provide statistical information of your results. How many data points went in the mean and deviations in Figs 8 and 9?

The single data points which went into the calculation of the mean and their deviation are plotted in Figures 8 and 9. and vary from interval to interval. In case of only 2 ore 3 data points the respective error bar reflects this, as the number of samples goes into their calculation. Each single data point is the result of $10^5 calculations. Thiswasdescribedinsection5.2.$

What are the fractions of the low N2O observations? How well do they represent the vortex season?

They do represent an air mass at an equivalent latitude north of 67 N above 340 K (as shown in Fig 10), thus, the area below the vortex region in the winter 2002/2003. Does this answer you question ?

6. Check the word "Criterium". It is more common to use "Criterion/Criteria".

Yes, changed it to "Criterion".