

Interactive comment on “Quantifying transport into the lowermost stratosphere using simultaneous in-situ measurements of SF₆ and CO₂” by H. Bönisch et al.

H. Bönisch et al.

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We thank you for the helpful and constructive review. In general, we followed most of your suggestions. In the following you will find detailed answers to your questions and suggestions.

Abstract

You should be more clear in your description of the origin of air in the LMS. Your results show that air in the LMS above the ExTL is predominantly either from the lower or upper branch of the Brewer-Dobson circulation. Very little air enters this region from the extratropical tropopause as was also shown by Hoor et al. 2005. Thus, the distinction of tropospheric vs. stratospheric origin of air is somewhat misleading. The air that

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enters the LMS by the lower branch of the BD circulation does enter the stratosphere in the tropics before moving to the LMS. So this air is really of 'stratospheric' origin as much as the upper BD branch air. I understand that it is the convention to refer to the air as originating from either the stratosphere or the troposphere but that really doesn't describe what is happening and is somewhat confusing in light of your results. All of the air in the LMS originates in the troposphere with some taking the high BD branch and some the low BD branch just as your Fig. 4 shows. Thus, it would seem to be more accurate to define the 2 BD branches early on and refer to the air as originating from one branch or the other.

It is true, the air in the LMS above the ExTL is a mixture of the 2 BD branches, but in addition to this there are also air masses that are transported horizontally across (or above) the subtropical jet (core) directly from the upper tropical troposphere into the LMS. That means, the fraction α_1 - called 'tropospheric fraction' here - is a composition of air masses transported isentropically from the TTL directly into the LMS and air masses transported along the lower branch of the BD which enters the tropical stratosphere above 380 K via the tropical tropopause. It is not possible to distinguish between these two pathways with the method applied here. In this study all quasi-horizontal pathways from the tropics in the extratropical stratosphere below the upper boundary limit of 3 years is subsumed to the tropospheric fraction. This might be different to other studies and should be stated out clearly because it is important for the understanding of the mass balance results here.

For this reason we add the following statement in the introduction:

' ... the LMS above the ExTL. In this part the chemical composition is dominated by the relative strength of quasi-horizontal transport and mixing from the tropics into the extratropics and downward transport driven by the upper branch of the Brewer-Dobson circulation (e.g. Hoor et al., 2005; Sawa et al., 2008). The quasi-horizontal transport into the LMS can be further divided into two different pathways: 1) across the subtropical tropopause and 2) along the tropical tropopause with subsequent subsidence in

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the extratropics. This second pathway is referred to as the lower branch of the Brewer-Dobson circulation, which transports air quasi-horizontally from the tropically controlled transition region above the tropical tropopause but below the lower edge of the tropical pipe to the extratropics (Rosenlof et al., 1997).'

1. Introduction

Bottom of pg. 21232. You mention that the transit time from the troposphere into the stratosphere has not been derived in previous mass balance studies of the LMS but in Ray et al. 1999 we did use SF6-CO2 correlations to infer a transport time scale for the case where the LMS was predominantly of tropospheric character. We showed that the transport must have occurred within roughly a month and certainly less than 2 months in September, consistent with your results for this season shown in Fig. 7.

Sorry about this. I forgot that you have also calculated transport times in your study. So we changed p-21232 l-26 into: 'The seasonality and spatial distribution of mean transit times are important new information that has not been derived previously in other mass balance studies of the LMS (e.g. Ray et al., 1999; Pan et al., 2000).'

Indeed, you already explored and mentioned the potential of simultaneous SF6 and CO2 measurements for transport diagnostics and you derive similar transport timescales for September than we do. So we added p-21246 l-12: ' ... and the shortest of about 0.2 years in August. The latter is consistent with the findings of Ray et al. (1999) who estimated transit times of roughly 1.5 month for September.'

2. Data Set

Top of pg. 21234. I'm not sure what it means when you say that you can only use the SF6-N2O correlation to derive SF6 if it's been observed 'in real time'. Does it mean that you have to have some flights with SF6 measurements during each season otherwise you can't assume a relationship based on other seasons?

That's it. We used the term 'real-time' because the SF6-N2O correlation in the LMS

varies with season. Therefore you should have relationships from the same month or ideally from the same campaign.

3. Mean age

Top of pg. 21236. Should be monotonically not 'monotonously'.

We revised it.

Middle of pg. 21237. In the discussion of the negative mean ages you mention that they are a result of more NH air entering the LMS. You also state that the negative values indicate the region of extratropical strat-trop exchange influence on the LMS. You don't mention whether a more NH vs. SH composition of air in the tropics could have an influence on mean ages in other parts of the LMS, where you have average positive ages. In other words, you imply that the region of negative mean ages is the only region influenced by tropospheric air of a more northern extratropical character, but this isn't necessarily true. There may be an influence beyond the negative mean age region.

Related to the above comment, in the discussion of Fig. 2 it would be interesting to know how much variability there is in the mean age at each location. What is the standard deviation? This would show whether you ever get negative mean ages above the ExTL but they are averaged out by the mostly positive values.

The remark is correct. Our formulation was not precise enough. Negative mean age values proof the influence of polluted or extratropical NH air in the LMS. Positive mean age values do not insure that there is no influence of extratropical NH air. As noted by the reviewer, there is an influence beyond the negative mean age region (see also answer on comment of Reviewer 2). For this reason, we use as lower boundary constrain for the mass balance study mean age derived from SF6 greater than 0.3 years. This criterion has been deduced from analysis of the 1-sigma standard deviation of mean age of air as a function of DTheta which represents the distance to the local

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tropopause (2-PV-contour) in units of potential temperature. Mean age as a function of DTheta minus 1-sigma standard deviation is positive above the level of mean age greater than 0.3 years. We do not intend to go into detail how the mean age criterion exactly matches the ExTL derived from CO-O3 or the TIL derived from temperature profiles. A study on this topic would be an interesting paper on its own, but we added a reference to recent studies on this topic 'This TIL roughly corresponds to the ExTL in terms of location and vertical extent and recent studies have suggested a radiative link between the TIL and the specific water vapour and ozone distributions within the ExTL (Randel et al., 2007; Hegglin et al., 2009).'

4. Mass balance

Bottom of pg. 21241, How much does the 3 year upper boundary condition affect the calculation?

During the setup of the mass balance, we did some sensitivity tests with the upper boundary condition. It turned out, that only the fractions show a significant impact on the changing of the upper boundary constraint whereas the transit times are more or less unaffected. As older or as higher the upper boundary is defined the greater the tropospheric fractions gets. This is simply caused by the fact, that 'tropospheric fraction' is defined in this study as that part of the air which has been transported below the upper limit of the mass balance from the tropics into the extratropics. However, it is not reasonable to use more than 3 years as upper boundary constraint because the seasonal cycle of CO₂ is smeared out above this level. Therefore both tracers give no linear independent information and the equation system is under-determined. Using less than 3 years would restrict the study to a smaller region what is also not preferable.

Bottom of pg. 21242, I'm confused about the boundary condition chi_{1,in}. You say that the control surface is the tropical and subtropical tropopause but you have insufficient data there. So you use the surface measurements averaged between 0 and

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20N to 'represent the temporal behavior of both tracers at the tropical and subtropical tropopause region.' This means that the control surface really isn't the tropical and subtropical tropopause is it? How did you come up with 0-20N surface measurements to represent the tropical and subtropical tropopause? Did you test different latitude ranges to see how it affects the results? Boering et al. used the average of Mauna Loa (19N) and Samoa (14S) surface measurements delayed by 2 months to represent the tropical tropopause entry value of CO₂. How does your 0-20N average compare?

Indeed, the formulation for the boundary condition Chi1 is misleading. The control surface for mode G1 and as well as for G2 is the earth surface. For clarification: All transit times are relative to the earth surface and not to the tropopause. Therefore we rephrase p-21242 l-19: 'Due to this restriction, the control surface Omega1 should be the tropical and subtropical tropopause only and the entry functions Chi1 for SF6 and CO₂ should be the time series of both tracers at this control surface.' And p-21242 l-23: 'Hence, same as for Omega2 the earth surface instead of the tropopause has to serve as control surface Omega1.'

And looking ahead to the results section and calculations of gamma1 you show values less than 2 months for some seasons. Is this a transit time from the surface or from the tropopause?

See above: All transit times are relative to the earth surface.

4.3 Results

Middle of pg. 21245, Why are there no troposphere fractions in July in Figs. 6 and 7 when there are mean ages for July in Fig. 2?

The reason is that there are no CO₂ measurements due to technical problems. Mean age is derived from SF6 only whereas both measurements (CO₂ and SF6) are necessary for the calculation of fractions and transit times. For clarification we add the following statement p-21245, l-19: 'We can not calculate tropospheric fractions for July

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because the CO₂ measurements are not available for this campaign due to technical problems.'

Bottom of pg. 21246, In the discussion of the fast transit times γ_1 you mention that 'the observed tropospheric fraction in winter has entered the LMS predominantly during August and September.' This is because γ_1 is 4-6 months in January and February. But this goes back to my point above that you are describing these transit times as air entering the LMS but the calculation is actually based on surface mixing ratios as the boundary condition so it seems like it is a transit time from the surface. The calculation of γ_1 is a really interesting result overall but with such small values of 1-2 months in summer especially it would seem to be very sensitive to the boundary conditions used. You should be more clear in describing what γ_1 actually represents and the uncertainties associated with the chosen boundary condition.

You are right, we have to formulate more accurate. In order to avoid misleading interpretation we rephrase p-21246, l-5ff:

'Figure 7 displays the mean transit time Γ_1 from the troposphere or more precisely from the earth surface (not from the tropopause) into the LMS derived from our mass balance approach in the same way as the associated tropospheric fraction α_1 in Figure 6.' For sure, the results are sensitive to the choice of the boundary constraints. The uncertainties associated with these boundary constraints are discussed in the section 4.2 Sensitivity study. The question is: How good does the measured time series represent the 'real natural variability' of the input region? This is the reason why we decided to use the best boundary constraints derived from measurements we could get. The construction of a tropopause time series for SF₆ and CO₂ derived from surface time series and some sparse aircraft data would introduce much more uncertainties to our study. That is the reason why we stick to the earth surface as reference level, but we should state this unambiguously as you noted correctly (see answer to your comment above). In general, the choice of the threshold 0°S to 20°N for control surface of χ_1 is motivated by the work of Berthet et al. (2007) who demonstrated that

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nearly all 30-day backward trajectories started in the LMS above the ExTL (at the 365 K level) are leaving the boundary layer ($z < 1$ km) between 0°N and 20°N . Therefore we add a comment at p-21242, l-23 (see our answer on comment of Reviewer 2 and 3).

Bottom of pg. 21247, In the description of the seasonal variability of the different quantities, the tropospheric fraction and gamma1 it would be nice to see a time series plot showing the annual variation. You could plot mean age, tropospheric fraction and gamma1 averaged over say the upper and lower LMS. That way it might be easier to see the subtle differences in the seasonal cycle that you describe.

This is a good suggestion. I played around with different types of time series plots but it turned out that they are not much more illustrative than the figures we already have. The reason is that the averaging over the upper and lower LMS is quite sensitive to the choice of parameter, i.e. potential temperature and equivalent latitude intervals. So, it would be better for this kind of diagram to use a different vertical coordinates, i.e. Delta_Theta or PV, which carry information about the location of the air parcel relative to the local tropopause. But, displaying the data in a different coordinate system would complicate the summary of the previous figures. Furthermore, the spatial information would be lost. For all of these reasons we decided that we will not show this kind of figure.

5. Conclusion

You don't mention how representative these results are for the entire LMS. Do you think different zonal regions would show similar results?

This is a rather difficult question but we should give a comment on this topic in the conclusion. First, the strongest departures from zonal symmetry are usually found in the extratropics. Second, zonal mixing on isentropic surfaces is hindered in the LMS because the tropopause intersects these surfaces, at least below 350-360 K. That means it would be possible, that the results might look locally somewhat different, e.g.

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on each side of the storm track. For sure, the data base must be extended to give an answer to this question. For these reasons we add the following statement at the end of the conclusion: 'The high resolution SF6 and CO2 measurements obtained during the SPURT campaigns allow for a comprehensive view of the LMS region above Europe. Despite the large seasonal and spatial data coverage, SPURT alone can obviously not provide a climatology and the results presented here only apply for the region and time of the SPURT observations and have to be confirmed by further investigations and measurements.'

Figures comment: It would be helpful if Figs 2,6 and 7 were made larger. It is hard to see some of the features with how small the figures are in my copy of the paper. Fig. 3 is a more appropriate size.

You are absolutely right. I designed the Figures for portrait and not for landscape format. We changed columns to rows in the Figure 2, 5 and 6.

Grammar comment: 'allows to' is used several times. Need to add an 'us' in the middle.

We revised it.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 21229, 2008.

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