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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying transport into the lowermost stratosphere using simultaneous in-situ measurements of SF_6 and CO_2" *by* H. Bönisch et al.

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This paper uses aircraft measurements of SF6 and CO2 to analyze transport time scales of air into the northern hemisphere lowermost stratosphere. The results are significant and interesting and this paper adds to the nice studies that have resulted from the SPURT campaigns. I have a few issues with the details of the method and the interpretation of some of the results, which are discussed below. I recommend publication with consideration of the following comments.

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 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.

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.

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?

3. Mean age

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

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.

4. Mass balance

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

Bottom of pg. 21242, I'm confused about the boundary condition chi1,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 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 CO_2. How does your 0-20N average compare?

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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?

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?

Bottom of pg. 21246, In the discussion of the fast transit times gamma1 you mention that "the observed tropospheric fraction in winter has entered the LMS predominantly during August and September." This is because gamma1 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 gamma1 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 gamma1 actually represents and the uncertainties associated with the chosen boundary condition.

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.

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?

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

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is a more appropriate size.

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

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