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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Strategies for measuring canonical tracer relationships in the stratosphere" *by* O. Morgenstern and J. A. Pyle

Anonymous Referee #1

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General

This is a useful and interesting paper which makes a first attempt at using a 3D model to better define measurement strategies - in this case using long-lived tracer correlations to diagnose mid-high latitude mixing in the lower stratosphere.

I think the authors are right to concentrate on presenting a methodology, rather than come to a firm recommendation about a particular platform being 'best' - as such statements can be misinterpreted and misused.

Overall I think it deserves to be published. I would suggest the following comments should be addressed first:

Specific Comments

1) General. The arguments about the necessary frequency/location of observations are based on sampling a 3D model. No doubt the SLIMCAT model they have used is 'good', but how well does it represent the details of tracer transport (mixing, filamentation etc) of the real atmosphere? One might expect the model (even at 2 x 2 resolution) to overestimate the mix-down time of filaments. The real atmosphere may be more inhomogeneous than the model. This may have important conseuqences of sampling, especially for in-situ data e.g. a single balloon flight may intercept a small filament. (Remote instruments will have the effect of averaging the atmosphere in a certain volume). I realise a full discussion of this is too much to ask for, but I do think the authors need to add a paragraph stating their views on these issues and possible limitations.

2) P 2077 line 6 (and throughout). In the paper aircraft/balloon data are used synonymously with 'in-situ' data. Of course aircraft/balloons can also be platforms for remote instruments (which often give a large number of species spectroscopically). I think the authors really want to argue for the merits of 'in-situ' data (?). If so they should make this clearer.

3) P 2080 Section 3.4. I don't fully understand the discussion of accuracy and precision related to the satellite data. The authors are ignoring the systematic errors, which is fair enough here (though as they point out such errors may also apply to aircraft/balloon instruments). However, adding random noise to the model fields seems to me like decreasing the precision and not the accuracy. With enough 'measurements' this random noise will be averaged out to give back the real model values. If this is really simulating lack of precision then the text needs to be changed and a discussion added of how the averaging of many observations (the advantange of satellites) can help in detecting the mixing with respect to the sparser platforms. On the other hand, if the authors really meant to simulate the inaccuracy then I think the section needs to be clarified to sort out my misconception.

4) P 2079 Line 6. The quoted lifetimes are, I believe, global averages. The authors point out that CH3Br is, in fact, longer lived in the stratosphere. Conversely, N2O is

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much shorter lived than 120 years in the stratosphere. Wouldn't it be better to quote the the 'lower stratospheric lifetimes'?

5) P 2085. Line 23. What do the authors mean when they say the coverage by the NDSC sites is enough? There are different types of NDSC sites - a few primary sites and many more complementary sites. Do they mean the primary sites are adequate? (I guess so because there are a huge number of complementary sites!) In any case it would be good to give the number of sites involved. Also, do they mean that observations actually obtained by these stations in 1999/2000 were adequate? I think they rather mean that there are enough stations (in the right places) but they need to make the right frequency/type of measurements in the future. This should be clarified.

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 2075, 2002.