

***Interactive comment on* “Estimating the contribution of bromoform to stratospheric bromine and its relation to dehydration in the tropical tropopause layer” by B.-M. Sinnhuber and I. Folkins**

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We thank referee #1 for the detailed review and constructive comments.

In our point-by-point response below we show the original comments of referee #1 in italics and our reply in plain text.

1) *The authors should explain why this model accurately simulates the TTL. In his previous work using this model, Folkins has avoided using it to analyze the TTL, and in discussions with him, my sense is that he didn't believe that this model would work*

there. I was therefore surprised to see this application of the model. I would like to see an explanation of why the model is believable in the TTL since it hasn't really been validated there. One possible way would be to show that it produces realistic O3 or H2O profiles, although I m sure there are other ways of doing this, too. Or perhaps it has been verified there and I missed it. In any event, please discuss why this model is believable there.

There are basically two issues with the use of the diagnostic model in the TTL: (a) the relative uncertainties in radiative heating rates are larger in the tropopause region than in the rest of the troposphere and (b) the assumption that exchange with the extratropics can be ignored is less well justified. However, as we have already stated, detrainment rates in the TTL from our model agree well with estimates from Dessler (2002). Moreover, modelled profiles of O3 and CO (and HNO3) agree well with observations, even in the TTL (Folkins et al., Testing convective parameterizations with tropical measurements of HNO3, CO, H2O, and O3: implications for the water vapor budget, submitted to J. Geophys. Res., 2006).

In the revised version of our manuscript we have included the following statement:

Although the underlying assumption of the model that there is little exchange with the extratropical atmosphere is less well justified in the tropopause region (Folkins and Martin, 2005), modelled profiles of ozone and CO agree well with observations, even in the TTL (Folkins et al., Testing convective parameterizations with tropical measurements of HNO3, CO, H2O, and O3: implications for the water vapor budget, submitted to J. Geophys. Res., 2006).

2) I find the comparisons with the data to be problematic.

a) The data in Fig. 4 come from the latitude range of 5N to 40N. But the model is really a tropical average, so only tropical data should be included. The most recent thinking is that most trop-to-overworld exchange is occurring between 10S-10N (see Bill Randel's recent papers showing high correlations between strat H2O and temperature fluctu-

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ations between 10S-10N), so data from that latitude range would be optimal. But data are rare, so I recognize a larger latitude range might be necessary

It is true that ideally one should include only measurements from the deep tropics. However, up to now tropical bromoform measurements are rare.

In the revised version of our manuscript we have now also included measurements from the Pre Aura Validation Experiment taken within 25 degrees from the equator. The Pre-AVE data are similar to the ACCENT data but show somewhat more bromoform in the upper troposphere and lower stratosphere.

b) The authors state that the STRAT bromoform data are much lower than the ACCENT/PEM-T data, and then explain this by arguing that these data are less affected by convection. The model, however, simulates the tropical average, so it seems to me that the model predictions should agree with a area-weighted average of non-convective and convective regions. If the ACCENT and PEM-T data are characteristic of a convective region, then the model should lie between the ACCENT/PEM-T data and the STRAT data. I think the authors should 1) show some STRAT data on the plot and 2) discuss exactly what average region the model is simulating and how that relates to the data.

With the newly included Pre-AVE measurements, the model which should represent tropical mean conditions lies now indeed between the Pre-AVE measurements and the much lower STRAT measurements (essentially zero). We have not included the STRAT data here because most of the measurements were below detection limit and the campaign average is close to zero. A statement is included in the revised version.

Again, we emphasize that currently there are probably not enough bromoform measurements available to construct a tropical mean climatology and consequently one should not overinterpret the comparison between model and observations. The situation will hopefully improve when measurements during the recently successfully completed SCOUT-O3 campaign become available.

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c) The authors characterize the model-data comparison as good . I take exception to that. The comparison IS good in the lower troposphere, but it appears to me to be quite poor in the upper troposphere. Since this is a paper about the upper trop, I think that the model-data comparison does not necessarily inspire confidence in their results. In addition, the authors have left the STRAT data out of the comparison, which would make the comparison even worse. Something needs to be done about this. I m not sure what to suggest about this, but I don t think it s correct as is. (One small suggestion is to avoid adjectives like good and provide quantitative calculations of RMS and average differences between the model and data.)

Actually we were surprised about this comment because almost everyone else characterized the agreement as "good". We still believe it is good but have now changed the wording to "reasonably well". With the Pre-AVE data now included (which show more bromoform in the UT than the ACCENT data) and the comment that the STRAT measurement show UT bromoform close to zero it is now more obvious that the model is somewhere between the extremes of available measurements.

d) Please explain why 0.75 pptv works better than 1 pptv in the model and what the implications are. Is that a more realistic value of boundary layer mixing ratio in convective regions? Are you assuming some entrainment in the model? etc.

We don't think that the bromoform measurements available really allow a definite conclusion at this point. The fact that 0.75 pptv works better could either mean that this corresponds better to a tropical mean value, or it could imply that entrainment of mid-tropospheric air plays a significant role. Here in our model calculations we have used the detrainment mixing ratio simply as a free parameter.

3) The model and paper can be simplified. Since this is a paper about the upper trop., and I don't think evaporative cooling is important in this region, one can eliminate that discussion from the paper, thereby shortening it. Similarly, the plots can be made to focus on the UT.

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We agree that the focus of this paper is the tropopause region and evaporative cooling is not important for our main conclusions here. However, showing the results not only for the TTL but the troposphere as well allows one to better assess the fidelity of the model approach and does in our point of view not lead to a significantly longer or more complicated paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 12939, 2005.

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