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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

Anonymous Referee #1

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Review of Sinnhuber and Folkins, Estimating the contribution of bromoform to stratospheric bromine ...

This is an interesting and well-written paper on a subject of interest to the community. I think this paper will be publishable after some major revisions.

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

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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.

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 fluctuations 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

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.

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.)

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.

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.

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

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