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Interactive Comment

Interactive comment on "The impact of tropical recirculation on polar composition" *by* S. E. Strahan et al.

S. E. Strahan et al.

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Referee 1

Thank you for the helpful review. I have adopted nearly all of your suggestions.

Section 2, on DAS met fields: a couple of sentences have been added to Section 2 to answer the questions of time averaging and assimilation type.

Section 2, on the years of the simulations. GCM and DAS simulations for the exact years of the HALOE observations do not exist. To account for the differences in CH4 mixing ratios for the different years of models and obs, I have scaled the model output by the observed global surface mean for the HALOE period (1993-2001) using NOAA/ESRL/GMD data. This is now described in Section 2.This causes small changes to CH4 throughout the stratosphere but changes none of the conclusions.

The variations CH4 annual growth rates are inconsequential to this analysis. Stratospheric air parcels have elements of many different ages so variations in growth rate will get smeared out. These variations (very small %) are quite small compared to the model-obs differences (tens of %).

Section 3.1. The statement about the agreement of the GCM vertical velocities with observational velocities from 20-26 km is not based on Figure 1 but on a more extensive comparison of the model with observational results in Schoeberl (2008), which is referenced in this sentence. But in Section 3.1, I do comment on the height range of agreement and I will change it to read 21-27 km there.

Section 3.1. Unfortunately, as you point out, the mean age profile is derived from a very small data set. This is now noted in the text.

Section 3.2. I changed the statement about the weakening of the age-ch4 correlation to read in the upper stratosphere instead of upper mesosphere.

Section 3.2, on the role of photochemistry in the tropical disagreement: The GMI-GCM is nearly 100% too high at 4 hPa where the photochemical lifetime is about 2 years. Yes, photochemistry affects CH4 mixing ratios here but loss rates would have to be grossly in error for the model/obs differences to be primarily due to chemistry rather than transport. I dont know where a more prominent place would be to put this information; this seems like the logical place to me.

Section 3.4. Thanks for catching this mistake. I do mean that diffusion causes this to be an upper limit on ascent rate and lower limit on transit time.

Section 3.4. The photochemical loss rates. My error - I should have said photochemical loss, not rates. The rates constants are of course the same in both simulations. Loss is not very temperature dependent here; primarily the loss varies with the amount of CH4 present. The GMI-DAS has less CH4 (due to greater tropical recirculation) so the total CH4 loss is lower than in the GCM. I have corrected the wording in this section.

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Section 3.4. The HALOE midlat profile was omitted by accident. Thanks for catching that. While the GMI-DAS vortex isolation is not as good as the GMI-GCM or HALOE, there is clear vortex/midlatitude separation at 40 hPa (22 km) and above. I had stated 50 hPa in the text but I have changed this to 40. In this section I am focussing on vortex isolation in the middle stratosphere and am judging that by the weak vertical gradient from 7-20 hPa. I am not discussing the lower stratospheric behavior here; you are certainly correct that the GMI-DAS shows a much high minimum between the midlatitude and vortex peaks in the pdfs.

After looking at the discussion of polar age spectra (Figure 8) that was in the conclusions, I quite agree that it makes more sense located directly after Section 3.4. It now has its own Section (3.5).

Figure 2c is removed and its relevant lines were place on Fig. 2b. Color bars have been added for Figures 4 and 7.

Referee 2

Thank you for the supportive review. I like your suggestion to include a comparison of how well these models simulate Cly in the Antarctic vortex as this strengthens the connection between age spectra and an important constituent quantity. I have added October monthly zonal mean Cly mixing ratios at 80S in each panel of Figure 8 and commented on them in the text. While the GMI-DAS has better tropical recirculation than the GMI-GCM, its leaky vortex ruins the mean age and Cly in the Antarctic lower stratosphere; thus, it appears to have worse looking polar chemistry. Antarctic temperatures are sufficiently low in both models. Having low Cly in the GMI-DAS vortex causes it to produce too little O3 loss.

How important is the GMI-DAS QBO in getting increased tropical recirculation? That is an excellent question, but I do not have an answer. The DAS met fields do not have a very good representation of the QBO. I hope to explore this in the near future in the newest version of the assimilation, GEOS5, which may have a better QBO. The

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Discussion Paper

GMI-DAS may have greater tropical recirculation simply because DAS winds tend to be noisier than GCM winds, especially in the tropics, which may lead to more diffusive transport. More diffusion across the subtropics would lead to greater recirculation (but for the wrong physical reasons).

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