Atmos. Chem. Phys. Discuss., 6, S5440–S5442, 2006
www.atmos-chem-phys-discuss.net/6/S5440/2006/


Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Stratospheric dryness" *by* J. Lelieveld et al.

**S. Fueglistaler**

stefan@atmos.washington.edu

Received and published: 16 December 2006

The interesting study of Lelieveld et al. adresses several important questions, and in some cases a more detailed description of their results would be helpful.

(1) Fueglistaler et al. [2005] and Fueglistaler and Haynes [2005] have demonstrated that stratospheric water vapour, including its seasonal and interannual variations of entry mixing ratios, can be well (i.e. to within observational uncertainties) explained by the large-scale temperature field and circulation, provided temperatures at tropopause levels are in close agreement with observations. It is thus nice to see that a GCM indeed gets realistic stratospheric water vapour concentrations. However, according to the description, the GCM is 'nudged' towards ECMWF data (I am also confused by the term 'forecast analyses' on p11257/l6), and it is thus not quite obvious what is new/different compared to the above mentioned studies. Certainly, as stated (p11257/l26) it is 'im-

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portant not to nudge the parameters ... that directly control water vapor', but in fact temperature *is* nudged, arguably the single most important parameter for dehydration. It thus would be important to see what modifications of water vapour entry mixing ratios arise due to 'non-nudged' parameters (I assume the authors were thinking of, e.g., cloud microphysics) compared to the above mentioned results. Further, the operational ECMWF analyses have a temperature bias that is not constant over time, have the authors considered this? In general, it appears crucial that the authors first show the accuracy of the model temperatures at tropopause level before they discuss water vapour (Temperatures at 70hPa, as shown in Fig. 4 are not too helpful). Figure 5 suggests important differences between model and AIRS temperatures at 100hPa, but the color plot does not allow a quantitative comparison. A difference plot (restricted to 30S-30N) would be helpful. Further, it would be interesting to learn whether the model water vapour is also dependent on strongly localized pathways as described in the papers above.

(2) Overshooting convection: As said above, a closer inspection of temperatures would be helpful. The speculation that the model dry bias arises from missing convective transport of condensate into the stratosphere is not well supported by the manuscript, and the much simpler explanation, for example that the model has a temperature bias, cannot be ruled out. The fact that the model has a severe high bias of ozone as shown in Fig.10 also at concentrations higher than 1ppmv (i.e. above approx. 20km) appears unlikely to arise from missing overshooting convection.

(3) Stratospheric drain: The authors touch on the subject of apparently diabaticallly subsiding air masses in the stratosphere, and conclude that their model shows a radiative process as proposed by Hartmann et al. [2001]. However, the paper does not show data supporting this claim. Fueglistaler and Fu [2006] recently looked into the issue and concluded that the Hartmann et al. [2001] hypothesis is probably not realized in the real atmosphere (i.e. the cases where heating in cirrus occurs far outnumber those where cooling occurs). It thus would be very interesting if the authors can sup-



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port their claim with a diagram/plot of radiative heating rates in the model that shows a dominance of radiative cooling in cirrus at tropopause levels.

Additional references:

Fueglistaler, S., P.H. Haynes, Control of interannual and longer-term variability of stratospheric water vapor, J. Geophys. Res., 110, D24108, doi:10.1029/2005JD006019, 2005.

Fueglistaler, S., Q. Fu, Impact of clouds on radiative heating rates in the tropical lower stratosphere J. Geophys. Res., 111, No. D23, D23202, doi:10.1029/2006JD007273, 2006.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11247, 2006.