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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Hydration and dehydration at the tropical tropopause" *by* C. Schiller et al.

Anonymous Referee #1

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Review of "Hydration and dehydration at the tropical tropopause" by Schiller et al.

This paper is well written and gives a strong indication that stratospheric water vapor is largely controlled by large scale temperature fields at the tropical cold point and that overshooting convection crossing the local tropopause plays only a very minor role in determining the amount of water substance crossing the tropopause. Although the amount of data is limited, i.e. three campaigns from three small regions within 1.5 years, there is little reason to believe that interannual variabilities will change this finding substantially.

For final acceptance of this paper, however, I would recommend a number of modifications that the authors should consider.

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Interactive Discussion
Discussion Paper

Although this might be a part of some of the earlier papers cited, it would be useful if the authors could provide a short discussion about the accuracy of the temperature fields that are the basis of their study. The main conclusion of their paper is that ECMWF temperatures are sufficiently accurate to describe their stratospheric water vapor measurements just above the cold point. This implies that all processes that are captured by ECMWF assimilations such as some of the equatorial waves (i.e. Kelvin waves) need not be considered separately in studying TTL dehydration, while processes that are not captured (not only convection, but gravity waves as well) only play a minor role. Since all of this hinges on the quality of ECMWF temperatures, it would be worth briefly discussing the quality of ECMWF temperatures in this context.

The trajectory based reconstruction of H2O with HALOE climatological data does not appear very useful. HALOE data are a very different data set, from a different time period (possibly wetter), with a much larger vertical averaging then any aircraft profiles or even model simulations here, and as the authors discuss possibly with some low bias. The conclusions drawn from this discussion do not add to the previous discussions and are strongly overshadowed by the lengthy discussions of the caveats that might reduce the validity of this approach. I would suggest removing this section to improve readability of the overall manuscript.

Since FISH measures total water, not just water vapor, using the term RHice is not quite accurate and at times confusing. While this does not impact their conclusions, it should be clear from the terminology that at values above 100% (and even to some extent below) ice particles may be present. A term called RHtotal could be defined as the ratio of the partial pressure of water substance (including ice) to the ice saturation pressure. This would alleviate some initial confusion when seeing RHice values reaching 300% or more.

The figures are extremely small and nearly impossible to read. The authors should consider the advanced age of some of their readers that can no longer read 3 pt print and make them larger and less dense. This is true for all figures, but especially for

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9, C6078–C6080, 2009

figures 2, 4, 5, and 6.

In their text the authors using something like “50-10%” referring to the fraction of trajectories crossing the cold point during their history. Do they mean 10% to 50%?

Do the authors have a tropospheric tracer (such as ozone), which could be shown as additional evidence for recent injection of ice particle layers high above the cold point.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 17495, 2009.