Atmos. Chem. Phys. Discuss., 8, S6333–S6335, 2008
www.atmos-chem-phys-discuss.net/8/S6333/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Classification of Northern Hemisphere stratospheric ozone and water vapor profiles by meteorological regime" *by* M. B. Follette et al.

Anonymous Referee #5

Received and published: 25 August 2008

General Comments:

This manuscript presents a new approach for compiling ozone and water vapor profile climatology using identified meteorological regimes. The main concept of the paper is based on Hudson et al., (2003 and 2006), but the analysis extends the ozone total column climatology there to profiles of ozone and water vapor. Although the concept is not new, the specific method is interesting and should be evaluated in the context of other existing approaches. The main problem is that the manuscript does not have a clear objective and lacks connection with the science questions that motivated this line of work. The main motivation for making trace gas averages according to dynam-

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ical/meteorological boundaries, instead of geographical latitudes, is to isolate the dynamical variability from the contributions of chemical processing. The manuscript has very little connection with these issues and the materials presented are a collection of what is being done without leading to a new understanding relevant to the issues. The manuscript needs a major revision. Some suggestions are given below.

Major comments:

1. Is the main goal of the manuscript demonstrating a new method or providing a new climatology? Or both? The most important point, in my view, is to demonstrate why the classification is useful for profile studies. It is not enough simply to show the similarities of the profiles within each defined regime. It will be much more satisfying if the authors demonstrate what new insights we gain with the classification that is not given by the zonal mean profiles. Without this type of discussion, presenting a new climatology has very limited value.

2. The connection with the dynamical processes that dictate the regimes is entirely missing. This is why the discussions of the mean profiles in each regime did not seem to have a clear purpose. For example, the dynamical boundaries that separate the four regimes work at different altitude ranges. Here the goal of making the classification becomes important. Depending on the intended use of the profile climatology, e. g., for the polar stratosphere or for UTLS research, not all of these boundaries are relevant to profile studies at a targeted altitude range.

3. To put this work into a proper context, the authors need to discuss the advantages and weaknesses of this method compared to other methods in use, such as the equivalent latitudes (e.g., Strahan et al, 1999, 2007), and tropopause referenced altitudes (e.g., Pan et al., 2004; Considine et al., 2008).

Specific comments:

1. Need to clean up the repetitive sentences in the abstract.

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2. The data description needs to be more focused. These are fairly well known datasets. Simple descriptions with adequate references will suffice.

3. The method from Hudson et al.,(2003) needs to be briefly summarized in the paper.

4. How is the Bethan et al., 1996 method (for deriving the ozonepause) implemented with the much coarser vertical resolution satellite data?

References:

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Pan, L. L., W. J. Randel, B. L. Gary, M. J. Mahoney, and E. J. Hintsa (2004), Definitions and sharpness of the extratropical tropopause: A trace gas perspective, J. Geophys. Res., 109, D23103, doi:10.1029/2004JD004982.

Strahan, S., M. Loewenstein, and J. Podolske (1999), Climatology and small-scale structure of lower stratospheric N2O based on in situ observations, J. Geophys. Res., 104(D2), 2195-2208.

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