Atmos. Chem. Phys. Discuss., 8, S12056–S12059, 2009
www.atmos-chem-phys-discuss.net/8/S12056/2009/

Atmospheric Chemistry and Physics Discussions

ACPD

8, S12056–S12059, 2009

Interactive Comment

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

M. B. Follette et al.

Received and published: 9 April 2009

The authors would like thank the reviewer for all of their comments and feedback. Below we have written responses to each comment. The referee’s comments are italicized and our responses are in print.

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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 dynamical/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.

This manuscript is an extension of the work done in Hudson et al. (2003, 2006). In those two papers it was shown that each regime displayed distinct tropopause heights, ozonepause heights, and total ozone. This indicated a reduction in atmospheric variability, with respect to the zonal mean, when examining profiles in the regime framework. In this paper, we wanted to explore the vertical and temporal extent of the previous results seen. The results indicate that when viewed in the context of regimes, ozone and water profiles show distinctness below 20 km in the winter and spring. This is consistent with previous papers on meteorological influence (Logan 1999; Koch et al. 2002, Newchurch et al. 2003) and the seasonal cycle of isentropic mixing across the tropopause (Chen 1995; Dunkerton 1995; Pan et al. 1997; Haynes and Shuckburgh 2000). We have added discussion related particularly to the variations in water vapor in the different regimes. The discussion highlights the advantages (and disadvantages) of this method relative to other methods.

ACPD

8, S12056–S12059, 2009

Interactive Comment

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.

In the revised manuscript, the authors have expanded the discussion of the results for both ozone and water vapor. In addition, the authors have attempted to be more specific when referencing an altitude range vs. a region of the stratosphere (e.g. LMS).

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).

Equivalent latitude is used to eliminate or decrease dynamical variability while conserving the chemical characteristics that would be lost in a zonal average due to zonally asymmetric flow. Essentially, the same concept is being applied here, only equivalent latitude uses results from meteorological models, whereas the regime method uses only column ozone measurements. Thus, the meteorological regime method has the advantage of making use of a directly measured quantity, but the disadvantage of not providing level-by-level information. Unlike PV and equivalent latitude, the meteorological regime method provides a coarse separation into three regions, but because the boundaries dividing these regions are based upon physical boundaries to mixing, they encompass relatively homogenous air masses.

Like tropopause height-referenced coordinates, the meteorological regime method removes the effects of tropopause level changes from measurements. However, because it identifies barriers to mixing, it will also help to distinguish between measurements

ACPD

8, S12056–S12059, 2009

Interactive Comment

on different sides (i.e. in different regimes) that may have similar heights above the tropopause.

The above discussion has been added to the revised manuscript.

Specific comments: 1. Need to clean up the repetitive sentences in the abstract.
These sentences have been removed in the revised version.

2. The data description needs to be more focused. These are fairly well known datasets. Simple descriptions with adequate references will suffice.
The authors have shortened and moved the TOMS data discussion, and shortened the sections on the SAGE II and HALOE datasets.

3. The method from Hudson et al.,(2003) needs to be briefly summarized in the paper.
The authors have included a brief description in Section 3 - Method.

4. How is the Bethan et al., 1996 method (for deriving the ozonepause) implemented with the much coarser vertical resolution satellite data?
In order to test how the criteria used were affected by the smoothing done in a typical retrieval algorithm, the authors took ozonesonde data from Wallops Island, VA from 1996 through 2002. Ozonepause heights calculated using the high resolution ozonesonde data were compared with those calculated after a 2 km boxcar smoothing had been applied to the data. Over the seven year period, 396 profiles were analyzed, and the mean difference (sonde height 8211; smoothed height) was $\sim$500 m.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 13375, 2008.

ACPD

8, S12056–S12059, 2009

Interactive Comment