Atmos. Chem. Phys. Discuss., 8, S12044–S12051, 2009
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Atmospheric Chemistry and Physics Discussions

ACPD

8, S12044–S12051, 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.

*The paper by Follette et al., uses satellite observations from HALOE and SAGE to investigate the stratospheric distribution of ozone and water vapour. They classify the data from seven years of measurements using the relation between total column ozone from TOMS and the location of the polar front and the subtropics and 8211; when present- the polar vortex. Based on this classification they find distinct mean profiles being characteristic for each region and state. They conclude that for trend analyses "changes within each meteorological regime and changes in the relative contribution* *of each regime" have to be considered.*

*Indeed the paper shows that mean ozone profiles of HALOE are remarkable compact when applying the aforementioned criteria, but currently not more. I miss any link to basic concepts of stratospheric or tropopause related dynamics which are important to understand the spatial and temporal distribution of ozone and H2O. Further there might be a conceptual problem, when applying the method, which is based on total column observations, to profiles, as stated below.*

*Overall the paper needs a major revision by either shorten it to discuss only Figures 1,4,5,11,12 or extend the analysis and discussion as stated below. In any case previous work and particularly the relation to atmospheric dynamics and transport in different regions of the atmosphere needs to be included in the analysis and discussion.*

*General remark: The main problem with the manuscript is, that it tries to investigates the distribution of ozone and H2O without an appropriate discussion of the underlying transport pathways. A classification and interpretation with respect to the regimes and subregions of relevance for dynamics and thus trace gas transport is not performed (the seasonal cycle of diabatic descent in the stratosphere is not even mentioned, e.g. Appenzeller et al.,1997). The interplay between dynamics and trace gas distributions is also ignored [e.g. Tuck, 1997, Strahan et al.,1999,a,b, also Rosenlof, et al., 1997, who analyzed water vapor profiles from HALOE in different latitudes]. Basic stratospheric transport issues and concepts like the surf-zone, the vortex breakup, changing permeability of the subtropical barrier [e.g. Haynes and Shuckburgh, 2000, Neu et al,2003] above =420 K are not addressed at all. Concerning the lower part of the atmosphere I also missed the definiton of the subregions which are used in the paper and which are also important for transport of chemical species in particular below 20 km (what is meant with the UTLS).*

*With regard to ozone profiles the results are not discussed in the context of sonde based climatologies, which partly did very similar analyses (e.g. Logan, 1999 and*



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*updates or Shadoz results).*

*In recent years a lot of research in the lowermost stratosphere has been performed addressing the coupling between dynamics and trace gas distributions, which is completely ignored [e.g. Fischer et al. 2000, Hoor et al., 2004,2005, Pan et al, 2004, Krebsbach et al., 2005, Berthet et al. 2007., Hegglin et al., 2007], but relevant for the results on display.*

In the revised introduction, other dynamical coordinates (e.g. PV, equivalent latitude, and tropopause based) are described and compared with the meteorological regime method. Many of the papers mentioned above have been added to the results sections, and our results are discussed in the context of the existing literature.

*Concerning the method the classification as it is done here might be sufficient as a start for a further analysis. However as indicated below (Discussion of Figs.4a, and 8) the method might be valid only in certain altitude regions of the atmosphere. Further, when analyzing profiles rather than columns I miss any relation to potential temperature surfaces, which are most important to understand transport in the stratosphere (e.g. are the often mentioned different altitudes of the ozonopauses or hygropauses in different latitudes a result of isentropic transport?)*

The authors did not expect to see distinction between regimes above 20-25 km. Below this altitude, meteorological influence on stratospheric ozone profiles has been observed (Logan 1999; Koch et al. 2002, Newchurch et al. 2003).

The different altitudes of the ozonepause are due to the abrupt decrease on the poleward side of the jet streams (Bluestein 1993). The altitude of the hygropause, however, is known to be affected by isentropic transport of water vapor either through a front or through horizontal transport from the tropical lower stratosphere (Dessler and Sherwood 2004; Rosenlof et al. 1997), where water vapor is regulated by the seasonal cycle of temperature at the tropical tropopause (Mote et al. 1996; Seidel et al. 2001). The authors have included more detailed discussions of the annual cycle of water vapor and the altitude of the hygropause in the revised manuscript.



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*It is also essential to account for the tropopause altitude when analyzing profiles in the UTLS or use tropopause based coordinates when comparing trace gas profiles in that region. This might be implicitly included in the method, but it has not been shown here. How do eg scatter plots of ozonopause altitude versus total column ozone, latitude or region as defined in the manuscript look like?*
Yes, since tropopause altitude changes across the fronts, the difference between profiles in each regime certainly is, as the reviewer suggests, in part due to differences in the tropopause altitude. A scatterplot of ozonepause altitude vs. total ozone shows the good correlation between the two, with a correlation coefficient of -0.72. When the same plot is done using latitude, the correlation coefficient is -0.64.

*Some specific points: p.13385,l.2-4: It's interesting that the five midlatitude profiles show a large variability. Doesn't it mean that the method has large difficulties or is not appropriate in midlatitudes?*
As the reviewer points out, the results do indicate that the midlatitude is noisier than the tropical or polar regimes, but, as we now show in Figure 4, they remain distinct from profiles in other regimes. This variability in the profiles could be for several reasons. Tropopause height-referenced coordinates and tracer-tracer correlations have been used to show that an extratropical tropopause layer (ExTL) exists just above the extratropical tropopause, poleward of the subtropical front (Logan 1999; Fischer et al. 2000; Hoor et al. 2002; Pan et al. 2004; Hegglin et al. 2006; Randel et al. 2007). This mixing layer would lie just above the tropopause in the midlatitude regime. Pan et al. (2004) conclude it is a result of two-way stratosphere troposphere exchange across the tropopause, and has characteristics of both tropospheric and stratospheric air (Pan et al. 2004). In addition, Randel et al. (2007) observed the frequent occurrence of double tropopauses poleward of the subtropical front. The authors have included this discussion in the revised manuscript.

*p.13386,l.1. Which definition of the UTLS is used?*
The authors meant to reference the profiles below 25 km. In the revised manuscript



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we are more specific about altitude ranges throughout the paper.

*p.13386,l10-21. I'm a bit confused. The separation into distinct regions was done to classify the different transport regimes in the stratosphere. Here it is in principle stated, that the classification does not work since one still find large latitudinal dependencies within each region and a large interannual variability. I expected the classification was introduced to account especially for that?*
While this method identifies the boundaries of regimes with similar characteristics, there certainly does unavoidably remain, within each of these very large regions, some dependence on latitude.

*p.13386,l.22 and Fig.6: Is the strong seasonal cycle which is evident in Figure 6a partly an artefact of the method? The spring maximum of ozone, as displayed in Figure 6a) is strongly affected by the minimum of tropopause height compared to summer. Note that the upper boundary is always at 20km, so different layer thicknesses in the stratosphere below 20 km are analyzed with a minimum in summer, when the tropopause altitude is highest. Thus the observed seasonal dependency in Figure 6a) mixes up dynamical, photochemical and tropopause related causes for the seasonal cycle on display.*
Yes, since Figure 6a shows ozone between 10 and 20 km, the seasonal cycle certainly does have dynamical, photochemical, and tropopause height related components. However, the spring maximum in ozone is less a result of a relative minimum in tropopause height and more a result of increased descent of high ozone (Logan 1999). Further, if the seasonal cycle at one particular altitude were analyzed, it would remain the same, having a March maximum and summer minimum.

*Why does Fig.6c) not show any regional difference particularly in March/June which is clearly evident in Figures 4ab) or 7ab) between 30-40km?*
The apparent discrepancy between these figures is a visual artifact of the plotting. The March and September column ozone in the tropical regime is actually slightly higher than in the other regimes (just as in Figures 4 and 7). The numbers agree in terms of





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percent difference.

*p.13387,l15. Both instruments also average over a long atmospheric pathway within each single measurement (200km according to the authors). Thus, there is already a kind of intrinsic averaging in each measurement. If the authors want to illustrate the good agreement between both instruments a comparison between the small number of coincident profiles (p.13387,l.18) would be more helpful than the means.*
The means were chosen to show the good agreement despite large differences in sampling, not to validate one instrument with respect to the other. There are a number of papers in the literature on the latter subject.

*p.13388,8-18. and Fig.8/4a: The discussion of the Figures.4a and 8 illustrates a general problem: The profiles at mid and high latitudes are better differentiated up to 20 km when using the proposed classification method compared to a zonal mean. One could expect this, since the method accounts implicitly for the tropopause altitude and therefore separates the respective profiles. However, at higher altitudes the opposite is evident in particular when comparing tropical and high latitude profiles around 25-40km. The normal zonal mean better seem to separate the ozone maxima by region and to remove variablity. This example illustrates that the method, which accounts for the tropopause altitude fails in the stratosphere, where other dynamical features determine the ozone distribution. Did the authors try to compare to equivalent latitude or correlations between $H_2O$ and ozone? Note further, that a mismatch in this altitude region strongly contributes to the (partial) total ozone column in Fig.6.*
Yes, we completely agree with the reviewer, although we do not understand why this is to be considered a general problem. The regime method only distinguishes profiles in the lower stratosphere. On page 13386-7 the authors stated, "Figure 6b shows the results for the 20-30 km region. This region is a transition between the dynamically controlled lower stratosphere, and the photochemically controlled upper stratosphere (Logan, 1999; Staehelin et al., 2001); therefore the regimes show no clear distinction from one another. Figure 6c and 6d are in the upper stratosphere and as such, show



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summer maximums in column amount (Logan, 1999). As expected, no distinction between regimes is seen in Fig. 6c and 6d." The end of the introduction now attempts to place this method in context with PV, equivalent latitude, and tropopause height-referenced coordinates, but we have not explicitly compared the methods. The authors have not examined correlations between ozone and water vapor. We are confused by what the reviewer is referring to when they state, "...a mismatch in this altitude region strongly contributes to the (partial) total ozone column in Fig.6".

*p.13389/13390, section 5.1: What is the message here? Neither the different hygropause heights are surprising nor the variation of H2O values with latitude. The conclusion about the tropical nature of the filament in Fig.1 could have been easily drawn from a vertical crossection or a correlation plot. Furthermore Fig. 10 a/b shows, that H2O-profiles based on the small number of profiles in Figure 10 do not exhibit significant differences by region: Hygropause altitudes and absolute values are similar in mid and high latitudes.*

There is certainly no surprising message here. Despite the fact that the water vapor data are noisier than the ozone data, the hygropause altitudes and values change in a manner consistent with expectations. A much more detailed discussion of water vapor is now included in the revised manuscript.

*p.13390: It is not entirely clear on what the focus is: The seasonal transport barrier between the tropics and the higher latitudes also varies with altitude. Are the authors talking about the UTLS below ∼380K or the whole stratosphere including the subtropical barrier separating the surf zone from the inner tropics above ∼420K? If the barrier at the subtropical jet is meant, how and where do the authors expect water vapour to be affected?*

The authors were referencing the barrier at the subtropical jet (the UTLS below ∼400K), but realize that the passage was confusing. A detailed discussion of the water vapor profiles in addition to a figure showing the annual cycle in water vapor within each regime at 15 km and 18km has been added to the revised manuscript.



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