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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 #2

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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 - 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 H_2O. 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 H_2O 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 Θ=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 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

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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.

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?)

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?

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?

p.13386,l.1. Which definition of the UTLS is used?

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 regon and a large interannual variability. I expected the classification was introduced to account especially for that?

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.

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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.
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?
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 helpfull than the means.
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.
p.13389/13390, section 5.1: What is the message here? Neither the different hygropause heights are surprising nor the variation of H_2O values with latitude. The conclusion about the tropical nature of the filament in Fig.1 could have been easily

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drawn from a vertical crossection or a correlation plot.
Furthermore Fig. 10 a/b shows, that H_2O-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.
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 Θ=380K or the whole stratosphere including the subtropical barrier separating the surf zone from the inner tropics above Θ=420K? If the barrier at the subtropical jet is meant, how and where do the authors expect water vapour to be affected?

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