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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.

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

Major comments: There is a circularity problem here: they are using ozone to define the regimes, then they show - surprise! - that the UT/LS ozone separates into regimes. Is it possible for it to have turned out any other way?

As the reviewer points out, there is indeed no surprise here. The correlation between total ozone and tropopause height is well documented (Petzoldt et al. 1994; Hoinka et al. 1996; Schubert and Munteanu 1998; Steinbrecht et al. 1998) and the results for the

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UTLS ozone were expected based on previous work using ozonesondes (Hudson et al. 2003). The results seen in this paper are an investigation on how this distinction varied with time (one day versus climatology), season, and altitude. In addition, we wanted to see how and if this behavior extended to another important chemical species (water vapor).

As explained in section 1 of this paper, the subtropical and polar frontal boundaries were calculated utilizing gradients in the total ozone field. This was done based upon the fact that ozone in the lower stratosphere both dominates the amount in the total column, and is a dynamical tracer on synoptic timescales (Danielsen 1968; Shapiro et al. 1982; Salby and Callaghan 1993; Wohltmann et al. 2005). The tropopause "breaks" across the upper troposphere fronts are therefore reflected in the total ozone data, making it an excellent diagnostic for the position of the subtropical and polar fronts. In fact, this method was able to track a net northward movement of the subtropical front that has now been observed in various datasets (Hudson et al. 2006; Fu et al. 2006; Wu and Fu 2007; Seidel and Randel 2007; Archer and Caldeira 2008). Therefore, while the method which we use creates frontal boundaries which are inherently based upon total ozone, this does not alter the fact that they correspond to real, physical features of the atmosphere.

The fundamental motivation of the paper is stated in the introduction: "The motivation is to determine how well, and over what altitude ranges and seasons, stratospheric ozone and water vapor profiles can be usefully differentiated by meteorological regime." Despite the incredible length of the paper, it's not clear that they have actually provided an answer to this. While they provide lots of plots, the results are qualitative, with statements like "In the lower stratosphere, the climatological profiles show distinct ozonepause heights within each regime for every month, except for the tropical and midlatitude regimes in September. ... Above approximately 25 km, however, profiles are in most cases not well differentiated by regime." We are supposed to look at the plots and see the evident truth of these statements. However, the plots are inconclusive

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to me, and thus I don't think they have actually proved their claims. There are, however, quantitative ways to evaluate this. See, e.g., Sparling LC, *Statistical perspectives on stratospheric transport*, *Rev. Geophys.*, 38, p. 417-436, 2000, equation 2. The authors need to do something to shore up their conclusions.

Using Eq. 2 from Sparling (2000), the authors have calculated delta vs. altitude for the climatological profiles shown in Figures 4. In general, higher deltas ($\delta > 0.75$) are found from 10-20 km from November through March. Values of $\delta > 0.75$ are only sparsely observed above 22 km. The 10-20 km altitude range is where one would expect to find a large meteorological influence on stratospheric ozone profiles (Logan 1999; Koch et al. 2002, Newchurch et al. 2003. This also agrees well with Haynes and Shuckburgh (2000) who observed a mixing barrier associated with the subtropical front from 8-15 km (330-390 K), with the minimum in Keff in winter. Figures showing delta vs. altitude, and accompanying discussions, are included in the revised manuscript.

Unlike ozone, whose mixing ratio monotonically increases with altitude in the lower stratosphere, the water vapor mixing ratio has a minimum in the lowermost stratosphere. The Sparling (2000) delta vs. altitude method based on mixing ratios does therefore not make it possible to distinguish between mixing ratios above and below the hygropause. As an alternative, deltas for the hygropause heights, seen in Figure 12 of the original manuscript, were calculated. However, the results were found to be very bin size dependent. They are therefore not included in the revised manuscript.

Finally, the idea that latitude is not the best coordinate is well known. People have been using things like PV and equivalent latitude for years (see the Sparling paper or do a web of science search on "equivalent latitude"; also see Noboru Nakamura's publications on his modified Lagrangian-mean diagnostics). They need to put their work into context with these previous analyses. Is this method better than PV? What's new here? It seems to me that the ideas in this paper have been described before, and more convincingly.

The authors did not intend to imply that latitude was the only other coordinate available.

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Knowing that the distribution of ozone becomes more zonally symmetric with increasing altitude, latitude coordinates were chosen to highlight the transition between the meteorologically controlled lower stratosphere, and the more photochemically dominated upper stratosphere. The extent of the meteorological influence on the ozone profile has been documented (Logan 1999, Staehelin 2001), and our results support these hypotheses.

As the reviewer points out, many other analyses have used PV, equivalent latitude, or tracer equivalent latitude in order 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, except that, whereas most methods use results from meteorological models, the regime method uses only column ozone measurements. Thus, the meteorological regime method has the advantage of being 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.

The above discussion has been added to the revised manuscript.

More minor comments: This paper is incredibly long (37 pages), considering the material presented. It reads like a thesis, as perhaps it is. This paper could easily be cut down by at least a factor of 2 with no loss of information (e.g., shorter instrument discussions, remove Fig. 7 and associated discussion, etc.)

The authors have changed the manuscript considerably. While it is not shorter than before, we hope to have added significant information and to have clarified points raised by all of the reviewers.

Abstract is repetitive.

This was a clerical error on the author's part. It has been removed from the revised

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abstract.

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