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Interactive Comment

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

Anonymous Referee #3

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Review of Classification of Northern Hemisphere stratospheric ozone and water vapor profiles by meteorological regime by M. B. Follette, R. D. Hudson, and G. E. Nedoluha

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?

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 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.

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 has been described before, and more convincingly.

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.)

Abstract is repetitive.

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