Atmos. Chem. Phys. Discuss., 15, C8115–C8116, 2015
www.atmos-chem-phys-discuss.net/15/C8115/2015/


Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Upper tropospheric water vapour variability at high latitudes – Part 1: Influence of the annular modes" *by* C. E. Sioris et al.

**Anonymous Referee #2**

Received and published: 16 October 2015

Sioris et al investigate two upper tropospheric WV data sets derived from satellite sensors to investigate co-variability of WV and annular modes in the high latitudes. The subject is of importance and the chosen data also makes sense since the focus on the UTLS region.

I, however, had a hard time following their analysis, explanations and arguments presented in the manuscript. I believe the primary reason for it is that at many places authors are over-analyzing their results, so as a reader I often had to extrapolate their reasoning in mind (which is not easy based on limited information provided here as you may interpret that information differently).



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I do understand where they are going with the proposed mechanisms, but I am not convinced yet that they could draw such conclusions just based on these results. Take Fig. 11 as an example. I don't understand how can authors conclude the relative importance of first and second mechanism based on these correlations alone. This is a typical overanalysis of the results. And I don't understand what do they mean by "meridional swinging of vertical gradients near a tropopause" either.

Section 3 is fine though (still at places difficult to follow).

In Fig. 10, AO response is analyzed for only JFM months. Why is so when AO can be active during the entire winter half year?

I do however like the ideas authors have presented and are discussed here, and they should be published, but definitely not in the current form. Please simplify and substantiate those ideas more robustly.

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 22291, 2015.