

Response to referee 2

We feel fortunate to have had referee 2 review our manuscript. Firstly, we agree with this reviewer that the subject is important. But more importantly, referee 2, like referee 1, have identified where the presentation could be drastically improved. Referee 2 shows great insight with their comment on “overanalyzing”. With the response of saturation vapour pressure to the annular modes now calculated including associated standard errors, we now see the need to refrain from drawing some of the conclusions in Sect. 4.2, including the relative importance of the mechanisms.

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).

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.

The discussion relating to ozone correlations to the annular mode was entirely deleted since we agree with the reviewer that we were guilty of “over-analyzing” for that atmospheric parameter and because the ozone correlations are not necessary in Sect. 4.2. We have deleted Figs. 11 and 12 and added the response of the saturation VMR anomalies to the annular modes (and the associated standard errors) to Figs 8-9 to more easily interpret the results. Most of Sect. 4.2 has been rewritten: the ACPD version had some conclusions stated before all of the arguments were presented subsequently. This is clearly backwards. The new paragraphs in this sub-section are in the response to reviewer 1 and not repasted here.

And I don't understand what do they mean by "meridional swinging of vertical gradients near a tropopause" either.

This sentence has been deleted.

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

Based on more specific comments about Sect. 3 by reviewer 1, we have eliminated less pertinent information from Sect. 3.1 (see response to reviewer 1). The reader was sent in many directions in the ACPD version and we hope we have changed this sub-section sufficiently so that it is now easy 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?

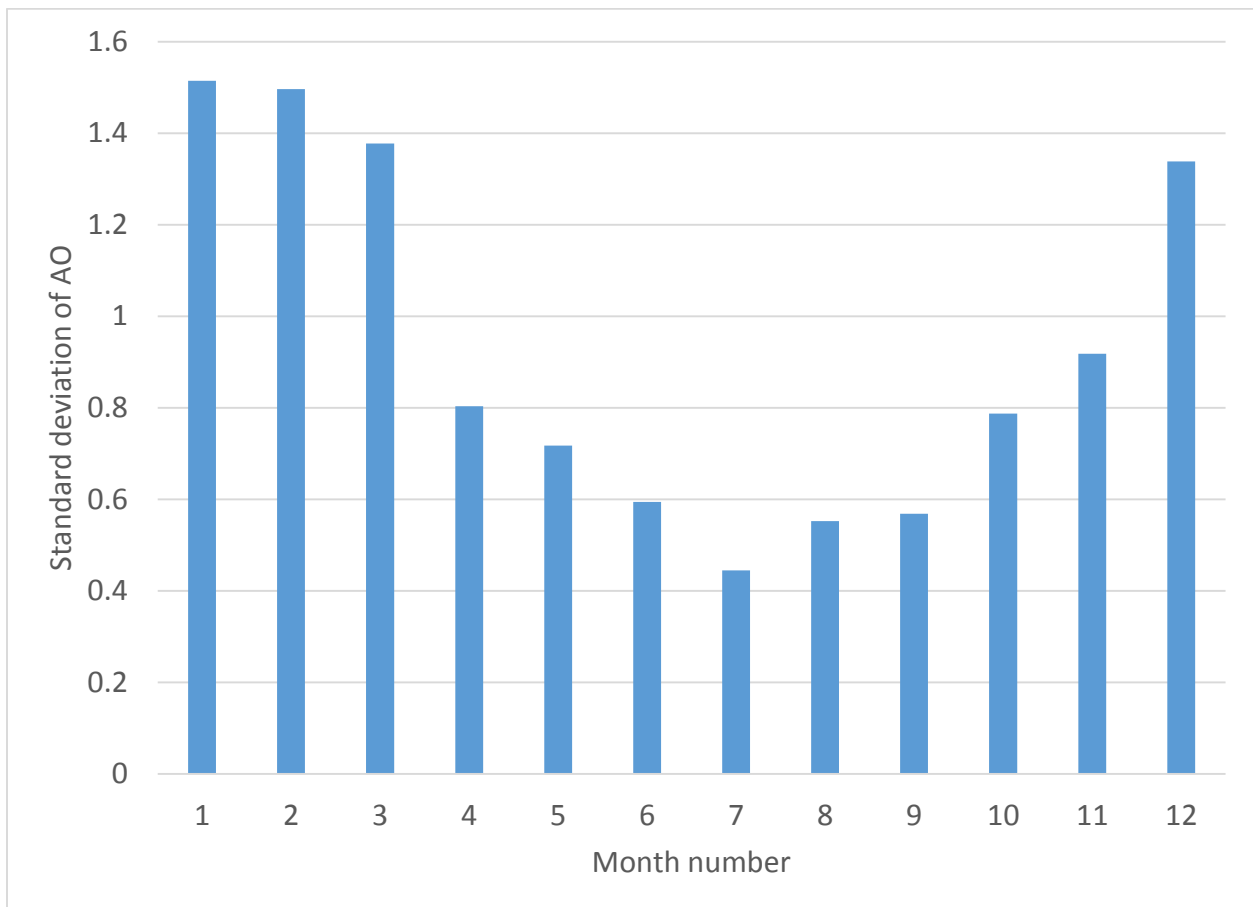
We have examined the month-to-month dependence of the AO activity, by calculating the standard deviation for each calendar month using the AO index from 1950-2015. The AO is

active during the entire year as shown in the plot below, not just the winter half (or ‘cold season’). In Fig. 9, we use all available months. However, for Fig. 10, we wanted to focus on the months with the strongest activity while still maintaining a significant sample of months (e.g. $N \geq 20$). Thus we chose January to March and the figure below confirms that we have selected the three most active months.

In one of the most relevant papers we cited (Li et al., 2014), the cold season is defined as the six month period from October to March. Our plot below suggests that April should have been preferred over October if the historically, most active months were desired. Another key paper we cited (Devasthale et al., 2012) used a five month ‘winter’ from November to March. This seems more justifiable. We followed the lead of Thompson and Wallace (2000) and used January to March to show how dominant the Arctic oscillation can be in terms of explaining the variability of deseasonalized upper tropospheric water vapour at northern high latitudes for the three months that comprise most of the winter.

We now write:

“The most active season for the AO is from January to March based on monthly standard deviations of the AO index in the period from 1950 to 2015. This three month period was used by Thompson and Wallace (2000).”



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

There are many simplifications to Sect. 2 and Sect. 3.1 as suggested by reviewer 1. Sect. 4.2 has also been drastically simplified (see above). The ideas which could not be substantiated statistically have been removed.

Again we thank the reviewers for suggestions which should improve the readability and validity of the final version.