Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-882-RC1, 2017

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Detection of a climatological short break in the Polar Night Jet in early winter and its relation to cooling over Siberia" *by* Yuta Ando et al.

Anonymous Referee #1

Received and published: 4 December 2017

Summary:

Using JRA-55 reanalysis data, the authors describe an approximately 15-day hiatus (which they term a "short break") in the seasonal strengthening of the Arctic stratospheric polar vortex during late November. They go on to attribute this hiatus to an increase of the planetary wave flux into the stratosphere at this time, itself resulting from cooling over Siberia and an increase in land-sea temperature contrast.

The paper is well written, logically structured, and the figures are clear. I think that such an analysis of the seasonal evolution of the stratospheric polar vortex could be of interest for the community, and if this "short break" is a robust feature it could be a

useful test of the seasonal evolution of the polar vortex in climate models. However, I have two major concerns, the first being whether this feature is indeed robust, and the second regarding the calculation of anomalies of the seasonal evolution within the paper. I hope that the authors find my comments below to be constructive.

Major comments:

1. The authors state that this "short break" feature is statistically significant at the 99% level using a two-tailed t test (L10-11, P3). I think more information is needed as to exactly how this statistical test was carried out for this to be convincing. Is it testing whether the late November trend is statistically consistent with zero? Or distinct from the trends before or after this period? (It might be a good idea to include both of these tests). Overall it is important to have an idea of whether this feature would persist if we were to have many more years of data? From Fig. 1b it is clear that there is large variability in the strength of the polar vortex in November, and that the short break does not occur every year, hence a reader may be sceptical as to whether this is indeed a robust feature.

I suggest trying a bootstrap test as follows: resample with replacement from the 38 available years (giving say 1000 different 38-year composites). Within these composites then how often is there a zero (or near-zero) trend in late November? Is it more than 95% of the time?

2. The authors define a deviation from 'expected' seasonal evolution as that from a linear trend. However, I would expect that the zeroth order expectation of a seasonal evolution would be sinusoidal (since the seasonal evolution of solar forcing is sinusoidal). Because of this, there is potential that the use of a linear trend is somewhat over-estimating anomalies. I encourage the authors to either calculate anomalies from a sinusoidal evolution (or at least demonstrate that this is not significantly different from a linear evolution).

Minor comments:

1. Fig 1a shows the maximum in zonal-mean zonal wind at 50hPa to occur at about 60N, but throughout the paper the “PNJ index” is taken to be at 65N (and the PNJ is described as being at 65N in the abstract). I think some motivation behind this choice should be included in the paper, or the PNJ index taken to be at 60N.

2. L22-23, P1 “The signal further propagates into the troposphere to produce the Arctic Oscillation. . .”. This makes it sound as though the AO is entirely produced by stratospheric variability. In fact, the AO would exist in the absence of a stratosphere. I suggest replacing with something like “PNJ variability can influence the AO”.

3. Section 2 is very short. I think appendix A could be included with this section since it is often referred to in the following analysis.

4. L12, P3 “We note the signal throughout the whole stratosphere”. What exactly is meant by ‘the signal’? Is there a decrease in the rate of strengthening of the vortex over the whole stratosphere?

5. The authors state that the momentum budget approximately closes (“A=B+C” L3, P4). This could easily be shown by adding a “B+C” line in Fig. 2.

6. L3-5 This speculation about changes in the frequency of SSW events and the relation to sea ice changes seems quite disconnected with the rest of the paper (i.e. it doesn’t relate to the results of this study). I suggest removing this sentence.

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