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Interactive comment

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.

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Interactive comment: Anonymous Referee #2

The authors present a concise discussion of an apparent feature in the climatological early-winter development of the Arctic stratospheric polar vortex during which the seasonal acceleration of the zonal mean zonal wind is slowed for several weeks in mid-November. This slow down is associated with enhanced upward wave fluxes at 100 hPa that are in turn argued to be connected to a climatological enhancement of a tropospheric trough over Siberia. The paper is generally well written and the arguments are for the most part clearly made.

Printer-friendly version

Discussion paper

Reply:

Thank you very much for your comments, which were extremely helpful for our revision. We have considered your comments carefully, and have been making changes accordingly.

I have several more general comments:

It is not immediately clear to me that this feature is in fact 'climatological' in the sense of being common in some sense to all years, or whether it is a result of early warmings (not necessarily major ones) that have happened to cluster in late November such that consideration of a longer record would reveal a smoother evolution. There is some text arguing that the feature is statistically significant but not enough details are given to evaluate this claim (e.g what precisely is the random variable being tested, and what is the null hypothesis). This would seem to be a pretty central issue for this paper to clarify given that the text mostly argues that this is a climatological feature. If it really is a feature of the climatology, models should recover it and this could (and should) be explored. However, appendix C seems to walk back on this claim suggesting that the feature could be a result of early warmings which is a bit confusing.

Reply:

Many papers studying on the extreme weathers or extreme events usually show anomaly fields from climatological mean. This paper does not show anomaly fields from the climatology, but those of the long-year mean seasonal march as climate. Readers may consider that this paper is anomaly fields from climatological mean. To avoid misleading, we repeatedly use the term of 'climatology' without explicitly showing our definition of the climatology. We defined climatological values as this 38-year average values. We will explicitly write the definition in the revised version. The warmings do not always occur in late November in each year. In some years the short break occurs in the middle of November, and in some years the short break occurs in early December. But on average – this is the climatology in our definition – the short break is largest in late November. In the revised version, we will carefully use the term of climatology.

A second issue is that I would like to see much more discussion of the literature. Both of the phenomenology of early winter warmings, sometimes called 'Canadian' warmings. See papers by Gloria Manney and Karen Labitzke, for instance, which are in fact referenced but only at the end of Appendix C – these should be part of the introduction! But also of some work with mechanistic models – see the fourth point below; Taguchi and Yoden 2002 Fig. 7 also seems quite relevant.

Reply:

We will cite these papers in the revised version, and we will move Appendix C to the main text.

This brings me to a third point which is that the figures and discussion in the appendix should be largely incorporated into the main text as they are central to the main argument.

Reply:

We will move appendix A into Section 2 in the revised version.

A fourth and final point is that it's not so obvious to me that the explanation for this 'short break' is in fact due to some feature of the tropospheric circulation. I'm not super convinced by the analysis connecting the 100 hPa wave activity flux to the Siberian trough (see specific comments given below)–in fact this kind of early-winter feature is not uncommon to see in mechanistic models (for instance see Fig. 1 of Gray et al. 2003) that have highly simplified tropospheric evolutions. Even in the figures presented in the present manuscript, the tropospheric flow features in late November are pretty subtle features – why should the heating associated with land-sea contrasts exhibit a climatological feature with a timescale of a few weeks? On the other hand the seasonal transition from easterly to westerly winds in the stratosphere is a highly nonlinear transformation in terms of the ability for waves to propagate into the stratosphere (see Plumb 1989 for a very relevant discussion of this point). It seems to me that an alternative reason for this climatological feature is that the onset of winter-time westerlies permits wave activity that is always present in the troposphere to propagation upwards - this propagation time (along with the timescale for the response) could be an explanation for the 2 week timescale of the feature. The first three of the points given above need to be substantially addressed in order for this work to be publishable. I would further encourage the authors to consider and discuss the possible relevance of the final point.

Reply:

We compared the difference between the zonal-mean zonal wind, EP flux, and the flux divergence with the year of easterly phase of the QBO (QBO-E) and westerly phase of that (QBO-W). The short break during QBO-E is clearer than during QBO-W. However, the difference is not statistically significant. We will add this result and figures in the revised version.

Specific Comments:

It would help to provide a clear definition of what ‘climatological’ means – physically it might be clearer to define the reference evolution as ‘radiative’ (see discussion in chapter 7 of Andrews Holton and Leovy 1987). I would think a sinusoidal reference state would be more appropriate than a linear one.

Reply:

Following to your suggestion, we executed regression analyses with sinusoidal reference state. The deviation is therefore defined as actual meteorological fields in late November from those of the expected sinusoidal seasonal evolution during the same period. The results were almost the same as its linear seasonal evolution. We will change corresponding figures in the revised version.

p 1. l 10: This is a pretty sweeping statement - please justify with multiple specific citations or delete. (The comment applies also to p2 l 5)

Reply:

We will add multiple citations in the revised version.

p 3. l 10. This statement needs to be much more clearly justified. need to justify statistical significance of this 'blip' - include pre-satellite period; radiosondes alone do a pretty good job of constraining the zonal mean state. How many winters is this break apparent in? interannual variability still looks pretty broad on the basis of Fig. 1b.

Reply:

We used t-test for the differences of two means. The difference of early and late November is not statistically significant (t=0.28), however, that of late November and early December is significant at 95% level (t=2.11). We further investigated an additional analysis, that is the difference of late November and the expected sinusoidal seasonal evolution of the same period. We will add this additional results in the revised version.

p. 3 l 15: These bumps are associated with stratospheric sudden warmings - while I appreciate the context, but calling them 'extreme short breaks' comes across as a bit unaware of the existing literature.

Reply:

We will change "extreme short breaks" to "short breaks (SSWs)" in the revised version.

Fig. 4, p 17 l15: The problem with the use of 100 hPa wave activity flux measures as indications of the wave source regions is that the wave activity at 100 hPa is only very weakly correlated with anomalies within the troposphere (see de la Camara et al. 2017, Fig. 15), especially on sub-monthly timescales. A big reason for this is that the long waves that propagate into the stratosphere are dwarfed by wave activity variability associated with waves that are trapped with the troposphere. This seems pretty consistent with Figs. A1d and A3d which show downward anomalies almost uniformly over the highlighted Siberian region in the troposphere below the upward wave flux anomaly at 100 hPa. It could be helpful to show the wave 1 and 2 wave activity fluxes down to the surface.

Reply:

We investigated the vertical component of the wave activity fluxes (figure 4) with wave numbers 1 and 2. We will add these figures in the revised version.

References:

R. A. Plumb, (1989) On the seasonal cycle of stratospheric planetary waves, PAGEOPH, 130, pp. 233-242.

L. J. Gray et al. (2003) Flow regimes in the winter stratosphere of the northern hemisphere, Q. J. R. Meteorol. Soc. 129, pp. 925–945.

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A. de la Camara et al. (2017) Sensitivity of Sudden Stratospheric Warmings to Previous Stratospheric Conditions J. Atmos. Sci., 74, pp. 2857-2877.

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