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## **Comment on acp-2020-1237**

Anonymous Referee #3

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Referee comment on "Does the coupling of the semiannual oscillation with the quasi-biennial oscillation provide predictability of Antarctic sudden stratospheric warmings?" by Viktoria J. Nordström and Annika Seppälä, Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1237-RC2>, 2021

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Review of "Does the coupling of the mesospheric semiannual oscillation with the quasi-biennial oscillation provide predictability of Antarctic sudden stratospheric warmings?" by Nordström and Seppälä

The goal of this study is to show that zonal winds in the tropical stratosphere affected the development of the Southern Hemisphere minor sudden stratospheric warming (SSW) in 2019. The results are emphasized by comparisons of this winter with dynamical developments during two other disturbed SH winters and during quiet winters. This is an interesting topic and is timely since it investigates developments during the recent 2019 winter, which showed an enhanced level of disturbance that is rarely seen in the SH.

Overall, the paper lays out the authors' argument in a straightforward manner. However, the language is often lacking in precision so some steps along the way are over-simplified or not carefully described. The net effect comes across as a very qualitative description in which complexities are ignored or glossed over; at times the discussion is not well-grounded in dynamical theory. This could be remedied with an extensive revision focused on more careful description of the links between cause and effect, better definition of terms, and overall effort to avoid generalizations that do not hold up. It would also be very helpful if the figures were revised to better illustrate the points being made in the text. The comments below elaborate on these concerns. The aspects that are of most concern to this reviewer are those given in major comments 1, 4, and 6.

## Major comments

- The paper is focused on the way that variations in tropical winds might contribute to the deceleration of the zonal winds in midlatitudes. The potential impact of the disturbed dynamics on the tropical wind is not discussed. The accepted understanding is that the stratopause SAO easterly winds in solstice periods develop in response to the global Brewer-Dobson circulation that is driven by planetary wave dissipation in the winter hemisphere. In other words, the anomalous EP flux divergence in particular southern winters could contribute to stronger SAO easterlies during those winters. That is not to say that the tropical winds cannot also affect the midlatitude dynamical disturbance but it is not appropriate to treat it as a one-way influence. The direction of influence is likely to be two-way; cause and effect cannot be easily separated.
- Section 2.1: The paper by Kawatani et al (2020), which was cited in the manuscript, shows that the equatorial winds in the upper stratosphere in MERRA-2 have significant deficiencies. The winds in the lower mesosphere have even poorer agreement with observations. Some mention of these discrepancies is necessary.
- It is often hard to see the features being discussed in the latitude x pressure figures (3, 4, 6, etc.). Can you make them wider in the latitude dimension by, for example, stacking two over two and/or cutting off the northern latitudes?
- Besides being difficult to discern in the plots, the text describing the easterly winds is difficult to follow. One problem is labeling all easterly winds to be part of the QBO or SAO. For example (l. 218) "eQBO comes to lay between 50-2 hPa extending to about 40°S". The QBO is an oscillation in the tropics; the term is also sometimes applied to periodic signals elsewhere that could be affected by the tropical oscillation. What evidence do you have that this individual instances of easterly winds in mid-latitudes is part of the QBO rather than a response to other dynamical activity? It is good to keep in mind that the QBO and SAO are oscillations that are defined by a timeseries of winds at a given location. Take care when referring to winds with the same sign seen on a single occasion elsewhere in the atmosphere.
- Figure 4 and discussion: The EP flux and its impact on the zonal wind are derived using zonal means and waves defined as perturbations from that mean. They cannot be applied to an arbitrary longitude sector. To look at the regional wave-mean flow process, you should apply analysis tools specifically developed for this purpose, such as the formulation given by Plumb (1985). I suggest to remove this figure and delete the discussion.
- Please define what you mean by "coupling of the SAO and QBO wind signals" used in reference to Figure 5. Do you mean to indicate that some dynamical interaction is "coupling" these winds? If so, please describe what it is. The terms "interact" and "interaction" are also used in the discussion of several other figures and in the discussion section but the nature of this interaction and the evidence that it is occurring are never presented.
- The speculation in the paragraph beginning at line 316 has several problems. First, it ignores the role that dynamics in the winter hemisphere have in driving the SAO and affecting its variability. Second, to support the speculation about the contrast with the NH, it is necessary to show or cite evidence that the development of the SAO is

different there. This is not done.

Minor comments:

- The reason for analyzing the results from quiet years in streams is not clear. Do we learn anything from this that would not be equally apparent from treating all the quiet winters together? If so, please describe what it is that we learn and what about the different streams accounts for the differences. If not, it would be useful to eliminate this as an unnecessary complication.
- (l. 36-37) "a reversal of the meridional temperature gradient, creating an easterly zonal wind" The meridional temperature gradient and vertical wind gradients are consistent. Be careful about saying that one is causing the other unless you have evidence that only one is being forced.
- (l. 46-47) "These clouds are where ozone depleting reactions occur (Solomon, 1999)" Not really. Reactions within clouds produce reactive species that then destroy ozone in the presence of sunlight.
- (l. 64-67) "The North Pole is ringed by mountain ranges, perfect for producing atmospheric waves (Duck et al., 2001). An enhancement of wave activity over winter causes disruption to the vortex, as the waves deposit their momentum at higher altitudes (Brasseur, 2005). However, Antarctica is enclosed by flat oceans, which don't excite waves as effectively as mountains (Holton, 2012)." It is not necessarily the topography near the pole that is associated with planetary wave generation. It would be better just to say that the NH has higher winter planetary wave activity and variability than the SH.
- (l. 86-87) "Westerlies maximise close to the equinoxes, whilst the easterlies maximise near the solstices (Brasseur, 2005)" Please be clear that this description applies for the SAO near the stratopause, not above.
- (l. 138-139) "if the 10 hPa equatorial flow is easterly during the austral winter months of June/July" Do you mean if the monthly mean values are easterly for these two months or if the values are easterly for every day?
- (l. 156-157, l. 181, and elsewhere) "As planetary scale waves can only propagate where the zonal flow is westerly (eastward)" This should refer specifically to stationary waves. To be precise, insert "stationary" and also verify that the wave or waves that are driving the SSW under investigation are themselves quasi-stationary.
- 1 and eq. 2 are the quasi-geostrophic version of EP flux. The full values derived from the primitive equations would be preferable.
- (l. 189) "the zero wind line formed by the QBO and SAO subsides" What do you mean by subsides?

*reference:*

Plumb, RA (1985) On the three-dimensional propagation of stationary waves, J. Atmos. Sci., v 42, p 217-229.