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Comment on acp-2020-1237

Anonymous Referee #4

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-RC1>, 2021

General comments

The authors propose a mechanism involving the interaction of the mesospheric semiannual oscillation (SAO) with the quasi-biennial oscillation (QBO) to explain the occurrence of SH weak vortex events. Overall I thought the authors' mechanism certainly warrants consideration and they provide some thoughtful, though qualitative, evidence for the importance of this interaction. However, my main criticism is that there was little quantitative or statistical evidence provided to show that these processes are linked. Additionally, the authors make some comments (e.g. line 10-11, 339) that argue the SAO/QBO configuration is the primary ingredient for these somewhat rare SH events to occur, without really discussing the fact that tropospheric wave driving was extraordinarily large, for example, in August 2019 (Rao et al. 2020). For these reasons and the other major comments below, I suggest a major revision.

Major Comments:

- A general recommendation for the authors is to be careful or more exacting with their wording regarding this event- for example about calling it "the second SSW" (line 3, line 27-28, line 325). This event was not a major SSW (by technical criteria) like 2002, and if you mean to refer to minor events, there have been other minor SH events before too as mentioned on line 57 (though none as large as this one in terms of temperature). Maybe instead for line 3, you could state something like "while this was only the second time stratospheric temperatures have risen this rapidly in the Southern Hemisphere (SH) winter/spring". Also the number of 6 events per decade in the NH (or one almost every other year) is mentioned a few times but again that's referring to events that meet the major SSW criteria so it's not really comparable. The authors do try to clarify this point on line 55 but it would improve the text to be careful with wording throughout (or move the statement on line 55 to earlier to try to be clear from

the beginning). I would disagree with the statement on line 61 as well, as 2019 saw no wind reversal at 60S and 10 mb; additionally, there is evidence to suggest that dynamics were quite different between the 2002 and 2019 warmings, so I'd rephrase.

- There needs to be some more analysis to connect the changes in the critical line/wave propagation in the subtropics with the momentum changes happening in the polar regions. The authors have a couple nice figures (Figure 3,4, and 6) qualitatively showing a potential connection. But what's not clear is if the momentum changes over the polar region are just happening simultaneously as the changes in the critical line, or if these things are truly related. Perhaps there is some way to examine the correlation between the momentum flux convergence near 60S/1 hPa with some measure of the critical line at 1 hPa (latitudinal location for example), for all years in the record during July, and see if 2019 and 2002 correspond to exceptional conditions on that scatterplot. This might also provide better quantification for how other years are different (e.g. Figure 9). Similarly, in section 3.3.2 there is a lot of qualitative discussion of the SAO-QBO interaction (e.g. lines 243, 250) but it would greatly improve the arguments if this relationship and its link to the polar vortex could be better quantified. For example, is the latitude of the SAO-QBO interaction the most important feature? The vertical extent? The magnitude of the SAO/QBO winds? These factors are brought up on lines 260-261 and 340 but can these be quantified in a more robust statistical manner?
- It wasn't clear to me why it was necessary to break the averaging into MERRA-2 streams. Is there justification for doing this? Maybe there are large jumps introduced (e.g., Long et al. 2017), but I wouldn't think it would have such a substantial effect on weekly averages. This made for a lot of unnecessary figures in my opinion, and it is not clear that a lot of the differences between, e.g. Figures 9, 10, and 11, aren't just due to sampling (only a few cases in each composite)- thus this was not particularly meaningful for me to be able to draw any conclusions. Why not just composite all the non-weak vortex years together? Or just pick a couple of years of interest and just show those individual years? (the averaging could be introducing differences just from differences in the timing of SAO-QBO interaction between years, for example). Similarly, the statements on lines 257-260 and lines 281-285 are implying some decadal differences based on arbitrary averaging over streams, but I'm not sure these statements are justified (what would be a physical explanation for why these relationships would be different over time?).

- The authors make an interesting case for the role of mesosphere-stratosphere interactions, but I think they ignore too much the role of tropospheric forcing and/or internal stratospheric processes. For example I think Figure 11 shows cases in which the SAO-QBO interaction is very similar to 2019 and 2002 yet no SSWs happened- why? Particularly during 2019 when tropospheric wave forcing was at record high levels, I think this is a mistake to not mention this or explore the combined effect of these two features- perhaps the mesospheric wind interaction is a necessary but not sufficient condition for a SSW to occur (or likewise for the tropospheric wave driving; perhaps you need both factors). For this reason I think statements such as 338-339 are overstated, and should be reworded unless stronger evidence is provided that this interaction can actually “trigger” the event.

Specific comments

Line 2, also line 30: specify the level/latitudes that this wind reversal and temperature rise occurred at (or at least something general like “polar cap” and “mid-stratosphere”).

Line 4, line 62: This is one important aspect that is thought to drive SSWs but one difficulty is that the only known SSW in the SH in 2002 was likely not due to tropospheric wave driving but resonant amplification internal to the stratosphere (see Albers and Birner 2014, Esler papers). A number of NH events are also thought to be caused by this, with the link to tropospheric wave forcing not always prevalent (de la Camara et al. 2019). So either also mention this mechanism or caveat these statements so that it’s clear this is not the only possible mechanism.

Line 33-35: This is true but the other key dynamical difference between a final warming and a sudden warming is that for the SSW the vortex recovers back to westerlies afterwards.

Line 31, 34: Here, and throughout, there are a lot of textbook references for things that are more or less "facts"- I don't think every statement that is considered general knowledge needs a reference (or at least, there might be more appropriate references to cite; for example on line 34 & 39 I suggest instead the review on SSWs, Baldwin et al. 2020 in Reviews of Geophysics).

Line 47-49: Rephrase; they don't have to be easterlies to descend to the troposphere; stratosphere-troposphere coupling occurs when the winds are anomalously strong, e.g. positive NAM, as well.

Line 54, and line 160: From Figure 2d, I'd argue that the vortex is not centered over the pole once the SSW occurs (even at 40 hPa it's fairly displaced).

Line 63: it's not really the North pole but the middle latitudes where the mountain ranges matter for wave forcing.

Line 76, and line 101: easterly QBO at what level? Note that a lot of the Holton-Tan relationships are looking at QBO at 40-50 hPa, not 10 hPa as done in this study.

Line 82: westerlies maximise at what level?

Line 140: It's not clear if "all experienced mesospheric wind reversals in October" is meant to refer to 1988 and 2017 or to the other years identified- also why October, isn't this when the final warming or seasonal transition begins to occur? Seems late compared to 2002 and 2019.

Lines 143-145: For a list of years when the SH polar vortex was weak, see Lim et al. 2019 (<https://www.nature.com/articles/s41561-019-0456-x>)

Line 168-169: "as may happen with wQBO"- can you clarify what is meant here?

Figure 5, figure 7: 15-20S is a narrow latitude band- which makes it difficult to see the easterly SAO winds in the tropics. What about 0-30S or 0-20S instead? Perhaps that would make the descent between the SAO and the QBO also clearer. The "downward connection" between the SAO and QBO in these figures was not particularly compelling for that reason; particularly for 2002, there only seemed to be some interaction for a week in mid-June, when the SSW didn't happen until late September (the connection is easier to see for this reason in Figure 6).

Line 225, 276: Figure 8d- there's not a wind reversal near 60S except above 1 hPa; and isn't this just the final warming/season transition? It's not clear to me the utility of showing late October for this reason since it seems likely there are different processes at play (radiative changes driving the seasonal cycle). Why not show what Sept looked like instead (as in Figure 3 and 6) to show what was different about 2017 so that it didn't result in a warming, given otherwise strong similarities with the critical line/QBO/SAO in June?

Line 257: smaller in what way? Can this be quantified? (see major comments)

Line 265: I don't understand why there are parentheses around polar and low-latitude, but I'm also not sure this sentence is clear.

Line 283-4: "We didn't find evidence of easterly momentum being deposited throughout the winter as we did for 2002 and 2019"; this is a bit circular, but it also shows the issue with causality in this study. The other years didn't have build up of easterly momentum and there was no SSW; but is this a reflection of differences in the SAO-QBO relationship or the fact that there was no SSW due to other processes? This lack of causality is reflected in the statement on line 286, where you mention the years you did have an SAO-QBO interaction but there was no easterly momentum build up or wind reversal.

Minor discussion comment/line 303-304: Some attempts have been made to look at the upper stratosphere in the SH winter to make predictions for the following spring which should be acknowledged here:

Lim, E.-P., Hendon, H.H. & Thompson, D.W.J., 2018. Seasonal Evolution of Stratosphere-Troposphere Coupling in the Southern Hemisphere and Implications for the Predictability of Surface Climate. *Journal of Geophysical Research: Atmospheres*, doi: 10.1029/2018JD029321.

Byrne, N.J. & Shepherd, T.G., 2018. Seasonal persistence of circulation anomalies in the Southern Hemisphere stratosphere, and its implications for the troposphere. *Journal of Climate*, doi: 10.1175/JCLI-D-17-0557.1.

Line 305, 344: 20-30 days is not typical for SSW predictability (maybe a handful of events in the record have been predictable on timescales that long). Refer to Domeisen et al. 2020 (part I)- not the one that is cited in the next sentence- typical predictability limits are 10-15 days in the NH. However, it might turn out that SH SSWs are predictable at longer leads, if they are more influenced by the mesosphere/stratosphere interaction than the NH.

Line 316: One question I had was why was the focus of this study on the SH rather than the NH (which would greatly increase your sample size?). Is this something presented already in the Gray et al. (2020) paper? Perhaps it should be stated somewhere why the focus here is on the SH (maybe it is and I missed it). In terms of the QBO, ENSO, and MJO influence it should be mentioned that the Rao et al. 2020 paper considers several of these factors on the 2019 SH warming.

Line 320: what is meant by the "MJO index was positive"? (the amplitudes of the EOF1/2 in the MJO phase space were positive?) What was the phase?

Line 348: suggest non-gendered associations such as "their boreal counterpart".

Line 347-348: Are their anti-symmetrical aspects of the QBO/SAO across hemispheres that could explain a difference between NH and SH frequency of SSWs? To me there seems an obvious difference in tropospheric wave driving between the two, but if there are clear differences in the upper atmosphere that could explain frequency differences it would be interesting to describe or speculate on here.

Technical corrections

Line 22: "Annual" should be "Annular"

Line 42: should be "forming a 'comma shape'"

Line 46: replace "intrusion" (which has a very specific meaning tied to stratosphere-troposphere exchange) with "mixing"

Line 50: correct misspelling of "split"

Line 57: remove "of"

Line 63: replace "geography" with "topography" or "orography" perhaps

Line 85: change to "roughly in June"

Line 117: change "were" to "where"

Line 139: change "left out of these groups" to "considered separately"

Line 146, 170, 175, 294, 329: change "deposit" to "deposition"

Line 182: change "form" to "from"

Line 187: change to "western side of Antarctica"

Line 192: change to "before it connects"

Line 223: not sure what is meant by "subsides" in this context- "moves down"?

Line 253: change "on" to "of"

Line 261: change "there" to "their"

Line 321: change "difference" to "differences"

Line 331: change "influencing" to "influences"