Response to CC1&2

L595-605 There have now been several studies that show that extratropical tropospheric responses to polar vortex variability do not actually project very well onto the annular mode structure, sometimes being displaced by as much as 5-10 degrees of latitude (see, e.g., Black and McDaniel 2007a;b and Sheshadri et al. 2014 for the tropospheric response to stratosphere final warming events). This was also demonstrated by Sheshadri and Plumb (2016) in the context of the tropospheric response to temperature perturbations in the Antarctic stratosphere (due to ozone depletion).

More generally, one might argue that downward "propagation" is an inaccurate characterization of tropospheric responses to stratospheric perturbations since the word propagation is typically used in the context of the propagation of waves.

Thank you for these comments.

Projections onto annular modes are not directly considered in the paragraph. Those points that are made follow the review of Kidston (2015) and, in my opinion, are still valid. On the other hand, I do sympathise with your concerns over the use of the word propagation and, therefore, have changed "downward propagation" to "downward progression."

Another thought: Maycock et al. 2020 and Goss et al. 2021 framed the Atlantic jet response to SSWs as regime shifts between three preferred jet positions, rather than a straightforward equatorward shift.

Thank you for this suggestion. A new sentence has been added to the paragraph noting the alternative perspective of regime shifting.

Response to CC3

These comments were generated during the SPARC stratospheric dynamics journal club* that met over two 1.5h sessions to discuss this paper.

The paper reviews the current state of understanding of dynamical variability in the stratosphere. Overall it is well written and provides good coverage of what is a broad and rapidly evolving topic. In some places, there is a bit too much detail given which lengthens the paper and in other places, we have felt there were some important omissions.

We very much enjoyed reading this paper and it generated some good discussions. Our comments are below.

Elio Campitelli, Meryl Chittethazhathu Anil, Alison Ming, Marisol Osman, Barbara Winter, Corwin Wright, Simchan Yook

------

Thank you for selecting this article for critical review in your Journal club.

Comments:

Title: Add the word "review". It helps put the review in the right frame of mind.

Done.

Figures could be bigger. Some are not easily readable.

Where appropriate figure sizes have been increased.

Line 47: typo 'an induced a mean meridional…'

Corrected.

Line 54: "..is not associated with the changing season". The QBO does have some seasonal phase locking. See Coy et al. (2020) and references therein.

Changed to "not directly associated with." Coy et al. (2020) is cited later in the review.

Figure 2: Maybe combine T and zonal wind plots with coloured/greyscale contours for T and lines for u.

As requested, the temperature and wind panels have been combined. Also, MERRA2 data for 1980-2016 has been replaced by ERA5 data for 1979-2020. This does not affect any of the points been illustrated by the figure or any conclusions.

Figure 3: "Horizontal" typo. Caption should say that the left column is N Hemisphere and that the right column is SH. The colour bar should be changed from a divergent scale to a linear one to reflect the data being plotted.

Typo corrected. The caption now says which hemisphere the panels are showing.

Figure 4: Confusing to have Equator-direction on the left-hand side of both panels. Add a note in the caption.

As requested, note added to figure caption.

Line 161: Should probably include a word of warning on the choice of scaling for the horizontal and vertical EP flux arrow. Jucker paper is relevant here. Figure 4 caption. Mention that the Equator is on the left in (b).

See above. Reference to Jucker (2021) has been added

Line 165: "relatively" typo.

Corrected

Section 2.2: Lots of gravity waves (GWs) break in the stratosphere! They are a major driver of many processes. This whole section underplays the role of GWs which are only mentioned briefly in the last paragraph.

The last paragraph of Section 2.2 has been rewritten so that it no longer gives the impression that GWs only break in the mesosphere.

The distinction between wave types is often not made clear when talking about the extratropics in Section 2 in general. This needs clarifying when "wave" is mentioned or cleanly at the start of the section. In contrast, in Section 3 on the Tropics, the type of wave is clearly stated.

I have carefully checked Section 2 and when a distinction needs to be made between wave types I've inserted "gravity," "mountain," "planetary," and "Rossby," as appropriate.

Figure 4 and Line 160: It is worth adding a word of caution on the visualisation of EP fluxes. We are aware that choosing various research papers choose to scale F(z) . When an arbitrary factor is picked (100 in this case), we do not think it necessarily leads to “a clearer visualisation”. Jucker (2021) has a particularly good discussion of this issue and would be worth citing.

See above. Jucker (2021) is now cited.

Figure 5 caption: Of course at least some of the extra detail comes from the model - the statement “it is possible” undersells this.

The figure caption has been improved.

Figure 6: lines are so dense that when zoomed out to page-scale it generates Moiré patterns on my screen! Would suggest using colour instead/as well or making the figure itself bigger.

Figure 6(a): add some vertical guides for Jan 17 and Jan 27?

I don’t have electronic versions of the original figures, but the figure has been improved. Without electronic versions it is difficult to add the vertical guides, as suggested.

Line 225: [Caveat. We have not read the Hardiman et al. (2020) paper in detail but will be doing so.] Stratospheric vacillations are interesting and worth mentioning. Is the 120-day period sine wave a noteworthy result that should be included in this review? It feels like too much detail. Since 120 days is the length of winter, it does not seem surprising that such a sine wave can be fitted. My understanding of this topic is that there are some winters which start showing this periodic behaviour but the picture is far from clear.

I am glad you agree that the concept of vacillations is something worth mentioning. Unfortunately, most of our knowledge seems to be based on idealised/simplified models. Hardiman et al. is highlighted as it is one of the few (only?) papers with a direct connection to the observations. As only one sentence is devoted to it, it is difficult to see how that can be considered too much detail.

Line below 259, footnote 3, typo: “locally rapid temperatures rises” - remove S from the end of “temperature”

Corrected.

Line 272: A reference discussing sudden warming definitions would be good here. For example, Butler and Gerber (2018) would be a good one.

Butler and Gerber (2018) now cited (In the last sentence of Section 2.4).

Line 303: Albers and Birner (2014) is about preconditioning from BOTH planetary and gravity waves. Worth mentioning both types of waves here.

Done.

Line 305: Tim Palmer (1981) to Palmer (1981)

Tim Palmer corrected.

Line 376, typo: “display variability from cycle-to-cycle” - remove hyphens

Done.

Line 404: It may be worth including some more references on chemistry-QBO interactions. For example: Tian et al. (2006) looks at variations in NOx driving O3 and Naoe et al.(2017) looks at changes in the future O3 QBO.

A reference to Tian et al. (2006) has been added but not Naoe et al. (2017) as that study is about future changes which are not covered in this review.

Line 407, typo: "which is the principle reason" - change to "principal reason"

Corrected.

Line 423, syntax: "A corollary (...) is the occurrence of an SSW is " - perhaps insert "that" to make the text clearer: "A corollary (...) is that the occurrence of an SSW is"

Changed.

Line 465, syntax: as above, alleviate the very long sentence beginning with "One consequence", for instance "One consequence (...) is that the paradigm", and set off the clause "and thereby provide a mechanism for interannual variability" with commas. Or break those four lines of text into several sentences.

"that" inserted.

Figure 11: It would be good to guide the reader to what they are looking for in terms of the QBO disruptions. This plot is not obvious since the eastward jet during the Dec/Jan 2019/2020 is not visible in this plot. We suggest using the Singapore sonde data instead that is available from here: https://acd-ext.gsfc.nasa.gov/Data_services/met/qbo/qbo.html. Indeed their plot of zonal winds does show the relevant feature. [The Kawatani et al. (2020) paper about the SAO has few relevant plots to the QBO. The spread between reanalysis is still large over the Equator, although it is improving, and it may be best to use the Singapore sonde data.]

Single station data is shown in another recent review that touches on QBO disruptions and therefore to avoid duplication the zonal mean perspective was presented here. I don't think the weaker signal for the second interruption in the ERA5 zonal mean is due to reanalysis spread as the signal is also weak in the MERRA2 zonal mean. The last sentence of the first paragraph of Section 3.1 has been revised to note the weak signal in the zonal mean.

Section 5 begins with a general historical introduction of stratospheric modelling, referring to the early studies in this domain. As such, Section 5.1 down to about line 495 seems better suited as a part of the general introduction of the paper. Section 5 could be re-titled simply "Predictability", without internal subsections.

All sections of the paper include some historical background of the topic. On the other hand, there is very little history in the Introduction, apart from on the global stratosphere and mention of the discoveries of SSWs and the QBO. This review is on the dynamical and variability of the stratosphere and it would be rather strange not to have a specific subsection on the modelling of the dynamics and variability.

Line 498 typo: "absence" -> "absent"

Corrected.

Line 510, typo: "despite been able to simulate" - change to "despite being able to simulate"

Corrected.

Line 530: reference to obs of ssw

I've added two references to ~~Tim~~ Palmer for first reporting a sighing of the 1979 SSW from space and another reference to Miller et al. for the new satellite observations.

Figure 16: I can see why you have grey-ed out the red line for a single ensemble member but I am not sure if this figure on its own is informative without the description in Scaife et al. (2016). The summary of Scaife et al. (2016) in the text is sufficient to illustrate the point.

The figure gives useful information as it illustrates the differences in ensemble behaviour between two winters.

Line 624: why should there be only one? Omit this sentence?

The sentence has been revised.

Line 646 typo: "Peninsular" -> "Peninsula"

Corrected.

Line 706: Domeisen 2019 is https://doi.org/https://doi.org/10.1029/2018JD030077 (line 950), but it might need to be https://doi.org/https://doi.org/10.1029/2019JD030920 or https://doi.org/https://doi.org/10.1029/2019JD030923 (lines 952, 956), which are labelled as 2020a and 2020b in the references.

Well spotted! Hopefully all is now correct.

Figure 18: You should alert the reader to non-linear colour scale. Is there a more recent illustration of the increase in predictive skill since more recent papers are cited on Line 705?

Readers are now alerted to the non-linear scale. I checked through all the papers cited in the line-705 sentence but I personally didn't think any of the available pictures illustrated this point better than Figure 18. Some of the figures would also require in-depth explanations which I prefer to avoid.

Final paragraph of Section 7: "high altitude leisure flights by dangerously polluting rocket powered hobby aeroplanes" and "could increase the risk of disastrous extremes" does not seem like the right way to end this good review paper on. We would suggest re-visiting this paragraph.

This paragraph had to go at the very end as it is targeting not just scientists but people who will probably only read the bottom line. While I was keen to include something on these less scientific aspects, I also didn't want it to take up more than a single paragraph.

References:

Coy, L., Newman, P. A., Strahan, S., & Pawson, S. (2020). Seasonal variation of the quasi-biennial oscillation descent. Journal of Geophysical Research: Atmospheres, 125, e2020JD033077. https://doi.org/10.1029/2020JD033077

Butler, A. H., & Gerber, E. P. (2018). Optimizing the Definition of a Sudden Stratospheric Warming, Journal of Climate, 31(6), 2337-2344. DOI: https://doi.org/10.1175/JCLI-D-17-0648.1

Jucker, M. Scaling of Eliassen-Palm flux vectors. Atmos Sci Lett. 2021; 22:e1020. https://doi.org/10.1002/asl.1020

Kawatani, Y., Hirooka, T., Hamilton, K., Smith, A. K., and Fujiwara, M.: Representation of the equatorial stratopause semiannual oscillation in global atmospheric reanalyses, Atmos. Chem. Phys., 20, 9115–9133, https://doi.org/10.5194/acp-20-9115-2020, 2020.

Tian, W., M. P. Chipperfield, L. J. Gray, and J. M. Zawodny (2006), Quasi-biennial oscillation and tracer distributions in a coupled chemistry-climate model, J. Geophys. Res., 111, D20301, doi:10.1029/2005JD006871.

----

* The SPARC stratospheric dynamics journal club consists of a group of about 25 members ranging from first year PhD students to more senior researchers who meet online every 2 weeks to discuss a paper. The size of the group varies depending on the paper being discussed and the various interests of individuals but typically between 6 to 10 people will be present.

<u>Response to RC1</u>

Major comments:

This is an excellent, in-depth review, and I recommend that it be published after minor revisions, detailed below. There are some questions of scope — topics that are not included but could be. I think this is largely up to the author.

Thank you for the positive and helpful review.

Minor comments:

Abstract Line 7 and other places. Baldwin et al. (2019) established that the mass of the stratosphere (and above) is ~17% of the total mass of the atmosphere. The mesosphere and above (~1 hPa) is about 0.1%. The 17% figure actually does appear much later in the paper.

“just under 20%” changed to “just ~17%”

1 P1

Lines 20-21 one fifth —> 17%. Also Mesosphere and thermosphere are about 0.1% of atmospheric mass

Changed to 17%.

**There should be a zonal-mean figure showing the stratosphere, tropopause, QBO etc. BEFORE page 3. P3 discusses the QBO, but readers don’t yet have a figure of the zonal mean.

The zonal mean structure of the stratosphere doesn’t get introduced until later in the review and the relevant figures are then provided at the appropriate points in the text. Only the discovery of the QBO is mentioned on P3 and that didn’t involve the zonal mean.

Line 70 Baldwin and Dunkerton 1999/2001. The text mentions past two decades.

More details of the historical developments are given in Section 6.1 where the important contribution of Baldwin and Dunkerton is properly recognised: e.g., “the discovery that NAM anomalies propagate downward ………… Is credited to Baldwin and Dunkerton [1999], ………”

2.1 P4

Figure 2 panels should go to 1000 hPa, and show the average tropopause.

The figure has been extended down to 1000 hPa with the temperature and wind panels combined. Note the reanalysis data used was also changed from MERRA2 to ERA5 and the period extended to 1979-2020. For consistency Figures 7 and 13 have also been extended down to 1000 hPa and changed from MERRA2 to ERA5 data. This does not affect any of the points the figures are used to illustrate or any conclusions.

2.2 P5

Figure 3 should have round panels. Like Figure 5.

Figure now has round panels.

Line 154. What is “the advective component of the Brewer-Dobson circulation”?

The sentence has been revised.

Line 170. I suggest explaining what a critical layer is.

Please see previous sentence in manuscript.

2.3 P8

Line 188. Is the exclamation point part of the quote?

No.

Figure 6. The panels are slightly different sizes and the (a) and (b) are not the same.

Figure improved a little. There are no electronic versions of the originals.

2.4 P10

I suggest deleting the paragraph from l211-217. It does to add much.

Retained as the Chemistry-Climate modelling community has singled out these diagnostics as the basis for a useful metric for evaluating their models (see Chapter 4 of the SPARC CCMVal report). I think it is important to say that most of the variability of the polar night jet can be represented by just two EOFs.

2.5 P13

Line 277. It is worth adding to 1979 that satellite observations began then, and before that the data were unreliable.

This is reported in the first paragraph of Section 5.2 with additional references now added.

3 P15

4 p19

Line 445 Lesley Gray did some nice work showing that the upper level QBO may influence the polar vortex.

A reference to Anstey et al. (including Gray) 2022 has been added.

5 P21

Line 473 has the 17% mass correct!

Yes, the stratosphere lost weight as the review was being written!

Line 495 occurrences of SSWs

Corrected.

Figure 14 should be published full page width, in order read the labels.

Figure 14 has been enlarged.

Line 564 How do you know the QBO is under-represented? Could it not be that the past 65 years have shown a too-strong Holton-Tan relation (compared to a longer period with no climate change)?

The caveat "at least for a hindcast period when the observed interannual variability of the polar vortex was significantly affected by the QBO." has been added to the end of the sentence.

6 P27

L595 paragraph. The paradigm shift arose following Thompson and Wallace (1998) who defined the "Arctic Oscillation" later called annular modes. That led to Baldwin and Dunkerton (1999).

The Thompson and Wallace (2000) reference is used here as they give the definition of the annular modes. Reference to Thompson and Wallace (1998) has been added later in the paragraph. The seminal work of Baldwin and Dunkerton (1999) is already cited in this paragraph.

L622. The reference says propagation of information, but what does that mean? Can you re-word this? Information is not a dynamical quantity.

I have reworded this, as requested.

L654. Wasn't Gray's hypothesis debunked, and the QBO is no longer used to forecast hurricane frequency?

Yes, there was a debunking – please see the following sentence in the manuscript.

L686. affected

Corrected.

7 P32

** There should be a reference somewhere to the AMS chapter (Baldwin et al., 2019). You are a co-author. This review parallels the AMS chapter in some ways, although the chapter is from a historical perspective.

As requested, there is now a reference Baldwin et al. 2019 (on page 1).

** I think that this review should contain more on the dynamical effects of ozone loss, especially in the SH.

The scope of a review articles always involves a compromise between overall length, spreading things too thinly and depth of detail. For the present review I believe the balance is about right. The effects of changes in composition (e.g., ozone loss, climate change) were left out to prevent the review becoming too unwieldy, though that doesn't imply that those effects are unimportant. The Introduction clearly states that "this review considers only the variability of dynamical origins."

Response to RC2

Excellent review, very enjoyable to read. A briefly and clearly expressed overview of fundamentals on global structure and large scale dynamical variability of the stratosphere. There are only a few locations where the presentation, the structure of the sections, the reference to recent reviews would benefit improvements, as highlighted hereafter. Based on these considerations, my recommendation is minor revision.

This review is about the mean structure and large scale dynamical variability of the stratosphere. The review provides the background theoretical understanding of stratosphere dynamics, and briefly describes its early historical evolution. The review focuses later on, on the SSW and QBO phenomena.

Given the global approach, part of the structure of the review is not optimal. This is my major comment. Because material of section 2 "Extratropical stratosphere", such as the hemispheric-scale mean meridional overturning circulation, the equations of section 2.2, Figure 7 with its large STD over the Equator, do not really fit under the header "Extratropical stratosphere". Please consider first a section on global theoretical and observational background, and thereafter distinguish between extratropical and tropical stratosphere.

Thank you for your insigtful comments.

The choice of the structure of the review is largely personal and what might seem optimal for one reviewer may not be considered optimal by another reviewer (cf., Reviewer #3 comments). The hemispheric-scale meridional overturning circulation is mainly driven by the extratropical dynamics therefore it is not inappropriate for it to come under the header "Extratropical stratosphere". Figure 7 was included to show the variability of the polar night jets. The large STD over the Equator is now noted, and the reader referred on to the section on tropical variability.

L12: "or at least a second layer to the Earth's atmosphere" unclear, possibly redundant.

I'm not sure what is unclear? All that was discovered was another layer to atmosphere above the one we live in and at that time it wasn't known if there were more layers above that, therefore "at least a second layer" seems appropriate.

L20: The subdivision of a lower (troposphere) and middle atmosphere (everything above) has limitations, in recognition of the strong two-way dynamical coupling between the troposphere and stratosphere. The two-way coupling is mentioned later on in the introduction, where it can be consequently noted that to divide the atmosphere in the lower and middle atmosphere is not necessary the best view, when considering atmospheric dynamical processes.

Given that one of the most used textbooks in this field is called “Middle Atmosphere Dynamics” I think it is important to say what is usually meant by the term “middle atmosphere.” This is neither endorsing the concept of the middle atmosphere nor discussing the pro and cons of such a concept.

L37-40: In the stratosphere there is "variability of dynamical origins" at other scales of motions, e.g., the ubiquitous gravity waves that drive the QBO, or that may condition the stratospheric vortex, for instance. Although the review focus is on the most distinctive phenomena, it would be worth, however, to highlight that not covered are the important workings of gravity waves, and possibly refer to a recent review on them, if any.

“variability of dynamical origins” changed to “large-scale variability of dynamical origins.”

L40: Maybe it is somewhat unfortunate for this review that SSWs have been very recently reviewed (Baldwin et al 2021). Please explain why we need a new review on SSW so close to the one of Baldwin et al 2021? What does this review adds and what is the difference in focus, perspective or else?

It would be a bit strange having a review of stratospheric dynamics without including something on SSWs. Here SSWs are considered as part of the overall polar vortex variability and general theoretical understanding of stratospheric dynamics. Reviews in the Baldwin et al. series are usually more specialised and, with a large team of authors led by Mark Baldwin, reach depths of knowledge other reviews don’t reach. The more comprehensive nature of Baldwin et al.’s assessment of SSWs is already acknowledged in the text.

L51: "this dynamical forcing" unclear

Changed to “the dynamical forcing of the stratosphere away from radiative balance”

Section 2: Although in the stratosphere "dynamics" does lead to (large) departures from radiative balance, the mean structure of the middle atmosphere is fundamentally dominated by radiative forcing (i.e., the summer easterlies and winter westerlies). Please consider being more explicit to pass this message very early in the section, instead of later on along the writing (i.e., L98: "underlying annual cycle throughout the extratropical stratosphere is fundamentally a radiatively driven phenomenon")

In the second sentence “thermal structure” has been changed to “thermal structure and underlying annual cycle.”

L81 "In this situation" not clear.

Deleted.

L85 "eastward circumpolar vortex" Given that commonly used is as well the term "westerlies" please consider to add in a note that here the "eastward and westward" terms are used, instead of the respective "westerlies" and "easterlies", as more commonly done in practical meteorology.

Footnote added.

Figure 2: Section 2.1 starts with "Unlike the troposphere", but then the troposphere is not discussed. That is fine overall, but it may be worth to further the distinction of the troposphere and the stratosphere, with very briefly explaining why there are westerlies year around in the troposphere, as show in the figure, which could actually be extended to 1000 hPa. Indeed, unclear why bottom at ~500 hPa?

The figure has been extended down to 1000 hPa. Also, the MERRA2 data for 1980-2016 is replaced by ERA5 data for 1979-2020 and the temperature and wind panels combined. For consistency Figures 7 and 13 have also been extended down to 1000 hPa and changed from MERRA2 to ERA5 data. The change to ERA5 does not affect any of the points the figures are used to illustrate or any conclusions.

Figure 3: Possibly a time mean state would be a better example than a daily field, given that there might be January days in the NH with a rather circular vortex.

Using the time mean is almost certainly going smooth out a lot of small-scale variability and is, therefore, problematic when used as evidence that only the largest scale waves propagate.

Figure 4: Consider a more holistic approach and include the troposphere (extend the plots to ~1000 hPa) to help in illustrating how dynamically coupled are these layers.

Extending Figure 4 below 100 hPa would require adding a lot more detail and explanation to the text on the EP-Flux in the troposphere whereas the section is focused on the stratosphere. Coupling to the troposphere is considered later in the review. Therefore, I prefer to keep the existing figure.

L172-173 "gravity waves tend not to break until the mesosphere" possibly good to note that orographic gravity waves, and in the SH in general, may break in the stratosphere, something not always appreciated.

These sentences have been revised to address this comment.

Figure 7: similarly as above, my suggestion is to be more comprehensive and to show the troposphere. Added value would be to briefly point out differences and similarity in the variability of these layers.

Figure 7 has been extended to 1000 hPa and for consistency with the revised Figure 2 MERRA2 replaced by ERA5. This has no impact on those aspects that the figure is used to illustrate or any of the conclusions.

L202-210: In general discussion of figure 7 needs to be improved, indeed noting as well the large inter annual variability centred at the Equator. This is another example that it would be worth to consider an easy restructuring of the review, first considering the global background of theoretical and observational aspects, and then deal with SSW and QBO.

The large inter annual variability at the Equator is now noted and the reader referred to the section on tropical variability.

L206: "This is what is expected for variability resulting from Rossby wave forcing from the troposphere." Unclear. Why should be SH October monthly STD similar to the NH winter monthly STD, on the basis of forcing from the troposphere?

This is explained in the following two sentences of the manuscript.

L481: "(starting with Boville (1984))" substitute commas to the brackets (to avoid double brackets)

Changed.

L495: "models simulate occurrences SSWs reasonably well" occurrence with the meaning of frequency or of their fundamental dynamics? The fundamental dynamics of the SSWs is known to be captured by models since some time (e.g. Charlton et al J. Climate, 20, 470-488, 2007). However, at that time, the modelled SSW frequency was diagnosed low. What is the current status on modelling the SSW frequency in climate models?

Changed to “models simulate the frequency of occurrences of SSWs reasonably well.”

Section 6: The first paragraph is more general than the "Middle and high latitudes". Consider moving it above the section 6.1 title, as mini-introduction to the topic.

The first paragraph only refers to the surface response in the middle and high latitudes.

L599: Kidston et al., 2015 is, as well, another review. Please mention that it is a review.

This is mentioned at the end of the next paragraph.

L604: Please consider the seminal work of Perlwitz and Graf, 1995, "The statistical connection between tropospheric and stratospheric circulation of the Northern hemisphere in winter", J Climate, 8, 2280-2295.

Reference to Perlwitz and Graf, 1995 added.

L614 "(Kidston et al., 2015, and ref. therein)" Please do report the key works instead, given they very importance in demonstrating via controlled model experiments the downward influence. In alternative, it would be necessary to write that the topic is not here reviewed, because already done in Kidston et al.

Changed to “as reviewed by Kidston et al. (2015).”

L615: Confusing the reference to the EOF at this stage, given that the previous paragraph ends with a "genuine physical downward influence" found in model experiments, results which are independent on EOFs. Consider revising and better connecting these paragraphs.

In general, a mechanism for downward influence was reviewed by Kidston et al., 2015, Box 1. It would make sense to critically comment on that review, and if anything new has surfaced since that time, please report.

The sentence is just making the rather important point that EOFs are not physical modes and therefore, by themselves, don’t provide an explanation for the genuine physical downward influence. This is then followed by a brief review of current knowledge of the mechanisms for the downward influence including advances since 2015.

L644 Please start a new paragraph for the SH. Concerning the SH, of interest would be to distinguish the two time scales over which downward influence has been found: the long time scales (trends) related to ozone depletion and the intra-seasonal time scale (by Lim et al etc.).

Paragraph break added. Secular changes were not considered in this review given the wide range topics already included. The scope of the review is given in the first and fourth paragraphs of the Introduction.

Response to RC3

The manuscript does an excellent job of documenting over 50 years of research on stratospheric dynamics and variability. This is a broad focus and I think that this review strikes a nice balance between highlighting two of the subjects that are discussed in more depth (L37-40), SSWs and the

QBO, and all of the other literature that is also encompassed by stratospheric dynamics and variability, e.g., the Brewer-Dobson Circulation, wave mean flow interactions, models and predictability, etc. These subjects are suitable for publication in this journal. I recommend publication following minor revisions.

I think it is probably impossible to include just the right amount of detail on every subject so that every reader is pleased. Me being one of those readers, below are my suggestions and questions that I hope can make some of the content of this manuscript more comprehensive. I do not advocate for any huge changes in the manuscript's structure. Setting the sections up as 1) introduction, 2) extratropical stratosphere, 3) tropical stratosphere, 4) tropical-extratropical coupling, 5) models and predictability, and 6) influence on the troposphere makes sense.

Thank you for this very positive and useful review.

L65: When considering the reverse influence of the extratropics on the tropics, perhaps a single sentence could be added citing work that shows a relationship between SSWs, the BDC, and subsequent changes in tropical convection via the BDC. These studies are mentioned later on in the manuscript (e.g., Noguchi et al. 2019).

This paragraph only refers to the stratosphere and the downward influence doesn't get mentioned until later in the Introduction. However, these aspects are covered later in the review.

L65: Consider citing Anstey et al. (2021) as their statistical analysis on the relationship between the QBO and SSWs reinforces this point.

Again Anstey et al. (2021) is referred to later in the review. My preference is for a concise Introduction with the extra detail left to the individual sections of the review.

L109-110: In an effort to be more comprehensive, consider adding something about ocean heat fluxes as a forcer of transient planetary waves. Garfinkel et al. (2020) considers this in addition to topography and land-sea contrast.

I didn't include anything on the role of ocean heat flux as I felt that for that level of detail the sensitivity first needs to be more firmly established using more than just a simplified model. Nonetheless Garfinkel et al. (2020) is cited for those readers who are interested in obtaining further details.

L166-168: Suggests removing "..is wave breaking .." in this sentence. I generally think of wave-breaking as coinciding with wave activity flux convergence (probably with intermittent wave activity flux divergence too) and so the part of this sentence relating wave breaking to both acceleration and deceleration of the mean flow confuses me.

Agreed! The offending sentence has been rewritten.

L223-224: Consider mentioning the study by Sjoberg and Birner (2014) on the vacillation cycles as a means of being more comprehensive. They corroborate these important results from Holton and Mass, but identify some key limitations of the 1976 modelling setup: https://journals.ametsoc.org/view/journals/atsc/71/11/jas-d-14-0113.1.xml?tab_body=fulltext-display

Thank you for the suggestion of Sjoberg and Birner (2014) however, in my opinion a more comprehensive assessment of vacillations in simple models such Sjoberg and Birner is too specialised for this review. Instead, I prefer to focus more on links with the observed variability such as the Hardiman et al. study.

L256-257: I think the language stating the planetary waves may *only* promote deceleration of the polar vortex should be removed. Downward wave coupling events, in which the majority of a transient planetary wave propagates downward towards the troposphere rather than upward into the stratosphere, coincide with wave activity flux divergence and acceleration of the high-latitude stratospheric wind (e.g., Dunn-Sigouin and Shaw 2015).

"vertically propagating Rossby waves" has been changed to "upward propagating Rossby waves"

L293-295: Would you please add some citations here to substantiate the point that radiative cooling is the most dominant process? I think this must be the case, but what studies (e.g., modelling, observational) are you thinking of when you write this?

The sentence has been revised slightly to remove the word dominant. I wasn't thinking of any study when I wrote this sentence as it didn't seem like any additional reference was needed.

L396-L308: While this section reads nicely as is, I think it could be expanded to include other mechanisms explaining transient planetary wave propagation. Consider mentioning linear interference, during which there is phase coherence between a transient wave and the climatological stationary wave (e.g., Watt-Meyer and Kushner 2015, mentioned in older work such as Holton and Mass 1976). Another mechanism to consider discussing is upscaling of synoptic scale wave activity to planetary scale wave activity (e.g., Boljka and Birner 2020).

Watt-Meyer, O., & Kushner, P. J. (2015). The role of standing waves in driving persistent anomalies of upward wave activity flux. Journal of Climate, 28(24), 9941-9954.

L296-308: While not dismissing the important or relevance of the suggested mechanisms my own feeling is the extra detail is too specialised for this review and risks blurring the main points I am attempting to make.

Comments on section 2: I enjoyed this section. I think beginning with the zonal mean climate and progressing toward potential vorticity, which involves deviations from zonal symmetry (waves), and then progressing towards wind variability and SSWs makes sense structurally.

Thank you for this very positive remark.

L380-382: Countering their being consensus on the role of extratropical waves modulating the QBO at that time, Hamilton et al. (2004) showed observational and modelling evidence that the QBO westerlies are zonally asymmetric. They attributed this response to equatorward propagation by the extratropical stationary wave. Shuckburgh et al. (2001) also showed evidence of horizontal wave propagation directly into the QBO westerlies. I think some of the earlier studies suggesting that extratropical planetary waves may influence the QBO could be mentioned. These are topical considering the current understanding of how QBO disruptions occur.

Hamilton, K., Hertzog, A., Vial, F., & Stenchikov, G. (2004). Longitudinal variation of the stratospheric quasi-biennial oscillation. Journal of the atmospheric sciences, 61(4), 383-402.

Shuckburgh, E., Norton, W., Iwi, A., & Haynes, P. (2001). Influence of the quasiâ biennial oscillation on isentropic transport and mixing in the tropics and subtropics. Journal of Geophysical Research: Atmospheres, 106(D13), 14327-14337.

The studies of Shuckburgh et al. and Hamilton et al. are now cited.

Figure 12: Rather than “E” and “W” as labels for eastward and westward, consider writing “eastward” and “westward.” I immediately assumed “E” meant easterly instead and if other readers make this mistake as I did, they will be wondering why the QBO-MMC looks reversed.

Thank you for this suggestion. “Eastward” and “Westward” have been brought on as substitutes for “E” and “W”, respectively

L428-436: Consider citing Yamazaki et al. (2020). By discussing a tropospheric pathway for the Holton-Tan effect, they have broadened the possible number of routes that the QBO can use to communicate with the polar vortex. The original HT mechanism, plus the QBO-MMC work, are very stratosphere focused, yet this new work is more troposphere focused (albeit the QBO’s influence must be communicated down to the troposphere from the stratosphere to initiate the teleconnection).

Yamazaki, K., Nakamura, T., Ukita, J., & Hoshi, K. (2020). A tropospheric pathway of the stratospheric quasi-biennial oscillation (QBO) impact on the boreal winter polar vortex. Atmospheric Chemistry and Physics, 20(8), 5111-5127.

Yamazaki et al.’s possible tropospheric pathway is now mentioned at the end of the paragraph.

L510: type: “been” vs. “being.”

Corrected.

L629-630: There are some paper perhaps worth mentioning identifying some characteristics of SSWs that have a downward propagating response, White et al. (2019) and Cámara et al. (2019). This will help make this section more comprehensive.

White, I., Garfinkel, C. I., Gerber, E. P., Jucker, M., Aquila, V., & Oman, L. D. (2019). The downward influence of sudden stratospheric warmings: Association with tropospheric precursors. Journal of Climate, 32(1), 85-108.

Cámara, A. D. L., Birner, T., & Albers, J. R. (2019). Are sudden stratospheric warmings preceded by anomalous tropospheric wave activity?. Journal of Climate, 32(21), 7173-7189.

This extra level of detail is already covered in the recent comprehensive review of SSWs in the Baldwin et al. series and to include it here risks duplication. Baldwin et al. is already referenced and, in turn, includes citations to both White et al. and de la Cámara et al.