

Response to Reviewer#2 for: QBO-induced anomalous transport in the Northern Hemisphere stratosphere: the exceptional 2018/2019 late boreal winter

Minganti et al., EGUSphere, 2026

We thank the reviewer for their useful and constructive comments. In our replies below, the italic type is used for the reviewer's comments, the plain text for authors' answers and the bold type for the revised text in the manuscript.

Replies to Major comments.

1. *Focus of paper, presentation style.*

The paper expands in many places on a lot of details, and is very long. I believe the paper would benefit from condensing the writing by keeping a clearer focus. Examples are given in the specific comments below and in the annotated pdf. A particular concern is, in my opinion, the merged discussion of, on the one hand, inter-model comparisons, and on the other hand the investigation of the processes that led to the early 2019 anomalies. In particular in section 3 this makes the text hard to follow. Therefore, I suggest that the authors either put less focus on the discussion of inter-model differences (given the overall conclusion appears to be that models agree well), or they

re-structure their paper into two sections, one with a clear focus on the tracer and dynamical anomalies (possibly with a reduced number of data sets), and another section with a focus on inter- model comparisons (i.e., do all models capture the tracer anomalies and the suggested mechanism). In this case, the latter needs to be more quantitative, see next comment.

Following the relative Specific Comments and the additional comments annotated in the submitted manuscript, we significantly reduced the parts describing the differences between datasets, especially in Sects. 4.X. For this reason, we did not change the layout of the Sections in the revised manuscript. See track changes for more details on the text reduction.

2. *Qualitative nature of inter-model comparison.*

If the authors decide to keep one focus on inter-model comparison, it needs, in my opinion, to be more quantitative (see e.g. below suggestion in comment on line 363ff). From the current Figures, the inter-model differences are hard to see, let alone quantify. For Example, I find it hard to understand what the concluding statement that the ERA5-driven CTM is superior to other reanalysis is based on (line 639).

As mentioned above, we significantly reduced the inter-dataset comparison and we expanded the discussion about the physical mechanism leading to the transport and tracer anomalies. In addition, as suggested in the Specific Comment of line 363ff, we added the proposed scatter plot in the revised manuscript. See the reply to that comment for more details on the new figure.

The statement at line 639 of the submitted manuscript refers to the results shown in the zoom inlet of Fig. 2, where the N_2O anomalies in BASCOE-ERA5 reach 3 sigma, while the other model-based datasets (BASCOE-MERRA2, BASCOE-JRA3Q, M2-GMI and to a lesser extent SD-WACCM) show smaller anomalies. We made this point more specific in the corresponding part of the text:

Concerning the amplitude of the N_2O anomalies in the lower stratosphere, the BASCOE-ERA5 simulation agrees best with the chemical reanalyses and with SWOOSH.

3. Discussion of dynamical mechanism

As the authors noted, a SSW occurred in January 2019, which is the focus month of the dynamical analysis in the paper. This is also visible in the quite weak mean zonal winds in the polar vortex region in Figs. 6/7. However, the authors do not consider the contribution of the extratropical circulation anomalies to the anomalous meridional advection of N_2O presented in their study. After the SSW, the negative winds will block vertical planetary wave propagation, so that they might rather dispose their momentum in the lower instead of the middle stratosphere. This is, in my opinion, a possible explanation of the vertical dipole seen in the EP flux divergence in Figure 8, with enhanced negative EPFD in the lower stratosphere in mid-latitudes, and reduced above. Indeed, this is to me more plausible than arguing with the poleward shift of the subtropical zero wind line to 15°N , given enhanced wave forcing occurs rather poleward of 30°N . I strongly suggest the authors include discussion of the effects of the SSW on the circulation anomalies.

Furthermore, the authors convincingly show that the meridional advection of N_2O was anomalous in the period of interest, resulting in the strong N_2O anomalies. They argue an important ingredient was the anomalous meridional circulation component v^* , associated with enhanced lower stratospheric wave driving and the secondary circulation from the QBO. However, anomalies in v^* are not shown, and in particular the latter point on the secondary circulation is therefore rather inferred than directly shown. Therefore, I suggest the authors include analysis of v^* , and/or anomalies in the residual streamfunction; in particular the latter is well suited to reveal the secondary circulation component (showing as closed cells of circulation anomalies extending from the equator to the subtropics).

Following a similar comment from reviewer 1, we substantially expanded the discussion about the impact of SSWs on the enhanced wave forcing and circulation/transport anomalies. Examples of additional discussion on the SSW impact are at lines: L386-405, L428-444, L566-601 of the revised manuscript.

Concerning the vertical dipole in the EPFD anomalies, we attribute it to the vortex displacement caused by the January 2019 SSW (Lee and Butler, 2020). The positive EPFD anomalies in the upper stratosphere correspond to the vortex westerlies displaced towards the mid-latitudes (as shown by the zonal wind contours), and the negative EPFD anomalies in the lower stratosphere are caused by the SSW (through mass conservation after the onset of the easterlies) and by the seasonality of the secondary circulation.

Concerning the addition of anomalies of the meridional residual circulation, we added black contour lines of $\bar{v}^*$ anomalies in Figs. 8 and 9 of the revised manuscript, and we related them to the A_y and EPFD anomalies. We decided to show $\bar{v}^*$ instead of the residual streamfunction to facilitate the connection with the A_y anomalies.

Replies to Specific comments.

- *Line 101 ff: Is this the first study that uses the N₂O SWOOSH data? If yes, it would be good to provide a more comprehensive description of the merging procedure, or point to a report or similar where this information can be found.*

If no, please refer to previous studies on the description.

This is not the first study using N₂O SWOOSH data. Ray et al. (2026) is the first study using N₂O from SWOOSH, and is now mentioned in the revised manuscript:

In this study, we use a new merged N₂O SWOOSH product (recently used in Ray et al., 2026), ...

- *Line 168: Not clear what is meant here by FWmaSD - is this a specific set-up of the nudging? Please specify, and/or refer to the relevant paper.*

The FWmaSD naming refers to a particular component set (i.e., configuration) of the WACCM model. In particular, "FW" means WACCM with observed sea surface temperatures (i.e., non-interactive ocean), "ma" refers to the middle atmosphere chemistry, and "SD" (specified-dynamics) indicates that the meteorology is nudged towards MERRA-2. Most of these information is already in the SD-WACCM description (except the observed sea surface temperatures). Hence, we decided to remove the mention to "FWmaSD" and we added that the sea surface temperatures are observed:

... and uses the middle atmospheric chemistry scheme, with the dynamics nudged to MERRA-2 meteorology and with observed sea surface temperatures (i.e., non-interactive ocean)

- *Line 195ff:*

The definition of the EP fluxes as in Equ. 4a and 4b is inconsistent with the EPFD in Equ 6a and 6b. Please only provide the Equations for the version of the actual calculation you have performed; the calculated EP fluxes and the EP flux divergence have to follow the same assumptions.

Done. We use the EPFD definition based on the primitive equations (i.e., Eqs. 4 remains unchanged and we adapt Eqs. 6, see updated version of the manuscript). We also explicitly include the term $1/\rho \cos\phi$ that multiplies $\nabla \cdot \mathbf{F}$ (with $\rho = \rho_s e^{-z/H}$ and $\rho_s = 1$) in the definition of EPFD (i.e., $EPFD = (1/\rho \cos\phi) \nabla \cdot \mathbf{F}$), and we adapt the definitions of $EPFD_y$ and $EPFD_z$ accordingly.

- *Line 240 to 264: I find the discussion of differences between the datasets quite difficult to follow. Also, I wonder whether the details are important given the focus of the paper on explaining the 2018/19 N₂O anomaly. Consider condensing these paragraphs, or at least clarify and re-phrase, focusing on the most important messages.*

We significantly shortened the first paragraph (on BASCOE-MERRA2) and we kept only the relevant information about BASCOE-MERRA2 (smaller amplitudes of N₂O anomalies compared to the other datasets) and the consistency with the recent AoA study. For the second paragraph (on the differences between BRAM2 and M2-SCREAM), we also largely reduced the text, and we kept the relevant information about the good agreement between BRAM2 and M2-SCREAM and their consistency with respect to Wargan et al., (2023). The new text is contained in the following paragraph of the revised manuscript:

One of the datasets that differs most from the others is the BASCOE-MERRA2 simulation, which tends to deliver smaller amplitudes (i.e., less positive/less negative) of the N_2O anomalies compared to the other datasets. In the lower stratospheric mid-latitudes, the N_2O concentrations are inversely proportional to the mean Age of Air (mAoA, M2020). Hence, the smaller magnitudes of the N_2O anomalies are consistent with the recent mAoA results from Vervaeke et al. (2026), who found that MERRA2 delivers the oldest mAoA compared to ERA5 and JRA3Q using the same BASCOE CTM simulations. Concerning the chemical reanalyses, the comparison between BRAM2 and M2-SCREAM generally delivers very good agreement, with a few differences at specific times of the record (e.g., in early 2005). Wargan et al. (2023) showed high consistency between the N_2O climatologies of BRAM2 and M2-SCREAM, and, even though they did not show comparison of N_2O anomalies, our results at 50 hPa are consistent with their results for the N_2O climatologies.

- *Line 275 to 290: Again, I find the discussion of the differences in the data products difficult to follow, and I think there is too much detail here that might not be as important. For me, the main message is that the products are overall all able to reproduce the strong anomaly, with some differences in absolute value. Please condense and simplify.*

A good example would be, in my opinion, your description of inter-model differences in the ozone anomaly timeseries (line 304ff)

We removed most of the text in these lines in the revised manuscript. We decided to still mention the good representation of the N_2O anomalies in SD-WACCM, consistently with Yu et al., (2022):

Concerning SD-WACCM, the good representation of the magnitude of the N_2O anomalies is consistent with Yu et al. (2022), who evaluated the water vapour variability in the tropical region and reported realistic trends in water vapour in SD-WACCM compared to merged satellite datasets.

- *line 312: If AO/NAO was the circulation pattern associated with the ozone anomaly, there still has to be a stratospheric transport anomaly that causes the ozone anomaly. And given you don't see an N_2O anomaly, was this of different nature compared to the other transport anomalies? Please comment on this.*

Done. We further investigated the ozone anomaly in early 2010. Since there was no clear anomaly in $\bar{v}^*$, we focused on the vertical residual advection $\bar{w}^*$ and we noticed that it was slightly negative in early 2010 (Fig. S5 of the revised supplement). Hence, we looked at the TEM budget of ozone (only in the BASCOE-ERA5 simulation for simplicity), and in particular at the contribution of the vertical advection term $(\overline{O_3})_z \bar{w}^*$ (i.e., the equivalent for ozone of A_z , see text in the revised supplement for its definition, Fig. S6). We noticed that this contribution was anomalously positive in early 2010, indicating ozone increase through vertical residual advection. Negative Arctic Oscillation phases (like the one occurred in early 2010) result in enhanced planetary waves breaking and ultimately in enhanced downwelling by the Brewer-Dobson Circulation (Zhang et al., 2017). Such enhanced downwelling is visible in the negative $\bar{w}^*$ anomalies in (Fig. S5 of the revised supplement) and results in enhanced vertical advection of ozone (Fig. S6 of the revised supplement), which ultimately builds up to the ozone anomalies in Fig. 3 of the revised manuscript.

We do not see equally large N_2O anomalies in early 2010 because the N_2O concentrations are smaller in the upper layers of the stratosphere compared to the lower layers, resulting in generally small N_2O concentrations transported downwards via the A_z term at mid-latitudes (M2020). Consequently, the enhanced downwelling $\bar{w}^*$ in early 2010 did not significantly impact the total advection term of the N_2O TEM budget ($A_y + A_z$, Fig. S3 of the revised supplement) in the same period.

- *line 356 to 362: As stated in line 357, biases in the mean BDC do not necessarily reflect in the strength of anomalies in a particular year. So I'm not sure this discussion on the difference in the anomaly strength in different products is very meaningful.*

The reviewer is correct. We reduced and reframed this discussion. We highlight that, despite the differences in the mean BDC intensity, the meteorological reanalyses can robustly capture and isolate the anomalous meridional residual transport in January 2019. This rephrasing allows to emphasize the good agreement between the meteorological reanalyses and does not relate anymore the mean BDC to the transport anomalies in January 2019:

Although the meteorological reanalyses can widely differ in their representation of the mean intensity of the BDC (e.g., Ploeger et al., 2021; Kobayashi and Iwasaki, 2024), the magnitude of the A_y anomalies in January 2019 is highly consistent between them. This agreement suggests that the meteorological reanalyses can robustly capture and isolate the contribution of the anomalous meridional residual transport in January 2019.

- *Line 363ff: For making the point on the relation of anomalous transport contributions versus the actually anomaly in a more quantitative way, it would be useful to produce e.g. scatter plots of A_y (and/or $A_y + A_z$) in Jan 2019 versus the N_2O anomaly in Feb 2019 across the individual models. Such a Figure would give a much more quantitative view of the relation of transport anomalies and tracer anomalies across model. As is now, this discussion appears a bit selective and qualitative.*

Indeed. We added a scatter plot of the A_y anomalies in January 2019 versus the N_2O anomalies in February 2019 for all datasets except SWOOSH (because it's not possible to compute its TEM budget, see Figure 5 of the revised manuscript). See lines L349-367 of the revised manuscript for the discussion of the new figure.

- *I'd strongly suggest to show the sum of EPFD_z and EPFD_y . If the latter has no strong anomalies, the former will dominate also in the sum, but that way you make sure you capture the full forcing.*

Done throughout the manuscript.

- *Line 391ff: I find those differences in WACCM difficult to see from the Figure (mostly lines are on top of each other, and differences between data sets are difficult to see). Also, I think those differences appear to be minor, so not necessarily worth discussing. Thus I suggest to drop this paragraph, also because the fact that the free-running WACCM versus the SD-version has different planetary wave forcing is rather obvious.*

Indeed, the paragraph was removed.

- *Line 504ff: Did the authors attempt to quantitatively determine the contributions of individual TEM budget terms by time-integrating the budget equation to obtain the tracer*

time-series? I know this can be difficult in practice due to large cancellation and residual terms, but the authors might want to acknowledge that this is in principle possible.

No, we did not do that precisely because of the large residuals and numerical errors that would arise from such time-integration. In the revised manuscript, we mention that this is theoretically possible, but practically prone to errors, see the revised text:

A quantitative comparison between the N₂O concentrations and the A_y term requires the time-integration of the TEM budget equation (Eq. 1), but this is practically prone to significant errors because of the large residuals and numerical artifacts (M2020). Because of this fundamental limitation, we limit our comparison to a qualitative analysis of the agreement of the signs of the A_y and N₂O anomalies, rather than a quantitative comparison of their magnitudes.

- *Figure 7: The label on the colorbar seems to be wrong (says w^* instead of A_y)*

Corrected.

- *Line 519: Given the important role of the secondary circulation in the discussion, it would be great to show the mean residual circulation anomaly of the month of interest (e.g. overlaying contours of the residual streamfunction anomaly in Fig. 7 could be very helpful).*

Following also a very similar comment from reviewer 1, we added black contour lines of $\bar{v}^*$ anomalies in both Figs. 8 and 9 of the revised manuscript. This addition allows to clearly see the relationships between the EPFD and A_y anomalies and the $\bar{v}^*$ anomalies, and to ultimately connect with the results shown in Fig. 7 of the revised manuscript.

- *Line 608/Figure 10: Why don't you show v^* ? This would be much more consistent.*

Following a major comment from reviewer 1, we removed Fig. 10 of the submitted manuscript (together with Section 5) because of a dynamical misconception. However, as mentioned in the reply just above, we included contour lines of $\bar{v}^*$ anomalies in Figs. 8 and 9 of the revised manuscript.

- *Line 670-697: This reads like a nice motivation and outlook for stratospheric science in general, but I think goes far beyond "Summary and Conclusions" related to the current manuscript. Please condense significantly.*

Done, we significantly condensed these informations in the revised manuscript. We kept the original intended meaning, which is the possible future increase of extreme events in a changing climate and the lack (and need) of continuous satellite measurements of N₂O and more in general of stratospheric composition. See track changes for the details.

Replies to the annotations in the pdf

1. *L7: determined - consider changing to "driven by"*

Changed "determined" to "forced" instead of "driven" to avoid repetition with the sentence before (that includes "driven by").

2. *L32: of $\rightarrow$ in*

Done.

3. L38: *of* $\rightarrow$ *in*

Done.

4. L41: *Rather: "Impacts of the QBO are not restricted to.." (to QBO itself is restricted to tropics)*

Done.

5. L48-49: *doesn't it depend on the phase of the QBO whether the easterly or westerly wind are in the upper/lower stratosphere?*

This description (L46-L49 of the submitted manuscript) refers to the condition of descending easterlies (mentioned at L45-46 of the submitted manuscript), i.e., when the easterly winds are located in the upper stratosphere. In the revised manuscript, we replaced 'The circulation...' with 'This circulation....' to clarify that the description refers to the descending easterlies condition (as opposed to descending westerlies described at L49 of the revised manuscript):

This circulation pattern is completed by poleward motion at the levels of maximum easterly wind (i.e., usually located in the upper stratosphere) and by equatorward motion at the levels of maximum westerly winds (usually located in the lower stratosphere) (Choi et al., 2002). During descending westerly wind shears (i.e., W-QBO), an opposite circulation develops.

6. L59-L61: *Isn't this prepetative from above (see line 55)?*

Indeed - the sentence was removed.

7. L82: *studies by*

Done.

8. L94: *I think this sentence can be deleted*

Following one of the comments of reviewer 1, we expanded this part with the text describing the datasets in the Introduction (L73-81 of the submitted manuscript). The new text in the revised manuscript is much more informative compared to the submitted manuscript:

In this Section, we describe the datasets used and the Transformed Eulerian Mean (TEM) diagnostics. Reanalysis datasets combine atmospheric observations with a global forecast model through data assimilation to provide a global multi-decadal and continuous state of the past atmosphere. Meteorological reanalyses assimilate dynamical observations such as surface pressure, wind and temperature and are often used to drive chemistry transport models (CTMs) to simulate realistic distributions of chemical species (e.g., Ménard et al., 2020). Chemical reanalyses assimilate chemical observations and their transport is driven by external meteorological fields (often obtained from meteorological reanalyses, Errera et al., 2019). Chemical reanalyses are often used to evaluate the evolution of the concentrations of chemical tracers in CTMs and climate models (e.g., M2020; Chabrillat et al., 2025). Specified dynamics models usually constrain the dynamics of a chemistry climate model towards the dynamics of a meteorological reanalysis (e.g., Davis et al., 2022).

9. *L107: see comments. Is this the first study that uses the N₂O SWOOSH data? If yes, it would be good to provide a more comprehensive description of the merging procedure, or point to a report or similar where this information can be found. If no, please refer to previous studies on the description.*

See the reply to the relative comment above.

10. *L168: see comments; Not clear what is meant here - is this a specific set-up of the nudging? Please specify, and/or refer to the relevant paper.*

See the reply to the relative comment above.

11. *L201: see comment*

Fixed, see the reply to the relative comment.

12. *L209: suggest rather something like: climatological profiles and their variability*

Done, the text was changed to **"... along with the latitudinal climatological profiles and their variability."**

13. *L229: i'd add here something like: "... from SWOOSH and various reanalysis and model products."*

Done, we added **"... from SWOOSH and the different reanalyses and model products."**

14. *L234: If you say it varies, give an example of which product varies relative to which - do you mean BASCOE-MERRA2 varies particularly strong in amplitude compared to SWOOSH, while other data sets tend to agree more?*

In that sentence, we meant that the amplitudes of the anomalies vary among datasets, and that BASCOE-MERRA2 is different from the other datasets. To make this more clear, we added:

... varies among the datasets (e.g., in BASCOE-MERRA2 compared to the other datasets).

15. *L240: unclear, please consider rephrasing*

This part was removed because of the reduction and re-phrasing of the text in the paragraph.

16. *L272: ...and the direct observation-based product (SWOOSH)..*

Done.

17. *L298-299: I believe this is ment to say that the tropospheric sources lead to higher mixing ratios/ concentrations in the tropical lower stratosphere compared to the mid-lats, and thus this gradient leads to the increase of N₂O in mid-lats by meridional advection? Please rephrase to clarify, and likewise the comment in brackets for ozone.*

Done, the text was rephrased:

The N₂O and ozone concentrations are anti-correlated in the lower stratosphere at extratropical latitudes because of stratospheric transport (Hegglin and Shepherd, 2007). N₂O enters the tropical stratosphere from the troposphere, and the northward climatological transport results in an increase of

N₂O concentrations in the mid-latitudes (M2020). Ozone, on the contrary, is produced in the tropical upper stratosphere and the northward transport reduces its concentrations by removing ozone-rich air (Abalos et al., 2013). Hence, negative ozone anomalies correspond to positive N₂O anomalies.

18. *L300: During boreal winter, there should in general occur both positive and negative anomalies, so do you want to stress here that anomalies are generally stronger in boreal winter because transport is strongest, and thus most effectively leads to anomalies? Please clarify and re-phrase.*

Yes, that is what we mean. We rephrased that part of the text to improve clarity:

Such transport-driven anomalies tend to be stronger during boreal winter, when stratospheric transport is strongest (Birner and Bönisch, 2011).

19. *L313: If AO/NAO was the circulation pattern associated with the ozone anomaly, there still has to be a stratospheric transport anomaly that causes the ozone anomaly. And given you don't see an N₂O anomaly, this was of different nature compared to the other transport anomalies? Can you comment on this?*

Done. See the reply to the similar comment above.

20. *L323: Rather: "The combination of co-occurring strong N₂O and ozone anomalies ..." are the evidence for transport as driver, rather than each tracer/Figure on its own, correct?*

Yes, we have rephrased the sentence to reflect this:

The combination of co-occurring strong N₂O and ozone anomalies (Figs 2 and 3) provides ...

21. *L325: really? How about the photochemistry, that dissociates N₂O? Rather "in the lower stratosphere" ?*

The N₂O photochemistry occurs mostly in the tropical upper stratosphere (above 15 hPa) with no significant impact in the mid-latitudes compared to the other terms of the N₂O TEM budget (see Figs. S1-S4 of the Supplement of M2020, the $P - L$ variable). The sentence specifies "...significant..." and "...at mid-latitudes", hence we believe that it clearly states our point of view. However, in order to be more specific, we add the reference to the Supplement of M2020:

... N₂O has no significant chemical reactions in the whole stratosphere at mid-latitudes (see Supplement of M2020).

22. *L331-332: could simplify: "i.e., meridional advection of N₂O."*

We use this elaborated phrasing only in this part of the text to remind the reader the physical meaning of the A_y term and its relationship with N₂O concentrations. For this reason, we would like to keep this description of A_y .

23. *L337-338: can delete this*

We removed the parentheses to make the text more natural and easy to follow: **Hence, positive (negative) A_y anomalies, on top of the positive climatological A_y , indicate ...**

24. *Figure 4 caption: typo*

Corrected.

25. L358: *As stated in line 357, biases in the mean BDC do not necessarily reflect in the strength of anomalies in a particular year. So I'm not sure this discussion on the difference in the anomaly strength in different products is very meaningful.*

See the reply to the relative Specific Comment.

26. L365: *For making the point on the relation of anomalous transport contributions versus the actual anomaly in a more quantitative way, it would be useful to produce e.g. scatter plots of A_y (and/or $A_y + A_z$) in Jan 2019 versus the N_2O anomaly in Feb 2019. As is now, this discussion appears a bit selective and qualitative.*

See the reply to the relative Specific Comment.

27. L385: *I'd strongly suggest to show the sum of EPFD_z and EPFD_y. If the latter has no strong anomalies, the former will dominate also in the sum, but that way you make sure you capture the full forcing.*

Done, for both Figures 6 and 9 of the revised manuscript.

28. L386-388: *suggest to formulate more concise: "convergence of EPFD_z (i.e., ...), so that positive anomalies indicate enhanced wave forcing of the poleward circulation"*

Also, strictly speaking your analysis does not reveal whether the forcing is due to planetary waves, or also smaller scale waves. I agree from all we know, the planetary waves will be dominant, but still you might want to be more careful in the formulation.

Done for both aspects: the description of the EPFD was shortened and we modified the planetary waves part to be more generic:

In this Section, we show the convergence of EPFD (i.e., -EPFD), because it represents the forcing driven by the breaking of resolved waves (generally planetary scale waves, Holton, 2004), so that positive anomalies indicate enhanced forcing from the wave breaking.

29. L392: *I find those differences in WACCM difficult to see from the Figure (mostly lines are on top of each other, and differences between data sets are difficult to see). Also, I think those differences appear to be minor, so not necessarily worth discussing, so I suggest to drop this paragraph, also because the fact that the free-running WACCM versus the SD-version has different planetary wave forcing is rather obvious.*

See the reply to the relative Specific Comment.

30. L409: *Again, I don't think this discussion of inter-model differences in the exact strength of the standardized anomalies in the EPFD between reanalysis products is very enlightening here. It is well known that there are differences between reanalysis products (can refer to previous S-RIP work), and the main message here is that all products show strong anomalies in the period of interest. Therefore, I suggest to drop this entire paragraph.*

With the inclusion of the discussion of the 2019 SSW impact, we expand this paragraph in the revised manuscript to include such discussion. We also decided to keep but significantly shorten the discussion about the differences between the meteorological reanalyses related to their vertical resolution. See L386-405 of the revised manuscript.

31. L429: 'over', remove

Done.

32. *L430: suggest to condense this paragraph; just saying that local strong EPFD does not correspond directly to meridional advection, as for example QBO related anomalies might be superimposed, and that this will be discussed in the next sections.*

This part was significantly rephrased to better investigate the differences and similarities between January 2019 and January 2021. We discuss the differences in the QBO conditions and SSW occurrences in 2019 and 2021, and we relate those (intertwined) aspects to the similarities in the EPFD anomalies and to the differences in the A_y anomalies between the two periods. In the following Sections 4.X, we focus on the 2019 anomalies and we do not show anomalies for 2021. Hence, we believe that the current Sections 3.X are the best place to investigate these differences between different years. See track changes for more details.

33. *L451: Since those are anomalies, does this imply that you need anomalously strong upwelling to produce the anomalies? It seems to be that this sentence is inferring from climatological tendencies (which to my understanding are shown in M2020) on anomalies? A solution could be to add the word "likely" here, as this is not explicitly shown.*

Done, we added "likely".

34. *L461: As before, a very long paragraph to essentially say that models agree overall very well. Please condense.*

Done, the paragraph is substantially reduced in the revised manuscript, see track changes.

35. *L474: shown*

Done.

36. *L481: are relatively weaker ?*

Done.

37. *L511-512: IS this sentence still talking about the positive anomalies close to the Equator? If so, isn't the vertical advection term the dominating one here? (see also earlier comment)*

Yes, the sentence refers to the positive N_2O anomalies around the Equator above 20 hPa. Indeed, the vertical advection term is the largest in this region (M2020). Hence, we replaced "leading" with "contributing", see revised text:

The positive A_y anomalies indicate then that the meridional advection of N_2O is enhanced over that region (as the negative $\bar{v}^*$ anomalies start to become positive), ultimately contributing to positive N_2O anomalies (see Fig. 7).

38. *L518: In $\rightarrow$ in*

Done.

39. *L519-520: Given the important role of the secondary circulation in the discussion, it would be great to show the mean residual circulation anomaly of the month of interest (e.g. the residual streamfunction anomaly).*

Done, see the reply to the relative Specific Comment.

40. L542-543: *see earlier comment: I sugest to show the full EPFD, as there are certainly regions where the meridional flux convergence contributes. And if it doesn't, there is no harm in showing the full convergence.*

Done, see the reply to the relative Specific Comment.

41. L544: *see earlier comment; in principle all types of waves.*

We replaced "planetary" with "**... resolved (generally planetary) ...**"

42. L567-572: *As before, I don't think this discussion is necessary. In particular as it trys to infer from climatological EPFD strength to the anomalies, which are not necessarily related.*

We removed the discussions relating the EPFD anomalies in ERA5, ERAI and JRA3Q with the studies on their climatologies.

43. Figure 8: *I would find it helpful to overlay the mean zonal winds as in Fig. 6.*

Done for Figs. 7, 8 and 9 of the revised manuscript (see Figure captions for details).

44. L599: *this is a strong statement, there yould be other contributing factos; how about "liekly contributed to the ..."*

The relative Section (Sect. 5) was removed from the revised manuscript after a strong suggestion from reviewer 1. This part about the northward displacement of the zero wind line was moved in the previous section and connected to the QBO phase at 50 hPa.

Around 30°N, the E-QBO phase at 50 hPa leads to a shift of the zero zonal wind line towards the winter subtropics, reaching approximately 15°N in January 2019. This displacement maintains the wave breaking at higher-latitudes (Anstey et al., 2022), and increases the wave forcing (i.e., negative EPFD anomalies) in the subtropics and mid-latitudes in the mid-lower stratosphere. Through this enhanced wave forcing, the northward shift of the zero zonal wind line influences the seasonality of the secondary circulation, extending it towards the mid-latitudes. This behaviour is evident from the positive $\bar{v}^*$ anomalies in the same region.

45. L608-609: *Why don't you show v^* directly? This would be much more consistent.*

See the reply to the relative Specific Comment.

46. L636: *rather "the CTM driven by reanalysis... shows similar results..."?*

We rephrased the sentence as "**.... the BASCOE CTM driven by the meteorological reanalyses tends to show very similar results....**"

47. L638: *why still?*

It was a trailing word from a previous version - it is removed in the revised manuscript.

48. L639-640: *to make this statement, the comparison needs to be more quantitative; for me this is not immediatly apparent from the Figures.*

Here, we refer mostly to Fig. 2 (and marginally to Fig. 5 of the revised manuscript). We added "**Concerning the amplitude of the N₂O anomalies in the lower stratosphere, ...**" to highlight this.

49. *L651-652: I agree this might be an important contribution, but likewise will the SSW in January 2019 have played a large role - see main comment.*

We significantly rephrased this paragraph to highlight the importance of the 2019 SSW as well. See also the reply to the relative Major Comment 1 and L626-635 of the revised manuscript.

50. *L653-654: This needs to be shown mre explicitly in my opinion; see main comments.*

We show now $\bar{v}^*$ anomalies in Figs. 8 and 9 of the revised manuscript, which help the interpretation of the results and the connection with the N_2O and A_y anomalies. See also the reply to the relative Major Comment 3.

51. *L669-697: All material from here to the end reads like a nice motivation for stratospheric science in general, but I think goes far beyond "Summary and Conclusion" related to the current manuscript. Please condense significantly.*

This part is largely reduced in the revised manuscript - see also the reply to the relative Specific Comment.

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