Reply to Editor

Dear Petr Šácha,

Thank you for your feedback on our manuscript. We were pleased to hear that you consider only minor revisions to be necessary, which we hope we have now implemented to your approval. In the following, we address all comments in further detail (Editor's comments in italics, quotations of the corresponding revised text passages in blue. The line numbering of the editor's comments follow the previous tracked changes version, the replies relate to the current tracked changes version.)

Editor's comments

Concerning the discussion phase, I see only one remaining complicated point - the comment from Dr. Añel about his long lasting, but unpublished contributions to the field. I have to make clear at this point that I am myself in a conflict of interest here, because I presented one of the presentations on behalf of the authors. Añel, J. A., Gettelman, A., Castanheira, J. M., de la Torre, L. (2018) Tropical widening from isentropic and potential vorticity fields, SPARC OCTAV-UTLS Workshop. 7 - 9 November 2018, Mainz (Germany). I am sorry for not realizing my potential conflict of interest before. Hence, concerning this point I refrain from making any editorial recommendations. My opinion is that this point could possibly be solved by acknowledging the discussions with Dr. Añel. I am sure that together with your co-authors you will find a balanced approach to this sensitive topic.

As already stated in the reply to the comments of Dr. Añel, we acknowledge his contributions to the topic of tropical widening. However, we would still like to point out that the work in question is difficult to reference or find for the interested reader and refer again to the ACP reference guidelines (`https://www.atmospheric-chemistry-and-physics.net/submission.html#references`). We hope to have found a compromise with the following reference:

L386-387: Our metric and findings are similar to other metrics based on cross-points between isentropic and potential vorticity fields (J. Añel, personal communication).

Editorial comments

1) At numerous places of the manuscript, the not shown statement is invoked (L238, L441) or the results are simply not shown (around L246 - sensitivity experiments with the DLM sample size, around L461 - LMS mass following Appenzeller et al). At all instances, those results are important for the study and should be shown at least in the Appendix.

We had refrained from presenting the corresponding results in order to avoid an extensive appendix. However, we acknowledge that the results can be important for the study. Regarding L238 (now L224, trend results with and without regressors): We have extended Fig. 2 so that it now shows the ERA5 tropopause trends with (color) and without regressors (grey). Moreover, the LMS mass trends across the reanalyses for the DLM run without regressors can be found in the appendix (Fig. A2). These examples show that the use of regressors has no strong effect on the trend results, as stated in the text:

L223-224: Overall, the results showed no strong dependency on the used regressors. However, tropical tropopause pressure trends, for example, become more significant when regressors are used (Figs. 2, A2).

Regarding 441 (now L416-418, location of boundary surfaces across reanalyses): We have added a figure illustrating the differences in the location between the individual LMS boundary surfaces in the reanalyses (A6). Also, we have refined the corresponding statement in the text:

L416-420: The MERRA-2 LMS mass deviates the most from the other reanalyses, generally showing smaller values but the relative differences between the data sets are smaller than $10\,\%$ in the NH and smaller than $20\,\%$ in the SH. The variations of LMS mass across the reanalyses can be primarily attributed to the differences in tropopause location, determining the lower LMS boundary as well as the dynamical upper boundaries (Fig. A6).

Regarding L246 (now L232-234, DLM sample number): We have added an example comparing DLM trend results for different sample numbers in the appendix (Fig. A1).

L232-234: Sensitivity experiments conducted as part of this study have shown that increasing the number of DLM samples (to e.g. 10 000) does not significantly improve the results, but comes at a considerable computational cost (see for example Fig. A1).

Regarding L461 (now L437-438, LMS mass similar to Appenzeller et al. (1996)): We considered adding the LMS mass values calculated for LMS boundaries similar to Appenzeller et al. (1996), to Tab. 2. However, as mentioned in L437-438, we shifted the ERA5 $2\,\mathrm{PVU}$ iso-surface by $60\,\mathrm{hPa}$ to approximately match the UKMO $2\,\mathrm{PVU}$ surface illustrated in Appenzeller et al. (1996) Fig. 3. This is intended as proof of concept but does not represent a physical LMS boundary surface. Neither did we use the same data asAppenzeller et al. (1996). Therefore, we decided not to include the respective LMS mass in Tab. 2.

2) Structure of the paper:

2)A) Fig. 13 is absolutely misplaced.

We recognize that the placement of Fig. 13 was not ideal. We have moved the figure (Fig. 13, now Fig. 4) up to Sect. 3.1. where it is first mentioned.

B) Many subplots are not mentioned in the text (Fig. 3b, d; Fig. 4b, c) and some figures are only introduced in the paper and not discussed afterwards (Fig. 7). I recommend you to either expand the description of the results or move the related figures (or subplots) to the supplement (Appendix)?

Regarding Fig. 3: We adjusted the referencing in the text and the subplots of Fig. 3 are now specifically addressed in the text:

L257-258: All considered reanalyses agree on negative pressure trends at the tropical lapse rate tropopause and the cold point (Fig. 3a and b), whereas potential temperature trends differ in sign (Fig. 3c and d).

Regarding Fig. 4 (now Fig. 5): The subplots Fig. 4b and c (now Fig 4 and A3) were shown for the sake of completeness. We realize that the additional subplots, which are not discussed in the text, can be moved to the appendix.

L276-277: The DLM trend state of $2\,\mathrm{PVU}$ and cold point pressure can be found in the appendix (Fig. A3).

Regarding Fig. 7 (now Fig. 8): This figure is intended to provide an overview of the location of the boundary surfaces. Unlike Fig. 1, Fig 7 (now Fig. 8) it is not a schematic, but illustrates the zonal mean, temporal mean pressure of the LMS boundaries as used in this study for the concrete example of ERA5. Another purpose of the figure is to justify the definition of the lateral LMS boundary. We realize that some references to the figure, especially corresponding to the latter topic, were lacking. They have now been added.

L335-336: The location of the different boundary surfaces used for LMS definition are displayed in Fig. 8 using ERA5 as an example.

L366-368: Specifically, the lapse rate tropopause and the $2\,\mathrm{PVU}$ surface at $350\,\mathrm{K}$ are close to the isentropic PV-gradient tropopause, which marks a clear transport barrier between the tropical troposphere and the extratropical lower stratosphere, i.e. the LMS (Kunz et al., 2011; Turhal et al., 2024).

L407-409: Since the $2\,\mathrm{PVU}$ tropopause is located at significantly higher pressure than the lapse rate tropopause (fig. 8), the LMS mass between $2\,\mathrm{PVU}$ and the respective upper boundary is 1.6-1.8 times larger than the mass with respect to the thermal tropopause.

C) L374-L394 In this part of the paper you keep jumping between Figs. 8 and 9 and also Fig. 13 is suddenly invoked. Can you rewrite the text or reorganize the figures for a better readability?

We realized that the presentation of Figs. 8 and 9 (now 9 and 10) did not match the corresponding text. In the text, the relationship between trends of the upper LMS boundaries and the potential temperature at the tropical tropopause are first discussed, using the example of ERA5. Afterwards, the reanalyses are compared. The figures have been reorganized in a consistent manner (Figs. 9 and 10). The text has been adapted accordingly and refined in some places. Note the new references in L338-345 and:

L349-357: Like in ERA5, a significant rise of the PPT10mean surface in the extratropics is also evident in ERA-I and MERRA2 (Fig. 10b). This can be attributed the increase of tropical tropopause potential temperatures, which define the isentropes corresponding to PPT10mean (Fig. 10d). JRA-55 and JRA3Q on, the other hand, suggest the opposite (Fig. 10b and d). At the cold point, ERA5 and ERA-Interim agree on continuously increasing potential temperatures between 1998–2019, whereas the JRA data sets show mostly decreasing potential temperatures and MERRA-2 almost no trend at all (Fig. 10e). This is reflected in the trends of the PPTcp10mean surface (Fig. 10c). The contrasting behavior of the reanalyses can at least partly be attributed to the respective temperature trends in the TTL region.

3) Lifting of the isobars, isentropes and the tropopause.
I think that you should approach the issue of different vertical shifts either more rigorously (e.g. analytically using the transformation of coordinates as eq. 1 in Šácha et al., 2019, ACP - note the corrigendum here) or maybe a visualization of different vertical shift rates of the tropopause and surrounding isobars and isentropes (e.g. similar to Fig. 4 in Šácha et al., 2019, ACP) would help the reader to orient in this field.

The LMS mass trends are supposed to be the core of this study. As the LMS mass is defined by the pressure difference between the upper and lower boundaries, we set focus on pressure trends, i.e. pressure as the vertical axis. Changes of the location of the tropopause and isentropes are presented to improve the understanding of the observed LMS mass trends and to illustrate the role of the respective LMS boundaries. For context, we decided to include geopotential height trends of the tropopause (Fig. 2) and isentropes in ERA5 (A4). However, we do not aim to rigorously study the relationship between geometric height and pressure at this point. We realized that at some places in the manuscript, arguments referring to pressure and height reference systems had still been mixed. We have revised the wording in the relevant places:

L407-412: Since the $2\,\mathrm{PVU}$ tropopause is located at significantly higher pressure than the lapse rate tropopause (Fig. 8), the LMS mass between $2\,\mathrm{PVU}$ and the respective upper boundary is 1.6–1.8 times larger than the mass with respect to the thermal tropopause. The LMS mass with respect to the different upper boundary surfaces varies by less than $15\,\%$ for the same lower boundary and hemisphere.

L536-539: In general, the vertical boundaries dominate LMS mass changes. However, tropical width changes can have a considerable impact on LMS mass due to the area decrease from low to high latitudes.

Technical comments

L8-9: The sentence "This is consistent with a strengthening of the Brewer-Dobson circulation." should be deleted from the abstract, because the connection is not clear at this point.

We have deleted the sentence from the abstract.

L40-41 If you are aware of any paper documenting the seasonal cycle in stratospheric mass, you should reference it here. Otherwise do not make statements about stratospheric mass.

Appenzeller et al. (1996), as cited in L39 (previously L41), present the mass of the lowermost stratosphere as well as total stratospheric mass, including the seasonal cycle of both. Estimated from Figs. 5a and c in Appenzeller et al. (1996), the proportion of LMS mass to total stratospheric mass is roughly 30–$50\,\%$. From our own calculations, we estimated the contribution of LMS mass at ca. $20\,\%$ of the total stratospheric mass. This number was derived from approximating the total stratospheric mass between the tropopause and $1\,\mathrm{hPa}$. We adjusted the wording as follows:

L37-38: From Fig. 5 in Appenzeller et al. (1996), the LMS mass can be estimated to about 30–$50\,\%$ of the total stratospheric mass.

L413-414: If the upper edge of the stratosphere is approximated at $1\,\mathrm{hPa}$, the proportion of LMS mass to total stratospheric mass is about $20\,\%$.

L541-543: LMS mass changes are important to consider because the LMS mass makes up a considerable amount of the stratospheric column, in our case approximately $20\,\%$, and therefore contains a substantial fraction of, for example, column ozone.

L74.. despite the warming of the stratosphere..please rephrase to make clear that contrasting effects can be anticipated from the ozone recovery and GHG cooling in the stratosphere.

We hope the amended wording makes the contrasting effects clearer:

L69-73: [...] increasing greenhouse gas emissions and the resulting warming of the troposphere and cooling of the stratosphere. The tropospheric warming was found to cause a persistent lifting of the tropopause, even as contrasting temperature effects are expected in the stratosphere, where ozone recovery is causing warming while increasing GHG load is exerting a cooling influence (Pisoft et al., 2021; Meng et al., 2021).

L144-145 ..For this study, ERA5 monthly mean data for the time period 1979–2019 has been used (Hersbach et al., 2020), 1979 marking the beginning of the satellite era...This sentence should be deleted because this information is already covered by the preceding paragraph in the revised version.

The first paragraph in section 2.1 very briefly introduces the data used, listing the different reanalysis data sets. The choice of time period and monthly mean data is only mentioned in the second paragraph. Therefore, we think the mentioned sentence is important. We have changed the sentence slightly in the hope of achieving a better reading flow:

L137-138. We use ERA5 monthly mean data for the time period 1979–2019 (Hersbach et al., 2020), 1979 marking the beginning of the satellite era.

Eq.1 - This formula originally from Appenzeller et al. (1996) does not take into account the lateral boundaries? I suspect that for your case the bounds of integration should be modified.

Thank you for pointing this out. The variable lateral boundaries are indeed important to be included in Eq. 1:

L205-208:

$$M(t) = \int_0^{2\pi} \int_{\Phi_1(\lambda,t)}^{\Phi_2(\lambda,t)} \int_{p_1(\lambda,\Phi,t)}^{p_2(\lambda,\Phi,t)} -\frac{1}{g} dp \cos\Phi d\Phi d\lambda, \tag{1}$$

g is the gravity constant, λ is the longitude, Φ is the latitude and dp is the pressure difference. $\Phi_1(\lambda,t)$ and $\Phi_2(\lambda,t)$ denote the lateral boundaries, defined by the intersection between the tropopause and the 350 K isentrope for every longitude and time step.

L275-276 - In Fig. 3, it seems to me that for cold point the reanalyses differ in both pressure and potential temperature trend?

The magnitude of the cold point trends differs across the reanalyses but the sign is the same. The latter is what we want to emphasize here. We have changed the wording, hoping to make the statement clearer:

L257-258: All considered reanalyses agree on negative pressure trends at the tropical lapse rate tropopause and the cold point (Fig. 3a and b), whereas potential temperature trends differ in sign (Fig. 3c and d).

Figs. 4 and 6 and the corresponding discussion in the text: You write about negative trends but the color scale starts at zero?

Figs. 4 and 6, now Figs. 5 and 7, illustrate the non-linearity of DLM trends. As stated in the figure captions, Fig. 5 (7) shows the DLM trend state time series, i.e. the hidden mean state of tropopause (isentropic) pressure relative to the minimum at every latitude. The point in time at which the trend state at one latitude reaches its minimum is indicated by a black dot. The pressure trend state is higher before reaching the minimum, i.e. decreasing. Accordingly, the pressure trend state is increasing after having reached the minimum. We have amended the wording to make the interpretation of Figs. 5 and 7 clearer:

L272-276: The non-linear DLM trend analysis for the entire time series 1979–2019 reveals a trend reversal of the ERA5 lapse rate tropopause pressure around the year 2000. This trend reversal is evidenced by the fact that the DLM pressure trend state reaches its minimum around the year 2000 (Fig. 5). Accordingly, the DLM results suggest decreasing pressure of the ERA5 SH mid-latitude lapse rate tropopause before the year 2000 and increasing pressure thereafter.

L311-312: This trend reversal is evident from the minimum in the DLM trend state (Fig. 7).

Fig. 5 and the corresponding discussion around L315 - Except maybe in the tropics the trends are very uncertain, but the discussion does not reflect this. You state this only at L319. But, the notion of significance should be the first information the reader gets.

We agree that the uncertainty of the trends should be mentioned along with their magnitude. We have added the missing information:

L290-292: The average pressure trends range between -0.3 to $+0.6\,\mathrm{hPa/decade}$, which corresponds to an absolute pressure change of up to $1\,\mathrm{hPa}$ during the 21 year period, albeit exhibiting large uncertainties, especially in the extratropics.

L318 - "...accompanied by a rise of the respective potential temperature.." - you should specify what is the vertical coordinate relative to which the rise is diagnosed (height, pressure. . .)

We have specified the vertical coordinates in the mentioned paragraph:

L293-296: [...] which is accompanied by a rise of the respective potential temperature iso-surfaces in geopotential height (Fig. A4a). The isentropic pressure trends [...]. However, while the $380\,\mathrm{K}$ isentrope in ERA5 and MERRA-2 shows no pressure trend in the tropics and subtropics [...].

L320-L323 - I like very much your discussion on the possible connection of the pressure of isentrops with BDC. Don't you want to devote more space in the manuscript to this?
and
Paragraph starting with L324 on isentropic pressure trend - Here again, BDC strength (tropical upwelling) can be a part of the story?

The discussion regarding the relationship between isentropic pressure trends and BDC changes is taken up again and deepened in lines L320-325 (previously L344-349). We recognize that a reference to the further discussion of the topic in the following paragraph (considering the non-linear DLM trends) can be helpful for the reader. We have added such a reference, linking the subsequent paragraphs:

L305-306: While the average pressure changes discussed in this paragraph provide valuable insights, examining the full, non-linear DLM trend state time series can offer a more comprehensive view of the physical mechanisms driving trend evolution. (L309-310 are therefore no longer necessary and have been deleted.)

Discussion around L340 - Negative trends can not be seen in Fig. 6. Moreover, why do you expect this kind of mismatch between the rise of isobars and isentropes for diabatic heating? Can you give some *reasoning (ideally analytically)?*

As explained above, Figs. 5 and 7 (previously 4 and 6) illustrate the non-linear DLM trend states. The color shading shows the pressure difference with respect to the respective minimum for every latitude bin and every time step. The minima are indicated by the black dots. At every latitude, pressure is decreasing until reaching the minimum and increasing thereafter. This way, Fig. 7, reveals continuously negative isentropic pressure trends in the tropics and extratropics above $380\,\mathrm{K}$.
The relationship between isentropic pressure and temperature simply follows from the definition of potential temperature: $\Theta = T * (\frac{p_0}{p})^{\kappa}$. An isentrope, i.e. $\Theta = const.$, is shifted to higher pressure if the temperature increases.

L315-316: This relationship between lower stratospheric temperatures and isentropic pressure directly follows from the definition of potential temperature.

L346-347 "Diabatic cooling of the tropical lower stratosphere results from increasing GHG load in the troposphere and a continuous decline of lower stratospheric ozone." - Stratospheric cooling is also a function of stratospheric GHGs. See the paper of Vallis et al. (2015, QJRMS) for the physical reasoning. But, it is hard to extrapolate this general knowledge to the tropical LS trends in particular.

Thank you for the reference. We agree that beside tropospheric GHG load and stratospheric ozone concentrations, (lower) stratospheric temperature changes also depend on stratospheric GHG concentrations. Accordingly, we have changed the wording from "GHG load in the troposphere" to „GHG load in the atmosphere" and added references to the mentioned study by Vallis et al. (2015) as well as an earlier study by Ramaswamy et al. (2001). Furthermore, we agree that these are general relationships and not specific to the tropics. We have therefore relativised the statement, using the wording "can be expected" instead of "result".

L322-325: [...] diabatic cooling of the tropical lower stratosphere can be expected from increasing GHG load in the atmosphere and a continuous decline of lower stratospheric ozone (e.g., Ramaswamy et al., 2001; Vallis et al., 2015). Such a cooling of the lower stratosphere [...].

P18 - I do not think that the footnote is neccessary.

We have removed the footnote.

L395 I miss here some provisional wrap-up of the results that will inform the following analysis. Like based on this and this, we choose the following two upper boundary definitions..etc. Sentences like this would enhance the readability of the text.

We agree that the readability benefits from a brief conclusion of the preceding paragraph (dealing with the upper LMS boundaries), which we have now added. Furthermore, we have added a short outlook on the mass trend, to make the context clear.

L360-362: For the LMS mass analysis in Sect. 3.3.2 and 3.3.3, we use $380\,\mathrm{K}$, PPT10mean and PTcp10mean as upper LMS boundaries and compare their respective effects on the mass. The different trends of the iso-surfaces discussed here are reflected in the LMS mass trends.

Starting at L418 - Here you forget to mention the behavior of ERAI, MERRA2 and partly JRA55, which is very different, and possible causes of this (different handling of O3 ?)

We agree that the mentioned discussion was a bit vague. We now refer more clearly to the differences across the reanalyses. However, we refrain from speculating on the reasons for the differences across the reanalyses, as we do in the rest of the manuscript. The objective of our study is to examine the physical changes in the LMS. The reanalysis comparison is a useful tool for assessing the robustness of our findings and highlighting differences across the reanalyses. It is, however, not within the scope of our study to elucidate the (technical) reasons for discrepancies between the reanalyses. In the conclusions, we refer to

Fujiwara et al. (2022) and Fujiwara et al. (2024) (L529-532).

L388-395: In the SH, on the other hand, the lateral tendencies of the intersections between tropopause and isentropes are less clear. For the lapse rate tropopause at $350\,\mathrm{K}$, all reanalyses except JRA3Q agree on an equatorward trend, however differing in magnitude and statistical significance. In contrast to the lapse rate tropopause, the $2\,\mathrm{PVU}$ dynamical tropopause in ERA5 shows a poleward trend at $350\,\mathrm{K}$ (Fig. A5a). The general shape of the lateral tropopause trends with respect to potential temperature, however, is similar in most reanalyses [...].

L430-431 - Maybe I missed it in the preceding text, but why do you choose in the end these two combinations of the LMS boundaries?

In fact, we use the combinations lrtp-380K, lrtp-PPT10mean and lrtp-PPTcp10mean for all reanalyses. For ERA5, we use all combinations 2PVU-* in addition. Here, the combinations lrtp-PPT10mean and 2PVU-PPTcp10mean are mentioned, because these result in the minimum and maximum average LMS mass. We tried to make this more clear in the text.

L404-407: The mean LMS mass in ERA5 between all considered boundary surfaces on both hemispheres is listed in Tab. 2. In ERA5, the average global LMS mass between the different boundary surfaces (lrtp, $2\,\mathrm{PVU}$ and $380\,\mathrm{K}$, PPT10mean, PPTcp10mean) ranges from $0.99 \pm 0.28 \cdot 10^{17}$ kg (resulting from lrtp-PPT10mean) to $1.89 \pm 0.37 \cdot 10^{17}$ kg (resulting from $2\,\mathrm{PVU}$-PPTcp10mean) within the time period 1979–2019.

L436 "The mean LMS mass in ERA5 between all considered boundary surfaces on both hemispheres is listed in Tab. 2." - I recommend this to be the starting sentence of the section 3.3.2

Thank you for the suggestion. Section 3.3.2 now begins with the mentioned sentence.

L461 - I suggest adding the LMS mass following Appenzeller to Table 2.

See comment above. We refrained from adding the mentioned mass to Tab. 2 because it was calculated as proof of concept and does not represent an actual result.

L469 For the ERA 5-> Only for ERA5 ...

We have changed the wording to:

L444: For the LMS in ERA5 bounded with $380\,\mathrm{K}$ [...].

Around L475 - The contributions of the mechanism 1)-3) behind the LMS mass changes can be quantified by differentiating the eq. 1 in a similar way as Šácha et al. (2024, GRL) differentiates the definition of the net tropical upwelling.

We agree that the discussion of the LMS mass trends benefits from a quantification of the contributions from the different boundary surfaces. In order to asses these contributions, we conducted sensitivity studies, allowing only one boundary surface to evolve in time while the other two had been fixed. More specific, for one boundary surface, we used the same 4D pressure filed as before, while we repeated the first year of the time series (1979) for the other two. We present the results of this sensitivity study in Fig. 13 to confirm the statements in L449-453. We realized, that these statements needed some refinement and changed their order.

L449-458: This suggests three things: 1) The decreasing NH LMS mass for a fixed $380\,\mathrm{K}$ boundary is largely due to the rising NH extratropical tropopause. 2) The "dynamical" upper boundary surfaces based on the tropical tropopause are indeed able to largely compensate for the tropopause rise in ERA5. 3) Locally, a considerable proportion of this NH LMS mass decline can be attributed to the poleward trend of the lateral boundary, which is the result of an expansion of the tropics, while the hemispheric effect is rather small.

Fig. 13 confirms these observations and helps to quantify the contributions of the respective boundary surfaces to the hemispheric LMS mass changes. The figure compares LMS mass changes for an LMS where all boundary surfaces are evolving in time (as before, Fig. 12) with an LMS with only one boundary surface evolving in time ("floating"), while the other two are fixed (here, the first year of the time series is repeated).

L473-475: [...] highlights the impact of the upper boundary surface, which is confirmed again in Fig. 13b.

L494 - "In the SH, LMS mass trends differ more strongly between the reanalyses than in the NH (Fig. 12)..." - I do not see by eye that the differences between reanalyses are greater in SH than in NH.

We recognize that Fig. 12 (now Fig. 14) can be improved in order to highlight the mentioned differences. Furthermore, we specified that we are especially referring to the mean trends and the LMS mass between lrtp-380K and lrtp-PPT10mean.

L476-477: In the SH, the mean LMS mass trends differ more strongly between the reanalyses than in the NH (Fig. 14g–l), especially for the LMS bounded by lrtp-$380\,\mathrm{K}$ and lrtp-PPT10mean.

L536 "...suggests a steepening of the tropopause break." - If this feature is that important to be highlighted in the conclusions, what prevents you to analyze it in the paper instead of only speculating about its role?

We realize that our wording may not have been sufficiently precise here. The lateral tropopause trends with respect to potential temperature, as presented and discussed in Sect. 3.3.1 suggest a steepening of the subtropical tropopause, not the tropopause break in particular. We have changed the wording accordingly. In general, we believe that this result is worth mentioning in the conclusions.

L395-396: The different magnitudes of the lateral trends for tropopause intersections with different isentropes suggests a shape change of the subtropical tropopause.

L398-400: Furthermore, such a steepening of the subtropical tropopause, including the tropopause break, could be an indication of a strengthened BDC (Birner, 2010b).

L513-518: The different magnitude of the lateral trends with respect to the potential temperature on both hemispheres suggests a steepening of the subtropical tropopause. [...] Such a steepening of the subtropical tropopause, including the tropopause break, can be associated with an intensification of the SH subtropical jet (Manney and Hegglin, 2018; Maher et al., 2020) and could be linked to a strengthening of the BDC (Birner, 2010b).

L550-L551 - Hints can be found in Fujiwara et al. (2022). - Also, in Fujiwara et al. (2024, ACP) it can be seen that JRA55 differs from other reanalyses in terms of SW and LW heating in the upper troposphere in DJF and JJA.

Thank you for the reference to the study of Fujiwara et al. (2024), which we have now included. However, we want to mention again that it is beyond the scope of our study to assess the reasons for the differences across the reanalyses in more depth.

L529-531: Hints can be found in Fujiwara et al. (2022) and Fujiwara et al. (2024), suggesting relationships between differences in tropical lower stratospheric temperature and radiative heating, related to ozone concentrations, among other factors.

L840 The references Škerlak and Šácha should be placed after S and before T.

We have fixed this error.

References

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