Reply to Reviewer 2

We appreciate the reviewer's interest in our study. However, we would like to clarify some apparent misunderstandings: The LMS mass is calculated based on the formalism of Appenzeller et al. (1996). However, our study does not address mass transport between the tropics and subtropics, nor does it utilize Transformed Eulerian Mean equations. Our study presents the mass of the lowermost stratosphere (LMS) and associated trends. It is correct that we define and compare different boundary surfaces enclosing the LMS.

In the following, we address all comments in detail (Reviewer's comments in italic, quotations of the corresponding revised text passages in blue).

Based on the suggestions of all reviewers, we have repeated our analyses of LMS structural changes with four modern reanalyses, namely ERA-Interim, MERRA-2, JRA-55 and JRA3Q, in addition to ERA5. The structure of the paper has essentially remained the same, although the individual sections have been expanded to include a comparison of the results between the reanalyses. We have also adapted the title accordingly: "Long-term changes in the thermodynamic structure of the lowermost stratosphere inferred from reanalysis data"

Major comments

Overall, the manuscript reads well and sounds reasonable, and I find it a nice work. However, a concern arises from the beginning, whether the authors have used here ERA5 or ERA5.1. It is now well known that ERA5 has a cold bias in the lowermost stratosphere; therefore, its data are not reliable for some time in the region of the atmosphere that is the focus here. To address this issue, ERA5.1 was produced. The authors may have used the corrected ERA5.1 data; however, they should make it explicit. If they have used ERA5, there is a chance that the results here are not entirely valid (as they are based upon data known to be erroneous), and they should update the study using ERA5.1. On the other hand, if they have used ERA5.1, this should be made explicit in the text.

Thank you for pointing out the cold bias in ERA5, rectified in ERA5.1. We agree that it is important to use ERA5.1 for our study of the lowermost stratosphere. All ERA5 analyses have been revised using ERA5.1 for the affected time period, i.e. 2000-2006. This revision has only led to minor alterations of the trend results.

As some of the authors know (they have approached us in conferences to talk about it), some colleagues and I have proposed similar metrics to the ones here used for the UTLS region since years ago (joint PV and potential temperature changes), to determine the transition from tropics to the extratropical region (where the LMS mass is computed here), and regularly updated them. Although not published in a paper, they have been widely presented in SPARC workshops. Examples are:

- *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)*
- *Añel, J. A., Gettelman, A., Castanheira, J. M. (2015) Tropical widening from isentropic and PV fields. SPARC Regional Workshop on the Role of the Stratosphere in Climate Variability and Prediction, 12-13 January 2015, Granada (Spain)*
- *Añel, J. A., Gettelman, A., Castanheira, J. M. (2009) Tropical broadening vs. tropopause rising. "The Extratropical UTLS" SPARC Workshop, 19-22 October 2009, Boulder (CO, USA)*

These metrics are based on the equivalent latitude of cross-points between isentropic lines and the PV field, so they are especially relevant for the discussion between lines 70 and 81 and the paragraphs after line 270 (which are the same as we have shown in the past) and Figure 9 (which is precisely the same kind of plot we have been presenting). Essentially, the results we have presented in the conferences have always matched those based on the tropopause break associated with the jet for all the reanalysis (ERA-Interim, NCEP2, JRA-55 and MERRA) and WACCM4 (the CCMI version with 66 vertical levels and in a configuration with 133 levels). It is comparable to the crossing between the 350 K isoline or 2 PVU and the thermal tropopause *used here. For the fairness and completeness of the discussion, it should be cited. The discussion in the Introduction in line 79, paragraphs 280-295, and the conclusions in line 367 are the right parts of the text to attribute the original idea and past results. Actually, I find it quite unfortunate that our works have not been cited in this submitted version. I have attached to this review one of our SPARC presentations to illustrate it.*

We did not mean to withhold any results and acknowledge your ideas and effort on the subject of tropical widening. Unfortunately, the mentioned talks are not easy to reference or to find for the interested reader. We therefore respectfully refrain from citing the mentioned work and instead refer to peer-reviewed literature, also referring to the ACP reference guidelines (`https://www.atmospheric-chemistry-and-physics.net/submission.html#references`).

Additionally, I have read the manuscript several times, and I find a gap (maybe biased by my own scientific interests) in all the exposition and discussion related to the changes in the structure of the UTLS: the changes in the structure of the tropopause itself. I agree with the authors that mass changes are the relevant variable and that they have a criterion to delimit the region where it is computed. Additionally, they mention the overlapping of the tropical tropopause over the extratropical as an issue. However, from the point of view of the lapse-rate definition, it is clear that the region studied here is changing, which is evident in the broader area of vertical stability, fingerprinted by an increase in the numbers of multiple tropopauses (e.g. Castanheira et al. 2009), correlated with increasing UTLS baroclinicity. I think it is relevant to mention this around lines 96 and 225 and, if possible, to add in the manuscript some discussion on how the metrics presented here could be related to the changes in the vertical stability and the widening of the tropopause region.

We agree that the increase of double tropopause frequency should be mentioned in our manuscript, as it can be related to changes in the thermodynamic structure of the UTLS. We have added according information, referring to the study by Castanheira et al. (2009).

L90-92: In addition to the general widening of the tropics, the frequency of double tropopause events, i.e. poleward excursions of the tropical above the extratropical tropopause, is found to have increased (Castanheira et al., 2009; Xian and Homeyer, 2019). This trend likely reflects an increase in baroclinicity in UTLS, driven by the GHG-induced climate change (Castanheira et al., 2009).

Finally, using different periods (since 2000 and 1980) sometimes makes the text confusing. I do not see the point of beginning in 2000 and then extending the analysis back to the 1980s. Does it provide some fundamental new insight here? I doubt it. The authors could think about simply removing the part pre-2000.

We take note of the experience that the discussion of different time periods can be confusing. Nevertheless, the main strength of the DLM (dynamic linear regression model) trend analysis is to infer potential trend reversal dates without prior specification. Even though our focus is on the time period after 1998, the non-linear DLM trends show interesting features also before this time. Especially the identification of trend reversal dates, as well as the specification of continuous trends, are helpful for interpreting the results after 1998. For example, considering both, the full time period 1979-2019 as well as changes after 1998, helps putting our results for the SH lapse rate tropopause pressure in context with trend studies focusing on different time periods.

Minor comments

Line 42: citing Hoerling et al. (1991) about the potential vorticity and the tropopause is right. However, the numbers given by Hoerling et al. limit the location of the tropopause to 1-3 PVU, which has been proven too restrictive. Hoinka et al. (1999) have a good discussion, showing that 3.5 PVU approaches extratropical tropopause better. I recommend adding a citation to Hoinka et al. so that those readers without a profound knowledge of the topic have a more comprehensive and updated view of the issue of using the PV criterion to "find" the tropopause.

In fact, Hoerling et al. (1991) compare tropopause definitions for PV threshold values between 1-5 PVU. Hoinka (1999) choose 3.5 PVU, referring to Hoerling et al. (1991).

Line 43: when discussing the chemical tracers, I think it is fair to add the e90 by Prather et al., e.g. (2011) https://doi.org/10.1029/2010JD014939

We acknowledge the existence of other tracers beside ozone, including e90, that allow for the definition of a chemical tropopause as shown by Prather et al. (2011). However, we aim to give a brief overview rather than a complete list of tropopause definitions in the mentioned text passage.

Lines 88-94: I find this paragraph explicative and well-referenced. The authors mention that the issue of the BDC trends is an ongoing discussion. They refer to models, satellite data, and reanalysis. First, I would clarify in the text that Tegtmeier et al. (2020) refer to reanalysis. Then, I recommend citing a more up-to-date study, recently published by Sacha et al. (2024), which shows consistent results from models and uncertainties from reanalysis (https://doi.org/10.1029/2023GL105919).

We appreciate the reference to the study by Šácha et al. (2024), which we have cited within the mentioned paragraph on BDC trends. Additionally, we have included the study by Zou et al. (2023), pointing out that the direction of tropical tropopause temperatures appears to have changed after 2005. Tegtmeier et al. (2020) report trends in the tropical tropopause layer for the period 1979-2005, comparing reanalysis data and observations.

L96-97: [...] According to Oberländer-Hayn et al. (2016) and Šácha et al. (2024), it is more precise to speak about a lifting of the circulation, which is connected to the tropopause expansion itself.
L105-110: [...] This is consistent with tropical lower stratospheric temperature trends close to zero within this period, inferred by Zou et al. (2023) from reanalyses data. The temperature reduction in the tropical tropopause region and at the cold point reported for the time period 1979–2005 by Tegtmeier et al. (2020) is consistent with increased tropical upwelling. However, different reanalyses often show a significant spread when compared, whether in terms of, e.g., temperature trends in the TTL region (e.g., Tegtmeier et al., 2020) or dynamical tropical upwelling (e.g., Šácha et al., 2024).

I would remove the explanation on the Bayesian basis of the DLM, the paragraph beginning in line 98.

We suppose this comment is related to line 168 of the original version of the manuscript. Its Bayesian nature distinguishes the DLM from other methods for time series analysis, e.g. multiple linear regression. We consider the short explanation on the DLM principles useful, especially for the understanding of the presented trend uncertainties.

Also, the explanation of the accessibility to the DLM model code is already included in the "Code and data availability" section. Regarding this, a minor issue: GitHub is not a suitable repository to store assets from scientific research or papers; GitHub states it on its webpage and offers an integration with Zenodo to store code that needs long-term archival, as the one used in papers, providing a DOI for it. I strongly recommend copying the DLM code in Zenodo and citing it instead of the GitHub repository.

Here, the goal was to show the source of the dlmmc which is available on github and can be referenced by Alsing (2019).

Also, instead of making the LMS code available upon request (which outcome is never assured), I recommend depositing it a permanent repository. ACP does not enforce this, but it is the usual practice in many other journals, including some of the EGU.

We agree that publication of our code and data makes it easier for the scientific community to benefit from our code and data and to compare methods and results. We have therefore made the code for LMS mass calculation and trend estimation available on zenodo, together with the different LMS boundary fields (i.e. lapse rate tropopause, PPT10mean, PPTcp10mean as well as the cold point) for all five reanalyses. See `https://zenodo.org/records/13890232`.

I think the colour scale for the DLM trend state in Fig. 3 should be improved. The discussion focuses on values lower than 7.5 hPa, and this is mostly yellow with independence of the values. It would be good if *the authors could provide a colour palette that helps to perceive the differences between 0 and 7.5 hPa.*

This is a valid point. We have improved the color scale.

Lines 235-236: I would delete this mention of the polar vortex. Overall, the link between ozone recovery and its thermal effect and the material barrier that the polar vortex represents to latitudinal mixing is well-known and clear. However, I do not think it is actually relevant to the discussion here and only introduces some confusion.

We think this is an interesting observation and always good to point out consistencies, also if relationships are well known.

In the Conclusions, I would emphasise the "model" dependence of the results shown here for the LMS changes. There are some disagreements with previous works and probably with another reanalysis if it was checked.

We are aware of the general strengths and limitations of reanalysis data and the differences between reanalysis datasets, specifically relevant for trend analyses. We realize that the manuscript was lacking a clear discussion on the uncertainties arising from this. Therefore, we have extended our analysis, comparing the previously presented results for ERA5 to three widely used reanalyses ERA-Interim, MERRA-2 and JRA-55 as well as the recently published reanalysis JRA3Q. In order to better address uncertainties of our findings, we point out robust features and discuss discrepancies across the different reanalyses.

References

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