

Response to Editor's corrections

Dear authors,

after checking your final response and the revised manuscript, I am happy to tell you that I reached the editorial decision of: "Publish subject to technical corrections".

Let me congratulate you to this incredible research effort and thank the referees for devoting their time for reviewing this enormous body of work.

In my eyes your manuscript has a potential of becoming a landmark paper in the fields of stratospheric transport and chemistry climate modelling and hence, although it is fairly long and on places very technical, I recommend it for a highlight paper of ACP.

Because of this and my interest in the topic, I read the manuscript in detail and provide below a list of typos and friendly suggestions that can help to polish the paper even more. Because I believe in your expert consideration of the few comments raised, the manuscript will be accepted automatically after you send us the revision and I will not have any further control about it.

Thank you for publishing in ACP.

Best regards,
Petr Šácha.

We are grateful for your positive assesment of the paper, and for the detailed constructive comments. We have addressed them below.

Comments and typos (chronological):

P2L26 ozone-depleting substances (ODS)->Consider the abbreviation ODSs, because similarly for plural you use CCMs, GHGs and on the page 9 you even use ODSs
changed

P3L66-P4L68" ... although some studies have examined mean age and other transport metrics of models in....(CMIP5, CCMI, CMIP6)" -> Consider rephrasing, or placing e.g. before the references you list, because I know about some studies, also with my personal involvement, that can qualify to this criterion and are not listed.

Added Sacha et al. (2019)

Tab.1 (and also Tab. 2, Tab. 3, L184, Fig. 19) - an overbar is missing for the components of the residual mean circulation. Although, it is probably already acceptable to refer to it as the residual circulation in the main text, it does not make any sense to relax the overbar (as for other mean quantities in the table) while keeping the asterisk, because it is meant to denote a "modified" mean.
Changed throughout the document

P8L146 (Sofieva et al., 2025; ?)
corrected

P9L184 Here I would say that also u should be with the overbar, or use U to stay consistent with the tables.

Chose U for consistency with tables

P9L208 double use of expected ...expected amount of stratospheric Cly expected from..
removed second expected

Tab.4 the FWD abbreviation is defined only at page 28
spelled out in table

P13L306 (and throughout the manuscript-L361, L451, L677) - the not shown statement is invoked, which I think is a shame given that the manuscript already has a comprehensive Supplementary Material. Considering the potential of the paper to become an important reference for years to come, I would strongly suggest to move also the "not shown" results to the supplement for a full transparency.

We included all (not shown) plots in the supp material except for one, which deviates more from the paper focus.

P15 Fig.2 -> Can you repeat for the reader the number of models (dots) for each colour in the plot?
I don't see the need, the dots clearly visible in the plot.

P15L330...also have a more realistic mixing efficiency...-> How do we know what is the realistic range for mixing efficiency? A reference missing here?

This is Suggested by the good correlation with mean age, given the linear relationship between the two magnitudes:

“the good correlation of the mean age with mixing efficiency suggests that the models simulating a tropical mean age most similar to the observations also have a more realistic mixing efficiency,”

P16L341 (and throughout the manuscript) ...the overturning circulation..->the mean meridional overturning circulation. I think that as your manuscript has the potential to be read by a broad readership and also by students starting in the field, I suggest being as precise as possible when naming physical mechanisms or phenomena.

Changed in most of the occurrences, not all to avoid excessively long sentences and repeating too much

P16L344-345 .. decreases rapidly with height due to the exponential decay of air density,...-> I do not think that this is a correct statement, please double-check. From continuity, the cause of the decrease is the meridional transport across turn-around lats.

It is correct, because $MF = w^* \rho$, where $\rho \sim \exp(-z/H)$, and w^* changes are much smaller (minimum around 50 hPa and then increase again up to 1 hPa).

P19L403 up to 10 km (~25 hPa)->up to the 10 km distance from the maximum?

Yes changed

Fig. 7 ... range 0-5 ppmv..->0-0.5 ppmv?

Yes changed

L481 As before, the weak tape recorder damping could also be caused by an excessive vertical diffusion in the models. -> At this place this statement seems contra sensus (weak damping implying stronger diffusion). Can you provide more reasoning for it?

Thank you for this comment. We have rephrased to specify that higher diffusion could extend the anomalies upward. In addition, this comment made us rethink our conclusions on mixing, and we realized that there is no support for weaker mixing from N2O metrics. We consequently toned down the conclusions on mixing mentioning this caveat in this section, as well as in the Conclusions and the Abstract.

L534 .. resolved wave drag..->resolved wave activity? You are not analyzing the drag here.

Changed

Fig. 13 .. Based on monthly mean output interpolated to daily values for the period 1980-2000..->Can you explain this methodological aspect better?

We have added some explanation in the text.

P31L580 The paragraph starting: In contrast with the lower stratosphere, ozone in the upper stratosphere is underestimated..-> The reader is left wondering, what the reason for the underestimation can be in relation with the previously identified biases. For other characteristics you give the reasoning already in the results section. If you do not have an explanation, state it here. This is also relevant in connection to the hypotheses explaining Cly and its fractional release later in the text and for the conclusions at P44L811-812

Unfortunately we do not understand what causes these global biases. We have added this sentence: “The reason for the opposite model biases in the global lower and upper stratosphere is unclear.”

Figure 17. Probability density function of...-> Is PDF the correct name for what is plotted here? Also, I miss more information on the contours and shading.

We extended the figure caption to include information on the contour spacing, and added ‘as a function of mean age’ to better describe the figure.

P35L680 ..negative trends An..->a comma is missing.

Added a dot.

P36L686 Typo upward)19b)

corrected

P37L703 .. The averaged reanalysis trends are noisier than the model trend..-> This is consistent also with Šácha et al. (2023), who have found that reanalyses due to large spread cannot constrain the CMIP6 model differences in the net tropical upwelling trends

Added a mention to Sacha et al. (2024) (we think this is the paper that is referred to: “[Disentangling the advective Brewer-Dobson circulation change](#)”)

P37L716-717 .. rise in tropopause pressure...->decreasing tropopause pressure?

...and the acceleration of tropical upwelling largely arise from the same underlying driver, namely tropospheric warming...->

...,namely increasing GHG concentrations...?

Because as shown in Vallis et al. (2014, RMetS) stratospheric cooling is an important factor for the decreasing tropopause pressure.

We have added primary driver, because tropospheric warming is the primary driver of the tropopause rise, as discussed in Vallis et al. (2014).

Fig. 20 - Reanalyses are not plotted in the plot b)

We added reanalysis results to the plot.

P43L794.. evidence of slightly weak mixing..-> weaker?

Changed to “too weak”

P45L846 (APA, 2025)? - this is probably connected with the incomplete reference at P49L885.. The Hunga Volcanic Eruption Atmospheric Impacts Report, <https://doi.org/10.34734/FZJ-2025-05237>, 2025.

Corrected, thanks