

Response to Reviewer #1 for discussion paper

Comparison of mean age of air in five reanalyses using the BASCOE transport model

Chabrilat et al., ACPD, 2018

We thank the reviewer for his/her positive and useful comments. In our replies below the bold type is used to highlight text in the revised manuscript.

- *It is stated that the model input is wind and surface pressure. Is the latter used to convert from model levels to pressure levels? Perhaps this could be said directly at some point.*

Yes: the output AoA datasets are indeed interpolated from model levels to constant pressure levels using the instantaneous and two-dimensional input surface pressures, i.e. prior to any averaging in the longitudinal or time dimension. This is now stated explicitly at the end of section 2.1.

- *Is the sentence in L15-16 (P1) of the abstract needed? It seems redundant.*

This sentence has been removed from the abstract.

- *L4 P2: “considerably depending on the considered period”: consider changing one of the two to avoid repetition.*

"Considerably" has been replaced by "substantially"

- *L30 P3: explain how these...*

Typo corrected

- *L32 P3: each tailored for a different reanalysis dataset.*

This sentence was outdated because the version published in ACPD compared with only one other transport model (TOMCAT) which is tailored for ERA-I. We have updated the sentence accordingly.

- *L14-15 P4: It could be mentioned why it is chosen not to use the vertical wind component directly.*

The revised manuscript states:

These models are usually set on a different grid than their input reanalysis dataset. Since this prevents the direct usage of the vertical wind component in the reanalysis, they rely on mass continuity to derive the vertical mass fluxes corresponding to their own grid.

- L31 P4: “idealized tracer which increases linearly at the surface”: throughout the surface or just in the tropics?

The choice of the surface as source region introduced confusing inconsistencies in the discussion paper (see the general comment by the second referee and also the next comment here). Hence we decided to re-run our calculations and re-plot all figures (except for figure 8, see below) using as source region the tropical tropopause region (defined as the 100 hPa isobar between latitudes 10°S and 10°N), and computing the AoA at each gridpoint as the time elapsed since the mixing ratio of the ideal tracer reached the same value in that source region. The figures did not change significantly from the discussion paper, indicating that this is a methodological issue which does not have any impact on our findings. The last paragraph of section 2.1 has been re-written to fully explain the updated procedure for computing AoA.

For figure 8 (and figure 8 only) we have kept the original calculations where the tracer was set to increase linearly *throughout* the surface, because this figure includes a comparison with observational values of AoA which used the surface as reference. We have moved to the discussion of figure 8 the description of this *surface* boundary condition and its propagation through the troposphere, because it is now irrelevant for all other figures. This description now states explicitly that it uses

... a synthetic tracer which is set to a global constant increasing linearly with time at the surface.

- L31-32 P6: “the AoA at the equatorial tropopause has been subtracted from the fields...”: did you use the climatological or time-dependent tropopause altitude?

See previous question: the revised manuscript shows AoA computes AoA directly from the tropical tropopause region and has dropped all *a posteriori* corrections by subtraction of AoA values at the equatorial tropopause. We have removed the sentences describing this procedure from the revised manuscript.

- L9 P8: GCCM: this has not been introduced before, do you mean CCM?

Yes. We have replaced all occurrences of "GCCM" by "CCM".

- L14-15 P8: “different latitude gradients between the tropics and ...”

Done.

- L13 P9: remove “globally”, it is not global but midlatitude average.

Done.

- L30 and 31 P9: “different with” should be “different from”

Done.

- L11 P10: “not significant”: in ERA-Interim or in observations?

Replaced "sign of observational trend not significant" by clearer

...sign of trends not significant in the observations.

- *L12 P10: “ERA-I does not show any overall trend after 2000...”. Why do you point out these trends after 2000 in ERA-I specifically? Same thing on P16 L22-23.*

This focus on ERA-I is due to the exclusive use of ERA-I in previous studies modeling the latitudinal structure of AoA for the post-2000 period (see first paragraph of section 4.3, P12 of ACPD manuscript). But this context had not been introduced yet for the discussion of figure 8 (i.e. L12 P10 and P16 L22-23). Since this question is specifically investigated through figures 11 and 12, we have simply removed the premature sentence from the discussion of figure 8.

- *L23 P12: standard error for which confidence level?*

This important information has been added in section 4.1 which describes our methodology for multi-linear regressions:

The uncertainties arising from the fit are calculated for the 95% confidence interval and corrected for auto-correlation in the residuals (Eqs. 3,4 and 6 in Santer et al., 2000).

and in the section 4.3 (discussing linear trends):

It is expressed in years per decade (yr dec^{-1}) and is deemed significant at a given grid point if its absolute value is larger than its uncertainty (as defined in section 4.1).

- *L1 P13: remove “unexpected”.*

Done.

- *L2 P13: remove “much”*

Done.

- *L7 P13: “Diallo et al. (2012)...” Ploeger et al. (2015a) show AoA trends for the total and later periods.*

Thanks for pointing this out. In the revised manuscript we now also compare our results with those by Ploeger et al. (2015a), both for the discussion of the latter period (figure 11):

Our results also agree well with those obtained by a diabatic model driven by ERA-I over the same period (Ploeger et al., 2015a).

and for the discussion of the total period (first paragraph discussing figure 12):

Our ERA-I results for the overall period partly contradict those obtained by diabatic models which use not only the wind fields from ERA-I but also its heating rates (Diallo et al., 2012; Ploeger et al., 2015a). Looking at slightly shorter periods of two decades (1989–2010 for the former and 1990–2013 for the latter), these papers reported negative AoA trends for both hemispheres below 28km altitude.

- *L10 P13: “using only wind fields”: do you mean not using heating rates? Perhaps this should be explicit.*

Indeed we meant that our results did not use the heating rates. During our revision we found that the whole sentence was confusing and removed it from the manuscript. The additional use of ERA-I heating rates by the diabatic models (Diallo et al., 2012; Ploeger et al., 2015a) is now explicitly

stated as soon as they are cited (see previous comment).

- *L26-27 P14: “While this may be a coincidence...”: but having more wave drag would imply a faster BDC, so I do not see the point of this sentence.*

Agreed. This sentence has been removed from the revised manuscript.

- *L34 P14: “Miyazaki et al. (2016) Fig. 11”*: The trends in annual mean tropical upwelling for these reanalyses are shown in Abalos et al. (2015) Fig. 11.

The following sentence has been added to the discussion:

Similar disagreements have also been reported between the trends of the annual mean tropical upwelling in three reanalyses over the period 1979–2012, with vertical residual velocities (w^*) increasing in MERRA and JRA-55 and decreasing in ERA-I (Abalos et al., 2015, Fig. 11).

- *L18 P15: Another difference with CLaMS is that it includes a mixing parameterization.*

The revised version states also this difference, citing Konopka et al. (JGR, doi:10.1029/2003JD003792, 2004).

- *Figure 7 caption: “No impact...”* This sentence does not belong to the caption but to the main text.

This sentence has been removed from the caption of Fig.~7.

Added references

Konopka, P., Steinhorst, H.-M., Grooß, J.-U., Günther, G., Müller, R., Elkins, J. W., Jost, H.-J., Richard, E., Schmidt, U., Toon, G., and McKenna, D. S.: Mixing and ozone loss in the 1999–2000 Arctic vortex: Simulations with the three-dimensional Chemical Lagrangian Model of the Stratosphere (CLaMS), *Journal of Geophysical Research: Atmospheres*, 109, doi:10.1029/2003JD003792, 2004.