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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of mean age of air in five reanalyses using the BASCOE transport model" *by* Simon Chabrillat et al.

Anonymous Referee #2

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This paper examines the distribution of age of air (AoA), its seasonal and quasi-biennial variation, and its long-term trend using result from the BASCOE model driven by output from five atmospheric reanalyses over the period 1980-2015. The principal conclusion is that the simulations of AoA obtained when BASCOE is constrained by different reanalyzed datasets differ substantially from one another. This is not at all unexpected given the differences among the reanalysis models.

The paper is well organized and clearly written, with some exceptions, the main one being that the procedure for computing AoA is not well explained. In particular, it is not clear whether AoA is calculated with respect to a reference level at the tropical tropopause or in the troposphere, and this introduces some ambiguity in the interpretation of the results.

The paper should be suitable for publication in ACP once the following comments are addressed.

Specific Comments (page, line):

(4, 20) "There is no other representation of convection": It is not clear that, in the Tropics, where deep convection can reach the 14-15 km level, this artificial diffusion can simulate vertical transport realistically. But perhaps this does not matter for assessments of AoA in the stratosphere if the base point for AoA calculations is taken to be at or near the tropical tropopause? Please comment (especially since it is not clear how the reference level for computing AoA is chosen). See also comment at (7, 24).

(6, 5) "at the wavelength number 47": "at wavenumber 47" might be better.

(6, 8) "Figure 1 compares the results": I do not believe you have stated how AoA is calculated. In the Introduction (2, 11) you write that "the mean age of air (hereafter AoA) is an evaluation of the time necessary for variations of long-lived . . . species to propagate from the troposphere to various regions in the stratosphere". On the other hand, the upper right panel of Fig. 1 suggests that the reference point is at or near 15 km, that is, just below the tropical tropopause. Is that correct? Please be more explicit.

(7, 18) Figure 2: This figure, as well as Figure 3, would benefit from a color bar to indicate the values of the AoA isolines not explicitly labeled.

(7, 24) Figure 3: I am confused by this comparison. One would think that the difference in AoA should vanish at the reference point, assuming the latter is the same for all calculations, but for some of the reanalyses (JRA55, CFS), AoA is younger than in ERAi everywhere. Is this because BASCOE is run on the native grid of each reanalysis, such that there is no common reference point for the AoA calculations? Also, contrary to the BASCOE–TOMS and BASCOE–SD-WACCM comparisons shown in Figure 1, where AoA = 0 in the upper tropical troposphere (Fig. 1, upper right panel), AoA in the simulations shown in Figs. 2 and 3 is not zero at this location. So, where is the reference point in these simulations? If it is at the surface, then AoA will reflect the effects of transport not just in the stratosphere, but also in the troposphere, including the artificial diffusive transport between the surface and the middle troposphere. Unless I am misunderstanding what you have done here, it seems to me that, if AoA is intended to highlight transport in the stratosphere (e.g., Waugh and Hall, 2002, Sec. 3.1) then the choice of a base point in the troposphere confuses the issue, especially given the use of artificial diffusive transport in the lower troposphere.

(8, 5) "The intercomparison at 50 hPa": You should state explicitly in the text that in these comparisons AoA is "normalized" to be zero at the tropical tropopause (this is only stated in the caption of Figure 4). Otherwise, the reader will wonder, as I did, why the AoA shown in Figures 2-3 are different from the AoA in Figure 4. By the way, a problem with the "normalization" of AoA to zero at 100 hPa is that it gives the impression that AoA above that level is determined only by the stratospheric circulation, when in fact the AoA also contains the effect of transport in the troposphere.

(8, 12) "overall, the spread ... is larger than the 1-sigma...": One wonders how this result would change if AoA were computed with respect to a reference point at 100 hPa.

(8, 26) "The spread between the four reanalyses reaches a maximum of 0.2 years at 30 hPa": Are you referring here to the gradient comparison, Figure 4d? How is this "gradient" calculated? The figure legend refers to "MLNH-Tropics" and shows values in months, not per unit distance, so this is really a difference between the Tropics and midlatitudes of the NH. How are Tropics and NH midlatitudes defined? Note also that the "outlier" behavior of the MERRA-2 simulation is confined to altitudes below 22 km or so, where, interestingly, it agrees better with the data than any other simulation.

(8, 30) "MERRA-2 yields an outlying vertical profile of AoA at northern midlatitudes": True with respect to the other reanalyses except for MERRA (Fig. 4c), and in fact, MERRA and MERRA-2 midlatitude profiles of AoA agree best with the observations.

You keep referring to MERRA-2 as an "outlier", which carries negative connotations, but in fact being an outlier in this comparison is a good thing if one considers the data to be the "truth".

(9, 14) "MERRA-2 starts with much older values": This behavior does appear to be anomalous. Any idea what might be causing it?

(9, 18) "The Pinatubo eruption does not appear to have any impact of the simulated AoA at 50 hPa": Insofar as one might expect that the largest impact of Pinatubo would be in the Tropics, it might be worthwhile to examine the AoA time series averaged over, say, 30N-30S.

(10, 8) "observational trend is not significant": One would not expect any trend calculated from the smoothed, model time series shown on the right pane of Figure 8 to be significant either. By the way, you keep referring to "trends" in connection with the model results, but you have not calculated any trends. Note also that Garcia et al. (2011) have argued that, even using model output for an ideal AoA tracer, trends over periods as long as 30 years are often not significant when the ideal tracer is sampled like the available observations of stratospheric tracers. Furthermore, trends derived from observation are also confounded by the fact that no real atmospheric tracer has a constant, linear growth rate.

(11,16) "in the polar regions and midlatitudes": Why limit this to extratropical behavior only? It would be interesting to show the seasonal amplitude in the Tropics as well, say a $\pm 30^\circ$ average.

(11, 24) "MERRA and MERRA-2 ... different amplitudes depending on the period used for the analysis": Does this have to do with the development (1989-2001) and stabilization/decline (2002-2015) of the Antarctic ozone hole? To explore this issue, one would have to examine the actual seasonal climatology at high SH latitudes, not just the annual amplitude.

(12, 9) “could not be expected from inspection of the native dynamic variables”: I do not understand what you are trying to say here. Please elaborate. And note also that the discrepancies you mention (“up to 50% dependencies on the considered time period”) are not even illustrated, so it is very difficult to even guess what the intent of your statement is.

(12, 15) “unexpectedly increasing”: Given the very short period covered by the SF6 observations, it is not clear that one should “expect” any particular sign for the trends. Determination of AoA trends from observations of stratospheric tracers is fraught with many uncertainties; even in models where an ideal, linearly increasing artificial tracer is used, one has to rely on zonal-mean results over long periods to obtain trends that are clearly statistically significant. Arguably, examination of AoA trends determined from observations of stratospheric tracers is not the best tool for documenting changes in the BD circulation. See Garcia et al. (2011)

(13, 1) “unexpected growth”: Again, there are no clear expectations about trends for short periods.

(13, 23) “the reversal is found for all five reanalyses”: This is true, but the reversals are in the opposite sense in ERAi and CFS vs. JRA-55, MERRA and MERRA2, so it is hard to know what to make of this.

(13, 25) “unexpectedly large disagreements”: I am not sure why you think the disagreements are “unexpected”. While presumably all reanalyses use more or less the same observational data, the manner in which the data are assimilated and the physical parameterizations included in the different models (in particular, those for convection and mesoscale gravity waves) are different. Note that at (14, 8) you suggest that “the disagreements found here may lie in the differences between the underlying models”; I agree that this is the most plausible working hypothesis.

Grammar, typos, etc.

(2, 21) “monotonously” → monotonically

(3, 10) “technical difficulties which” → technical difficulties that

(6, 27) “very similar than” → very similar to

(9, 33) “twice smaller” → twice as small

(11, 30) “between reanalyses” → among reanalyses → among reanalyses

References cited:

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