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Interactive comment on "Time varying changes in the simulated structure of the Brewer Dobson Circulation" *by* Chaim I. Garfinkel et al.

Anonymous Referee #5

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General comment:

This paper presents GEOSCCM model simulations of the Brewer-Dobson circulation (BDC) and mean age over the past decades. The authors consider different ensemble members from their simulation which show a very different evolution of the BDC. One member even shows increasing mean age in the NH since 1988, similar to observed mean age trends. In the lower stratosphere, on the contrary, the BDC continues to accelerate. Hence, structural changes evolve in the BDC after 1988. ODS and volcanic eruptions are identified as the main forcing agents of these structural changes.

This paper addresses a very timely aspect of stratospheric dynamics and transport and will definitely be very interesting to a large readership. However, I have three major concerns which the authors need to consider before I can recommend publication.

Major comments:

1) Model vs. real atmosphere:

As mentioned above, it is very interesting to see how strongly simulated mean age trends (on decadal time scales) depend on internal variability, and therefore considering the different ensemble members is a very good idea. However, to me the main question remains: which ensemble member is closest to reality and hence most reliable? And this question is not addressed in the paper.

Clearly, if started from different initial conditions the system evolves differently. For instance, there will likely be significant differences in the representation of the QBO between the three members. The authors try to discuss dynamical differences based on residual circulation and EP-flux in Sect. 3.1 (Fig. 3) - but in my opinion this discussion needs to be further substantiated. The following two main questions should be answered:

1) What exactly causes the differences in wave flux and residual circulation between the ensemble members (e.g., differences in the background flow)?

2) Which member has dynamical characteristics closest to the real atmosphere (e.g., a comparison of the QBO with observations could be enlightening)?

If the conclusion of the paper should be "models can simulate trends generally consistent with observations" (P19, L12) it needs to be confirmed that also the underlying dynamics is consistent with available observations or reanalysis data (similar statement on P17, L9), such that the resulting age trend is not due to cancelling effects of errors in different modes of variability. Moreover, I think the wording of the above conclusion is too strong: Only one of the considered ensemble members shows some consistency with observations – and further it is not clear why. If the authors want to make the statement that "BDC in GEOSCCM is generally consistent with observational constraints",

both dynamical quantities (as mentioned above) and mean age need to be more thoroughly compared to available observations. Unless these comparisons are done, the main conclusion should be rephrased rather as: “current model uncertainties due to the representation of internal variability are so large that simulations may be consistent with available observations”.

In this context, I don’t agree with the commentary of Sect. 4.3 that “deducing trends in the BDC from reanalyses is fraught with danger”, while “the modeled evolution of the BDC in GEOSCCM is consistent with available constraint” (P16, L24ff). Mean age simulations based on reanalysis data have been extensively compared to available balloon borne and MIPAS satellite mean age observations and are consistent within observational uncertainties (e.g., Diallo et al., 2012; Ploeger et al., 2015). Furthermore, dynamical variability in the reanalyses is consistent with available observational constraints by definition. The 2002-2011 trend of ensemble member 3 shows aging in the NH similar to MIPAS (e.g., Haenel et al., 2015, Fig. 9) and reanalysis driven simulations. However, negative mean age trends in the SH as observed by MIPAS and consistently simulated by reanalysis-driven models are not simulated by GEOSCCM. Overall, I don’t agree with the statement that GCMs are better for estimating decadal (!) trends than reanalysis and I recommend removing Sect. 4.3 (see also my specific comment regarding the representation of volcanic effects in reanalysis).

2) Attribution of mean age trends:

Although the authors explain in the beginning that “mean age is an integrated measure of the total transport” and “only in the tropical lower stratosphere can be thought of as dominated by vertical advection” (P1, L24ff), the following analysis aims to directly link mean age variability with residual circulation (e.g., Fig. 3 and Sect. 3.1). It is known that mixing processes have a strong impact on mean age and its trends (e.g., Neu and Plumb, 1999; Garny at al., 2014) - so what about these effects? I think Fig. 3 can be very misleading in relating the mean age trend just to the residual circulation without including the additional mixing effects, and these likely matter. For instance, why is the

residual circulation and wave flux trend in the NH (northward 30N and above 18km) almost the same for ensemble members 1 and 3, but the resulting mean age trends very different (negative vs. positive NH age trends for members 1 and 2)? I think these mixing effects need to be either analyzed or at least a more careful discussion is needed.

Specific comments:

P4, L31: What diagnostics are included in the model? If some measure for mixing could be easily added, this would significantly strengthen the analysis presented here (see my Major comment 2).

P8, L20ff: The discussion here is not entirely clear to me: Why is the “no change in mean age” caused by a “reduction in planetary wave flux entering the stratosphere”? Shouldn’t this reduction cause a weakening circulation and increasing mean age? Overall, I have the feeling that the paragraph here is more a discussion than belonging to the results section, as it just discusses the presented mean age changes against the background of published literature.

P10, L5: “... factors that led to aging (Pinatubo ...)...” seems not an optimal choice of wording to me. The direct effect of Pinatubo is to decrease mean age (e.g., Fig. 4a). What you mean here is that this decrease of mean age due to Pinatubo causes a stronger aging trend, as Pinatubo is at the beginning of the considered period. Please improve the wording.

P15, L6ff (section 4.1): The sentence ”GEOSCCM mean age lies within the error bar for most measurements, and thus is generally consistent with observations” seems too strong to me. Even if GEOSCCM age lies within the error bars of most of the observations, the model clearly underestimates the mean age after 2000. Further, I think Ray et al. (2014) mapped to 42N equivalent (!) latitude. Is your mean age also sampled at equivalent latitude or just latitude (as I read from the text)? Please clarify.

P16, L29: It was shown by Abalos et al. (2015) and Ploeger et al. (2015) that there is NO "apparent inconsistency" between increasing mean age in the NH between 2002-2012 and an acceleration of the residual circulation in the long-term if mixing effects on mean age and the appropriate time period are taken into account. Please clarify what you mean here.

P17, L1: Diallo et al. (2012) showed increasing mean age after volcanic eruptions only at lower levels in the stratosphere around 19km, and this is indeed consistent with the GCM based results of Muthers et al. (2016) (see their Fig. 3). Hence, there is no "contrast" between the two papers - both are very consistent! The authors are right in saying that ERA-Interim does not assimilate aerosol data. However, parts of the volcanic aerosol effect is included in the reanalysis due to assimilating observed temperatures. And can we be sure that the representation of volcanic aerosol in climate models is correct (e.g., amount of injected aerosol, injection height, ...)? Why, for instance, don't we see an effect of Pinatubo in the SH (Fig. 4c/f) although Pinatubo's effect on temperature appears rather symmetric about the equator – even stronger in the SH (see Fujiwara et al., 2015, Fig. 5)? Hence, I again doubt that decadal trends from climate models are more realistic than from reanalysis-based simulations (see also my Major Comment 1).

Figure 1, caption: Include a decription of the smoothed curve in (a) in the caption. In the model simulation the version without smoothing is used, right? Please clarify in the text.

Figure 3: The statistical significance of the trend is only mentioned in the text but not plotted. It would be better to plot the trend and its significance in the same figure (e.g. as additional shading).

Figure 6: It would be helpful to see also the SSTGHG case in the figure, to estimate the ODS-effect. And why not showing the net upwelling mass flux averaged between turn-around latitudes here? This should give a much more reliable measure of net

upwelling than the flux averaged between fixed latitudes (and as far as I understand, the authors have calculated this already).

Figure 7: It would be helpful to include also the linear trend values for both observed and GEOSCCM simulated mean age in the figure (or in the caption).

Technical corrections:

P1, L11: I would better say: "...and is not NECESSARILY the case..."

P1, L23: The $*$ for defining the residual circulation vertical velocity is not raised, it should read $\overline{w}^*$. This occurs several times also at later places in the paper.

P4, L20: CO_2 should not be in italics.

P4, L29: I would cite Hall and Plumb (1994) or Waugh and Hall (2002) here, for the calculation of mean age from the linearly increasing tracer.

P6, L8: The number of years "n" should be upper case.

P7, L8: "...resolved waveS..."

P7, L28: "... one ensemble member simulateS..."

P8, L7ff Ray et al. (2010) also found that a "moderate increase in the horizontal mixing into the tropics" has to be assumed in their leaky pipe model (this is also related to my Major Comment 2).

P10, L1: "...concentrations impact..."

P10, L4: "...aging in the deep branch..."

P16, L6: "...recent aging trend...' - I think you mean decreasing mean age?

P16, L16: "...but decreases at 70hPa..." I guess you mean 50hPa, right? At least this is the level shown in your Fig. 6.

Figure 1, caption: Blank missing before "total solar irradiance".

Figure 3, caption: "...decreasing mean AGE OF air..."

Figure 6, title: I guess you mean "32S to 32N", and not "32S to 32S"?

Figure 6, caption: Blank missing after "...100hPa"

P15, L13: "...age tracer (see Fig. 7)."

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