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Discussions

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

Anonymous Referee #3

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The study by Garfinkel et al uses a comprehensive set of hindcast simulations with a chemistry-climate model to assess past trends in the Brewer-Dobson-Circulation. They show that the circulation speeds up over longer time scales (i.e. 1960 to present), in agreement with past studies, but trends differ in different regions when considering only the last few decades (since the late 1980s). In particular, they show that their model is able to simulate positive AoA trends in the NH mid-stratosphere, which would be in agreement with tracer measurements there. They further show how different forcings contribute to the different trends, and emphasize that the flat or positive trends in the mid-stratosphere since ∼1990 are due to the timing of volcanic eruptions and the recovery of ozone (declining ODS concentrations).

Overall, the paper presents interesting new results that contribute significantly to our understanding of the evolution of AoA over the past decades. The statements are

supported by the results shown, and the paper is overall well written. However, the paper could be strengthened significantly by clearer and more quantitative description and presentation of the results, as detailed below. I suggest that the authors revise the paper according to the following comments.

General comments:

1. Overall, the paper would benefit from a more quantitative assessment of the results. The large number and design of the experiments is very well suited for the purpose, but it would be great to see more quantitative measures of the trends and contributions of different forcings. In particular, the availability of 3 ensemble members for each experiment is a perfect basis to estimate the robustness of trends, and I would strongly suggest that this is done. Some examples: Fig.3: It is hard to deduce quantitative changes in the residual circulation from this Figure. I would suggest to include e.g. a Figure of the trend in tropical upwelling as a function of height to justify the statements in the text better. Also, while the text mentions at many points that trends are "significant", please indicate regions of significance in the Figures (e.g. in Fig. 3). Fig. 4: The ensemble members of the experiments can be used to assess the influence of internal variability (e.g. as seen in Fig. 5 for the all-forcing experiment). I agree that it might be too messy to add all the ensemble members in Fig. 4, but for example adding a shaded region to indicate the variability for each experiment would allow better to distinguish between forced differences and variability. Also, the trends discussed from Fig. 4 should be assessed more quantitative rather than only "by eye" - for example calculate the trends for each region as function of start year for each ensemble member and for the ensemble mean (see below).

2. In general, the idea to reconcile the current results with previous studies (Section 4) is a good addition to the paper. However, the discussion is mostly qualitative, and I was left puzzled at the end whether the agreement is good or not. I suggest to make the comparison more quantitative (e.g. add the trend as function of start year from the Ray/Engel observations to the Figure suggested above) where possible, and shorten

the other parts.

3. The authors argue that the recovery from the influence of ODS contributes to positive trends in AoA in the NH mid-stratosphere, but in the tropics and in particular in the SH, ODS leads to AoA "being flat" (page 9, line 18-22). As the influence of ODS concentrations on ozone and subsequently on dynamics is far stronger in the SH, I don't understand this result. If anything, I would expect that the effect of declining ODS is apparent first in the SH. Also, the "positive trend" in the NH appears mainly due to rising AoA in the last few years (from ~2005), and this positive trend is already visible (though weaker) in the SSTE and SSTGHGE experiments, so I would think that this might have to do with anomalous SSTs, as you actually mention in Section 3.2.1. (1. SSTs). So why do you conclude that ODS are important for the positive trends, when SSTs might contribute to at least a flattening of the trend? Furthermore, as mentioned before, this discussion is somewhat qualitative. A suggestion would be to calculate trends for each experiment (and ensemble member, to get a measure of uncertainty due to internal variability) as function of start date (see above), to gain a more quantitative assessment of the contributions of different forcing to the trend.

Specific comments:

Abstract, line 6: I would rather emphasize that the trend in NH mid-stratospheric AoA are flat/positive in the ensemble mean than that trends are positive in "a simulation" (=one ensemble member?), as the latter result is far less compelling.

Abstract, line 9: Please make the statement that changes in AoA and trop. upwelling "are similar" more quantitative (e.g. both have similar relative trends?)

Abstract, line 11: ".. and is not necessarily the case.." (as it is in some regions).

Page 2, line 2: "...increased or remained unchanged" (as Engel report insignificant changes).

Section 2 (Methods): I agree that it is not necessary to specify all details of the model

(simulation) if described elsewhere. However, at least a comment on the resolution of the model and the upper model level would be desirable. Furthermore, are the boundary conditions and design of the simulations as those specified e.g. for CCMVal2 or CCMI?

page 4, line 25: is the version of the model used in this study is no longer supported, or the one used in Oman 2009? Please specify.

page 4 , line 30: "long" = X years ? Please specify.

page 6, line 30: The upwelling trends are hard to detect from Fig. 3. Include e.g. a Figure with upwelling trends as function of height (see above).

page 6, line 32: Please indicate regions of significance in Fig. 3 (see above).

page 7, line 16: The changes in the residual circulation should be consistent with changes in wave driving in a self-consistent free-running model, so it's good to show this, but does not add that much information. It would be interesting to look into the mechanisms of why these changes in the wave flux changes occur, but I understand that this is beyond the scope of the paper. Also, I personally would find it more interesting to see the ensemble mean changes in circulation and wave fluxes for the different experiments rather than the individual ensemble member of the all-forcing simulation - I think more on the mechanisms could be learned from them.

page 7, line 30: The argumentation of "anomalies occurring in the same integration.." is very speculative and not very physical. Of course, you can always construct ensemble members that show all sorts of trends - however, the question is how likely this is. Given a certain internal variability plus the forced variations, the likelihood to gain a certain trend over a certain period can be calculated.

Paragraph 4.1: I have to admit I'm left puzzled at the end of this section whether trends/variability of the current study and Ray/Engel are consistent or not. I suggest to add a more quantitative evaluation (see 2. general comment). Furthermore, can you please clarify what the difference between the Ray and Engel time series are?

page 16, line 1-3: The reasoning here is a little weak. I would think what you want to say is that trends over one decade are strongly influenced by internal (unforced) variability, so that a free-running model is not designed to reproduce this trend. You can estimate this uncertainty from your ensemble members (e.g. trend is X+/- Y years/dec) and argue whether the observed trend lies within this uncertainty range. This makes more sense than saying that different aspects of trends are "captured by at least one ensemble member...".

page 16, line 5: Which time period is considered in Bönisch (2011), and do trends only agree in sign or also in magnitude?

page 16, line 6: "NH lower stratosphere aging trend": here AoA decreases, right? so "aging trend" is maybe not the correct wording?

Section 4.2: In general, the discussion in this Section is very qualitative, and for me it is hard to follow which observed aspects are in agreement with the model results and which are not. Please clarify.

page 16, line 25: Abalos shows that trends among reanalysis, and in particular among different methods to estimate w* strongly vary. E.g. tropical upwelling estimated from diabatic heating rates in ERA-Interim show a deceleration in the NH (see their Fig. 11, Fig. 14), consistent with Ploeger and Diallo (that use diabatic heating rates from ERA-Interim). Please clarify.

page 17, line 1ff: I think you have to differentiate in which region you look for the response to volcanic eruptions, as the response is likely spatially not homogeneous. Note that while ERA-Interim does not assimilate aerosol data, it does assimilate temperatures and thus might be able to capture the influence of volcanos indirectly.

page 19, line 27 ff: see above (comment on page 16, line 1ff): the likelihood to obtain a certain trend can be calculated when the internal variability is known, which you

could estimate from the ensemble members. And/or the trend (quantitative!) plus the uncertainty can be calculated, and you can argue whether the observed trend lies within the modelled trend uncertainty. This would make for a much stronger statement then speculating about other ensemble members.

Technical:

page 10, line 4: "the aging since...": delete since

Fig. 6: 32S to 32N (replace S by N)

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