Response to Anonymous Referee #1:
(our response in italics)

This paper compares trends in the stratospheric Brewer-Dobson circulation from 5 different CCMI-1 models in terms of mean age, residual circulation, aging by mixing and wave drag trends. The study focuses on the subtropical lower stratosphere regions where all models considered robustly show largest negative mean age trends. A new and interesting aspect of the study is the consideration of the effect of the vertical shift of pressure levels under climate change on the age trend. The authors argue that the effect of such a shift should be largest in the subtropical lower stratosphere where the mean age gradient is largest, and that the shrinkage of the stratosphere likely contributes to the strong negative trends in that region.
The paper is overall well written and the results are well presented. The topic is of high relevance for the atmospheric community and the effect of a vertical shift has not been sufficiently discussed so far. I therefore recommend publication, but have a few comments to be addressed below.

We are grateful for the reviewer's positive review, valuable comments and suggestions, which helped us to improve the quality of the paper. Especially we acknowledge the comments on wave driving and the role of variability in the relationship between wave drag and residual circulation.
We have adapted almost all of your suggestions, please refer to the revised, marked up version of the manuscript. Following is our reply to your minor and specific comments:

<u>Minor comments and specific comment P22L535:</u>

1.I find it difficult to follow the discussion of wave driving in Sect. 3.3. Most arguments here are based on numerous numbers in several tables. Condensing the key information into a figure would be very helplful for the reader, e.g. to illustrate that AbM and RCTT can not easily be linked to the local quantities.

Based also on the comments from Ref#2, we have shortened the Sect. 3.3. Tables 1 and 2 have been moved to the Appendix (Tabs. A1, A2) as well as the text concerning the seasonality of the trends.
Instead of a figure that Ref#1 suggests, new table (Tab. 1) has been created showing only local residual circulation and wave driving trends on annual basis. One paragraph has been rephrased and one added (P19L453ff) to summarize that neither the annual nor seasonal local trends can be unambiguously linked to the AbM and RCTT trends.

2. This point is related to the one above and concerns the missing clear link between wave drag changes and the residual circulation acceleration (e.g., stated on P1, L26ff). When looking at Fig. A1, on the contrary, there seems to be a strong (anti-) correlation between the meridional resid. circulation velocity and EPFD in the upper part of the Ex region and with GWD in the lower part of the Ex region - as expected. I guess that this is due to the different time scales

considered: The stated missing link concerns long-term trends, the correlation in Fig. A1 is dominated by shorter term variability. Nevertheless, I find Fig. A1 very interesting and would suggest to move it to the main part, or a zoomed in version of it showing just time series in the Ex region and their (non-) correlation. It would then be very interesting to investigate further at which time scale the correlation between residual circulation and wave drag changes breaks down (e.g., by filtering out specific parts of the variability), and maybe similarly for correlations between other quantities (related to my comment 1).

P22, L535: The authors state a "weak correspondence between GWD and RC trends", however Fig. A1 shows a strong correlation between about 18-21 km. I guess this point is again related to the time scales considered (see comment 2 above). Please clarify.

We would like to thank very much the Ref#1 for this minor and specific comment. We followed his/her recommendation and examined how the correlation evolves when filtering out parts of the variability. This is done by smoothing the time series (monthly mean data from 1960 to 2100) by a running average with increasing number of months included in the average. As an illustration, we show in Fig. D1 the evolution of a correlation between GWD and $\overline{v}^*$ in the part of the ExNH region after the correction for a vertical shift of pressure levels. Indeed, the correlation gets smaller in absolute value when filtering out larger parts of the variability.

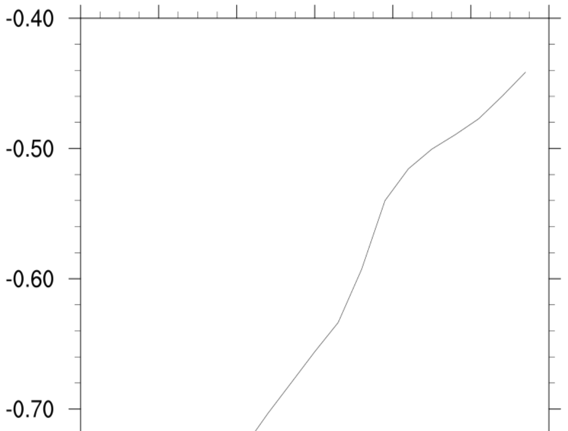

Figure D1. *Evolution of a correlation between GWD and $\overline{v}^*$ in the part of the ExNH region after filtering out parts of the variability and after the correction for a vertical shift of pressure levels. On the x axis is the number of months included in the running average, on the y axis is a value of the correlation.*

For EPFD we observe similar, but smaller decrease - including 240 months in the running average gains a correlation of about -0.7. What is interesting, for the correlation between $\overline{v}^*$ and the total drag on a broader domain (whole ExNH region) the decrease is negligible and for 240 months in the running average we have a correlation of -0.95. This indicates that the compensation mechanism (or *better the whole complex of compensation mechanisms) on a broader domain allows using the quasi-geostrophic scaling (i.e. neglecting the terms with zonal mean zonal wind that would otherwise enter the eq. 4 in the manuscript) with almost perfect precision. This is obviously not true for the individual drag components and especially for GWD.*

As it became clear during the discussion that the wave driving section should be shortened, we decided not to include those new results to the manuscript. The Section 3.3 is now focused to give a clear message that there is no unambiguous link between the local quantities and AbM and RCTT trends.
Nevertheless, in our future research (as also pointed out in the Discussion) we intend to produce a detailed study of the role of GWD for the BDC and transport in the models in general.

3. I find the argumentation that the AoA distribution widens due to AbM and RCTT changes a bit hand-wavy (e.g., P16, L402). Maybe including global trends of AbM and RCTT in Fig. 8 could be useful for making the point clearer?

The argumentation concerning widening has been shortened and reworded (P16L396ff) to list all mechanisms leading to the AoA isoline widening and to highlight the specific role of the decreasing AbM. The AbM trends are not shown in the paper, but a reference to the study Eichinger et al. (2019) has been added (P16L402), where in their Fig. 3 (blueish colors where dominant) the distribution of the effect of the AbM change on the AoA change is shown.

<u>Other specific comments that led to more substantial changes than rewording:</u>

P4, L96ff: I would find a table containing the main information regarding the different models considered helpful.

On P4L100 we have added two references to the papers, where information on the simulations used in our study is summarized in tables.

P4, L116: I don't get the point why RCTTs and AbM starts 1970. Largest RCTTs are about 5 years. Thus for the simulations starting in 1960 it should be possible to have RCTTs already in 1965. Is it because a longer spin-up is needed for having reliable AoA and AbM?

As RCTTs are calculated as backward trajectories, it is unclear at the start how many years the trajectories will need to reach the tropopause. To be on the safe side, we generally use a buffer of 10 years for that. In hindsight, you are right, most trajectories need less than 5 years, but still, not all of them. So to be correct, it should probably be a 6 years buffer, which would give us at the maximum 4 extra years of RCTTs. We are confident that our results are robust enough with the (even) 30 years used in the Ref period and that these 4 extra years will not have a considerable impact on them.

P5, L141: Shouldn't the relation Y=Y' be just the general property of a scalar function?

We think that strictly mathematically speaking Y and Y' are not identical, because they are functions of different variables.

P6, L172 (and throughout the paper): A somewhat picky note regarding the notation of TEM quantities: Usually the star is placed next to the overbar and not below.

Thank you for noticing this. The notation of TEM quantities has been changed through the main text and Appendix.

P14, L336ff: I would prefer presenting percentage changes here (as is the case for the upwelling changes in the next paragraph).

As suggested, we have added information on percentage changes (P14L339ff in the revised manuscript).