Authors' Response to Reviews of

Volcanic Aerosol Modification of the Stratospheric Circulation in E3SMv2 Part II: Brewer–Dobson Circulation

Joseph Hollowed, Christiane Jablonowski, Thomas Ehrmann, Diana L. Bull, Benjamin Wagman and Benjamin Hillman

egusphere-2025-4598

RC: ***Reviewer Comment***, AR: Author Response, ☐ Manuscript Text

1. Reviewer #1

1.1. Author Comments

We thank the reviewer for the careful reading of our manuscript and the useful feedback. Addressing each of these points of discussion has improved our manuscript. Each comment below appears as a reviewer comment (RC) followed by an author response (AR). Closed boxes show text from the manuscript. Red text with strikethrough represents deleted text, and blue text with wavy underlining represents new text. Ellipses (. . .) represent text portions from the manuscript which have been omitted from snippets here for brevity.

1.2. Comment 1

RC: ***The top of the model at 60 km is located in the mesosphere and low in comparison with other state-of-the-art stratosphere-resolving models. I suggest adding a comment on the possible effects of this (relatively) low top, for instance on the BDC driving by gravity waves, the sponge layer, . . . It could be perhaps affecting the longest-lived air parcels and explaining the misbehavior mentioned in the paper (missing trajectories, bouncing between the two hemispheres, e.g. L202-204).***

AR: We thank the reviewer for their insight on this feature of our results, and agree that this is a useful point of discussion. We have added text regarding this matter near the referenced discussion of the outlier RCTT trajectories, and also clarified the behavior of our RCTT solver at the model top:

> . . . where the function f is a linear interpolator in t and x of the monthly-mean residual velocity components (v^* for $x \equiv y$, and w^* for $x \equiv z$). As a necessary boundary enforcement, on each step of the solver the vertical trajectory position z_{n+1} is clipped to the range $[z_{\max}, z_{\min}]$, which are the model top and lowest model level height, respectively. This causes trajectories that would prefer to exit vertically through the model top to instead "slide" along the top model level until they descend.
>
> . . .
>
> It turns out that while the 10-year integration time is sufficient in the average, some individual trajectories get "stuck" in an oscillating motion between the south and north polar upper stratosphere~~, in a~~. This could be due to several effects, including (1) a realistic reflection of the seasonal cycle of the deep branch of the BDC, (2) an artifact of the model-top boundary enforcement in the RK4 solver, or (3) an artifact inherited from the underlying wind fields, due to the sponge layer of E3SMv2's relatively low 60 km model top affecting wave activity (and thus the residual circulation) in that region. We did not investigate the cause of this effect in detail, but also did not find that it impeded our analysis. The

> practical effect is that these outlier trajectories may not reach the tropical tropopause within ten years, resulting in missing RCTT data at certain (ϕ, p) gridpoints near the poles. Averaging over many launch times generally fills in the missing data. The fraction of affected gridpoints seemed to increase with growing h, though we did not perform a convergence test to fully understand the behavior.

1.3. Comment 2

RC: ***L115-125 (and Fig. 1): This discussion on the simulations is confusing. Why are a 3-year and a 7-year simulations needed? Why not carry out a single control simulation spanning the entire time period 1981-1998, and branch from it the different ensemble members, with and without perturbation? Please explain this in a simple and concise way in the paper.***

AR: We appreciate the fact that this can be confusing to readers, and hope we can clarify it succinctly. The simple answer is that the datasets used here resulted from a simulation campaign within a large collaboration, which was serving the interests of several research projects other than ours. For reasons irrelevant to this paper, the boundary between the PS1 and PS2 was also a branch point for other ensembles. The important point for our purposes is that, when combined, they represent a practically continuous timeseries.

In hindsight, this level of specification was never necessary here, since it only poses a potential confusion point while contributing no important details. We have trimmed the text, removing all mention of the PS1 or PS2 simulations in favor of mentioning a single “control simulation". We retained the detail about a temperature perturbation present in the data near the PS1-PS2 branch point, for transparency.

The point about the discontinuous calendar (and background volcanic forcing) at the LV initialization point is also a potential confusion, but in our opinion this needs to be said for transparency. The reason for this is slightly complicated, and we find it more efficient to reference the paper which presents the control simulations from which our work derives (Ehrmann 2024) than to insert an further explanation. The important point, as mentioned, is that all of these differences are irrelevant to our studies since they cancel out when looking at the LV−CF impact.

The next text is as follows:

> Because the LV ensembles were all initialized on June 1 1991, we were required to concatenate the v^* and w^* time series from the LV runs, and the control simulation ~~run~~ from which the perturbed LV ensemble initial conditions were seeded, in order to obtain a 10-year history. ~~We refer to the latter as the first prior simulation (PS1). The PS1 included only three years of model integration prior to the LV initialization, and so seven more years were also required from a second prior simulation (PS2), which in turn seeded the PS1. Combining these three datasets was needed to maintain continuity in the residual velocity fields in time.~~ The concatenation of the ~~PS1, PS2~~control simulation and a single LV ensemble member is illustrated in Fig. 1 for a midlatitude gridpoint at 10 hPa. RCTT integration bounds for selected dates are plotted as a reference.
>
> The ~~PS1 and PS2~~control simulation~~s are~~was generated from~~built upon~~ E3SMv2-SPA with an equivalent configuration ~~equivalent~~ to the LV and CF runs, though in principle there are discontinuities at the concatenation point~~s~~. In part, this is because the ~~PS2-to-PS1 and the PS1-to-~~LV ~~transitions~~ ensemble initialization involved random temperature perturbations on the order of 10^{-14} K to drive differences between the members.~~, as those times both served as points of ensemble initial condition generation (the PS1 is a member of different ensemble, which is not discussed here)~~There is also a second instance of a temperature perturbation of the same magnitude on Jan 1 1988 in the control simulation that was

inherited from the parent dataset, and is unrelated to the present study. In addition, while the winds across the ~~PS1-to-~~LV ~~transition~~initialization point are approximately continuous, non-volcanic climate forcing sources are not. This is because the ~~PS1~~ control and LV calendars are different, and needed to be manually aligned (again, this is due to an inherited experimental design that is unrelated to the present study; see Ehrmann et al. (2024) for full details). Because the RCTT results from long-time integrations over monthly-averaged v^* and w^*, we expect the consequences of ~~this~~these features on the results to be ~~minimal~~negligible. To whatever extent ~~this~~ that these discontinuit~~y~~ies ~~is~~are present in the recovered transit times, ~~it is~~they are present equally in the LV and CF ensembles, and thus will be removed when taking the (LV−CF) impact.

Consequently, Fig 1 has also changed so that the PS1 and PS2 simulations are no longer referenced. The new figure and caption are shown below.

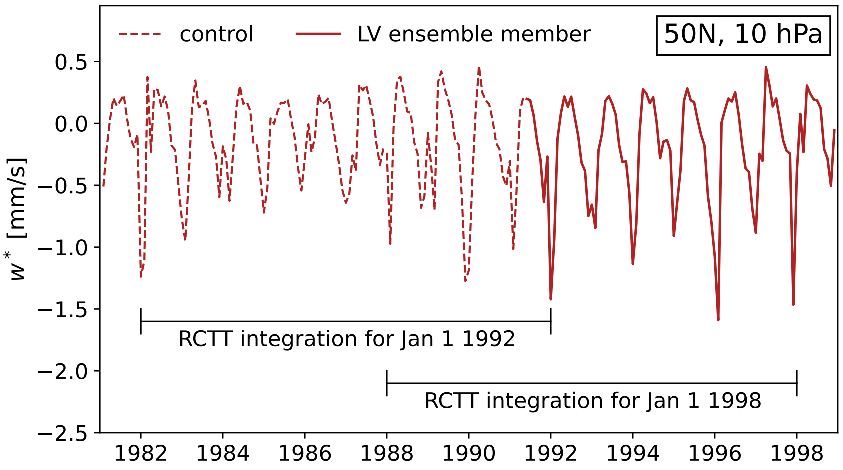

Figure 1: Concatenated time series of the residual vertical velocity w^* at 50°N, 10 hPa for a single LV ensemble member (June 1 1991 to Jan 1 1999; ~~black~~ solid dark red line), and the ~~PS1~~control simulation (Jan 1 198~~8~~2 to June 1 1991; dark red dashed line)~~, and the PS2 simulation (Jan 1 1982 to Jan 1 1988; solid blue line)~~. Two example RCTT integrations are annotated as bracketed black lines. The RCTT on Jan 1 1992 is computed by residual velocity integration over the decade 1982–1992, and likewise for the RCTT on Jan 1 1998 and the decade 1988-1998.

1.4. Comment 3

RC: ***Throughout the paper, the term 'tracer transport' is used to denote 'advection by the residual circulation', in opposition to mixing. In my view, both advection and mixing lead to 'tracer transport' (while mixing alone does not lead to mass transport). Since this term is commonly used in the literature, and is the standard nomenclature of the TEM terms in Eqs. (5) and (6), I recommend to use it instead. For example in L154, L247, L254, L322, Fig. 10 title of panel (b) and figure caption.***

AR: We agree that "advection by the residual circulation" is the appropriate term to use. We appreciate that the reviewer identified line numbers for us; we have corrected the mentioned occurrences, and checked the remainder of the manuscript for others. We also updated Figure 10 accordingly.

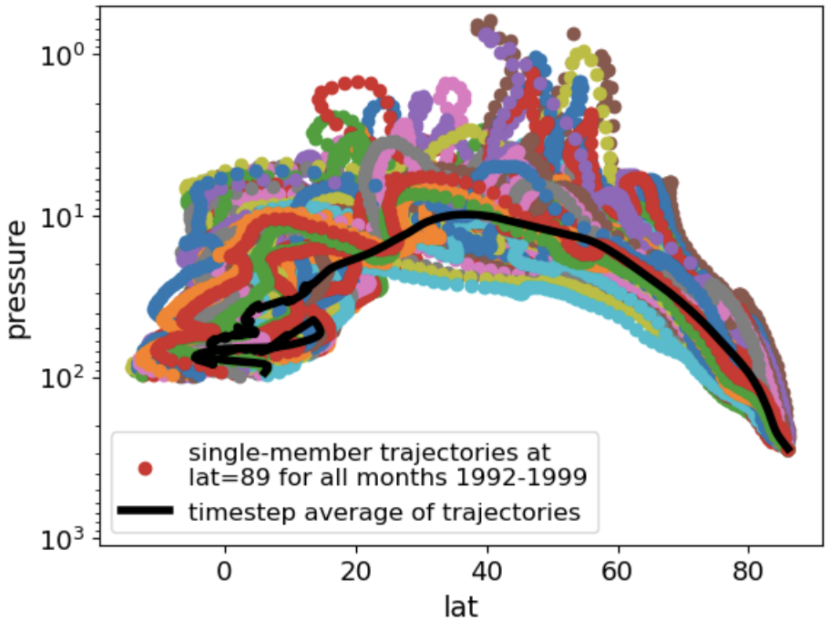

Figure 2: A demonstration of the averaging artifacts in averaged trajectories near the tropical tropopause, supplied for context in our response to Reviewer Comment 4. Colored points represent individual trajectories from ensemble member 9, originating from a position near the tropopause at 89°N. The thick black line shows the average. The average is representative of the trajectory distribution for most of it's range, but becomes chaotic near the tropical tropopause.

1.5. Comment 4

RC: ***Fig. 4: Why do some trajectories seem to stop in the tropical lower stratosphere (below 30 hPa) with transit times below 1 year?***

AR: Those are visual artifacts from the trajectory averaging process. Unfortunately, we did not compute trajectories from the ensemble-averaged residual circulation fields, but rather computed trajectories at the member-level, and then attempted to average those trajectories for visualization in this figure. This causes discontinuous behavior in the average near the tropical tropopause, since the individual trajectories become highly variable in that region. To illustrate what we mean, we have provided the reviewer with a demonstration in Figure 2. There, we show the average (thick black line) of many trajectories (scatter plots corresponding to the RK4 timesteps of each solution) from one origin position and one ensemble member. We can see that the average is well-behaved throughout most of the atmosphere, except for the tropical tropopause, where the trajectories are highly variable and the average becomes chaotic. This averaging effect gives rise to the visually discontinuous trajectories in Figure 4.

To address this, we have replaced Figure 4(b) with the same measure for a single ensemble member, rather than the ensemble average. In that case, these artifacts are cured. For the temporal average of Figure 4(a), there is no way to remove these artifacts without doing a highly nontrivial amount of work to implement new processing codes, and process the data from scratch. We feel that this reprocessing is not necessary for the follow reasons:

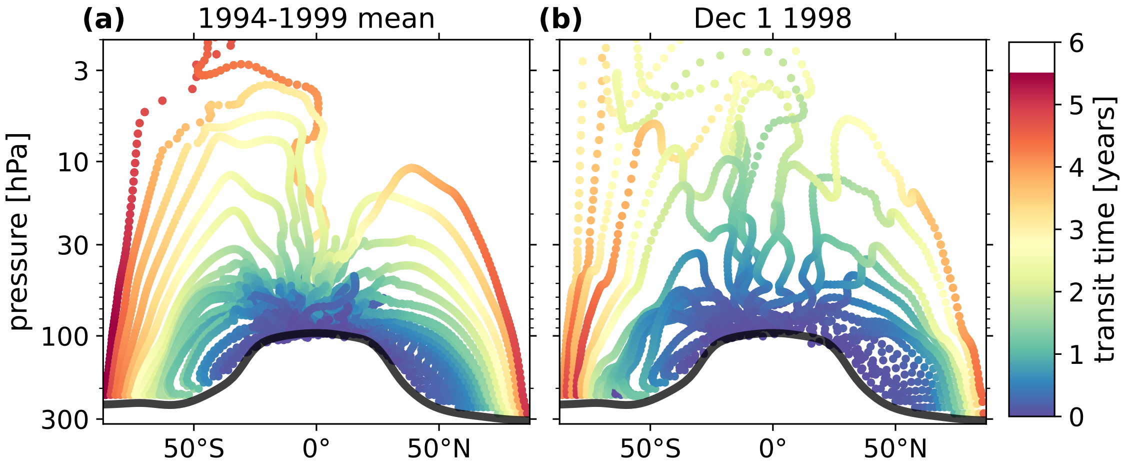

Figure 3: Select single-member (CF ensemble member 9) RCTT trajectories for gridpoints near the tropopause, every 2° in latitude, from 20°–90° on each side of the equator. **(a)** the 5-year mean trajectories **(b)** the trajectories computed on Dec 1 1998. The colors indicate the transit time along the trajectories. The ensemble member used was ensemble member 9. Note that visually discontinuous trajectories near the tropical tropopause in panel (a) are only an artifact of the trajectory averaging, which become highly variable in that region. These artifacts are not present in the individual member trajectories, and do not affect the ensemble-mean RCTT in any way.

1. The artifacts affect a visually small region, and are not material to the understanding and interpretation of the figure as a whole, or our results, or the description of the RCTT technique.
2. We have added a comment in the text which points these artifacts out explicitly, and explains their origin.
3. We have improved Figure 4 such that the issue is minimized in panel (a), and removed entirely from panel (b), by choosing a single ensemble member. The improved figure is shown above, with an improved caption.
4. The problem does not arise in all trajectories (e.g. the trajectories chosen for visualization in Figure 9 do not exhibit this problem).
5. These artifacts *only* affect the exercise of visualizing the trajectories. They have no bearing on the correctness of the ensemble-mean RCTT. The RCTT itself (which is an ensemble average of the *total time-length* of each trajectory for a given position) is correct and unaffected.

We thank the reviewer for their diligence in noticing this issue, as it encouraged us to look deeper and understand the effect more fully.

1.6. Comment 5

RC: ***L281-282: ‘while the deep branch in the summer hemisphere is weakened’. I suggest specifying ‘the upwelling in the summer hemisphere is weakened’.***

AR: We agree that that would improve the clarity of this result. The text has been modified as

> The result is that the volcanic effect lacks the seasonality of the background residual circulation, and does not always project onto the CF Ψ^*. In August of 1991 and 1992, the robust $\Delta\Psi^*$ indicates acceleration of the shallow branch of the BDC in both hemispheres, while the deep branch in the summer hemisphere is weakened (specifically, the upwelling in the summer hemisphere above 10 hPa is weakened). During January of 1992, the southern shallow-branch cell is enhanced along its northern edge, the Ψ^* zero line moves northward, and the positive overturning circulation in the SH is decelerated.

1.7. Comment 6

RC: ***The enhanced residual circulation is seen only above 10 hPa in boreal winter. At lower levels it is actually weakened, as shown by the negative streamfunction response over the region of positive control streamfunction in Fig. 6. In the summer months instead there is an acceleration also in the lower stratosphere.***

AR: That is correct, which is precisely what we described in the section of the text included in our response to the previous comment; "...the volcanic effect lacks the seasonality of the background residual circulation, and does not always project onto the CF Ψ^*". As for why there is some asymmetry in the signature of the enhancement between the northern and southern hemisphere, as the reviewer points out, that is largely due to the eruption occurring in the NH at 15°, which we discussed at length in the discussion surrounding Sect. 5.2. We do not find that any specific text needs to be modified per this point.

1.8. Comment 7

RC: ***Figure 9: In panel (c), the RCTT in the control simulation trajectories seem to stay constant after crossing the 20 hPa level, is this because the colorscale gets saturated? In this case it could be extended (otherwise it seems that they arrive to the final point with the same RCTT as the perturbed ensemble).***

AR: Yes, the colorscale was saturated. Despite this, there is enough information in the figure to know that the two overlain trajectories in fact do *not* arrive with the same RCTT, since the red/blue colored contours in the background show the difference in arrival times for each gridpoint in the meridional plane (one of which the displayed trajectory pairs correspond to). However, we appreciate this feedback and have adjusted the figure for more clarity by extending the trajectory colorbar to 5 years (rather than 4), and choosing a new colormap which visibly shows the difference in trajectory age both near 30 hPa, and at the trajectory endpoints. The new figure is shown below.

1.9. Comment 8

RC: ***Figure 10: In Fig. 7e the SH older-age region (SHLS) extends to July 1993, so Fig. 10 should be extended until that date to show the 'abrupt' termination of this feature. Is this what is referred to in L364-366? In that line it says the termination happens in June 1992, which is not what we see in Fig. 7e. Please clarify when does this aging terminate and show it in a figure (also 'terminates after one year' in L373 should be changed to 'two years'?).***

AR: We apologize for the errors regarding this point, and appreciate the reviewers keen eye in finding them. "June 1992" in L365 should instead read "July 1993", in agreement with Fig. 7(e). Likewise, "one year later" in L373 should instead read "two years later". We have made those changes, updated Figure 10 to show the full extent in time of the anomaly in the RCTT impact (figure shown below), and improved our language around

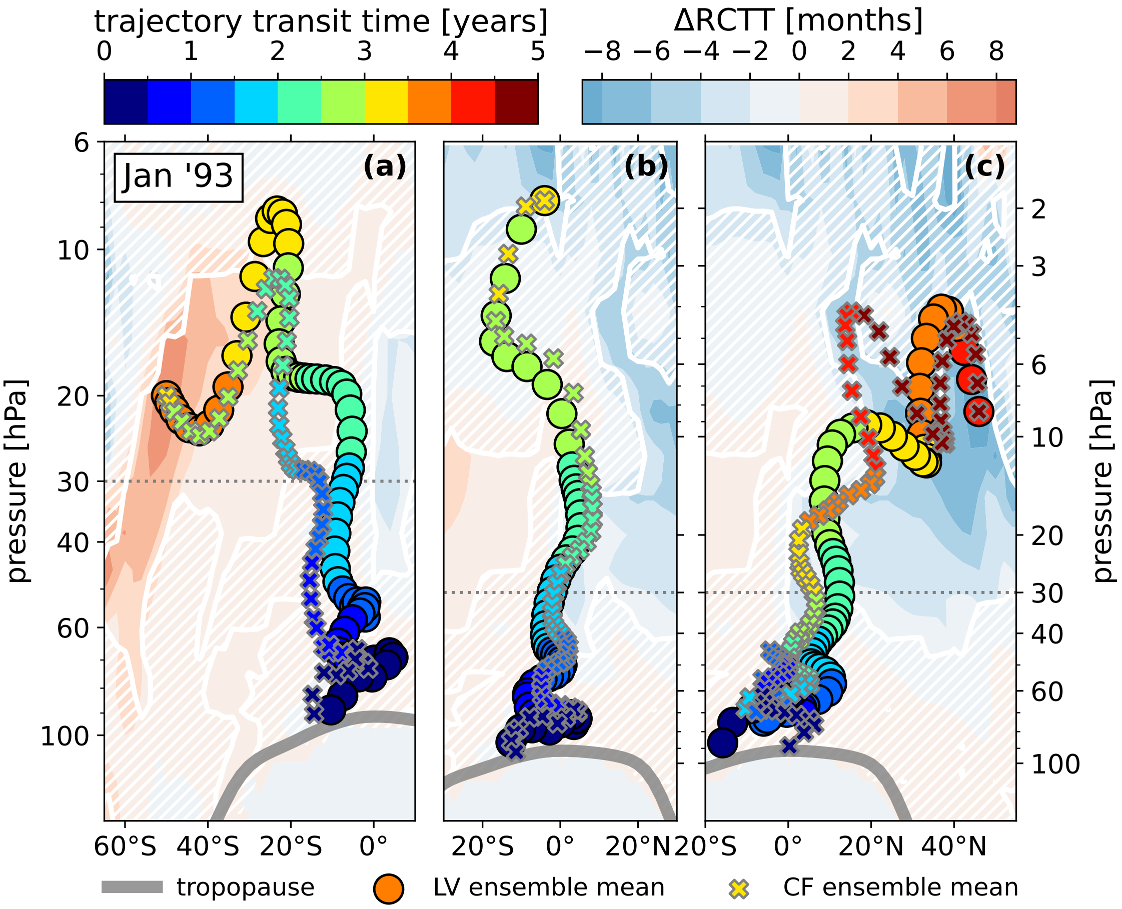

Figure 4: The replacement Figure 9, per Reviewer Comment 7. The caption has not changed.

the interpretation of this point as follows:

> This “seasonal mechanism" also explains why the SHLS aging abruptly ends near ~~June 1992~~July of 1993 (as seen in Fig. 7(e))~~; by this time, the southern w^* zero line has moved sufficiently far north such that the southern $\Delta w^* < 0$ feature no longer opposes the sign of the background motion (Fig. 10(a))~~. Note that the positive impacts on the local transport flux in the SH of Fig. 10(b) are bound by the $w^* = 0$ line; these impacts arise during austral summer, and dissipate during austral winter as the meridional oscillation of the $w^* = 0$ line modulates the relative sign between Δw^* and w^*. This process repeats for as long as the volcanic forcing is significant, or about two years, after which the northern motion of the $w^* = 0$ line starting in January 1993~~This pulls relatively young air into the southern BDC cell, which~~ ushers out the roughly two-year-long era of diminished transport in the SHLS. This signal then arises as a delayed positive ΔRCTT, which itself ends equally as abruptly about nine months later.
>
> . . .
>
> We observed that a robust feature of increased age develops in the SHLS soon after the eruption, and ceases abruptly ~~one year~~two years later.
>
> . . .

1.10. Comment 9

RC: ***Fig. 10 (less important than previous comment): For consistency with Figure 7e, this figure could show 40 hPa instead of 30 hPa. Finally, the black arrows in this figure are not necessary (the upwelling/downwelling regions are identified by the w*=0 contours) and in my opinion make the figure confusing, so I suggest removing them.***

AR: We agree, and decided to move Figure 7(e) to 30 hPa, rathe than move Figure 10 to 40 hPa. The two figures now show the same vertical position at 30 hPa.

We respectfully disagree with the reviewer about the necessity of the black arrows in Figure 10(a). The reason we included these is that by only plotting the $w^* = 0$ line, there is no visual indication of which side of the line is positive or negative in w^*. This information could of course be inferred by any informed reader, but we prefer if the information is presented explicitly. We found that drawing positive/negative w^* contours was more disruptive to the figure legibility than the arrow annotations.

1.11. Comment 10

RC: ***Figure 12: Suggested modification of the schematic: Remove the annual mean picture, does not help to make the argument and it is not a realistic situaltion. Mark the turnaround latitudes (with vertical dotted/dashed lines, or shading the upwelling region), since the important point here is whether the anomalous downwelling falls into the climatological upwelling or downwelling region.***

AR: While we agree that the annual mean picture is not necessarily realistic situation (though it could be argued that it represents an equinoctial configuration, as the BDC transitions from one phase to another), we find it valuable to include since it illustrates exactly why this seasonal damping effect is missed when inspecting annual averages. In other words, it visually demonstrates how a naive conception of the interaction between volcanically-induced circulation and the BDC leads one to expect only *negative* aging impacts globally. Indeed, we feel that it is exactly this annual-mean conception that made our findings of the SHLS aging and the seasonal BDC damping hypothesis counter-intuitive and surprising to us in the course of our research. This is why we emphasized in the text that “the anomalous meridional circulation induced by the volcanic forcing (red arrows) only projects onto the background BDC in the annual mean..."

We appreciate the suggestion of adding a visual marker to the schematic for indicating more clearly how the anomalous circulation and background circulation relate to each other in sign. Rather than annotate the turnaround points or highlight the regions of upwelling, we've decided to instead only highlight the regions where the sign of Δw^* and w^* disagree (specifically, regions of anomalous downwelling atop regions of background upwelling). The new figure is shown below. This also involved some minor changes in the text:

> As a result the relative sign between Ψ^* and $\Delta\Psi^*$, as well as w^* and Δw^* changes seasonally, which is illustrated as the yellow hatched regions of $\Delta w^* < 0 < w^*$ in Fig. 12. This is precisely the story told in Figs. 6,10, and 11.

1.12. Comment 11

RC: ***Section 5.2: The damping of the seasonal cycle of AoA is interesting but could be shown more clearly. Fig. 11 shows that the eruption impact on AoA has the same sign as the boreal summer climatological tendency. But how that implies a reduced seasonality is not clear. Could you maybe show the reduced seasonal cycle for some specific region where it is most evident?***

AR: We experimented with various ways of showing the seasonal damping, and the AoA tendency shown in Figure 11 was the most clear. The extent to which this figure shows a reduced seasonality is in the fact that during the seasonal transition from boreal summer to boreal winter, the effect of the mean volcanic impact on the AoA tendency from July to January was to dampen the transition, i.e. to nudge the winter distribution toward that summer distribution.

The reviewer is right to find that this is an incomplete demonstration; our intention here was to demonstrate a necessary, but not sufficient, observable effect under the assumption that the seasonal damping is true. Given our current datasets, this is the best that we can do, which is the reason why we call our idea a seasonal damping a hypothesis. As we suggested in the Discussion section, a separate experiment would need to be designed to identify and demonstrate the effect clearly, one which uses a steady and hemispherically symmetric forcing source averaged over several years. Our goal here was to hypothesize the existence of a explanatory mechanism, and use the data to make a plausible claim that this mechanism could apply to our Pinatubo simulations.

We feel that we did clarify this in several locations in the text, but we appreciate if it was not clear enough as-is. We have changed the text in the caption of Figure 11 as

> ~~Demonstration of~~Indication of the hypothesized BDC seasonal cycle damping during the first 8 months of the volcanic experiments

and have adjusted the text at the end of Section 5.2 to be more clear:

> If this seasonal–damping mechanism is true, then ~~surely~~ it should be detectable by averaging the anomalous residual circulation and/or AoA over several years, given a steady and symmetric forcing source. The effect should also be accompanied by corresponding modifications to global EP wave activity, which may in turn offer a more generalized interpretation of the results of Part I . . .

1.13. Comment 12

RC: ***L199-201: This is already discussed above, consider reducing or removing and referencing back.***

AR: We thank the reviewer for pointing out this oversight. The text has been reduced as

> For the present experiments, we launched RCTT trajectories for each month from June 1 1991, to Jan 1 1999. For each trajectory, the~~This~~ procedure above was~~must be~~ completed for a total backward-integration domain spanning ten years, as discussed in Sect. 2.~~prior to the point when the RCTT is being estimated. A ten–year domain is chosen since we do not know *a-priori* how long the longest transit times will be. However, we can reasonably expect them to typically be less than a decade. For the present experiments, we launched RCTT trajectories for each month from June 1 1991, to Jan 1 1999.~~

1.14. Comment 13

RC: ***'dampening' should be 'damping' in L12 and L34.***

AR: These have both been corrected, and we searched the manuscript for other occurrences of this mistake.

1.15. Comment 14

RC: ***L380: 'impact of' should be 'impact on'***

AR: This has been corrected.

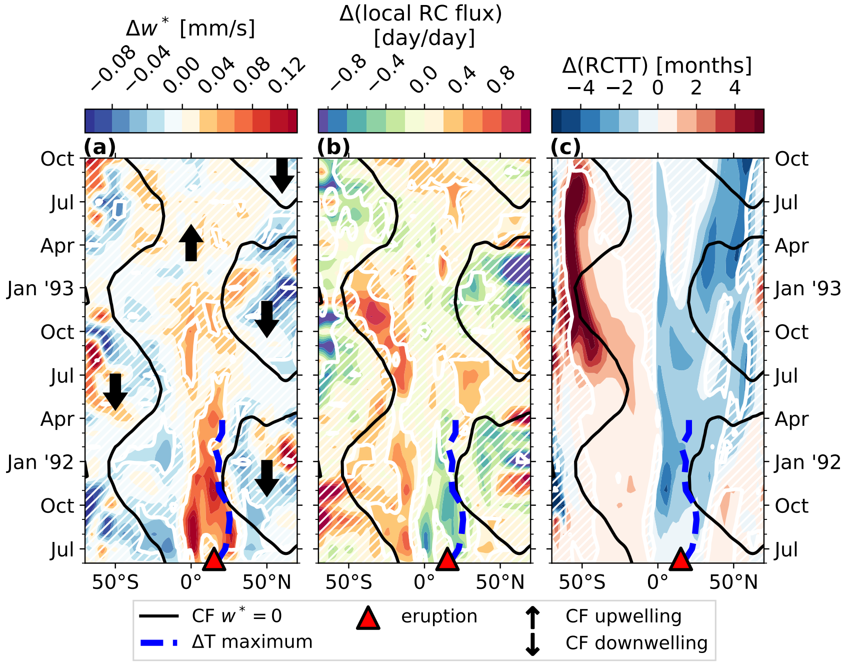

Figure 5: Relationship between the ensemble-mean impacts in vertical residual velocity, and aging by ~~transport~~residual circulation advection at 30 hPa from July 1991 to ~~January 1993~~October 1993. In all panels, solid black contours are drawn at $w^* = 0$ in the CF ensemble mean, a thick dashed blue line shows the latitude of maximum ΔT from July 1991 to April 1992, and a red triangle shows the position of the Mt. Pinatubo eruption. White contours are drawn at 95% significance, and regions of insignificance are filled with white hatching. **(a)** Δw^* in mm s^{-1}. Thick black arrows show regions of upwelling and downwelling in the CF data. **(b)** impact in AoA local ~~transport~~ residual circulation advective flux (Eq. (5) + Eq. (6) in days per day. **(c)** RCTT impact in months.

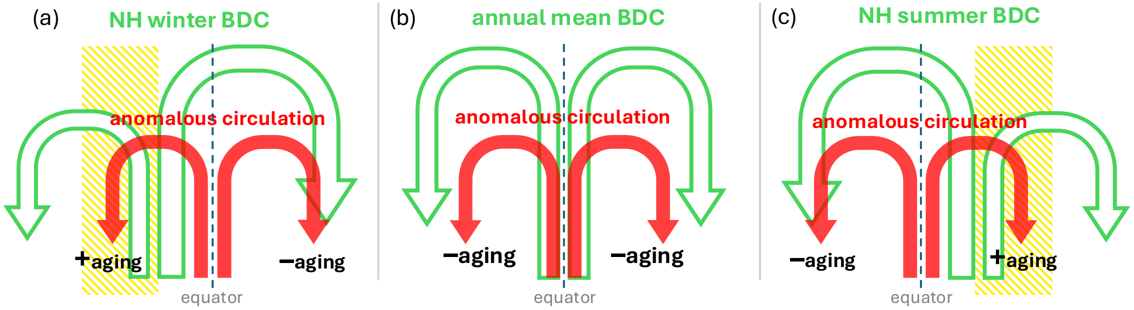

Figure 6: Schematic of the proposed BDC seasonal damping mechanism. Hollow green arrows represent the advective shallow BDC branches (Ψ^*) in the meridional plane, and solid red arrows represent the anomalous meridional residual circulation ($\Delta\Psi^*$) induced by a volcanic or similar forcing. Panels (**a**), (**b**), and (**c**) show the situation for boreal winter, the annual mean, and boreal summer, respectively. The sign of the resultant AoA impact is shown as "+aging" and "–aging". Regions where $\Delta w^* < 0 < w^*$ (anomalous downwelling lies in a region of background upwelling) are filled with yellow hatching.

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1. Reviewer #2

1.1. Author Comments

We thank the reviewer for the careful reading of our manuscript and the useful feedback. Addressing each of these points of discussion has improved our manuscript. Each comment below appears as a reviewer comment (RC) followed by an author response (AR). Closed boxes show text from the manuscript. Red text with strikethrough represents deleted text, and blue text with wavy underlining represents new text. Ellipses (. . .) represent text portions from the manuscript which have been omitted from snippets here for brevity.

1.2. Comment 1

RC: ***line 1 and lines 16-17: Since volcanic eruptions are very diverse, I would appreciate if the authors would specify what kind of eruptions they are investigating upfront (for example, in the case of this study, explosive volcanic eruptions injecting large amount of SO2 into to stratosphere).***

AR: We have adjusted the language in L1 and L16:

> Great attention has been paid to the short-term climate response following ~~large~~explosive volcanic eruptions, in order to understand effects on zonal winds, the polar vortex, and surface temperature across latitude.
>
> . . .
>
> Large volcanic eruptions are capable of injecting very large quantities of sulfate aerosols into the stratosphere. In the years following such an eruption, temperature forcing by stratospheric volcanic ~~sulfate~~ aerosol~~s~~ concentrations plays an important role in the interannual variability of the climate, and leads to significant and persistent anomalous climate states.

1.3. Comment 2

RC: ***line 12: I would recommend specifying that the hemisphere opposite the eruption in the southern hemisphere as the authors do not simulate SH eruptions. This would be consistent with the wording of point 3 in Section 7.***

AR: We have adjusted the text to read:

> We also observe a localized increase of AoA near 20–100 hPa in the southern hemisphere (the hemisphere opposite the eruption), which we attribute to a dampening of the seasonal BDC cycle by the volcanic aerosols.

1.4. Comment 3

RC: ***line 66: The authors define the abbreviation SAI later in the manuscript but I would appreciate they did it here already.***

AR: This was an unintentional omission. The text on this line has changed to:

> In the case of the stratospheric aerosol injection (SAI) geoengineering simulation studies of of Richter et al. (2017) . . .

1.5. Comment 4 & 5

RC: ***lines 89-90: I assume the model is fully coupled.***

RC: ***line 98: Although the authors specify the altitude and the latitude of the volcanic SO2 injections in Part I, I would appreciate if they did it here as well.***

AR: We have modified the text as:

> Briefly, we use the fully-coupled E3SMv2-SPA model (Brown et al., 2024) to simulate the eruption of Mt. Pinatubo as an emission of sulfur dioxide (SO2) over 6 hours and 9 grid cells between 18 and 20 km near 15° N, ~~which~~and evolve~~s~~ the resulting ~~volcanic sulfur dioxide (SO2) and~~ sulfate aerosols via a prognostic treatment . . .

1.6. Comment 6

RC: ***line 171 (Eq. 11): The authors explain the variable X as the forcing by 'parameterized processes and diffusion', i.e. unresolved forcing. Later (for example in Fig. 5c and lines 247-251) they discuss this parameter only in terms of diffusion. Does this mean that unresolved processes other than diffusion are negligible in the TEM framework? If so, I would be interested in reading about it. And if not, I would be interested in reading more about which processes are not resolved and their relative importances.***

AR: We thank the reviewer for the thoughtful question; it is true that we began speaking of this quantity as purely a diffusion without clear explanation. AoA is a passive tracer, and the only parameterizations which would influence its concentrations are the model's shallow and deep convection schemes and boundary-layer mixing, which are predominantly tropospheric in their direct tracer effects, and are negligible in the stratosphere. The influence of parameterized gravity-wave drag in the stratosphere is an indirect one, entering through the wind fields, and in turn the residual circulation and resolved eddy transport, which are already accounted for in the TEM balance. For these reasons, yes we assume the term $\overline{X}$ to be mainly a diffusion.

We did not explicitly test this assumption (we did not have the data outputs to evaluate every possible parameterized contribution to $\overline{X}$), but at least two previous works that we found instructional during our research offer some justification.

First, in Abalos et al. (2017) (https://doi.org/10.1175/JAS-D-17-0135.1), the authors use a TEM framework to decompose tendencies of the artificial "e90" tracer in WACCM. While this tracer is different from AoA, it is passive, and is designed to evaluate troposphere-stratosphere coupling and tracer transport in the UTLS region. There, the authors show the same TEM terms that we do, but further split $\overline{X}$ into contributions from parameterized convection, boundary layer diffusion, and a “residual". They argue that this residual is almost entirely explained by implicit numerical diffusion, plus some interpolation and averaging contributions from the fact that the TEM analysis is performed on pure pressure levels and daily data (which differs from the hybrid levels and much finer timescale used by the dynamical core in the simulation). They additionally show that the convection and boundary-layer diffusion transport is confined to the troposphere, and perform an additional test which implies that there are minimal transport contributions from gravity waves in the residual (see their discussion around their Fig. 3).

Second, Dietmüller et al. (2017) (https://doi.org/10.5194/acp-17-7703-2017) used the RCTT technique to evaluate the aging contributions of residual circulation advection, resolved mixing, and diffusion. In our study, we were unable to do this same decomposition of resolved mixing and diffusion, because we did not implement the same ability to integrate the eddy-tracer flux divergence along the RCTT trajectories, as Dietmüller et al. did. In any case, they likewise consider the “residual" remaining after accounting for resolved mixing and residual circulation advection to be an “aging by diffusion". That is, they identify all unresolved tracer transport with a diffusion. This is essentially the exact same assumption that we are making about $\overline{X}$ in the TEM framework, though they are obtaining it in a slightly different way.

We’ve added a brief mention of these details in the text near Eq. (11) so that this is more clear:

> …The parameterized production and loss $\overline{S}$ are those defined in Sect. 3.1. Informed by Abalos et al. (2017) and Dietmüller et al. (2017), we will henceforth refer to $\overline{X}$ as purely a diffusion, since contributions from other parameterized sources, namely shallow and deep convection and boundary-layer mixing, are essentially absent in the stratosphere. Further, gravity wave drag parameterizations in the stratosphere do not act directly on tracers, but rather indirectly through the wind fields, and thus the residual circulation and resolved eddy transport.

1.7. Comment 7

RC: ***line 220: ‘[...] whereas our reference point is close to the surface.’ Can you say that 700 hPa (ca. 3 km) is close to the surface? I suggest replacing ‘close’ with ‘closer’.***

AR: We adopted this change:

> This is primarily because those studies compute the AoA with respect to a reference point at the tropical tropopause, whereas our reference point is closer to the surface.

1.8. Comment 8 & 9

RC: ***line 254: For clarity, I would appreciate if the authors specified panel (a) of which figure they are referring to (Fig. 3a).***

RC: ***line 257: For clarity, I would appreciate if the authors specified in which figure the ‘relatively small local diffusion tendencies’ can be seen (Fig. 5c).***

AR: We made both of these instances more clear, and also made a similar change for clarity on L247:

> ~~This figure~~Figure 5 tells the same story as Fig. 3,...
> ...
> To be clear, this difference is due to the local nature of the measurement; rather than being integrated, ~~panel (a)~~Fig. 5(a) shows the age by transport given the simultaneous, pre-mixed AoA distribution.
> ...
> We did not perform this calculation, but the relatively small local diffusion tendencies we see throughout the tropical column in Fig. 5(c) are consistent with the result of Dietmüller et al. (2017) ...

1.9. Comment 10

RC: ***line 259: For clarity, I would appreciate if the authors referred to Fig. 5e in the context of the net tendency.***

AR: We have adjusted the text as:

> The net tendency is much larger in seasonal averages than it is in the annual mean of Fig. 5(e).

1.10. Comment 11

RC: ***lines 275-277: From Fig. 6, it looks like that not only has the significance of the features discussed 'notably diminished by August of 1992', but almost disappeared in the tropics (as the authors note in line 296).***

AR: That is correct. We feel that it is appropriate to leave the text here as it is, since it is not an inaccurate description, and also acknowledges that there do still remain some small regions of significance in w^* in the tropics in August 1992.

1.11. Comment 12

RC: ***Figure 13: Since the bulk of study focuses on the Pinatubo eruption (10 Tg SO2), I wonder why the authors choose to normalise the relative max impacts in the lower sub-panels to the results from their 15 Tg SO2 simulations.***

AR: That decision was made only as a matter of taste, so that the eruption of the largest magnitude had the largest relative impact. However, that decision was made *a-priori*, before we knew that the resulting trends would not always be monotonic. In hindsight, it would have made more sense to normalize the data with respect to the 10 Tg run, so that the results may be more intuitively compared to the eruption presented throughout the paper. We have made this change and replaced Figure 13, and we thank the reviewer for an effective suggestion. The new figure is shown below.

1.12. Comment 13

RC: ***lines 486-487: The authors expand on their study of the Pinatubo eruption by simulating similar eruptions (in terms of location and injection altitude) of different emission magnitudes (Section 6). In Section 7 (lines 486-487), they further suggest that a future study could explore the sensitivity of their results to variations in eruption latitude and season. I would like to add that exploring the sensitivity to injection altitude would be interesting. Particularly in the light of the recent study by Toohey et al. (https://doi.org/10.5194/acp-25-3821-2025) which showed how the lifetime of volcanic aerosols in the stratosphere is very sensitive to the injection altitude.***

AR: We appreciate this suggestion by the reviewer. After reviewing the mentioned paper, we have added some

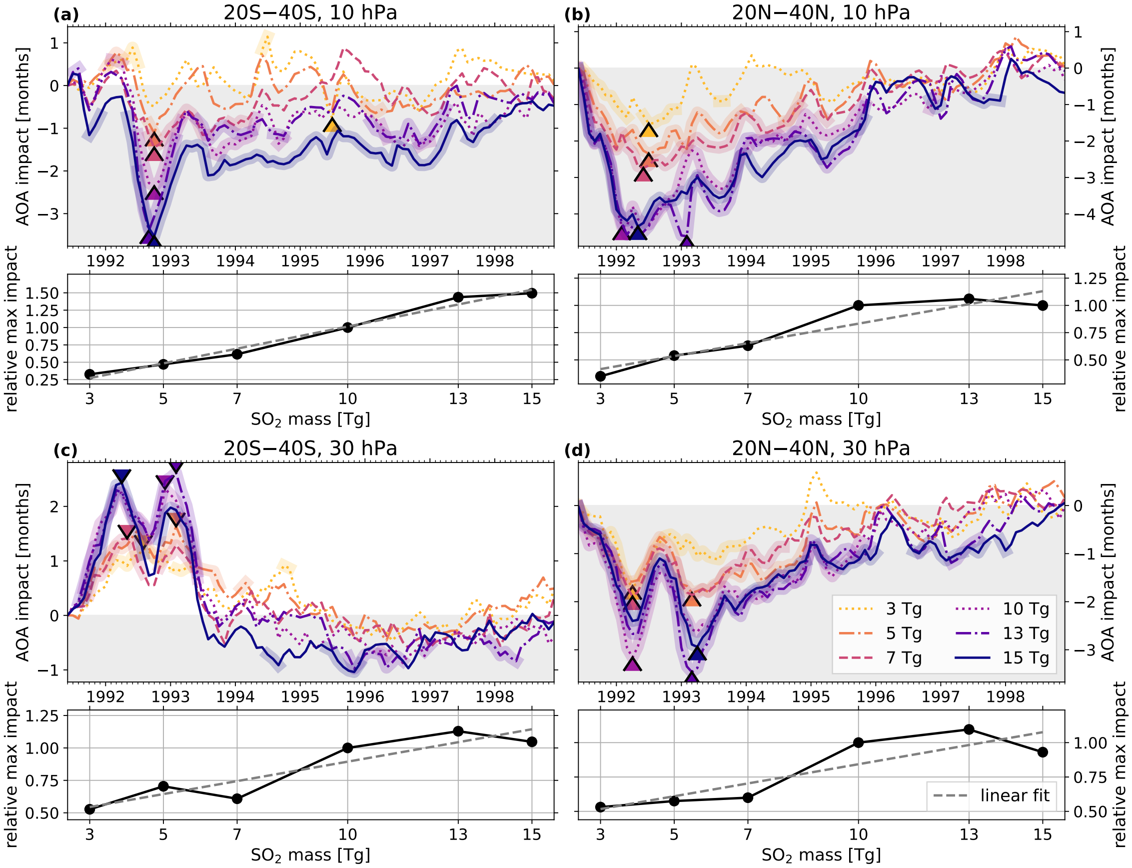

Figure 1: The replacement Figure 13, per the Reviewer Comment 12. The caption has not changed.

text regarding this idea, and a reference as follows:

> It would also be enlightening to understand how exactly wave activity balances (or fails to balance) the global residual circulation response, which we only described in a more local sense in Part I. Finally, there may be interest in exploring the sensitivity in the age impacts analyzed here to the SO_2 injection altitude, which was recently shown by Toohey et al. (2025) to strongly control to the aerosol lifetime (and thus forcing timescale) in the stratosphere.