Authors' Response to Reviews of

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

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egusphere-2025-4598

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

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

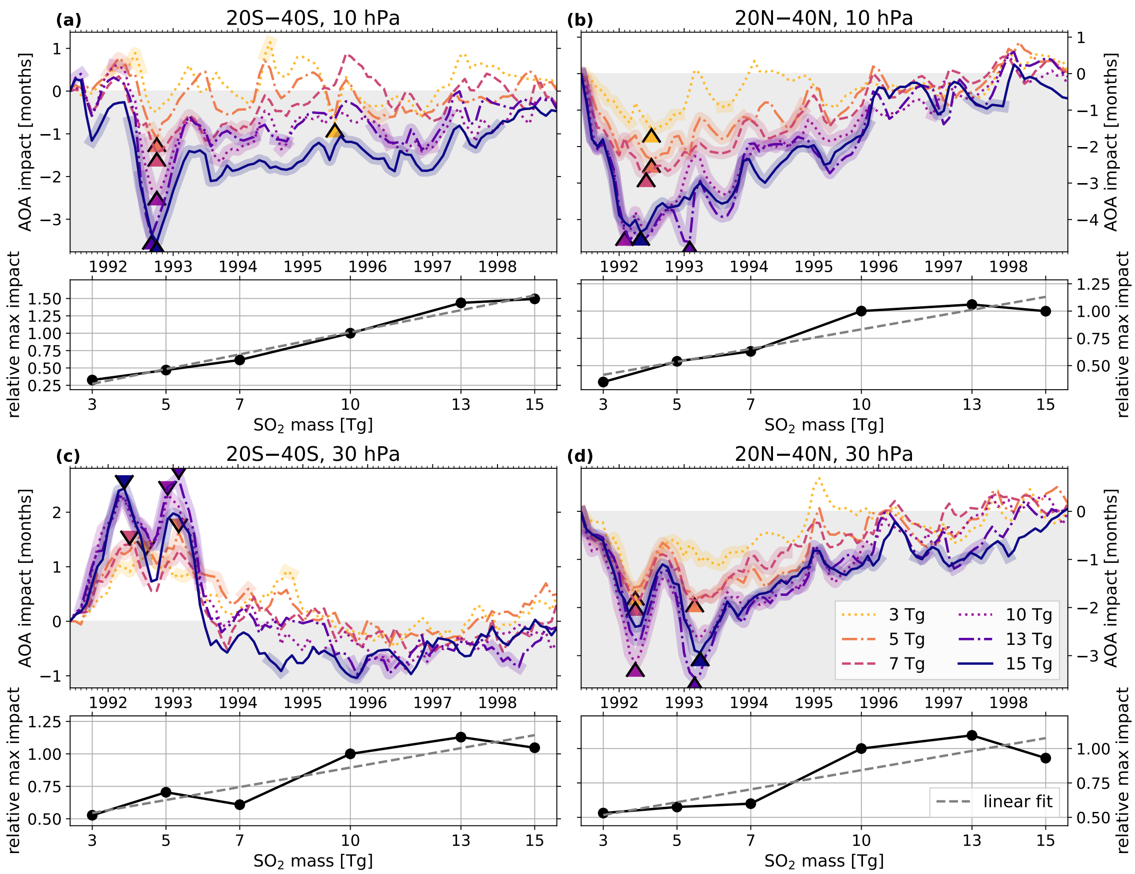

Figure 1: The replacement Figure 13, per the Reviewer Comment 12. The caption has not changed.

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.