Authors' Response to the Editor 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

EC: ***Editor Comment***, AR: Author Response, ☐ Manuscript Text

1. Author Comments

We thank the editor for their additional comments, which were very useful in identifying a few remaining holes in our manuscript. We have responded to the editor's comments below, in order of the simplest to most involved responses. Each comment appears as a editor comment (EC) 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.

2. Comment 1

EC: ***General: Over the whole manuscript and also in captions of the figures - an overbar is missing for w* and delta w*. You refer to the circulation as residual mean (I think this shortcoming comes from the Birner and Boenish paper), which is probably acceptable, but do not forget that it is in fact the residual mean circulation and reflect this with the overbar.***

AR: This was an oversight, and we appreciate the editor for identifying it. Indeed, all residual mean circulation velocities do have overbars in our previous companion paper, and so we have updated all occurrences of w^*, v^*, ω^* to have overbars as well. This includes the labeling within Figures 1, 5, 6, 10, A1, A2

3. Comment 10

EC: ***L418 What is the global EP wave activity? You mean Rossby wave activity?***

AR: Essentially yes. We have cleaned this up as:

> The effect should also be accompanied by corresponding modifications to global EP ~~wave activity~~flux divergence, which may in turn offer a more generalized interpretation of the results of Part I.

4. Comment 11

EC: ***L472 "..climatological w*zero-line" -This line has a name - the turn-around latitude.***

AR: We thank the editor for the useful note here, which we have incorporated:

> Notably, the negative anomalies in the SH occur north of the ~~climatological w^* zero-line~~turn-around latitude (their Fig. 6)

5. Comment 12

EC: ***L511 - The possible role of the QBO phase is very important and would be nice to mention in the methodology, how QBO is treated in the simulations you use (internally generated, nudged, constant phase...)***

AR: We have added detail to Sect. 2 so that this is more clear:

> ...
>
> 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, and evolve the resulting sulfate aerosols via a prognostic treatment. The simulations utilize a 1° horizontal grid and 72 vertical levels that extend to approximately 60 km, and feature an internally-generated QBO with a period of approximately 24 months (see Sect. 4.2 of Part I). We again use the paired limited-variability volcanic ensemble (LV) and its counterfactual ensemble (CF), introduced by Ehrmann et al. (2024), for the impact analysis.
>
> ...

6. Comment 3

EC: ***L48-49: ".. and local concentrations corresponding to mean age." - The whole statement here is a bit vague, as it obscures the whole underlying theory of Age-of-air and the fact that your method is able to derive the mean AoA only. At least, I would ask you to highlight the fact that the mean stratospheric age-of-air is analyzed in your manuscript.***

AR: We agree that this statement is vague, and have modified it as:

> In a modeling context, AoA is often implemented as a scalar "clock tracer"~~,~~ with a globally uniform and constant source ~~(it "ticks")~~, ~~and local concentrations corresponding to mean age.~~ and gridpoint-level mixing ratios which correspond to the mean stratospheric age-of-air.

7. Comment 7

EC: ***Fig. 4 - the colorbar includes white for the values between 5 and 6. Is this intentional?***

AR: No, that was not intentional, and we appreciate the editor noticing this error. We changed the upper bound of the colorbar when editing the figure per the reviewer feedback, but did not scale the colormap accordingly. The new figure is shown here as Fig. 1.

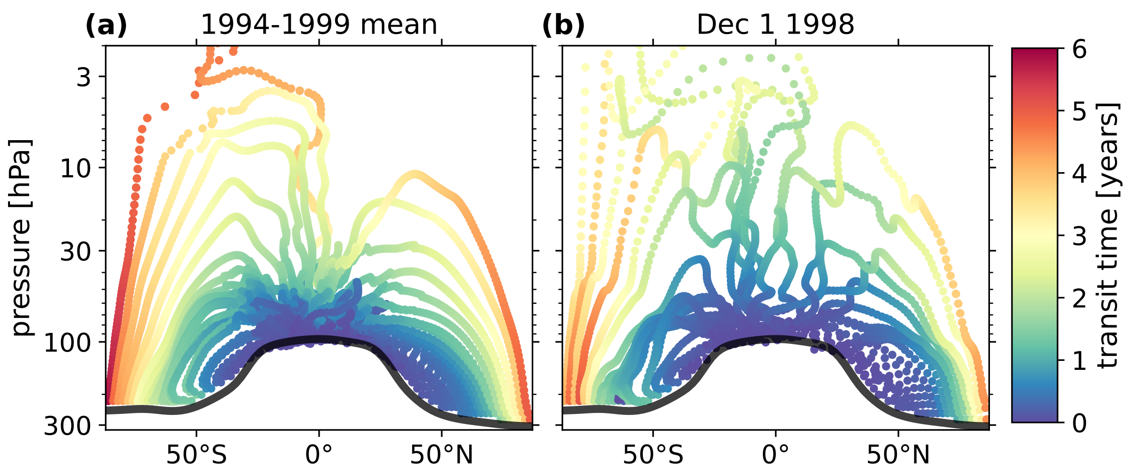

Figure 1: The manuscript Figure 4 with corrected colorbar

8. Comment 2

EC: ***L39-L41 "Tracer isopleths (surfaces of constant mixing ratio) are raised and lowered in the tropics and polar regions, respectively, by the thermally-direct overturning cell that is the residual circulation. In the midlatitudes, isopleths are homogenized by two-way adiabatic mixing of mass along isentropes, caused by breaking planetary waves in the surf-zone (e.g. Plumb (2007))...." - Nowhere you mention the fundamental effect of the Stokes drift, very important in the midlatitudinal LS, where it smears out the indirect Eulerian mean cell. Also, you should make clear that mixing is active not only in the midlatitudes and, generally, also vertical mixing is very important.***

AR: For clarity on this point; we assume that the reviewer means here the true Eulerian mean cell, which differs from the Lagrangian mean cell by a Stokes correction, rather than the transformed Eulerian mean cell which, in theory, accounts for the Stokes drift and does not smear out under steady, conservative waves. For more realistic conditions which violate those assumptions, we acknowledge that the relationship between the eddy flux derivatives in the TEM residual velocities $\overline{v}^*$, $\overline{w}^*$, and the corresponding Stokes correction terms, become only approximate.

We did not mention Stokes drift specifically so as not to mix terminology from both Lagrangian and Eulerian frameworks of the mean flow, which we thought might be cleaner. After a brief literature review per this round of revision, we note that while many foundational papers during the early development of the TEM and GLM frameworks did mention aspects of the two in tandem (including the Stokes drift), most papers today using the TEM as an analysis tool do not. However, we see how that approach could be providing less clarity rather than more, and we agree with the reviewer that it would be beneficial to have the effect at least mentioned. In addition to addressing the points about mixing, we have modified the paragraph as:

> ...
>
> This circulation describes the net ~~flux~~transport of mass in the meridional plane, as the superposition of an advective component which approximates the Lagrangian mass flow, and an eddy-driven com-

> ponent which approximates the effects of Stokes drift, wave transience and dissipation (Dunkerton, 1978). Tracer isopleths (surfaces of constant mixing ratio) are raised and lowered in the tropics and polar regions, respectively, by the thermally-direct overturning cell that is the mean residual circulation. Throughout the stratosphere, breaking planetary waves cause vertical diabatic mixing across isentropes, and especially in the midlatitude surf-zone, isopleths are homogenized by two-way adiabatic mixing of mass along isentropes~~, caused by breaking planetary waves in the surf-zone~~ (e.g. Plumb (2007)). In other words, the BDC is essentially an expression of the TEM balance from a tracer perspective.

where the reference to Dunkerton (1978) was specifically included as a foundational paper which compares the TEM/EP flux frameworks with a Lagrangian framework, with discussion of where the Stokes drift fits inbetween the two.

9. Comment 9

EC: ***L338 "Whatever the dynamical explanation of the advection-mixing balance at play here.." -Let me suggest a possible dynamical mechanism - the Stokes drift, which influences the Lagrangian mean transport (and the residual mean circulation) is connected with the pure presence of Rossby waves, when the Rossby waves break, they induce the quasi-horizontal mixing but also their amplitude is lowered and hence also the Stokes drift and the residual mean circulation are affected. I assume that the majority of Rossby waves will be upward propagating modes and in the full picture they will also be affected by the anomalies in the zonal wind field you describe around L329 (possible critical level occurence/shift for some modes).***

AR: This is an interesting interpretation which offers valable insight into a plausible mechanism. We greatly appreciate the editor's contribution, and have modified the text as:

> ...
>
> From a tracer perspective, the equivalent statement is that enhanced aging by residual circulation advection (panel (e) SH) tends to be associated with decreased aging by mixing (corresponding feature in panel (f)). This cancellation (above 30 hPa) is achieved by a downward displacement of mixing contours, ~~which in turn yields~~yielding an excess aging by mixing below the RCTT anomaly (below 30 hPa), which in part drives the net aging signal in panel (d). One possible physical interpretation of this feature is that as the circulation is perturbed, the critical level for propagating Rossby waves is altered. This would induce anomalous mixing through wave-breaking at the critical level, but also lead to further anomalous Lagrangian transport via the Stokes drift effect as the waves continue to propagate with perturbed amplitude.
>
> Whatever the dynamical explanation of the advection-mixing balance at play here, it appears to be initiated by the anomalous vertical advection in the first place ...

10. Comment 4

EC: ***L58-L60: "Later, Garny et al. (2014) pointed out that the difference (AoA–RCTT) could be used to isolate aging by isentropic mixing, which throughout much of the upper atmosphere was shown to be at least as important as aging by residual circulation advection. While this type of differencing is a useful***

procedure, it is also a blunt one, as it cannot separate contributions of mixing by resolved wave breaking, and diffusion..." -You should be aware that the difference between AoA and RCTT is partly also due to the advective processes. For one, already Garny et al. (2014) mention the effect of recirculation on AbM. However, there is also an additional mechanism arising from the fact that the residual mean circulation approximates the Lagrangian mean circulation only to the second order of wave amplitude and for steady waves. Hence, for the transport connected with wave transience events and finite amplitude waves the residual mean circulation is not accurate and the difference goes to AbM.

AR: These are important distinctions, which we did not represent well. We have modified this passage as:

> Later, Garny et al. (2014) pointed out that the difference (AoA−RCTT) could be used to approximately isolate aging by isentropic mixing, which throughout much of the upper atmosphere was shown to be at least as important as aging by residual circulation advection. While this type of differencing is a useful procedure, it is also a blunt one, as it cannot separate contributions of mixing by resolved wave breaking~~,~~ and diffusion. Nor can it identify any contributions by unresolved advective affects, such as recirculation of Lagrangian tracers, or nonlinear or transient wave contributions which affect the Lagrangian mean circulation, but are only partially represented in the residual circulation of the TEM.

11. Comment 8

EC: ***Fig. 5 - Are the units [day/day] correct here given the large tendencies that are resulting (even though these are only the partial tendencies)? Further, there is a typo in the caption.. "resiudal". Also, the reader would welcome more information on how the tendencies are computed. From daily data and then averaged over the year/season?***

AR: We have double checked and can confirm that the units of [day/day] here are correct. Though the tendency values in some regions are perhaps surprisingly large, the strong cancellations between the tendency components can often be counterintuitive. We have also fixed the typo in the word "residual" in the caption of Fig. 5.

For the computation of the tendencies, the text of Sect. 4 notes:

> For this, we recruit the TEM decomposition of the net age tendency, shown in Fig. 5. The panels show
>
> . . .
>
> The data are averaged over the same 5-year time period as Fig. 3.

and earlier in Sect. 3.3:

> The net tendency is estimated by a first-order finite difference taken on the daily-mean $\overline{q}$ data, as it was not available as a model output.

However, we appreciate that these pieces of information are separated in a way that lacks clarity. We also noticed that at no point other than this brief remark in Sec. 3.3 did we mention that the data frequency used across our analysis is daily. Readers could find this information in the heavily-cited Part I companion paper, but it is not stated clearly here. We have edited Sect. as

> We primarily use the same 10 Tg SO2 eruption as Part I, while Sect. 6 additionally includes a brief analysis of the tracer sensitivity with respect to eruption magnitude. This is accomplished through additional LV ensembles with 3, 5, 7, 13, and 15 Tg SO2 eruptions. All data products from the LV and CF ensembles were provided as daily-averaged fields.
>
> ...
>
> The AoA is computed as a tracer mixing ratio at each gridpoint during the model run. Here, we will use the zonal mean of the daily-averaged tracer field. For computing the residual circulation transit time, the history of the daily-averaged residual velocity components v^* and w^* is required for a period of ten years prior to the time of analysis.

In Sect. 3.3, we also added a reference to Appendix A of our companion paper, where the construction of the net tendency as a finite difference of the input daily-averaged data is discussed in slightly more detail:

> The net tendency, which was not available as a model output, is estimated by a first-order finite difference taken on the daily-mean q data, ~~as it was not available as a model output.~~ as
>
> $$\frac{\partial \overline{q_i}}{\partial t} = \frac{\overline{q}_{i+1} - \overline{q}_i}{24\text{ hr}}$$
>
> where q_i and q_{i+1} are adjacent data in the timeseries.

12. Comment 5

EC: ***Subsection 3.2, 3.3 - please state what version of TEM and residual mean circulation you use (I suppose that it is a hydrostatic primitive equation version in geometric coordinates, and not in the log-pressure coordinates or the QG version).***

AR: In line 160 near Eq. (6), we noted

> The meridional and vertical residual velocities v^* and w^* are defined in Eq. (9) and Eq. (10) of Part I, respectively.

We originally decided to only reference these equations from our companion paper for the sake of brevity. The answer to the Editor's question is that we are using a hydrostatic primitive equation version in pressure coordinates, which we chose for consistency with the DynVarMIP standard specified by Gerber and Manzini (2016).

However, we agree that all of this information was not as clearly implied as we originally intended. Thus, we have now written out the residual circulation forms explicitly, and adjusted the text as:

> The TEM formulation for the time (t) evolution of a zonal-mean tracer mixing ratio $\overline{q}$ ~~along constant pressure surfaces~~ is analogous to the formulation for zonal momentum $\overline{u}$ (see Sect. 3.2 in Part I) specified in spherical coordinates for the hydrostatic primitive equations on constant pressure levels

by Gerber and Manzini (2016). The time tendency of $\overline{q}$ is written as

$$\frac{\partial \overline{q}}{\partial t} = \frac{\partial \overline{q}}{\partial t}\Big|_{(\overline{v}^*)} + \frac{\partial \overline{q}}{\partial t}\Big|_{(\overline{\omega}^*)} + \frac{\partial \overline{q}}{\partial t}\Big|_{\nabla\cdot\mathbf{M}} + \overline{X} + \overline{S}. \tag{4}$$

The first and second terms represent tracer advection by the residual circulation,

$$\frac{\partial \overline{q}}{\partial t}\Big|_{(\overline{v}^*)} = -\overline{v}^* \frac{\partial \overline{q}\cos\phi}{\cos\phi\partial\phi} \tag{5}$$

$$\frac{\partial \overline{q}}{\partial t}\Big|_{(\overline{\omega}^*)} = \overline{w}^* \frac{p}{H}\frac{\partial \overline{q}}{\partial p} \tag{6}$$

The symbol ϕ denotes the latitude, and p stands for the pressure. The meridional and vertical residual velocities $\overline{v}^*$ and $\overline{w}^*$ are ~~defined in Eq. (9) and Eq. (10) of Part I, respectively. The symbol ϕ denotes the latitude, and p stands for the pressure.~~

$$\overline{v}^* = \overline{v} - \frac{\partial \psi}{\partial p} \tag{7}$$

$$\overline{w}^* = -\frac{H}{p}\overline{\omega}^* = -\frac{H}{p}\left(\overline{\omega} + \frac{\partial\psi\cos\phi}{a\cos\phi\partial\phi}\right) \tag{8}$$

where ω is the vertical pressure velocity, v is the meridional velocity, and the eddy streamfunction ψ is

$$\psi = \frac{\overline{v'\theta'}}{\partial\overline{\theta}/\partial p} \tag{9}$$

where θ is the potential temperature. The residual streamfunction Ψ^* is defined as

$$\Psi^* = \frac{2\pi a\cos\phi}{g_0}\left(\int_p^0 \overline{v}^* dp\right), \tag{10}$$

with g0 as the global-mean gravitational acceleration at mean sea level.

The third term on the right of Eq. (4) is the eddy-driven tracer mixing tendency,

$$\frac{\partial \overline{q}}{\partial t}\Big|_{\nabla\cdot\mathbf{M}} = \frac{\nabla\cdot\mathbf{M}}{a\cos\phi}. \tag{11}$$

The vector **M** is the eddy-tracer flux vector, with meridional and vertical components

$$M_{(\phi)} = a\cos\phi\left(\frac{\partial\overline{q}}{\partial p}\psi - \overline{q'v'}\right) \tag{12}$$

$$M_{(p)} = a\cos\phi\left(-\frac{\partial\overline{q}\cos\phi}{a\cos\phi\partial\phi}\psi - \overline{q'\omega'}\right). \tag{13}$$

~~where ψ is the eddy streamfunction defined in Eq. (11) of Part I.~~

...

13. Comment 6

EC: ***Subsection 3.3 In relation with the previous comment, the reader needs to know more details on the p->z transformation (around L191). Hopefully, actual geopotential height and not the log-pressure formula is used. Because the log-pressure relation may result in uncertainties in measuring the vertical distances due to changes in the thickness of atmospheric layers induced by variable heating (see e.g. Eichinger and Šácha, 2020, QJRMS or Šácha et al., 2019, ACP for the impact of vertical shifts on AoA and AbM, which can be also relative to pressure levels).***

AR: We are in fact using a log-pressure formula for the $p \to z$ transformation when preparing the data for the RCTT calculation. Specifically, we apply

$$z = -H \log\left(\frac{p}{p_0}\right) \tag{1}$$

where $H = 7$ km, and $p_0 = 1013$ hPa, before running our RK4 solver as specified in Eq.(12)−(13). We did this for consistency with Garny et al. (2014), who used the same constants, and also obtained height z from the input pressure levels using the same log-pressure form, to enable consistent comparisons between our results with the literature. Garny et al. (2014) does not explicitly state that they do this log-pressure conversion in their paper, although they did share their processing code with us via private communication for reference, where log-pressure height is implemented.

However, we recognize that our context is quite different than that of Garny et al. (2014) and most other AoA studies, since we are differencing simulations which do and do not have an imposed stratospheric heating. In that sense, we should have the same concerns about our results as Eichinger and Šácha (2020) has about climatological trends given a cooling stratosphere.

After reviewing Eichinger and Šácha (2020), we want to emphasize that it is not lost on us how important the issue of using constant H in our formulation is. It seems that even more consequential than using a log-pressure $p \to z$ transformation is our usage of a log-pressure definition of the residual vertical velocity $\overline{w}^*$:

$$\overline{w}^* = -\frac{H}{p}\overline{\omega}^* = -\frac{H}{p}\left(\overline{\omega} + \frac{\partial \psi \cos\phi}{a\cos\phi\partial\phi}\right) \tag{2}$$

As previously stated, we made this choice for consistency with the DynVarMIP standard set by Gerber and Manzini (2016) (and our Part I paper), and indeed most other TEM analyses in the literature. However, we understand that this form neglects to account for the change of scale height with temperature as

$$H = \frac{RT}{g}. \tag{3}$$

In the language of Eichinger and Šácha (2020), the relationship between our log-pressure expression for the vertical residual velocity, which we'll now refer to as $\overline{w}^*_H$, and a temperature-dependent $\overline{w}^*_T$ is

$$\overline{w}^*_T = \overline{w}^*_H \frac{R\overline{T}}{Hg} \tag{4}$$

Now, what we care about in our analysis is the volcanic impact $\Delta\overline{w}^*$, which in our manuscript we defined as the difference between the LV data $\overline{w}^*$ and the CF data $\overline{w}^{*\mathrm{CF}}$. Therefore, we will now estimate how much of the volcanic impact on $\overline{w}^*$ is a genuine change to vertical motion, and how much is due to expanding pressure levels in the heated stratosphere, which the analysis in our manuscript currently does not distinguish between.

We can express the vertical velocity and temperature in the volcanic ensemble as

$$\overline{T} = \overline{T}^{\mathrm{CF}} + \Delta\overline{T} = \overline{T}^{\mathrm{CF}}\left[1 + \frac{\Delta\overline{T}}{\overline{T}^{\mathrm{CF}}}\right] \tag{5}$$

$$\overline{w}_H^* = \overline{w}_H^* + \Delta\overline{w}_H^* = \overline{w}_H^{*\mathrm{CF}}\left[1 + \frac{\Delta\overline{w}_H^*}{\overline{w}_H^{*\mathrm{CF}}}\right] \tag{6}$$

and thus by Eq. 4, $\overline{w}_T^*$ is

$$\overline{w}_T^* = \overline{w}_H^*\overline{T}\frac{R}{Hg} = \overline{w}_H^{*\mathrm{CF}}\left[1 + \frac{\Delta\overline{w}_H^*}{\overline{w}_H^{*\mathrm{CF}}}\right]\overline{T}^{\mathrm{CF}}\left[1 + \frac{\Delta\overline{T}}{\overline{T}^{\mathrm{CF}}}\right]\frac{R}{Hg} \tag{7}$$

$$= \overline{w}_T^{*\mathrm{CF}}\left[1 + \frac{\Delta\overline{w}_H^*}{\overline{w}_H^{*\mathrm{CF}}}\right]\left[1 + \frac{\Delta\overline{T}}{\overline{T}^{\mathrm{CF}}}\right]. \tag{8}$$

With some algebra, this can then be rearranged to give the *relative* impact in $\overline{w}_T^*$ as

$$\delta\overline{w}_T^* = \delta\overline{T} + \delta\overline{w}_H^* + \delta\overline{T}\delta\overline{w}_H^* \tag{9}$$

where the relative impacts are defined as

$$\delta\overline{w}_T^* \equiv \Delta\overline{w}_T^*\left(\overline{w}_T^{*\mathrm{CF}}\right)^{-1} \tag{10}$$

$$\delta\overline{w}_H^* \equiv \Delta\overline{w}_H^*\left(\overline{w}_H^{*\mathrm{CF}}\right)^{-1} \tag{11}$$

$$\delta\overline{T} \equiv \Delta\overline{T}\left(\overline{T}^{\mathrm{CF}}\right)^{-1} \tag{12}$$

Eq. (9) now allows us to ask the question, is the quantity $\Delta\overline{w}_H^*$ which we analyzed in our work the dominant contribution to the true temperature-dependent impact Δw_T^*? And in either case, is $\Delta\overline{w}_H^*$ an over– or under–estimation of Δw_T^*? In other words, is the expansion of pressure levels in the heated stratosphere biasing our vertical velocity impacts low or high, and by how much?

We decided to first obtain representative answers to these questions by considering the scale of vertical velocity and temperature impacts in the tropics during August of 1991 (corresponding to Fig. 6(a) in the manuscript), near the equator and between 10–20 hPa. We chose this region because $\Delta\overline{w}_H^*$ is maximized here, and $\Delta\overline{T}$ is also generally highest in the tropics, and earlier in the time series. From the figure, we see that

$$\overline{w}^{*\mathrm{CF}} \approx 0.5 \text{ mm s}^{-1}, \text{ and } \Delta\overline{w}^* \approx 0.25 \text{ mm s}^{-1}.$$

To find the temperature impacts at that same time and location, we can look to Ehrmann et al. (2024), the paper which introduced the LV and CF simulations that we utilized for our work. In their Fig. 29(c), we can see that throughout August 1991 between 10 and 20 hPa and averaged on $\pm 20°$ in latitude,

$$\Delta\overline{T} \approx 2 \text{ K}$$

Those authors did not publish the CF temperature distribution itself, but we checked in the raw dataset while writing this response to the editor, and see that at this time and location,

$$\overline{T}^{\text{CF}} \approx 220 \text{ K}.$$

We also checked that the zonal-mean temperature anomaly in the meridional plane is approximately uniform in magnitude and shape in it's meridional extent between $\pm 20°$ in latitude, such that the estimate of 2 K from Ehrmann et al. (2024) is not averaging over larger localized values.

With this information, our estimate for the bias induced by using a constant scale height is

$$\begin{aligned} \delta\overline{w}^*_T &= \delta\overline{T} + \delta\overline{w}^*_H + \delta\overline{T}\delta\overline{w}^*_H && (13)\\ &= \frac{2 \text{ K}}{220 \text{ K}} + \frac{0.25 \text{ mm s}^{-1}}{0.5 \text{ mm s}^{-1}} + \frac{2 \text{ K}}{220 \text{ K}}\frac{0.25 \text{ mm s}^{-1}}{0.5 \text{ mm s}^{-1}} && (14)\\ &\approx 0.01 + 0.5 + 0.005 && (15) \end{aligned}$$

Which shows that the correction due to temperature dependence of on the vertical velocity impact in this region is negligible. Specifically, of the total $\delta\overline{w}^*_T \approx 0.515$, $\sim 97\%$ of the impact is explained by $\delta\overline{w}^*_H$, with $\delta\overline{T}$ and the cross term $\delta\overline{T}\delta\overline{w}^*_H$ together accounting for a correction of only $\sim 3\%$.

For completeness , we now show these relative impact terms explicitly for the $\pm 20°$ band between 10 and 70 hPa for 2.5 years following the volcanic eruption in Fig. 2 below. The estimated $\delta\overline{w}^*_T$ from Eq. (9), $\delta\overline{w}^*_H$, $\delta\overline{T}$, and $\delta\overline{T}\delta\overline{w}^*_H$ are shown in the top, second, third, and bottom axes, respectively.

Here we see that the temperature impact generally never exceeds $\sim 2.5\%$ of the CF temperature, and the cross term is about an order of magnitude lower over much of the domain. The $\overline{w}^*$ impact, on the other hand, is generally between 40% and 10% of the CF $\overline{w^*}$ in the middle stratosphere for at least the first year of the simulation, and intermittently exceeds 100% above 20 hPa. Ultimately, $\delta\overline{w}^*_T$ is almost identical in magnitude and distribution to $\delta\overline{w}^*_H$, meaning that the vast majority of the temperature-dependent $\Delta\overline{w}^*$ is explained as by log-pressure $\Delta\overline{w}^*$, and not effects of temperature on pressure thickness.

Finally, Fig. 3 shows the relative contribution of each term on the RHS of Eq. (9) to the total $\delta\overline{w}^*_T$. The temperature has brief, localized contributions of $\sim 10 - 20\%$ of the net signal, but for the majority of the domain is <10% or <1%. The cross term contribution is $\lesssim 1.5\%$ over the entire domain.

We therefore conclude that this issue is not a significant cause for concern regarding our results because

1. It appears that the effect of temperature would only very minimally alter our results, likely not to a degree of difference in our qualitative conclusions.
2. Unlike the case of climate change as discussed in Eichinger and Šácha (2020), stratospheric temperatures increase rather than decrease in our simulations. This means that the effect of temperature through the scale height H, and the dynamical effect on $\overline{w}^*$ have the same sign. In other words, this type of bias would cause us to underestimate, rather than overestimate, the magnitude of the volcanic impact.
3. Regarding the RCTT vertical coordinate, we think that there is actually no issue here even in principle, since our $\overline{w}^*$ and z are both consistent in their use of a log-pressure form with constant scale height. As pointed out in Eichinger and Šácha (2020), the question is fundamentally one of units; $\overline{w}^*_H$ implicitly uses log-pressure m s^{-1}, while $\overline{w}^*_T$ implicitly uses geopotential m s^{-1}. In performing trajectory integrations, the units of the vertical coordinate and vertical velocity should match. Further, our approach is consistent with that of Garny et al. (2014).

4. Finally, all of the analyses in our Part I companion paper use the same TEM formulation as presented here. Since Part I relied heavily on interpretation of EP flux anomalies in its methods and conclusions, which Eichinger and Šácha (2020) also note should be affected by the choice of log-pressure vertical coordinates and velocities, it would be best to remain consistent in our formulation here.

Having said all of that, we greatly appreciate the enlightening details given in Eichinger and Šácha (2020), and are convinced that it is important for authors to disclose their $p \to z$ mappings, and all other details of their vertical velocity formulation. We are also convinced that $\overline{w}_T^*$ is the correct formulation to use, especially when forcings of stratospheric temperature are in play.

We hope that the information presented here satisfies the editor, but we have decided not to include this analysis in the paper in its full form. Rather, we will just state the results, cite Eichinger and Šácha (2020), and mention our $p \to z$ transformation explicitly. If the editor would prefer that we include the present derivations and figures as either a supplement or appendix, we would oblige.

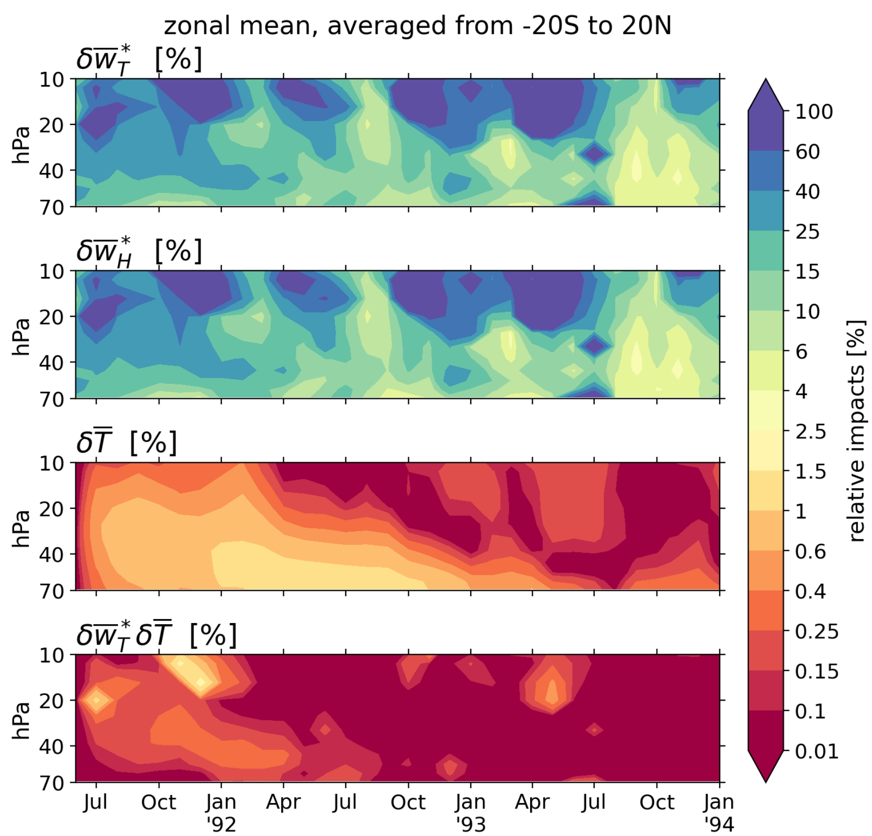

Figure 2: Relative impacts used for estimating the temperature-dependent component of the residual vertical velocity impact. Each panel corresponds to a term of Eq. (9). All panels share the common colorscale, in units of percentage, with contours spaced approximately logarithmically.

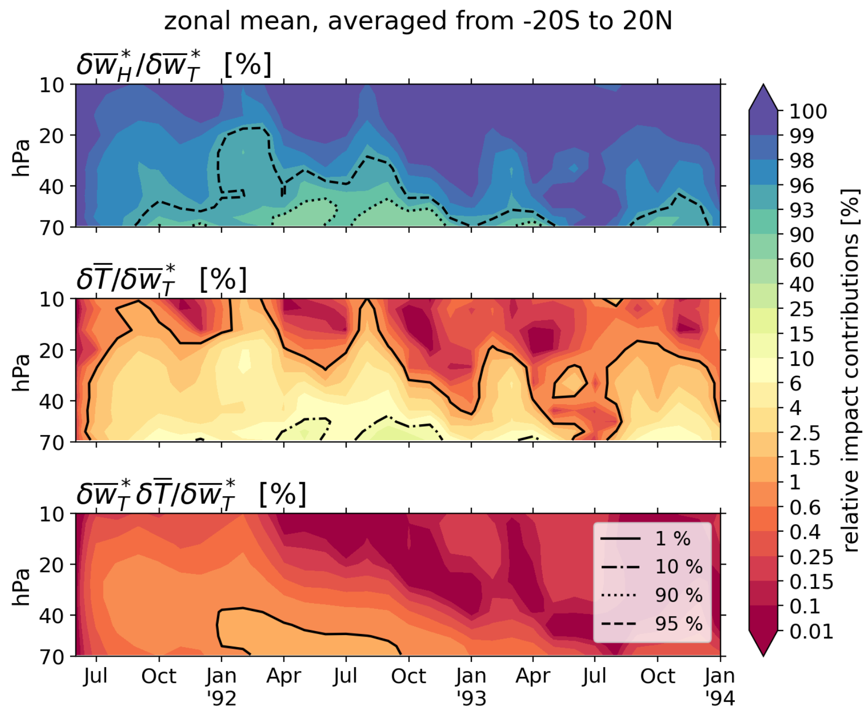

Figure 3: Relative impact contributions to the total $\delta\overline{w}_T^*$ of each term on the RHS of Eq. (9). All panels share the common colorscale, in units of percentage. Individual contours at 1%, 10%, 90%, and 95% contribution are shown in black with varied linestyles for reference. Note that the colorbar here is different than in Fig. 2, and that several contours between 90% and 100% are expanded in the upper range of the scale.

Ultimately, we edited Sect. 3.3 as

> . . .
>
> Backward trajectories are then "launched" from each of these points, and the motion is solved by two standard fourth-order Runge-Kutta (RK4) procedures in the meridional and vertical dimensions. Each dimension is transformed to Cartesian meters ($\phi \rightarrow y$, $p \rightarrow z$) before integration, with a log-pressure vertical coordinate
>
> $$z = -H \log\left(\frac{p}{p_0}\right). \tag{16}$$
>
> where H = 7 km, and p_0 = 1013 hPa. We use a step size of $h = 5$ days and define

We've also created a new subsection Sec. 3.4 following the RCTT description:

. . .

3.4 Remarks on log–pressure quantities

Because we use a log-pressure form of the vertical component of the residual circulation $\overline{w}^*$ (Eq. 8) with constant scale height H, it is necessary to briefly discuss the results of Eichinger and Šácha (2020). There, the authors note that for contexts with climatological stratospheric temperature anomalies (such as the stratospheric cooling associated with anthropogenic climate change, or indeed the heating associated with large volcanic eruptions), the scale height H should be allowed to vary as the distances between vertical pressure levels change. By failing to do this, the deduced behavior of $\overline{w}^*$ will necessarily include an artificial component inherited purely from the choice of vertical coordinate. That is, movement of pressure levels via thermal expansion will be interpreted as a vertical residual velocity.

Therefore, the true or temperature-dependent volcanic impact of the vertical residual velocity (denoted $\Delta\overline{w}^*_T$ in the notation of Eichinger and Šácha (2020)) is, in principle, a function f of both our calculated $\Delta\overline{w}^*$, and the temperature impact $\Delta\overline{T}$ which we aren't accounting for,

$$\Delta\overline{w}^*_T = f(\Delta\overline{w}^*,\ \Delta\overline{T}). \tag{17}$$

Using the tools in Eichinger and Šácha (2020) we estimated (but do not show here) the relative effect of neglecting scale-height changes on our calculated $\Delta\overline{w}^*$ in the tropics ($\pm 20^\circ$ in latitude) and between 10 and 70 hPa over the first two years of the simulations (corresponding to the domain of greatest volcanic temperature anomalies in the LV ensemble Ehrmann et al., 2024). We concluded that this effect can contribute up to $10\% - 20\%$ of the true $\Delta\overline{w}^*_T$ in brief and localized occurrences, but that in general the contribution is $< 10\%$ or $< 1\%$ throughout the analyzed domain. Further, while Eichinger and Šácha (2020) identify that this effect causes *over*estimation of e.g. the BDC acceleration by climate change, in the present case it would instead cause *under*estimation of the volcanic enhancement of vertical residual velocities, since the stratospheric temperature anomalies associated with those two phenomena are of opposite sign.

In summary, we infer that $0.9 \lesssim \Delta\overline{w}^*/\Delta\overline{w}^*_T \lesssim 1$ throughout the tropical stratosphere, where $\Delta\overline{T}$ is largest and most significant. Hence, we will ignore this complication for the remainder of this work, and assert that our qualitative conclusions would be unaffected by implementing a minor correction term.

References

Dunkerton, T. (1978). On the Mean Meridional Mass Motions of the Stratosphere and Mesosphere. *Journal of the Atmospheric Sciences*, 35(12):2325–2333.

Ehrmann, T., Wagman, B., Bull, D., Hillman, B., and Hollowed, J. (2024). Identifying Northern Hemisphere Stratospheric and Surface Temperature Responses to the Mt. Pinatubo Eruption within E3SMv2-SPA. Technical Report SAND2024-12730, Sandia National Laboratories. https://doi.org/10.2172/2462901.

Eichinger, R. and Šácha, P. (2020). Overestimated acceleration of the advective Brewer–Dobson circulation due to stratospheric cooling. *Quarterly Journal of the Royal Meteorological Society*, 146(733):3850–3864. _eprint: https://rmets.onlinelibrary.wiley.com/doi/pdf/10.1002/qj.3876.

Garny, H., Birner, T., Bönisch, H., and Bunzel, F. (2014). The effects of mixing on age of air. *Journal of Geophysical Research: Atmospheres*, 119(12):7015–7034.

Gerber, E. P. and Manzini, E. (2016). The Dynamics and Variability Model Intercomparison Project (DynVarMIP) for CMIP6: assessing the stratosphere–troposphere system. *Geoscientific Model Development*, 9(9):3413–3425.