Authors' Response to Reviews of

HSW-V v1.0: localized injections of interactive volcanic aerosols and their climate impacts in a simple general circulation model

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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. 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. Section numbers refer to those as they appear in the updated manuscript (for example, some Appendix section numbers have changed).

Our responses to Comment 1, 2 and 3 consist of important text edits to make more clear the relationship of this work to the broader field of climate-attribution science. Our response to Comment 4 identifies a mistake that was present in the manuscript.

1.2. Comment 1

RC: ***The authors do not show that this first-order treatment of transport, temperatures, radiation, and aerosol processes create a trustworthy climate-attribution environment. One criticism, for example, might stem from a lack of a quasi-biennial oscillation in the model. This deficiency both eliminates one way in which volcanic eruptions impact circulation and one mode of dynamical variability that impacts the transport of volcanic aerosols and their precursors.***

AR: We appreciate this feedback from the reviewer, and have thought carefully about how to better express the application of our model to the climate-attribution problem. We would like to emphasize that this model configuration is intended to be a trustworthy environment in which to *develop* new climate attribution methodologies, not one in which the specific Pinatubo impacts are accurately modeled. Pathways of impact will exist even if the general circulation does not specifically represent that of the historical Pinatubo scenario, or even that of the Earth's observed climatology. This is an intentional simplification. To be clear, the idealized environment is being employed to eliminate some of the complexity of a fully coupled / full physics Earth System Model (ESM) whilst still preserving the progressive multi-variate pathway through which impacts arise, such that the implementation of novel methodologies can be verified as operating correctly. This lowers the risk of moving to more complex problems in which realistic representations are present.

We have made some changes to the language in Section 1 in the hopes that this nuance is more clear. Specifically, we replaced occurrences of the phrase "attribution problem", since it may have implied something specific and different from what we intended:

...

Prescribed and prognostic methods have also been applied to model other forms of sulfur-based radiative forcing, with significant research recently being devoted to stratospheric aerosol injection (SAI) climate-change intervention activities (Crutzen, 2006; Tilmes et al., 2018, 2017; McCusker et al., 2012). ~~In addition, there is growing interest in solving the "attribution problem" of quantifying~~ . One key goal of SAI research is to quantify the causal connections between an observed climate impact, and an upstream forcing source, i.e. to attribute the SAI source as the cause of a detected, anomalous atmospheric response. Volcanoes are a natural analog to SAI, and thus offer ~~a pathway~~ an avenue for developing novel attribution methods of quantifying these causal connections.

The climate impacts that are most ~~relevant to society, such as~~ societally-relevant tend to be spatially localized (e.g. droughts, heat waves, or fires~~, are multiple steps away~~) and located downstream from their associated sources (e.g. volcanoes, or other solar radiation modification) ~~.~~ by multiple causal connections. "Multi-step attribution" involves a sequence of single-step attribution analyses, but is generally not employed, as the single weakest attribution step limits its confidence (Hegerl et al., 2010). Therefore, there is a need for ~~robust multi- step~~ novel multi-step attribution techniques in both climate change studies (Burger et al., 2020) and climate intervention studies (National Academies of Sciences, 2021; Office of Science and Technology Policy (OSTP), 2023) ~~. Multi step attribution involves a sequence of data analyses that connect a source to a downstream impact with specific assessments of each step (Hegerl et al. , 2010). Examples of multi-step attribution are uncommon, with the storyline approach from the extreme weather attribution community coming the closest (Trenberth et al., 2015; Shepherd, 2016; Pettett and Zarzycki, 2023).~~ that overcome these issues to enable attribution of societally-relevant impacts.

...

Accordingly, we suggest that a ~~useful testbed for the attribution problem between stratospheric aerosol forcing and atmospheric temperature perturbations, could be built upon a new idealized representation of a large volcanic eruption event within a highly simplified atmospheric environment.~~new idealized representation of prognostic volcanic forcing within a highly simplified atmospheric environment would be a useful testbed for the development of novel multi-step attribution methods (i.e. constructing relationships between stratospheric aerosol forcing and atmospheric temperature perturbations).

...

In addition, see our response to Comment 2 for a few more changes relevant to this comment.

As to the issue of the QBO specifically, we hope that these clarifications demonstrate that an accurate representation of the QBO (and other specific modes of climate variability) are not necessarily required in order for the model to serve as an climate-attribution development testbed. We agree with the reviewer that the lack of a QBO in our model will change the aerosol transport, and thus the specific atmospheric impacts of the volcanic forcing, with respect to the historical event. However, we do not think that this fact necessarily challenges the utility of our model as we have presented it.

Having said this, we do think that it is worthwhile to emphasize more explicitly the lack of a QBO in our configuration, and what implications this has on the downstream impact development. We would also like to emphasize to readers that it would be possible to activate an auxiliary parameterization which nudges the equatorial winds toward a realistic QBO, if desired. To this end, we added a new paragraph at the end of Section 2.2:

> In the tropical stratosphere, easterlies with speeds up to -30 m s^{-1} dominate. Note that while the tropical stratospheric winds will vary about this average, the HSW atmosphere does not include any kind of regular quasi-biennial oscillation (QBO) analog. Yao and Jablonowski (2016) showed that whether or not a QBO spontaneously develops in an HSW configuration will largely depend on the dynamical core in use. For a spectral element (SE) dynamical core, they observed that wave forcing was never strong enough to cause a reversal of the tropical stratospheric winds. The same conclusion appears to hold for our configuration of E3SMv2. Despite this, the QBO may be a desirable target for future studies employing this model configuration, as it has been shown that the QBO phase is a significant modulator of the volcanic climate response (Thomas et al., 2009). We do not consider this issue further in the present work, but note that it could be possible to prescribe a QBO by nudging the horizontal winds toward a specified reference state (as has been done for e.g. the Whole Atmosphere Community Climate Model (WACCM) by Matthes et al. (2010)).

1.3. Comment 2

RC: ***A related issue stems from the assertion that, on line 62, "the goal is not to accurately replicate any particular historical eruption". This assertion seems at odds with the rest of the paper which is dominated by an example of tuning parameters in order replicate the specific (and unique) eruption of Pinatubo in 1991. This highly idealized setup requires tuning these parameters; the authors should clarify how this scheme could be used in a general fashion that isn't based on tuning parameters to a specific eruption. One suggestion for future work might be to tune the parameters to some kind of average of many eruptions.***

AR: This is an issue that we may not have been clear enough about in the manuscript, so we appreciate the reviewer bringing it to our attention. When we say that "the goal is not to accurately replicate any particular historical eruption", what we really mean is that our model is not attempting to capture the specific *observed atmospheric response* to the Pinatubo eruption. This is necessarily true, since our atmosphere is hemispherically symmetric, and does not represent certain atmospheric modes that were present during the historical event (e.g. the QBO). Despite this, we still chose to tune the *volcanic forcing itself* toward a specific exemplar. In other words, the goal was to represent plausible atmospheric impacts of a Pinatubo-like event, and not to be predictive of the observed impacts themselves.

We have adjusted the text in Section 1 to be more clear about this intention:

> Our approach sacrifices realism by design. The goal is not to ~~accurately replicate any particular historical eruption, or to asses a model based on its specific post-eruption climate predictions~~simulate an accurate post-eruption climate of a particular historical volcanic event, but rather to produce a plausible realization of a generic volcanic ~~event~~eruption, simulated with a minimal forcing set.
>
> . . .
>
> Our model isolates a single volcanic event from any other external source of forcing or variability, and allows the flexibility to be embedded in a simplified atmospheric environment. ~~Specifically, we describe the implementation of~~ Though the implementation is generic, we present here a particular tuning of the parameterizations for an eruption similar in character to the 1991 eruption of Mt. Pinatubo, and the subsequently observed impacts. . .

We note that the final paragraph in Section 5 does describe more general usage of our parameterizations, as the reviewer has suggested, though we have adjusted the text slightly to be more explicit:

> We illustrated that our implementation can be used to mimic the spatio-temporal temperature anomaly signatures of large volcanic eruptions, and presented one specific parameter tuning that gives rise to a Pinatubo-like event... Nevertheless, the formulation remains flexible to modifications. Our parameterizations ~~could~~can be tuned toward eruption scenarios other than the 1991 Mt. Pinatubo event. They can also support any number of co-injected tracer species, concurrence of multiple eruptions, and injections at any latitude and height. In fact, the description is generic enough that by replacing the vertical and/or temporal injection profiles, we could imagine simulating the aerosol direct-effect of various localized emission events of the troposphere (e.g. wildfire smoke) or the stratosphere (e.g. geoengineering SAI experiments) in an idealized model configuration.

1.4. Comment 3

RC: ***Specific to the Pinatubo eruption, the authors should expand on parameter choices. Observations (especially early on) of Pinatubo are uncertain. That being said, a plume center of mass at 14km for Pinatubo is extremely low. It is implied that this is due to unrealistic plume rise observed in this system—could the authors expand on that?***

AR: We agree that this figure is a bit jarring to familiar readers, and that more discussion is warranted. This was an outcome of the tuning process, and was required to obtain the desired long-term temperature anomalies. As we state in Section 3.5:

> The longwave attenuation mechanism of the model is tuned to produce realistic stratospheric heating rates by sulfate aerosols. The mass extinction coefficient b_{LW} for sulfate is instrumental in tuning the long-term mean temperature anomalies ... Not as obvious is the importance of b_{LW} for the very short-lived ash tracer. The lofting speed of the plume will be controlled by the aggressive early heating of ash in the fresh plume (Stenchikov et al., 2021), since the initial ash mass loading (50 Tg) is dominant over that of SO_2 (17 Tg). As such, the mass extinction coefficient for ash serves as the main tuning parameter which controls the settling height of the aged aerosols.

In fact, the final sentence in this quote is not quite correct, and SO2 still contributes significantly to the initial plume heating, and subsequent lofting. When we tuned the mass extinction coefficient for ash, in order to achieve a realistic settling height near 25 km, we did *not* also tune the SO2 mass extinction coefficient. In hindsight, this could have been done differently. This essentially means that we heat the initial plume more than we really intend to, which we must accommodate for by lowering the initial injection height. To avoid this, we could have tuned the SO2 b_{LW} simultaneously with that of ash, or could have simply set $b_{\mathrm{LW,SO2}} = 0$, and thus controlled the heating of the fresh plume by ash alone. We attempted to explain this in Appendix C3, and specifically recommend changing this parameter choice in future usage of the model, if a higher injection height is desired:

> The tuning process would be easier, and a higher initial injection height of 18-20 km could be supported, if the degeneracy between these three extinction parameters were removed. We recommend having the SO2 tracer instead behave as a radiatively passive tracer, acting only as the vehicle for sulfate production. In this case, the LW mass extinction coefficients for ash and sulfate would truly be independent knobs for the lofting height, and long-term temperature anomalies, respectively. We would consider this tuning choice an improvement of the parameterization.

We did not find this issue to be problematic enough to warrant re-tuning the model. This is because the signature of the forcing and associated atmospheric impacts of the mixed (zonally symmetric) aerosol distribution would not change, which was our priority. In addition, we do not think that the current configuration would preclude an analysis which focuses more on the initial plume evolution, as the modeled scenario is still physically plausible, even if not perfectly reminiscent of the Pinatubo event (also see responses to Comment 1 and Comment 2).

We have ensured that all of this is more clear to the reader. In particular, we added a few more words about this issue to the main text, rather than only appearing in the appendix, where it might be missed. First, the text in Section 3.5 has been adjusted:

> ...
>
> Not as obvious is the importance of b_{LW} for the very short-lived ash tracer. Though radiative forcing by ash does not directly contribute to the eventual stratospheric temperature anomalies, it does control the mechanism by which the aerosols are delivered to the lower stratosphere (Stenchikov et al., 2021). The lofting speed of the dense, fresh plume will be controlled by the aggressive ~~early~~ heating of ash~~in the fresh plume (Stenchikov et al., 2021), since the initial ash mass loading (50 Tg) is dominant over that of SO_2 (17 Tg)~~, which is the dominant component of the initial injection. As such, the mass extinction coefficient for ash serves as the main tuning parameter which controls the settling height of the aged aerosols. Meanwhile, SO_2 ~~, on the other hand,~~ participates both in the initial lofting of the plume, as well as the short-term temperature anomalies for the first couple months. This behavior by SO_2 creates some degeneracy in the longwave extinction tuning parameters which could be avoided with a slight modification; see Appendix C4 for a discussion.

We have also added more detail to the text formerly found in Appendix C3, and moved it to it's own new Appendix C4:

> **C4 Avoiding a low injection height by revising the LW mass extinction coefficient tuning**
>
> As alluded to in Section 3.5 and Appendix C3, there is some degeneracy between $b_{\mathrm{LW,ash}}$ and $b_{\mathrm{LW,SO2}}$ for controlling the initial heating of the aerosol plume, as well as degeneracy between $b_{\mathrm{LW,sulfate}}$ and $b_{\mathrm{LW,SO2}}$ for controlling the stratospheric temperature anomalies during the first few months post-injection. This makes the manual process of iteratively tuning the parameters more laborious. In the present case, it also results in the implementation of a unusually low initial injection height of $\mu = 14$ km. Specifically, we did not tune $b_{\mathrm{LW,SO2}}$ along with $b_{\mathrm{LW,ash}}$ and instead needed to compensate for the aggressive early plume lofting by lowering μ.
>
> The tuning process would be easier, and a higher initial injection height of 18-20 km could be more easily supported, if the degeneracy between these three extinction parameters were removed. We ~~recommend~~ suggest having the SO_2 tracer instead behave as a radiatively passive tracer, acting only as the vehicle for sulfate production, by setting $b_{\mathrm{LW,SO2}} = 0$ and $b_{\mathrm{SW,SO2}} = 0$. In this case, the LW mass extinction coefficients for ash and sulfate would ~~truly~~ be independent knobs for the lofting height, and long-term temperature anomalies, respectively. We would consider this tuning choice an improvement of the parameterization.

We have also added a more explicit pointer to this discussion in Section 2.1:

> After tuning the model with these considerations in mind, we use the even lower value of $\mu = 14$ km, which we found to result in a realistic settling altitude for the sulfate tracer distribution. ~~(see Appendix C)~~. The need for this exceptionally low injection height is due to an overly aggressive heating of the

initial plume given our parameter choices, which is discussed further in Section 3.5 and Appendix C4.

1.5. Comment 4

RC: ***In a similar vein, the 30-day e-folding time used for the Pinatubo SO2 is considered fairly uncertain—faster e-folding times (23±5 days or 25±5 days depending on choice of dataset) have been proposed (Guo et al., 2004, https://doi.org/10.1029/2003GC000654).***

AR: We thank the reviewer for catching this error. We do indeed use a 25-day e-folding time for SO2, as informed by Guo et al. 2004. This figure was presented correctly in Table 1, but was later quoted incorrectly as 30 days in the text in both Section 3.1 and Section 3.2. We have corrected these mistakes in the text to instead read "25".