Responses to Reviewers' Comments for Manuscript #2024JD041098R

Radiative forcing by supereruptions

Addressed Comments for Publication to

Journal of Geophysical Research — Atmospheres

by

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- *Stuff to-do*

Authors' Response to the Editor

Associate Editor Evaluations

Evaluations.

Accurate Key Points: No

General Comments. Thank you for submitting "Radiative forcing by supereruptions" [Paper #2024JD041098R] to Journal of Geophysical Research - Atmospheres. I have received 3 reviews of your manuscript, which are included below and/or attached. As you can see, the reviews indicate that major revisions are needed before we can consider proceeding with your paper. I am therefore returning the paper to you so that you can make the necessary changes.

Response: We appreciate your handling of the review process.

According to the reviewers' comments, we have checked our manuscript and addressed them in the following way:

1. We added content.
2. We removed our wrong statements in Section I.

Concluding Response to the Editor. Thank you for your valuable comments on our manuscript. We have done our best to incorporate changes to reflect the suggestions, which allowed us to improve the quality of our work.

Authors' Response to Reviewer #3

They want:

- Overall goal is still unclear
- Make simulations more clear (how many in ensembles for example)
- Why this (heavy) model specifically?
- Should ERF and RF be mixed?
- Why are some papers are used in some figs, but others not?
- Remove geoengineering results from the figure and only keep it in the discussion
- Why are certain behaviours modelled? (timing of AOD and ERF)
- Discuss the caveats of the model study
- Why include the outdated Jones et al. 2005?
- Adjust the title

Reviewer #3 Evaluations

Evaluations.

Recommendation	Return to author for major revisions
Significant	The paper has some unclear or incomplete reasoning but will likely be a significant contribution with revision and clarification.
Supported	Mostly yes, but some further information and/or data are needed.
Referencing	Mostly yes, but some additions are necessary.
Quality	The organization of the manuscript and presentation of the data and results need some improvement.
Data	Please Select
Accurate Key Points	Please Select

Reviewer #3 Formal Review for Author (shown to authors)

General Comments.

I would like to thank the authors to have addressed many of my comments. Here is now my second review on the revised manuscript which still needs substantial revisions. Pls take all my comments into account.

Reviewer #3 Specific Comments

Response: Thank you for your feedback.

We have carefully addressed all the issues item by item as follows.

Comment 1

There are still unclear parts in what the overall goal of your study is. F.e. the climate sensitivity in the introduction and 3.4 is not taken up in the Conclusion-Summary and Abstract. So, what is the take home message of this part also in relation to the rest of the results?

Comment 2

The model runs details are now becoming clearer, thanks for explaining these, but some others still remain unresolved such as:

- Why not adding the simulations for the ERF calculations into Table 1 as well to make your two-step model approach clearer?
- How many ensembles have you run per SO2 injection experiments?
- How many years do you run? Which initial state (ENSO, QBO, AMOC, NAO, polar vortex) have you chosen and why?
- Were all started from the same initial state? Is the QBO included in the set-up?

Response: Thank you for the comment.

Check Toohey et al. (2014).

Comment 3

Why have you run and setup this computational heavy CESM2WACCM6 model? Why do you need exactly this model version? The motivation for this is still not becoming clear as you are also comparing with model studies with prescribed aerosols which are linear scaled to Pinatubo. I feel explaining this will help to define the aim of your study better.

Comment 4

It would be interesting to see what the difference in ERF-AOD and RF-AOD is between your two different model set-ups. Following this line of thought in Fig. 4 you have added RF values as ERF from other studies (Os20, B20, T10...). Is this appropriate to mix? I guess not as you explain now in your introduction. So I suggest to clean up and only add studies with an ERF model set-up and calculations which may explain some of your limitations to use certain studies and others not.

Response: Thank you for the comment.

Can cite probably also O'Connor et al. (2021).

Comment 5

Related to the previous bullet point. It is not becoming clear to me why certain studies are included in some of your figures and argumentation lines and some others are neglected. It would rather help to understand your argumentation flow better why you have done so. F.e. why is Jones et al. (2005) in Figs. 2 & 4 but not Timmreck et al. (2010) (T10)? Why is Robock et al. (2009) overall neglected? Be consistent throughout the ms and in all figures or explain why this is impossible.

Comment 6

The geoengineering study is mixed within the context of the volcanic modelling studies, which I think is not appropriate as continuously SO2 injections give different results. Thus, I suggest to take Niemeier and Timmreck (2015) out of the main context and all figures and include it instead solely at the end of the discussion.

Comment 7

Why are you modelling certain behaviors (Figs. 1-3) such as the different timing of AOD and ERF? Details are not shown and explained with your own results but instead discussed based on other models and CESM model studies.

Comment 8

Discuss the caveats of your own model study: model set up, model comparison, ...

Comment 9

Why including the outdated study by Jones et al. (2005)? I think there is enough more recent papers with prognostic aerosol schemes which are more suitable here. Or motivate why you need to focus exactly on Jones et al. (2005).

Comment 10

Reference style needs to be corrected to JGR style.

Comment 11

Wording: "observe" for model studies shall be rather model/simulate/project

Comment 12

Include English et al. (2013); Robock et al. (2009); Metzner et al. (2014)

Minor comments

Comment 13

Title to be adjusted to address the new and main aim of the paper along the lines of f.e. "the focus regarding the non-linearity relationship between AOD and ERF is on the development over time in the post eruption period" (from your Reply to Reviewer 3).

Abstract

Comment 14

"Volcanic activity" should to be "Volcanic eruptions"

Comment 15

"Where the climate effect is only loosely tied to the magnitude of the eruption." Metzner et al. (2014) has covered this in detail before; see also my below comments. So, this sentence needs to be rewritten. Be specific what you mean with the magnitude. The magnitude in volcanology is based on the mass of the erupted magma but I guess you mean the release of SO2 here?

Comment 16

L34: peek > peak

Comment 17

L36-37: Change "While the largest uncertainty in the models is found to relate to the chemistry and physics of aerosol evolution". This is not a new finding, what is new and different in your study?

Comment 18

L43: "where they cause a >surface< cooling"

Comment 19

L46: The two measures >which?

Comment 20

L48: seen > occurring

Comment 21

L49: Pls add to be correct: up to the largest know eruptions . F.e. the Siberian Trap volcanism was larger.

Comment 22

L55: "for larger eruptions the ratio is also found to also change over time". Be more specific what you mean here. Why does this matter?

Comment 23

L56-57: Do you mean "reaches limit of ..."? The linear aspect is well known from before. Also reaching a limiting value <of ...>?

Introduction

Comment 24

L61: AOD is defined for the whole atmosphere unless otherwise specified, Pls clarify what you mean here.

Comment 25

L64-71: Thanks for including the ERF background here, which I think could be a bit shortened.

Comment 26

L71-72: "and a general assumption of linearly dependence is commonly adopted." I strongly disagree here. To which kind of studies are you referring here?

Comment 27

L73-76: "of volcanic eruptions" For which sizes and periods? Be specific.

Comment 28

L80-81: small eruption with AOD ... at most 0.7: These are not small eruptions.

Comment 29

L98: "are readily removed by wet deposition". Really only wet?

Comment 30

The original reference that climate variability is forced by volcanic eruptions during the past millennium is Hegerl et al. (2006) next to Schurer et al. (2013). For the Holocene period you need to add van Dijk et al. (2024).

Comment 31

L111: observe > model/ simulates

Comment 32

L126-142: Structure and content to be reworked. Separate geoengineering study Niemeier and Timmreck (2015) from volcanic studies. Next, I suggest structuring the paragraph along earlier studies with prescribed aerosols which are linear scaled AOD from the more sophisticated prognostic aerosol climate (chemistry) models studies. This will allow a better flow in case you really need to add both.

Comment 33

L143-144: Metzner et al. (2014) has done this before with a two-step model approach using MAECHAM5HAM with varying SO2 injections from 0.4 to 687 Tg SO2 to derive aerosols, AOD, RF and then fed this into the CLIMBER model to calculate the climate response. The model results were then compared to the traditional linear approach (see Table 3, Figs. 6-8 of Metzner et al. (2014)). So, you need to include this study here as well to introduce what has been done before.

Comment 34

L145-163: Do you need this ECS paragraph for your study? Consider shortening and clearly relating it to the results of your study here. There is Section 3.4 but not much in the Discussion, Conclusion-Summary nor Abstract.

Comment 35

L164: "Magnitude or more greater": See my above comment. Magnitude has a specific meaning in volcanology based on the erupted magma mass which I assume you do not refer to here or?

Comment 36

L176: Needs to add Toohey et al. (2019) as this is the key paper in this context here.

Comment 37

L184: Why have you chosen these erupted SO2 masses, any motivation?

Comment 38

L189-190: Cut here. Separate geoengineering Niemeier and Timmreck (2015) from the volcanic studies and put it into a separate discussion at the end.

Comment 39

Introduction: Introduce different SO2 strength of eruptions and their climate impacts (Miles et al., 2004; Metzner et al., 2014; Schmidt & Black, 2022). To just cite SB22 is not enough.

Method

Comment 40

L201: MAM3: It is MAM4 in WACCM6 (Liu et al., 2016; Gettelman et al., 2019).

Comment 41

L210-211: So for what do these long component names stand, their context? Be specific and explain details for the non CESM model experts.

Comment 42

L215: How did you inject the SO2 mass; into one grid point at one time step? Please be specific and explain your model experimental set-up in more details as this matters.

Comment 43

L229: Why have you chosen these dates starting on the 15 Feb...? Pinatubo erupted on June 12th, unknown eruptions are put on Jan eruption month in PIMP4. So why those dates?

Comment 44

L235: A 400 Tg SO2 eruptions does not represent a 144-170 Tg SO2 eruption. Pls reword.

Comment 45

Table 1 is not complete: Pls also add the simulations with prescribed ocean sea ice. Don't you have a control run? How many ensembles? Initial states?...

Comment 46

L239: Why citing Jones et al. (2005)? You should add Robock et al. (2009) as well.

Comment 47

L239-242: Your injection height has likely a significant impact on your results, which needs to be motivated and discussed. Can you show your vertical SO2 injection distribution in the supplement? Why have you chosen it that way? The motivation for this is missing. Why not at 24 km altitude as was observed for Pinatubo?

Response: Thank you for the comment.

The default CESM2 historic (link to file) uses altitudes 18-20 for Pinatubo.

Comment 48

L267: show<s>

Comment 49

Fig. 1: Why the different temporal behavior for S26 compared to the others?

Comment 50

Fig. 2: grey lines are showing what? STrop? Make the link to the Gregory study/ECS more clear; how does this relate to the rest of your study? What is the take home message of it?

Comment 51

Fig. 3 Why adding linear fits?

Comment 52

Fig. 4 Exclude N15 line and markers. Include in the discussion.

Comment 53

L514-519: How realistic is a Reff >2.5 um McGraw et al. (2024)? "good model agreement": Well which model did McGraw use...?

Comment 54

L525-526: Include Metzner et al. (2014) here.

Comment 55

L534: Include Marshall et al. (2018) here as well.

Comment 56

L548ff: Discuss considering the Toohey et al. (2019) and Zhuo et al. (2024) results.

Comment 57

L556-559: Cite original studies such as Pinto et al. (1989); Bekki (1995) here as well. (commented before)

Comment 58

L574-576: Cut. I suggest to not relate and cite Jones et al. (2005) here anymore.

Comment 59

L594: "is in contrast ... MAM 3" Isn't it MAM4 Liu et al. (2016); Gettelman et al. (2019)?

Comment 60

L595-596: Distinguish between modal schemes and line by line schemes. Take out N15.

Comment 61

L614: ensembles? How many ensembles did you run per SO2 injection experiment?

Comment 62

L626-627: Not a new finding see Zhuo et al. (2024).

Comment 63

Brenna et al. (2020) does not use the same model configuration f.e. they use 1 deg resolution, which may matter for the aerosol evolution.

Concluding Response. Thank you for your valuable comments on our manuscript.

Authors' Response to Reviewer #4

They want:

- Specify model/method better
- Better describe ERF, IRF, SARF
- Use correct longitude notation
- Clarify figure

Reviewer #4 Evaluations

Evaluations.

Recommendation	Return to author for minor revisions
Significant	Yes, the paper is a significant contribution and worthy of prompt publication.
Supported	Yes
Referencing	Yes
Quality	Yes, it is well-written, logically organized, and the figures and tables are appropriate.
Data	Yes
Accurate Key Points	Yes

Reviewer #4 Formal Review for Author (shown to authors)

General Comments. This paper is a modeling study investigating the nonlinear response of effective radiative forcing (ERF) to stratospheric aerosol injection due to supereruptions. Supereruptions in this paper refer to eruptions with a Volcano-Climate Index (VCI) ranging from 3 to 6. The authors perform simulations with 4 levels of injected sulfur encompassing the range of supereruptions inclusive of and larger than Mt. Pinatubo, thereby extending the understanding

of the climate effects of volcanic eruptions. The temporal variation and sensitivity to SO2 injection magnitude of ERF and GMST were determined, and comparisons with previous studies' and model findings were included for corroboration.
This work is relevant to the journal and may also be of interest to the broader scientific community, as geoengineering applications of stratospheric aerosols continue to be explored. I recommend this work for publication with a few minor revisions.

Reviewer #4 Specific Comments

Comment 1

Line 47, "ocean and sea-ice is held fixed.": Please clarify what aspect is being held fixed.

Comment 2

Line 49, "While ERF takes into account rapid adjustments": It may be helpful to briefly describe the differences between how ERF, IRF and SARF are determined.

Response: Thank you for the comment.

Use O'Connor et al. (2021).

Comment 3

Line 135, "cilmate": Correct to "climate"

Response: Fixed.

Comment 4

Line 225, "287.7degree signE": The longitude should lie between 0 and 180 degrees East. Please clarify what this coordinate means and why 56degree signN was chosen for the high-latitude simulation.

Comment 5

Figure 2: Please clarify how many yearly means are included in the plot for each of the STrop simulations. Is it every year from eruption to year 20? What about the season of eruption?

Comment 6

Figure 4: It may be worth mentioning that the quantities described in the plots (ERF, GMST) are absolute values, since most of them are actually negative.

Comment 7

Line 554, "peek": Correct to "peak"

Response: Fixed.

Authors' Response to Reviewer #5

They want:

- Better model/method description
- Improve language

Reviewer #5 Evaluations

Evaluations.

Recommendation	Return to author for minor revisions
Significant	The paper has some unclear or incomplete reasoning but will likely be a significant contribution with revision and clarification.
Supported	Mostly yes, but some further information and/or data are needed.
Referencing	Yes
Quality	Yes, it is well-written, logically organized, and the figures and tables are appropriate.
Data	Yes
Accurate Key Points	Yes

Reviewer #5 Formal Review for Author (shown to authors)

General Comments. Review of "Radiative forcing by supereruptions" by Eirik R. Enger et al. (revised MS for JGR, following earlier peer-review of initial-MS submission to the same journal.)

This manuscript presents an analysis of interactive stratospheric aerosol model experiments to predict the radiative forcing that would result from so-called "supervolcanic eruptions", i.e. from emission of sulfur more than 10 times larger than Pinatubo.

The model simulations apply the WACCM6-MAM3 model, within the CESM model framework, and explore ensembles of simulations at 400, 1600 and 3000Tg of SO2, i.e. around 25, 100 and 200 times the sulfur emission from Pinatubo. The MAM3 model is an aerosol microphysics scheme, and for such very large eruptions, this model capability is particuarly important, to capture a key "self-limiting effect" within supervolcano-climate impacts. Such microphysics schemes are able to represent how the high sulfur generates much larger sulphate particles, with then a reduced lifetime in the stratosphere, for a short-lived, and also less severe surface cooling effect.

Although there have been other model studies published quite recently on this topic (e.g. McGraw et al. (2024)), there can be quite large differences between predictions with different interactive stratospheric aerosol models, and this study is very valuable, for providing further experiments to quantify volcano-climate impacts in this super-eruption regime.

The McGraw et al. (2024) study applied aerosol microphysics scheme also within a similar (but different) GCM, the NASA-GISS ModelE, with a different modal aerosol microphysics scheme "MATRIX". With there being relatively few studies to have explored the self-limiting effect, and the curve for how the effective radiative forcing and aerosol optical depth vary for SO2 injection larger than Pinatubom it is important these results are published.

This manuscript is a revised version, from an initial version already reviewed by 3 reviewers. I was not one of the initial reviewers, but have read through the reviewer comments, and the replies from the author team, and have referred also to the track changes manuscript (which indicates the changes from the original version).

Whilst my review focuses on this latest manuscript, without prejudice from referring to the reviews, I can see that the authors have made substantial changes

to the Abstract, Plain Language Summary, and the Introduction, to address the comments from the 3 reviewers, and I can see then that the manuscript is much improved from the original submission.

I was initially quite surprised that there are only 4 Figures within this submission to JGR, but the authors have included, within the key synthesis Figure 4, results from a remarkable number of other studies, further putting the results from this study into then a useful broader context, including the recent similar curves for ModelE-MATRIX from McGraw et al. (2024).

There is an encouraging consistency in the results from the two models, for the curves of ERF vs emitted SO2 (Figure 4b) and global cooling (delta-GMST) vs emitted SO2 (Figure 4c). There is good alignment also with the Osipov et al. (2020) study, but that applied the same ModelE-MATRIX model from the McGraw et al. (2024) study, so the alternate model predictions presented here will be of substantial interest to the community.

The authors improved the labelling of the model experiments to better communicate the relative magnitude of the SO2 emission and hemisphere-focus, and whilst my review identifies still a substantial number of minor revisions are required, these are straight-forward to make, and remain at a minor level overall, the recommendation then to publish after minor revisions.

The main area of the manuscript that still requires substantial revision, is the description of the model (section 2.1) and the model experiments (section 2.2), and these are the first 2 of the 5 "main minor comments" I raise in this review.

Within those initial 2 "main minor comments", I list there specific sets of revisions required, in each case, to enact the substantial improvement required to sections 2.1 and 2.2 respectively.

I can understand the focus has been to address the main comments from the prior review, but these earlier sections, to describe the model and its experiments are

important, to ensure reproducibility and traceability for the model experiments forming the analysis.

Overall, the manuscript is much improved, and my judgement is that the manuscript is now within "minor revisions", to then be able to be published once this specific set of revisions set out below are addressed.

Reviewer #5 Specific Comments

Main minor comments

Comment 1: M1

Section 2.1, lines 174-185 – set of minor revisions to improve the model description.

M1.1 Lines 174-178 refer to the fully coupled atmosphere-ocean configuration of CESM2, and lines 187 refers to applying "the coupled model version BWma1850 component setup", but then line 189 explains also the fixed SST version is also used

M1.2 Line 178 add a sentence to this first para to give the chemistry scheme used. The aerosol scheme is explained in lines 179-185 but readers need to be aware also of how the SO2 oxidation is calculated in the model. Are the simulations applying interactive stratospheric chemistry, as well as interactive aerosol? Or are these "specified chemistry" simulations, based on climatological or "prescribed" oxidant fields?

M1.3 Line 181 Further to comment M1.2, the text here says "default setting", but this needs to be clarified which settings are "default" here. I am assuming this refers to the aerosol setting, and am guessing maybe this refers to the specified geomtric standard deviation (sometimes referred to as "mode width"), which are specified then within the MAM3 scheme. I see the text here refers to Liu et al. (2016), but that is a tropospheric aerosol paper, and

I'm aware some models (e.g. MA-ECHAM-HAM) use different mode widths for when simulating volcanic aerosol clouds, to those used when the model predicts tropospheric sulphate aerosol. Note that the Liu et al. (2016) paper is re: MAM4, the 4-mode version, not the 3-mode version. Please check this, and re: the Mills et al. (2017) paper, applying MAM3.

Please clarify what is meant here by "default setting" and suggest to change to "modal settings" if that is the case, adding "(for the MAM3 aerosol microphysics scheme)". Rather than the Liu et al. (2016), better to cite a paper applying MAM3 for stratospheric aerosol, such as Mills et al. (2017). Further to comment above, is the approach here equivalent to one of the "specified chemistry" runs within that paper? Or is the SO2 oxidation calculated from interactive sulfur chemistry?

M1.4 Line 181 Further to comment M3 below, add a sentence to the end of this paragraph, to state explicitly that the model predictions here include, from the MAM3 aerosol microphysics, the key self-limiting effect of reduced stratospheric residence time from larger sulphate aerosol particles (e.g. Pinto et al., 1989; Rampino & Self, 1982; Turco et al., 1979).

Comment 2: M2

Section 2.2, lines 187-195

M2.1 Line 187 – "We are using" is too colloqial for JGR article, revise to "For predicting the super-eruption impacts, we use the .." and suggest to add "pre-industrial setting" before "coupled model version" (assuming that is the case, re: the 1850 there).

M2.2 Line 187 – "coupled model version BWma1850 component setup" – whilst this labelling here will mean something to the group of modellers running

CESM, this needs to be re-worded. Is this "BWma1850" referring to the year 1850, and then that greenhouse gases and ozone depleting substances etc. are set at pre-industrial levels within the GCM? Please just state what the corresponding "setup" corresponds to, referring to the basic forcings within the climate model. I guess the 2 main aspects of the "setup" will be the greenhouse gases and the anthropogenic precursor emissions (for tropospheric ozone and aerosol). Please provide the specifics of this 1850 setup here.

M2.3 Lines 188-189 – Similarly, "an accompanying" is too colloquial, change "and an accompanying fixed sea-surface..." to "with a 2nd ensemble applying fixed sea-surface temperatures". Delete the word "version" there, as this could be confusing, it's the same version of the atmosphere model, just the boundary condition is different. So avoid "version" (unless it's actually a different version of one of the model components).

M2.4 Lines 190-192 – For JGR article it is not appropriate to specify variable names from a code or script, even within the model description text. This sentence here seems to be explaining what is different about "fSST1850", but my understanding is that is just a label for the fact that the model is using climatological sea-surface temperatures from a previous pre-industrial control run of the fully coupled atmosphere-ocean model. If that's the case, replace the current sentence with a sentence that states that.

Comment 3: M3

Introduction, lines 76-79 (and suggest to join-up with 81-82).

Whilst the authors have replied to the reviewer comments acknowledging the role of aerosol microphysics, and effective radius, the Introduction in the revised

manuscript still does not include any sentences explaining about this, only the 3 words "including aerosol size" on line 73.

The simulations here apply the MAM3 aerosol microphysics scheme, and then the predicted AOD and ERF include the key self-limiting effect of a shortened residence time of volcanic aerosol from these very large SO2 emission cases.

My suggestion here is to add the following 2 sentences about this

"Within very high SO2 eruptions, a key effect identified since the late 1970s (Rossow, 1977, 1978; Turco et al., 1979) is a self-limiting particle-size effect implicit within the microphysics of the volcanic aerosol. In summary, for larger amounts of emitted SO2, particles in the aerosol cloud are larger, to then have greater fall speed, and reduced residence time in the stratosphere."

These sentences (or similar 2 sentences about this) could be incorporated into the text via adding a paragraph break before "In the case of tropical eruptions"

The 2 new sentences would follow-on well after the current 2 sentences there (the first being "In the case of . . . " and the second being "Upon descending below the . . . "). The 4 sentences would then be a paragraph referring to the processes and transport within the stratospheric circulation.

See also main minor revision M1.4 above, to also add a sentence to the model description to be clear this effect is resolved in the model predictions here.

Comment 4: M4

Introduction, lines 108-113

Whilst volcanic forcing is often given as a natural analogue for geoengineering, and certainly shares common processes with impacts from a one-off volcanic SO2 emission, the continued emission from SAI may result in a quite different resultant size increase.

The studies on lines 108-113 are from continuous SO2 emission, and although that's interesting to consider, then with the inverse exponential relationship also

indicating a self-limiting effect, the effect may not be comparable to one-off super-eruption case.

This paragraph from lines 108 to 125 is a long paragraph, and suggest to have the text "Even the 100 x Pinatubo ..." be the start of a new paragraph (but re-worded to begin simply "The 100 x Pinatubo ..."

That then gives space to add a sentence after line 113, to note this, something similar to the 1st sentence above, re-worded slightly (delete "certainly" for example).

Comment 5: M5

Results line 230-231

The sentence here refers to "means across the ensembles", but the section 2 hasn't given any information about the ensembles

See "other minor revision" O15) (comment 20) below, where I've suggested to delete the sentence beginning "The AOD ..." on lines 204-207, this being superfluous. Doing so would then give space within section 2.2 to give information about the model ensembles.

I am assuming that there are 2 types of ensemble, one within the alternate fixed-SST configuration, and other other in coupled atmos-ocean model configurations (from the explanation in section 2.1).

With the additional variability in the ocean, I am assuming the ensemble woudl need to be larger for the coupled atmos-ocean ensemble, unless the sampling protocol has prior state, to then be enacting only a limited ocean variation within the coupled ensemble? Please clarify how the ensembles are set-up from the prior coupled atmos-ocean 1850 control.

List of other minor comments

Comment 6: O1

Manuscript title, line 1

The authors have revised the title, replacing "super-volcano eruptions" with "supereruptions", which is OK, except that the word supereruptions would be much easier to read if hyphenated as "super-eruptions". I notice hyphenated spelling "super-eruption" is used quite extensively, although certainly not exclusively, and for example see this BBC article `https://www.bbc.co.uk/sn/tvradio/programmes/supervolcano/article.shtml` consistently has the hyphenated spelling. In my opinion it is much easier to read that way.

Please change all instances of "supereruption" to "super-eruption". Thanks.

Response: Fixed.

Comment 7: O2

Introduction, line 45 – "are crucial metrics used to ..." the word "crucial" is hyperbole, and not really appropriate for a JGR article. Perhaps "the main climate-relevant metrics"?

Comment 8: O3

Introduction, line 46 – change "represent the opacity" to "represents the opacity"

Response: Fixed.

Comment 9: O4

Introduction, line 46 – change "while ERF specifically is the energy imbalance". I get what you mean by "imbalance", in that the forcing agent causes a difference. But it's not necessarily moving the system towards an imbalanced state. Better to refer to the ERF simply as a difference in radiative flux or "flux difference".

Suggest then to re-word to "while the ERF is the energy flux-difference at the top-of-the-atmosphere" or similar.

Response: Fixed.

Comment 10: O5

Introduction, lines 47-49 – change "Radiative forcing can however be calculated differently". That wording there is not specific enough, and the follow-on "an agreed-upon methodology has thus not always existed" is a strange thing to say. One could say "has not always existed" about any scientific quantity. Suggest to re-word, to explain instead the difference between instantaneous radiative forcing and effective radiative forcing (i.e. re: rapid adjustments within the atmosphere system).

Comment 11: O6

Introduction, line 53 – "the most precise indicator" – it's not really correct to refer to an issue of "precision" here. Any calculation can be precise or imprecise. Maybe you meant "a reliable indicator"? But even then, I'd say that's not really the issue, it's more accounting for the energy changes at the top of the atmosphere, rather than any predictabillity of surface temperature change.

Comment 12: O7

Introduction, line 59 – Re-word "Yet, a wide spread in the estimated aerosol forcing efficiencies". The word "Yet" there doesn't really make sense. The models are making different predictions, but they're all predicting that based on different simulated clouds etc., so whether there's a linear relationship or not isn't really relevant to the spread in the model predicted forcings. It's certainly relevant to the magnitude of any 1 model's prediction, but it's not really relevant to the spread

between different model predicted forcings. Basically the spread among different models isn't really relevant to this discussion about the relationship between ERF and AOD. One can consider any 1 model has some error bar associated with its predictions, and then that is relevant, but multi-model spread is a different issue.

Comment 13: O8

Introduction, line 67 – delete "molecules" there, and suggest to change "transformation" also, instead to "reaction"

Response: Fixed.

Comment 14: O9

Introduction, line 68 – suggest here to simply refer to sulphate aerosol (rather than sulphuric acid).

Comment 15: O10

Introduction, line 123 – change "revealed" to "found".

Response: Fixed.

Comment 16: O11

Section 2.2, lines 197-207

These sentences beginning "ERF is calculated as the ..." need to be re-worded to avoid referring to variable names in the code. The explanation is OK, but for JGR article, needs to refer to the actual quantities, rather than variable names in the code. It's also not quite correct to refer to an "energy imbalance", it's better just to explain the forcing as the difference in the radiative flux at the top of the

atmosphere caused by the forcing agent. Please discuss with the co-author team, and replace with a revised wording here.

The text beginning "The AOD" on lines 204-207 is superfluous, and can be deleted. See main minor revision 5, I've suggested to delete this text, and give information here instead about how the ensembles are set-up, within the alternate fixed-SST and coupled atmos-ocean model configurations explained in section 2.1

Comment 17: O12

Line 208 – Further to the comment earlier about ensemble – suggest to include in Appendix A the detailed description of how the ensembles are set up, and then in the text (within lines 230-231) can simply refer to the Appendix A for details, perhaps with a label for each ensemble such as "FixedSST_ENS10" and "CoupledAO_ENS30" or similar (the 10 or 30 indicating the ensemble size).

Comment 18: O13

Table 1 – Change the column "Eruption months" instead to "Eruption timing". (To be clear it's not simply a sampling at different months in the year, it's when the eruption is enacted in the model).

Comment 19: O14

Table 1 caption – the text here states "The three smallest in eruption magnitude tropical ensembles have four members, indicated by the number of eruption months". But presumably each eruption month has more than 1 ensemble member, right? These are composition-climate model predictions so the ensembles are presumably of a reasonable number of members, not just 1 per eruption month? Please clarify, I'm assuming the ensembles are comparable in magnitude to the ensembles within McGraw et al. (2024), right?

Comment 20: O15

Figure 1 legend – add "Wm-2" and delete the 3 characters "C = ". (It's important to communicate the units of the normalisation constant here.) Changing the "18.27" and "12.43" to "18.3" and "12.4" here will give an extra 4th character, then sufficient space to add " Wm-2" where the "-2" is super-script. and do the same in panel b), for "-71.42" and "-63.71" to "-71.4" and "-63.7". This also ensures numbers are all reported to 3 significant figures).

Comment 21: O16

Figure 1c – it's important to be able to see the tick-marks on the right-hand inset Figure. You could give the values to 1 decimal place for the S3000 and S1629 to give then room for the Wm-2, after also deleting the "C = " (only 2 significant figures is fine here).

Comment 22: O17

Figure 1 caption – Change "The time series have been normalised to have peak values at unity" instead to "Each time series has been normalised to the peak value". You don't need to say "at unity", people will understand that's what you mean by "normalised". Also change ", where C is the normalisation constant" instead to ", values of C given in the legend.". The value of C is more significant than simply a normalisation constant, and better just to give this without that labelling.

Comment 23: O18

Figure 1 caption – The "across the ensembles" is not clear. See comment 19: O14, the text currently does not communicate sufficiently what each ensemble is representing, and once this is clarified the Figure needs to be amended to refer to the label for each ensemble. Please also add the number of ensemble members

within the fixed SST and coupled-ocean ensembles (or clarify which are shown in this Figure).

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