

We appreciate the time and effort by the referee in reviewing this manuscript. We will address all issues, as highlighted below (reviewer text in red):

“The ITCZ shifts away from the hemisphere with greater forcing”- please specify that you mean greater NEGATIVE or VOLCANIC forcing.

Agreed that this is improved phrasing, we will correct to “The ITCZ shifts away from the hemisphere with greater volcanic forcing.”

Line 136- how many latitudinal bands does G08 use? Also line 144- could you add some more detail about the G08 dataset and how it was derived- e.g. ice core based? And more information about the aerosol transport model?

and,

Line 157 The stratospheric sulfate aerosol loadings given by G08 are a function of latitude, altitude and month”- What resolution is G08 both horizontally and vertically?

The G08 dataset provides sulfate aerosol loading from 9 km to 30 km (at 0.5 km resolution) for each 10° latitude belt (from 90°S to 90°N – i.e., 18 latitude bands). It is derived from ice core sulfate records. We will add more information in the revised manuscript.

Line 140- “the impact of these smaller amplitude and slowly varying forcings is very small.”- Did you test this, or is it speculation?

This was tested in single-forcing runs in CESM, and with multiple simultaneous forcings in GISS (no volcanoes in each). The “composite” results obtained by averaging over the same dates as in the volcano composites are indistinguishable from noise, and averaging over hundreds of “events” would produce a nearly blank anomaly map (white almost everywhere), just as if the analysis were repeated in a control simulation. This is due to averaging over internal variability, and due to the fact that any instance of solar, greenhouse, orbital, or land-use forcing (relative to the immediate five year interval before) is very small prior to 1850.

Line 154- is this at different levels in the stratosphere as well?

Yes– in the GISS model, there is an optical thickness from 15-35 km. We will make a note of this in the revised text.

Line 175- How big is Pinatubo for comparison?

Pinatubo remains elevated at ~20-30 Tg sulfate aerosol in the G08 dataset for about a year, and drops off to <1 Tg after 4-5 years.

Line 184- is there any reason for using MJJAS and NDJFM for the warm and cold season rather than MJJASO and NDJFMA? I expect the results would be similar, but I’m just intrigued!

In our analyses, we intended only to capture any sensitivity of the anomalous response to seasonality, and we feel our choice is appropriate for that target. In the early stages of the manuscript we did the analysis for DJF and JJA, and decided to expand the month range to include more of the data, but the conclusions did not change, and any differences were barely noticeable “by eye.”

Lines 217-218 – “The G08 reconstructions used a simple transport model that does not allow for cross-equatorial aerosol transport” –I’m a bit confused as to what exactly this means and what the implications are- does it mean that if an eruption happens one side of the equator that none of the aerosols go to the other side?

We apologize for the confusion, and will modify the text. Two datasets emerged from the G08 study, the first an aerosol injection dataset for each hemisphere (in mass units). The second dataset (used for forcing GCMs) provides latitude/altitude information of aerosol concentration (at the resolutions previously mentioned). In this second dataset, the spatial evolution was derived from a simple model that parameterizes transport between the tropics, extratropics, and poles, and they interpolated the vertical distribution of aerosols based on information from the Pinatubo eruption. Cross-equatorial transport of aerosol was not allowed, and so tropical eruptions that left an imprint in both polar regions were represented by separate aerosol injections in both hemispheres. This was done since the ice core estimates provide information on the hemispheric distribution of volcanic aerosols, information that could only be preserved in their setup if hemisphere-only transport was permitted.

It is true that these details influence the volcanic forcing in all of the CMIP5/PMIP3 (and CESM LME) runs that utilized G08, and we do not take a position in this paper on the realism of the reconstruction. Improvements in volcanic forcing are at the forefront of research on last millennium climate, and we expect advances in CMIP6. For our purposes, however, this does not matter since the different composites (ASYMM_{NH}, ASYMM_{SH}, SYMM) have been formed from a forcing distribution that was imposed on the GCM and is perfectly defined. Thus, while forcing uncertainty (either in timing, magnitude, or spatial structure) is an important consideration for connecting the model results with paleoclimate proxy data, the responses we report are self-consistent with the forcing.

Line 248 “In the ASYMMNH and SYMM results, the cooling peaks over the Eurasian and North American continents.” - But not in SYMM MJJAS

This is correct, we will modify the text. Thank you.

Line 250: Mid latitudes? or is it more high latitudes? Maybe mid to high latitudes?

We will write “mid-to-high latitudes.” Thank you.

Line 264: “suggesting AET away from the forced hemisphere” Do you mean towards?

Yes, thank you for spotting this typo.

Line 271: “after normalizing each event by a common global aerosol mass excursion, thereby accounting for differences in the average forcing among the different eruptions”. Maybe add a caveat that this doesn’t take into account things like coagulation of aerosols for bigger eruptions which tends to reduce climate effects for a given mass of aerosols (see Timmreck et al 2009), and assumes that the response pattern scales linearly. For ITCZ excursions, the end of the paper suggests that this is not the case for asymmetric forcing- the ITCZ moves more for a bigger forcing gradient between hemispheres.

Agreed on all points, thank you. We will modify the text to caution interpreting a normalized metric in the presence of non-linear effects.

Line 288- does this alignment error affect all/many eruptions in this dataset? Or is it just Laki?

Most eruptions are not impacted, although some smaller eruptions were excluded or mis-aligned (<http://climate.envsci.rutgers.edu/IVI2/IVI2Version2ReadMe.pdf>). Note that all participating models used version 1 of the Gao et al. dataset. We will carefully consider which dates were affected that also overlap with eruptions in our analysis, although Laki is a well-known, well-studied, and large asymmetric (Icelandic) eruption, which is why we highlighted the error. Of course, our analysis is not impacted since all dates used in the composites are consistent with those in which the aerosol loadings occur in Gao v1 dataset.

Line 323- Maybe mention some more of the ENSO and volcano studies that have been done in the past- Whether or not volcanoes influence ENSO was certainly a bit of a controversial issue in the literature at one point. I am not totally up to date with the most recent studies though. Line 336- how big is a 0.5 C El Nino anomaly compared to a typical El Nino event in the model? (e.g. a 1 standard deviation event?) Also, is it statistically significant?

We will add citations to the revised manuscript to improve the ENSO segment.

In CESM, the El Niño events are too large (relative to observed amplitudes over the historical period) and a 0.5 °C anomaly is well within the model’s range of natural variability. However, the composite results always represent an average over hundreds of events, and the event mean stands out following volcanic eruptions (Figure S6). We will improve the statistical justification for this conclusion in the revised manuscript. We interpret the positive SST anomalies in the eastern Pacific (in the composite) as a forced response, or equivalently, we argue that volcanoes pre-condition the system toward El Niño conditions. We will clarify this in the text and in the presentation of Figure S6.

“Line 345- It might be helpful to remind the reader that Samalas is somewhere between NH and SYMM.”

We will add a note to the revised text. Thank you.

Line 364 “[rivers] are a useful variable in the context of monitoring since they integrate precipitation changes over time” – and space. I would have thought that rivers would be more useful in integrating precipitation changes over space than over time?

We agree; we will modify the text appropriately. Thank you.

Lines 371-393- I feel that these paragraphs disproportionately emphasise the regions where streamflow increases, when it actually decreases in a lot of areas. Can you make it more balanced? Line 383-384 In ASYMMSH, “the ITCZ moves northward, resulting in reduced river flux in the Amazon sector and increases in the Niger of central/wester Africa” This is true for summer, but river flow decreases over the Niger in winter.

We emphasized increases just because there is previous literature that reports declines in riverflow following large eruptions, but we fully agree the discussion should be more balanced. We will modify the text to highlight both increases and decreases in river flow. Thank you.

Line 407- is it worth mentioning that there are factors that affect d18O in the process of being incorporated into paleo archives from precipitation? Lines 426-417- could you outline briefly how the amount and temperature effects work and in which direction they affect d18O concentrations?

In this paper, we wish to avoid discussing/speculating on the proxy system itself (e.g., cave-specific sites, etc.) which is a further complication for paleoclimate. The aim here was to highlight the large-scale imprint on d18O in precipitation following volcanic eruptions.

We will add text to make clear the known d18O-T and d18O-P relationships. Thank you.

Line 455 “In regions where tropical South American precipitation does not exhibit very large changes, such in the NDJFM SYMM composites, temperature may explain much of the isotopic response, again consistent with findings in Colose et al. (2016).” Can you specify in which direction and how temperature affects the isotopic response?

As above, we will revise the manuscript to make this explicit. Thank you.

Line 470-1:- are the arrows the right way round for the LW fluxes? They seem to be the opposite way round to the SW ones.

There was a mistake in this section. We will modify the SW arrows. Thank you.

Line 509 “Moisture makes it more difficult for the tropical circulation to transport energy poleward”. How?

In the tropics, the latent heat flux is towards the Equator owing to the transport of moist air in the low-level branch of the Hadley circulation. The circulation that cools (warms) the deep tropics (subtropics) by adiabatic expansion (compressional heating) also carries latent heat equatorward.

Figure 1: It would be nice to be able to see the absolute size of the volcanoes as well as the hemispheric contrast in aerosol loading- can you put in an extra time series? At the moment a perfectly symmetrical eruption will not show up at all. The overlap between the red and black lines also makes it difficult to see how big the black lines are in some cases. Also- what does FSNTC stand for?

We agree. We will create a completely new figure 1 to also highlight the absolute size of the eruptions and remove line overlap.

FSNTC is the name of the clear-sky net shortwave flux (at TOA) diagnostic in the CESM (CAM) history fields. We will replace this for clarity.

Figure 2- I assume this is surface temperature? (Rather than temperature at a different level in the atmosphere?)

Yes– all temperature plots (except the latitude-pressure 3-D temperature figure in the supplemental) are for the surface. We will write this in a revised caption for clarity.

Figure 8 panel a- the legend is a bit small. Panels b and c- The colour of the thin lines is confusing because they are not that similar to their corresponding thick line- e.g. the thin orange lines look like they go with total AET rather than the dry component. Also- what depth of ocean is this for? All of it? And: “Grey envelope corresponds to the total AET anomaly vs. latitude in a control simulation using 50 realizations of a composite formed from the same dates as the ASYMMNH results”- I am not sure I entirely understand what you mean by this- are there 50 control runs? If there is only one control run, how are there 50 realisations if the same dates are used each time?

Thank you for highlighting an error in the description. First, we will remove the climatological ocean heat transport curve, since it is not part of our study. The poleward heat transport was for the full ocean. We will make the colors of all lines on the anomaly plots consistent with those used on the climatology plot, and improve the legend size.

There is only one control run. The anomalous transport plots show the post-volcanic spread in AET and its dry/latent components (each line shows a different eruption after averaging over the ensemble member dimension). The grey envelope and rectangles in Figure 8b,c are there to illustrate that the post-volcanic response is typically larger than would be expected if the analysis were repeated on a control simulation. To do this, we selected 16 different two-year intervals (each expressed as an anomaly relative to the

previous five years) in the 850-1850 C.E. period, and averaged those 16 anomaly fields together. This analysis produces a single line in the transport-latitude plane, which does not collapse to zero due to the finite averaging size. Averaging over a larger number of cases than 16 would suppress the spread further, essentially mimicking the effect of having a larger ensemble. The analysis is repeated 50 times, in each case with a random selection of years, in order to generate a spread in AET anomaly at each latitude. This is a benefit of a long control run.

The value we used should be the size of the actual ensemble for comparison, which was 14 in the discussion manuscript (for this figure). Since then, additional ensemble members have been released (now 17), so the analysis will be repeated and reflected in all plots and in the discussion. We will improve the caption and discussion.

Figure 9: Could panels a and b be on the same scales to make them more obviously comparable?

Yes, we will revise the figure. Thank you.

Supplement figure S7- You don't mention what the box is showing.

Thank you, this is the Niño 3.4 domain. We will insert this in the figure caption.

Technical corrections: Line 226- you have missed Iles and Hegerl 2015 off the reference list at the end.

and,

Line 454 – “such >AS< in the NDJFM SYMM composites”

Thank you, we will modify the text and reference section accordingly.