Response to Reviewers

Ram Singh et al. (2022): - Investigating hydroclimatic impacts of the 168–158 BCE volcanic quartet and their relevance to the Nile River basin and Egyptian history.

We thank the reviewers for their time and valuable comments on our manuscript. We present below the comments in black, our replies in blue, and the changes made to the text of the manuscript in blue italic font. Line numbers correspond to the original manuscript.

Apart from the reviewer's comments, we also revised the manuscript sections for some general editorial revisions for better representation of results in the final version of the manuscript and included more relevant references. Track change version of the manuscript and supplementary information are also uploaded.

Response to reviewer 1.

1.0. This article employs paleoclimate modelling to investigate the impacts of volcanic eruptions on hydroclimate, particularly the African monsoon and Nile flow, and thereby to assess whether and how historical eruptions may have been responsible for revolts in Ptolemaic Egypt. The study represents a valuable step in integrating historical research and paleoclimate modelling. However, the article could benefit from substantial reorganization, and it requires a clearer discussion of whether and how to attribute historical societal impacts to volcanic eruptions and climatic variability.

I would recommend substantially reducing and reorganizing the introduction for greater precision, clarity, and a logical flow. Currently, this section is very long and shifts among a number of topics. The introduction needs to establish only the following contexts and in the following order: (1) Volcanic eruptions are a major driver of historical climatic variability. (2) This includes suppression of precipitation, the ITCZ, and the African monsoon. (3) Thus, volcanic eruptions probably reduced the flow of the Nile. (4) Nile flood levels were historically crucial for Egyptian agriculture and thus the populations and states that relied on that agriculture. (5) There is a correlation between the timing of volcanic eruptions and timing of revolts in Ptolemaic Egypt but not sufficient historical records to demonstrate that there was a low Nile flow during those years. (6) Therefore, this study uses paleoclimate modelling to determine to what extent volcanic eruptions such as those experienced in the Ptolemaic period were sufficient to suppress the flow of the Nile. (7) This study can enhance our understanding of volcanic forcing of the climate, as well as the study of Egyptian history and the integration of paleoclimate and historical research.

We thank the reviewer for their obviously considerable time and effort in reviewing this paper in such detail, for the frank assessment of its merits and limitations, and constructive guidance on possible ways forward. We agree, to begin, that the article's overall Introduction should be amended and more extraneous material condensed or removed. In our revisions, we have thus removed several paragraphs outright, which are now accessible in the Supplement. We have also emphasized the key points recommended by the reviewer. In our revisions to the introduction (for more on which see our responses to the specific points raised by the reviewer below), we have also clarified the intent and scope of this article which, in our initially submitted draft, was clearly not sufficiently conveyed.

The article itself has been intent primarily on (1) detailing the efforts (methodological) to credibly model the hydroclimatic impacts (particularly for the Nile basin) of a set of four closely spaced explosive eruptions as registered in polar ice-cores between 168 and 158 BCE, for a period sufficiently remote in time as to require a careful accounting for model parameters such as vegetation cover that was at the time of these eruptions meaningfully different from the modern era. The intent following from this was then (2) to present the results of this modeling in the context of an ongoing interdisciplinary project (US NSF Award #1824770: “Volcanism, Hydrology and Social Conflict: Lessons from Hellenistic and Roman-Era Egypt and Mesopotamia”) to more broadly establish the socioeconomic and cultural impacts of hydroclimatic variability arising from historical volcanism during the Ptolemaic period in Egypt (305-30 BCE).

Given the space required to do justice to the modeling efforts and results, the goal of the paper is therefore not to break new ground, per se, in assessing the societal impacts of the “volcanic quartet” of 168-158 BCE. Rather, the goal is to present the modeling results here in full, providing a (modeling) foundation for carrying out such an assessment in a later (informal follow-up) paper, without competition here for space with the many other relevant lines of historical and archaeological evidence that are being considered in this follow-up paper.

In presenting the modeling results here, we do wish however to (1) reflect upon (as per our Discussion section) the importance of modeling as a contributor to interdisciplinary studies of human-environmental entanglements, (2), present the model results in the context of the project’s work to date on establishing a diachronic statistical link between political activity such as revolt and volcanically induced hydroclimatic variability in Ptolemaic Egypt (as per Manning et al. (2017)), and (3), set the stage for how a close case study of a particular decade known for its political instability and (now) for its likely marked hydroclimatic stress in Egypt (i.e., the 160s BCE), can allow us to push further in understanding underlying causal linkages.

In the above respect, the reviewer’s methodological guidance on causality is certainly relevant to highlight, though its actual practical application in the present paper is beyond its intended scope.

1.1. Most of the other material currently in the introduction, including the discussion of climate as a causal factor in Egyptian history, should be edited out or moved to the discussion section. The introduction should also acknowledge previous research on volcanic eruptions, Nile flood levels, and famines in during recent centuries, for which there are Nilometer measurements and abundant historical records—see especially Alan Mikhail, ‘Ottoman Iceland: A Climate History,’ Environmental History 20 (2015): 262–84. https://doi.org/10.1093/envhis/emv006. This research, particularly for the Ottoman era, already makes a strong case that volcanic eruptions have had major historical impacts on Egyptian society by causing low Nile flow, shortages, epidemics, and political instability (indeed, a stronger case, with richer detail, than is possible for ancient history).

We have now included reference to Mikhail’s work, as well as several other authors studying human-environmental relations in both earlier and later periods of Egyptian history. These include:

Bell, B. “Climate and the History of Egypt: The Middle Kingdom,” American Journal of Archaeology 79/3 (1975): 223-269.

Butzer, K. W. “Long-term Nile flood variation and political discontinuities in pharaonic Egypt.” In: From Hunters to Farmers: The Causes and Consequences of Food Production in Africa. Eds. Clark, D. and Brandt, S. A. Berkeley, 1984, pp. 102-112.

Hassan, F. “Nile Floods and Political Disorder in Early Egypt.” In: Third Millennium BC Climate Change and Old World Collapse. Berlin: Springer, 1997, pp. 1-23.

Hassan, F. “The Dynamics of a Riverine Civilization: A Geoarchaeological Perspective on the Nile Valley, Egypt”, World Archaeology 29(1) (1997): 51-74.

Said, R., The River Nile: Geology, Hydrology, Utilization. Oxford, 1993.

McCormick, M. “What climate science, Ausonius, Nile floods, rye, and thatch tell us about the environmental history of the Roman Empire.” In: The Ancient Mediterranean Environment between Science and History. Ed. Harris, W. V., Brill, 2013, pp. 61-88.

1.2. The real question is whether this was also the case in the Ptolemaic period. The article’s arguments regarding attribution of societal impacts to volcanic eruptions are often imprecise. I would stress that the attribution of societal impacts to climate variability should be as clear and logical as the attribution of climate impacts to climatic forcings. In this case, the authors aim to evaluate whether and to what extent volcanic eruptions were responsible for revolts in Ptolemaic Egypt. They have made a prima facie case for a causal connection in previous research, which demonstrated a correlation between the timing of eruptions and timing of revolts. Now they are taking this causal argument one step further.

The reviewer in fact expresses one of the underlying goals of our project very well here: that the attribution of societal impacts from volcanic eruptions in our study period/region should be as clear as the attribution of hydroclimatic variability from these eruptions. We are, however, now clearer in stating that this is not the ultimate goal of the present paper.

Thus, we state in the Introduction: *“In this study, our main intent is to advance our understanding of the likely hydroclimatic impact of his eruption quartet as a foundation for further work aimed at establishing the nature of the causality underlying the observed association between volcanic eruptions and Ptolemaic-era internal revolts.”*

Given this, in the present paper, our contextual discussion of the potential role of the hydroclimatic variability (which our modeling results now bring into much greater clarity) must for now be expressed in more contingent and conditional terms. That said, as per our response further below, we have added a more explicit statement on what our work in previous papers has done to date (by way of establishing a causal link between eruption-induced hydroclimatic variability and revolt) and what remains to be done.

1.3. In this regard, the article should first specify its causal argument(s), preferably in contrastive terms. (For more on this issue, see e.g., S. White and Q. Pei. 'Attribution of Historical Societal Impacts and Adaptations to Climate and Extreme Events: Integrating Quantitative and Qualitative Perspectives'. Past Global Changes Magazine 28, no. 2 (2020): 44–45. https://doi.org/10.22498/pages.28.2.44) Do the authors mean to argue that the presence (rather than absence) of volcanic eruptions caused the occurrence (rather than non-occurrence) of revolts? Or do they mean to argue that the timing of the volcanic eruptions explains the timing of the revolts (which may have occurred anyway but in different years)?

Or is it some other distinction about the eruptions or climate forcing that explains some other difference in societal impacts? I would stress that these are each very different arguments (though not mutually exclusive). They each require different evidence and each have different implications for Egyptian history. Until the authors specify which causal argument(s) they are making, it is difficult to determine whether they have succeeded or failed.

We thank the reviewer for their reflection on the nature of possible causal linkages and characteristics. We have now included several citations to White and Pei's (2020) valuable framing paper in attempting to better clarify the contribution of the present paper, and how it may contribute to future research into establishing and characterizing the causal relationships between sudden hydroclimatic variability and various political and socioeconomic behaviors in Ptolemaic Egypt, including revolt. See also our response to the point below.

1.4. If the article intends to determine whether and to what extent the occurrence of eruptions were responsible for the occurrence of revolts in Egypt, then that will require a more clear and rigorous approach to causation. To clarify this problem, and to avoid some of the confusion that often clouds discussions of climate impacts on human societies, it may help to use a simple analogy. Let us suppose a doctor prescribes vicodin (v) to a bus driver without offering appropriate warnings about its side effects. The bus driver subsequently causes a road accident in which another driver is injured. The injured party sues the doctor on the basis that the negligent prescription (v) caused erratic driving (d) and therefore the accident (a) and the injury (i). In common law, to demonstrate the doctor's responsibility for the injury the injured party would have to demonstrate with a preponderance of evidence at least the following two points: First, that the negligent prescription for vicodin was specifically necessary for the injury to occur (i.e., the "but-for" test). Second, that negligently prescribing medication is somewhat sufficient to cause injuries in general (i.e., the "harm within risk" standard). We could also express these two causal chains as two sets of conditional probabilities that would have to meet a reasonable threshold: first,

p(v|d), p(d|a), p(a|i) and second, p(D|V), p(A|D), p(I|A), where lowercase letters stand for specific real-world events and the capital letters stand for a type of event in general. These legal standards capture everyday understandings of causation and responsibility as well as centuries of philosophical discussion and legal experience.

While all this might seem a long way from volcanoes and instability in Ptolemaic Egypt, the issue of attribution here is basically the same. To what extent was a volcanic eruption (v) responsible for political instability (i), throughout the mechanisms of drought (d) and famine (a)? To attribute the political instability to the eruption, a preponderance of evidence should demonstrate a strong chain of specific necessity and at least a weak chain of general sufficiency from (v) to (d) to (a) to (i). If there were alternative sufficient causes and the eruption was not necessary for the outcome—let's say another climatic event would have caused a drought even in the absence of an eruption—then we cannot attribute the societal impact to the volcano at all. If the chain of causation depended on extraordinary contributory factors—let's say the Ptolemaic empire was unusually reckless or vulnerable to instability (not wearing its seatbelt, metaphorically speaking)—then the causal responsibility of the eruption would be much diminished, and it would be misleading to refer to the eruption, rather than weaknesses within the empire, as "the cause" or even "a cause" of the occurrence of revolts. Much of the historical discussion in the paper suggests this may have been the case.

We again thank the reviewer for this commentary, and we are particularly happy that it is accessible as a guide to others given the open peer review format of Climate of the Past. In our revisions, we have now placed more explicit emphasis on the importance of establishing and qualifying the character of causality in future work, such as in our planned follow-up case-study paper. For example, in our Introduction, we now state:

"For Ptolemaic Egypt, the temporal correspondence between internal revolts and explosive volcanism certainly appears recurrent and non-random (Ludlow and Manning, 2016, 2021; Manning et al., 2017; Izdebski et al., 2022). That the revolts and volcanic eruptions under study are known from different archives with independent chronologies (historical documentary and ice-core) has also helped to exclude potential biases in estimating this statistical significance. For example, inflated positive correlations may result when events are known from the same sources (e.g., between extreme weather and societal stresses such as famine or disease, if those instances of extreme weather that contributed to such stresses were more likely to have been documented than those that didn't (White and Pei, 2020)). While the results of Ludlow and Manning (2016, 2021) and Manning et al. (2017) thus imply a causal linkage between explosive eruptions and Ptolemaic-era revolts, much work remains to determine its underlying character, including how direct or indirect it may have been, whether this changed meaningfully between revolts (which varied in date, geography and scale), and (relatedly) what pathways were in effect to "operationalize" any such linkage. Answering such questions is now deemed a key challenge for climate historians and related scholars (White and Pei, 2020). Taken alone, such a correlation does not establish (nor

necessarily even imply) causation. Causality is, however, at least implied in cases where analyses are conducted alongside statistical significance testing, with the resulting correlations considered unlikely to have arisen purely by chance, and when such results are interpreted with reference to the relevant historical context, allowing causal "pathways" to be credibly hypothesized (Izdebski et al., 2022).

1.5. What this study has done is to take a one small but important step toward demonstrating potential causal responsibility of volcanic eruptions for Egyptian instability by demonstrating the causal sufficiency of eruptions for Nile droughts in general: p(D|V). The paper needs to put this contribution in perspective and not claim to do either more or less. It should neither hide nor exaggerate the significance of this contribution with vague language about volcanoes "playing a role" or an "environmental context" for the disaster.

As per our response to 1.2, we have been deliberately careful in our use of language precisely because it is beyond the scope of the present paper to ultimately delineate the character of the potential underlying causality which (agreeing with the reviewer) is not likely straightforward. In our revisions, we have now emphasized that the goal in future work will be to move to a greater precision in specifying causality than is currently allowed. We also better emphasize (as stated previously) that the modeling results presented here, by informing us of the likely magnitude and persistence of the hydroclimatic variability experienced in the 160s BCE, will provide an important aid to this effort, and that this is the main intent of the paper.

1.6. It is entirely possible that we could one day demonstrate that volcanoes were causally responsible for revolts in Egypt, with similar standards and rigor that courts use to assign legal responsibility for damages. This is more than "playing a role": it is causal responsibility. However, this would require further research into other steps in those causal chains, including comparisons with better documented episodes during the medieval and Ottoman eras. On the other hand, if there were alternative sufficient causes of the drought, famine, or instability, or if Ptolemaic Egypt only faced problems because it was extraordinarily vulnerable, then it does not make sense to talk about the eruption as the cause of revolts at all (except perhaps as a trigger for the timing of the revolts). Talk about "a role" for the eruptions would be more misleading than helpful. Nor does it help to include additional historical context (i.e., lines 795-843) if that context is not clearly addressed to a causal argument. If the authors intend to state that there were (or were not) alternative sufficient causes for Egyptian revolts besides eruption-induced droughts, then they should state that clearly. If they intend to state that changes in Egyptian leadership explain why some eruptions were followed by revolts but others were not, then they should also state that clearly. Otherwise, readers are left to infer causal arguments where the authors may not have intended them and where they may not be warranted. I can see that the authors are aiming for greater subtlety and sophistication; however, additional information that is not clearly tied to the causal argument(s) creates more confusion than clarity. Clearly, this study cannot yet provide a definite answer to

the question of causal responsibility of volcanic eruptions for the occurrence (or is it timing?) of Egyptian revolts—nor does it need to. However, the authors need to be clear what contributions they can make to this question: that is, how we may update our assessments of the probabilities of necessity and sufficiency along relevant chains of causation. They may also explain what questions remain to be answered and how further research might address them.

We thank the reviewer for the continued constructive guidance here, which will be put to good use in our planned follow-up paper, which will undertake a case study of the “role” of the volcanic quartet of 168-158 BCE in the revolts and other major societal stresses of this period of Ptolemaic history, building upon the insights provided by the modeling in the present paper. In this follow up, explicit attention will be paid to the causal character of this role.

1.7. The sections on climate modelling are mostly beyond my area of expertise to evaluate. However, with respect to evaluating historical societal impacts, I would question the emphasis on mean precipitation anomalies. To evaluate whether eruptions were a sufficient cause of a low Nile flow, what I really want to know is how much more probable a low Nile flow would be with an eruption vs. without an eruption: $p(D|V)/p(D|\neg V)$. That is, I need some help in assessing the counterfactual scenario: if there hadn’t been those eruptions, would there probably have been droughts in Ptolemaic Egypt anyway? The conclusion on lines 578-580 (“likely to have strongly influenced”) is too vague. The crucial issue in attributing societal impacts to volcanoes is just how likely it was that deficient Nile flows occurred due to eruptions.

We agree that this is an important consideration in the assessment of causality. We have emphasized in our revised manuscript that the Nile summer flood was famously mercurial, and that historical explosive volcanism was responsible only for “some” of this variability. We have also cited important precursor work (Manning et al., 2017) using the Islamic Nilometer that has shown tropical and extratropical eruptions to be repeatedly associated with a below-average summer flood, i.e., lower Nile floods were more likely in “volcanic years” than “non-volcanic years”.

For the present paper, however, the intent of the modeling is to provide an assessment of what likely happened to the Nile flood given that we do at least know (with fair confidence, thanks to the improved ice-core volcanic forcing history of Sigl et al. (2015)) that four notable eruptions *did* occur.

As part of this, considerable attention has been paid to specifying appropriate conditions for the period in terms of vegetation cover and other forcings that will have mediated the impact of these eruptions. Perhaps more germane to the reviewer’s comment here is that in conducting multiple model runs, we have some additional insight into the range of possible Nile flood responses to this eruption sequence, and have noted occasions when there is high variability among model ensemble members (i.e., notable departures from the mean response).

1.9. Much of the material currently in the introduction and results sections reads more like discussion. I would encourage the authors to create a larger discussion section in two parts: one for the discussion of volcanic forcing and hydroclimate anomalies and another for discussion of societal impacts. The article would also benefit from a real conclusion that summarizes findings and returns to issues raised in the introduction.

As noted earlier, we have now revised the paper, including by cutting a substantial portion of introductory historical context, while in the Discussion and Conclusion, we circle back to reflect upon the issues raised in the Introduction, including with a more explicit statement on the need to go further in assessing historical causality.

1.10. The authors may also wish to address the methodological significance of the work and, in particular, make proposals for further integration of paleoclimatology, climate modeling, and human history.

We agree fully that an increased integration between palaeoclimatology, climate modeling and human history is an important methodological goal. We have taken the opportunity provided by this paper to note that climate modeling can make a tremendous contribution to our understanding of human history, in particular by providing insight into the mechanisms by which events like distal explosive eruptions might impact agriculturally critical environmental resources like the Nile summer flood, and by filling in the “blanks” for periods and regions when and where palaeoclimatic proxies (natural archives) are not available in abundance or at sufficiently high temporal and spatial resolutions. We also note the importance of developments in palaeoclimatology for the study of environmental influences on society, in particular the important work of the PAGES 2k Consortium. We then note that extending reconstructions beyond the nominal “2k” target period would help provide environmental data for some of the most well-documented societies of ancient world.

1.11. Specific issues: Line 15: The phrase “sometimes widespread” is confusing. Based on context, I would suggest “both local protests and widespread revolts”.

We have kept the use of this phrase, placed in parentheses, because the events in question appear to have been more substantial than local protests, sometimes taking the form of organized attempts at the overthrow of Ptolemaic rule. To help give the reader a greater grasp of the potential scale of these events, in our Introduction we cite the example of the Great Theban Revolt that lasted approximately twenty years and in which the Ptolemies appear to have lost control of much of southern Egypt.

1.12. Line 24: I assume that “observe” here refers to finding an average in the simulations, not an actual observation of the real climate. Please clarify.

By “observe”, we mean here that we are observing (reporting) that our model produced an average (mean) surface cooling of the order of 1.5C following the first (tropical) eruption in 168 BCE. We have kept this term, but have made multiple textual edits to the manuscript for purposes of clarity (detailed in the Track Changed manuscript).

1.13. Line 55: This statement already presupposes the conclusion.
We feel that stating that Egyptian civilization was heavily dependent on the Nile is relatively non-contentious, and we mean this in a general sense more broadly for Egyptian history than solely for the Ptolemaic period that we are studying.

We have added multiple additional citations (see earlier) that have studied the inter-relations between Nile flooding and Egyptian civilization in different periods, including the reviewer's valuable recommendation of Mikhail, A. 2015. 'Ottoman Iceland: A climate history', Environmental History 20: 262–284.

1.14. Line 56: The phrase "potentially climatically effective" is awkward. I would recommend perhaps "eruptions that may have had regional or global climatic impacts."

We thank the reviewer for their recommendation. Respectfully, we have maintained the use of this phrase as being slightly more concise. The phrase "climatically effective" is also relatively common in the volcano-climate literature to denote those minority of eruptions having the characteristics capable of impacting climate on more than local scales.

1.15. Line 57-58: Again, this statement presupposes the conclusion.

Rather than stating that *"Egyptian civilization may have been repeatedly influenced by the "hydroclimatic shocks" wrought by these events (Manning et al. 2017)"*, in our revisions, we now state more carefully that *"Egyptian civilization provides a valuable test-case for the study of human vulnerability and resilience to abrupt environmental change in potentially experiencing repeated "hydroclimatic shocks" induced by these events (e.g., Manning et al., 2017, 2021)."*

1.16. Line 146-152: I do not find that this example supports the authors' arguments. Instead, it serves as a reminder that there were, at times, other sufficient causes of political change in Egypt besides climatic variability, such as conflicts with neighbouring empires.

This portion of text has now been cut from the Introduction as part of our efforts to condense the overall size of that section.

Response to Reviewer 2.

General: **The manuscript investigates the hydrological response of a series of volcanic eruptions during the 2nd century BC, focusing also on the societal impacts of the eruptions in the context on the Egyptian history. In addition to empirical evidence, authors use the output of an ensemble of simulations with a comprehensive Earth System Model, simulating potential trajectories of plausible climatic scenarios in the aftermath of the volcanic eruptions.**

The manuscript is very well written, material and methods are comprehensively presented and results are discussed within the context of present literature. Therefore, I think the manuscript should be published with some minor comments addressed below. Most specifically, the comments relate to the modeling and statistical part, including a more nuanced discussion and interpretation of model results in the context of past civilizations. Moreover, I would encourage the authors to reduce the overall length of the manuscript by summarizing dedicated paragraphs or moving parts into the supplementary material whenever possible.

The manuscript is revised as described in detail under the specific replies below. We have reduced the main manuscript length as recommended. Some of the old text has been deleted, while other text has been moved to the supplementary material. We note that the manuscript is still comparatively extensive, but this is in line with other published article in this special issue, which (given their interdisciplinarity) have had to introduce a wider range of methods and contexts than might otherwise be required.

1 Introduction:

l. 114: Linking volcanic eruptions directly to revolts or warfare might be afflicted with high degree of uncertainty: In past societies single upheavals or riots always happened – likewise, a close inspection of ice core records will typically also yield one or two eruptions per decade. Linking both just because of their synchronicity might be co-incidental. The processes of both, the impact of the volcanic eruption on climate and the prerequisites leading to riots or revolts during the period previous to the volcanic eruption can have multiple drivers and causes. Therefore this line of evidence in terms of wiggle matching single historical events with volcanic outbreaks should be handled with care.

We fully agree with the concerns listed by the reviewer. In the manuscript, we have now clarified that our work takes place in the context of the previous results of Ludlow and Manning (2016, 2021) and Manning et al. (2017), which considered in detail the correspondence between explosive eruptions (using the volcanic chronology of Sigl et al. (2015) and independently dated revolts across the Ptolemaic era. This work has identified the correspondence as highly statistically significant, also identifying significant correspondences between dates of phenomena such as sales of hereditary agricultural land, which have been previously hypothesized to occur with greater frequency during times of socioeconomic stress, as might follow years of poor Nile flooding, providing a glimpse into the mechanisms by which the impacts of explosive volcanism on Nile flooding might ultimately provoke revolt.

Scholars have now also recommended that case studies of more specific periods and regions might shed greater insight into the drivers and prerequisites of complex phenomena such as revolts. Our present paper thus intends to provide a foundation for such a case study by offering greater detail about the likely hydroclimatic impacts of a key decade in Ptolemaic Egyptian history, the 160s BCE.

l. 118: "hydroclimatic shocks" should be replaced by "pronounced changes in hydrology"

We have corrected the relevant sentence at line number 118 to state "*pronounced changes in hydrological cycle"*. (Line 155 in Revised Manuscript)

l. 120: The hydrological cycle after very large explosive tropical eruptions is not only driven by the north-south contrast of the monsoon (the African monsoon system is far more complex in this respect), rather than less evaporation caused by lower temperatures according to the Clausius-Clapeyron equation.

We have modified the relevant sentence in line 120, to acknowledge the role of regional factors in addition to the reduced temperature gradient *"….well as reducing the meridional (north-south) temperature contrast that controls the intensity of the African monsoon, alongside other regional factors*". (Line no. 155-157 in Revised Manuscript)

ll. 134–170: This whole section should be shortened/summarized and focus on the very area of research, as outlined in the section ll. 171–186.

We have moved the a substantial portion of the introductory historical context (lines 134-170) to the supplementary material (now appearing there as Section S1.1 Introduction (Historical Context)). (Reference to supplementary info at line number 175 in the Revised manuscript)

ll. 190–205: This section should also be shortened to the most relevant information introducing the content of the subsections.

We have shortened the section (by deleting some information related to model that is available elsewhere (as per the cited paper) as suggested. (Around the line 200 in Revised Manuscript)

In addition to the points mentioned above there are two additional points that should be mentioned already in the introduction:

1) The importance of natural climatic/hydrological variability in the occurrence of Nile floods and their counterparts. This is important to put the proposed "hydrological shocks" in the aftermath of volcanic eruptions into context of externally undisturbed periods.

This point is addressed in the introduction in lines 90-105. In addition, we have inserted some text and relevant citations to emphasize that indeed the considerable variability of the Nile was well known historically, and that explosive volcanism contributed to "some" of this variability (i.e., it certainly did not drive all the observed variability).

See lines 109 in Revised Manuscript: *"But the Nile summer flood was also famously mercurial, with insufficient flooding often leading to adverse societal impacts (e.g., Bell, 1975; Butzer, 1976, 1984; Said, 1993; Hassan, 1997a, b; Hassan, 2007; McCormick, 2013). Some of this variability was likely driven by explosive volcanism."*

2) A more differentiated introduction of the impact of large explosive tropical volcanic eruptions vs. medium-to-small sized high latitude northern/southern hemisphere eruptions. This relates for instance to the overall amount of cooling, the potential for a dynamical response on natural modes of climate variability (cf. North Atlantic Oscillation/El Nino). Introducing this difference in location and magnitude will also help to better explain the different climatic and hydrological response of the initial tropical eruption E1 and the following high-latitude eruptions E2 – E4 that are presented and discussed further down in the manuscript.

Point 1 and 2 are addressed in the Introduction section where volcanic eruptions and their impacts on the Earth's climate system are summarized (lines 55-75).

In our opening paragraph (lines 52-58 In Revised Manuscript), we thus now state: *"The cooling caused by such events can also reduce net evaporation and hence precipitation over large areas (Lui et al., 2016; Iles et al., 2013), while also potentially leading to a near global-scale dynamical suppression of the northward migration of the inter-tropical convergence zone (ITCZ) during the boreal summer, as the convergence follows the surface area of maximum temperature (Petterson et al., 2000; Chiang and Bitz, 2005; Broccoli et al. 2006; Colose et al. 2016). These changes in precipitation can, moreover, impact river outflow (Oman et al., 2006; Sabzevari et al., 2015; Kostiç et al., 2016)..."*

Additionally, we have added the following in lines 77-84 (Revised Manuscript):

"*Extratropical eruptions generally have a comparatively weaker climate impact than tropical eruptions. This happens* following the background Brewer-Dobson circulation upwelling in the tropics and downwelling at higher latitudes, which *directly affects the stratospheric lifetime of volcanic aerosols (Kirtman et al., 2013; Myhre et al., 2013; Schneider et al., 2009). Recent studies though illustrated the potential for extratropical eruptions having disproportionally strong forcing and climate impacts, consistent with past reconstructions (Toohey et al. 2019)."*

2.1 Model Description

l. 226: How is the impact of volcanic eruptions implemented?

We have provided a paragraph on how the eruptions were implemented in the NASA GISS ModelE was originally stated under the section "Experiment Design" (line 272 in original Manuscript), We moved this paragraph from experiment design to discuss how the volcanic eruption has been implemented here (Line 239-252 in Revised Manuscript).

2.2 Experiment Design

l. 233: What is the rationale [for] using the PMIP4 mid-Holocene protocol? Maybe the authors could explore in one or two sentences why especially the vegetation is closer to mid-Holocene conditions rather than the one representing the situation during pre-industrial times.

NASA GISS ModelE's Terrestrial Biosphere Model (TBM) Ent (NASA-GISS Version name) (Kiang, 2012; Kim et al., 2015) is not a full Demographic Global Vegetation Model (DGVM). A key missing functionality is the ability to migrate vegetation, driven by changes in climate. In CMIP5, the lack of a fully dynamic vegetation model GCMs led to a failure to reproduce the mid-Holocene wet Sahara conditions over Africa (Harrison et al. 2013). Our model simulations for the mid-Holocene period using the vegetation distribution based on the PMIP4 protocol produced a more realistic result in terms of a wetter Sahara region. Thus, in the absence of a better approach, we linearly interpolated the mid-Holocene PMIP4 vegetation distribution to the 2.5k period to achieve more accurate background climate conditions.
We have outlined our arguments for the use of the PMIP sensitivity vegetation distribution in our results section while discussing the implications of using these boundary conditions in section 3.1.1, (line 300 in Revised Manuscript and specific discussion is at line 340 onwards).

l. 272: The authors could add some effects on the timing of the eruption, i.e. when the eruption date is set to a summer date, especially for the potential effects on monsoon and the northern hemispheric winter atmospheric circulation. It could also be explicitly stated that it is not possible to decipher the exact timing of the eruption in the annual cycle because of dating uncertainties involved in the ice core reconstructions.

Thank you for pointing this out; we have thus added the line "*Because the exact date of an eruption cannot be directly determined based upon ice-core sulphate deposition data, both because of possible uncertainties in the ice-core chronologies and because of variable time lags between eruptions and the atmospheric circulation of the resulting sulphate and its deposition in the polar ice, we selected a summer eruption date to investigate the impact on northern hemisphere monsoon and wintertime atmospheric circulation.*" (Line 291 in Revised Manuscript)

l. 273: I suggest to move the following section on the implementation of the volcanic forcing at the end of the model description paragraph – also some words on the uncertainties of the

sulfate reconstructions based on ice cores would be helpful to indicate that modeling results on the subsequent simulations are dependent on the magnitude of the reconstructed sulfate injected into the stratosphere.

We moved the relevant paragraph to the model description section as suggested, and added following lines at line 295.

"*We also note that the accuracy of our modelling will depend in part upon the accuracy of the ice-core-based volcanic forcing reconstruction being employed. Uncertainties in reconstructed forcing can arise, for example, because of variation in the deposition of sulphate across the polar regions for any given eruption. In this respect, it is important to note that the Sigl et al. (2015) volcanic forcing reconstruction employs several ice-cores from Antarctica and Greenland, but our results can be revisited as reconstructions become more reliable by incorporating larger numbers of ice-cores*" (line 295 in Revised Manuscript)

3. Results

– The header for paragraph 3.1 is missing –

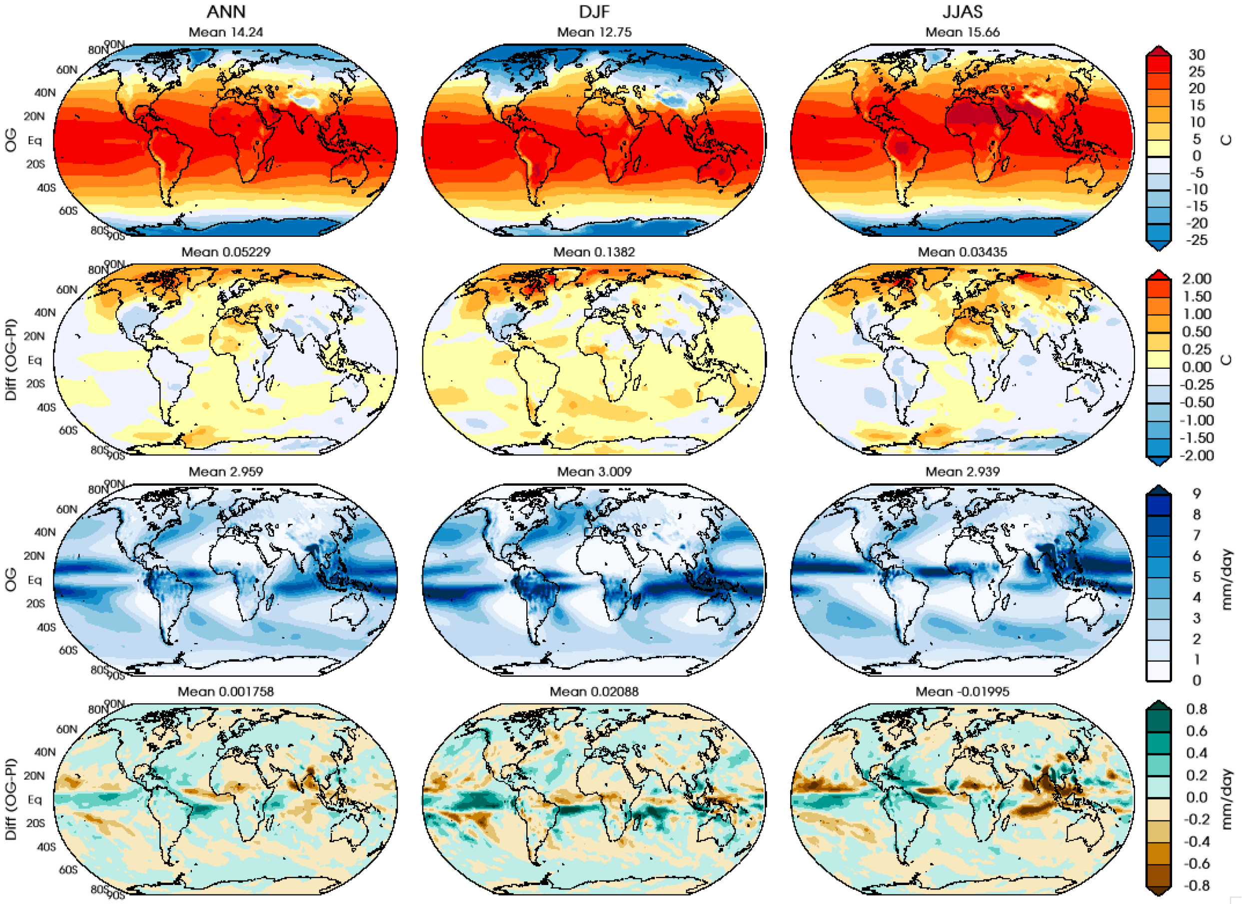
We have introduced the header for 3.1 as "**3.1 2.5ka control runs**".

Changes in orbital forcing – the supposedly most important factor between 2.5 and 6k – were already considerable different at 2.5 k. Therefore I guess that also the classical mid-Holocene pattern is different, even without dynamic vegetation. It would be good to at least indicate those implications when interpreting the 2.5 k pattern in the context of the mid-Holocene 6k climate and vegetation changes.

Another note: Changes due to orbital forcing are mostly effective on a seasonal basis on Holocene timescales, because changes in the inclination of the earth axes do not change the annual amount of radiation received by the sun. An alternative in structuring Fig 1 and Fig 2 is to omit the mean climate states in the upper and middle panel (also for section 3.1.2) and replace them by the patterns for the winter and summer season for the different experiments (together with the annual). This would also show better the impact of the (orbitally induced) background climate conditions between 2.5 k and PI.

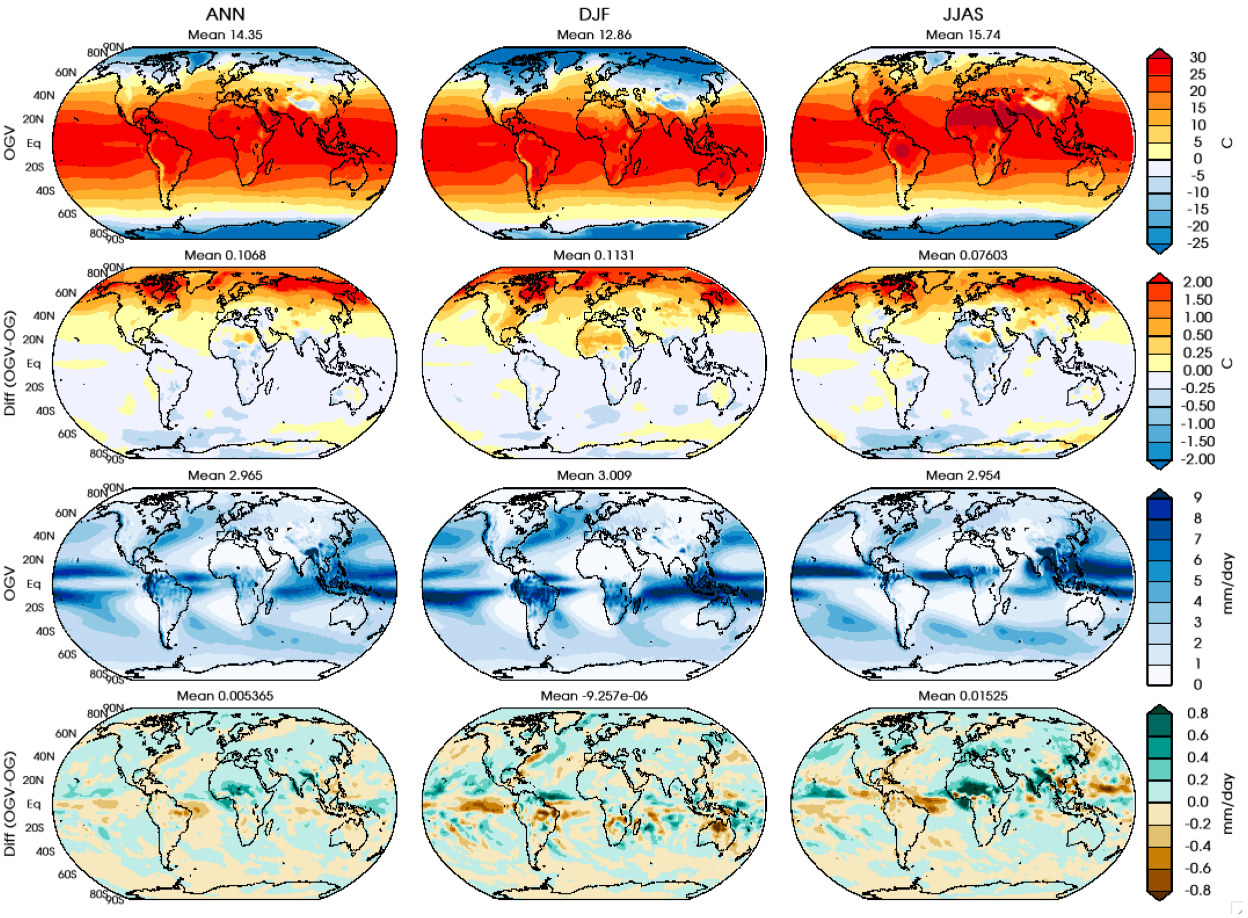
We certainly agree that the impact of orbital forcing for mid-Holocene may be slightly different (cooler for mid-Holocene) than the PI control for both the current (PMIP4-CMIP6) and previous (PMIP3-CMIP5) generation of models (Brierley et al., 2020). We have focused on the North African monsoon season rainfall and the impacts due to the inclusion of PMIP4 vegetations over the region. Since the impact of changes in orbital forcing for a 2.5k period is evident in the surface temperature changes over the northern hemisphere, but rainfall changes over Africa appear more reasonable with vegetation changes only.

We have now modified the plots (Fig 1 & Fig2). We have included the seasonal differences for Annual, DJF, and JJAS seasons along with the mean seasonal climate for the 2.5ka period with GHG and ORB in fig 1 and GHG, ORB, and vegetations in fig 2. We included the mean panels in both plots for the reader to understand the mean climate with the difference with the inclusion of different forcing factors. See the revised Fig 1, below.



“Fig 1. *Seasonal means (Annual, DJF & JJAS) of surface air temperature (top row) for 2.5k period equilibrium run, differences from the preindustrial period (2.5ka-preindustrial) for all three seasons (2^{nd} row from top) and seasonal (Annual, DJF & JJAS) mean precipitation (3^{rd} row from top) and the difference (bottom row) from preindustrial period (2.5ka-preindustrial). The equilibrium run for 2.5k period have the orbital and GHG concentration changes for the 2.5k period (referred as OG), the preindustrial period (as PI), and their difference (OG-PI) as simulated by GISS ModelE2.1*.”

For the revised Fig 2, see below:



"Fig 2. Mean surface air temperature for Annual, DJF and JJAS seasons (top row) and seasonal mean precipitation (3rd row from top) for the equilibrium runs with the PMIP4 vegetation for 2.5k period and surface temperature difference (2nd row from top) as well as the seasonal precipitation differences (bottom row) for 2.5k period as simulated by GISS ModelE2.1. We used a short initial notation for forcing to denote the difference (ORB+GHG+VEG = OGV and ORB+GHG= OG)"

3.1.2 2.5Ka ORB+GHG+VEG climate

l. 344: The authors should provide some implications the linear interpolation of vegetation might have on their results (e.g. it is also likely that vegetation changed considerably earlier to preindustrial-like conditions, resulting in a higher albedo due to less forest over the high northern latitudes.)

We have thus summarized the implication of the inclusion of vegetation cover specific to 2.5ka as increased temperature over higher latitudes and northward expansion of the African monsoon during the JJAS season. Although the albedo changes due to introducing vegetation over Africa are not substantial, this enhanced rainfall supports the role of biogeophysical processes in reproducing the wet African conditions over Africa relative to PI period for the mid-Holocene period (Kutzbach et al., 1996; Claussen et al., 2003; Kutzbach and Liu., 1997; Hewitt and Mitchell, 1998). These results also support the importance of having a dynamic vegetation component to represent regional-scale processes and their impact on the climate.

3.3 Volcanic aerosol properties

Concerning the overall length of the manuscript, I suggest to move this section into the supplementary information, as the general content of the manuscript is for an interdisciplinary readership.

We prefer to keep this section in the manuscript because it conveys important information on the model setup, which might get lost in the supplement. This section also complements the description section on how we model sulfate aerosols, aerosol-radiation interaction, and related properties. We also note that the journal does not have strict word limits, and our article is not overly long relative to others in the same special issue. We of course take the reviewer's general point that the manuscript should not be needlessly long.

3.4. Latitudinal temperature response to volcanic aerosol forcing

l. 483: How did the authors estimate their level of significance ? A few words in the supplementary [material] or within the section would be helpful to assess the robustness of the test, using only a limited number of ensemble simulations for the estimation of the level of statistical significance.

Thanks for pointing this out. To highlight this, we have added additional sentences in section 3.4 along with our results:

"The statistical significance level is estimated using the 2-tail student t-test after Deser et al., (2012) and following the assertion that 10 ensembles are sufficient for reasonable estimation of internal variability at a regional scale (Singh and AchutaRao, 2019)." (Line 480 in Revised Manuscript)

3.5 Latitudinal precipitation response to volcanic aerosols

ll. 506: The authors should add one or two sentences on the potential complications [of] investigating the direct output of global and coarsely resolved earth system models. For instance, the simplified parameterizations used for the simulation of precipitation in global models which impact a realistic simulation of tropical convection.

The coarser resolution of Earth system models is a notable cause of uncertainty in the modeling of convective rainfall. However, recent finer resolution models with convective cloud resolving capabilities have shown a significant improvement relative to coarse resolution models. But

coarser resolution models can still be successful in simulating large-scale patterns of changes in rainfall. We have thus added these lines:
"*We used a coarser resolution earth system model having a simplified parameterization and was successful in simulating the large-scale patterns of rainfall change*".

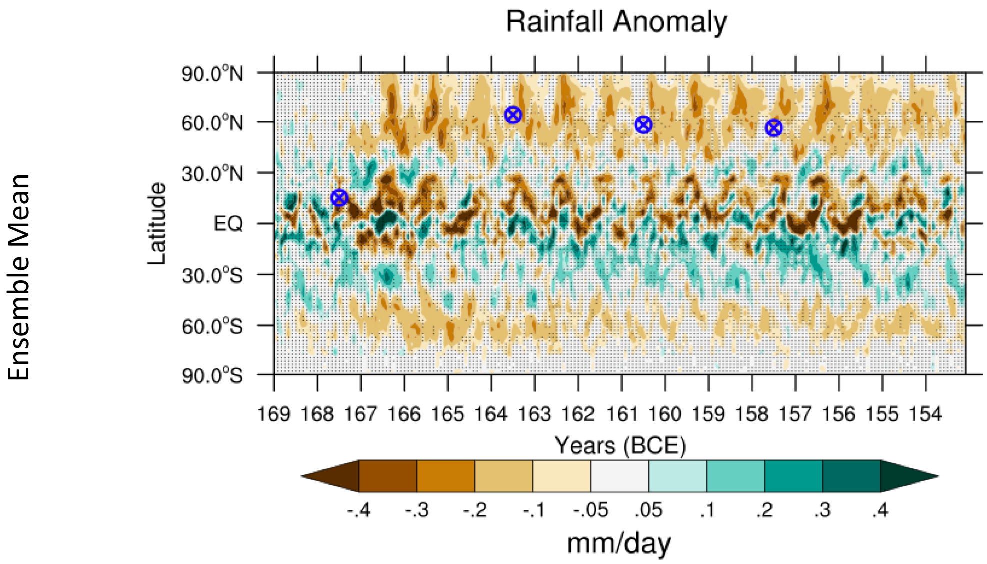
l. 514: This section is one of my critique zones, especially in the context of interpreting climatic trajectories in the context of past societies: The ensemble mean never happens in the real world – if any, a single trajectory compares best to a real world manifestation. Therefore it would also be imperative to show trajectories for single ensembles. This also reflects the bandwidth of potential hydrological changes in the aftermath of volcanic eruptions.

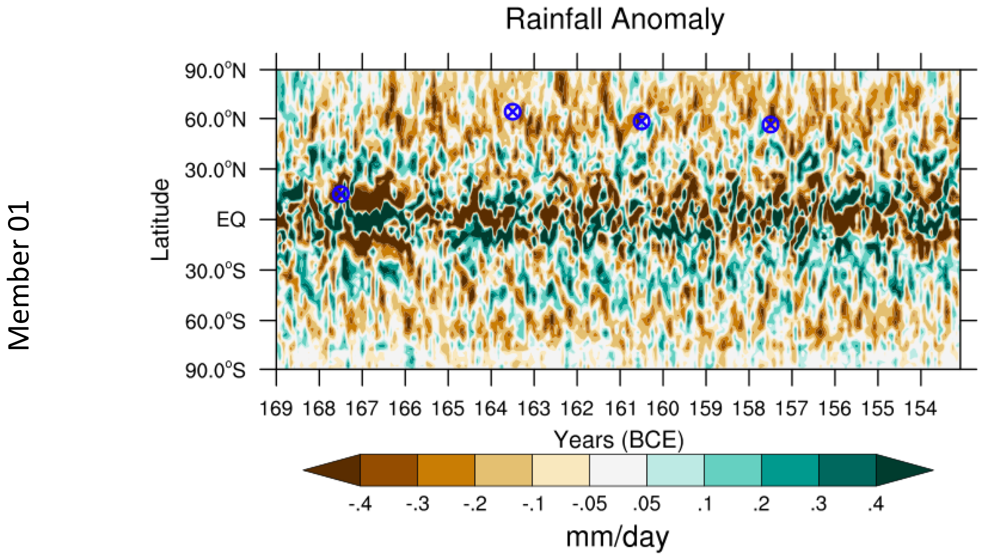
The reviewer's point. The ensemble mean may, for example, be biased by several factors, such as outlier members of the ensemble. One of the reasons for focusing on the ensemble mean is that it is impossible to select the most accurate member as we do not have the historical observations to compare to. However, the spatial representation of ensemble means with accompanying statistical confidence information can be helpful in the interpretation of the robustness of the magnitude and sign of change across ensemble members.

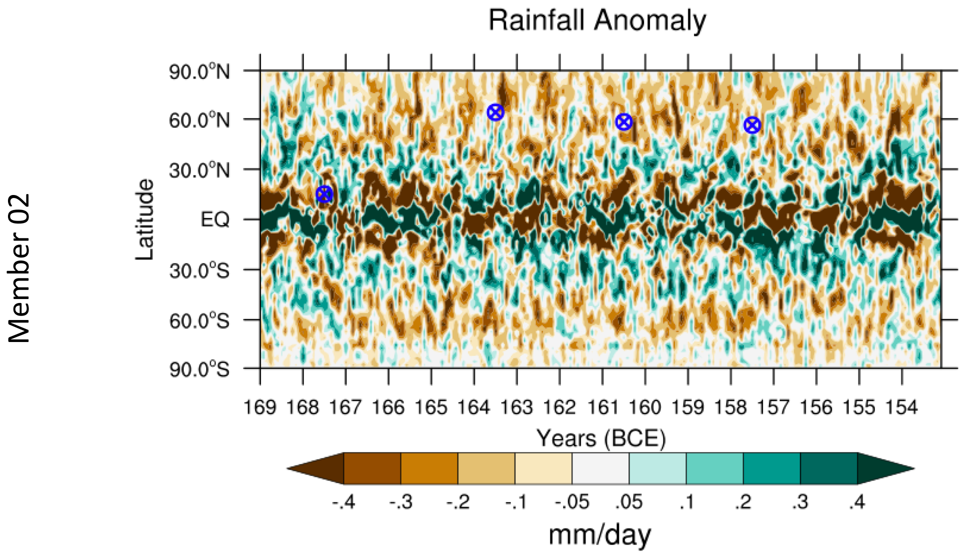
To convey how the results of individual members may differ meaningfully from the ensemble mean with, for example, stronger variability in the signal for a field such as rainfall, we have now plotted two (see below) ensemble members and compared them with the ensemble mean. These two individual members do exhibit some difference in rainfall response in the northern hemisphere tropics after the eruption E1. Still, we feel that in the absence of any indication of which ensemble member is the more accurate, the ensemble mean is the most relevant to focus on in the main text, but we explicitly note that it may be the case that one or more of the ensemble members is more accurate, though we cannot at present tell which. We have added the following text to the manuscript:

"It can be argued that an individual ensemble member can represent the historical period, but it is impossible to select in the absence of observation. Also, the added noise due to natural variability can alter the sign of change at the spatial scale among the individual ensembles. Thus, we selected the ensemble mean with statistical significance to show the response to volcanic eruptions with robustness for the climate variable. "

To additional reflect this uncertainty, we emphasize (see, for example, lines 542-547) cases in which ensemble variability around the ensemble mean is particularly high, thereby alerting the reader to instances in which the ensemble mean may be less parsimonious.


Ensemble Mean
Rainfall Anomaly
Latitude
90.0°N
60.0°N
30.0°N
EQ
30.0°S
60.0°S
90.0°S
169 168 167 166 165 164 163 162 161 160 159 158 157 156 155 154
Years (BCE)
-.4 -.3 -.2 -.1 -.05 .05 .1 .2 .3 .4
mm/day


Member 01
Rainfall Anomaly
Latitude
90.0°N
60.0°N
30.0°N
EQ
30.0°S
60.0°S
90.0°S
169 168 167 166 165 164 163 162 161 160 159 158 157 156 155 154
Years (BCE)
-.4 -.3 -.2 -.1 -.05 .05 .1 .2 .3 .4
mm/day


Member 02
Rainfall Anomaly
Latitude
90.0°N
60.0°N
30.0°N
EQ
30.0°S
60.0°S
90.0°S
169 168 167 166 165 164 163 162 161 160 159 158 157 156 155 154
Years (BCE)
-.4 -.3 -.2 -.1 -.05 .05 .1 .2 .3 .4
mm/day

l. 540: Here again, a more detailed information on the evaluation of the statistical significance would be helpful.

Please refer to our reply to the reviewer’s comment on section 3.4, above.

3.6 African monsoon and Nile River response

l. 581: The already mentioned information that a more consistent comparison between model and empirical evidence can only happen at the single simulation level can again [be] picked up here, because in the real world one could not expect a mean response of different simulated trajectories for single events in history.

Please refer to our response to the reviewer’s comment on section 3.5, above.

l. 584: Results for the E1 eruption seem convincing and also have a large-scale character that can be attributed to an external event – However, eruptions E2 – E4 show a very inconsistent pattern (even in the ensemble mean). This is also reflected in the statistical test (that presumably uses standard testing techniques that are not taking account the small sample size of n=10 samples). This heterogeneity in the response of the northern hemisphere E2 – E4 eruptions should be more emphasized, also in the subsequent interpretation in the context of their sustained effects on Nile floods.

This is an important observation. We have now inserted two new paragraphs along with the discussion on the possible reasons for heterogeneity in the response over the Nile basin.

(Line 743-785 In the Revised Manuscript)

“It is evident that the mean surface temperature response in the northern hemisphere is significant at the control period's $1\sigma_{ctrl}$ *and* $2\sigma_{ctrl}$ *levels. However, while rainfall and river discharge responses are significant at the* $1\sigma_{ctrl}$ *level, they fall within the* $2\sigma_{ctrl}$ *levels, although a few individual members do show significance at* $2\sigma_{ctrl}$ *as well. However, the statistical significance of the rainfall and discharge response may be sensitive to the dearth in the modeling Nile River basin at a relatively coarse resolution of the GISS ModelE, as well as the boundaries chosen to model the Nile basin and its headwaters. In particular, given the complexity of the Nile’s hydrology and disparate sources of discharge for the White and Blue Niles. We thus investigated the post-volcanic change in river flow for the southern (White Nile-dominated) and northern (Blue Nile and Atbara river-dominated) parts of the basin by dividing it at 10° N (Fig 13). Annual mean river flow change for the south (blue lines) and north (red lines) of the Nile basin were in broad agreement with a negative flow anomaly after eruption E1. This was most notable in the eruption year and the first year following, with the 95th percentile envelopes (dotted lines) deemed significant at the 95% confidence level for both these years (i.e., crossing the dashed lines parallel to the x-axis (Fig 13). In contrast, the mean north and*

south responses disagreed, including in the sign of the observed changes, after the extratropical eruptions (E2, E3 & E4). More specifically, while the mean flow anomalies in the year of E2 were unremarkable and showed little north-south contrast, a more notable divergence was observed in the first year following, with a positive flow anomaly in the south and negative in the north. In the year of E3, flow in the south showed no notable anomaly, while flow in the north was marginally negative. This distinction became more marked in the first year following, mainly due to a larger negative anomaly in the north. In the year of E4, a negative anomaly was again observed in the north, persisting for three post-eruption years, and contrasting with positive or unremarkable anomalies in the south.

These results are consistent with our earlier-described results (e.g., spatial rainfall variability over the Nile River basin, as per Figs. 10 and 11) and proposed mechanisms, alongside expectations from the literature (e.g., Manning et al., 2017). Thus, tropical eruptions (like E1) may result in a more consistent (negative) north-south flow response due to their more even interhemispheric aerosol burden and associated radiative impact. Extratropical NH eruptions (like E2-E4) that can result in a more asymmetric hemispheric aerosol burden may, by contrast, introduce contrasting flow anomalies by suppressing the northward migration of the ITCZ, negatively impacting flow in the Blue Nile and Atbara rivers by diminishing monsoon rainfall in the Ethiopian highlands, while potentially enhancing flow in the White Nile, fed by rainfall over the equatorial lakes"

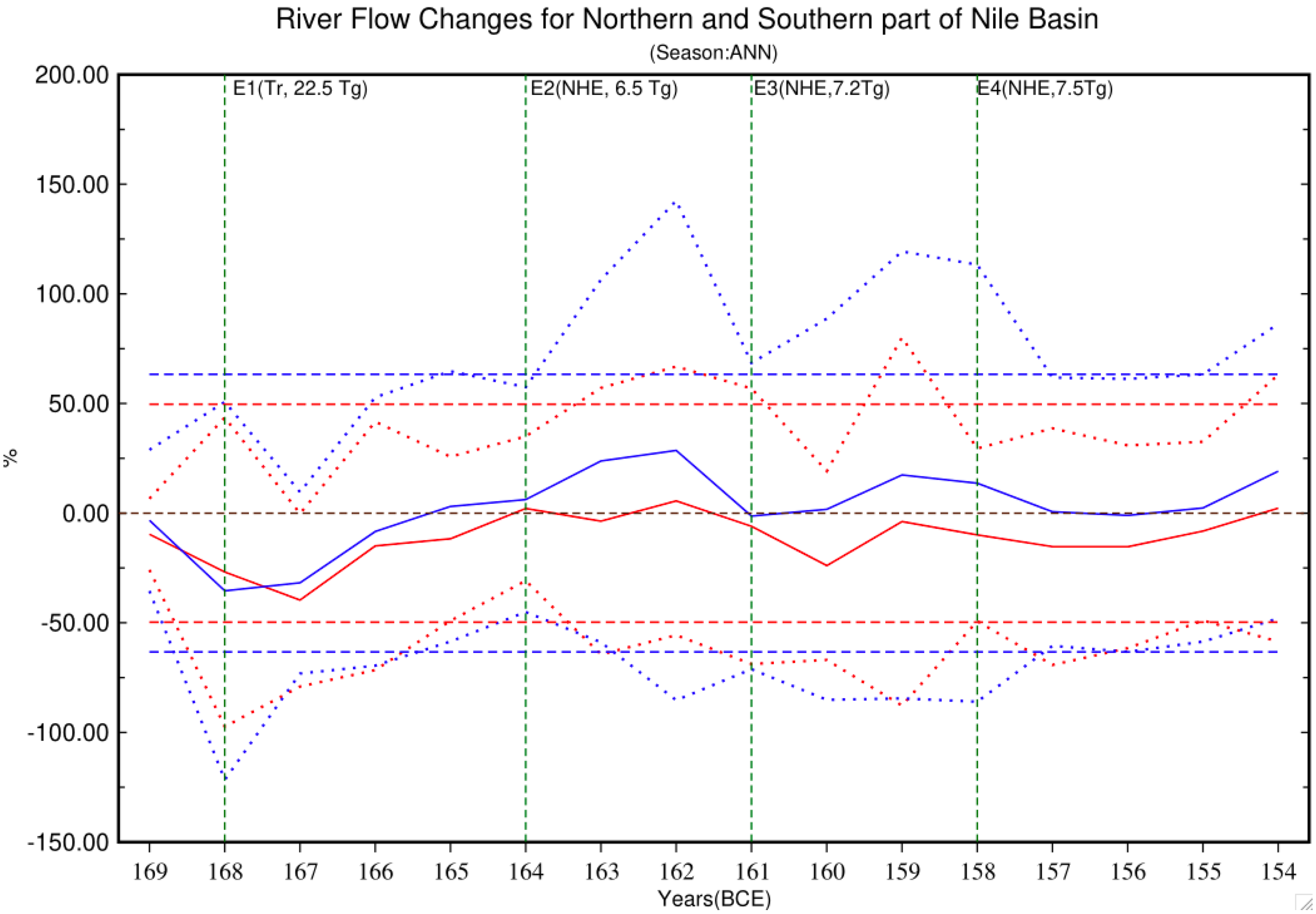


Fig 13. Annual Nile River flow changes averaged over the northern (red) and southern (blue) parts of the basin (divided at 10° N) for the entire simulation period. The solid lines represent the ensemble mean for each part of the basin; the dotted lines are $\pm 1.95\sigma$, *where* σ *is derived from across all the ensembles, and the horizontal dashed lines parallel to x-axis are the* $\pm 1.95\sigma_{ctrl}$ *where* σ_{ctrl} *is the standard deviation across the 100-year control run. Red and blue lines correspond to the northern and southern parts of the Nile basin, respectively.*

Please refers to the explanation for next comment (l. 614 incl. Table 2: Interpreting….)

l. 614 incl. Table 2: Interpreting the Table and the calculation of the according values correctly, the standard deviation is based on the volcanically forced ensemble members and the difference on the mean over all ensemble members minus the climatological mean of the 100 year control?

An alternative is to calculate the annual standard deviation of the 100 year control run and include it as the 1.95*std = 95% confidence interval. This will give an indication how the mean discharge value is outside the natural range. In the present form it gives the bandwidth of the volcanically forced simulations, not taking into account the natural undisturbed variability. The interpretation of the 95% confidence interval based on the control will give an indication how exceptional the respective year after eruption E1 – E4 was in the context of the natural variability.

We thank the reviewer for this suggestion. We have calculated the variability (σ_{ctrl}) for the 100-year control period and compared the statistics in Table 2 against the confidence interval ($1.95*\sigma_{ctrl}$) suggested by the Reviewer. The calculated variability (σ_{ctrl}) and 95% confidence interval ($1.95*\sigma_{ctrl}$) for annual river flow are 25.20% and 49.155% respectively. It is noticed that annual ensemble mean change is within the 95% confidence interval, but individual ensemble member reaches beyond 95% confidence interval for some years. River flow change over the Nile basin varies up to $3*\sigma$ for a few ensembles for some years.

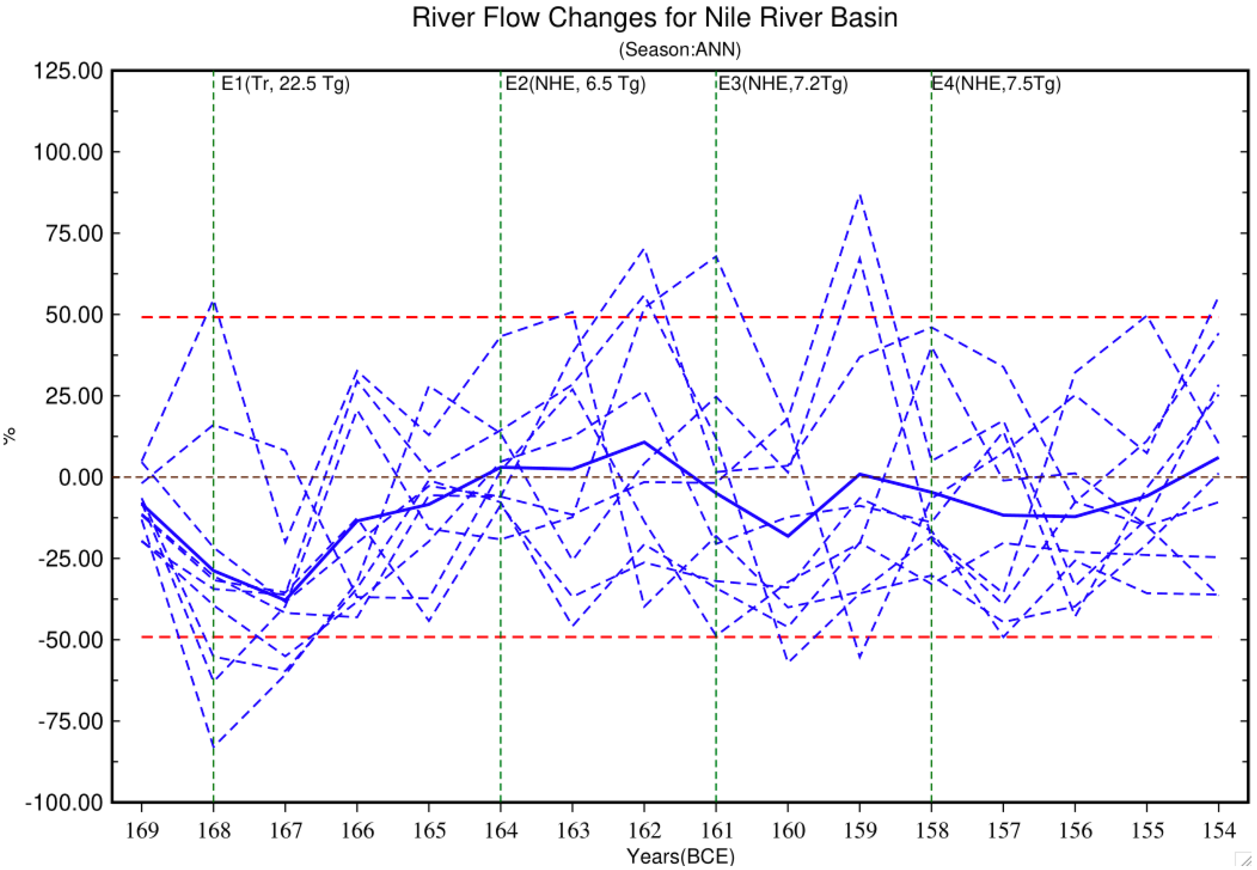


Fig Rev2.1. The solid blue line shows the annual mean change in river discharge over the Nile River basin, and dashed (blue) are the individual ensemble member. The red-colored dashed line parallel to the x-axis represents the 1.95*σ_{ctrl} for the 100-year control run.

Fig 12: This figure contains basically very good information – Similar to Table 2, and to show the exceptional behavior of the different metrics, it would be better to illustrate the 1.95*std of the natural 100 y control run as two lines parallel to the x-axis, together with the individual trajectories of the 10 volcanically forced simulations. When a considerable number of trajectories falls above or below the 95% confidence levels for an individual year, one can speak of a robust response – according to the hypothesis proposed, the discharge trajectories should then fall below the lower boundary for the years after the volcanic eruptions.

In addition, without the green vertical lines it is difficult to decipher the volcanic eruptions based on the evolution of precipitation and discharge, because also other sub-periods with considerable reductions in stream flow appear that are unaffected by volcanic forcing (e.g. year 159). An alternative interpretation that could be hypothesized relates to an increased intra-ensemble variability after volcanic eruptions compared to undisturbed conditions.

We have now inserted the 1σ (Red dashed line) and 2σ lines (Red solid line) on the plot for all three diagnostics. The modified Fig 12 is now shown below.

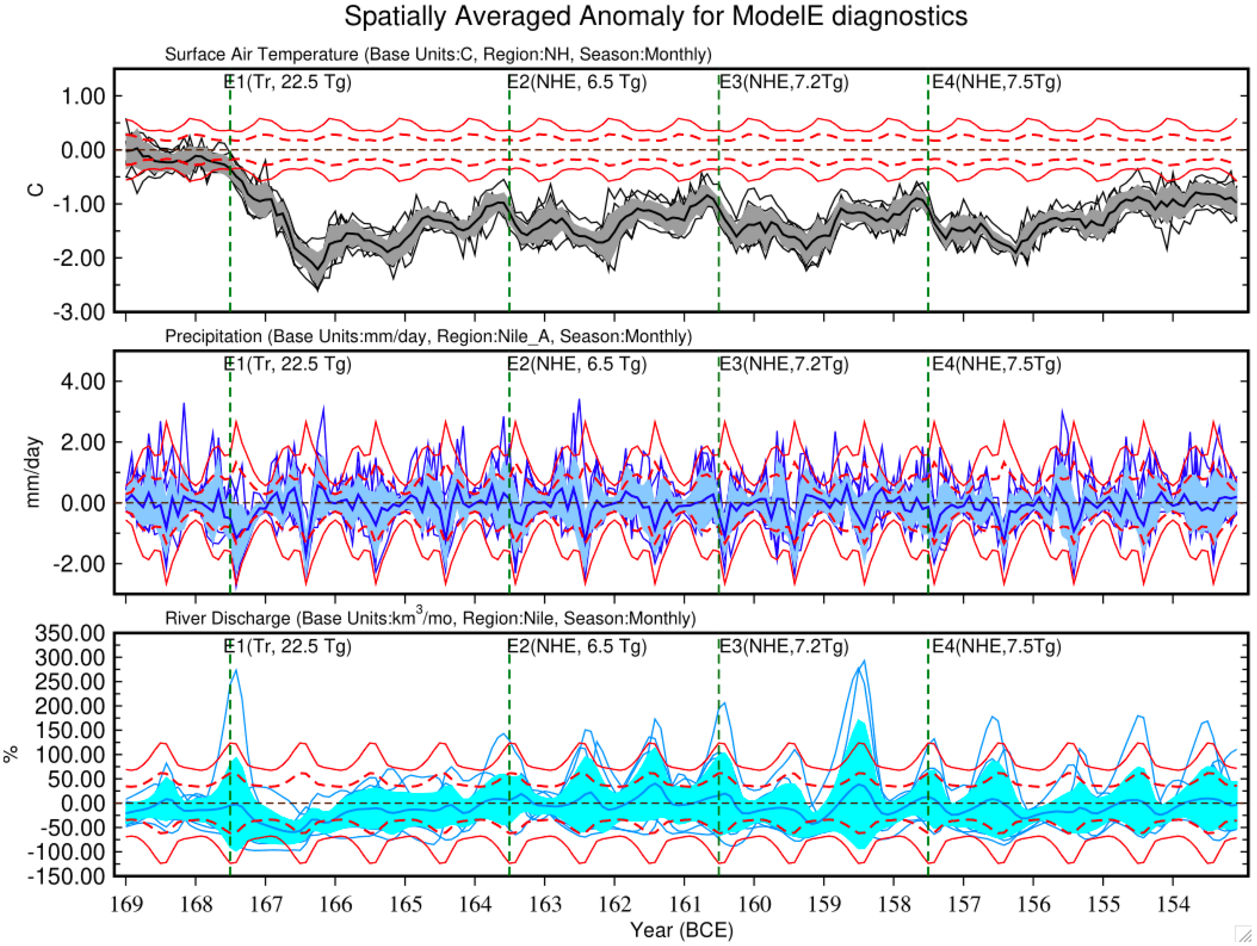


Fig 12: Monthly time series of individual ensemble and mean of surface temperature response (˚C) averaged over northern hemisphere (NH) (top panel), rainfall change (mm/day) over the spatial box over Nile River watershed (Latitude: (5N, 18N), Longitude: (30E, 42E)) (middle panel) and Nile River discharge anomaly (%) at the delta region (grid box centered at 29.0N, 31.25E). The dark solid (Thick) line shows the multi-ensemble mean, individual member (thin line), and the color envelope shows the associated variability ($\pm\sigma$; Standard deviation). The annual cycle of climate variability of control run is shown as $1\sigma_{ctrl}$ (Red dashed line) and $2\sigma_{ctrl}$ lines (Red solid line) along the x-axis for all three variables. The vertical dotted green line shows when each eruption happens

4. Discussion and Conclusions

ll. 712: This paragraph also relates to the interpretation of empirical evidence in the context of earth system and climate model simulation: It is important also taking into account the natural or stochastic nature of historical processes that are not always determined by external environmental forcings. Otherwise a state in the interpretation and explanation of historical events will be reached, where numerous single historical events are only interpreted within the climate-determinism concept, which can be true for severe events, but might be misleading for most medium-to-small size events, especially in the context of volcanic eruptions.

We have now added a more substantial introduction to the Introduction section to address challenges in assessing connections and causality between environmental forcings and historical human/societal events. This also in part addresses similar challenges highlighted by Reviewer 1, and which are addressed more fully in our response to this reviewer.

Additionally, we have clarified that the present paper builds upon the work of Ludlow and Manning (2016, 2021) and Manning et al. (2017), that explicitly address the challenges of testing statistically the association between explosive eruptions and revolts in Ptolemaic Egypt (also noting that it is not assumed that all revolts were “triggered” or otherwise caused by volcanically induced hydroclimatic variability (or indeed hydroclimatic variability of any origin).

The intent of the present paper is, therefore, to provide clarification on the likely hydroclimatic variability experienced in Ptolemaic Egypt during the 160s BCE, a time already well known to historians as one of considerable societal upheaval in Egypt. Our work here should provide a firmer foundation for a planned follow-up study that will more closely examine the available historical evidence for the impacts of these eruptions (now informed by our modelling) and assess their contribution to the turbulent history of the period.

l. 731: For producing a basis for “historical realization” it is again of utmost important to look and investigate the trajectories of individual realizations of ensemble simulations from climate models and not (only) their mean response.

We agree with the reviewer’s argument, and would refer back to our earlier responses to this important issue.

l. 791: As the authors state correctly, from a conceptual point of view there is no “best” member, because all members are equal probable under the same set of external forcings implemented. What might be more important is the notion that the combination of external AND internal forcings shape the exact evolution in both, the real and the model world.

We again agree fully with the reviewer here.

Figures and Tables:

In general, Figures and Tables are presented with high quality and an appropriate level of information included. Below just a few minor suggestions how to improve or modify selected items:

Fig 1: As already stated in the main text, Fig 1 and 2 might be combined into one single Figure by representing only the differences for annual, winter and summer (alternative JJAS) mean.

Figures 1 and 2 are now modified to include more seasonal details as suggested.

Fig 5 center panel: The style of the presentation of the single trajectories could be used as template for the precipitation and discharge Figure 12 to show the variability of the different ensemble simulations together with the 95% confidence level of natural variability of the 100 yr control simulation.

Figure 12 has now been modified to include the individual ensemble members along with the $\pm\sigma$ envelope for the volcanically forced ensembles. We have also included the $\pm 1*\sigma_{ctrl}$ and $\pm 2*\sigma_{ctrl}$ annual cycle for the 100-year control run as suggested.

Table TS1: The authors might include also the volcanically forced simulations as an additional column and highlight those simulations that are presented in the manuscript.

We have inserted the column for the volcanically forced ensemble.

Additional Revisions

1.) The word "stratovolcanic" has been changed to "Large, strato-volcanic" for more precise reference in abstract line 20 and introduction and 46
2.) in the abstract "NASA GISS ModelE" is changed to "NASA GISS ModelE2.1".
3.) Abstract line 29: "South and East Asian" changed to "South Asian, and East Asian".
4.) Inserted a sentence at line 48 (introduction) "*The sulfate aerosols of the 1991 eruption of ~18 Tg* SO_2 *from Mt. Pinatubo increased the optical depth of the atmosphere from ~0.6 to ~0.75.*"
5.) Inserted reference "Colose et al. 2016" at line 53.
6.) Text inserted at line number 85. "*Effectively, the ITCZ shifts away from the hemisphere with the greatest amount of volcanic aerosol; for tropical eruptions, this movement is typical more southward as well owing to the larger amount of land in the Northern Hemisphere and higher thermal capacity of the oceans.*"
7.) Modified the format of conditions stated at line 250-255.

Reference:

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