Draft Reviewer Responses
Ram Singh et al. - Investigating hydroclimatic impacts of the 168–158 BCE volcanic quartet and their relevance to the Nile River basin and Egyptian history

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|¬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.