Reviewer 2.

This paper combines climate simulation data with novel growing degree-day (GDD) model and high-resolution pollen analysis from southern Norway to study the agricultural and societal responses of the 536/540 volcanic double event. It provides new evidence on the local societal responses to the crisis in Scandinavia. Furthermore, the novel GDD model can be applied to other locations in further studies. Consequently, the manuscript is an important contribution to the field and it is well suited for the Climate of the Past in general and for the 'Interdisciplinary studies of volcanic impacts on climate and society' SI in particular.

However, this paper would have needed more careful editing before the submission, as pointed out by the RC1. In addition to his comments (e.g. considering vocabulary), the authors should pay attention to typesetting and figure captions. For example, in many places the font style and size alters (see page 10, line 334, this matter repeats throughout the manuscript).

Dear Reviewer 2, thank you for your thorough comments. We have gone through the manuscript and have carefully checked the editing, and altered some sections in the document according to your and Reviewer 1's comments. We have checked the typesetting as well as the figure captions to make sure the font style and size are consistent throughout.

I would recommend that the authors go carefully through the manuscript, and pay attention to both stylistic and scientific matters. After this, the manuscript may go through a second review round, where more detailed matters may be addressed by the reviewers.

In addition to the editing, we have also rewritten parts of the manuscript with an eye on the stylistic and scientific matters, see also the comments from Reviewer 1.

Consequently, I will not give point-by-point review. Instead, I will raise some general matters on each chapter that caught my eye.

Our point-by-point responses to your individual comments are given below.

Introduction:

1. Why Toohey and Sigl 2017 (https://doi.org/10.5194/essd-9-809-2017) is not referred to here (e.g., p. 2, l. 53)? If I am not mistaken, the AOD data on Fig. 2. comes from this publication? If so, the Fig 2. should have a reference to this study as well.

Thank you for your comment. On page 2 line 53 we refer to the update in ice core chronology that preceded the making of the AOD data set. Therefore, Toohey and Sigl (2017) is not cited here. However, Toohey and Sigl (2017) did indeed publish the AOD data used in the model simulations and displayed in our figure 2. Thus, we have added the reference to the figure caption.

2. Pay attention to the (pre)historical periodization. Is it relevant to discuss "late antiquity" or "beginning of medieval state formation" (e.g. p. 2, l. 64; p. 3, l. 91) in the Scandinavian context? Would it be more informative to describe the transformation to the Merovingian period (especially

as this period might not be well known by people outside Scandinavia and thus some readers might confuse it with the Merovingian dynasty)?

Thanks for your comment. We have described the period in the archaeological context relating to more generally known periods, like the Late Iron Age instead.

'In Norway, the mid-6th century is associated with profound changes in social organisation and material culture, which defines the very transition from the Early to the Late Iron Age (500 BCE-550 CE/550-1050 CE). Often understood as a turning point in Norwegian prehistory, many archaeologists discuss a possible causal relationship with the volcanic double event in 536/540 CE (Iversen, 2016; Gundersen, 2019).'

Methods

3. Pay attention here (and elsewhere in the MS) if "peak of estimated volcanic forcing" (or some similar wording) should be used instead of "year of the eruption" (e.g., p .3, l. 126), as we do not know neither the eruption date nor the location. For example, the first eruption may have happened one year before it is evidenced in the ice-core data in 536 CE.

Thanks for your comment. You are correct in that the actual eruption source is often unknown, as is the eruption season. However, when describing the response '2 years after the eruption' in this manuscript, we are referring to the climate model simulations. We do know when the eruption was simulated in the model, and we would therefore like to keep this wording.

For the 536 CE eruption, historical documents from the Mediterranean report a dust veil in March of 536 CE, and so we are fairly confident that this eruption occurred in this year. This information is added to the introduction.

4. Section 2.1: commonly, NAO+ and NAO- are not considered as two **different** patterns, but as fluctuations of the strength of the SLP difference between Iceland and Azores (that influences, e.g., the westerlies). Furthermore, I would have liked to know here what kind of weather is commonly associated with the positive and negative modes of the NAO, the North Atlantic ridge, and the Scandinavian blocking in Norway in general, and on the study areas in particular.

Thanks. We have rephrased the NAO+ and NAO- as being two different modes instead of them being different patterns. Furthermore, we have added a description of the weather patterns associated with the different pressure patterns over Northern Europe.

'During winter to spring, climate in Scandinavia is influenced by four winter atmospheric circulation modes: A positive North Atlantic Oscillation (NAO), a negative NAO, the North Atlantic ridge, and the Scandinavian blocking (Vautard, 1990; Michelangeli et al., 1995; Cassou, 2008). One of the most prominent teleconnection patterns in all seasons is the NAO. The NAO+ is defined as a high pressure center over the mid latitudes, generally around Portugal/the Azores, and a low pressure center over Iceland (Hurrell, 1995). The NAO is associated with changes in the location and intensity of the jet stream, as well as the patterns of heat and moisture transport (Hurrell, 1995). These changes affect the temperature and precipitation patterns over Europe and Scandinavia (van Loon and Rogers 1978). The NAO+ related to higher temperatures and increased precipitation over Scandinavia, and the NAO- with lower temperatures and decreased precipitation. The North Atlantic ridge is associated with a high pressure over the Atlantic and a low pressure over Scandinavia, leading to colder and wetter conditions over Northern Europe. The

Scandinavian blocking is represented by a stable high pressure system over Scandinavia, resulting in warm and sunny conditions over this region (Cassou 2008, Tedesco 2020). …'

5. Section 2.3: to make this sub-section more understandable, remember to define the GDD at the very beginning (e.g., that it is the **daily** mean above 5°C, not, e.g., seasonal or monthly mean).

Thank you for your comment. We assume you mean Section 2.2 here, about the GDD model. We have clarified that the GDD is defined based on daily mean data at the beginning of this section.

"Growing degree days' is defined as the accumulated daily mean temperature sum during the growing season, where the growing season is the period with daily mean temperatures at or above 5 °C (Fig. 1c, Carter, 1998; Hanssen-Bauer et al., 2017).'

6. The sub-chapter 2.4 gives lots of historical information. However, I am not quite sure if all this is relevant for the findings and the discussion of the MS (see, e.g., p. 6, l. 222-228, 239-242). Simply, if there have been some degree of societal collapse following the 536/540 event, is it relevant to name tribes living in the area half a millennium later? Instead, I would have liked to know what previous (archaeological) research has established about these societies, e.g., regarding their livelihoods and population size. Considering the focus of the paper, it would be important to know if crop cultivation was the main or just a supplementary source of nutrition. Now some of this information was brought up in the Discussion (p. 26-27), but perhaps the state of the previous research could have been introduced already here?

Thank you for your comment. We agree that the relevance of this does not become clear from the context. Thus, we have rewritten Section 2.3 and the Discussion 4.4. to clarify the historical context. In 2.3, we have taken out the information that is not of direct relevance to this paper. Where possible, we have added on the societies from before the mid-6th century. See also our reply to comment #25 from Reviewer 1.

In addition, we have added information on subsistence strategies in Section 2.3.

7. Minor point: p. 6, l. 354, should it be "Section 3.4" instead?

Thanks for your comment. We refer here to Section 2.4 because this is where the data is described. The results of the pollen diagrams are described in Section 3.3.

Results

8. Please, provide a reference where the AOD data is gained from (Toohey & Sigl 2017?).

Thanks. We have added a reference to Toohey and Sigl (2017) to the figure caption and the text.

9. In figure 3 and p. 12, l. 361: define what is the "5°C line." This information comes on p. 23, but it would be essential to mention already here that the line refers to the **AMJJAS mean below** 5°C (and not to be confused with the GDD of **daily mean above** 5°C discussed elsewhere in the manuscript).

Thank you for your comment. We have clarified in the caption of Figure 3 and the corresponding text that the 5 degree line here corresponds to the AMJJAS mean temperature.

10. The last paragraph (l. 371-380) on page 12 is rather difficult to follow. For example, it is not clearly stated that the dominant NAO+ is based on model simulations over **pre-industrial** times. Furthermore, it is not clear for the reader what the SLP anomalies indicate. Does the higher SLP over the high latitudes and the decreased SLP over mid latitudes indicate a shift to a negative NAO?

Thank you for your comment. The dominant NAO+ pattern as described in this section is based on observations from 1949-1992 (Michelangi et al. 1995) and reanalysis from 1974-2007 (Cassou, 2008), as well as climate model data from 1-1850 CE (van Dijk et al 2022). We have clarified this in the manuscript accordingly.

We do agree that a further explanation of the shifts in SLP are good to have in the text, and have rephrased the paragraph to include a description of what the SLP anomalies indicate.

'... After the 536 CE and 540 CE eruptions, the overall large-scale atmospheric circulation pattern shifts to a higher pressure over the high-latitudes and decreased pressure over the mid-latitudes, over the North Atlantic region (Fig. 4). This corresponds to a shift towards a more negative NAO. 25 years after the 536 CE, the SLP has significantly increased over Scandinavia (Fig. 4a). … '

11. Minor: I could not find a Section 2.1.2 (p.12, l.372) in the MS that was referenced in this section.

Thanks, this should have been 2.1.1. We have corrected this.

Discussion

12. Overall, I would have liked to read further discussion on the challenges to link specific volcanic eruption(s) to societal impacts evidenced in archaeological records. Although the resolution of the pollen evidence is good (c. 8-24 years, p. 9, l. 307), can we make a claim that the societal impact resulted from volcanic cold pulse? For example, there are some claims that the Justinian plague could have extended all the way to Scandinavia. Thus, could the mid-6th century pollen and archaeological signal result from the Justinian plague, and have nothing to do with the 563/540 volcanic eruptions? Likely, this was not the case. Yet, in my opinion, more critical assessment on the challenges of combining the different temporal resolution of model simulations, proxy data, and archaeological record would have strengthened the discussion.

Thank you for your comment. We have added a discussion on the impact of the Justinian Plague to the pollen section in the discussion.

'In addition to the volcanic double event, there was an outbreak of the Justinian Plague in 541 CE. This has been proposed to have led to a decline of up to half the population in the Mediterranean area (Wagner et al., 2014), and has been found to have impacted societies as far as Germany (Harbeck et al., 2013) or the UK (Keller et al., 2019). If this was the case in Norway, this would have appeared in the pollen. However, even though the plague has been assumed to have reached Scandinavia, no DNA evidence has been found on skeletal remains so far (Gundersen, 2019). In addition, recent studies suggest that the plague was

not a primary factor in the demographic changes in the 6th to 8th century (Mordechai et al., 2019). We therefore argue that the decline in pollen during this time was not likely a direct effect of the Justinian Plague but may have been related to volcanic induced cooling.'

In addition, we have added a discussion on the challenges of combining the different temporal resolutions in the synthesis section.

'To connect the coarse spatial resolution from the climate model to the local pollen and archaeological data, we utilised the GDD model. This acts as a connector, enabling us to still use the high temporal resolution information the climate model provides, without losing the spatial variability between landscapes from the different case study areas.'

13. The term "vulnerability" is used rather vaguely here (p. 26 onward). What do the authors mean? For example, the crop sensitivity to climate anomalies because of topography/location, or the societal vulnerability due to monoculture and/or few livelihood options?

Thanks for your comment. Here we are primarily talking about the sensitivity of crops to climate anomalies. We have clarified this in the text. See also our reply to Reviewers 1 comment #2.

'The results for the three case study areas suggest different levels of agricultural vulnerability to a volcanic cooling event, with Fron being particularly vulnerable and Sarpsborg being seemingly resilient towards colder climate conditions. …'

14. The discussion on p. 27, l. 759-764 is very good and interesting! I would have wished more of this style of approach over the whole discussion section. In addition, perhaps you can pinpoint more clearly how your novel findings contribute to and/or challenge the previous research.

Thank you for your comment. We have rewritten parts of the discussion session with this comment in mind. We have also added a discussion on the contribution of this work.

'... there is no real contradiction between the two narratives and a new pictures is gradually emerging that indicates great diversity in disaster impact and human responses throughout Scandinavia, depending on the character of subsistence strategies, and local and regional variations in landscapes and climates, but also possible secondary and additional factors such as pandemics, social unrest, warfare, competition over resources, and trade disruptions (Gundersen, 2021). Our study contributes in this direction, by utilising a wide range of cross-disciplinary datasets and exploring the many facets of volcanic climate change on both regional and local scales. By contextualising the results, we have provided a diverse picture of the possible consequences of the 536/540 event for farming societies. However, although the present study demonstrates the potential of combining climate and GDD modelling with paleo-botany and archaeology, more issues should be thoroughly explored in order to be able to better understand the role of climate variations for social change, in particular those addressing subsistence strategies in a wider sense than just farming. …'

15. As noted by RC1, comparing the southern Norway results to other Nordic case studies might have been interesting. For example, Oinonen et al. 2020 (cited earlier in the MS) found that a population in western Finland was not affected by the 536/540 climatic downturn, most likely due to the resilience gained from diverse livelihoods.

Thanks, we have elaborated on the discussion and included additional publications from Denmark and Finland, including the publication from Oinonen et al. (2020). See also comment 6 from Reviewer 1.

16. Relating to the comment above, I would have liked to know more how different likelihoods are evidenced in the pollen/archaeological record. Were there differences between the three study areas?

Thanks for your comment. We have added a description of the subsistence strategies for the different study areas in section 2.3 and we have included this in the discussion in section 4.4.

17. Minor: in later historical times, oat cultivation is associated with animal husbandry (as oat was used as fodder). Could the increase of oat pollen in the Fron area be connected to a shift to more intense cattle herding?

Thanks for your comment. This is an interesting idea, but not very likely at this point in time. The relatively short and cold summers of Norway meant that grain, including oats, was more or less exclusively used as human food. See for instance Myhre et al. (2002).

18. Minor: define what is ergot (p. 24, l. 616). Now the description comes only on page 27, but as this rye fungi might not be known by non-experts, the definition should be given when it is mentioned the first time.

Thanks, we have included a more thorough description of ergot.

'Another indicator of rising water levels is the fungus claviceps purpurea, commonly known as ergot, which thrives in cool areas with poor drainage and damp conditions (Alm and Elvevåg, 2013). In Southwest Norway, ergot has been discovered at the Forsand excavation site (Løken, 2020), thus suggesting wet conditions during our study period.'

Synthesis:

19. Why was the year 537 CE selected here (Fig. 9)? Why not 536 as this year is evidenced as the coldest year of the period in many Scandinavian tree-ring records? And, if I am not mistaken, also the simulation data support year 536 being the coldest one (Figure A1).

Thank you for your comment. We have picked the model simulation that reflects both the temperature as well as the precipitation changes according to the pollen data and archaeological finds and other reported sources. Most individual simulations show a distinct cooling over Scandinavia for both the first and the second year after the eruptions, with the first year being the coldest after the 536 CE eruption. On the other hand, for the precipitation anomalies the patterns are quite diverse with 537 CE showing wetter conditions in both southwestern Norway and eastern Sweden. We have clarified this by adding to the 4.6 Synthesis :

'The chosen climate model run and post-volcanic year is based on the temperature and precipitation pattern together.'

This part of the study is to illustrate that individual model simulations are a possible representation of reality, but there is only one reality. Using the proxy data and archaeological evidence can help restrain which model simulations were more likely.

Conclusions:

20. Minor: It is mentioned that "historical evidence" has indicated cold and wet conditions in Southwest Norway over the 536/540 event. This should be changed to "archaeological evidence" (or the like), as there are no contemporary written records from the area.

Thanks, we have corrected this.

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