

Clim. Past Discuss., referee comment RC2
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Comment on cp-2022-23

Anonymous Referee #2

Referee comment on "Climatic and societal impacts in Scandinavia following the 536 and 540 CE volcanic double event" by Evelien van Dijk et al., Clim. Past Discuss.,
<https://doi.org/10.5194/cp-2022-23-RC2>, 2022

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 also 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).

I would recommend that the authors go carefully through the manuscript, paying 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.

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

1. Introduction:

- Why Toohey and Sigl 2017 (<https://doi.org/10.5194/essd-9-809-2017>) is not referred

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.

- 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)?

2. Methods

- 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.

- 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.

- 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).

- 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 in the Discussion (p. 26-27), but perhaps the state of the previous research could have introduced already here?

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

3. Results

- Please, provide a reference where the AOD data is gained from (Toohey & Sigl 2017?).
- 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 the GDD of **daily mean above** 5°C discussed elsewhere in the manuscript).
- 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?
- Minor: I could not find a Section 2.1.2 (p.12, l.372) in the MS that was referenced in this section.

4. Discussion

- Overall, I would have liked to read further discussion on the challenges to link specific volcanic eruption(s) to societal impacts evidenced in archaeological record. Although the resolutions 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 strengthen the discussion.
- The term "vulnerability" is used rather vaguely here (p. 26 onward). What 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?
- The discussion on p. 27, l. 759-764 is very good and interesting! I would have wished more 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.

- 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.

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

-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 Fron area be connected to a shift to more intense cattle herding?

- 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.

Synthesis:

- Why the year 537 CE was 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).

Conclusions:

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

Over all, this paper presents interesting results and has a great potential for related discussion with novel insights. Thus, after major revisions, I am looking forward to read the revised manuscript.