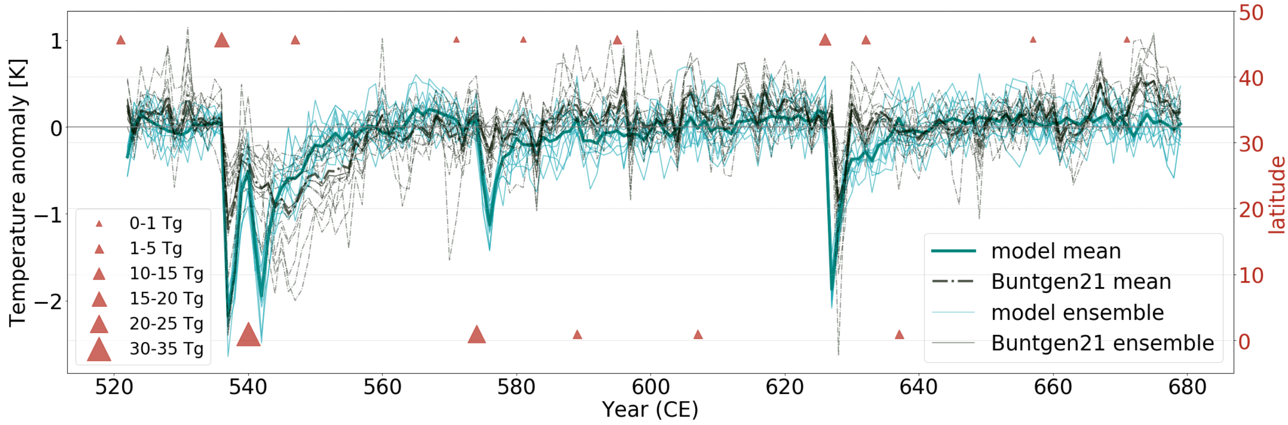
Dear editor and reviewers, thank you for the detailed review of the paper. We have compiled a point by point response going through all comments of the four reviewers. Our comments are given in blue and bold text.

General reply:
We have considerably revised the manuscript by rewriting the abstract, introduction and summary and conclusion, as well as separating the discussion from the result section. We updated the references in the introduction and discussion. As suggested, we now use the Büntgen et al. (2021) tree-ring reconstruction ensemble for the model - tree-ring comparison (see figure below) and have rewritten the comparison section accordingly, focusing on the Northern Hemisphere temperature anomaly.



Reviewer 2:
This is an interesting paper showing new model based information on seasonal climate response following major volcanic events during the 6th and 7th century. In addition, the paper shows also proxy based reconstructions for different regions of the NH and a few simple model/reconstruction comparisons.

There are a couple of shortcomings, gaps and weak parts of the manuscript that need to be revised, I thus recommend major revisions

Thanks for your constructive comments. We have taken your comments into account in the revised manuscript, as described in the general comments. The detailed comments are answered below.

Comments (in no particular order):

Abstract

1. The abstract needs a major revision, currently it is a compilation of individual results without a clear statement. There are statements about proxy reconstructions, model output and model-data

comparison, but there is no connection between the sections and also no dynamic explanations of what was found and new conclusions about climate during the 6/7 century.

The abstract was rewritten accordingly.

2. The first sentence is confusing as there is no actual detection/attribution study (yet) to support the finding that volcanoes are the main cause of cooling and not natural variability or a combination thereof.

We have changed the formulation of the sentence.

'While the onset of this cold period can be clearly connected to the volcanic eruptions in 536 and 540 Common Era (CE), the duration, extent and magnitude of the cold period is uncertain.'

3. Further, several paleoproxies are mentioned, but it is not clear what they refer to, whether they are used to date volcanic eruptions or to derive past climate.

The multiple paleoproxies, ice-core records, tree-rings and historical documents, are referring to both the past climate and the date of the volcanic eruptions. We elaborate on these in the introduction, and would like to leave it in the abstract as it is.

General manuscript

4. It would be interesting and valuable to have a paragraph on the socio-economic impact of the cooler and more variable climate during this period. This would be a nice link to the other sections of the paper.

We have a small paragraph about our future work at the end of the conclusion, as we want to investigate this in our next study. See also our reply to reviewer 1.

5. The paper is not up to date with the latest publications and the introduction needs to be revised accordingly. The first paragraph of the introduction is incomplete, confusing and needs better focus and the inclusion of more appropriate and new references to the state of the art in paleo-reconstructions for the study period.

Thank you for your suggestion. Newer publications have been added to the introduction, and the section has been rewritten, taking your first comments into account. The first paragraph has been restructured so that it is better focused and we added new references for the paleo-reconstruction part. We now cite the new Pages2k consortium (2017) paper instead of the

previous one, and papers from a.o. Moreno Chamarro et al. (2017) and Büntgen et al. (2021) have been added as well.

6. In general, the paper needs to be updated with recent findings related to reconstructions, post-volcanic responses in the paleocontext, regional interpretations, and data/model comparison studies.

Thanks for your suggestion. The part on reconstructions in the introduction has been better explained. The Büntgen et al., (2021) tree-ring reconstruction ensemble has now been used for the proxy comparison and we, therefore, focus the model - tree-ring section mainly on the new NH compilation. The regional tree ring comparisons figures have been moved to the appendix. The papers of Moreno-Chamarro et al. (2017), Zhu et al. (2020), and Halloran et al. (2020) have been added to the discussion to update the section with newer publications.

7. Furthermore, the paper also needs considerations on uncertainties.

Thanks for the suggestion. Now, we address the uncertainties in volcanic forcing, using different models, and the model - tree-ring comparison in the new discussion section.

8. In addition, two studies from 10 years ago on the onset of the LIA are cited (lines 39 ff). They are superseded by new findings.

The LIA is not in the focus of our study, and we do not aim to make a direct comparison. However, we added one more paper by Moreno-Chamaro et al. (2017) on the LIA period.

9. It is not clear why Stoffel et al., Büntgen et al. and Esper et al. are used for comparisons with the model (Fig. 5). New reconstructions are available (see Büntgen et al. 2021 Nat. Comm for a review) and possibly an ensemble series could have been used instead of single reconstructions, which do not reflect the true NH conditions but are locally biased.

At the time of writing and submitting our paper, the study by Büntgen et al. (2021) was not published yet. However, we acknowledge the importance of using the newest data available and have therefore reanalyzed the model – tree-ring comparison with the Büntgen et al. (2021) tree ring reconstruction ensemble data. The figures (Fig. 5 in the original manuscript) and corresponding text (Section 3.4 in the original manuscript) have been replaced and rewritten respectively.

10. It is also not clear why the authors compare the grid-based model output with local tree-ring-based reconstructions for three regions. In lines 390ff. they mention that such a comparison can be misleading, so they might reconsider this section.

Since we now use Büntgen et al. (2021), the focus in the main manuscript is on the Northern Hemisphere and we no longer focus on the local tree-ring-based reconstructions.

11. A more appropriate comparison could be made with continental reconstructions for Europe (Luterbacher et al. 2016, Env. Res. Lett) and for Asia (Zhang et al. 2018 Nat. Sci. Reports, 8, 7702).

See our response to comment 9 above. We now use the Büntgen et al. (2021) tree-ring reconstruction ensemble. We, therefore, focus on the Northern Hemisphere compilation in the main manuscript.

12. The *methods* and associated measures to compare reconstructions and model output need to be explained in more detail.

Thanks for your suggestion. Since we use the data from Büntgen et al. (2021) instead we rewrote the methods section accordingly. We have taken up your comment and added some details in the method section for the comparison. All the details of the tree-ring reconstruction ensemble can be found in the Büntgen et al. (2021) paper.

13. The authors report on the summer precipitation behaviour in the Mediterranean region in the model world. This part needs to be revised, as in reality there is hardly any precipitation in the warm season and if there is, it is mostly on the northern rim. Even in post-volcanic summers there is no clear signal in observations and reconstructions of the last centuries (Wegmann et al. 2014; Fischer et al. 2007, CRL). Please note that there are hydroclimate reconstructions from different areas of the Mediterranean with which the model output can be compared.

Both Wegmann et al. (2014) and Fischer et al. (2007) describe an increase in precipitation over southern Europe in summer, though a weak one. The modelled increase in precipitation in the Mediterranean is significant and corresponds to a shift in the larger scale atmospheric circulation, which is why we take it up in our discussion. The aim of this paper is to investigate if we simulate a long lasting cooling after volcanic eruptions and its mechanisms in general. Thus, we do not want to go in detail and compare single model variables with limited area-wide hydroclimate reconstructions, which is beyond the scope of this paper.

We have now included the papers from Fisher et al. (2007) and Rao et al. (2017), as suggested by you and reviewer 3.

14. In general, the work has a bias towards summer, which is not surprising as the tree-ring reconstructions resolve summer conditions. However, it would be important to provide some more insight into the conditions during the cold season and how the volcanic influence could change the annual cycle after the short and decadal volcanic influence.

The winter response is also described, especially when we connect it to the ocean - sea-ice feedback during winter/spring. In the discussion, there is more focus on the surface climate response in summer, because the patterns are significant, as opposed to the winter patterns, that are less significant due to the large internal variability in the winter months. To go into more detail of the winter circulation response would be interesting but goes beyond the scope of this paper here.

Section 3.1. volcanic response:

15. It seems that the Figures 2,a, c and d are not commented and interpreted in the main text. Are the time series in Figure 2 referring to summer?

Thanks for the comment, the description of figure 2a, c and d have been added to the results. Figure 2 refers to yearly mean responses and this has been elaborated in the text.

16. The plots are small and details cannot be seen. Please could you increase the readability of the figures in general, thank you.

The figure sizes have been increased, and figure 3 has been separated into two figures, so the maps are better readable. The sea-ice extent now is a separate figure.

17. It is not entirely clear what Figure 3 shows. Are all post-volcanic years during the study period averaged and shown in relation to pre-volcanic non-volcanic conditions? Please state this more clearly in the caption and main text. Also, please provide more explanation of the multi-decadal analysis and how it is carried out.

The new figure 3 (shown below) shows indeed an average of all the post-volcanic years, in relation to the 0-1850 CE mean. The 0-1850 CE mean was taken because this is a long enough period for the volcanic response to be negligible. Test calculations were carried out to check the difference between the mean of the 0-1850 CE run and the mean of the 1200 year control run (without volcanic forcing). The difference was very small and not significant. Therefore, the 0-1850 CE mean was used to calculate the anomalies. This has been elaborated in the Results section and the caption. Some sentences have also been added to the methods to clarify the calculations.

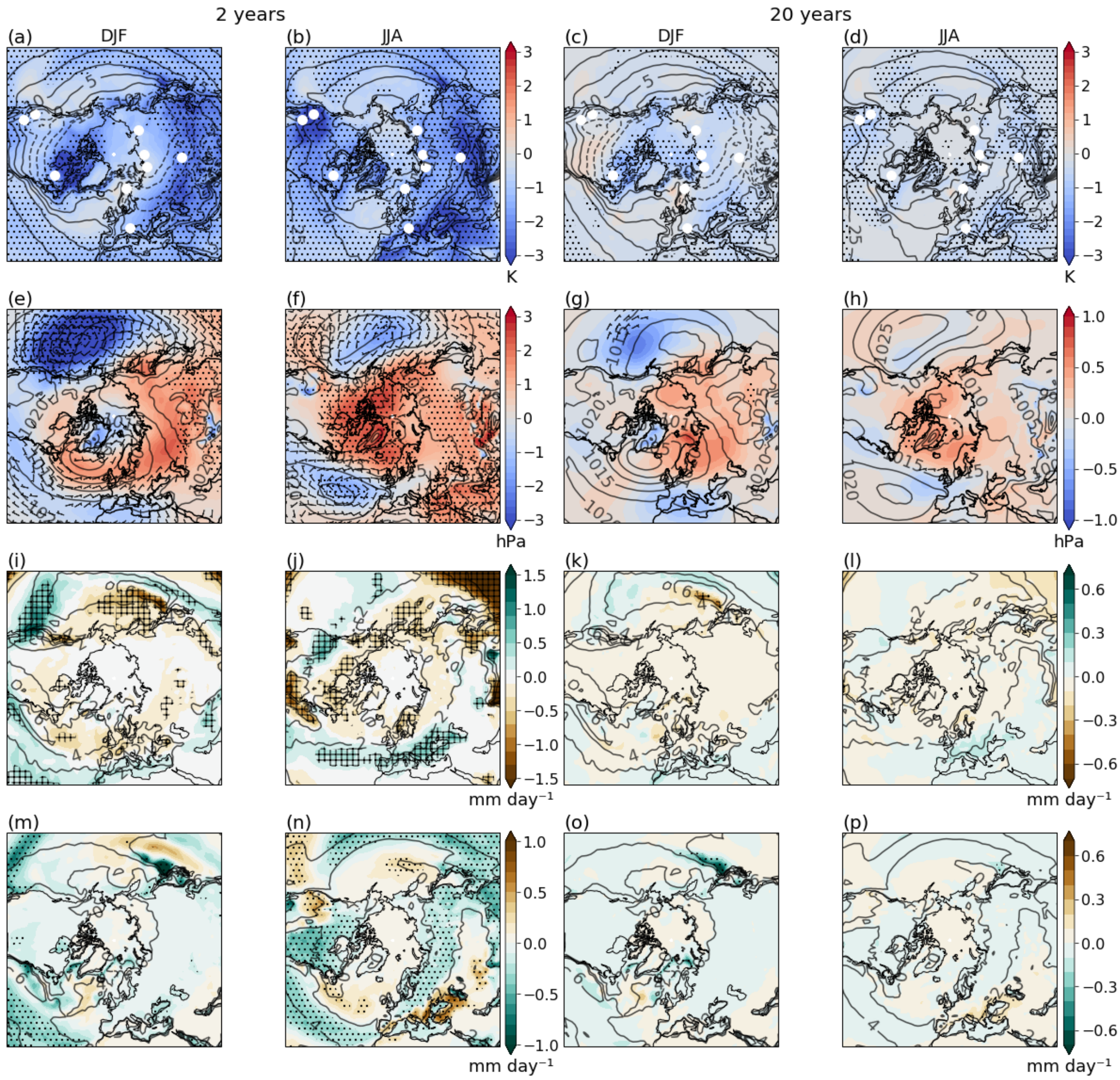


Fig. 3 NH maps of boreal winter (DJF) and summer (JJA) 2m air temperature (a-d), sea level pressure and 10 m wind (e-h), precipitation(i-l), and evaporation (m-p) for 2 years and 20 years after the eruptions, poleward of 30◦N. The maps represent the ensemble mean of the 2and 20 year mean after the four major eruptions in the study period. The 2 years after the eruption are year 1 and year 2 after the eruption forDJF, and the year of the eruption and one year after for JJA, respectively. All variables are given as anomalies wrt 0-1850 CE. The 0-1850CE climatology is given as contours and the tree-ring locations are represented by white dots in the 2 m air temperature maps. The ensemble standard deviation 1σ(2σ) for 2m air temperature, SLP, evaporation and precipitation are hatched (stippled). Note the different scales for the2 year and 20 year maps and that wind anomalies are shown only for 0.5 and 1.0 m/s intervals.

18. In Figure 3b I see a strongly negative NAO with higher absolute pressure in the subpolar regions and lower pressure in the subtropics. This does not seem correct given the independent evidence of a strongly positive NAO following strong tropical volcanoes.

The sea level pressure pattern in DJF is not significant at the 2 sigma significant level. Also, the maps are the mean of 2 years after the 4 eruptions. Around 40-45N we do see increased pressure over Europe and decreased pressure over the subtropics and anomalous easterly wind. When calculating the NAO index, we obtain a positive NAO for 3 out of 4 eruptions, and the second year shows a neutral or more negative NAO. It is known from other studies that CMIP models do not consistently simulate a NAO+ response after volcanic eruptions (following Driscoll et al 2012) that is evident from observations (following Robock and Mao, 1992), and that a large ensemble is needed to show this model response (Bittner et al 2016). This has been added to the discussion.

19. It might be good to show the statistically significant areas and instead use a field sign test to show which areas are different compared to the reference period.

We chose to also show non-significant areas, as they are important for some of our discussion, such as for winter sea level pressure and NAO responses. The significance of the figures is calculated by using the 2 sigma of the reference period. Significant areas are therefore by definition different from the reference period.

20. Please consider the interesting paper by Moreno-Chamarro, et al. 2017 re: Winter amplification of the European Little Ice Age cooling by the subpolar gyre. Nature Sci. Reports, 7, 9981. It is not about the 6/7th century, but about an active period during the Maunder Minimum and the role of internal variability versus forced influence on European seasonal climate. It might be worthwhile to review this publication as well to see whether similar processes might have been in effect and to include it in the interpretation.

Thanks for your suggestion. We have incorporated this paper in the discussion about the ocean sea-ice feedback.

Summary and conclusions

21. This part needs to be shortened and cleared of duplication. It is more a listing of some results without clear connection and explanation and what the main conclusions are from this study.

The summary and conclusions have been shortened, cleared, and rewritten.

22. Fig. A2: Please provide the units of a) and also how the NAO indices have been calculated.

Units have been added to the figure and a more detailed description has been provided in the text.

‘For this study, we calculated the NAO index using the method from (Hurrell et al., 1995). The NAO index is calculated by taking the first principle component of the SLP anomalies for the area 90W, 40E, 20N, 80N.’

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