Climate of the Past Discussions

Clim. Past Discuss., referee comment RC3
https://doi.org/10.5194/cp-2021-49-RC3, 2021


# Comment on cp-2021-49

Anonymous Referee #3

Referee comment on "Was there a volcanic induced long lasting cooling over the Northern Hemisphere in the mid 6th–7th century?" by Evelien van Dijk et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-49-RC3, 2021

This paper develops and analyzes an ensemble of climate model simulations covering the period of 4 large eruptions in the 5th and 6th century as well as the decades following. These modeling results are really very exciting because they provide brand new and much needed insight into the potential behavior of the climate system following large and important eruptions (including two closely-spaced eruptions in the 6th century) in the first millennium and the potential for new paleoclimate proxy/model comparisons of this important but still sparsely known period of the Common Era. These results will therefore be of great interest in understanding the range of responses to volcanic eruptions and relevant for both modelers and proxy paleoclimatologists.

My primary critical observation is that the manuscript is excessively descriptive. I think a stronger manuscript would result from placing the model (and the proxy-model) comparison directly in the larger context of uncertainties about first millennium climate and response to large eruptions, isolating the model behavior that is most interest to understanding this unique time period, a more complete treatment of the proxy data, and more accounting for uncertainty (the real strength of the model ensemble is the range of possibly behaviors of the ocean-atmosphere system that can be observed and used to quantify variability in the response). My major comments below are mostly about these issues, and some minor comments and suggestions follow.

Major comments:

Abstract and Introduction: I think the paper (and readers) would benefit from a re-writing of the abstract and a reframing of the importance of this work. As it currently stands, one of the particularly interesting aspects of the 6th/7th century eruptions (that persistence or not of the cooling and therefore the duration of the 'Late Antique Little Ice Age') is mentioned only rather vaguely, and much of the abstract is given over to a simple description of the model phenomena. A stronger abstract would set the stage for the proxy disagreements (and note the sparseness of the proxy data as exacerbating the difficulty in understanding these uncertainties) and then frame the results in terms of this as well as the importance of understanding large events like the closely spaced eruptions of the 6th century. Likewise, the Introduction would benefit from using more recent publications about the Little Ice Age as well as a more structured framing of the uncertainty and motivation for the study.

Initial discussion of proxy data (Lines 53 to 59). This section needs to be enhanced, as an appreciation for the uncertainties and causes of uncertainties for first millennium climate reconstructions, particularly with the resolution needed to resolve volcanic eruptions precisely, is important for the comparisons that come later in the paper. While Ahmed et al. 2013 (which is properly PAGES 2k Network 2013) was a distillation of temperature proxy data for the last IPCC, it is superseded by a number of papers, including PAGES2k Consortium 2017 (Emile-Geay et al. 2017). The authors might also want to cite Esper et al. 2018 (doi is 10.1016/j.dendro.2018.06.001) which analyzes tree-ring proxy uncertainties in the early part of the last millennium (and therefore these uncertainties will be even greater in the first millennium of the Common Era). Particularly here: there is a substantial body of literature now (some of it discussed later) about the ability of different tree-ring proxy measurements to resolve or 'smear' volcanic cooling - MXD vs tree-ring width. Similarly, multiproxy approaches that mix seasons, hemispheres, or are low resolution might not resolve the volcanic signal or may require additional post-processing of model data to make an appropriate comparison. Since the source of possible uncertainties in proxy reconstruction of 6th/7th century climate is important for the comparisons that come later, I think a more thorough and up-to-date discussion is warranted here in the introduction.

Proxy data (Section 2.2): As above, I think a more complete and clear description of the proxy data here would be useful for later in the paper when comparisons become important. Table 2 lists the individual proxy data that are available (this is good to have this, since the representation of tree-ring width and MXD can sometimes be subsumed when using a reconstruction), but the wording in Section 2.2 is confusing - for instance, what does 'The first four sites combined are the "NH land" compilation by Stoffel et al. (2015)' mean? Does this mean that the 4 sites listed first in the Table were also used by Stoffel? This isn't clear. By the time one arrives at Figure 5 and the associated text, it isn't clear what/which of each of these proxies is going into the comparison, so a more thorough discussion of the proxy data used and what each reconstruction in Figure 5 contains is necessary. The following line say 'The data sets 135 contain a mix of tree ring width (TRW) and maximum latewood density (MXD).' and this is true, but only the NSCAN MXD data are available for the 6th and 7th century - the rest are tree-ring width and subject to the potential problems described in the following lines. Again, this section seems rather sparse and is not clearly organized, and yet limitation of the proxy data (or their particular time series properties) will become important later in the paper. Since the authors prepared this manuscript, there as been a new ensemble reconstruction of Common Era temperature (Buntgen et al. 2021, doi is 10.1038/s41467-021-23627-6) - while I realize this paper actually came out after this manuscript was submitted and the authors cannot have been expected to use these reconstructions (of course!) I would encourage them to at least consider them (formally analytically or informally as comparison, since the early part of the LALIA is examined in the paper), at the discretion of the editor. Finally, it would be worth I think mentioning why multiproxy reconstruction like PAGES2k, LMR etc. are likely to be unsuitable for this comparison and why the authors rely (and rightly I think) on tree-ring data.

Proxy-model comparison: This section is unclear in places and speculative without support in others; for instance, (Line 355) I'm not sure what 'The temperature anomalies from the model simulations and the 2 sigma variability range fall within the 2 sigma variability of the NH of the model simulations' means? I also find it to be too qualitative - what does 'good agreement' mean and how to measure it? In Line 362, 'More deviation is visible' is also vague. I think this section would benefit from a more straightforward and quantitative exploration of the proxy-model comparisons. In Line 365, I'm not sure how something could be both 'less good ... but still remarkable'? However, also the full range of the model ensemble should really be considered in the comparison - the 'real world' is just simply one iteration of what could have happened under different initial conditions, forcing uncertainty, feedbacks, and interactions and stochastic variability. So the comparison is not simply to the multimodel ensemble mean or even peak cooling, but taking into account the full range of ensemble variability and seeing the tree-ring data as one 'ensemble member' of possible actual and physically plausible atmosphere-ocean states.

Later in Line 376, the authors write that 'There is a good agreement between the tree-ring temperatures and the model temperatures after normalization' - but again this lack of precision doesn't do justice to the comparison - indeed there appears to be reasonable association fr the major eruptions for NH temperature from Stoffel (including some MXD) and the NSCAN MXD, but for Alps and Altai the lag recovery is longer. So simply saying there is a good agreement masks interesting differences. In Line 381, this seems very highly speculative: 'could be due to the prescribed volcanic forcing in the model, and that the 547 eruption might have had a stronger impact on NH land than the model simulates.' - why wouldn't the same apply to Stoffel or NSCAN then? There would need to be some support for this to claim it as a source of the discrepancy. On Line 390, again this seems highly speculative: 'Perhaps the century long lasting cooling may be only apparent in the Alps and Altai tree-ring records, as the cooling is a local feature occurring at high altitude of the mid-latitudes.' Again, in Line 395 the authors speculate that 'The change in hydro-climate corresponds to the soil moisture availability for the trees and thus could have impacted tree ring growth', but again this is just speculation, and indeed for the Alps, which have the longest lag at odds with the model, the 20 years summer precipitation anomaly (Figure 3c) is positive and the winter signal is mixed.

Potentially the most parsimonious answer is that tree-ring width has a tendency to increase the 'tail' of the post-volcanic cooling and change the timing of recovery to baseline. But the authors give this only a brief mention in Line 371.

Summary and Conclusion: This section is largely a restatement of the paper, but would be stronger with a distillation of the main points of the article and major conclusions.

Additional Comments:

Line 12, Line 13 'land–sea'
Line 50: perhaps 'multidecadal cooling, as has been reconstructed by Buntgen et al. (2016).'
Line 55: 'Common Era'
Line 101-102: this sentence seems out of place? 'A common paleo-model set-up is to use 1850 pre-industrial conditions, due to model simulation limitations'
Line 103: This is confusing as written - but why use standard deviations instead of the temperature deviations in the bootstrap?
Figure 1: are these data from Jungclaus et al. and Toohey and Sigl? Best to include a citation with the figure caption as well
Table 1: I assume the months (January) are assigned because the actual month of the eruption is not known for these? It would be worth mentioning this (and some of the potential consequences, e.g. Stevenson et al. 2017) in the methods section as well
Line 152: '2 K' - relative to which baseline? I presume the 521-680 CE mean mentioned in Line 142, but please clarify
Line 157: 'decreased for □20 years' - it is difficult to see this in Figure 2 because of the scale - can you provide a range of the actual return to baseline periods? Particularly since the closely-spaced 536/540 eruptions would be expected (I think) to collectively show a longer recovery time than the individual eruptions in the 570s and 626 event
Line 169-175: Some other more recent papers to consult (and cite as appropriate) might include Lehner et al. 2013 and Slawinska and Robock 2018
Line 270: Perhaps useful to consult Fischer et al. 2007 (doi is 10.1029/2006GL027992, 2007) and Rao et al. 2017 (doi is 10.1002/2017GL073057) with respect to Mediterranean (and Europe) response to eruptions in historical and paleoclimate data
Figure 5 caption: 'Fennoscandia' ?

Line 372: as well as estimate of the forcing, spatial distribution of AOD anomalies, feedbacks, uncertain in timing of the eruption (Stevenson et al.) - so, lots of potential uncertainties.
Line 377-: I'm not sure this requires any further explanation - the models and data have different references periods and likely different means, but what is of interest is the volcanic signal, so a renormalization isn't that remarkable