

Clim. Past Discuss., referee comment RC2
<https://doi.org/10.5194/cp-2021-4-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on cp-2021-4

Oliver Bothe (Referee)

Referee comment on "The unidentified eruption of 1809: a climatic cold case" by Claudia Timmreck et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-4-RC2>, 2021

Dear editor, dear authors,

the manuscript preprint cp-2021-4 by Timmreck and co-researchers is a valuable contribution to our understanding about the climatic effects of an unidentified volcanic eruption sometime in 1808 or 1809 CE.

The manuscript reports (i) results from a set of dedicated climate simulations sampling the forcing uncertainty and (ii) the comparison of these simulations with available observed and reconstructed local, regional, and hemispheric data sets.

I do not have any major concerns with the manuscript, but I have a small number of minor comments. One of these minor notes is tending slightly towards major.

That is: I do not believe that the manuscript does what the authors claim in the title. While I appreciate a catchy title, I think overselling is a net-minus. My understanding of the title is that the authors claim to show why 1809 remains a cold case. The title plays with the cold-case terminology from criminal investigations and procedural crime series on TV and streaming. However, from my point of view the manuscript does not show enough in that respect to claim this title. A positive reading is, the title already announces the failure to provide major new insights into location, seasonality, or strength of the eruption, a negative reading is that the title suggests large new insights, why we won't have much success in becoming more certain about this eruption. I don't think these large new insights are provided. The manuscript is in a sense incremental while also presenting some very valuable - and I think new - simulations and in addition supporting previous understanding about the eruption in 1809.

Minor

Line 19: The claim that the early 19th century is the coldest period of the last 500 years is based on a reference to Jungclaus et al. (2017). While I am not entirely sure whether this is meant to be a global statement or a hemispheric one, there have been a number of recent reconstruction efforts spanning the period of the last 500 years and beyond, which may or may not require to reassess or qualify the statement. That is, such an absolute statement requires assessing the newest evidence, which in this case may include, for example, Büntgen et al. (2020), PAGES2k Consortium (2019), and more.

A note on the paragraph starting in line 78: the authors detail changes in their atmospheric component but they only mention in passing that there were also changes in their land component. That is fine, but the authors probably also can be briefer in describing the atmospheric model.

Figures: some of the Figures have a strangely low resolution (on my screen). While this will be caught by the technical editing at Copernicus anyway, I wanted to mention it.

Data: EEIC. I have to admit I am not really up to date and may express my ignorance but how does the EEIC data differ or improve on the most recent ICOADS data?

Data: Stations. I was thinking there should exist tropical stations for the period of interest. Do they have too little temporal coverage, or were they not of interest?

At line 284 I was wondering whether a very short comparison between reconstructed and observed results would be of value there.

Index comparisons: Line 347ff. I fail to see the relevance of the circulation results for the manuscript. They feel unrelated to the rest of the manuscript. They are again referred to in the discussions but to me it also remains unclear there why they are relevant for the argument of the manuscript. The point in the discussion does not depend on the analysis, does it? This is not necessarily a problem but skipping the relevant parts may make the manuscript more concise.

Discussions of central Asia: The authors give good reasons for potentially weak reconstructions in Asia and particularly central Asia. Has there been a general evaluation of how well the MPI-ESM performs in these regions?

TR: I invite the authors to skip the abbreviation TR. "Tree ring" is not too long to be spelled out everywhere.

NA: similarly to TR, is it really necessary to abbreviate North America?

There are a small number of grammar/spelling/typos etc that I assume are artefacts from tracking changes in a document. I only mention Line 655: proxi -> proxy

Is a "comparative assessment" a comparison?

I wonder whether it makes sense to spell out S in line 638.

Note

Finally, I have to mention that I cooperated with some of the authors in the past.