Climate of the Past Discussions

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Comment on cp-2021-4

Anonymous Referee #1

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-RC1, 2021

The model-data discrepancy with respect to the climate response to volcanic eruptions is a long-standing problem that has been documented in IPCC AR5. Uncertainties can arise from both the model and data sides, and numerical experiments are a great tool to investigate the impact of volcanic forcing on the climate from aspects such as magnitude and hemispheric symmetry of the forcing, eruption location, eruption season, etc.

This study by Timmreck et al. takes the case of the unidentified 1809 eruption, a large eruption nearly missing in historic documents but its existence is apparent according to ice core sulfur records. The authors conducted numerical experiments with the MPI-ESM1.2-LR climate model, investigating the climate response to different volcanic forcing patterns regarding orders of magnitude and spatial structure (hemispherically symmetric/asymmetric forcing). In addition, they surveyed several instrumental observations and reconstructions based on different proxy types, and performed detailed model-data intercomparisons over the tropics and the Northern Hemisphere extratropics, based on which, they gave an estimate of the VSSI level for the 1809 eruption, and confirmed the importance of the spatial structure of the forcing. Therefore, this study has a topic that is both timely and of importance, and is so far a unique modeling study focusing on the 1809 eruption, which contributes to the understanding of the impact of volcanic forcing on the climate response. The model-data intercomparisons are also valuable for the paleoclimate reconstruction community.

Overall, I think the manuscript is in high quality: the text is well organized with a clear structure; the analyses are to the point and presented with instructive ï¬gures. That said, some cleaning up and clarification needs to be done, which I listed below. Once those have been addressed, I recommend the work be accepted for publication.

Details

- Section 2.1.1: It might be worth introducing the parameterization scheme for the aerosol microphysical processes in MPI-ESM1.2-LR. As suggested in recent studies (e.g. LeGrande et al., 2016, Nat. Geosci.), some CMIP5-era climate models can produce overly strong volcanic cooling due to unrealistic aerosol microphysics. How is the scheme in MPI-ESM1.2-LR different?

- Eruption timing: In L107 it is mentioned that the 1809 eruption is set to occur on Jan 1st of 1809. Recent studies (e.g., Predybaylo et al., 2020, Commun. Earth & Environ) have suggested that the eruption timing may also affect the climate response, especially the ENSO response, due to different circulation conditions and ENSO phases. Since the SOI response is also assessed in Fig. 6, it might make this study more complete to add one extra experiment testing the sensitivity to the eruption timing.

- Fig. 2: It might be better to use green triangles or other readable colors and symbols to denote the location of the tree-ring proxies. Red can be misleading given that red also represents a high temperature anomaly. The colorbar may also be adjusted to drop the white color to differentiate the missing values.

- Fig. 5 & 6d: It might be worth showing the relative sea surface temperature (RSST) (Khodri et al., 2017, Nat. Commun.) to highlight the impact of volcanic forcing on ENSO relative to tropical mean cooling in the supplementary information. It may or may not affect the results significantly, but either way it is a valuable assessment.

- Fig. 6: If I am not misunderstanding, the volcanic forcing magnitude in Best, Low, and Hight experiments can be ranked as High > Best > Low according to Fig. 1, and there's no difference in their meridional structure. I am curious about the reason that the SOI response in the Low experiment seems to lie between that of the Best and Hight experiments during both winter and summer. Particularly, does the fact that the High and Best experiments show opposite signs during the summer indicate that the SOI response is actually more internally driven than externally forced? Similar doubts exist for other indices that the impact seems not following the same monotonic order of the magnitude of the forcing. It might be good to reorder the bars in the figures as pre, Low, Best, High to highlight the potential impact of magnitude, no matter exists or not.

- Fig. 7a and L200-204: In L200-204, it is mentioned that the authors accounted for the sparsity and irregularity in spatial and temporal sampling of the EEIC data, but it is unclear how good the performance of the processing is, and the authors still see overall dampened tropical SST anomalies in EEIC compared to model simulations in Fig. 7a. I was wondering what the comparison would look like if compare the mean of the model simulated SST anomalies over grid cells nearest to the locales of the EEIC logs to the mean of the original EEIC data. Similar strategy might be worth taking for other comparisons if the observations/reconstructions are available over multiple sites instead of a processed regional mean.

- Fig. 9: It seems that the strategy mentioned above is taken here in Fig. 9 as the model simulations are "similarly sampled". Perhaps can add an extra column for the visualization similar to Fig. 7a, comparing EEIC to ensemble means, but for two separated regions. Is it overall a better agreement in Fig. 9 than in Fig. 7a? If so, does it mean the sparsity of the EEIC logs is not well accounted for as mentioned in L200-204?

- Fig. 13 & 14: What is the rationale that the anomalies are calculated with respect to the years 1806-1820 here instead of 1800-1808 as in previous figures? The decision will largely affect the model-data comparison on the response to volcanic forcing.

- L527: a typesetting issue (delta-18-O)