

Interactive comment on “Volcanic and ENSO effects in China in simulations and reconstructions: Tambora eruption 1815” by D. Zhang et al.

Anonymous Referee #1

Received and published: 25 July 2011

Summary

The authors use an ensemble of five simulations of the last millennium performed with the COSMOS model in order to investigate the climate impact of volcanic eruptions on China. The study tries to separate the impact of ENSO and the volcanic forcing. Specifically, the Tambora eruption in 1816 AD, which is the second strongest eruption of the last millennium, is one focal point of the study.

General remarks

The authors use a standard approach to investigate the impact of ENSO, volcanic eruptions and the combined effect, so the originality is weak. Still the focus on China, a

C1124

region which might be strongly affected, is interesting and timely. Unfortunately, the paper appears to be inattentively written and analyzed. There is a lack of describing the methods of analysis, model evaluation with respect to ENSO and volcanic eruptions, the interpretation of the results and the placement of the results in existing literature. The most important problem arises from the fact that the analysis of the Tambora eruption is not based on ensemble means, thus the robustness of the results is questionable. Given these shortcomings I regret to recommend that the manuscript should be conditionally rejected. Still, I hope that my comments and suggestions below stimulate the authors to work on a revised version of the manuscript (MS).

Major comments

1. There is a lack of important references:

- Page 2063, line 1: Fischer et al. (2007, GRL) showed in reconstructions that there is an impact on precipitation and pressure (positive NAO after an eruption). Shindell et al (2003, J Climate, 2004 JGR) also showed the dynamical response due to volcanic eruptions and which is also important for this study to solar forcing. These publications are important as the authors focus on Tambora, which erupted during a phase of low solar activity (Dalton Minimum). Yoshimori et al. (2005, J Climate) investigated in ensemble simulations of another period of low solar activity (Maunder Minimum) the imprint of volcanic forcing also on regional scales. Schneider et al. (2009, JGR) showed similar results with another model.

- Brovkin et al. (2010, Tellus B) use the same ensemble simulation but focusing on the 1258 eruption, so please mention it in the MS and explain similarities.

- Robock (2000, Rev Geophys) give a review on the climate impact of volcanic eruptions.

- Coob et al. (2003, Nature) and Mc. Gregor et al. (2010, ClimPast) reconstructed ENSO-type variability. I think these data might be helpful when comparing the model

C1125

simulations with proxy evidence.

- Mann et al. (2009, Science) combined globally proxy data and investigated the forcing impacts during the last millennium.

- Mann et al. (2005, J Climate) investigated the volcanic impact on the tropical Pacific, i.e., ENSO. Again this study is very important to evaluate the simulation presented here.

2. Model evaluation: I think the authors need to include a section dealing with the model's ability in simulating the response of climate system in the Northern Hemisphere and the tropics due to volcanic eruptions. Moreover it is necessary to show the ENSO variability - I guess that the model has a rather regular ENSO mode and a double ITCZ. Moreover the authors state that ENSO is connected with the Asian Monsoon systems (page 2066, Blender et al. 2010) but is this realistic? So please discuss the weakness of the model simulations as well as the resemblance with observations. In particular how well is the so-called winter warming after an eruption simulated. In Stechikov et al. (2006) the observations show a rather strong warming in Northern China and Siberia. In Fig. 2a (yearly mean) no warming is simulated – is this due to the different response during summer or a model bias.

3. Method description:

- Page 2066: How is the volcanic forcing introduced to the model? Is it introduced in terms of solar irradiance (problematic as it ignores the long-wave radiation and the direct-indirect effects of volcanic aerosols) or aerosols in the stratosphere (which levels?) or changes in the optical depth (in which levels?)? Is there a latitudinal dependence? Do the volcanic eruptions start in January or June for eruptions where the exact date is missing?

- SOI index definition (page 2067): The Southern Oscillation is the “atmospheric” part of ENSO, so it is a bit misleading to use the leading EOF of SST and call this SOI

C1126

index. Classical it is defined as pressure difference between Darwin and Tahiti. So I suggest to use a different name maybe Nino-index or just leading mode of SST in the tropical Pacific. Given the fact that it is highly related to the NINO3 index – why not use this index, which is widely accepted.

- Page 2067: What is the advantage to use SPI and not just precipitation? By the way the SPI was introduced by McKee et al. (1993), so please mention this. Sienz et al. only applied it to model simulations.

- The authors show anomaly patterns but they do not describe how they obtain the anomalies. In the literature a common approach is to calculate the anomalies with respect to the mean of some years (3-5) before an eruption. Is it here the same – if so how do the authors handle serial clustered events?

- A significance test is missing in all figures, which is important in particular for figures 2, 3 and 8 (not sure if this is possible for Fig. 8). I suggest to use a non-parametric test, e.g. Mann-Whitney test (see Wilks).

- To separate the ENSO response from the combined ENSO volcanic response the authors select corresponding years with ENSO neutral phases or years without preceding eruptions. There are also other methods available, e.g. to estimate the ENSO response in CTRL simulations without any volcanic and solar forcing, or to use statistical approaches to remove e.g. ENSO variability (Compo and Sardeshmukh 2010, J Climate). So why not use such methods? At least the authors should mention that there are other possibilities and they should discuss the advantages and disadvantages of the selected approach.

4. The authors ignore the solar influence, which might be important during the Dalton Minimum and therefore for the Tambora analysis.

5. The analysis of the Tambora eruption is based on 5 ensemble members which are split into neutral, El Nino, La Nino and optimal. So only cases are shown including

C1127

the full internal variability. So how robust are the results if one would have a large number of ensemble members which allow to average over more cases (and reduced the internal noise)? I know that it is probably impossible to integrate more ensemble members, but maybe the authors could focus more on the volcanic impact on China in general and strongly reduce the discussion of the Tambora eruption or search for other similar events in the last millennium and average over those. This would, however, also lead to a reorganization of the paper including the title.

Specific comments

1. Page 2062, line 6-7: As ENSO has a rather regular periodicity of probably 2.5 years this sentence sounds wired. Maybe the authors mean that in the Year of the eruption, ENSO is in a “neutral” phase.
2. Page 2062, 9-12: The timescales might also depend on the selection of the volcanic eruption, please clarify that only the estimates refer to the strongest 22(?) eruptions.
3. Page 2063, 20-23: Not only the ocean but also a Carbon cycle feedback could contribute to a recovery on decadal time scales (see e.g., Brovkin et al.)
4. Page 2063, line 24,25: This sentence sounds awkward. It is the temperature which could not recover?
5. Page 2063, line 26: Please include some references of the “few results which are available.
6. Page 2064, line 5-6: Please be more specific of how the combined signal differs from the linear combination?
7. The structure of the introduction is not clear, I suggest first giving a review of the studies, showing the impact of volcanic eruptions then the studies which investigate the ENSO imprint on China and then the combination of both. At the moment everything seems to be mixed.

C1128

8. Page 2065, line 20: Typo: simulation → simulations.
9. Page 2065, line 23: I am puzzled about the reference. Goosse et al. use a model of intermediate complexity, i.e., a quasi-geostrophic atmospheric model, so how could such a model serve for comparison with respect to ENSO events? Maybe I misunderstood the sentence.
10. Page 2066, line 23,24: This is somehow misleading, I think the authors mean that the short-wave incoming radiation is reduced.
11. Page 2068, line 10: Significant using a t-test? Which significance level or confidence interval?
12. Section 3.1.2: The analysis of the recovery time is interesting, still I do not see the connection to the linearity/nonlinearity of the combined/separated responses of the climate system. Maybe a more detailed discussion of this could improve the paper.
13. Page 2069, line 15-18: Maybe I am wrong but in the North Atlantic we see long relaxation time scales. This is one of the areas with deep water formation and therefore a rapid mixing so I think this is in contradiction to the authors statement. Nevertheless, Fig. 5 is interesting. What is the reason for the relaxation long time scales in the Arctic – the snow albedo feedback? There are white areas where no time scale could be estimated – What was the criterion for this?
14. Fraedrich and Blender (2003) are not listed in the references. I have not checked all reference, but it shows that the authors hardcoded the reference list. I encourage the authors to use endnotes (word) or bibtex (latex) to avoid such mistakes.
15. Table 1, content: Typo: Helka → Hekla
16. Fig 1: I think the 30-yr running mean is not needed as it is not used/discussed in the MS. The low-frequency behavior is obvious from the times series.