Final Author Comments cp-2011-70

"Volcanic and ENSO effects in China in simulations and reconstructions: Tambora eruption 1815" by D. Zhang et al.

Dear Editor

Thank you for the final response on the discussion of our paper. We acknowledge the helpful comments of two anonymous referees.

In this file we describe the extensions and the alterations of our submitted paper based on the comments begin with a brief summary of the major changes.

Summary of changes

The substantial changes in a revised version of the manuscript are:

(i) Revision of the cited literature and re-organization of the introduction (comments by Ref-I and Ref-II).

(ii) A new figure showing the global temperature response to volcanic eruptions in the following winter (Ref-I-2.1).

(iii) A new figure showing the global response of temperature and SPI to volcanic eruptions in the year after eruptions (Ref-I-2.1).

(iv) A new figure with reconstructions of temperature (Mann et al., 2009) and PDSI in Asia (Cook et al., 2010) after volcanic eruptions (Ref-II-7).

The inclusion of these figures required re-organization of the Results (Note that the numbers of the figure will change):

3 Global volcanic impacts
3.1 Temperature and SPI anomalies
3.2 Winter warming
3.3 Recovery time scales

4 Volcanic and ENSO impacts in East Asia
4.1 Volcanic impacts without ENSO events
4.2 ENSO impact without preceding eruptions
4.3 Volcanic impacts with ENSO events

5 Tambora eruption in 1815

(v) Revision of Fig. 6 (including further indices (Mann et al., 2009 and Cook et al., 2010), Ref-II-7) and Fig. 7a (further El Nino reconstructions by McGregor et al. (2010) and Mann et al. (2009), Ref-I-1.4 and Ref-II-7)

Detailed comments on the changes suggested by the two referee reports

Anonymous Referee #1

Major comments

1. There is a lack of important references:

Ref-I-1.1

- 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.

Proposed changes:

We cite Fischer et al. (2007) on Page 2062, line 26, and on page 2063, line 4:

We cite Schneider et al. (2009) on Page 2063, lines 2, and 4, and add 'which is also found by Schneider et al. (2009)' on line 12.

We cite Shindell et al. (2003) on Page 2063, line 4:

We cite Shindell et al. (2004) on Page 2063, line 2:

We cite Yoshimira et al. (2005) on Page 2063, line 8 and add on page 2063, line 12: 'The high variability of the climate response on regional scales requires ensemble simulations of GCMs (Shindell et al., 2003; Yoshimira et al., 2005).'

Ref-I-1.2

- 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.

Proposed change:

We include the citation of Brovkin et al. on Page 2063 line 21-22:

'... which can be attributed to the ocean heat uptake in a coupled ensemble experiment (Stenchikov et al., 2009) or to the carbon cycle feedback (Brovkin et al., 2010).'

We include on Page 2066, line 29:

'This eruption led to a temperature drop of 1 K recovering with a relaxation time of 10 years (Brovkin et al., 2010).'

Ref-I-1.3

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

Proposed change:

We cite Robock (2000) on Page 2062, line 25 and on Page 2063, line 4:

We add on Page 2063, line 13:

'Large volcanic eruptions inject sulphur gases into the stratosphere, which convert to sulphate aerosols with an e-folding residence time of about 1 year. The resulting disturbance to the Earth's radiation balance affects surface temperatures as well as the atmospheric circulation (Robock, 2000).'

Ref-I-1.4

- 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 simulations with proxy evidence.

Proposed change:

The ENSO time series published by McGregor et al. (2010) and Mann et al. (2009) are included in Figure 7a (extended in revision). In the new version of figures 7 and 8 the term 'op'timal' will be replaced by 'Quinn'.

We add on Page 2072, line 8:

'In the respective time period the reconstruction by McGregor et al., (2010) deviates from Quinn et al. (1993) representing an El Nino in 1815/16. The data of Mann et al. (2009) does not contradict Quinn's data.'

Note:

The data published by Cobb et al. (2003) does not include information with the necessary resolution. The data of Mann et al. (2009) does not contradict the Quinn et al. data. In the respective time period the reconstruction by McGregor et al. deviates from Quinn et al. and represent an El Nino in 1815/16. Although we favor the Quinn et al. reconstruction, since it is widely accepted, we consider both situations.

Ref-I-1.5

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

Proposed changes:

We cite Mann et al. 2009 (see Ref-I-1.4).

Furthermore, we add a new figure on the spatial reconstruction of the temperature response to volcanic eruption using the data by Mann et al. (2009), see Ref-II-7.

Ref-I-1.6

- 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.

Proposed change:

We cite Mann et al. (2005) on Page 2064, line 1 (see Ref-II-3).

2. Model evaluation:

Ref-I-2.1

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.

Proposed change:

We include a figure showing the global temperature and SPI responses after volcanic eruptions in Section 3.1 following Figure 1.

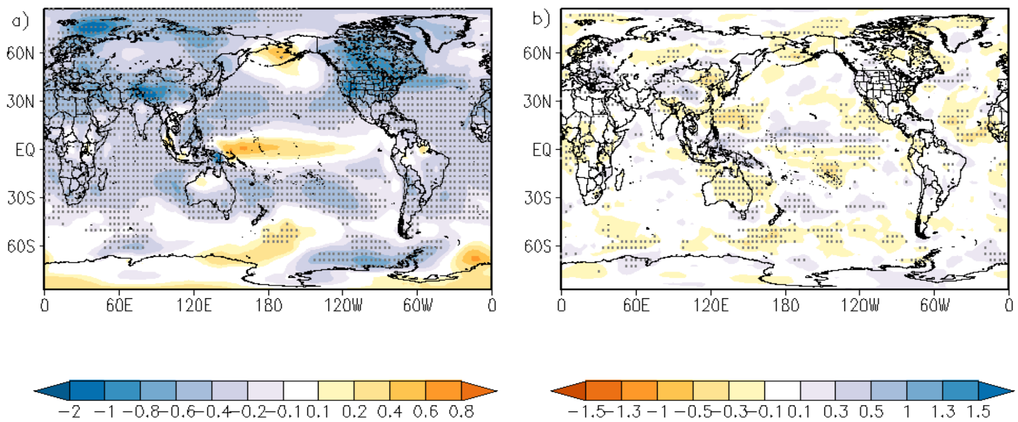

Figure (a) Temperature anomaly and (b) SPI during the year after eruptions in ensemble E1.

Ref-I-2.2

Moreover it is necessary to show the ENSO variability - I guess that the model has a rather regular ENSO mode and a double ITCZ.

Proposed change:

We add a new paragraph in Section 5, on Page 2075, line 16:

'Please note that although the pattern of the precipitation response to ENSO is captured in all ECHAM5 simulations (Hagemann et al. 2006), one of the model weakness is the regularity of ENSO (3-4 years) while observations indicate a more broadband variability of 2-7 years (Guilyardi et al., 2009). Meanwhile, as a rather common feature in IPCC AR4 models (Guilyardi et al., 2009, BAMS), our simulation expresses preferably Central Pacific ENSO anomalies.'

Ref-I-2.3

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.

Proposed changes:

We add on Page 2071, 3:

'Numerous studies have demonstrated that the Indian monsoon rainfall tends to be below (above) normal in the developing phase of a warm (cold) ENSO event (Ropelewski and Halpert 1987; Sperber and Palmer 1996; Krishna Kumar et al. 1999).). High correlations between ENSO and monsoon variability are found in observations (Webster and Yang, (1992), Arpe et al., (1998), and in GCMs (Sperber and Palmer, 1996). As for ECHAM5 model, Hagemann et al. (2006) find the all-India rainfall in La Nina year (1988) is generally higher that in the El Nino year (1987).'.

We add on Page 2069, 22:

'This anti-symmetry is consistent with Hagemann et al. (2006), supporting the view that that interannual variations of precipitation at lower latitudes are largely driven by SST anomalies in the equatorial Pacific.' The following sentence is moved to the beginning of the paragraph (Page 2069, 20).

Ref-I-2.4

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.

Proposed changes:

We add a new subsection including the winter warming (see the Summary above, note that the figure numbers will change):

' 3.2 Winter warming

Stechikov et al. (2006) observe warming in Eurasia during the following two winters which is weaker in models. In our simulations, warming in the following winter is found in the Arctic Ocean, the tropical pacific, the Bering Sea, and the Southern Pacific Ocean in 180W-120W in both ensemble means (see the Figures below). In Asia, warming is detected in Southeast China, Thailand and Malaysia in E1. For the strong solar forcing in E2, the warming extends to Siberia and Central Asia. In the second winter, the warming decreases in E1 but remains strong in Arctic Ocean in E2 (not shown). Within the ensemble members the warming amplitude differs.'

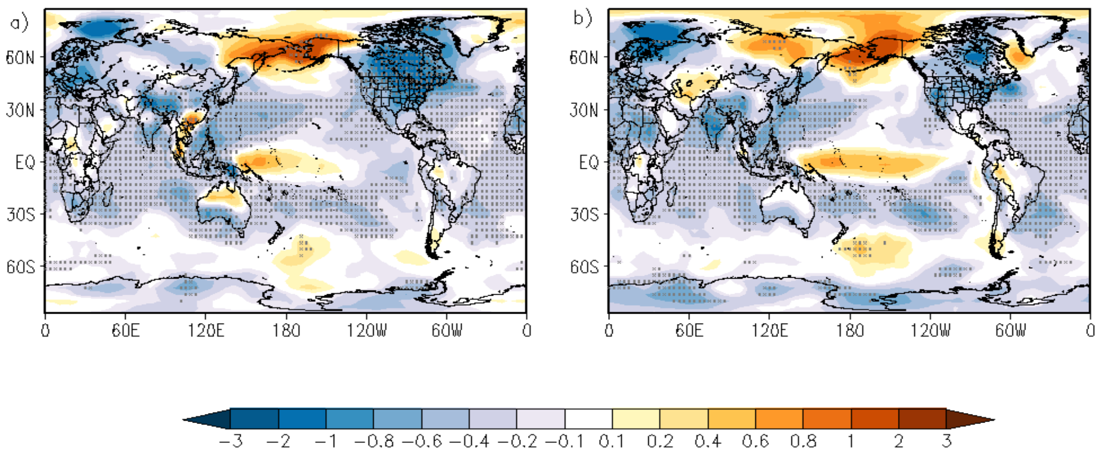

Figure Temperature response during the first winter after volcanic eruptions in ensemble (a) E1 and (b) E2.

3. Method description:

Ref-I-3.1

- 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 ad the direct-indirect effects of volcanic aerosols) or aerosols in the stratosphere (which levels?) or changes in the optical depth (in which levels?)? Is here a latitudinal dependence? Do the volcanic eruptions start in January or June for eruptions where the exact date is missing?

Proposed change:

We include on Page 2066, line 21:

'The volcanic eruptions start in January if the exact date is missing.'

Note:

These questions are answered in the manuscript on Page 2066, lines 18-21:

'The volcanic forcing is represented in terms of the aerosol optical depth and the effective radius distribution (Crowley, 2008). In the simulation (Jungclaus et al., 2010) this forcing is resolved in four latitude bands (30–90◦ N, 0–30◦ N, 0–30◦ S, 30–90◦ S) with a temporal resolution of 10 days (Fig. 1b, c).'

Ref-I-3.2

- 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 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.

Proposed changes:

We alter 'SOI' and use 'PC1-SST'.

On Page 2067, line 6, 'ENSO is characterized by the Southern Oscillation Index (SOI), given by the principal component time series (PC1) of the first EOF of the tropical Pacific sea surface temperature (SST) variability in winter (DJF) (see Hoerling et al., 2001)' is changed to 'ENSO is defined by the principal component time series (PC1) of the first EOF of the tropical Pacific sea surface temperature (SST) variability in winter (DJF), i.e. PC1-SST (see Hoerling et al., 2001).'

On Page 2074, line 9, 'characterized by the Southern Oscillation Index (SOI),' is deleted.

Ref-I-3.3

- 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.

Proposed change:

We change on page 2067, lines 13-15:

'In the present publication precipitation anomalies are analyzed in terms of the monthly Standardized Precipitation Index (SPI, McKee et al., 1993) averaged for summer (JJA), see Bordi et al., (2004) and Sienz et al. (2007).'

The additional reference is:

Bordi, I., K. Fraedrich, Jian-Min Jiang, and A. Sutera, 2004: Spatio-temporal variability of dry and wet periods in eastern China. *Theor. Appl. Climatol*., 79, 81-91, DOI 10.1007/s00704-004-0053-8.

Notes:

The monthly SPI is an indicator for meteorological droughts. We choose the SPI since it has some advantages compared to precipitation: the SPI is less sensitive to deficiencies in the absolute rainfall amount and extracts the climatological relevant variability. Different regions can be compared objectively, especially in China where precipitation varies more than 3000mm from Southeast to Northwest.

Note that the WMO suggests that the SPI should be used to characterize meteorological droughts. (Copenhagen, October 2009, http://www.wmo.int/pages/mediacentre/press_releases/pr_872_en.html).

Furthermore, since referee #2 (Ref-II-7) suggests the comparison of our results with the PDSI (Palmer drought severity index) reconstructions (in a new figure) we like to describe precipitation variability in terms of the SPI.

Ref-I-3.4

- 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?

Proposed change:

We add on Page 2067, line 15:

'All anomalies are with respect to the time mean obtained for the ensemble member. '

Ref-I-3.5

- 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).

Proposed change:

The Mann-Whitney test is included in figures 2 and 3 with 95% significance marked by a cross. In figure 8, significance is given if the anomalies are larger than twice the standard deviation.

Ref-I-3.6

- 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.

Proposed change:

For a detailed method description, we add on Page 2067, 16:

'The problem of removing ENSO-related variations from climate records has been addressed in many previous studies using a variety of methods (for example Compo and Sardeshmukh, 2010; Penland and Matrosova, 2006). Here the two ensemble simulations with 21 volcanic eruptions in each member enable the separation of intense eruptions with different ENSO responses. In order to assess both impacts of eruptions and ENSO events separately, a simple and direct method is used to avoid years with overlaps: First the response of surface temperature and precipitation/SPI during the year after an eruption for neutral PC1-SST (no ENSO events) during two preceding winters is considered. Secondly, the ENSO impact in East Asia is determined during years without preceding eruptions. The final analysis considers years with concurrent eruptions and ENSO events. All the surface temperature and precipitation/SPI are considered at year one relative to volcanic event years if not indicated otherwise.'

Therefore, lines 17-24 on Page 2068 are deleted to avoid repetition.

Note:

In our analysis we use a standard approach that is applicable to derive the ENSO response and the volcanic response. The motivation is to use a simple method which can be easily applied to other annual data. A control simulation is available which is fixed, however, to the climate in 800AD, therefore we decided to analyze the ENSO impact within the ensemble to avoid possible inconsistencies.

4. Solar influence

Ref-I-4.1

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

Note:

The solar influence and the eruptions cannot be separated in the TSI here. In this analysis we assume a time scale separation between the slowly varying solar influence (decadal time scale) and volcanic eruptions (inter-annual). Therefore, in the analysis of the volcanic impacts (not only for the Tambora), we suppose a constant solar forcing during the period after the eruptions. In the analysis of the simulation and in the comparison with documentary sources we consider short term influences (time scale of a few years).

5. Tambora eruption

Ref-I-5.1

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 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.

Proposed change:

We add on Page 2075, line 15 ‘However, the results are constrained to only one optimal simulation. A large number of ensemble members which fall into the optimal selection of ENSO would help reduce the internal noise and thus allow better comparison with reconstructions.’.

Note:

Clearly, it would be helpful to have a larger ensemble to increase significance. On the other hand the intensity of the Tambora eruption suggests that the impact exceeds internal variability. Fortunately, a small ensemble with different ENSO states is available for comparison with documented ENSO variability. Therefore, in the light of the relevance of this eruption in climatology we decided to analyze it in more detail. From our point of view the extraction of a particular ensemble member using historic El Nino variability and the comparison with documented climatological information in China is a central part of our analysis which could be useful for further research.

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.

   Proposed change:

   The sentence is changed to ‘Volcanoes, which are the dominant forcing in both ensembles, cause a dramatic cooling in West China (–2°C) and a drought in East China at the year one after the eruption for neutral PC1-SST during two preceding winters.’

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.

   Proposed change:

   ’The recovery times for the volcano induced cooling’ will be replaced by ’The recovery times for the volcano induced (31 major eruptions averaged in 5 members) cooling’

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.)

   Proposed change:

   We cite Brovkin et al. (2010), see Ref-I-1.2

4. Page 2063, line 24, 25: This sentence sounds awkward. It is the temperature which could not recover?

Proposed change:

The sentence is slightly changed:

'During the Dalton minimum (1790-1830) at the end of the Little Ice Age a series of volcanic eruptions occurred which lowered temperature persistently since the oceanic mixed layer could not recover from the previous eruption (Crowley et al., 2008; Cole-Dai et al., 2009)'.

Note:

Our text is an abbreviated version of Crowley et al. (2008):

'These closely spaced eruptions are not only large but have a temporally extended effect on climate, due to the fact that they reoccur within the 10-year recovery timescale of the ocean mixed layer; i.e., the ocean has not recovered from the first eruption so the second eruption drives the temperatures to an even lower state.'

5. Page 2063, line 26: Please include some references of the "few results which are available.

   Proposed change:

   We change the sentence:

   'Brovkin et al. (2010) detect a relaxation time of 10 years of global temperature to the large eruption in 1258 using a complex coupled atmosphere-ocean-general-circulation model (AOGCMs). However, the global spatial pattern of time scales of climate in AOGCMs and its casual factors behind are still vacant'.

6. Page 2064, line 5-6: Please be more specific of how the combined signal differs from the linear combination?

   Proposed change:

   We replace the sentence on Page 2064, line 5-6:

   'The combined signal is greater in magnitude than that suggested from a simple linear combination (Kirchner and Graf, 1995).'.

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.

   Proposed change:

   We reorganize the introduction: We combine paragraphs 1 and 2 as introduction to volcanoes; paragraph 3 remains as introduction for time scale; we combine paragraphs 4 and 5 as volcano-ENSO (Ref-II-3), and paragraph 6 for Tambora.

8. Page 2065, line 20: Typo: simulation –> simulations.

   changed.

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.

   Proposed change:

   The reference is too short, we extend it:

'As the eruptions in the ESM ensemble simulation concurred with different simulated ENSO states, a selection of an optimal combination is possible by comparing the model results with documented El Nino events (the selection of optimal ensemble members has been suggested by Goosse et al., 2006).'

10. Page 2066, line 23,24: This is somehow misleading, I think the authors mean that the short-wave incoming radiation is reduced.

    Note:

    Here the solar irradiance means the radiation that equals Total Solar Irradiance (TSI) minus the reduction caused by aerosol optical depth and the effective radius. The method is also used in Peng et al. (2010). See comment Ref-I-3.1 (the response is already included in the manuscript).

11. Page 2068, line 10: Significant using a t-test? Which significance level or confidence interval?

    Proposed change:

    The significance level is 95% for temperature (standard deviation times 1.98), hence 'at 95% level' is added on Page 2068, line 10.

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.

    Note:

    The concept of the time scale is not related to linearity (or nonlinearity) of combined responses. There is no such remark in Section 3.1 (recovery time scales). There is a remark on a finding on the nonlinearity of the combined volcano/ENSO signal in the Introduction (Page 2064, line 5) found by Kirchner and Graf (1995). Our results indicate weak nonlinearity (Section 3.3, Page 2070, lines 25 etc.).

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 Artic – the snow albedo feedback? There are white areas where no time scale could be estimated – What was the criterion for this?

    Proposed changes:

    We change on page 2069, lines 1-3:

    'The global distribution of the fitted decay amplitudes ΔT_0 and the relaxation timescales τ are included in Fig. 5a and b at grid points where fits can be achieved (fits require a negative amplitude and a sufficient decay of the anomalies). Ensemble E2 reveals similar results (not shown).'

    We change the sentence on page 2069, line 15 to explain the void area in the North Atlantic we:

    'A possible physical mechanism for small amplitudes on the ocean is rapid mixing in the surface layer (for example in the North Atlantic where the small amplitides inhibit fits).'

    Notes:

    White areas are explained in lines 7-9:

‘Areas with negligible amplitudes inhibit fits (void in Fig. 5a, b); furthermore areas with weak positive amplitudes (mostly in the North Atlantic, the tropical Pacific, the Bering Sea, and in parts of the Southern Pacific) are excluded.’

The snow albedo feedback is a possible mechanism for long relaxation times; furthermore, soil wetness and permafrost may induce long memory. Since the snow cover on the Himalaya reveals short time scales (see Fig. 5a,b) the interpretation of long relaxation time scale as a snow-albedo feedback is unclear.

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.

    added.

15. Table 1, content: Typo: Helka –> Hekla

    changed to ‘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.

    deleted

Anonymous Referee #2

Ref-II-1

I share many of the first reviewer's concerns about the manuscript. Discussion in this manuscript of the literature dealing with model and data studies is incomplete. In particular, papers like Fischer et al. (2007) , Schneider et al. (2009), Shindell et al. (2003), Brovkin et al. (2010) and others really need to be discussed in terms of, at the very least, the similarities and difference in the modeled or observed anomaly sign, seasonality, and spatial expression associated with strong tropical eruptions, if not Tambora specifically.

Proposed changes:

These four papers will be included, please see our comments on the referee report #1 (see Ref-I-1.1 and Ref-I-2.)

Ref-II-2

There is also the study by Robock et al. (2008) that includes simulation of tropical atmospheric sulfate injection.

Proposed changes:

We cite Robock et al. (2008) on Page 2062, line 26.

We include on Page 2063, line 13:

'Large volcanic eruptions inject sulfur gases into the stratosphere, which convert to sulfate aerosols with an e-folding time of 1 year; the resulting disturbances of the radiation balance affect surface temperature and the atmospheric circulation (Robock et al., 2000; Robock et al., 2008).'

Ref-II-3

The Adams et al. (2003) result should really be discussed in the context of the Mann et al. (2005) result, as well as the findings of D'Arrigo et al. (2009) and the recent paper by McGregor and Timmermann (2010).

Proposed changes:

We change the two paragraphs starting on page 2063, line 28:

'Volcanic eruptions and ENSO events can influence the climate on similar time scales and with comparable magnitudes. The combined signal is greater in magnitude than that suggested from a simple linear combination (Kirchner and Graf, 1995). Furthermore an increase of the probability of El Niño events after volcanic eruptions is detected in reconstructions (Adams et al. 2003; McGregor et al. 2010), although an association between low-latitude volcanic events and lower SST in the tropical oceans is also found (D'Arrigo et al. 2009). The enhancement of the probability of El Ninos after eruptions is supported in simulations with the Zebiak-Cane model (Mann et al. 2005; Emile-Geay et al. 2008). Such impact is highly relevant for Southeast Asia since the El Niño/Southern Oscillation (ENSO) is related to drought during El Niño and wetness during La Niña phases, due to that both phenomena can lead to either a partial cancellation or to an enhancement with even more disastrous consequences. Since reliable reconstructions of past El Niño events are restricted to the last three centuries (Quinn et al. 1993), AOGCM simulations are necessary to retrieve reliable correlations and possible causal relationships.'

Ref-II-4

In some places the summary of the existing literature in the manuscript is also inaccurate. For instance, [Page 2064 Line 24] D'Arrigo et al. (2009) is a composite zonal study and doesn't really allow for a statement to be made about the global extent of a single eruption.

Proposed changes:

We change on Page 2064, 24, 'In agreement with other volcanoes located in the tropics, the Tambora impact was global (D'Arrigo et al., 2009)' to 'As a major eruption located in the tropics (D'Arrigo et al., 2009), Tambora impact was global (Robock, 2000).'

Ref-II-5

Similarly, the study of Anchukaitis et al. (2010), [Page 2064 Line 16] looks at reconstructed Asian moisture conditions only and two model simulations, but doesn't make the kind of sweeping statement that authors ascribe to that paper.

Proposed changes:

We change on Page 2064, Line 16, using the exact quotation:

'However, using a comparison with reconstructions Anchukaitis et al. (2010) suggest that GCMs may not yet capture all of the important ocean-atmosphere dynamics responsible for the influence of explosive volcanism on the climate of Asia.'.

Ref-II-6

Similar to the first reviewer, I note several problems with references (for instance on Page 2064 I note the misspelling of 'Emile-Geay').

Changed

Ref-II-7

More problematic, the authors miss a critically important opportunity to compare their model simulations to reconstructions of the actual climate anomalies following the Tambora eruption. They do make some limited comparison to single timeseries (index) reconstruction of temperatures, but since the paper deals with (and shows in figures) the spatial patterns of both precipitation and temperature anomalies associated with different influences, the lack of any comparison to (reconstructed) gridded observations is glaring. Although spatiotemporal reconstructions of climate are subject to their own uncertainties, model-data comparisons anchor the GCM simulations in the real world. For temperature comparisons, the authors should make use of recent reconstructions of temperatures by Mann and coauthors (2008,2009), I think. Also, the authors make minimal comparisons to reconstructed estimates of precipitation (their citation to Garnaut 2010 for instance is a personal communication and not a published paper). The authors also cite Zheng for the 'Dry/Wet Index', which has been used in multiple studies (Qian et al. 2003, Bordi et al. 2004, Shen et al. 2007), but unfortunately don't show any of these data. In the conclusion, the authors note that 'This simulation caused ... moderate wetness in south China and extreme drought in the North and the Northeast' but then don't provide a comparison to even the DWI data. While the SPI is a reasonable and useful metric, it provides only a limited chance for comparison against reconstructed precipitation or drought fields. For instance, the authors could have calculated the Palmer Drought Severity Index and compared to the gridded PDSI reconstruction that Anchukaitis et al. (2010) used (from Cook et al. 2010). So, a direct spatial comparison between (1) the model simulated temperature field, (2) a derived PDSI field, and the reconstructions by (3) Mann and (4) Cook would provide this paper with a very necessary anchor to (paleoclimate) observations. This may also prove an alternative, complementary way to infer which of the forcing combinations might actually have happened.

Proposed changes:

Spatial Coverage of reconstruction Figure:

We include a further figure (following Fig. 2) showing spatial patterns of reconstructed temperature and PDSI anomalies at year one after volcanic eruptions for a) without ENSO events and b) with coinciding ENSO events. The reconstructed temperature is from Mann et al. (2009) and the PDSI is from Cook et al. (2010). The list used for volcanic eruptions is according to Amman and Naveau (2003); the event years with reconstructed El Nino are according to Gergis and Fowler (2006). The results from two further lists (see table in Anchukaitis et al., 2010) are similar. These lists will be included and discussed in the revised paper.

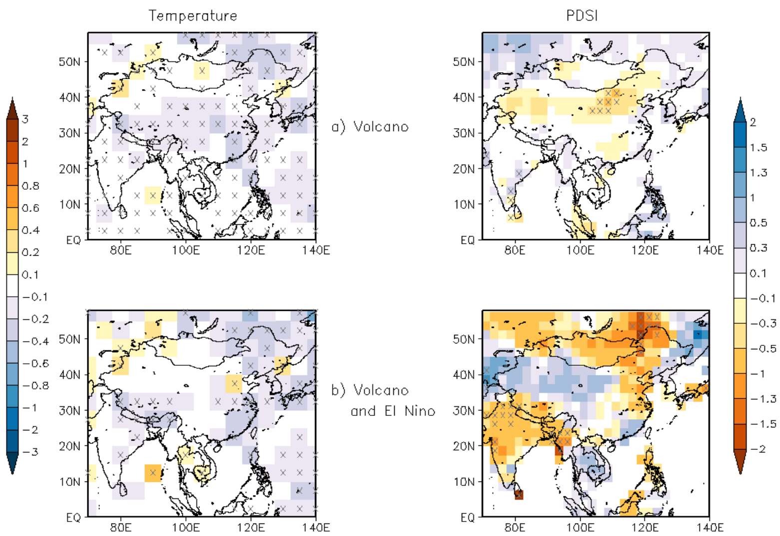

Figure Reconstructed temperature (Mann et al. 2009) and PDSI (Palmer Drought Severity Index, Cook et al., 2010) after (a) volcanic eruptions without ENSO events, (b) volcanic eruptions with El Nino events in the following winter; 95% significant anomalies marked (x).

In Fig. 6 we add the time series of reconstructed temperature from Mann et al. (2009) and PDSI from Cook et al. (2010).

Dry/wet indices:

Unfortunately, the data of Qian et al. 2003 and Shen et al. 2007 are not available to us. According to Qian et al. (2003), the data from Central Meteorological Bureau has the largest spatial coverage with 120 sites, this data has been attributed to Garnaut in our references (see minor changes at the end). The analysis of Bordi et al. (2004) considers instrumental data (for the last 50 years).

PDSI:

According to Zhai et al., (2010, J. Climate), both PDSI and SPI indices can be used to describe the tendency of dryness and wetness severity and for comparison in climate impact assessment in China, while in individual river basins SPI is able to explain the spatial distribution of dry or wet severity. Sims et al., (2002) suggest SPI to be more representative of short-term precipitation than PDSI. Therefore, we use SPI in our analysis.

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The authors could also, since they consider the SOI as their ENSO metric, look at the reconstruction of the SOI by Stahle and coauthors from 1998.

Note:

We use the first principal component of an EOF analysis of the SST as SOI metric. Therefore, 'SOI' is changed to 'PC1-SST'. We do not use an atmospheric SOI as reconstructed by Stahle et al. 1998 For ENSO reconstruction we apply Quinn et al. (1993), Mann et al. (2009), and McGregor et al. (2010), see also the responses to the comment of referee #1, Ref-I-3.2 and Ref-I-1.4.

Minor

According to the recent contacts, reference Garnaut (2011) is changed to Central Meteorological Bureau: Yearly charts of dryness/wetness in China for the last 500 years (in Chinese). Cartographic Publishing House, Beijing, China, 1981.