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Interactive Comment

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

Anonymous Referee #2

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The manuscript presents a clever use of the COSMOS millennium ensemble and the different forcings applied to the different members. This approach certainly provides improved opportunities to diagnose the influences (and potential interactions) of different internal and external influences on large-scale and regional climate.

However, 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

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sign, seasonality, and spatial expression associated with strong tropical eruptions, if not Tambora specifically. There is also the study by Robock et al. (2008) that includes simulation of tropical atmospheric sulfate injection. 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). 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. 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. Similar to the first reviewer, I note several problems with references (for instance on Page 2064 I note the misspelling of 'Emile-Geay').

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. 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.

Interactive comment on Clim. Past Discuss., 7, 2061, 2011.