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Interactive Comment

Interactive comment on “The influence of tropical volcanic eruptions on the climate of South America during the last millennium” *by* C. M. Colose et al.

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This paper assesses the effects of large volcanic eruptions on temperature, precipitation and water isotopic composition in tropical South America. As such, it is a most welcome contribution to the literature. Knowledge gaps about the climatic response to volcanic events are still considerable, particularly in the Southern Hemisphere. Similarly, information about past millennium climate variability in South America (and many other regions of the globe) is still sparse, and quantification is challenging, particularly for hydroclimate. In tropical South America, most high-resolution climate proxies are water isotope records and therefore understanding the climatic drivers behind the datasets is crucial. This paper is an important contribution for improving our understanding of these processes and I enjoyed reading it.

I strongly support publication of this article in CP. At the same time, I do have some suggestions for improvement of the manuscript, most of them are not critical though. Please understand all my comments as constructive criticism.

Major points:

1. Capability of the model to simulate oxygen isotopes in precipitation over South America. The conclusions of the papers stand and fall with the ability of the model to simulate oxygen isotopes in precipitation (δ18Op) in general, not only in response to volcanic events. This is assessed in Figure 6. While the model appears to be quite good in simulating the seasonal cycle, I am not sure whether this analysis is sufficient to be confident about the skill of the model. I am not an expert in this field but I could think of the following options:

Literature: It is possible that this has been assessed the literature describing GISS ModelE2-R. If this is the case I suggest including a paragraph reviewing this.

GNIP data and volcanic events: The authors state that data availability is not sufficient to perform a reasonable composite analysis for the volcanic events. Is there not sufficient data available to at least show the response to, let's say, the most recent event (Pinatubo)? If this is not the case, which I suspect from reading page 3386, would there be a chance to analyze this based on composites from other years? For example, one could make δ18Op composites for the warmest/coldest/driest/wettest years during the period of reasonable data coverage and compare to the model data. This would require composites of reasonable size, so given that I don't know about the exact data situation in the GNIP data, I cannot be sure that this is feasible. But such an analysis would also be helpful to interpret the paleo results (see next point).

2. Similarly to the last point, it would be good to simply see how the modeled δ18Op

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responses to climate in the study area (not only during volcanic events). This not only to assess the skill of the model, but also to better understand the results. This could be tested using the control simulation. For example, one could select years with high/low temperature (but normal precipitation) and years with high/low precipitation (and normal temperatures). How do the δ18Op anomalies look like? Most probably as expected from theory and described in the text but I think nevertheless it would be helpful to have an illustration confirming this (for example in the SM). This is relevant particularly for the explanation of the seasonal asymmetry shown in Fig. 10, which I think is one of the key findings of the paper. The interpretation provided by the authors (that the strong temperature response masks the precipitation signal in some seasons and regions) could be supported by this analysis.

3. This is a modeling study. However from the title and abstract this does not become clear and one could still think that proxy data are also used. In the abstract it says "...and allows for a direct comparison between GISS simulations and paleoclimate proxy archives". This comparison is not provided in the paper, so I suggest to clarify this (e.g. by saying "future comparisons") and move this statement to the end of the abstract (as kind of an outlook). Even after reading the introduction (with a specific section on reconstructions), one could still expect proxy data to be used somewhere in the text. Given proxy data are mentioned repeatedly in the paper, the reader can hardly wait to see how the anomalies of the proxy data look like following the LM eruptions... I suspect (and hope) that the authors plan to show this and the proxy-model comparison in a subsequent study, and this should be clarified as early as possible. The importance of this paper for such future analysis can then be stressed (again) in an outlook at the end of the manuscript. I would like to emphasize that I do not think that clarifying this in the title and/or abstract will make this paper less appealing.

4. The paper focuses on tropical South America. Although the entire continent is shown in the figures, the analysis and interpretation is clearly focused on the tropics (and maybe subtropics), which makes sense (e.g. given the distribution of isotopic

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proxy data). Again, I suggest clarifying this in title and abstract. And again I think doing so will not make the paper less attractive but help the reader to know what to expect. The authors may even consider removing the parts of the paper describing extratropical features, for example Figure 13, to make the paper more focused. Suggest to replace “South America” by “tropical South America” in many instances of the paper.

5. I suspect that the response to volcanic forcing in tropical South America in the instrumental data is not as clear as described in the text, particularly for temperature. None of the obs-panels in Fig. 4 shows consistent negative anomalies in the region except for Pinatubo in JJA, where the signal appears to be rather weak. A composite analysis could clarify this picture. While Figure 3 impressively shows the consistency in observational and model data in the tropical belt, I think an identical (or similar) figure for tropical South America would be more helpful for this paper (the current fig. 3 could be provided additionally or moved to the SM). The authors may also consider showing an instrumental composite anomaly map for South America to allow a good comparison with Figs 7 and 8. (see my other point regarding figure 4 below).

Minor points:

6. Abstract line 4: consider including “instrumental” before “observations” to clarify that proxy data are not used in this study

7. Abstract lines 5-9: This is a very long sentence. I suggest to split up.

8. P. 3377 line 8: Although this is described in more detail below, I think the statement “most important” should be accompanied with a literature reference (or “see below”).

9. P. 3377 line 17ff. Although see Zanchettin et al. (2012) for decadal-scale responses to volcanic eruptions, at least in the North Atlantic sector.

10. P. 3379 line 25. Do the authors mean “records” instead of “archives”? The number of archives offering high-resolution proxy data is not increasing that much. . .

11. P 3380 Section 1.3 does not describe the climate of the entire continent so I

suggest to change the title to “tropical”.

12. Although I somehow like the expression, “rather Mars-like” does not appear to be a very scientific description. I leave it to the editor to decide whether it is appropriate. Given the point above (and point #4), the first paragraph of this subsection could also be considered to be entirely removed.

13. P. 3381 last paragraph. To be exact, the ENSO response described here is only valid for the SAMS-affected regions. There are parts of tropical SA that have a different (reverse) response (e the Pacific coast area with strong wet anomalies during El Niño events).

14. P.3382 lines 17-21. This is a long sentence, consider splitting up.

15. P. 3383 line 5: One or more References for the amount effect would be helpful.

16. P. 3383 line 7: is there an “at” missing after “be”? Or maybe use the word “occur” instead.

17. P. 3382 line 12 and P. 3383 line 19: I think the use of the terms “Medieval Climate Anomaly” and “Little Ice Age” is generally not appropriate and precise. These terms were defined based on data from Europe and North America and there is growing evidence that there were no globally consistent and synchronous anomalies during these periods. Apart from that there is no generally accepted temporal definition of these terms, so the period referred to should always be specified. For example, the text could be changed to: “. . . detected an anomaly in the oxygen isotope composition of the ice contemporary to the ‘Little Ice Age’ (LIA, AD XXX-YYY) described from the Northern Hemisphere . . ..” And on page 3383 changing to “Medieval times (AD XXX to YYY)”.

18. P. 3389 line 2: The linear time trend is later also subtracted from the data to remove the global warming signal or why is it included in the regression?

19. P. 3390 line 7: The “cooling over much of the globe” is not really visible in the obs

panels (expectations often bias our interpretation. Therefore, I showed the graph to persons not knowing what it shows and they confirmed that it does not visibly show more blue than red). Unless it can be undermined with numbers, this statement should be removed. Potentially, the signal gets clearer if the three events are combined into a composite? This could be added as an additional panel in the bottom of the figures (see also point 5 above).

20. P. 3390 line 22: Please specify what “this” refers to.

21. P. 3392 line 8: What are the composites compared against in the t-test?

22. P. 3394 line 1: Although see Greve et al. (2014) regarding the (non-)validity of the “dry gets drier” hypothesis.

23. P. 3396, last paragraph: I think Figure 12 could be moved to the SM. I was missing confidence intervals in Figure 9. These could be inserted by shading the 95% range of the distribution from the random composites in Fig. 12. This would make Fig. 9 much stronger and the additional information in Fig. 12 would then be minor so that it could, in my perspective, be removed from the main manuscript.

24. P3397: I think section 3.2.4 could be removed. The first part, discussing the high latitude influence including Fig. 13 does not really fit in the current concept of the paper that is focused on tropical SA (see my point 4 above). While I find this topic interesting, I think the high latitude influence is discussed a bit superficially here and could be better addressed in a separate paper in sufficient detail. The last part of the paragraph starting on line 27 could be moved up to the discussion of the LM composites. I think this would also improve the reading flow of the manuscript.

25. Figure 1. Suggest to mark the eruptions that are finally used to create the composites with a different color in the top panel.

26. Figure 3: I think this Figure should contain confidence intervals, so the reader can see what magnitudes of anomalies are significant. A standard approach in su-

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perimposed epoch analysis plots is to show the 95% range of years no affected by an eruption.

27. Figure 3: The positive anomalies in instrumental precipitation between ca. 1.8 and 3.5 years after the eruption appears to be about as large as the immediate drying response. Do you think this is an artifact? Is it seen in both eruptions? Any reference to this in the literature? Again, indicating the significance threshold could help here.

28. Figures 7,8,10: Include "anomalies" to the color bar caption. I think it is worth mentioning in the caption that only significant results are shown (at least that's how I understand it from reading page 3392)

29. Figure 9. This figure should also include a significance threshold and this could be taken from Fig. 12 as mentioned above (point 23).

30. Figure 12: The blue colors are hardly visible and somehow masked by black in the print version of the manuscript.

References:

Greve P, Orlowsky B, Mueller B, Sheffield J, Reichstein M, Seneviratne SI (2014) Global assessment of trends in wetting and drying over land. Nature Geoscience, 7, 716–721.

Zanchettin D, Timmreck C, Graf H-F et al. (2012) Bi-decadal variability excited in the coupled ocean–atmosphere system by strong tropical volcanic eruptions. Climate Dynamics, 39, 419–444.

Interactive comment on Clim. Past Discuss., 11, 3375, 2015.