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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.

Anonymous Referee #2

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The paper by Colose and co-authors aims at evaluating the impact of volcanic natural external forcing on the South American Monsoon and on isotopic composition of precipitation (δ18Op) over the last millennium.

To address this issue they analyzed instrumental observations covering the last 3 major tropical volcanic eruptions and compared the results to coupled model simulations for the last millennium that explicitly tracks stable water isotopes.

To characterize the volcanic response over the instrumental period the authors relied on composites of observations for the last 3 or 2 most recent volcanic eruptions depending on the variable considered. They used the same superimposed epoch anal-

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yses for model simulations over the historical and last millennium periods. They explored temperature and precipitation response a few years after the eruptions in both model and observations and concluded that the most robust response in both cases concerned the immediate tropical cooling, while the precipitation changes were more noisy, especially during the summer monsoon season (DJF) probably because of the concomitant El Nino events in most cases, at least during the instrumental period, where the number of event is too small (2 or 3 events), thereby reducing the signal to noise ratio.

Based on these analyses and the depleted signal in isotopic composition of precipitation (δ18Op) over most tropical land of South America in the aftermath of historical and last millennium eruptions, the authors state that δ18Op response during the first austral winter after the eruption is due to the strong tropical cooling. The paper is well written and such study is needed to improve our understanding of the influence of volcanic eruptions on climate and on isotopic compositions of precipitation, which is crucial to help interpret signal in natural archives. However I have strong concerns on the methods used and on the model appropriateness to address this issue. The authors need to significantly improve the analyses, as there are many important points to clarify or to be corrected before publication. I've listed bellow my main comments and criticism to be addressed:

1. To start with, an evaluation of the model performance in correctly simulating the global mean TOA SW anomalies and global mean temperature anomalies needs to be shown so that to prove the model skills is correctly capturing the first order forcing and temperature response to the historical eruptions. Same remark for the South American precipitation mean seasonal cycle at least in DJF (selecting two levels of precipitation contours as on figure 6 won't just do the trick). This should be a first order sanity check for the South American Monsoon mean climatology and for the L20 eruptions of the historical periods for which observation are available. 2. My second comment concerns the method used to build the super-posed epoch and composite analysis. The authors

compute anomalies respectively to the period three years before and 5 years after each eruption for both temperature and precipitations in observations for El Chichon and Pinatubo eruptions. By doing so the authors remove part of the volcanic signal. Why choosing this period? GISTEMP anomalies are based on the 1961-1990 climatology. Did you check the consistency between the two anomalies? I'd suggest removing the 1961-1990 climatology, for precipitation and temperatures so that to avoid removing the climatology with part of the climate response to volcanic forcing. 3. A general comment for all the analyses displayed in the manuscript is the absence of statistical significance evaluation on each figure or plot. I suspect that two eruptions only, is not enough and most of the signal (which is very small) shown on the first figures is within the interval of internal variability. This needs to be evaluated with appropriate statistical methods used to extract the signal from the noise. Tropical South America temperature and precipitation interannual variability is high and the authors should discuss the results respectively to the background noise. No statistical confidence levels are shown. For example on Figure 4 and Figure 8, the colors map is built to be white between +/-0.1°C (mm.day-1). I really doubt that this is a real measure of significance applicable for the whole globe. The authors should address this matter seriously so that they can discuss in a convincing way the signal attributable to the volcanic forcing.

4. Why the authors did run only 6 model members for the L20 eruptions? How were they built? ENSO might be the dominant factor in the simulate response over South America so I would suggest to increase the ensemble size and sample initial states so that in the ensemble mean, the volcanic signal could be extracted from internal unforced variability without any bias toward any ENSO phase. As it is now, we can't really trust the model results as no discussions or diagnostics are shown concerning the appropriateness of the model ensemble to detect the volcanic forcing.

5. The model results displayed on both Figure 4 and 5 show absolutely no agreement with observations (temperatures and precipitations) while the estimated robust signal attributable to any of these volcanic eruptions is not shown (signal to noise ratio). Same

CPD

11, C1562–C1567, 2015

remark as above using a color map built to have white shade at a fixed contour is not a measure of significance. The authors can't state based on these figures that the model is able to reproduce the temperature or the precipitation responses, as the spatial patterns and amplitude are not consistent with observations. So far figure 4 and 5 suggest that the model is not able to reproduce any post-eruption signal and is not appropriate to evaluating the impact of volcanic forcing on the South American Monsoon.

6. Last paragraph of page 3387: The authors should clarify what is the mis-scaling of the Gao forcing and why for the model composites covering the L20 eruptions, it is not an issue.

7. First two paragraph page 3388: The authors state that the volcanic forcing should dominate the response in the LM composite. This is a very strong statement as different solar forcing scenarios have been used not to mention the two different land-use forcing scenarios (especially over South America) employed in the different LM members. The authors can't make such statement without providing detection-attribution analyses and other diagnostics over South America showing that the various land-use and solar irradiance forcings didn't have any impact on the post-eruption mean response (temperature and precipitations) and ensemble spread for each selected LM eruptions. Addressing this issue is not trivial and it shouldn't be overlooked. As it is, the LM composites can't be used to address specifically the volcanic response as other forcings are at play and may very well contribute significantly to the simulate response.

8. Section 3.2.1 first paragraph: Is -/+0.1°C statistically significant as shown on Figure 7 or is it again a color map choice? Does not look right owing to the high SST variability over land and ocean in these regions. I'd ask the author to verify this.

9. Page 3395, line 17-22: It is difficult to believe based on the results displayed that Âń in the case of volcanic forcing it appears that the amplitude of the temperature-response to volcanic eruptions over tropical South America is much larger than the

rather weak and spatially incoherent precipitation signal. Âż The forcing used (Gao and Crowley) for the LM simulations are well known now to have been largely overestimated as the temperature response in CMIP5 LM simulations while the good performance of the model used in this study against Pinatubo eruption (for which plenty observation are available) for the forcing and response has not been shown. Same for the South American mean climatology.

All in all, the methods (poor or absent statistics), model simulations design (i.e. mixing forcings in LM runs) and the lack of model skill evaluations in capturing the correct post eruption response over the instrumental periods strongly weakens the displayed results. For these reason I recommend major revisions before considering this work for publication.

Minor comments:

- Page 3377, line 27-28 and page 3378 line 1-2. The authors state Âń Sulfate aerosols from the Mt. Pinatubo eruption had an effective radius of up to âĹij 0.5–0.8 μm, comparable in size to a visible wavelength and strongly scattering to incoming solar radiation. Unless the particles can reach sizes larger than âĹij 1–2 μm, this scattering more than offsets the small increase in infrared opacity from the aerosols, and results in a cooling of Earth's surface (Turco et al., 1982; Lacis et al., 1992) Âż

I'd replace "of up to âĹij 0.5–0.8 μm" by "ranging between 0.2 and 0.8 with unimodal size distribution mean radius of 0.5 μm" As for the statement "larger than âĹij 1–2 μm", according to theoretical calculation (Lacis et al 1992) the LW forcing would dominate for particles larger than 2.2 μm.

-Page 3380, last paragraph: Âń The continent spans a vast meridional extent (from âĹij 10âŮę N to 55âŮę S), contains the world's largest rainforest (the Amazon), in addition to a rather Mars-like desert (Atacama) that competes only with the dry valleys of Antarctica for the driest location on Earth. Âż

CPD
11, C1562–C1567, 2015

What is a “Mars-like” desert? Not really scientifically meaningful. I'd rather give the amount of precipitation per year. As for the comparison to Antarctica for the driest location on Earth, is it proven? If yes the reference is missing.

- Methodology section: Line 14: The authors need to clearly define how the ensembles were built, in terms of forcings and initial conditions. How many members and how they differ exactly from each other? A table summarizing this is needed.

Page 3385, line 19: GPCCv6 is better and is actually what you show in the supplementary material. Please clarify.

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CPD

11, C1562–C1567, 2015

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