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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 #3

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This is the third time I have been asked to review this paper. I reviewed it twice for the Journal of Climate, and apparently it was rejected, as the authors are submitting it again to another journal. Just like the last time, there is no evidence that they addressed the comments. It seems to me that rather than acting ethically to try to produce and communicate good science, they are now submitting the paper to another journal to try to get a bad paper published.

For example, I twice told them that they have to evaluate the climate model before using it. They chose not to do this, as picked up by two reviews already submitted. This just goes to waste their time, my time, and the time of the Editor. For this unethical behavior alone, the paper should be rejected.

This also points out a big problem with the EGU review process. The paper slipped by with a very light review and is not published as a Discussion paper, with a DOI. This means that if it is rejected it will always exist in a state where it can be referenced, even though it is bad science. EGU needs to reform their editorial process, and only publish papers that have passed peer review, like AMS an AGU do.

So I am not going to waste my time once more reading in detail a paper that has not been changed to address comments. Perhaps some comments were addressed, but in response to my request to the Editor to have the authors provide a detailed response to the previous reviews, I did receive anything. Rather, I repeat below my review from the last time and attach the almost identical manuscript from the last time, with my comments there, for the use of the Editor.

Review of Colose et al. submitted to Journal of Climate

This paper should be rejected because the authors refused to address the first review I did. I mentioned the problem of isotopologues in my previous review, in the annotated manuscript, and it was completely ignored. Their response says, “Oxygen to water isotopologues: Fixed.” but it is not true. Now I am wasting my time dealing with it again.

The authors refuse to test the model with observations. In their new Fig. 2 they could have included observations as well as the model simulations, for the recent period, and shown the difference so we can evaluate the model errors. Their refusal, based on a claim that another paper also did not do this, is unacceptable. Two wrongs do not make a right. As I said in the previous review, “The first test of any climate model is its ability to simulate the current climate.” Without this, how can we evaluate the results? This is enough to invalidate the paper, but I will give other comments, since it may be that the model is OK, and they just have to show it.

On line 258, they continue to call the model “fully coupled” and again refuse to address the comment in the previous review.

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This paper has an excellent review of the impacts of volcanic eruptions on climate, but no explanation of oxygen isotopes. First of all, the definition of isotopologue includes all variations of isotopes in an atom, not just oxygen 18, so is used incorrectly on lines 26 and 106. Second, on line 107 we find δ18Op, which we are told is called "isotope" and then on line 219 we find δ18O without the "p." In neither case is this defined or explained. How is this variable calculated? What does the "p" mean? What does δ18O without the "p" mean? The paper needs a paragraph to explain this, and explain the different process that can produce the fractionation. On p. 10, the authors assume the readers understand all this, and I was already lost. Take out some of the volcano intro, which was too extensive, and introduce this topic for readers not familiar with it. And always define symbols when you use them for the first time. Later on, they discuss temperature vs. precipitation impact on δ18Op, but never explain what the mechanism is or what it means. What about readers who completely understand the impact of volcanic eruptions on climate but know nothing about isotopes? You have to write for them, too.

For all these reasons, there is no way to evaluate the new science in this paper.

In lines 593-595, they say, "a consistent description of how to interpret oxygen isotopes into a useful climate signal cannot be given without considering all of these processes and the target process of interest." But they have a climate model that includes all of this. Why don't they just do it? This is an opportunity completely lost. With a tool that enables them to track and quantify all these processes, they choose not to use it.

The choice of eruptions (Table 1) is flawed. How can an eruption start date be before the eruption occurred, for 1883 Krakatau? Why did the authors choose to evaluate up to three seasons for some eruptions, two for others, and only one for others?

The graphics have many problems, all detailed in the attached annotated manuscript, which also addresses a number of other issues. For example, there are postage stamp size images in Figs. 4, 5, 12, S4, and S5, which are too tiny to see any useful infor-

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mation, and with text that has a font so small it cannot be seen. Fig. S2 says, “AOD distribution for all 16 events,” but for what period in each event? Fig. S6 should use the same scale as Figs. S4 and S5, so they can be compared. Oxygen isotope data are given in several figures, such as Fig. 10, but are these raw values or anomalies? Fig 14 c,d have units that cannot be right.

On line 369-370, they say, “it is now well appreciated that any climate response under investigation will be slaved to the spatial structure of the forcing imposed on a model.” This is not true. If so, you would not need a model. There are many non-linear responses.

The table, figures, and supplemental information have to stand on their own, so all acronyms have to be defined and referenced.

The authors have a big problem with acronyms. They use many without defining them. They define some multiple times. And they define them and then don’t use them again more than one time. This is very confusing for the reader.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/11/C1759/2015/cpd-11-C1759-2015-supplement.pdf

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