

## ***Interactive comment on “The sensitivity of the ENSO to volcanic aerosol spatial distribution in the MPI large ensemble” by Benjamin Ward et al.***

**Anonymous Referee #2**

Received and published: 18 September 2020

This study investigates the controversial topic of the influence of volcanic aerosol radiative forcing on ENSO. Model studies have suggested that volcanic forcing has a significant impact on tropical ocean-atmosphere circulation which manifest as anomalous ENSO signals. This study uses a very large ensemble of historical simulations, and explores the ENSO response to 3 large tropical eruptions, Agung, El Chichon, and Pinatubo, which each had contrasting spatial distributions of the aerosol forcing. The study shows a strong difference in ENSO response for the Agung eruption vs the other two eruptions, and links this difference in response to the differences in forcing structure. The authors then argue that this result supports a hypothesized mechanism linking volcanic aerosol forcing and ENSO, namely that hemispherically asymmetric forcing leads to a latitudinal shift in the ITCZ, leading to anomalous zonal winds at the

C1

equator, and related changes in ocean current and SSTs.

The topic is certainly an active area of research that fits well in the scope of ESD. The model ensemble utilized is impressive in its size, which adds much to the statistical significance of results and aids the study to be able to contribute to the debate. However, the analysis does not sufficiently support the conclusions made, and I suggest the authors undertake major revisions before publication.

The main conclusion of the work is that the ENSO response is driven by the volcanically induced displacement of the ITCZ. It is not, I think, so clearly stated, but the implicit argument seems to be that the “other” proposed mechanisms such as ODT should depend only on the magnitude of the tropical forcing. The fact that different spatial patterns of forcing lead to (what appear to be) nearly opposite ENSO responses in the model results shown does seem to be a valid challenge to the ODT mechanism hypothesis. But, that in no way proves that the ITCZ mechanism is correct. This would only be the case if there were absolutely no chance of any other mechanism being important, and that is difficult to prove. There is not any strong evidence shown that the ITCZ shift actually happens, let alone that it is the cause of ENSO (or relative ENSO) anomalies. As a result, this main conclusion is not well supported by the results shown.

Specific comments:

Line 10 (and title): the paper focuses mostly on the *relative* ENSO, based on RSST anomalies. It is, I think, critically important to be clear with statements whether you refer to ENSO or relative ENSO. While the simulations show positive relative ENSO responses, the absolute SST anomalies seem to be small in this case, so, if ENSO is defined only by the Nino3.4 temperature index, these wouldn't qualify as positive ENSO. To put it another way, even a reader who is familiar with the topic would assume from this abstract that the model produces positive Nino 3.4 temperature anomalies from NH or symmetric forcings, which I believe is incorrect.

C2

L12-13: “El Nino-like” or “La Nina-like” is used many times through the manuscript, without any description of what it means in comparison to the “actual” El Nino or La Nina. Why is it only “like” these things? How is it similar and/or different?

L13: again, I think you mean relative anomaly here?

L26: The atmospheric and ocean responses to volcanic forcing are difficult to prove from observations due to the limited sample size, and evidence for their existence is largely from models, which are imperfect. It’s important not to overstate our confidence in the existence of these responses.

L19: I don’t this Robock 2000 says this explicitly.

L31: Discussion of paleoclimate proxies should include the paper from Dee et al. (2020):

Dee, S. G., Cobb, K. M., Emile-Geay, J., Ault, T. R., Lawrence Edwards, R., Cheng, H. and Charles, C. D.: No consistent ENSO response to volcanic forcing over the last millennium, *Science* (80-. ), 367(6485), 1477–1481, doi:10.1126/science.aax2000, 2020.

L35: Please replace instances of “Krakatoa” with the preferred “Krakatau”

L39: Reference should be made in the paragraph to the analysis of CMIP5 models by Paik et al (2019): Paik, Seungmok, et al. "Volcanic-induced global monsoon drying modulated by diverse El Niño responses." *Science Advances* 6.21 (2020): eaba1212.

L40: “Most recent” does not necessarily mean most correct. Also, you say “studies” but cite only one paper.

L54: “due to the underlying dynamics of the Pacific Ocean” is vague and does not help the reader to understand this mechanism.

L66: Do you mean “moves southward”? Usually the ITCZ is fairly centered on the equator.

C3

L66: One should give a precise definition of “trade winds”. Also, please provide a clear explanation of how a meridional shift in the ITCZ produces a change in the magnitude of the trade winds, and not just a similar shift in their meridional location. This mechanism is central to the argument but is never given a physical basis.

L81: “volcano and no-volcano members” won’t be clear to all readers.

L89: The size of eruptions refers to the amount of magma released. Here you are more interested in eruptions that produced the strongest stratospheric aerosol radiative forcing, which is not always the same as being the largest eruption.

L92: The aerosol loading from Pinatubo was \*roughly\* hemispherically symmetric, but not exactly.

L93: The actual hemispheric distribution for Krakatau is quite uncertain, since there are few observations from that time. The forcing used in the simulations is however roughly symmetric.

L103: The relevance of the “total of 600 years of climatology” is not clear.

L107: “over-estimate” rather than “amplify” would be more accurate.

L115: The source of the aerosol forcing distributions used in these simulations should be given, since it is very important when comparing the results shown here to other studies which use different forcing sets.

L117: I’m not sure we know for sure what processes are most important for controlling the hemispheric asymmetry of the aerosol forcing. Robock (2000) has argued that meteorological conditions at the time of the eruption are important, as it seems winds pushed the Pinatubo SO<sub>2</sub> cloud to the equator. Otherwise, the Pinatubo distribution might have been more NH biased.

L125: Only 2 forcings are strongly asymmetric.

Fig 4: x axis label should be more specific (not “year”), and axis extended to include

C4

the year of the eruption, with markers at the month of eruption.

L127: the description and Figure are at odds here, as the figure shows a difference in cooling between NH and SH, while the text mostly refers to hemispheric cooling.

L128: the magnitude of the eruptions is less important here than the magnitude of the forcing included in the simulations.

L131: This statement is not accurate, as strongest cooling seems to occur at the end of year 2.

L133: Unclear what “results” are referred to. Also, “are consistent with” is quite weak language—with the large amount of fields output by a climate model, one should be able to determine whether the Hadley Cell is displaced or not!

L134: There is nothing on Fig 5 that looks like a simple shift. On plots of anomalies, a shift should show up as a dipole, with negative and positive anomalies. The anomalies for Agung are almost only negative, while for EC and Pinatubo, the anomaly patterns look more like a narrowing of the ITCZ over the equator.

L136: This seems like a rather complicated hypothesis: simpler might be that the response is also sensitive to the magnitude of the forcing, which is stronger for Pinatubo?

L210: Nothing presented proves that these mechanisms are completely absent. They can still be active even if another process is more important to the overall response.

L211: A “limited number” of ensemble members is not a drawback per se: there are no studies that use an unlimited number of ensemble members.

L212: Why is reliance on statistical tools framed as a limitation of these prior studies?

---

Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2020-63>, 2020.