

(OUR ANSWERS ARE INDENTED AND IN BOLD, BELOW THE REFEREE'S COMMENTS)

This paper is still misleading, and I recommend that it only be accepted if the title and abstract are changed. It only presents simulations with ENSO neutral conditions, but that is not mentioned in the title or abstract. Both I and the other reviewer mentioned this, but the authors have not responded to this issue clearly. The authors have to make clear that they ignored SST impacts on atmospheric circulation, and only present simulations with this restriction. Why not change the title to this? "Volcanic stratospheric injections up to 160 Tg(S) yield a Eurasian winter warming indistinguishable from internal variability with El Niño/Southern Oscillation neutral conditions" And this ENSO neutral limitation also needs to be mentioned in the abstract.

We are grateful to Dr Robock for taking the time to look over our manuscript one more time. We disagree with the statement that our paper is "misleading", and with the suggestion that the title and abstract need to be changed. Hence, we have decided to leave title and abstract as they are.

Lines 7-8 say, "no such warming is seen in simulation with more recent climate models" First, shouldn't it be simulations? Second, this is not true. Coupe and Robock (2019) do show such warming.

We are grateful for the typo correction. Coupe and Robock (2019) is cited.

Line 402: The authors add a reference to Dee et al. (2020) claiming that they support the claim that volcanic eruptions do not produce El Niños. But Dee et al. made a fundamental error, looking at absolute SSTs for an El Niño signal, failing to account for volcanic cooling. This was pointed out by Robock (2020), so it is incumbent on the authors to point that out and not just reference the incorrect results from Dee et al.

Robock, Alan, 2020: Comment on "No consistent ENSO response to volcanic forcing over the last millennium." Science, 369 (6509), eabc0502, doi:10.1126/science.[abc0502](https://doi.org/10.1126/science.abc0502).

We see no need to enter into the merits of the Dee et al. (2020) paper in our manuscript, since ENSO is a secondary issue. Nonetheless, we thank the referee bring to our attention the "Comment" that was also published in Science, which we have now cited in a footnote in our paper.

The authors responded to my review:

"First, we note that in their pioneering study, Robock and Mao (1992) specifically subtracted out the ENSO signal to isolate the volcanic response."

This is not correct. Robock and Mao removed the average surface temperature response from El Niños and La Niñas that occurred in the absence of volcanic eruptions, in the naïve assumption that the climate responses were independent and could just be added. But they did not consider the nonlinear atmospheric circulation response to a simultaneous El Niño and volcanic eruption. That was the state-of-the-art 30 years ago. But now it is incumbent to include atmospheric dynamics and not simply add two separate surface temperature patterns.

We are including atmospheric dynamics in our model, in addition to fully-coupled ocean, land and sea-ice components. So, if the atmospheric circulation is changing non-linearly, our model will capture such effects. In any case: the referee is not stating what changes he is suggesting, so there is no action item here.

"Second, we agree with the referee that the question of whether El Niño conditions do or do not alter the Eurasian winter response to volcanic eruptions is potentially interesting. But, the referee will agree, it is a separate question from the one we are addressing in this paper. In fact, is [sic] is a secondary

question. Before addressing that question, we need determine whether volcanic

eruptions - over a much broader range of amplitudes than those analyzed in Coupe and Robock (2021) - are able to produce a surface winter response without additional factors. This is the primary question.

"The referee will recognize, we hope, that our paper is focused on answering that primary question. Therefore, we deliberately chose to start all runs in an ENSO-neutral state, to isolate the volcanic from the response to ENSO, as we explain in the paper. We plan to address the secondary question too, and a follow-up paper on that is in preparation."

I do recognize that the authors are trying to understand how large the forcing has to be after a volcanic eruption in an ENSO-neutral situation to produce winter warming. They call this a "primary question." But since the two last large volcanic eruptions, 1982 El Chichón and 1991 Pinatubo, were accompanied by El Niños, and even the third largest one, 1963 Agung, had a small El Niño. [Whether the volcanic eruptions produced or enhanced the El Niños is a separate, but interesting issue.] So why is the primary question one that does not apply to any of the volcanic eruptions in the past 60 years? The authors do not explain this.

The referee asks why the primary question does not include ENSO? The answer is nearly obvious, and we have already spelled it out in our paper.

Just to be clear: in the published literature on this matter, two studies (cited in our manuscript) have clearly shown that ENSO makes no difference to the Eurasian winter warming. In contrast, Coupe and Robock (2021) have claimed ENSO makes a difference. The very fact that papers disagree on this issue shows that the ENSO signal is not robust (if it exists at all).

In any case: there are scores of papers on the potential post-eruption Eurasian winter warming, and most of them never mention ENSO. We invite the referee to read Azoulay et al (JGR, 2021), which is the immediate predecessor to our study: there is no mention of ENSO or El Niño mattering at all. Many other studies, starting with Robock and Mao (1992, 1995) have gone to great pains to make sure they linearly remove ENSO. Obviously ENSO is a source of confusion, at best, not a key player.

In summary: we see no need to make further changes to our manuscript.

"In the present manuscript, we explore a very wide range eruption magnitudes, from Pinatubo to Tambora to Samalas, and well beyond."

Not really. The 1257 Samalas eruption was also accompanied by an El Niño. See Robock (2020, reference above).

"Not really" what? In our paper we have explored a very wide range eruption magnitudes, including Samalas and well beyond. We do not understand why the referee is again talking about ENSO, since it is not mentioned in that phrase.

There was probably an El Niño at the time of the Tambora eruption, too:

A wet and cold El Nino: The Tambora volcano's impact in the Australian colonies, by Don Garden, Journal of the Royal Australian Historical Society, 01 December 2021 <https://search.informit.org/doi/10.3316/informit.188355038885462>

So ignoring a simultaneous El Niño with large volcanic eruptions makes the current work less relevant for what has actually happened. I think it is incumbent on the authors to make that clear in this paper.

If the referee can point us to a peer-reviewed paper showing that most major volcanic eruptions are followed by an El Niño, we will be happy to cite it. At present, the overwhelming evidence in the literature is to the contrary. We honestly do not understand what else needs to be made clear.

Alan Robock

Thank you again, and best regards. The authors.