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Interactive comment

Interactive comment on "Scant evidence for a volcanically forced winter warming over Eurasia following the Krakatau eruption of August 1883" *by* Lorenzo M. Polvani et al.

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General comments:

This paper focuses on the "volcanic winter warming" theory that stratospheric aerosols from volcanic eruptions cause changes in atmospheric circulation, that lead in turn to warming over the northern Eurasian continent in the 1 or 2 winters after the eruption. The study looks specifically at the 1883 eruption of Krakatau, and presents analysis of surface temperature reconstructions, reanalyses and climate model output. The conclusions of the study are very similar to those of an earlier study (Polvani et al., 2019) which focused exclusively on the 1991 eruption of Pinatubo. The main conclusion of

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the paper is that “the observed warming over Eurasia in the winter of 1883/84 was, in all likelihood, unrelated to the Krakatau eruption”. Taken together with the prior paper on Pinatubo, the authors argue that volcanic winter warming is not a real phenomenon for eruptions of this magnitude, and that the warm Eurasian temperatures in the winters after these two eruptions were chance occurrences resulting from natural climate variability.

The paper is provocatively written, and indeed a major promise of the study is its direct challenge of the winter warming theory. Where earlier studies have raised doubts about selected components of the overall theory, this study aims to call into question its very validity. The topic is certainly open for scientific debate, and there is room for critical perspective.

However, this study contains numerous fallacies which undermine the logical argumentation. Most generally there are two main problems. First, the authors disregard the observational basis of the winter warming theory–it is mentioned in passing only once in the introduction. The fact that models do not reproduce the expected winter warming signal is perplexing, even disappointing, but it is no reason on its own to disbelieve observations. Secondly, the potential influence of volcanic aerosol on circulation or continental surface temperatures–in the single realization of reality–cannot be assessed by focusing on a single eruption. The observational basis for the winter warming theory has established that the signal is within the range of natural variability but is detectable because of its consistency across eruptions: the observational studies identified winter warming or positive NAO anomalies by compositing observations after more than 10 eruptions (e.g., Robock and Mao, 1992; Christiansen, 2008). It is only because of the statistical significance of the observed winter warming across many eruptions that one may interpret the modest observed Eurasian warming after Krakatau as being linked to the eruption. By focusing on a single eruption, and by neglecting the observational basis, the study fails to mount a valid challenge to the volcanic winter warming theory.

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L6: The first main finding is that "observed post-Krakatau winter warming over Eurasia was unremarkable (only between 1- and 2-sigma of the distribution from 1850 to present)." However, prior studies do not suggest that warming over Eurasia after any single eruption is necessarily remarkable. Robock and Mao (1992) show temperature anomalies from 12 eruptions since 1883: Eurasian anomalies rarely exceed +3C, are in some cases negligible, and on average suggest a mean warming of ∼1-2C. As another example, Christiansen (2008) showed that in the winters after 13 eruptions from 1880-2000, 11 winters showed a positive NAO. The NAO magnitude in each of those years is unremarkable–and the mean NAO anomaly is a very pedestrian ∼0.6–but what is remarkable is the consistency of the post-eruption anomaly. The repeated description of the Krakatau winter warming as "unremarkable" is in no way evidence against the winter warming theory, in fact, that amount of warming is quite consistent with what one would expect based on observational studies.

L7: The second finding is that "reanalyses indicate the existence of very large uncertainties, so much so that a Eurasian cooling is not incompatible with observations". This finding does not follow from the results shown. First, the phrase "not incompatible with observations" obscures the fact that only 3 out of 56 ensemble members in the reanalysis produce negative Eurasian temperature anomalies. Based on the ensemble, one should conclude that Eurasian cooling was very unlikely. Secondly, the statement refers vaguely to "observations" that "a European cooling is not incompatible with", without specifying that these observations are only the surface pressure observations that are assimilated into the reanalysis. Without more careful language, a reader might easily understand that a European cooling is not incompatible with all observations. But given the surface temperature reconstructions based on temperature measurements described in the study, this is clearly wrong. Overall, the conclusion seems to be an attempt to decrease confidence in the observation of a Eurasian winter warming after Krakatau, but there just isn't any reasonable way that results from 3/56 ensemble members from a reanalysis assimilating surface pressure measurements can have any influence on our understanding of Eurasian temperatures which are based primarily on

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actual temperature measurements. In contrast, the fact that the reanalysis ensemble mean "temperature anomalies are in good agreement with the observations" can only increase our confidence that Eurasian temperatures in winter 1883/84 were indeed warmer than normal.

L8: The crux of the author's argument then comes down to the third finding, which is that "models robustly show the complete absence of a volcanically forced Eurasian winter warming". Based on this finding, the authors later conclude that "low-latitude eruptions as large as Pinatubo or Krakatau are unable to cause a forced surface temperature anomaly over Eurasia that can be distinguished from unforced variability". While the absence of winter warming in present-day model simulations is perplexing, model results cannot be used as a basis to discount a theory based on observations. The authors barely mention the observational basis of the theory, focusing more on describing the "stratospheric pathway" mechanism proposed by the early studies. This represents a "straw man" fallacy: the author's attack on the "stratospheric pathway" mechanism is justified, but this is not a valid argument against the observation-based winter warming theory itself.

L17: No justification needs to be given for a summary of background literature. It may be true that extant literature is "confusing and often contradictory", but this is hardly unique to this scientific topic, and to label prior work so might come across to some readers as a rhetorical tactic to undermine confidence in prior work.

L24: This "in a nutshell" description of the stratospheric pathway cites only papers from the 1990s, and neglects recent work that has both challenged the simple "meridional temperature gradient" mechanism and investigated other mechanisms, including planetary scale waves and tropospheric eddies (e.g., Toohey et al., 2014, Bittner et al., 2016, DallaSanta et al., 2019).

L32: It is not true that "very little aerosol is left in the stratosphere in the second post-eruption winter". Satellite-based retrievals of stratospheric aerosol optical depth (AOD)

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after Pinatubo show that the peak in global mean AOD was around winter 1991/92, with a value of ∼0.1. One year later, the AOD is around 0.68, still elevated by an order of magnitude above the background value of ∼0.06. A similar result can be found if one looks particularly at the AOD in the tropical region. There may be valid pragmatic reasons to limit focus to the first post-eruption winter, but the statement that there is "very little aerosol is left in the stratosphere in the second post-eruption winter" is not true, and this claim should not be used to invalidate prior studies which averaged 2 post eruption winters.

Ll34: Stating that these papers all failed to replicate the results of earlier studies is very much oversimplifying their work. For example, Bittner et al. write: "For eruptions of the size of Krakatau and Pinatubo, the multi-model ensemble shows a strengthening of the polar vortex in the first post-eruption mid-winter, which challenges the assumption of a general failure of coupled climate models to simulate the dynamical response to volcanic eruptions." Also, Wunderlich and Mitchell (2017) do not present any results from CMIP5 models regarding the winter warming: they explore winter warming in reanalyses and present from some CMIP5 models simulated tropical temperature anomalies.

L41: While some of the earliest model studies used very small ensemble sizes, most past studies used ensembles of some reasonable number and quantified the statistical significance of the ensemble mean response, taking into account natural variability. It is therefore unjustified to claim that “much of the earlier literature had failed to properly account for the large internal variability associated with the stratospheric polar vortex and with the NAO”.

L164: The number of deaths caused the Krakatau eruption has absolutely no bearing on the expected relative magnitude of the winter warming signal, as the number of deaths depends strongly on the population living in proximity to the volcano.

L224: The response of the two most extreme ensemble members illustrates that there is natural variability, and that the anomaly in any one post-volcanic year may vary from

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case-to-case, but it does not negate the possibility of a non-zero mean response, i.e., a higher probability of either negative or positive anomaly.

L231: “acceleration”

L257: Wunderlich and Mitchell didn’t look at winter warming in the CMIP5 models.

L258: This misrepresents the results of Stenchikov et al. (2006) who state in their abstract “The IPCC models tend to simulate a positive phase of the Arctic Oscillation in response to volcanic forcing similar to that typically observed. However, the associated dynamic perturbations and winter surface warming over Northern Europe and Asia in the post-volcano winters is much weaker in the models than in observations.”

Footnote 4: For the sake of balanced consideration of prior work, reference to Zambri and Robock (2016) should come in the introduction rather than here near the end of the paper. Also, editorial commentary characterizing the work as "a single dissenting voice" or "not, to date, ... independently reproduced" is clearly rhetoric meant to undermine confidence in this study, and would benefit from being recast in more objective and quantitative terms.

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