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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Northern Hemisphere continental winter warming following the 1991 Mt. Pinatubo eruption: Reconciling models and observations" *by* Lorenzo M. Polvani et al.

Anonymous Referee #2

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This manuscript argues that Mt. Pinatubo's 1991 eruption had little to no impact on continental surface temperatures, and hence observed surface warming in midlatitude winters was due to natural variability. The implication is that similar conclusions may hold for other eruptions. Indeed, there has been perhaps excessive focus on explaining and simulating the observed Pinatubo response, without sufficient regard to the inherent role of natural variability.

This manuscript contributes to ongoing discussion by frankly addressing the issue of natural variability, and its novel employment of a coupled atmosphere-ocean-chemistry model for volcanic simulation is a useful addition to the literature, even if having only

13 members for that model leads to difficulties with statistical significance.

After tempering the overall claims and investigating a lower-stratospheric pathway as detailed below, this manuscript is suitable for publication.

General comments

1. The text is too quick to dismiss temperature reconstructions. Despite the inherent uncertainties of temperature reconstruction, the key is that averaging over several centuries reveals a statistically significant pattern of winter warming, apparently even stronger for the subsequent winter (Fischer et al., Geophys. Res. Lett. 2007), which would be highly coincidental if volcanic eruptions were unrelated.

2. P5 L19 and elsewhere describes the ensemble sizes as "large" (13, 42, and 50 members). However, P3 L12 mentions that Bittner et al. (2016) needed 60 members for 95% confidence in the stratospheric response, and other comparable examples are mentioned. Thus "large ensemble" seems incorrect a priori, so the difficulties throughout in achieving significance should not be surprising.

3. To address the underlying mechanism(s), the manuscript uses a 13-member ensemble with an improved stratosphere to argue against a pathway between stratospheric vortex perturbations and surface temperature perturbations. However, the discussion does not address the possibility of a lower-stratospheric pathway, which from Fig. 7 seems plausible. This could be important if the impact on the vortex does not have the same vertical structure as natural variability. Addressing this would be straightforward by repeating the analysis of Fig. 5 at one or two lower levels such as 50 hPa.

Specific comments

P1 L17: “is likely to be very small” is unclear—it was likely very small based on these model results?

P2 L1: “short lived” is relative; P6 L12 cites an e-folding time of 12 months, longer than most natural modes of variability.

P2 L16–25: The suggestive tone is biased. In the “widely believed” (P1 L1) viewpoint of winter warming, it should not be “remarkable” that the subsequent winter after Pinatubo “happened to be” warm, nor is it “highly perplexing” and “difficult to reconcile” that historical warming is not exactly correlated with eruption magnitude. Rather, the question is whether eruptions of a given strength can induce a statistically and physically significant winter warming. Missing here is Fischer et al. (Geophys. Res. Lett. 2007), which should be cited here as the most recent (known to this reviewer) post-eruption temperature reconstruction.

P3 L5: An implicit assumption of this mechanism is that the balanced acceleration lies in the vortex region, which is not necessarily the case. Bittner et al. (J. Geophys. Res. Atmos. 2016) discusses this.

P3 L13: “tiny” is relative; perhaps relate this to annular mode, standard deviation of lowpass-filtered winds, or similar. See also comment for P9 L13–15.

P3 L19: Stenchikov et al. (2002) had only 4 ensemble members, and argued for a reduction in planetary wave activity, contrary to what Graf et al. (2007) said about a single eruption. Perhaps this paragraph should conclude that the mechanism is not demonstrated by these single-model, small-ensemble studies.

Remove “clearly, “abundantly clear,” etc. for P3 L21, P7 L24, P8 L5, P9 L34, P10 L11, P10 L29, P11 19, P12 L22. The word doesn’t enhance the argument and may come across as proof by intimidation.

P4 L11–14: should also cite Barnes et al. (J. Clim. 2016), which does find a significant response. Thus even when experimental design and intermodel spread are controlled

for, the result can still vary!

P5 L30–P6 L7: a common limitation, here and in many other studies, is that it is unknown whether or not the response is linear. (Perhaps a threshold magnitude of forcing is necessary, or stronger forcing induces feedbacks.) This limitation should be stated, as the conclusions for these Pinatubo-sized eruptions may not hold for smaller or larger eruptions.

P7 L23: ensemble averaging reduces, but does not eliminate, internal variability.

P7 L25: in F3, the larger ensembles have slight windows of significance. An estimate (even via simple bootstrapping) of the necessary number of ensemble members to achieve continental-scale significance would be very helpful for this discussion and for future studies.

P7 L35: internal variability is not superimposed to any forced response—it may very well be nonlinear (i.e., the higher moments of the underlying probability distribution functions may change).

P7 L11 and F4: rather than a box-and-whisker plot, a plot of the 3 probability distribution functions is preferable here in my opinion, so that the reader can compare the distributions.

P8 L25–26: the other two models may not have as accurate a representation of the stratosphere, but do they give similar results? If so, they should be included. If not, why is the subsequent comparison with Bittner et al. (2016) (which similarly has a non-interactive stratosphere) valid but not with the other two?

P9 L13–15: it is not appropriate to compare weekly SSW variability with volcanic forcing as their timescales are well-separated. The appropriate comparison would be something like variability of DJF average, which is approximately 10 m/s at 10 hPa and 6 m/s at 50 hPa, more comparable to the 3.5 m/s reported here.

P9 L16–17 and F5: It might be helpful to add a third scatter plot of a lower comparison point like ∇T_{50} and T_s, which may correlate better than 10 hPa, if the perturbation is comparatively larger than natural variability.

P9 L23–34: examining two individual ensemble members does not offer any insight into the mechanism, especially since the manuscript already argues that natural variability is large. This paragraph and the corresponding F6 should be removed.

P10 L7: even if 10 hPa is "canonical," it may be the wrong level for finding the mechanism. Repeating the same analysis at a lower level, such as 50 hPa, would either strengthen the current null-hypothesis argument or provide new insight into the mechanism's vertical extent.

P10 L14–16: "quite likely" and "would have emerged" are purely speculative and should be removed, as the null hypothesis was not rejected by the significance test. Instead, a simple bootstrapping estimate of the requisite number of samples to achieve significance may again be helpful.

P10 L17–24: again, the possibility of a lower stratospheric pathway should, and could easily, be addressed here with the existing methodology.

P11 L28–30: are their low model tops thus an indirect argument for a lower stratospheric pathway? P10 L25 to P13 L4: the conclusions should be updated following any relevant changes made as a result of these comments.

Technical comments

P8 L6: "stonger" should be "stronger"

P8 L11: "Fig. 5" should be "Fig. 4"

P9 L2: "need" should be "needed"

F4: "nbox" should be "box"

F5: "R_2" in legend of subplot (a) should be "R^2"

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