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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 #3

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The paper deals with a longstanding issue of the inability of climate models to reproduce the high-latitude near-surface winter warming following the major low-latitude volcanic eruptions. I appreciate the authors have risen this issue again using a set of the "new generation" models. I believe the reviving this issue is useful but I cannot completely agree with some interpretations and methodology the authors use in this study.

General comments:

To test the mechanism based on the troposphere-stratosphere dynamic interaction, the authors conducted the Pinatubo case study focusing on the first winter after the June

1991 volcanic explosion in the Philippines. However, the choice of the case-study is unfortunate as in the winter of 1991/92 the positive AO was not forced by the "stratospheric" mechanism. In observations, the polar vortex was weak and asymmetric with the wave number 2 prevailing. So, it is pointless to analyze this response to prove or disprove the stratosphere/troposphere dynamic interaction mechanism. Stenchikov et al. (2004) indicated that the easterly QBO phase in winter of 1991/92 weakened the polar vortex, and winter of 1992/93 with a westerly QBO phase provides a better case-study to test the "stratospheric" mechanism.

As it is correctly stated in P8, L22-24, the "stratospheric" mechanism involves two steps: strengthening of the stratospheric polar vortex and downward propagation of the signal. The proof of the latter portion of the mechanism did not come directly from "volcanic" studies, as volcanic eruptions are rear and provide insufficient statistics, but from climatological studies of Baldwin and Dunkerton (1999). As mentioned by Stenchikov et al. (2006) the strengthening of the polar vortex caused by the equatorial lower stratospheric warming due to aerosol-induced heating, is robust in the models, but the models fail to reproduce the downward transport. So, to disprove this "stratospheric" mechanism the authors have to deal with the climatological analysis as well.

It is not surprising that some of the model ensemble members could produce a "winter warming" pattern. It is more important how frequently this pattern appears and what mechanism causes it. Models have to produce this pattern more frequently to be consistent with the climatological studies that show a statistically significant positive AO pattern after compositing multiple equatorial eruptions. The conclusion that the up-to-date models could perfectly reproduce the winter warming based on the fact that some ensemble members capture it, is not supported.

Specific comments:

P3, L13-15: A vertical propagation of the planetary waves is a threshold process as suggested by Charney and Drazin (1961), so small change of the wind could qualitatively change the planetary wave reflection coefficient.

P4, L26-27: AO response is an atmospheric effect. Why increasing of model complexity should matter to answer the question that the Stratosphere-Troposphere Interaction is real.? E.g., if ozone additional radiative effect matters, this has to be specifically shown.

P5, L23: The chosen models are inconsistent in reproducing the aerosol forcing. In Figure 2 the aerosol forcing in the models differs by 50%. It would be useful to mention what was the observed forcing to compare with.

P5, L30-33: The winter of 1991/92 after the Pinatubo eruption is a wrong choice (see Figure 5). A "composite" approach has to be considered to obtain statistically significant anomalies in observations.

P6, L2: This is incorrect. The eruption of Mt Agung of 1963 developed an aerosol equatorial reservoir that caused warming of the equatorial lower stratosphere and enhanced equator-pole temperature gradient in the lower stratosphere. The re-distribution of aerosols between the hemispheres is not directly relevant.

P6, L8: The first winter is a wrong choice.

P6, L12: Volcanic aerosols remain in the equatorial reservoir in the second winter after the eruption that is why the effect is seen in the second winter as well.

P6, L18: Driscoll et all. (2012) adopted this methodology from Stenchikov et al. (2006).

P7, L8-9: The shortwave (SW) radiative forcing in three chosen models differs by 50%. There is much more differences in SW and Longwave (LW) aerosol absorption.

P7, L13-15: The models three times overestimate the equatorial lower stratospheric heating caused by volcanic aerosols. This is the main forcing of the stratosphere-troposphere dynamic interaction. There is something wrong here.

P7, L20: "With this IN mind"

P7, L32-35: I think the correct question to ask is whether models are able to correctly reproduce the probability distributions of the Arctic oscillation (AO) responses to volcanic forcing. But for this one has to extract multiple cases from observations and to construct the observed probability distribution. It is not doable with only one post-eruption season considered.

P9, L10: Planetary wave reflection is the threshold process (Charney and Drazin, 1961) when small changes matter.

P9, L14: The wind variability coming from SSW is not relevant to the process. As soon as polar vortex zonal wind weakens below the threshold, planetary waves can propagate upward nonlinearly weakening the polar vortex. So, the amplitude of wind changes below the threshold, no matter how large it is, does not count. Sampling has to focus on the strong vortex cases for this purpose.

P10, L17-18: Exactly, the winter of 1991/92 is not suitable to study the forced stratosphere-troposphere dynamic interaction, as the positive phase of AO in the troposphere was caused by a different mechanism.

P11, L2: You mean surface cooling/warming, not in the lower stratosphere. Please clarify.

P11, L5-8: You have to explain why do we see a positive AO anomaly climatologically after multiple volcanic eruptions. If this would be extremely rear events as in the models, then a positive AO anomaly would not be seen in observations.

P11, L15-23: The strengthening of the polar vortex caused by the volcanic aerosols heating in the lower equatorial stratosphere is robust. This is the threshold process, so a weak strengthening matters. And it is unfair to apply wind variability in SSW to scale the increase in maximum wind.

P11, L25-35: The downward propagation mechanism was proved using climatological analysis (Baldwin and Dunkerton, 1999) and has to be challenged on this basis.

P12, L25-30: ENSO definitely could affect surface temperatures, although Volcano-ENSO interaction is highly nonlinear. At least contribution of ENSO variability in the volcanic signal has to be removed properly, which was never done in this analysis. Another important mode of variability is QBO that was not considered and reported in this study. QBO plays an important role in stratospheric wave propagation and could directly affect polar vortex and shape the stratosphere-troposphere dynamic interaction.

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