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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of volcanic aerosol on the Northern Hemisphere stratospheric polar vortex: mechanisms and sensitivity to forcing structure" *by* M. Toohey et al.

Anonymous Referee #2

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There is some observational evidence for an increased strength of the stratospheric vortex in the NH in the winters after explosive volcanic eruptions. The stronger vortex is usually explained by an increased equator to pole temperature gradient in the lower stratosphere caused by the radiative effects of the volcanic aerosols.

In the present paper the impacts of 4 different aerosol forcings, all estimates of Pinatubo, are studied with a climate model. For each forcing an ensemble of 12 simulations is performed and the different ensembles are compared to a control ensemble. Large differences are found among the ensembles and the changes are found to be fragile and sensitive to the details of the forcings. A more robust result is that the tem-

perature gradient in the lower stratosphere is not due only to the direct radiative forcing but to a large extent is due to an increased wave-driven meridional circulation.

I find that the topic is interesting and that the paper contributes with important results. The paper is also well written, although it is a little long.

My main concern is the relatively small ensembles of 12 members. The winter stratosphere shows a lot of variability and I can not help worrying that many of the differences reported between the different forcings are only an effect of natural variability. If just a few members more in one ensemble than in another ensemble by chance end up in a weak vortex state this could dominate the ensemble means (like in Fig. 2). The authors do perform significance tests but bootstrapping is not so reliable for small ensembles. It would be very interesting to see an additional 12 member ensemble for one of the forcings. The authors should also inspect all the individual simulations in the different ensembles and make sure that the ensemble means are not dominated by a few members.

There is a brief description of the bootstrapping in section 2.5. This description should be more detailed. Is it the ensemble members that are sampled? Normally one ends up with a distribution describing the null-hypothesis of no difference between ensembles and this null-hypothesis is then rejected if the observed difference falls in the tails of the distribution. Something else is done here but what?

The authors seem to expect that the effect of the eruption last two winters; that is how they analyse the observations (Fig. 3). But when it comes to the model experiments only the first winter is considered. The authors should argue for this choice.

Section 2.2 and 2.3: Why is there such a big difference between the two types of forcings (Fig. 1)?. The model based forcing seems much stronger than those based on observations. Also, is there any significant difference between the two model based forcings?

Section 3: It would be illustrative to see plots for all four individual winters of the zonal mean wind.

l327: But the flux divergence in the stratosphere is very different in the two experiments and the small region you refer to is hardly significant.

Paragraph beginning at line 487: I don't understand the arguments related to Fig. 13. The largest difference between the control and the forced experiments is in the first panel which do not include the wave forcing. If changes were due to the dynamics I would expect that also the forced experiments would fall on the same line as the control experiment, but displaced towards one of the ends. The fact that the line has moved indicates a radiative change instead of a dynamical change?

Paragraph beginning at line 504: The discussion of the tropical upwelling does not seem to be central to the topic of the paper and could be deleted.

In Figs. 7 and 11, (lower panels) it is difficult to see the difference between thin and thick curves. The authors should consider if these figures could be discarded as I don't think they contribute with important new information.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 16777, 2014.