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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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.

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We thank the reviewers for their insightful and helpful comments. In the following, the reviewers comments are reproduced in plain text, and our responses to the reviewer's comments are in bold. Note that references to figures in our replies correspond to figure numbers in the revised manuscript.

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 temperature 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.

Through modifications made to the manuscript based on the suggestions of the reviewers, we have decreased the length of the paper by about 400 words, and also significantly reduced the number of panels in the figures.

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.

We agree with the reviewer that more ensemble members would be ideal. In order to be consistent with the simulations used already in this work, new ensemble members should be produced from independent simulations of the historical period, with restarts at Jan 1, 1991. Producing new initial conditions is therefore

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non-trivial.

In the time since the original manuscript was submitted, 4 new independent CMIP5 historical simulations have been completed using the MPI-ESM. We therefore have used these new 4 simulations to define 4 new initial condition states for our simulations, thereby increasing the ensemble size from 12 to 16 for all ensembles. The results based on 16 rather than 12 members have been included in the revised version. As a result of the increase in ensemble size, the degree of difference between the S98 and SAGE_4λ ensembles has decreased and differences at high latitudes is no longer significant. We have therefore modified the descriptions of the results, as well as the abstract and conclusions of the paper accordingly.

The significant difference between the SVC and WVC forced ensembles has remained with the larger ensemble size. The degree of difference between SVC and the other ensembles is made visible in the new Figure 9 (shown below), showing the zonal mean zonal wind at 60°N, 10 hPa. In this figure, one can see that in the SVC ensemble, only 2/16 members have zonal winds less than the mean of the CTL ensemble. We therefore feel that the main conclusion of the paper regarding the impact of spatial structure of the volcanic forcing on the stratospheric response is still justified. However, this conclusion is carefully qualified in the Discussion and Conclusions, where we argue that since the vortex response to volcanic forcing is mediated by wave forcing, modelling studies will need many ensemble members (even more than 16) to derive full confidence in the results.

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?

We have added the following more explicit description of the bootstrapping technique:

> **For example, for each latitude and pressure level, and for each forced ensemble, 1000 bootstrapped sample means are produced by sampling the 16 ensemble member values with replacement, resulting in an approximation of the uncertainty distribution for the mean value. The same process is applied to the CTL ensemble, with n=50. The difference between the two bootstrapped distributions defines the uncertainty in the mean difference, and is used to define the 95% confidence interval of the ensemble mean difference. When the 95% interval excludes zero, differences are considered significant at the 95% confidence level.**

Our bootstrap method produces a conservative estimate of significance, since it incorporates not only the variability of the control run, but also the uncertainty in each forced ensemble mean due to the variability of the forced ensemble members.

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.

We have added an argument for focusing on the first winter to Sec 2.5, "Experiments". Briefly, we argue that it is possible the mechanisms linking volcanic forcing and the vortex response may be different in the first and second winters, and that focusing only on the first winter simplifies the analysis and interpretation of the model results.

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?

We have added a sentence regarding the potential reasons for differences between the observation- and model-based forcings. Basically, differences are due to a combination of underestimation of the AOD by observations due to saturation effects, or errors in the model simulations due to model-error or uncertainty in the SO2 injection amount and height.

Differences between the two model-based forcings are shown in Fig 5. The significance of these differences is quite difficult to define: since the "control" (either the control ensemble defined here, or preindustrial control run) has effectively a stratospheric aerosol forcing of zero – with zero variability – any differences between the two forcing sets will be significant. What really matters though is whether these differences have any influence on the response to the forcings, which is why we focus only on the significance in the responses to the forcings.

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

We have added Figure 9 (shown below), which shows the zonal mean wind at 60 N and 10 hPa for all members of all ensembles.

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.

This point has been moved to the discussion of the "grand" ensemble results. In this case, the flux divergence at 30N, and from 30-10 hPa is significant, and very likely clearly related to the positive anomaly in the residual circulation.

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

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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?

We have amended Fig 13 (shown below), by removing panel (c) and adding ensemble mean values to the horizontal and vertical axes of the remaining panels (a) and (b). We have also reworked the description and argumentation regarding Fig 13, which shows that for a given polar cap temperature, or for a given EP-flux, there is a small increase (5 m/s) in zonal mean wind. This increase however is offset for 3 out of 4 of the volcanic ensembles by increases in EP-flux, which leads to a warmer, disturbed vortex. This figure is meant therefore to make 2 points. Firstly, it shows that typical relationships between dynamical quantities such as T and u, which are usually very highly correlated, are anomalous in volcanic winters. Secondly, the figure attempts to illustrate the negative feedback the increase in EP-flux produces in the simulations, and how the EP-flux increase counteracts the more direct impact of low-latitude heating due to the volcanic forcing. It is not necessarily an attempt to separate diabatic (radiative) from adiabatic (dynamical) heating, and we have removed any comments that could be construed in this direction.

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.

As suggested we have removed the tropical upwelling results.

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.

We agree with the reviewer's opinion that Figures 7 and 11 were hard to read, but we feel that the information contained in them, while strictly obtainable from

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other figures, summarizes a major finding of the paper, that the temperature anomalies at high latitudes result from circulation changes, not direct radiative aerosol heating. We have combined Figure 7 and 11 and simplified the presentation (including removing the gradient plots) in the current Figure 12.

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

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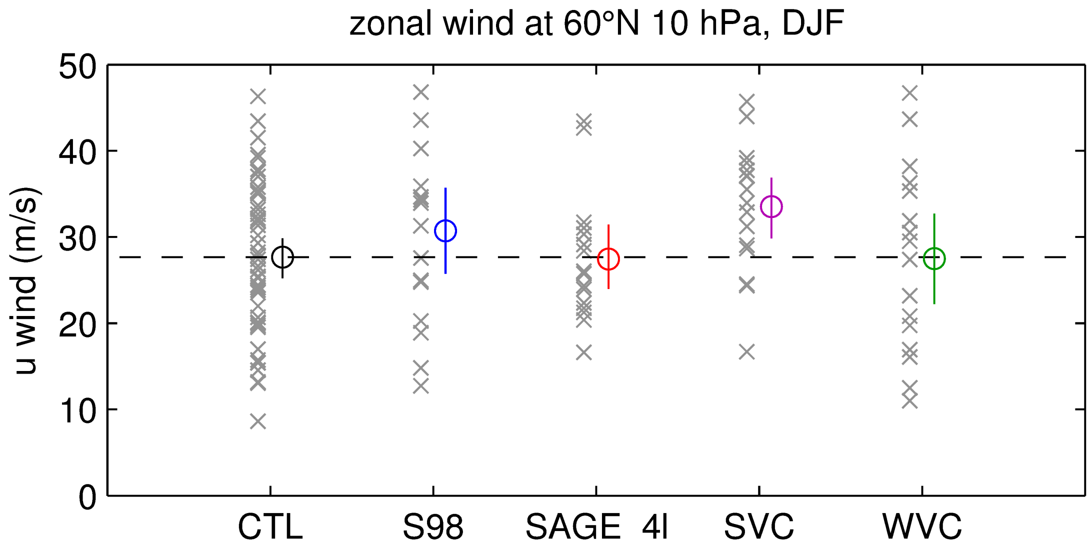

Fig. 1. New figure (Figure 9 in revised manuscript) showing the zonal mean zonal wind at 60 N, 10 hPa for the individual (gray symbols) and ensemble means for the 4 volcanic forcing sets and the CTL ensemble.