We sincerely thank the reviewers for their very helpful comments and suggestions, and feel that the paper has benefited from modifications motivated by the reviewers' input.

In the following, referee's comments in blue, our responses in black.

Anonymous Referee #1

General comments

- One of the conclusions you draw from your study (mostly Section 3) is that the AOD and radiative effects are independent of the longitude of the eruption. I would suggest you downplay this, as you're essentially verifying a well known result.

We wanted to make sure the results of our E17 experiment, with eruptions in Central America, could be interpreted as applicable to the 1991 Pinatubo eruption, hence the comparison of different eruption longitudes. While it has often been assumed (we're not aware of any published results on the issue) that longitude of eruption is irrelevant, we feel that there is the potential that longitude plays a small role: for example the ITCZ is not zonally symmetric, and if the 2D structure of the tropical pipe in the lower stratosphere is similarly "wavy", then longitude might play some small role in the resulting AOD evolution. Given this, the interpretation of our comparison of two longitudes might have been overstated, and we should say only that our results are valid for the two latitudes we have tested, not for all longitudes. The text in Sec. 3 and in the conclusions has been modified to reflect this somewhat more conservative conclusion.

- As you state in your conclusions, there is much more work needed in this study. It will be very useful to see simulations of eruptions at 15#S and on the equator. Changing the phase of the QBO would also be really good to see, as that is known to have a really significant effect on aerosol distribution.

Yes, we agree with the reviewer, and as stated we plan a future study with eruptions in other tropical latitudes. We've added a short paragraph mentioning the impact we would expect the QBO to have on our results in the conclusions, see also reponse to similar comment from Reviewer #2.

- In your time integrations, I don't understand why you stopped at 2 years. There are still aerosols in the atmosphere after this, and integrating longer can only help you achieve more robustness of your conclusions. You state (page 22454, line 21) that the length of time you integrate over doesn't matter, but that's only true if you integrate for long enough that the radiative effects of the volcanic eruption drop to zero. Pinatubo aerosols were found in the atmosphere longer than two years after the eruption. I think integrating over all 4 years of your simulations would make a lot more sense.

This is an excellent point. All cumulative anomalies have now been integrated over 4 years. Values of the cumulative quantities in the Figures and some quantitative values in the text have been changed, but the general qualitative features of our analysis and the interpretations of the results have not changed.

- There's no discussion of effective radius for the E700 experiment, which seems odd.

This would go a long way toward explaining why your aerosols fall out after a couple of years and what the AOD and radiative effects are. Also see specific comments for Sections 2.2 and 4.

We have inserted a comment on the size of the aerosols for the E700 experiment in regards to the temperature anomalies which set up the vortex winds in Sec. 4.2, as well as a general comment on aerosol size in this model compared to other model results included now in the model description. However, we have chosen not to focus too much on the aerosol size issue here, since 1. it has been dealt with extensively elsewhere (see Timmreck et al. 2010), and 2. it doesn't seem to have too much of an impact on the sensitivity to eruption season.

Timmreck C, Graf H-F, Lorenz SJ, et al. Aerosol size confines climate response to volcanic super-eruptions. Geophysical Research Letters. 2010;37(24):L24705.

- On your figures, if at all possible, using the same scales for all of the axes would be really helpful. Also, when you show negative anomalies, putting the axes markers in proper numerical order is less confusing (i.e., make the curves go down for anomalies that are more negative). Figures 5-7 and 10-11 should have a legend.

As suggested, legends have been added, and negative anomalies pointed downwards. Figures 7 and 11 have been modified to have consistent y axes (these are the only plots we can have consistent axes for and keep the plots readable).

Specific comments

Abstract: When you mention sensitivity, a brief definition would help, especially since this is your own construction.

We have included a succinct description of our sensitivity calculation in the abstract.

Section 2.1:

– Say what data set you used to prescribe SSTs

Our SST climatology comes from AMIP2 data compilation (Hurrell et al., 2008), this is now stated in the text.

Hurrell, James W., James J. Hack, Dennis Shea, Julie M. Caron, James Rosinski, 2008: A New Sea Surface Temperature and Sea Ice Boundary Dataset for the Community Atmosphere Model. J. Climate, 21, 5145–5153.doi: 10.1175/2008JCLI2292.1

- Is SO_2 radiatively active in the model? (This is relevant for my comments later on the E700 eruption)

No, SO_2 is not radiatively active, just the greenhouse gases (CO_2, CH_4 H_2O, O_3, and CFCs). We have added this list of radiatively active gases in the model description.

- Using fixed SSTs is sufficient for your experiment, since you're only looking at AOD and surface shortwave RF, but you should state this.

Added text in bold:
Sea surface temperatures are prescribed as an annually repeating climatology. **As a result, simulated surface temperature response to volcanic aerosols is largely damped, however, this should have no direct impact on the radiative impacts of the aerosols.**

Section 2.2:
- Line 14: Tg instead of Mt

Fixed.

- Line 23: Be careful with the term “atmospheric response” since you’re using fixed SSTs. It might be more helpful to say what you mean.

Changed to “This time period contains the vast majority of the radiative forcing impact of the volcanic aerosols”

- Lines 24-25: Other supereruption simulations have shown that it takes a LOT longer than 4 years to reach small levels of atmospheric loading of aerosols. I’m not about to say which modeling studies are correct, but putting your results into the context of the existing literature could be useful.

Note we are saying that after 4 years our aerosol loading is small compared to the maximum, not necessarily compared to background. The upshot being that our integrated quantities wouldn't much change if we continued the runs longer and integrated over longer time periods. Anyhow, the 2% after 4 years figure also happens to be just what one would expect from a simple exponential decay in aerosol with a 12 month e-folding timescale. We have added this point to the text in order to make it more clear.

- Line 1: Is your background case (the control run) present day?

Yes, all simulations are performed under present day forcings. We clearly state this in the text now.

- Line 8: June 15 and July 1 aren’t that different. Were your results so different that it warranted the extra simulations?

For comparisons of model results with data, we wanted to remove as many potential causes of mismatch between model and observations as possible. Also, there is no guarantee that the seasonal dependence varies smoothly with time, For example it could be that the wave-breaking and related southward meridional circulation in the SH begins suddenly each year. These reasons motivated our extra June 15 simulations. However, in the end, we saw only small differences between June 15 and July 1 eruptions. Interestingly, July 1 exhibited a greater degree of variability between ensemble members.

Section 3:
– Page 22450, Line 17: Repetitive use of “larger”

Fixed

- Page 22451, Lines 18-19: AOD is very dependent upon effective radius, so your AOD measurements should tell you whether the aerosol size in your model is giving you the right answer.

Yes, but AOD is also dependent on sulfur quantity, so it is important that the model get the right AOD and the right aerosol size for a given sulfur injection.

Section 4:

– Page 22452, Lines 20-23: This is an odd sentence. Is it really necessary?

This sentence has been removed.

- Page 22453, Lines 16-20: Can this be explained by natural hemispheric asymmetry of the BDC?

Yes, in fact the BDC is stronger in the NH winter than in the SH winter, which would explain the asymmetry we see here. However, we still feel the location of the eruption within the tropics may play some role: the text has been changed to raise both mechanisms as possible influencing factors.

- Page 22454, Line 19: What, specifically, do you mean by “total radiative impact”?

It's true, the statement “and so is a good measure of the total radiative impact” is vague, so we have removed it.

- Page 22454, Line 26: I have no problem with the units you use, but you should give the scaling factor to convert to J/m2.

This has been added.

- Page 22455, Line 17: Showing deposition rates would be really helpful, as this has a strong seasonal dependence and could be part of your explanation.

True, although surface deposition brings in a number of other processes, namely wet and dry deposition, scavenging and all other tropospheric aerosol processes which we feel might distract from the issue at hand. Our argument here is that the important processes leading to differences in AOD between the different eruption months seem to be occurring in the mid to upper stratosphere, and we feel that the plots are sufficient to outline these processes.

- Page 22456, Lines 1-4: An explanation of why the aerosols are lofted higher for the January eruption would be really helpful. What dynamics mechanisms are at play? Is this model-dependent?

Tropical upwelling is controlled by extratropical wave-breaking, which has a maximum during NH winter. This could be very model dependent, and one would need to fully diagnose the model's upwelling and inspect the microphysical processes of aerosol formation to really understand what's going on here. We have added some detail to the introduction of the concept of tropical upwelling, including a reference to the SPARC CCMVal report (2010) with diagnosed tropical upwelling in a number of models.

SPARC CCMVal: SPARC Report on the Evaluation of Chemistry-Climate Models, SPARC Report No. 5, WCRP-132, WMO/TD-No. 1526, 2010.

- Page 22456, Lines 5-10: Kravitz and Robock said a summer high latitude eruption would have a larger integrated radiative impact. Are your results consistent? Is this about how long it takes for the aerosols to move from the tropics to higher latitudes?

Yes, we see maximum AOD at high latitudes roughly 6 months after the tropical eruption, so it follows that the transport takes about 0.5 years which explains the difference between K&R's results and ours. This point has been added to the text.

- Page 22457, Line 7: Do climate models do this erroneously?

We have added: “Post-eruption decreases in clouds can be understood to be a result of decreases in evaporation following decreases in SW radiation at the surface (Soden et al. 2002), although other processes that might affect cloud cover in reality, such as changes in cloud condensation nuclei are typically not included in climate models.”

- Page 22457, Line 27: 30 K seems like a LOT just for sulfate aerosols. Does this include the radiative impact of SO_2 as well?

No SO_2 radiative effect, the heating is due just to the aerosols. The large degree of heating is a function of our quite large effective radius (>1 um) and the dense aerosol cloud . We now quote this aerosol effective radius, as an explanatory factor for our large temperature increase.

- Page 22459, Line 12: Again, a discussion of effective radius would help you be more conclusive than “it may be”

This sentence was poorly phrased. The conjecture regards the mechanism responsible for the smaller effective radius, not the fact that the aerosols are smaller (which was discussed, but not shown a few lines prior). The sentence has been rephrased to make this more clear.

Section 5:
- Page 22462, Lines 26-27: The Gao et al. study was based on actual data, which is necessarily all-sky. So according to them, isn’t it true that season of eruption is less significant than in your clear-sky simulations? Does this contradict what you said in lines 9-10?

The Gao et al. analysis of sensitivity was actually done with their simple parameterized transport model, and they quoted their sensitivity as relating to sulfate burden. As such, we've compared their sensitivity to our AOD sensitivity, which is the best comparison.

- Page 22463, Line 1: Be careful with this. There’s a big difference between 17 Tg and 700 Tg, and the sensitivity response does not appear to be linear with injection amount.

The text has been changed to make clear we talk just about two widely differing SO2 injections.

- Page 22463, Line 25: Another interesting idea for future work is where the cutoff is, i.e., at what magnitude of injection does the season start to become important?

Indeed. This point has been added to the text.

- Page 22464, Lines 29ff: Ocean memory of the cooling is probably a lot more important than a latent heat flux analysis, especially in terms of the integration that you do.

Yes, these lines have been removed.

Figures:

- Figure 1: You might want to check the updated version of the Sato et al. data: data.giss.nasa.gov/modelforce/strataer

This is where we retrieved the data, reference in text and citation now modified as to reflect.

- Figure 4: Doing zonally averaged cloud maps can be problematic (e.g. monsoons), so mentioning this would be useful. Also, it would be helpful to state your methods in more detail, i.e., that you calculated the anomaly first and then did the zonal averaging.

We have added some clarification to the introduction of the Figure in Sec. 4.1. Concerning the zonal means of zonally inhomogeneous fields, I believe this might cause problems when one, e.g., compares zonal means between two models (if, e.g., the zonal means might agree when the actual 2D fields are quite different). For our purposes here – showing in one model the climatological latitudinal distribution of clouds and the variability in those clouds – I don't believe its necessary to qualify the representativeness of the zonal mean fields.

- Figure 5 (and similar): Orange and red are too similar. Could one of these be green?

Yes, in fact we've slightly changed the color schemes of all the plots with lines for the four eruption seasons.

- Figure 5: Doing the grey shading around zero would be much more helpful, so you can actually see if the black line is still within the noise (like you did in the discussion of Figure 7).

I think it makes more sense to keep the confidence intervals on the calculated quantities here. Generally, in a figure like Fig 9, the gray shading around zero signifies the variability of the control run (i.e., the zero line is like the “anomalies” of the control). But for Figs 5 and 10, its not the variability of the control we're interested in (e.g., the variability of the control's AOD is very very small), its the range of possible values of the difference between the different eruption months. When this difference is indistinguishable from zero, then the gray shading will cross the zero line. That is what is shown, and I think makes the most sense.

- Figure 10: This figure suggests that you have aerosols piling up against the vortex. Is that what's happening? Then what makes them fall out? A discussion of this would be nice.

There is some discussion of the “pile up” against the vortex in Sec 4.2, but as the phenomenon doesn't appear to have a very strong impact on the sensitivity to season, we haven't focused too intently upon it here. We do plan to examine the process more in a future study, especially in regard to its impact on the deposition of aerosol to the ice sheets.