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 red, our responses in black.

General comments

1) Page 22448, Line 13: You state that the model has no QBO. I think for the scope of your study it is fine to use a climatology; however I feel you should state the limitations of your approach a bit better. What differences (if any) would you expect in terms of e.g. aerosol size distribution, AOD, sulphate deposition, etc? Maybe, you could discuss (qualitatively) and contrast your method to the studies that looked at the impact of the QBO?

We expect that since the QBO modifies the tropical upwelling and meridional transport of the QBO, it would play a role in the poleward transport of volcanic aerosols, as has been shown in the past. Based on our results, we can also hypothesize that, like season of eruption in our simulations, the phase of the QBO could affect the tropical vertical distribution, and hence effective radius and lifetime of the aerosols. We have added a brief paragraph in the conclusions outlining this potential conection between our results and the QBO, although we point out that future studies with a coupled aerosol model which produces an internally generated QBO would ideally be used to investigate this possibility, but to this point we are unaware of such a model.

2) Page 22455: Your study suggests that a January eruption (of Pinatubo-magnitude) results in the largest cumulative AOD when compared to an eruption commencing in any other month. You do explain you findings by means of the smaller effective radius resulting from a January eruption. However, I do struggle in understanding your explanation of the smaller effective radius by means of the sulphate burden. My main concern arises from the units of kg per km2 that you assign to the term “sulphate burden” (see for example Fig. 6). According the IPCC the term “burden” is defined as “the amount of a gaseous substance or particulates in the atmosphere at a given time” with units of mass of substance (e.g. kg of SO4). Can you please explain what you mean by “burden” and why it has units of kg per km2? Do you mean “mass fraction” or “loading”.

It's true, we have been careless with units and terminology here. We have converted our kg/km^2 “burden” to a true burden by multiplying by the area of the earth. We now see a maximum of ~8 Tg(S) of sulfate, which corresponds nicely to the 8.5 Tg(S) of SO2 (i.e., 17 Tg of SO2) injected (some sulfate is lost by the time all SO2 has been converted to SO4). We have also corrected the labels for (and references to) what is correctly the sulfate mass fraction at 10 hPa shown in in Fig 6d.

To me it sounds as the key differences is the vertical distribution of the sulphate mass; you do explain that with tropical upwelling; however you also show that an October eruption shows the lowest “sulphate burden at 10 hPa” – why is that?

In Chapter 4 of the SPARC CCMVal 2010 report, tropical upwelling from a number of coupled chemistry models is diagnosed and compared to that based on reanalysis data. It is shown there that the majority of models (along with the reanalysis data) have a strong peak in tropical upwelling during NH winter (DJF), with a broad minimum over the rest of the year. The minimum upwelling is seen for many models in NH summer (JJA), but for some models it is in NH fall (SON). Based on our results, one might expect that in our model, the minimum upwelling to occur in SON. However, it should be noted that the upwelling is also dependent on latitude, and that this dependence is itself seasonally dependent, as the region of tropical upwelling oscillates about the equator with time. With these complications in mind, it is obvious that in order to fully understand the differences in aerosol evolution between each of the eruption months, one would need to fully characterize the nature of the tropical upwelling in the model in terms of seasonal and spatial variations, and assess in detail its impact on the aerosol microphysics. This is beyond the scope of this paper, but something we hope to look at in future work. But for the present work, the strong annual peak in upwelling in NH winter seen in both observations and model results is, we feel, a very likely mechanism for the notably different aerosol evolution we see for our January eruption simulations here.

We have reworked the text of the manuscript in light of the discussion above, and we thank the reviewer for the insightful question.

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

Also, is the above also true for the E700 eruption?

Yes and no. Once again the maximum sulfur mass fraction at 10 hPa (i.e., vertical transport) is seen for January eruptions, but the values for a July eruption are almost as large. While the natural season cycle in upwelling may well still be important, the very strong aerosol heating in the tropical stratosphere which occurs in the E700 experiment very likely has a strong impact on the vertical upwelling. There is still some seasonal dependence, shown by Jan and Jul eruptions reaching higher altitudes, however, we will need to examine the dynamics and aerosol properties in more detail (in a future work) to understand why.

We have modified the text in our discussion of the E700 global mean AOD, and chosen not to guess at the mechanisms at play in the Jan and July maximums. Rather, we make the conservative conclusion that in addition to the seasonal cycle in upwelling (as seen for E17), aerosol heating also plays a role in controlling the vertical distribution, effective radius, and global mean AOD, especially for very large eruptions.

3) You use a aerosol microphysics model; but you do not discuss any aerosol microphysical processes in detail – I am wondering whether looking at the differences in aerosol microphysical processes could help to explain the lower effective radius seen for a January Pinatubo-magnitude eruption? It it really down to dynamics only?

Yes, the idea is that the dynamics impacts the microphysics, i.e., that SO_2 at higher altitudes creates smaller particles. We haven't looked at the microphysics in detail, it is true. Again, we hope to look at this in the future.

4) It might be worth checking the work by Harris & Highwood (2011) in JGR – given your findings, can you put your results in context? What implications arise?

Harris & Highwood (2011) assess a climate model's temperature response to different magnitudes of volcanic AOD perturbations. To do this, they use a scaled version of a “Pinatubo-like” AOD latitude/time pattern. Our present study can be seen to address the question of whether the AOD pattern used by Harris & Highwood (2011), or using arbitrary months of eruptions in climate reconstructions (e.g., as in Gao et al., 2008) might bias results. Our results imply that the AOD pattern is not critical to obtain realistic surface SW anomalies for Pinatubo magnitude eruptions, since the pattern of SW forcing is highly affected by clouds. However, problems might arise as Pinatubo-like AOD patterns are scaled to higher magnitudes, since we saw the AOD pattern in our simulations to change significantly for a near-super eruption, and the sensitivity to eruption season (i.e., to spatial pattern of AOD) to become significant. We have added these valuable interpretations of our results within the context of earlier studies to the conclusions.

Minor comments

- It is hard to see the y-axis scales in Fig. 6. Can you please revise this Figure and increase the axis scales?

We have improved the readability of Fig 6 by changing the y-axes from log-scale to linear scale, and by increasing the size of the tick marks on the axes.

- Page 22446, Line 18, “using prescribed aerosol effective radius”; I think these authors use a prescribed aerosol dry effective radius when initialising their simulation; however the aerosol is allowed to grow after that (depending on relative humidity) – it might be worth checking with these authors; the way it is phrased at the moment sounds like the effective radius is fixed in their simulations.

The humidity in the stratosphere is very low therefore the modulation with the relative humidty are small compared to the troposphere. To be precise we changed the text to: “... performed using aerosols of prescribed dry radius.”

- Page 22446, Line 20, “impact of tropical volcanic eruptions . . .” impact on what?

Text changed to “radiative impact of...”

- Page 22450, Line 13, please spell out AVHRR the first time you use it

Fixed, and also for SAGE.

- Page 22451, Line 1, AOD abbreviation already used earlier, please check

Fixed.

- Page 22451, Line 21, remove () around SW

Fixed.

- Page 22456 Line 1 “higher mean height of aerosol burden” – see my comment above, I really struggled to make sense of this when I first read it

Changed to “a higher peak in the aerosol vertical profile”.

- Page 22461, Line 18, say January eruption or something like that instead of “certain season”

At this point in the paragraph, we are trying to make general statements about the impact of season of eruption, irrespective of magnitude. Since we see max global mean AOD for July eruptions for E700, but January for E17, its hard to make a general statement without the vagueness of “certain season”. We have hoped the “concrete example” which follows the general statements would make the point clear.

Additional References:

Harris, B. M., and E. J. Highwood (2011), A simple relationship between volcanic sulfate aerosol optical depth and surface temperature change simulated in an atmosphere - ocean general circulation model, J. Geophys. Res., 116, D05109, doi:10.1029/2010JD014581.