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Interactive comment on "Chemistry-climate model simulations of the Mt. Pinatubo eruption using CCMI and CMIP6 stratospheric aerosol data" *by* Laura Revell et al.

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This manuscript describes coupled chemistry climate model simulations utilizing two different volcanic forcing data sets, and explores differences in the simulated responses to the volcanic forcing, including stratospheric temperatures, circulation and ozone. The experimental set-up is clear, and the results show that the stratospheric temperature anomalies produced by the newer CMIP6 volcanic forcing reconstruction are in closer agreement to observations. The paper also provides information regarding the construction of the CMIP6 forcing data, which is presently not available elsewhere.

I find the work to be well within the scope of ACP, and the conclusions to be in general

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Discussion paper

well justified by the results shown. I have a few minor comments I encourage the authors to consider before publication.

General comments

1. The results of the study focus almost exclusively on the tropics. Extratropical ozone changes in the SH are mentioned in passing, but there is no analysis of extratropical NH ozone changes (which seem to be positive for both forcing sets, inconsistent with observations), or polar temperatures (despite large changes in the forcing at high latitudes), etc. However, the title is very general, and some statements throughout the manuscript could be construed as applying to the stratosphere as a whole, rather than just the tropics (see specific comments below). I suggest the title be changed to reflect the concentration on the tropics, and some care be taken to be clear about the specificity of the results.

2. The introduction mentions the wide range simulated temperature and ozone responses to volcanic forcing in the CCMVal activity. But since the CMIP6 volcanic forcing data set is nominally an update to forcings used in past CMIP activities, it would make sense to briefly review the simulated responses to volcanic forcing in past CMIPs. Charlton-Perez et al. (2013) show CMIP5 global mean stratospheric temperature changes associated with Pinatubo split into high-and low-top models, and Driscoll et al. (2012) show tropical temperature anomalies, split into groups of models using different forcing reconstructions. Toohey et al. (2014) compare temperature and circulation anomalies from the Stenchikov forcing (basically equivalent to the Sato et al. (1993) forcing used in many CMIP5 models) and the CCMI (SAGE-4λ) forcing.

3. In a few places, the authors draw a direct line of causation from heating of the tropical lower stratosphere and increased tropical upwelling, and in some cases, link this further with increases in extratropical downwelling. While this may be true, there is also evidence of post-volcanic changes in extratropical large-scale wave breaking in observations (Graf et al., 2007; Poberaj et al., 2011) and model results (Bittner et

al., 2016; Toohey et al., 2014). This increased wave breaking should increase transport from the tropics to extratropics, and induce residual circulation anomalies. I think there is still a low degree of understanding on how the related processes of enhanced wave-breaking and tropical heating affect stratospheric tropical upwelling, extratropical mixing and downwelling. The issue of causation is not central to this study, so I'm not necessarily suggesting a detailed review of the topic, but I encourage the authors to not oversell the understanding of the mechanistic explanation of circulation changes, for example in the Introduction (p2, l24), results (p7, ll10-13) and Conclusions (p8, l29).

4. Pedantic semantic comment: these are not really simulations of the Mt. Pinatubo eruption (as stated in the title and throughout the document), they are rather simulations of the atmospheric response to stratospheric aerosols resulting from the Mt. Pinatubo eruption. This is of course obvious to many readers, but can be confusing to readers new to the field.

Specific comments

P1, l9: suggest "uses measurements from CLAES (. . .) on UARS, the. . ."

P1, l14: this overestimated heating and ozone loss is specifically in the lower tropical stratosphere. Comparisons of extratropical temperatures and ozone are not shown.

P1, l17: Again, this applies only to tropical temperatures.

P1, l20: This ozone loss is specific to 30 hPa, 15S-15N I believe, and is a peak value I guess?

P2, l2: I think the IR absorption by aerosols was known about, and reasonably well understood before 2013.

P2, l15: The last sentence here is a strong statement, which could use some support from prior work. Son et al. (2010) comes to mind, but there are surely other references that would support this.

P2, l26: This statement is supported by two pretty random references from a great sea of literature. At the very least, an "e.g." is called for, otherwise review articles (e.g., Robock, 2000; Timmreck, 2012) would seem to be a better fit.

P3, l13: I don't think the analysis of tropical lower stratospheric temperatures really constitutes an investigation of "climate".

P4, l13-20: Some more details regarding the correction are needed. It is written that the issue pertains to the "extra-tropical lowermost stratosphere", but later the correction is applied "below 20 km", is this at all latitudes or only in the extratropics? Does the correction increase or decrease the SAD, by roughly how much, and is it seasonally varying? It is written the correction applies to H2SO4 mass, should this not affect IR absorption then, which is roughly proportional to aerosol mass?

P4, l31-: This got confusing for me. If n, r and σ are found in step 1, what are the "remaining two parameters" mentioned in step 2? And with some constructed relationships between k_{1020} and r_{eff} and σ, how can you use these to calculate number density (p5, l2)? I really wonder how one can retrieve 3 pieces of information (n, r and σ) from measurements at a single wavelength, there must be some assumptions that go into this reconstruction.

P5, l16: "...derive heating rates." But also scattering, atmospheric transmission, etc.

P6, l2: The simulations are free-running, but what about the QBO? The temperature anomalies in Fig 4 seem to be oscillating quasi biennially, with the simulations right in line with the observations.

P6, l28-30: This last sentence seems at least misplaced (in this subsection on the aerosol mass comparison), and also not well supported by any results shown here.

P8, l14: Focusing on a single height level always runs the risk of sampling error. Observed temperature anomalies after Pinatubo appear to peak around 20 hPa, slightly higher than most simulations (see Fig 1 of Toohey et al., 2014). Just to be sure that

30 hPa is telling the right (and/or full) story, it would really be great to include a latitude/height cross section of zonal mean temperature anomalies (and perhaps ozone too).

P8, l31: the temperature and ozone anomalies quoted here are specific to locations and times.

P9, l7: suggest "tropical stratospheric temperature..."

Fig 1: It's hard to read anything quantitative from the color scale used in this plot. Perhaps percent difference (CMIP6-CCMI)/CCMI would work better?

Fig 3: Showing the results of the CMIP6 simulations as a difference plot wrt the CCMI forcing is very useful, but on the other hand, it would also be nice to see the results in their absolute values. Many potentially interesting results are hard to glean from only the difference plot, for example, it's clear that the upwelling in the lower tropical stratosphere is decreased in the CMIP6 simulations compared to CCMI, but it's not obvious then what the magnitude of the upwelling anomaly is in the CMIP6 simulation ensemble. Such a plot could (rather easily I assume) be added to the main text or included as a supplement.

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