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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Model simulations of the chemical and aerosol microphysical evolution of the Sarychev Peak 2009 eruption cloud compared to in-situ and satellite observations" *by* Thibaut Lurton et al.

Anonymous Referee #3

Received and published: 13 November 2017

In this study the authors use CESM1(WACCM)-CARMA simulations to show the impact of volcanic HCl on volcanic SO2 life time and on ozone and NOx depletion. Further the authors compare their simulations with IASI SO2, balloon-borne particle measurements, and OSIRIS SAOD. Special emphasis was put on the comparison with OSIRIS data accounting for the instrument's limitations.

Fundamentally the study is sound. I recommend it for publication in ACP following revisions suggested below.

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Discussion paper

Major comments:

page 2 line 33: Although the IASI SO2 retrievals are sound and precise, I'm not convinced that they should be the first choice the estimate injection heights. ACE (Doeringer et al, JGR, 2012), CALIOP (e.g. Solomon et al., Science, 2011), MIPAS (Höpfner et al., ACP, 2013, 2015) and the ground based lidar measurements you mentioned clearly show that a significant part of the Sarychev SO2 was injected above 15 km.

page 5 line 14-16: Although you justify your choice of a Sarychev injection on 15 June only into altitudes between 11 to 15 km in the next paragraph there are also studies demonstrating that a substantial amount of SO2 reached higher altitudes (ACE (Doeringer et al, JGR, 2012), CALIOP (e.g. Solomon et al., Science, 2011), MIPAS (Höepfner et al., ACP, 2013, 2015)). Images of different instruments (e.g. http://sacs.aeronomie.be/nrt) show that there was a significant amount of SO2 injected before the 15th and a very recent study in this journal provides an emission time series that placed the onset of the strongest eruption phase in the afternoon of 14 June (Wu et al., ACPD, 2017). Also Levin et al. (2010) found the onset of the second strongest eruptions on 14 June at 18:50. I suggest taking this into account. Please see also further minor comments on this aspect.

Minor comments:

page 2 line 15: What do you mean by "global visible AOD was enhanced by up to 0.15"? Is 0.15 a factor or the AOD?

page 2 line 19: Please consider also the Arctic, e.g. Tilmes et al., ACP (2008), as the Sarychev eruption that is discussed here affects the Arctic.

page 2 line 24: Can you add references?

page 2 line 31: Only SO_2 and HCl or also ash?

page 3 line 8: Here I'd like to add that a very recent study in this journal found sim-

ulations with a “sedimentation radius” of 0.5–1 μm to match best with observations (Günther et al., ACPD, 2017).

page 3 line 10-15: The r_{eff} derived from ACE remote sensing measurements was also 0.1 – 0.3 μm.

page 5 line 28, 30, 32: What are the uncertainties of the SO_2 burdens? Do they agree within their uncertainties?

page 6 line 1: What is the uncertainty of the HCl injection?

page 6 line 18/19: How did you determine the tropopause? What is the uncertainty of the tropopause altitude?

page 6 line 21-29: How do you justify a comparison with SO_2 column data, while neglecting all injections below 10 km in the simulations?

page 7 line 6: Please provide a valid URL for the STAC data in indicate your last access (for all urls). After a short search I found the following site claiming to provide STAC data, but ended up at blank pages or 404: http://cds-espri.ipsl.upmc.fr/etherTypo/index.php?id=667L=1

page 8 line 5/11: I suggest considering adding the IASI SO_2 retrieval threshold information and its altitude sensitivity range to the description of the data set in Section 2.2.

page 8 line 8: How do you know that this is due to SO_2 injected before the 15 June?

page 8 line 11-16: Which model output time did you use for the comparison? The same as the measurement time of each orbit?

page 8 line 24 - page 10 line 6: This part was confusing. I’d suggest reordering and rewording. E.g. present your simulation results first, second your simulation results but with IASI detection threshold, third Haywood model and IASI data. Also consider moving the information on the IASI SO_2 retrieval threshold to Section 2.2.

page 9 figure 1: Would your comparison improve if you use 18:00, which is right in the middle of the post-meridiem period, instead of 00:00 model output? What do you mean with “this precise IASI retrieval”? particular? I suggest reducing the number of colors in this figure. I cannot distinguish the many shades of red, blue, and green in the figures. I assume that 7 distinct colors are enough. This type of figure I’ve just seen in Wu et al. 2017 for a comparison between AIRS data and model output. I suggest a comparison.

page 11 line 1. What do you want to say? Do you mean all model runs or only the “unadjusted” model runs?

page 11 line 2-5. This sentence is confusing. Please clarify.

page 11 line 13/14: Do you mean the maximum on 0.9 Tg here? Please clarify.

page 11 line 15-page 12 line 2: Jumping between your results and the findings of Haywood confused me. Consider presenting your results first and compare then with the results of Haywood.

page 12 table 2: What is the significance of your e-folding time? All results are presented as integers, but the one for your model run with HCl and IASI detection threshold says 11.5 days.

page 12 line 16: Can you quantify the “good general agreement”? Is the agreement in the upper panel of Figure 4 within the error of the OPC and the uncertainty of your volcano-off simulation?

page 13 figure 4: I suggest to add the measurement errors (that are given in Section 2.2) to the OPC data. Without them it is really difficult to judge if the simulations and observations agree quantitatively within their errors on the logarithmic scale. Further, can you indicate the uncertainty range of the simulations?

page 14 line 2: In Section 3.2 (page 10) you mentioned that your model is too disperse. It seems to me that here it is the most likely source of error. I’d suggest to compare with your IASI observations as in Fig. 1 and add this to Figure A1. You could also compare

with Wu et al., ACP, 2017 and discuss.

page 14 line 13: How do you know that the CN particle mode has ever been different from the volcano-off simulation over Laramie?

page 14 line 25-30: You discuss the discrepancies between the measurements and your model results at altitudes below and above your injection height. I assume that not injecting SO_2 below 10 km and above 15 km also contributes to the differences. I suggest to add this to the discussion.

page 15 figure 5: Why are you using different colors for similar size bins (e.g. top: 885 nm is orange, bottom, 850 nm is red)? Why are there 3 size bins below 440 nm on 18 May 2010 but the other profiles start with 440 nm? I suggest to merge the lowest size bins for 18 May to make it comparable to the measurements in August. Please indicate the measurement and simulation uncertainty. On a logarithmic scale it's really hard to tell if there is a good agreement. Please also optimize the colors. In the 2 bottom panels there are two indistinguishable green lines.

page 16 line 1: Has there ever been a comparison between STAC and OPC that might explain the difference?

page 16 line 1-3: Does your model simulation suggest coagulation, condensation and sedimentation? What about transport to lower latitudes and dilution between August and November? What is the sedimentation speed and distance of e.g. 0.5 μm particles over 3 months? Shouldn't they show up at a lower altitude in the OPC data then? Please substantiate your explanation.

page 16 line 32/33: How do you estimate the local tropopause? Do you use the thermal or dynamical tropopause? Which PVU threshold? What do you use at the pole/equator? What is the uncertainty of your tropopause? Please provide details.

page 17 figure 6: As I understood, the main purpose of this figure is to compare the STAC measurements with the model simulations, I suggest to select a smaller range

on the y-axis so that it fits to the STAC data(e.g. dN 1e-3-1e2, dV, 1e-15-1e-12, dV 1e-21-1e-17). In the present figure I can only see that the no-volcano runs do not fit. Further, I consider error bars on the STAC measurements helpful.

page 18 line 4-6: Why don't you rely your $Z_{min}(\Phi, \lambda, t)$ not solely on your analyses of 2009 shown in Fig A2? I consider a 2009 histogram more appropriate than a 2012 histogram with corrections.

page 18 line 6. Ok, it's dynamical tropopause. Which PVU is your tropopause? What do you use in the tropics/at the equator? 380 K? Thermal tropopause? What is the accuracy of your tropopause? Please provide details.

page 28 Figure A2: Please extent the y-Axis to accommodate all data points and provide information on the color code. Does the black line mean that you used only a $Z_{min}(\Phi)$ for your model degradation and not a $Z_{min}(\Phi, \lambda, t)$ as described on page 18? To me it seems that there is some seasonality. Would your analysis improve if you used a $Z_{min}(\Phi, t)$? At high and very low latitudes (0-10N, 50-90N) the minimum altitude threshold seems to be below the median of the data points. What is your reason not using the median?

page 18 figure 7: I suggest checking seasonality for your degradation. Unfortunately I cannot tell from Fig. A2 in which months your degradation altitude fits best, but at high latitudes you have a good agreement in October, November, April, and May and at low latitudes (0-20N) you have the yellow (day 250-350) and blue (day 425-525) features that might coincide with your data points above and below your $Z_{min}(\Phi)$. Please clarify.

page 20 line 1-3: I don't understand what you mean. Please detail where and to what extent the anomalies in Fig 8 agree better than the SAODs in Fig 7. Except from the shaded area indicating OSIRIS measurement gaps in the polar region in the middle panel of Fig. 8 I cannot see obvious additional information. Figure 7 already shows impressively that OSIRIS misses a substantial fraction of lower stratosphere sulfate aerosol.

page 20 figure 9: Why are you showing 550 nm extinction here? It is not used anywhere else, all other OSIRIS data is presented for 750 nm. Please clarify and consider using less colors (7 might be sufficient) in the bottom figure. Some are indistinguishable.

page 21 line 3-5: Please specify what you mean with “strongest measurement-biases shortly after the eruption”. Do you mean OSIRIS high Zmin, or its saturation, or its rather coarse sampling that might miss local maxima of the plume filaments shortly after the eruption? Perhaps you want to compare with Günther et al. , ACPD (2017) Fig. 6, which is similar to your Fig. 9, but with different model and satellite data.

page 21 table3, line 12-19: For which purpose do you present e-folding times from other studies? They are not discussed here.

page 22 line 18/19: Please add a reference for those removal processes.

page 22 line 25-28: You might want to include the r_{eff} retrieval by Doeringer et al. (2012), who found 0.1-0.3 μm for the Sarychev, into your discussion.

page 23 figure 10: Please reduce number of colors. There are too many indistinguishable shades of red and green.

page 22 line 34: Which chlorine and bromine species do you mean?

page 23 line 2: Please note, the washout is not necessarily as efficient as in the Pinatubo case (von Glasow, Chemical Geology, 2009).

page 23 line 20 - page 24 line 2: I did not understand this sentence. Please fix it.

page 24 line 2-5: I did not understand this sentence. Please reword and provide a reference.

page 24 line 5: I suggest starting a new paragraph here to clearly differentiate between heterogeneous reactions on aerosol particles and PSC particles. Isn’t HCl the main reservoir of Cl and not ClONO2? There is HNO3 uptake by PSC particles that sediment out and hence lead to denitrification. I suggest to explicitly mention PSCs in

this process and to reword this sentence.

page 24 line 16: is 5% versus 7% a and 50 versus 60% a significant difference in your model? What is the uncertainty?

page 25 line 8: I did not find your results convincing that an injection altitude of 11 to 15 km is realistic. I'd rather interpret your results that there are discrepancies between simulations and measurements above 15 km (see Fig. 4). Further your lower SAOD in the degraded model data compared to OSIRIS (Fig. 7 and 8) may be a result of not accounting for the SO_2 injections into altitudes above 15 km that have been observed by several independent measurements.

page 25 line 9-11: Comparing your plume simulation in Fig 1 to IASI data and to the model simulations and AIRS data in Wu et al., ACP (2017), I find some shortcomings in this approach, which become also visible in several details and interpretations on which I commented before. Hence, I suggest to rephrase this sentence and add some discussion on potential errors due to the injection assumption.

page 26 line 29-31: In which respect is this statement different from the findings in Ridley et al. (2014)?

Technical Suggestions
page 1, line 19: confirm

page 5 line 5: please sort references chronologically

page 5 line 7, 8: references for Sindelarova and Kettle are missing

page 6 line 22: just write IASI

page 6 line 31: Define abbreviation at first usage only. Please also check in other places e.g. for OSIRIS page 7 line 20.

page 7 line 5: What does StraPolEte stand for? Is there any reference to the "AEROWAVE" project?

page 10 figure 2: In the figure the IASI retrieval is green, but the caption says light blue.

page 11 line 10: "... Section 3.6." Please start a new paragraph here.

page 13 figure 4 caption: "solid" instead of "full" line

page 14 line 7: "... observations." Please start a new paragraph here.

page 14 line 12: Do the profiles only appear to be close to each other or are they close?

page 18 line 15: Do you mean: A comparison for these months is therefore impossible?

page 21 line 7: (9) = (Fig. 9)?

page 21 line 21/22: Please replace "... elsewhere, notably ..." by e.g. and refer to Table 3.

page 22 line 17: ... , which

page 22 line 22: material

page 22 line 29: ...are thus a major ...

page 22 line 33: "... investigating the impacts of modern day eruptions on stratospheric ..." Please fix this sentence.

page 24 line 30: Please write "... over one day of eruption ..."

page 25 line 5: Please write "... suggest that the effective radius becomes ..."

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