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Interactive Comment

Interactive comment on "Radiative and climate impacts of a large volcanic eruption during stratospheric sulfur geoengineering" *by* A. Laakso et al.

Anonymous Referee #2

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Summary

This paper describes model simulations of stratospheric aerosol, and assesses the simulated impacts of volcanic sulfur injections under background conditions and during continuous sulfur injection for solar radiation management (SRM). The general topic is of potential practical interest in the case that SRM would actually be used in the future, to gain an idea of the potential changes in the resulting impacts of volcanic eruptions under SRM conditions. More generally, the paper introduces the SALSA stratospheric aerosol model configuration, and the experimental set-up allows for a short validation of the module with comparisons to the Pinatubo eruption, and an interesting perturbation

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Interactive Discussion

Discussion Paper

experiment which tests in a novel way understanding of the behavior of a stratospheric aerosol model.

However, as presently written, the conclusions reached by the paper are not well supported by the results.

General comments

The main conclusion of the paper is that the impacts of a volcanic eruption would be “significantly different depending on whether the eruption occurs during SRM deployment or into a clean background stratosphere” (pg 21857, lines 17-19). In fact, the results could be used to argue the opposite. In Fig 2, the peak sulfate burden reached after a volcanic eruption during continuous SRM (“SRM Cont”) is identical to the sum of the individual results of SRM alone and an eruption alone. (There are some differences in burden between months 10 and 20, but these seem small compared to the peak burden, and the significance of this difference is impossible to judge from single ensemble members). Similarly, global mean temperature and precipitation responses shown in Fig 4 are consistent (within error bars) for the “Volc” and “SRM Cont” experiments, which implies that the background state has little-to-no impact on the climate response to a volcanic eruption.

The discrepancy between the author’s conclusions and those above originates from the definition of what exactly “during SRM deployment” means. The paper puts most of its focus on simulations of a scenario where SRM is employed before the eruption, but suspended upon the occurrence of the eruption. (The authors argue that this the most likely scenario, but one could counter-argue that the politics of SRM deployment would be so complicated that it is near impossible to state with any confidence what might be most likely.) Since the difference between the results of the “Volc” and “SRM Cont” simulations are small (as argued above), the conclusions described in the abstract and conclusion section are not due to the existence of SRM before the eruption, but to the sudden suspension of SRM at the time of the eruption. This (as the authors

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acknowledge within the manuscript) is a trivial result, and very specific to the scenario constructed. Unfortunately, this is not well communicated in the text of the abstract nor the conclusions, and I think many readers could understand that the study finds a significant decrease in the volcanic response under continuous SRM. A readers confusion might be justified, since a comparison of the simulations of volcanic eruptions under background and continuous SRM is the more natural experiment, where only one parameter is being changed between the simulations. The authors' choice to focus primarily on the SRM scenario with a sudden stop therefore increases the chance for misinterpretation and seems to oversell the role of aerosol size in the radiative and climate impacts of volcanic eruptions, at least for the scenarios explored here.

The paper describes differences in regional precipitation patterns between the different scenarios. The significance of the precipitation anomalies in the three volcanic scenarios is weak outside of the tropics, due likely to the high natural variability and the relatively short runs and few ensemble members. Therefore, the conclusion that precipitation anomalies after an eruption during SRM would be different than during background conditions is not well supported by the results, since it is not shown whether the differences between the scenarios is significant. Furthermore, a major caveat is that if SRM is ever employed, it will be under elevated CO2 concentrations, which are not accounted for here. The combination of SRM and CO2 forcing would lead to different total temperature and precipitation anomalies than shown in panels (a) of Figs 5 and 6, therefore the impact of volcanic forcing (e.g., on the meridional temperature gradient) would likely be different in the real world case (with SRM and CO2 forcing) than in these simulations with only SRM.

Appendix A, which validates the model results compared to observations of the Pinatubo eruption, serves a valuable purpose in regards to the paper and is justifiably included as an appendix to sharpen the focus of the main text. If anything, Appendix A would benefit from more material, a comparison of simulated and observed AOD seems to be missing. On the other hand, Appendix B contains results which seem to be outside the scope of the rest of the paper, and only briefly described and explained with little more than speculative reasoning. Either these results should be incorporated into the main results of the paper and more thoroughly explained, or removed from the manuscript.

Specific Comments

P21838 L10: “decay” might be a better word choice than “recovery”

P21839 L23: “very likely” seems a strong statement given uncertainties in eruption return rates and the term of (hypothetical) SRM. For instance, between Katmai and Pinatubo passed almost 80 years. So it seems one could easily do SRM for 30-50 years and not have a major eruption.

P21840 L8,11: no ‘s on Max Planck Institute

P21840 L10: The Niemeier and Stier references don’t pertain to HAM-SALSA specifically.

P21841 L10: There is a stratospheric version of HAM. It might be explained why you have not used this version.

Sec 2.2: It is not clear in the experimental description (esp. for the SALSA simulations) how many ensemble members have been run, and whether the results shown are ensemble means or single ensemble members.

P21845 L14: How well constrained is the 8.5 Tg S for Pinatubo?

P21846 L24: “yr**-1” should be removed?

P21847 L14: The SW forcing seems to oscillate a fair degree, so it might be wrong to call it “steady state”. . . at least, you might clarify that the -6.22 Wm**-2 is an average value.

P21848 L14: Here we learn that Fig 2 shows a single ensemble member. Why not take

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the ensemble mean (and show the variability of the ensemble with error bars)?

P21849 L1-5,21-30: The significance of the difference between the “SRM Cont” and the sum of the “Volc” and “SRM” experiments is not convincing, with only single ensemble members shown and no error bars, see major comments.

P21851 L24: Another example of a statement which could be easily misinterpreted, since the SRM scenario used here includes a suspension at the time of the eruption, which is not readily clear from the immediate context.

P21853 L13: Driscoll et al., 2012 shows that models don’t produce the winter warming pattern.

P21855 L16: This idea has been around for much longer than since 2013, see e.g., Bala, G., P. B. Duffy, and K. E. Taylor (2008), Impact of geoengineering schemes on the global hydrological cycle., Proc. Natl. Acad. Sci. U. S. A., 105(22), 7664–9, doi:10.1073/pnas.0711648105.

P21856 L21: In the SE Pacific yes, but it’s not clear if this opposite response holds for “most of the tropics”.

P21858 L28: This explanation for the widening of the ITCZ in the simulations seems inconsistent with the understanding of the observed widening of the tropical belt being related to a decrease in the meridional temperature gradient. See Adam, O., T. Schneider, and N. Harnik (2014), Role of Changes in Mean Temperatures versus Temperature Gradients in the Recent Widening of the Hadley Circulation, J. Clim., 27(19), 7450–7461, doi:10.1175/JCLI-D-14-00140.1.

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