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Comment on acp-2022-514

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Referee comment on "Interactive stratospheric aerosol models' response to different amounts and altitudes of SO_2 injection during the 1991 Pinatubo eruption" by Ilaria Quaglia et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-514-RC2, 2022

General comment

This study uses a multi-model ensemble of global aerosol simulations performed within ISA-MIP HErSEA to assess the effect on volcanic stratospheric aerosol of uncertainties related to the SO_2 injection (height and amount) by the 1991 Pinatubo eruption. As a main result, the study identifies large inter-model differences as well as common limitations, particularly related to a too strong simulated meridional transport of aerosol in the northern hemisphere, that results in a faster simulated decay of the post-eruption enhancement of the stratospheric aerosol layer compared to observations. The study also highlights how different SO_2 injections are required for different models to "best match" observations (and how these vary for the chosen observed parameter as well).

I have only minor comments on the study, which I found overall well-conceived and well conducted. My evaluation of the study considers it as a "MIP" study, so based on results from a predefined protocol-driven set of experiments. I recognize that some aspects of the study remain open to discussion and thus require further investigation (the role of the Cerro Hudson and the role of ash emission as far as comparison with observations is concerned, but also the causes of the found inter-model differences). This calls for a retrospective on the HErSEA protocol (was it effective or has any weakness emerged?) and for a discussion about the implications of the findings for the original purpose of the experiment and for the purpose of ISA-MIP in general (this is mentioned for instance in lines 61-62 of the manuscript). As another general comment on the study, I encourage a more explicit discussion (if not presentation) of within-model uncertainties, intended as differences between realizations of an experiment with the same model. These might be negligible in most cases, but this is not stated and, instead, there are occasions where illustration of results from individual realizations reveals distinct behaviors (for instance in Figure 3). I have some more specific comments on this below.

I have also just a few minor editorial comments, as in my opinion the manuscript is overall well-structured and well written. As a general comment, I felt there is a difference

in style between sections 3.1 and 3.2 (just focused on presentation of results) and section 3.2 (which mixes introduction, results and discussion, especially from the paragraph starting on line 374 onward). Maybe the authors could consider some homogenization, for instance by moving some of the more discussive parts of section 3.2 in section 4.

Then, the manuscript could serve as a reference for future analyses based on the HErSEA experiments, especially as far as final choices in the experiment setup differ from the original protocol. In this sense, it may be worth to provide any guideline provided for the generation of the ensemble, and how this was actually done for each model. I see that for most models this is not reported, while in the other cases it is not clear if the parameter perturbation was maintained for the whole simulation or just for some initial steps (ECHAM6-SALSA).

Specific comments

Line 44-46: maybe it is worth mentioning here that a possible cause of the inter-model discrepancies in radiative fluxes are minor differences in forcing implementation.

Line 58: proposed cooling is unclear, maybe "a certain cooling target"?

Line 61: to me initial conditions refer to the initial state of the system as a whole, so more than the "initial conditions of SO_2 injection" that is implicated here. I recommend the authors to always explicit this to avoid confusion. Also, other "initial conditions" such as the phase and amplitude of the QBO may be relevant here and deserve some explicit consideration in the presentation and discussion of results (see also comments below).

Line 161: by climatological do you mean "observed" values during the simulated period?

Line 267: is this related to the QBO phase? There seem to be little information regarding this aspect in the presentation of results and discussion. If the model spontaneously produces a QBO, it would be instructive to know how QBO phase and amplitude compare with observations. In this regard, one of the realizations of ECHAM6-SALSA is clearly different from the other two, especially in terms of rms (see Figure 3): what is the reason behind this difference? I wonder if the ensemble mean is truly representative for this model at least. This might motivate some focus on individual realizations as well (or on sub-ensembles).

Line 354: why not testing the differences? Even if the sample size is low, a Mann-Whitney U test, for instance, could provide you a basis for a stronger statement here.

Figure 8: especially for the Laramie comparison, given the punctual location of the datum, would it make sense to consider more explicitly the individual realizations instead of just the ensemble mean in order to include uncertainties linked to the "internal component" of atmospheric circulation? I understand that also due to the vertical averaging this might still lead to small differences across realizations, but it would be important to have some estimate of the uncertainty anyway (for instance an error bar at the peak value of the profile). Also, the error bar for the OPC data is not defined.

Technical corrections

Line 332: typo (produces)

Line 391: twice especially, maybe the second can be skipped

Line 425: at analysing

Line 574: typo Higher

Figure 3: I had some difficulties tracking the colors. I suggest using a more varied color palette for the different experiments

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