Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-344-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluating the simulated radiative forcings, aerosol properties and stratospheric warmings from the 1963 Agung, 1982 El Chichón and 1991 Mt Pinatubo volcanic aerosol clouds" *by* Sandip S. Dhomse et al.

Anonymous Referee #2

Received and published: 28 May 2020

This manuscript evaluates UM-UKCA simulations of the Agung, El Chichon, and Mt. Pinatubo eruptions and presents conclusions on the SO_2 injection amount that provides the best comparison with observations.

Overall, I think this is an interesting and well written manuscript. The evaluation is detailed and well presented, the graphs are mostly clear, and the discussion is well structured and relevant. The introduction is informative and gives a good overview of modeling and observational constraints.

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Discussion paper

I would see this manuscript more fitting to GMD, rather than ACP, but in any case I think this manuscript is publishable after minor changes.

Comments

- Line 60: it is correct that the stratospheric aerosol load was enhanced in both hemisphere , but one also needs to account for the Cerro Hudson eruption that increased the aerosols in the southern hemisphere shortly after the Pinatubo eruption. There is a comment about this later in the manuscript, but I think it would be useful to mention this here, too.
- Line 160: 10 years of spinup might not be enough for some slow adjusting variables such as age of air. Did the authors check that the stratosphere was indeed at equilibrium?
- Line 163: Three ensemble members is not many. Jones et al (2016, doi:10.1002/2016JD025001) showed that the dispersal is highly sensitive to the initial conditions. It would be useful to add, at least in the supplementary material, results from each of the ensemble members, to understand how the latitudinal dispersal varies within an ensemble.
- Line 163: The different injections all have the same altitude, but the injection altitude is also a degree of freedom. The chosen injection altitudes are all above the tropopause, but an larger injection with a lower boundary in the UTLS could deliver similar results. I understand that the setup of this experiment was dictated by SSiRC, but it would be interesting to comment on the importance of the vertical distribution of the injection.
- Line 165: Not sure what you mean with “for simplicity”. For simplicity of set up or for simplicity of analyzing the results, as it reduces the degrees of freedom?

- Line 225: It is not really correct that satellite measurements constrain particle size. Some satellite instrument provide the Angstrom coefficient, which is a proxy (not a measurement) for size. The angstrom coefficient depends on the size of the particle but also on the composition of the particle and hydration.
- Line 250: Mann et al (2019b) and (2020) are conference abstracts. Does ACP allow them as references?
- Line 277: Larger injections produce a stronger upwelling (which push toward a longer S lifetime) and larger particle radii (which push toward shorter e folding time). Do your result imply that the net effect is driven by the particle size, rather than the changes in upwelling?
- Line 295: Is there a published paper or report that documents the changes brought up by increasing the resolution? Maybe something was published when the model with higher resolution was released?
- Line 306: How is stratospheric AOD calculated? Is aerosol extinction integrated above the tropopause or above a fixed altitude?
- Line 310: I'm confused by this. Are the authors referring to Fig.1, when they write that Pin20 best matches the satellite observed SO_2 estimates? Pin20 is the one that compare the worst with HIRS.
- Line 313: I am not sure it is fair to say that Pin10 has the best agreement. All of them, including Pin10, overestimate the peak sAOD in the tropics. 18 months after the eruption Pin20 seems actually to do better. A similar statement requires a metric such as the globally averaged root mean square error. I generally find the qualitative comparison a weak point of this manuscript. It is very difficult to judge which simulation is performing best just by looking at the figures.
- Line 344: Cerro Hudson is at 45S, 12 km could be above the tropopause.

- Line 390: I think Pin20 should be included in Fig. 4. Even if high biased, it is interesting to see how the effective radius scales with injection burden.
- Line 392: I am confused by this sentence. Pin20 is not shown, and between Pin14 and Pin10 I don't see any clear difference. There is a need of some kind of metric, such as mean error. Judging from the current plot, both simulations seems to perform pretty poorly when compared to the CMIP6 dataset (if that is a valuable benchmark)
- Line 405: I am not sure where to look to see this. Please specify latitude and months of the part of the plot that you are commenting on.
- Line 598: Figure 13, not 12, right? Also, take out either "as" or "hence"

Minor comments and typos.

- Line 3: aerofsol
- Line 7: here and in several other instances in the text the "2" of SO2 is not written as subscript
- Line 112: "to to"
- Line 182: closing parenthesis should not be there
- Line 269: "Applyig"
- Line 329: "disussed"
- Line 386: "model IS not resolving"

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