



EGUsphere, referee comment RC2
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Comment on egusphere-2022-401

Anonymous Referee #2

Referee comment on "Climate response to off-equatorial stratospheric sulfur injections in three Earth system models – Part 1: Experimental protocols and surface changes" by Daniele Visioni et al., EGUsphere, <https://doi.org/10.5194/egusphere-2022-401-RC2>, 2022

The authors compare the output of geoengineering simulations performed with three Earth's system models (1 ran with two different aerosol schemes) to determine the difference in AOD, temperature, and precipitation response produced using the same injection of SO₂. The authors provide an exhaustive comparison of these quantities (especially AOD and temperature, less so precipitation) and attempt to provide a hypothesis about the reasons for discrepancies.

Generally, I have found this article clear, with a good choice of figures, but the complete lack of observations limits its impact. Of course, I am aware that there are no observations of geoengineering, but variables to evaluate, for instance, the isolation of the tropical stratosphere or the background (non-SAI) AOD can be evaluated against observations. Introducing observations would allow understanding which model has a more reliable representation of transport and dynamics, as well as of background aerosol and sensitivity to changes. I understand that this evaluation against observations is not the focus of this paper, but it has been probably (hopefully?) done in other articles and the main findings could be reported here. Otherwise, the main message of this paper is "the models differ", which is for sure correct but not particularly telling unless we can understand whether all of these models produce equally possible outcomes or if one is less reliable than the others.

Secondarily, I am not sure if the OMA experiment has been set up correctly. I couldn't find anywhere how the aerosol radius was chosen. Is it the usual radius used for tropospheric aerosol? It seems like most of the differences between OMA and MATRIX result from a much smaller aerosol radius than the other models. The authors should have first run an experiment with MATRIX, calculated the resulting effective radius, and set up OMA to have that effective radius. As it is I am not sure about the significance of the OMA experiment.

Specific comments

Section 2.1-to 2.2: I suggest harmonizing the three model descriptions. CESM2 has comprehensive stratospheric chemistry and simplified tropospheric chemistry, what about GISS and UKESM? GISS only mentioned heterogeneous chemistry, UKESM doesn't mention anything at all. I would at least mention if UKCA is bulk, modal, or sectional and if it's coupled to the chemistry. I know they are described better below but all three descriptions should have the same format.

Line 145: ". Condensational growth leading to a transfer between Aitken and Accumulation modes is also treated differently than in the other two models" differently how?

Table 1: I imagine that the GISS bulk model also assumes a size distribution, for instance, to calculate the optical properties and that the 0.3 um is the modal radius of the fixed size distribution. Is that the case for OMA (If so, a standard deviation must be specified for the prescribed mode) or does OMA really prescribes that all particles are 0.3 um? Also, I would add the aerosol effective radius that is simulated by the three models with microphysics. Lastly, how many ensemble members have been performed? I don't think I have found it anywhere.

Line 149: I'd specify the diameter here rather than the radius, to avoid confusion with the table where the diameter is specified.

L165: I am not sure I understand why choosing 22km over 25 km would make it easier to inject in one grid box. Also, it is not clear what "same grid box" refers to. Same across models (I suspect it's not because they have different layers)? Same in time? I am confused by this paragraph.

L 188: I do not understand the goal of the second half of this paragraph, starting from Line 186. Is the point to say that the authors don't care about the fact that the same injection leads to very different AOD? I don't agree with including a sentence like this since it is a pretty fundamental conversion that models should agree on. Rather than this, an attempt should be made to explain why there is a difference. Is it because the SO₄ removal is less efficient (maybe the particles are smaller in GISS bulk than with explicit microphysics) or because of the different aerosol optical properties due to the different sizes? Is it possible to include the effective radii calculated in all models, to see how they compare with each other and with GISS bulk, as well as the SO₄ burden? This is partly answered in Fig. 4, and it would be good to mention it here.

L 210: I imagined the models must have been compared to observations at some point. It would be helpful here to give a description of how each model compared to observations with respect to basic stratospheric circulation: for instance, is UKESM known to have a too isolated tropical pipe or to strong vertical transport in the tropical stratosphere? What about interannual variability: are the simulated variabilities similar to the observed ones (I

mean in control simulations that must have been performed in the past).

Fig. 3 needs improvement. The labels and ticks of the color scale are illegible. Since the same color scale is applied to all panels, I suggest using one larger color bar at the bottom of the figure, and also enlarging the fonts on the axis.

Line 237: I think it's panel 4h, not 4g.

L240: as I mention above, I suspect OMA assumes a lognormal distribution with a modal radius of 0.15 micron. If that's the case, the effective radius can be calculated for OMA using relationships between modal and effective radius in lognormal distributions (I think it's in Seinfeld and Pandis, but in any case is also included in Aquila et al. 2012). If that's the case, I suggest adding the effective radius for OMA for comparison.

L253: number of the supplementary figure is missing

L257 radius _IN_ GISS model

L272: there are three "for instance" in three lines.

L286: how many models were included in the multi-model average of GeoMIP G6?

L297 and following: the discussion about tuning is quite vague and can be made more precise by looking into the model setup and seeing which tuning parameters have been changed to keep remedy the low background AOD. Also, I am not sure I understand the reasoning; the background (non-SAI) AOD can be verified against observations, and comparing against observations could tell us whether 0.03 or 0.11 is more reasonable. If 0.03 is too low (compared to observations) the most obvious "fix" to me seems like increasing emissions, or decreasing the radius, rather than changing the temperature sensitivity to aerosols. Also, which tuning parameter would affect the temperature sensitivity to aerosols specifically?

Fig. 6: the letters identifying the panels are missing

L343: one "at" too many

L353: what is the difference between (I_0, I_1, I_2) and (L_0, L_1, L_2)? Generally, I find this explanation a bit confusing. It's pretty clear in Kravitz et al. (2016). I would either make it longer and more explicit, or shorter and more qualitative with an explicit reference to go look in Kravitz et al. (2016). It is a bit difficult to keep in mind the physical meaning of what the text explains. I have also found this section quite disconnected from the previous ones in terms of style and clarity, at the point that it could be moved to a different paper where it would be easier to expand on the meaning of the results.