

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-1003

Ben Kravitz (Referee)

Referee comment on "Stratospheric ozone response to sulfate aerosol and solar dimming climate interventions based on the G6 Geoengineering Model Intercomparison Project (GeoMIP) simulations" by Simone Tilmes et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-1003-RC1>, 2021

This is an important study. It is the first to perform a multi-model comparison of the ozone change differences between insolation reduction and stratospheric aerosol injection, which is much needed. The analyses are clear and well described. I'm overall quite pleased with this study. Nevertheless, I am recommending some revisions.

My biggest concern is that there are some overstatements and loose claims, especially regarding mechanisms. The advantage of a multi-model intercomparison is to learn things you cannot learn from single model studies. If the models are showing vastly different things, you can tie that to individual processes that may or may not be present, albeit doing so can be difficult. If the models are showing similar things (which is often the case here), then you can identify robust mechanisms. My impression of this paper is that sometimes you do this, and sometimes you simply report results and speculate on the mechanisms or reasons for model differences. In the latter case, there is little value added beyond single model studies. I would like to see some more in-depth investigation into why you are getting the results that you are. (See some specific examples below.) I acknowledge that not all models have all of the output you would need to do this. But if the models are showing similar results, you can at least investigate mechanisms in one of the models or cite someone who has already done this.

It's interesting that the main result you found is an increase in TCO, whereas many previous studies found a decrease. I would like to see you talk more about this. What's different between your study and the previous ones?

You may want to consider moving the appendix to supplemental online material. Appendices count in your page charges, but supplements do not (unless I'm operating under some old assumptions about fee structures).

Specific comments:

Line 8: solar insolation is redundant

Line 16: Do you mean 2/3 of the models? There are more than 3.

Line 47: denoted

Lines 171-176: I had a lot of trouble with this paragraph. G6sulfur and G6solar contain changes in greenhouse gases. Also, the point of these experiments was not to reverse stratospheric temperature changes in SSP5-8.5, nor would a reversal of those changes be expected.

Line 178: I was expecting you to talk about the specific differences between the models.

Line 189: I'd like to see more discussion here. The range of uncertainty for models that actually inject SO₂ is much smaller than the total range. Can you explain/show why?

Figure 2c: I know this panel is called correlation, but did you compute a correlation or best fit line?

Figure 3: Why does panel (d) have a different way of denoting statistical significance?

Line 204: Why? I don't think you've convincingly showed this.

Lines 215-216: I'm puzzled by this sentence. If it's not related to geoengineering then that feature would show up in the baseline simulation. Or it's due to natural variability. Either way, you can't make a claim like this without backing it up.

Lines 231-234: Possible reason? Did you investigate this or do you have a citation to back this up?

Line 235: 2015

Lines 240ff: Could be? It seems like you could (should) figure this out.

Lines 301-302: Can you verify this?

Line 304: SSP5-8.5

Lines 311-315: It would be nice if you could say more about this. Why are you getting differences?