

Interactive comment on “Upper tropospheric ice sensitivity to sulfate geoengineering” by Daniele Visoni et al.

Anonymous Referee #2

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Review of "Upper tropospheric ice sensitivity to sulfate geoengineering" by Visoni et al

This manuscript analyzes geo-engineering simulations of sulfur injection in the ULAQ CCM. The paper is generally well written. It suffers from some minor grammar mistakes, but the scientific points are made. I am not sure how valid they are however. I think the methodology may be deeply flawed, since I am not certain that applying another model SSTs, from a model with no ice nucleation and poor upper tropospheric cirrus clouds, is a sufficiently useful method to look at perturbations. I think the resulting dynamical response could just be a model bias when the SSTs from another model is applied, and I fear that this would simply confuse the literature with another dubious single model study. This study needs some major revisions to address these points, and it may not actually be acceptable for ACP given the possible methodological flaws.

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General Comments: 1. I know the authors' first language is not English, and English is not an easy or kind language for the article and plural mistakes they are making, but I would suggest an edit by a native English speaker.

2. As noted below, I am uncomfortable with some of the validation references. They should probably focus on papers, rather than other notes or presentations.

3. Most significantly: how does imposing SSTs from another model with an uncertain response tell us anything about the real atmosphere. You are just shocking one model with another, and you get a response. Why does the no feedback response matter, and how is it relevant? It is stated that some other models get a similar response, but I am not convinced. How would you even know if the model was self consistent?

P1,L2: incomplete sentence "The goal of the present study..."

P1,L15: Relative to the clear sky net...

P2,L10: How is this study different than previous work?

P4,L12: how much of these results are due to just individual model climatologies? Seems like the effects depend on how much homo v. Heterogenous ice a model has, and what a large scale model does to create and maintain cirrus. Why would your study be any more definitive?

P4, L14: The goal....

P4, L15: by including...

P4, L17: ULAQ model description and a reference are needed. Does the description appear later? It does. Add see below.

P4, L18: CCSM-CAM4 needs a description and the acronym spelled out. At least a reference for the simulations. Is there more model description later? Applying the cooling from another model seems problematic: presumably CCSM4 has some of the same feedbacks, operating in different ways?

P5,L1: I don't like that you have created a very arbitrary perturbation that changes vertical motion and transport in a coarse resolution GCM. The result is that I believe your perturbation is very model specific and artificial. I support attempting to understand processes in a model but this whole paper seems very dependent on a single model formulation. I'm not convinced you can or should separate all the affects this way.

P6,L20: I think you need to describe relevant features of the cloud and transport scheme of ULAQ, and the basic features of CCSM4 here. What ice nucleation mechanisms are included and how does the cloud scheme create cirrus clouds? What radiation scheme is used? How do the volcanic emissions evolve? For CCSM4: how do it's volcanic emissions evolve and how is that related.

P7, L1: the inconsistency here I think is problematic for the study. I'm not convinced you should look at this perturbation turning on and off surface temperature perturbations, and expect that the resulting impact on the model has any reference to reality since the system breaks any feedbacks that might modify the surface temperature.

P7,L14: Why should the surface temperature pattern be believed? CCSM4-CAM4 does not have interactive chemistry or a stratosphere. How are the emissions put in? Wouldn't this be different than ULAQ? Especially at high latitudes, impacts are dependent on a stratospheric circulation that I don't think CCSM4-CAM4 does correctly at all. Why not use WACCM4 Geoengineering experiments, which are at least based on a stratospheric model with interactive sulfur emissions.

P7, L25: So how is what you are doing different than Kuebbler et al 2012? Why is this novel or unique?

P7,L34: Updrafts responsible for....

P8,L3: So most of the vertical velocity is heavily and crudely parameterized by gravity waves and TKE. The TKE is probably linked strongly to the temp gradients. Does the model actually use this vertical velocity in advection? Or ice nucleation? Please

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explain what is going on. It is not possible for the reader to understand whether the model formulation is realistic, though I am pretty convinced the perturbation (applying SSTs from another model) is NOT realistic for reasons described above.

P9,L5: Is a 3% change in a parameterized vertical velocity significant? Is 10% significantly different from 3%? From Figure 6, I don't think any of this is significant.

P9,L20: MODIS ice effective radius is not a reasonable product, especially for thin tropical cirrus, unless you have a validation paper that says otherwise.

P10,L3: The mention of what looks like a maximum updraft velocity here is an indication that the ULAQ ice nucleation needs to be better explained.

P12, L1: This section needs to go before all the results presented earlier.

P16, L20: It's not clear to me what fraction of ice formed in situ ($T < 238\text{K}$) is from homogeneous and heterogeneous freezing. It would be useful to note the fraction homogeneous (or heterogeneous). This looks like it is in Figure 10c, but I don't think that is what I am interested in. What fraction of ice is heterogeneously formed?

P21,L5: this is a decent summary that the changes are due to changes in vertical velocity and tropospheric temperatures. How model dependent do you think these quantities are?

P27, L5: Why the 5/8 scaling of the RF results?

P30, L25: How realistic is the decrease in updraft? Is it consistent with the overall circulation? I am concerned that fixing SSTs from another model will not yield a reasonable result, and it is likely to be a single model configuration, not even a general result. How can you convince me and other readers that the mechanism in ULAQ is reasonable, especially since it is imposed from another model and not-interactive, and from a model with no stratosphere.

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