Geoscientific
Model Development
Discussions

Interactive comment on "The Geoengineering Model Intercomparison Project Phase 6 (GeoMIP6): simulation design and preliminary results" *by* B. Kravitz et al.

B. Kravitz et al.

ben.kravitz@pnnl.gov

Received and published: 1 October 2015

We thank the reviewer for the extensive and helpful comments. Addressing them has improved the paper. Original reviewer comments are in plain text. **Responses are in boldface text.**

This manuscript details the Tier 1 and Tier 2 experiments of the Geoengineering Model Intercomparison Project (GeoMIP) associated with the upcoming CMIP6 effort. The experiments are well justified and for the most part clearly documented; however, some minor unclear aspects remain. I recommend publication in GMD once these discrepancies and the other comments below have been addressed.

C2346

Specific comments: 1) Introduction: The authors use the old division of geoengineering techniques to SRM and CDR, which excludes cirrus cloud thinning. They also explicitly state that they use the term "geoengineering" to specifically refer to the broad range of proposed SRM techniques. However, this is not true since they also call cirrus thinning 'geoengineer- ing' (e.g. in subsection 2.4). Hence the first paragraph of intro should be rewritten.

This is a good point. We have reworded the first paragraph.

2) P. 4704, lines 18-24: It is not immediately obvious how comparison of G1ext and single forcing CFMIP experiments will help gain better understanding of the response to radiative forcing. Are the irradiance changes in CFMIP comparable in magnitude to G1? Overall, I feel that the authors expect the reader to know quite few details of the CMIP6 or associated other MIP experiments; it could be helpful to give slightly more background info.

We have added additional description that addresses the reviewer's questions.

3) P. 4706, lines 21-23: "Modeling groups that have –". I'm not sure what this sentence means; please reformulate.

We have rephrased this sentence to better express what we mean: "Some modeling groups may have an internal sulfate aerosol treatment; the radiative response to stratospheric sulfate aerosol injection should be determined in each model so the proper amount of aerosol is injected into the model such that the total radiative forcing objectives of the experiment are met."

4) The difficulties in setting up G3 are mentioned a few times, but it isn't explained what those difficulties were and why the new proposed setups are better. Either elaborate or leave mentioning earlier difficulties out.

We have now omitted such discussions.

5) Why isn't sea salt geoengineering included in the G6 experiment family?

C2347

There are two main reasons for this:

1) Sea spray experiments have just been completed, and analysis is underway. We felt it was premature to suggest a new sea spray experiment when the old ones haven't yet been well understood, particularly since we don't know where the dominant uncertainties and inter-model differences lie.

2) We already have four experiments, which is about the maximum that modeling groups will do.

We had included a longer explanation to this effect in an earlier draft, but we found it made the tone overly negative and disrupted the flow of the paper. To address the reviewer's comment, we have added to the conclusions section a brief mention of the possibility of this experiment in the future: "In addition, by standardizing designs, we have provided future avenues for a G6-like experiment looking at sea spray geoengineering, an experiment we have chosen not to include at present, as the previous sea spray geoengineering experiments (Kravitz et al., 2013a) are still being analyzed."

6) Are the cirrus clouds descriptions in most ESMs mature enough at this point to yield meaningful results from G7cirrus?

As we discuss in Section 2.4, there has been a validation of many of the parameterizations used in representing cirrus clouds. Based on these results, we believe the representations of cirrus clouds in many participating ESMs are sufficient for our purposes.

7) Subsection 2.4 (G7cirrus) should be rearranged as it is in many places difficult to follow its logic. I urge the authors to go carefully through the whole subsection, but give some pointers below:

We acknowledge the reviewer's point and have gone through this section carefully. Please also see specific responses below.

- Third paragraph (p. 4708, line 23 –): The placement of the sentences "However, this approach could be difficult — not optical depth." is very odd, and distracts from the fact that (presumably!) the GISS ModelE2 simulations are the previously mentioned "first attempt at representing the effect of cirrus cloud thinning"; Why can factor e "only be implemented for ice clouds with temperatures below 35 C and pressures lower than 600 hPa"?

We agree there are some ambiguities in this sentence. We have modified it to better express what we mean: "However, modifying cirrus optical depth in this way could be difficult in some models, as many models only distinguish between liquid and ice clouds. A specification that most models can handle would be to implement the factor epsilon in ice clouds with temperature below -$35°$C and pressures lower than 600 hPa. This will not account for models that formulate the effects of cirrus clouds in the infrared as a modification to atmospheric emissivity, not optical depth."

- Fourth paragraph (p. 4709, line 4-): "not enough to achieve the goal of G7cirrus" – however, the goal of this experiment is not mentioned before this statement; Do the single model simulations with treatment of cloud microphysics show reduction of cirrus coverage? Shortly summarize what is the main difference between these and the GISS simulations.

This sentence was phrased ambiguously. This entire paragraph has been substantially rewritten, and we believe such ambiguities are now clarified.

- Fifth paragraph: (P. 4709, line 17-): "Ice sedimentation velocity is used —" seems a very random comment in the context of G7cirrus description; "They found that —" Who does "they" refer to (i.e. if it doesn't refer to Liu et al., does it include Muri et al?)?; "lending credence our chosen method" You haven't told yet what your chosen method is; "involving parameter perturbation" what parameter are you talking about (presumably fall speed)?

This entire paragraph has been reworked, and there is now a logical flow to the sentences.

- Could Fig 4 be excluded as there is very little discussion of it in the text?

We feel this figure is important and would like to keep it, although we acknowledge that we didn't discuss it as much as we should have. We have now added additional discussion highlighting the results in Figure 4.

- P. 4710, line 5: "This representation is also not ideal—" What representation? Increasing fall speed by varying amounts?

This sentence has been removed in the course of reorganizing this section.

- Shortcomings of the chosen approach are scattered in several places throughout the subsection → rearrange; It could be clearer to first introduce the chosen approach, and only then discuss its benefits and shortcomings.

We agree, and this comment has been addressed in the reorganization of this section.

- P. 4711, lines 19-26: I don't understand why this paragraph is included here (i.e. only under G7cirrus) considering that time slice simulations are performed also for all other Tier 1 experiments.

Good point. We have removed this paragraph, as the timeslice simulations are discussed later.

8) I assume the motivation for using RCP6.0 (instead of e.g. ScenarioMIP high forcing scenario) as a background scenario in G4SSA is that some of the required model runs have already been performed? Would it be unrealistic to rerun those simulations to obtain consistent baseline scenarios throughout GeoMIP6?

The primary purpose of including this experiment is for comparisons of ESMs with CCMs, meaning we are somewhat beholden to the experiment design of

CCMI. In CCMI, they used a background of RCP6.0, which is why G4SSA is based upon that scenario. It is likely overly burdensome to ask the CCMs to re-run their models using one of the new (as of yet undersigned) scenarios, but regardless, it's not really our call, as CCMI is an independent project.

9) P. 4714, line 1: "different treatments of — aerosol microphysics" But isn't the whole point of this experiment to eliminate the differences in aerosol microphysics?

We apologize for the confusion, and if we have said anything to the effect of eliminating differences in aerosol microphysics, we would appreciate help in pointing it out so we can correct it. The purpose of GeoMIP is to understand inter-model differences in response to common forcings. Some of those differences are likely due to the aerosol microphysics, and capturing that range of responses is an important goal of the project.

10) P 4714, line 2-5: I don't doubt this sentence is true but you should elaborate on how the comparison of CCMs and ESMs could reveal the important mechanisms (+ which mechanisms) or guide identification of processes in need of improvement.

Agreed. We have identified some examples of the sorts of comparisons we envision: "As examples, CCMs generally have more thorough treatments than ESMs of stratospheric chemistry, transport, and aerosol microphysics, but they have less thorough treatments of cloud microphysics and atmosphere-ocean coupling. Comparisons between CCMs and ESMs can reveal whether complex treatments of some of these processes have large effects on the answers obtained. As such, these comparisons can reveal some of the mechanisms behind the climate model response to stratospheric aerosol geoengineering, as well as provide a guideline for identifying which model representations of physical processes need improvement."

11) P. 4714, line 7-10: Emissions reduced linearly to zero (or even below if lowest scenario assumes negative emissions?) during what period? Assumably 2100-2300,

but this should be explicitly stated.

This hasn't been decided yet, so we are unable to provide additional information at present. The leads of ScenarioMIP will provide further description in their manuscript, which is currently in preparation. We will request that this paper be linked to the CMIP6 special issue of GMD so that the ScenarioMIP experiment description paper will be readily accessible to interested readers.

12) GeoMIP Testbed: Is the testbed closed, i.e. meant only for the three experiments listed or could further experiments be suggested? If so, what is the procedure to do so?

The Testbed is open, and we thank the reviewer for pointing out this omission. We have added instructions for new experiment proposals.

13) P. 4715, line: 17: "As was stated in Sect. 2.2—" I cannot find this statement in Sect 2.2.

This clause has been removed.

14) The goal of G6sulfur_limits does not seem very clear to me. It is stated that it is to find out "what is the limit of achievable radiative forcing"; however, the experiment design seems to focus on whether stratospheric SRM could be used in sufficient amount to reduce forcing from high to low forcing scenario – it is very unlikely that the exact amount of sulfur to achieve this would be the *limit* of achievable stratospheric SRM.

This is a good point. We have rephrased the experiment description to better represent our intentions.

15) My final comment is a non-scientific one and therefore should not affect the review process, but I'd love to hear the authors' views on it: I seriously worry whether the shift towards policy relevancy in geoengineering experiment designs, e.g. using SRM to move from worst-case forcing scenarios to more tolerable forcing scenarios, reflects a shift in scientists' views on geoengineering. Some years ago there used to

be quite a wide consensus among those in the field that overall geoengineering is a bad idea (due to a variety of ethical, governance and natural science uncertainty issues) and should only be considered in case of climate emergencies. However, the new experiments were "designed to open the door toward possible conversations with designers of climate change scenarios", so I am left wondering whether the community has started to consider geoengineering as a viable policy option (I find it difficult to believe that the outside world wouldn't interpret it this way). Whether or not this is the case, one is of course free to run whichever scenarios one likes purely for scientific interest. However, no matter how carefully we scientists try to formulate our message to the general public/policy makers/stakeholders, in the end we have relatively little control on how our results are interpreted and used. Therefore, I personally would have preferred continuation of the more idealized, and less policy-oriented scenarios also in GeoMIP6.

As the first author, I'm going to answer this question, so please don't interpret my response as being on behalf of any other authors.

I think the reviewer brings up an important point and a rather delicate one. Idealized scenarios are great for probing climate system behavior, but they are necessarily limited. As an example, G1 cannot tell us about how models handle stratospheric transport of aerosols or aerosol microphysics. Nevertheless, results from experiment G1 (and some of the other idealized experiments) have been interpreted as being more policy relevant than one might expect. This may be due to how researchers talk about G1, or it could be how the results are interpreted once they leave the care of the scientists who produced those results. Regardless of the reason, if G1 is seen as policy relevant, the inherent implication is that geoengineering is the only response to increased CO2. Most researchers I know would find serious issue with that conclusion (the only permanent solution to climate change is to stop putting CO2 in the atmosphere, and Niemeier and Timmreck 2015 ACP strongly suggest that there is a practical upper limit to

the amount of radiative forcing that can be achieved by sulfate aerosol injection), but that's what has happened.

The point I'm trying to make is that regardless of whether the experiment designers view a simulation as idealized, it may not be interpreted as such (as the reviewer rightly points out). Given that, I feel as though it's important to get ahead of these conversations if at all possible. I believe that if geoengineering is going to be used, it will be used as part of a portfolio of responses to climate change (e.g., mitigation, CDR, adaptation, and geoengineering). It's important to me that this point be made in the actual experiment designs, which is why not all of the experiment designs are idealized. This was a strong motivation behind the G6 experiments going from a high forcing to a medium forcing, and not a lower forcing (although we do plan to explore whether geoengineering is capable of going to a lower forcing in G6_limits, as well as what that climate might look like). Another issue is that, from a purely scientific standpoint, the results of the more "policy relevant" designs have a lower signal-to-noise ratio or more inter-model uncertainty, so the results are harder to interpret. In this sense, idealized simulations have a substantial advantage.

In short, our experience has led us to believe that both types of simulations are important, for different purposes. That's why we've included both of them in our experiment protocol. These sorts of decisions (idealized vs. "realistic") are the subject of a great deal of discussion and debate amongst the GeoMIP participants. If the reviewer is interested in more insight into how and why we made the decisions we did (such insight is not easy to capture in this format), I would encourage him/her to contact me (ben.kravitz@pnnl.gov) at some point in the future. I would be happy to discuss all of this at length.

Technical comments:âĂĺ- P. 4708, line 9: show allow → should allow?

Corrected. Thanks.

C2354

- P. 4710, line 21: "*a* simple approximation similar to Muri" or the "*the* simple approximation of Muri"?

Thanks for the correction.

- P. 4712, l 17: response → responds?

Yes, thanks for correcting this typo.

Interactive comment on Geosci. Model Dev. Discuss., 8, 4697, 2015.