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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multi-model dynamic climate emulator for solar geoengineering" *by* Douglas G. MacMartin and Ben Kravitz

Anonymous Referee #3

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The paper entitled "Multi-model dynamic climate emulator for solar geoengineering" by MacMartin & Kravitz presents a simple numerical emulator of the complex GeoMIP models which could be used to discuss Geoengineering scenarios. This paper is well written, fairly straightforward, and is interesting – I believe – for the community.

One could wonder, however, if ACP is the best journal for publication, as a lot of technical detail regarding the modeling (i.e. establishing the response functions) is given, whereas the more physical aspects remain (maybe too) brief. Maybe GMD would have been a better choice. But that is ultimately an editorial issue. And I don't think this point alone prevents publication in ACP, especially as the physics is well understood and already published elsewhere. It goes in favor, however, of improving the narrative so that the reader can grasp both the modeling approach and the modeled physical processes.

Ultimately, I do recommend publication, but provided the few points below are answered.

Major points:

1. As mentioned: the end of the paper can be improve. Specifically, while the beginning (the methods, mostly) is well documented, the last part (the results) appears too short. This creates a sort of frustration, as the reader realizes the emulator performs well but is not always sure what physical behavior/process is actually well emulated. A couple of sentences, here and there, to remind the reader of the main conclusion of already cited studies (e.g. Kravitz et al., 2015; Andrews et al., 2010) would help.

2. The paper lacks an introduction to EOFs! There is a quite lengthy explanation of what IRFs are and how they are obtained, but almost nothing about EOFs in the methods section. This should be re-balanced as EOFs are presented at the end of the paper. Maybe the part on IRFs could be shortened a little so as to avoid a too lengthy methods section.

3. The analysis of the performance of the emulators is limited to looking at some plots. It would be better to have at least a few quantitative metrics, to better understand the emulators' performance. Metrics could be provided in a table, both for the IRFs (time-series) and EOFs (spatial patterns).

4. This is more of a request, but it is maybe the most important point of my review. I believe the IRFs calculated by the authors should be provided as supplementary material. The paper would strongly benefit from it, as it would have much more impact on the modelers' community (and, therefore, it would be much more cited). This is especially true as the rationale behind the study is presented as being using those emulators in future studies of geoengineering scenarios. An Excel spreadsheet with one time-series per model and global variable should do it.

Minor points:

l. 3: I suggest adding “further” to “without relying *further* on GCMs” and removing “for every possible pathway”.

l. 15: I find “be a more accurate estimate” than GCMs too strong. I would rather say “more cost-effective”, especially as for GCMs the multi-model approach, as well as the multiple realizations, do compensate for the possible bias induced by natural variability. In the end, it is an issue of computation time requirement, not of accuracy.

l. 21: I don’t like the word “interpolation” here.

l. 22: Change “fidelity” for something like: “spatial and temporal resolution”.

l. 29: Define GeoMIP and explain briefly.

l. 38: Other variables such as precipitations are not always assumed to be strictly proportional to global mean temperature by simple models. E.g. some simple models use the relationship to GMT and RF by Andrews et al. (2010) for precipitations. Overall, I suggest being slightly less categorical.

l. 46-48: That sentence referring to Cao et al. (2015) should either be developed or removed. I found it incomprehensible.

l. 71: I suggest removing NPP of that study. See point below about figure S5.

l. 93-94: I find that last sentence too brief: please develop.

l. 113-114: I think a reminder that when the difference is done between these two simulations, you’re assuming the system is linear.

l. 144-150: The drawback of training over lower forcings would be a reduced domain of validity of the emulators, wouldn’t it?

l. 191-193: This drifting issue makes one wonder about the results of the study... Maybe this should be slightly expended. Can the drift be actually explained? How significant is it?

l. 202: Example of where one or two sentences could improve the paper. Explain/recall why there is a difference in the fast response.

l. 230-233: That sentence is a bit obscure. Is this a property of the IRFs or the GCMs? Develop.

l. 234: Change “indicate” to “provide”?

l. 239-242: I honestly don’t understand how the authors can claim that there is “no evidence of non-linearity”. What would be the evidence? Do you mean that the non-linearity is negligible, and therefore captured by the IRF?

l. 242-245: As in the abstract, I find this statement far too strong. It should be moderated. I would basically remove the sentence, unless actual proof can be provided...

l. 256: Change “metrics” to “impacts”?

l. 260: Again, moderate a little bit: more insight *on some aspects*. Maybe recall the computing-efficiency of the emulators. I believe this is definitely their most significant strength.

l. 267: I fear the use of the word “moments” here may be confusing for the majority of the community. Maybe write “*statistical* moments”, or expend or rephrase.

l. 281: It is always possible to develop emulators, except that they have to be non-linear. So basically, the next step is to build box models with non-linear coefficients.

Fig.1: Check units. For this specific plot, the IRF units should be e.g. $^{\circ}$C/[W/m2] I think. Check also units for precipitations.

Fig.3: Units.

Fig.5: Needs a title over each map.

Fig. S1: Could be in main text.

Fig. S5: Units are likely wrong. NPP should be tens of PgC/yr. But more importantly I suggest removing that plot on NPP. NPP is not a variable of the climate system stricto sensu, it is a variable of the carbon-cycle. NPP responds firstly to changes in atmospheric CO_2, then to changes in climate and incoming radiation (at least in current-generation ESMs). The response to CO_2 is strongly non-linear in intensity (can be captured with a simple log function, at global scale) and it is virtually instantaneous at the yearly time-scale. So here there is virtually no difference between the two simulations because NPP is basically responding to the annual atmospheric CO_2. In short: the IRF approach is *not* the right approach for NPP: wrong driving variables, and wrong time-scale.

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