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Earth System Dynamics
Discussions

Interactive comment on "Differing precipitation response between Solar Radiation Management and Carbon Dioxide Removal due to fast and slow components" *by* Anton Laakso et al.

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The authors model two geoengineering methods, the Solar Radiation Management (SRM) and Carbon Dioxide Removal (CDR), in two Earth system models, the CESM and the MPI-ESM, and run simulations under several scenarios and analyse the global mean and regional temperature and precipitation responses. The considered scenarios are the following:

(1) RCP4.5

(2) RCP4.5 with SRM controlled to approximately maintain present-day global mean

temperature throughout the 21st c.

(3) RCP4.5 with SRM controlled to approximately maintain present-day global mean precipitation throughout the 21st c.

(4) RCP4.5 with an ambitious CDR removing 1% of CO2 concentration per year

They give some evidence in both ESMs that the global mean precipitation response has a global mean temperature-dependent component, which is based on the methodology proposed by Gregory et al. (2004). Besides this the precipitation can respond very fast, at least to the greenhouse co2 forcing, and this fast response is of opposite sign compared with the temperature-dependent part. To aerosol forcing the fast response is negligible. This is the reason why there is less precipitation in an SRM scenario when global warming is fully compensated. Conversely, the authors make the point and demonstrate in the models that maintaining global precipitation levels (3) requires less sulfate aerosol injection than maintaining temperature (2). The authors also point out that in the long run, without geoengineering, the temperature-dependent part of the precipitation response will dominate, and so the worst impacts of climate change we would be just yet to see. In the CDR scenario of net co2 reduction (4) they find a wettening in both ESMs and an overall very similar precipitation response, however, this turns out to be a coincidence, because the temperature response is not so similar, which drives the precipitation response, and these are the fast response components to other forcing agents that seem to "compensate for" the difference. The ESMs also differ in other aspects. The temperature-dependence of the precipitation is more sensitive in the MPI-ESM - while the fast response (per unit forcing) are surprisingly similar - which is why stronger SRM is needed in MPI-ESM to maintain the temperature (2). The regional responses, both temperature, and precipitation, are very different between the models; it seems hopeless to predict in any location even the sign of the side-effect!!! Although it would not be completely useless to be able to predict at least bounds on the magnitude of the possible change.

The paper is written fairly decently. Although there are some repetitions, and the mathematical symbols and notation should be consistent. I attach an annotated version of the manuscript with corrections, comments, and questions. Overall, i found the paper a very worthwhile read. Even if i had my own experience with the drying under geoengineering and the fast components of the precipitation response, i have not been aware of the link of these two, and i did not know that the slow response can be put down approximately as a temperature-dependent component. The numerical work handling the EMSs was clearly a great effort too.

I have just a few perhaps/hopefully minor points to make.

1. Why did you base your four scenarios (1)-(4) on the RCP4.5 emission scenario when it remains within the Paris Agreement as represented in both ESMs, even within 1.5 K warming? I mean why not consider a business as usual scenario when we would much rather need geoengineering?

2. Regarding the control method for (2) and (3): You change the forcing level in the ESMs year-to-year, if needed, but is this forcing realistic, would it be consistent with changing the sulfate injection rate year to year? Is there not a transient effect? You wrote that the aerosol model ECHAM-HAMMOZ took 2 years spinup runs.

3. It seems to me that Fig. 7 does not verify that you have a temperature-dependent precip response component. But rather Fig. 4 does. In this respect, a more clear wording is needed at relevant points in the text.

Note: My policy is to not make a recommendation to editors on the publication of manuscripts. (Please consider my selection of the recommendation “minor revision“ void, which i did only to be able to submit my review.) It is the editor’s duty to make up their mind based on (ideally factual) referee reports, or one that reflects the referee’s (ideally unbiased) opinion.

Please also note the supplement to this comment:

https://www.earth-syst-dynam-discuss.net/esd-2019-48/esd-2019-48-RC2-supplement.pdf

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