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Earth System Dynamics Discussions

Interactive comment on "Climate response to imposed solar radiation reductions in high latitudes" *by* M. C. MacCracken et al.

Anonymous Referee #2

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The authors of this manuscript do not explicitly say what the goal of their study is or which question they intend to answer, except for the vague statement in the introduction that they "investigate a polar-focused approach to SRM". My impression is this paper tries to do two different things: a) provide a review of SRM in general with a focus on stratospheric sulfate engineering and high-latitude SRM (hlSRM), and b) to describe results from numerical experiments in which solar insolation is reduced in more or less large polar-centered regions in both hemispheres.

a) is mainly done in the introduction, rationale, discussion and summary parts and is well written albeit not very original and quite imbalanced. It seems to me that the authors want to advocate their approach (in this case hlSRM), instead of discussing it on a scientific basis. The authors ignore potential arguments against hlSRM that have been put forward earlier e.g. by Robock et al. (2008) and Caldeira and Wood (2008) (shorter lifetime of aerosols that are put into high latitudes; less insolation and generally higher albedo at high latitudes, so more material would be needed to reach the same forcing; no reduced impact on monsoon systems when compared to global deployment). One may also think of a stronger impact on ozone or potential effects of increased high latitude sulfate deposition? I would suggest to considerably shorten this overview and argument part (in particular the "rationale" should get much shorter; "summary and next steps" could be completely omitted), and to find a better balance. What is missing in both introduction and discussion is a clear statement on the goal of this study, and what distinguishes its approach and results from earlier studies.

b) I think the novelty of the study is that hlSRM is applied in a numerical model not only in the northern but also the southern hemisphere. I have learned from the study that the climate sensitivity of SRM depends strongly on the latitudes for which insolation is reduced. It seems in particular strong in regions where the impact on sea ice is large, i.e. where sea ice depleted through GHGs can be recovered through SRM. These latitudes are different in NH and SH. Besides that, the climate sensitivity to SH hlSRM is in general stronger than to NH reductions of insolation. However, I'm not sure if this result is correctly interpreted, and to what extent the method to calculate the forcing may have contributed to it (see below). In general it seems to me that the authors have not paid much attention on the presentation and analysis of the results. This is very evident from the too small and partly wrong titled figures. If the authors think the results are important for the paper they should be presented in a way that the reader can identify them. If results are considered unimportant, they should be removed. It seems also to me that the results are more or less ignored in the discussion and summary sections. No specific reference to results is made. Most arguments presented could have been made without the numerical experiments of this study, and have actually already been made in the "rationale" section. It should be made very clear which of the arguments are supported by the experiments presented here.

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One issue with the numerical experiments is obviously the use of a slab ocean model with "thermodynamic sea ice parameterization". Ideally I would suggest to only use models for such studies which are participating in the GeoMIP model intercomparison (Kravitz et al., 2011) which would make it easier to estimate the general performance of such models. I understand the motivation to use simpler models. They are faster, easier to interpret, and slab ocean models have the advantage over deep ocean models that they don't need hundreds of years to approach equilibrium. However, in a study where the central point is the response of sea ice to forcing, an evaluation of the performance of the model with respect to this parameter is necessary. Some references to earlier studies may suffice.

I will provide comments to some specific issues in the following, but in general I think that the manuscript needs to be rewritten in large parts.

Specific comments:

717L11: "larger responses contributing to the additional changes . . ." To which "additional" changes?

717L21: "global average temperature is on a path to exceed 2-3C" ??

717L22: "In this paper we investigate . . ." This would be the place to tell the reader what the goal of this study and what one can expect to learn by the chosen approach with respect to existing knowledge/earlier studies.

720L5: "Further favoring use of stratospheric approach as the optimal approach . . ." I think this statement would need a much more thorough assessment, which I see, however, not as the goal of this paper.

721L25: "Further simulations by Irvine et al. (2009) . . ." To me this sounds like Irvine et al. had done simulation on hISRM, which is not the case.

722L28: ". . . in contrast . . ., suggesting that monsoons would not be directly impacted" In contrast, in the cited paper by Robock et al. (2008) it is said that "there would still

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be large effects on the summer monsoons . . .” A statement that would be supported by the work on high-latitude volcanic eruptions by Oman et al. (2005, 2006). The authors may disagree with these studies, but then should argue, why.

723L23: “As reported by . . .” Sentence is difficult to understand.

723Section 3: Important information on the model simulations is missing, e.g. on how many years were simulated, how many used for the analysis, etc.

724Definition of experiments: Why were the reductions in N51 and S51 not chosen to yield a global deflection of TOA insolation of 0.37% as it was the case for the other latitude bands? This makes a quantitative comparison difficult.

724Section4: It would be nice to see numbers on global and regional responses of temperature and precipitation (e.g. similar to those presented by Caldeira and Wood, 2008) to support the interpretation of results. Instead of all these very difficult to read stamp plots it may also be useful to present some of the results as zonal mean values, maybe with lines form different experiments in one plot to see differences.

726L12: “Figures 2 and 3 . . .” are particularly unreadable, and not useful to support the statements in the text.

726L19: “Larger difference of TOA radiation . . .” Larger with respect to what?

727L1: “. . . sea ice insulates . . . from the heat held in the ocean waters” Is it only additional insulation or is there also less heat?

727L29: “. . . global solar reduction . . . leading to a near counterbalancing of precipitation changes . . .” The numbers of precipitation change reported for this experiment by Caldeira and Wood do indicate a global overcompensation of precipitation changes by about 25%. In more complex models, with likely more sophisticated land surface and vegetation schemes, this overcompensation is on average larger than 50% as reported by Schmidt et al. (2012).

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728L20: "... suggest the possibility of optimizing the pattern and extent of solar reductions ..." No. If one could optimize the patterns one may be able to design desired climate responses (based on uncertain model predictions). But it is very unclear if patterns can be designed similar to those used in this idealized study, because the spread of particles emitted at high latitudes would be subject to natural mixing processes.

728L24: It is unclear why the paper by Ban-Weiss and Caldeira (2010) is referenced here.

730L4: "... decrease in cloud fraction was more than made up for by ... increases in sea ice ..." The other way round?

730L14: Primarily because of ... sea ice, the global climate sensitivity for SH reductions ... was about double that for NH reductions" Is that true? Fig. 8 seems to suggest that the different response in cloudiness is responsible for about 2/3 of this difference. It would be important to discuss a) reasons for the different cloud feedbacks, and b) if the use of IRF (see appendix) has an influence on the calculated sensitivities.

730L17: "...even though the decreases in global mean temperature were similar" I do not understand this. If sensitivities differ by about a factor of 2, and the forcing is very similar, how can temperature responses be similar? And how large are they? As said above, these values should be provided in a table.

733L4: "... that the NS51p06 simulation produced very similar counter-balancing of temperature and precipitation changes to GSRM suggests that at least some of the unintended consequences of global solar reduction may be avoided ..." a) Very similar? Is that true? In Fig. 6b I see e.g. significant changes in precipitation over Europe, high-latitude Asia, parts of North America and east Africa for NS51p06 that do not seem to result from GSRM. b) Which unintended consequences? And which not? Fig. 9: a) I guess this is the change with respect to 2xCO2. This should be explicitly stated. b) FS is not discussed in the text. Is it unimportant?

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731L27 “. . . a local balancing of the terms need not result.” Why not? From Fig. 9 it looks like the terms are locally balanced. And why shouldn’t they when equilibrium situations are analyzed?

732: I completely disagree with the first sentence of the discussion. That “solar reduction in polar regions can counterbalance . . . some of the warming in each hemisphere” is really not new. And why would emerge from this the described potential? For this potential one would need techniques to provide latitude dependent reductions of insolation.

736L13” “pattern and degree of deployment would likely involve different extents . . . in each hemisphere” Above the authors have mentioned that asymmetric deployment would likely shift the ITCZ. Would this not have to be considered? And again, the impression is given that reduction patterns could be designed at will.

737 Appendix: The calculation of “Instantaneous radiative forcing” needs to be described and discussed. It sounds to me like it was based on a one time step-calculation. In this case the calculated forcing would depend on scenario, season, weather and time of the day of that specific time step.

737L25 “This near similarity is caused because the stratospheric adjustment in the case with added GHG is primarily the result of the increased emissivity . . .” True, but in the case of insolation changes, the stratosphere would need to adjust to changes in ozone and absorption by ozone. Can this be neglected?

Interactive comment on Earth Syst. Dynam. Discuss., 3, 715, 2012.