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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 #3**

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Reviewer #3

General Comments

Overall this paper presents interesting new material, expanding on the work of Caldeira and Wood (2008), by including the southern hemisphere and analyzing the mechanisms behind the polar amplification, but I believe significant alterations are required before publication:

1. The purpose of this study appears to be two-fold and at odds; a review of the 'effectiveness' or 'quality' of SRM schemes and the potential advantages of polar geoengineering, and an extension of the highly idealized Caldeira and Wood (2008) study to include the southern hemisphere and further analysis of the polar amplification of



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the cooling effect. The rationale section and the discussion section discuss the 'advantages' and 'effectiveness' of polar geoengineering as if the interventions analysed in the paper were actually feasible or realistic, whereas the methodology and results make it clear that the polar geoengineering under investigation is a highly idealized experiment. The discussion of the advantages and effectiveness are critically underdeveloped and I'd advise altering these sections such that they are in accord with the idealized nature of the study. I'd also strongly advise changing the final sentence of the abstract due to the problems with the 'effectiveness' sections I will outline below.

2. The paper is fairly long but some of the general results have been presented before, in brief, in Caldeira and Wood (2008). Section 5 is a shorter section than section 4 but seems to me to present the greatest novel contribution. I'd advise reducing section 4 and extending section 5 to give greater detail on the mechanisms behind the polar amplification of the cooling effect. Some suggestions are made in the specific comments section.

3. Many of the figures shown in this paper are over-complicated and replacing these with more useful, simpler figures would be for the best, e.g. figures with 12 anomaly maps could be usefully replaced with a single multi-line zonal-mean plot. Specific recommendations below.

4. Little discussion of the results or of the limitations of the model and highly idealized experimental setup is made.

Specific Comments

Abstract

716L21 [major]: The final sentence of the abstract needs to be rewritten to reflect the contents of the study and to improve clarity. The claim that there would be 'fewer undesirable side effects than for global solar radiation reductions' is not demonstrated in this paper and I'd suggest that a reading of the relevant literature on sulphate aerosol



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geoengineering (not discussed in depth in this paper) would suggest that there may in fact be more undesirable side effects.

Introduction

717L3: more references needed

717L10: rephrase to improve clarity

717L21: further explanation of 2-3C warming by end of 21st C.

718L2: 'The sheer hubris' is this the only reason?

718L9: I don't think these groups would accept this distinction, e.g. the royal society funded a large research program into the governance of geoengineering.

718L25: rephrase for clarity.

718L28: what about methods section?

2. Rationale for a polar-focused approach

[major] This sections structure is a little muddled and does not provide the evidence for the two significant claims made: 'polar geoengineering would have fewer side-effects' and 'would be more effective than global geoengineering'. There is not a sufficient discussion of non-sulphate aerosol geoengineering techniques and it does not sufficiently develop aspects of sulphate aerosol geoengineering directly relevant to the rationale, i.e. cost, transport (critical for polar injection), required loadings, radiative effectiveness, and feasibility of polar-focused implementation.

719L17: what about land albedo schemes?

720L5: why does low cost suggest that stratospheric sulphate injection is 'the optimal approach to counter-balance global warming'?

720L7: this paragraph contains only 1 citation and goes into very little detail on the benefits, risks, effectiveness, feasibility, etc. of the non-sulphate schemes. Why include



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these in the discussion if so little attention is then paid to them?

720L21: 'augmenting the stratospheric aerosol layer appears to be the most plausible option for global SRM' This statement is not backed up by the material presented in the preceding paragraphs, more Is needed.

720L27: Is this a robust finding? See (Schmidt et al., 2012). Also (Bala et al., 2008) Shows an increase in continental runoff and doesn't report on land-sea temperature gradient.

721L8: (Murphy, 2009) – diffuse light effect on concentrating solar power

721L26 [major]: the Irvine et al. (2009) does not indicate what the authors suggest. It is a study of the climate effects of a global reduction in solar constant on the Greenland ice sheet. This is a worrying mistake as this is one of only a handful of papers that (the authors believed) are of direct relevance to this study. Care must be taken by the authors to ensure that the cited papers make the claims they suggest.

722L1 [major]: 'These results suggest... Without inducing many of the unintended consequences of augmenting the global sulfate layer' I don't believe the authors have shown this at all. By excluding considerations of transport and deposition the 'effectiveness' of polar sulfate geoengineering has been blown out of proportion. Robock et al. (2008) find for tropical injection, i.e. near global sulfate cloud, a forcing of $\sim$0.4 Wm-2 per MT radiative effectiveness and for polar injection (68 North) they find $\sim$0.07 Wm-1 per MT radiative effectiveness. The lack of unintended consequences is not shown satisfactorily either.

722L8: what other consequences may arise form an increased meridional temperature gradient? Consideration of potential negative consequences is needed for balance.

723L1: again Schmidt et al. 2012 may be a better reference.

3. Model Description



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Overall model description is very similar to, but less clear than the Caldeira and Wood (2008) description, and contains a discrepancy in the reported model resolutions.

724L19: define approach more clearly.

724L23: explain reasons behind these choices.

4. Temperature and Precipitation Responses

Overall, it is not clear that much is added in this section that was not already in Caldeira and Wood (2008) and the presentation of the material could be improved. Much of the analysis in this section is imprecise and unquantified and would benefit from the inclusion of clearer graphics and more quantification of effects. There is an overabundance of panels which are not analyzed in depth in the text, simpler and more appropriate graphics should replace some of these.

725L2: rephrase for clarity

Fig1 a+b: zonal mean plots would seem more appropriate as most of the discussion focuses on the meridional differences between the experiments which are hard to compare in the current figures (12 anomaly plots per figure).

725L25 [major]: '. . .The global response does not appear to be sensitive to the latitudinal extent. . .' After consulting table 1, I find I disagree with this conclusion.

725L19: 'the solar reductions . . . were chosen to lead to. . ..' Why is this not in the methods section and why does the 51 degree simulations not have the same global mean reduction as the other experiments?

726L1: What about sea-ice and snow cover feedback?

Fig 2+3 [major]: these 24 panels could happily be replaced with a global-mean table, a very simple graphic or if it was deemed necessary a set of zonal mean plots. 24 panels with 1 short paragraph to illustrate only that there is more sea-ice seems like overkill.



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726L18 [major]: 'Figure 4 shows that the increases in sea ice caused changes in . . .' I disagree with this assertion. More work is needed to separate the effects of the changes in sea-ice from the other responses to the perturbations. For example the authors could display the zonal albedo change.

727L5: an alternative cloud feedback could be at play, this may be shown with a zonal mean albedo change plot.

72710: More work separating the mechanisms behind this amplification would be good, i.e. to distinguish the contributions of albedo (surface and cloud) and surface energy flux changes.

Fig6: Again zonal mean or other plots may be clearer and more useful than 24 anomaly plots.

728L2: 'leading to a near counterbalancing of precipitation changes' I disagree with this statement after consulting the figure.

728L4: 'lead to somewhat different results' not useful.

728L8: This sentence is not clear.

728L11 [major]: I don't believe this claim is backed up by the results presented.

728L16: this sentence is confusing.

728L16: Very long and confusing sentence.

72826: '. . .critical low latitude are. . .' critical how?

728L21: This discussion of optimization would be more appropriate in the Discussion section.

729L4: 'persistent latitudinal shift' in which direction? And would consistent be more appropriate?

729L3: Why does this happen? More explanation and quantification of the effect would



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be valuable.

5. Relative effectiveness of the alternative solar reduction extents

Overall this section is the most valuable of the paper, as it goes the furthest in explaining the effects driving the different effectivenesses of the polar implementations. However, the section is too short, e.g. it is shorter than the rationale for the polar focus and has only a quarter of the figures of section 4.

729L15: Why is a measure of the relative effectiveness especially needed? Why not assessment of climate and other impacts?

730L4: is there a mistake here?

730L20: 'the climate sensitivities for high latitude solar reductions appear to be roughly additive' what does this mean exactly?

731L8: where is this heat transport shown? Please quantify.

731L14: This sentence is unclear.

731L24: 'over the globe' do you mean on average?

731L18 and fig9: make the figure and text consistent, i.e. use the same terms.

731L29: are you sure this is solely due to the sea-ice, how have you excluded longwave cloud effects?

6. Discussion

There is little discussion of the quantitative results of the study or of the limitations of the method employed, which needs to be addressed. The claims of 'effectiveness' and 'reduced unintended consequences' are made again but are seriously flawed, either strong evidence to support these claims should be provided or these claims should be dropped.

732L8: This paragraph sounds like future work but also sounds like a description of the



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Ban-Weiss and Caldeira (2010) study. This seems like an inappropriate way to start the discussion.

732L13: I don't see the relevance of this second paragraph.

732L17: reducing the effects of which solar heating?

732L20: A brief explanation of the mechanism for polar amplification may be useful here.

733L4 [major]: Why does this suggest that NS51p06 would produce fewer unintended consequences? Is the argument that a lesser reduction in solar constant (in a highly idealized experiment) produces a proportionately greater cooling than a global implementation? If so this is flimsy reasoning, as previous studies have shown that polar sulfate geoengineering is much less effective at producing such a reduction in incoming sunlight than tropical or global injection (Robock et al., 2008). What about other potential problems such as Ozone loss, Dynamic effects of increased meridional gradient, etc.?

733L8 [major]: 'solar reductions over high latitudes may also be more feasible. . .' this paragraph is very problematic. Without reference to literature on transport, chemistry, etc. of sulfate aerosol clouds this discussion of feasibility is inappropriate in the context of this highly idealized study.

733L12: reference to (Bala et al., 2010) and (Andrews et al., 2010) may be useful.

733L13 [major]: Why have no snow-cover results been shown? Can this effect be quantified? This has an impact on albedo and is of relevance to the high latitude feedback.

733L17: this would be the appropriate reference to the Irvine et al. (2009) results on the ice-sheet response to geoengineering.

733L18 [major]: What would be the anticipated problems with these shortcomings?



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734L5: (Robock et al., 2008) presents results on the spread of the aerosol cloud, see figure 3.

734L8 [major]: The effectivenesses reported in (Robock et al., 2008) are 0.2 Wm-2 for 3 MT injected in arctic and 1.8 Wm-2 for 5 MT injected in tropics. This seems very relevant to the discussion.

734L9 [major]: this factor of a third assumes that injected particles have no lifetime in the atmosphere which is a very naïve assumption given the amount of research that has been carried out into this topic and into volcanic eruptions.

734L11: 'might limit interference with precipitation systems in lower latitudes' didn't your results show large changes in precip and large shifts in the location of the ITCZ?

734L12: this sentence needs rephrasing.

734L25: differing how?

734L27: '. . .limit this shift to a small residual of the southward shift. . .' Why might this be possible?

735L3 and L10: these paragraphs seem out of place.

735L25 [major]: first mention of the limited nature of the sea-ice model, must expand on the consequences of these limitations.

735L29: This last sentence is very poor: it contains 5 'differences' and doesn't seem to contain much information.

7. Summary and next steps

This last section is good but neither of the highly relevant Ban-Weiss and Caldeira (2010) and Caldeira and Wood (2008) papers are cited.

736L7: Do you mean statistically significant?

736L8: No results on snow cover were presented so it seems inappropriate to mention



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this potential here.

736L10: No results in this study indicate anything about the beneficial effects of these high latitude modifications on sea-level rise.

736L16: temperature offset?

736L19: is this discussion of cloud brightening relevant?

736L23 [major]: This 'reduced unintended consequences' argument has not been made satisfactorily.

736L8: how are these highly idealized studies relevant for broadening the choices of policy makers?

A. Appendix

This section is very well written and clear. There are only a few minor comments.

737L13: does this IRF have a precedent, is it widely used? Provide a citation.

738L6: delta missing

738L27: what are these spurious non-zero energy transports and why do they occur?

Figure comments

Fig 1a. mistake in labeling of top left panel. Panels are very small and not very clear.

Fig 2 and 3. 0-50% distinction is not clear, also contour levels seem inapropriate. These figures would be better as a table or other simple plot.

Fig 4 and 5. Units differ between caption and title. Similar plots for sea-ice and snow cover may be useful.

Fig 6. Panels are very small and not clear. Zonal mean plots may be more useful.

Fig 8. Global mean changes relative to what? Could you include some assessment of



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the statistical significance of these results?

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