Response to reviewer #2
Kravitz et al., esd-2015-51-discussions
Original reviewer comments in Roman typeface. **Responses in bold.**

Reviewer #2

The authors' primary aim with the manuscript is "to introduce a design perspective ... to more systematically evaluate ... potentials and limitations of geoengineering". To this end they present two climate model case studies where it is attempted to reduce climate change resulting from an atmospheric CO_2 increase at a rate of 1% per year by altering latitude-dependent solar insolation. In the first (2x2) case, Arctic and Antarctic insolation are manipulated independently to restore Arctic temperature and a latitudinal center of global precipitation. The 2nd case involves three degrees of freedom for the manipulation of insolation and, as climate objective, a latitudinal temperature distribution also defined by three parameters. The authors show that through annual adjustment of the design parameters in a feedback process they are able to well approach the intended climate goals. In this respect the paper is very interesting and I would like to see it published. Albeit I have to admit that I don't feel competent to review the details of the proposed multivariate feedback strategy because I'm lacking basic knowledge on the control theory behind the approach. So I'd suggest that this would require a reviewer with a different background.

On a different note, I'm surprised and concerned how, apparently, light-mindedly the authors connect their interesting theoretical discussion on controlling climate variables with the current debate on geoengineering. To my mind, they fail to discuss the practical implications of their approach, despite briefly mentioning in the conclusions that "accomplishing the objectives with physically achievable mechanisms ... introduces additional complications". I find this an oddly optimistic formulation in particular given the fact that one of their examples (the 2x2 case with CESM) shows that the design goal cannot be reached with any of the so far proposed solar geoengineering methods. In this case, Antarctic insolation would need to be increased and not decreased. In the 3x3 case such an issue might also occur but it cannot be identified if negative insolation reduction (i.e. increase) occurs because of the color scale of Fig. 20. Theoretically, as said in the beginning, the optimization is still interesting, but the authors explicitly position their study in the geoengineering context by saying "here we use the common idealized representation of reducing solar irradiance ... analogous to the idea of space mirrors." To me this seems misleading, not only because space mirrors haven't been discussed, yet, for increasing insolation, but also in the sense that I don't know of any proposal to install space mirrors that would allow for designing specific latitude dependencies of insolation reduction.

We have gone through the manuscript and added additional discussions regarding feasibility and practical implications, with a particular focus on the introduction and conclusions. Please also see individual responses below.

We also agree that the simulations affecting Antarctic insolation are only illustrative

of the technique and would likely not be implemented in practice. We have also removed the unnecessary reference to space mirrors.

More in general, I find the final discussions and conclusions insufficient. Indeed, the study shows that additional design goals beyond global mean temperature could be reached if sufficient degrees of freedom were available. This availability, of course is unclear in reality. I guess that the suggested design strategy could work for other than the described cases, but of course, two cases are no proof. This needs to be discussed. Even if the strategy would work in general, the study also gives an interesting example that even if a design goal is reached, like in this case the annual mean restoration of the precipitation centroid, a lot of other climate features could remain unchanged or even further away from the original climate change. In the presented case, the seasonal cycle of the centroid stays wrong. In a lot of maps shown later, I have the impression, that e.g. precipitation anomalies are not strongly reduced by introducing the additional design goals. This would need to be discussed.

We agree with the reviewer, in that we would not expect the climate to be "improved" for goals that the feedback algorithm was not specifically targeting. We have gone through the manuscript and added additional description to this effect, noting in particular that choosing goals is an important subject. (And, of course, provided that the goals are chosen judiciously, the ability to meet two or three goals simultaneously can yield better outcomes than only meeting one goal as in previous geoengineering simulations.)

A further issue that needs to be discussed is related to detection and attribution. It is very unlikely that on a one-year time-scale, deviations from some mean state can be attributed unambiguously, but the proposed design strategy would try to remove them anyhow. Decadal climate variability (probably often unforced, i.e. internal) is known to occur in many different earth system features. To what extent would the suggested feedback process affect this internal variability?

The proposed design strategy does not remove one-year timescale deviations, and this is better clarified in the manuscript. With regard to the impact on internal variability, this is a good point (see in particular the reference to MacMartin et al., 2014). We have added a mention of this to Section 3.3 (relevant when discussing the sensitivity function) and again in the concluding section.

In summary, I would only be able to recommend publication of this article after a substantial revision of not only (see below) the Introduction and Conclusion sections.

We thank the reviewer for the insightful comments. We have extensively modified the introduction and conclusions accordingly.

Furthermore I'd like to ask the authors to attempt shortening the manuscript and potentially also reducing the number of Figures. The aim of the paper, as cited above, is relatively general, and it might be useful to provide many details for other scientists to be able to use the suggested approach. However, I think it could considerably improve the accessibility of the manuscript if the authors identify which are the major points they want to make. Some of the more technical parts could be provided as appendices.

We have moved some of the technical discussion to an appendix. Also please see responses to specific comments below regarding individual figures.

In the following I will provide more specific comments:

P1637, discussion of Fig. 1: It seems like a banality to me that less reduction of insolation would shift the global temperature signal. This could be said in two sentences and one could remove Fig. 1. I also don't agree with the formulation that the question of the "climate effects of geoengineering ... is ill posed except in the context of specific ... objectives". If one asks what climate effects the emission of a specific amount of sulfur via a specific strategy would have, this is a well posed question.

We often hear variants on the sentence, "Geoengineering <u>will</u> cause overcooling of the tropics and undercooling of the poles." To us, this suggests a fundamental misconception regarding the fact that geoengineering has choices, and we find that belaboring the point (as we have done so with Figure 1 and the associated explanation) improves understanding and sets the stage for a broader discussion regarding design choices. Therefore, we prefer to keep this point in the paper.

With regard to the other comment, we agree this sentence wasn't as carefully worded as it needs to be, and we have rephrased it to be more specific.

P1638L10,13: Keller et al. (2014) used only one SRM technique. In contrast to Kalidindi et al. (2014), Niemeier et al. (2013) and ferraro et al. (2014) do argue for differences between effects of sulfate aerosol injection and idealized solar dimming.

We have replaced Keller et al. (2014) with the other references. Also see response to reviewer #1, point 2.

P1638L22 "Accomplishing this end ...": It is unclear which "end" this is referring to. Introducing a "design perspective", "evaluate ... potentials and limitations", "exploring two examples"? Furthermore it is not well explained why any of these goals "requires" the 4 criteria specified below. Indeed, these seem useful criteria, but why "require"?

We have rephrased this sentence to "For any strategy, achieving multifaceted goals can be accomplished via fulfilling a certain set of criteria"

P1638L28, criterion 3: Couldn't it be that some objectives cannot be reached? Shouldn't this already be discussed, here?

Agreed. We have added a paragraph discussing these issues in detail.

P1639L1, criterion 4: "Independent" of what?

This has been rephrased to "verification of the designed strategy in a different

evaluation model"

P1639L15: Forcing efficacies may be different for different agents for other reasons than forcing patterns.

Agreed. We have removed the offending text.

P1640L10, "Offsetting multiple independent features of climate change requires modifying multiple simultaneous degrees of freedom." This is just one of many formulations in the document which sound like there always is a strategy to reach the defined goal, which may not be true. As mentioned in my introduction, I think the full article needs to be revised to formulate more carefully. Besides, in this case the statement could also be wrong in a different sense. As CO_2 increase causes changes in multiple climate parameters, I don't see why one should exclude the possibility to offset the changes with a small number of degrees of freedom.

We have gone through the document and added multiple caveats about how some objectives may not be achievable. We disagree with the reviewer's claim that our statement is wrong. Modifying multiple independent climate features does require multiple degrees of freedom. If changes in those climate features are successfully offset by a single degree of freedom, then they are not independent.

P1644L9: I don't think it is correct to say that L0 to L2 are the same basis functions used by Ban-Weiss and Caldeira (2010) and Mac Martin et al. (2013). The shape of the functions may be the same, but the first referenced study used increases of AOD, while the 2nd used strictly positive reduction functions (see their Fig. 1a), while here, increases of insolation would be allowed.

Good point. We have changed "same" to "similar".

P1644: The authors mention that the chosen functions for input and output are linked by a "clear physical mechanism". I could imagine climate goals for which it is much less easy to define input functions as closely linked to the goals. I think this needs to be considered in the conclusions, because again, there may be a false impression given that goals are easy to reach with the proposaed strategy.

We agree with the reviewer and have added a discussion to the conclusions section.

P1645L9, "With proper design, the process will converge ..." Where does this certainty come from? And converge to what? Not necessarily to the goals.

We have clarified this sentence to state that the process will converge to the goals. We are having difficulty with understanding the reviewer's question, but we have attempted to add clarity to the manuscript along the lines of the following: A feedback loop is *designed* to meet the goals, so it will necessary meet those goals if designed properly. If it is not designed properly, or if the algorithm is used to solve problems for which it was not designed, then it will not necessarily meet those goals.

P1647, Eq. 4: I find it confusing that the same symbols are used for functions in time and frequency domain.

Equation 4 is the definition of the Laplace Transform (although we agree the first instance of f should have been capitalized). With regard to p. 1647, lines 3-6, this comment is not needed, as there is no duplication of symbols between the time and frequency domain; we have removed this sentence.

P1647L12, "temperature change". This is not well defined. I guess change in time is not meant.

We are not certain how to interpret the reviewer's comment. y(t) is a function of time, so change in time is meant. We do not understand why "temperature change" in association with a time-dependent function is not well defined.

P1648, Eq. 5: Why is y(t+D) only defined for t>D? Does that mean that the solution is only available for t>2D?

D is a time delay; we simply left out the obvious implication that y(t)=0 for t<D. We do not understand the reviewer's comment on solutions only being available for t>2D, as the equation is well defined for t≥D.

P1649L23: Kravitz et al. (2015) a or b?

Fixed.

P1660L18: Is the poorness of the fits really an inherent problem with step response simulations? In Fig. 8 it looks a bit like there was an initial overshooting in some of the quantities (although longer simulations or more ensemble members would be needed to confirm this). I speculate that this might not be a general step function issue, but related to regional forcing.

As was described in Section 3.5, the frequency response to a step change is 1/s, indicating that this input contains all frequencies, including high frequency variability. Therefore, this is indeed a general problem with step functions.

The "overshooting" the reviewer describes can be attributed to fast feedbacks, particularly rapid changes in cloud cover; this is also a known response to a step change and is encompassed by our statement regarding high frequency variability. We are uncertain what the reviewer means by "related to regional forcing" and would appreciate some clarification.

P1667L20, "Additional degrees of freedom would be required to offset these local changes ..." Again: Why is it guaranteed that the changes can be set off?

Agreed – we have added a caveat.

P1667, Fig. 14: The symbols indicating the position of the precipitation centroid are too difficult to identify. Additionally this is an interesting example for the issue that even if more than one design goals are specified, the climate might stay relatively far away from its original state. This should be discussed in the conclusions.

We have reduced the latitude range to improve visibility. We agree regarding the additional discussion and have added it to the conclusions section.

P1668L11: I don't understand why this "indicate(s) the importance of carefully specifying the objectives of geoengineering". In which sense carefully? To have reachable goals? Or is it just important for the design procedure to have exactly, not carefully specified goals?

We have removed this clause.

P1668: I'd suggest to remove the discussion on position of the ITCZ and the cross-equatorial energy flux or to put it to an appendix. It just deters the attention of the reader from what I perceive as main points of this manuscript.

We have moved this discussion to an appendix.

P1672: It's not clear to me what an "increase in L1" means. I guess, multiplying L1 with a positive factor? But it is confusing that this would mean a reduction of insolation in one hemisphere and an increase in the other. So the wording here should be chosen very carefully.

We say what this means immediately following: "…which increases Northern Hemisphere insolation and decreases Southern Hemisphere insolation…"

P1675: If I understand correctly, "impacts" is used with two different meanings, here. Please formulate carefully to avoid misunderstandings.

Corrected. Thanks for pointing that out.

P1675L14: Please be more specific when discussing the advancement of this study over Kravitz et al. (2014). Is it different with respect to the simultaneousness of the multiple goals?

We have rephrased this sentence to better differentiate the two studies.

P1675L22: I don't understand why there is "some flexibility" with respect to L1 and L2. Below you are talking about the goals. Wouldn't this be T1 and T2? And why is there only "some" flexibility. Couldn't one invent goals at will? And again: In which sense does one need to be "careful in ... specifying the problem". And how is that shown, here?

We have removed the word "some".

We have change the last sentence of this paragraph to "There are numerous other potential specifications, each with potentially different feedback algorithm designs;

carefully specifying the problem is crucial."

Fig. 13 and following maps: I'm not convinced that all of these maps are necessary. Most details shown are not discussed, and it is also not clear how significant they are. As a courtesy to the reader, please reduce the number of and information content in the figures to underline your main points. In some cases, presenting zonal means could be more instructive, in some other cases maybe just RMS differences.

After addressing this reviewer's other comments, we realized the importance of the maps, as they highlight residuals that are not being controlled for by the feedback algorithms. These serve as important examples of additional degrees of freedom that may be needed and provide an indication that not all objectives may be achievable. As such, we are convinced these figures are even more important than we thought at original submission. We have modified the text to reflect this in various places.