

I want to thank the Author's for the careful and thoughtful revisions and response to reviews. The revised manuscript is technically sound, well written and has addressed my previous concerns. I believe the conclusions are sound, novel and will make a strong impact on the existing literature on geoengineering. I continue to disagree with the emphasis on the surface energy budget (as opposed to TOA) and think the distinction between shortwave feedbacks and rapid response to SRM could be brought to the forefront of the manuscript in order to reach a broader climate dynamics audience. However, that emphasis is simply my opinion on the importance of the work and, as written, the focus on the changes in net shortwave at the surface is justified within the text. I elaborate on my suggestion below and otherwise have only minor suggestions of clarification. I think this manuscript is publishable nearly as is. Thanks again to the Author's for their efforts.

Primary emphasis of the text.

The main conclusion I draw from the results presented in this manuscript are: if one accounts for inter-model spread in the adjusted radiative forcing under SRM management, inter-model spread in surface temperature response is primarily due to differences in the shortwave forcing (as opposed to feedbacks). The spread in the adjusted forcing is a consequence of both the inter-model differences in the direct aerosol forcing and the rapid cloud response and the Authors have developed a novel technique for separating the direct forcing and rapid cloud response. The implication of this work is: the climate response to greenhouse gases and SRM can be understood from simply calculating the adjusted forcing and using the same climate sensitivity. While there is work to be done to understand the rapid cloud response – and where the observed system may lie within the substantial inter-model spread – I think this conclusion is a powerful result that I have not seen stated elsewhere. If robust, it would suggest that running long SRM model simulations is less important than understanding the short term cloud response to aerosol. I think this result is significant to the larger climate dynamics community and should be the central focus of the writing in the manuscript.

Again, this is just my opinion. As written, I think the manuscript addresses the more specific issue of how surface shortwave will change under SRM and this may be of more interest to the geo-engineering community that is reading this special issue. I personally think the results and implications are impactful to a larger community. But I leave it to the Authors to make this determination since they know their audience better than I do.

Specific points:

Section 3.4: Comparison of feedbacks with those estimated from greenhouse forcing; The Authors find an ensemble average surface albedo feedback of $+0.38 \text{ W m}^{-2}\text{K}^{-1}$ and a shortwave water vapor absorption feedback of $-0.91 \text{ W m}^{-2}\text{K}^{-1}$ at the surface. It would be helpful to compare these feedbacks to those (ensemble average over the larger CMIP5 ensemble) reported in the literature under the response to greenhouse forcing. Numbers for the TOA are more prevalent in the literature and I suggest reporting TOA numbers here too (since the technique used in the manuscript gets at both). Specifically, these numbers seem consistent with the ensemble mean surface albedo feedback at TOA of $+0.3 \text{ W m}^{-2} \text{ K}^{-1}$ given by Bony et. al feedback review paper (since the surface number should be slightly bigger in

magnitude as discussed in the current manuscript) and the shortwave water vapor absorption of $+1.0 \text{ W m}^{-2}\text{K}^{-1}$ in the atmospheric column and $0.3 \text{ W m}^{-2}\text{K}^{-1}$ at the TOA (implying -0.7 at the surface) given by Donohoe et al. 2014 (Shortwave and longwave contributions to global warming under increasing carbon dioxide, PNAS). I think the point that the SW feedbacks in response to SRM are consistent with those under greenhouse forcing is a powerful statement.

Page 14. Line 18. Worth pointing out that the 35% difference between surface albedo feedback as measured at the TOA compared to that at the surface is consistent with the (single pass) basic state atmospheric opacity (1-A-R).

Page 14. Line 20. The impact of shortwave atmospheric absorption on reflected SW at the TOA has 2 contributions in response to decreased water vapor under SRM: 1. Less absorption of shortwave above cloud top results in less shortwave reflected off the top of clouds reaching the TOA and 2. Less absorption above bright surfaces reduces the surface contribution to reflected SW at the TOA. The Author's are correct in pointing out that 2 dominates in the isotropic SW model used in this study by construction since the atmospheric absorption and reflection occur at the same vertical level (hence prohibiting the absorption above cloud top. This is a limitation of the model (I'm criticizing myself not the Authors here – Lol). But, in the real world it's possible that the SW water vapor absorption above cloud top is the dominant affect over the surface contribution. I don't know of and can't think of a way to back out the relative contributions, but it's worth discussing this point in the text.

Page 10. Line 32. Suggest rewording to "... implying that decreases in water vapor and cloud amount under SRM lead to more downwelling SW at the surface, counteracting the enhanced aerosol reflection by SRM".

Page 10. Lines 33-34. The text suggest that decreases in water vapor are due to surface cooling only, where as I would have thought that the robust positive rapid response in E_{wv} (positive) implies that the atmosphere dries out before the surface cools due to reduced convection as soon as the aerosol is added to the atmosphere. If the specific humidity data are readily available, it would be nice to see if this logic holds (i.e. the direct response to SRM is a reduction in relative humidity).

Page 12. Line 5. Confusing intro sentence – I think it meant to say no qualitative differences in E_{wv}. I suggest the following: "WV changes lead to a robust ensemble average increase in surface SW under SRM. This increase in surface SW is due to both the rapid decrease in WV in direct response to SRM ($+0.3 \text{ W m}^{-2}$) and the WV decrease due to surface cooling ($-0.91 \text{ W m}^{-2} \text{ K}^{-1}$ – note the negative sign corresponds to an increase in surface shortwave with cooling)."

NOTE: I think there was a sign error in the manuscript, since the two should have opposite signs if one is cited as a feedback.

Page 13. Line 28-29. Is the robust cloud reduction in the Western Pacific part of the feedback or rapid response to SRM? I don't know the literature well enough to evaluate if this spatial pattern is consistent with the cloud response one would expect from the cooling, the rapid adjustment, or is all together different.

Section 4.2. This whole section confused me. Shouldn't the impact of stratospheric changes in LW emissivity be evaluated from radiative changes at the tropopause after stratospheric temperature adjustment? The argument presented is instead written in terms of TOA radiative response and I'm not sure the sign of the impact is even the same. Additionally, the second paragraph talks about evaluating the impact of stratospheric LW adjustment from the rapid response where I would have though the LW rapid adjustment would have been dominated by the substantial changes in clouds in the troposphere.

Page 17. First line. I would make this statement stronger – one should expect that the rapid adjustment in response to SRM is very different from that due to CO₂ because the vertical distribution of the direct forcing is very different (partitioning of radiative anomalies between the surface and atmospheric column).