Response to reviews

We submitted this article to The Cryosphere as a brief communication after communicating to the editors that our article that was someway between a commentary and a technical review of the sea-level rise response to solar geoengineering. In such articles novelty is not the central goal. However, in responding to the reviewer comments we have added some novel analysis of the surface mass balance response to solar geoengineering. With the revisions recommended by the reviewers we believe this article makes a useful contribution to the discussion on the sea-level rise response to solar geoengineering.

Reviewer 1

This article reviews the links between solar engineering and the surface mass balance of glaciers and ice sheets. Given the potential importance of the topic I am rather reluctant to report this article to be a rather awkward read. It pokes out in many directions, but not sufficiently far enough in any one to be truly novel. Perhaps this opinion reflects that I am well-read on the general topic, and personally feel that this qualitative discussion on cryospheric implications fall short of The Cryosphere community's consistent ability to deliver quantitative assessments on just about every other front. Personal opinion aside, this article objectively resurveys many of the same well-trodden roads of Irvine et al. [2017; Earth Future], Keith and Irvine [2016; Earth Future] and Irvine et al. [2012; Nature Climate Change] – clear disambiguation of a novel core is paramount.

We thank the reviewer for their suggestions and have made several major changes to address the concerns raised and to improve the manuscript:

- We've added a quantitative analysis of the factors driving surface mass balance changes for the GeoMIP climate model ensemble.
- We've restructured the main section of the paper. Sections 3 and 4 from the original paper are now sub-sections of a broader section which frames the issues we address more clearly and also briefly addresses thermosteric sea-level rise.
- We've removed the "sea level rise engineering" section
- We've rewritten the recommendations for research.

P6L15 – "These examples suggest that solar geoengineering would be more effective at changing surface melt than achieving the same reduction in temperature with a reduction in GHG forcing." – A fundamental assertion of this article is that SW reduction is more effective in modulating melt than a LW reduction, but there is a huge body of literature to suggest that melt is LW-dominated. To review Ohmura (2001; J. Applied Meteorology) – under cloudless-sky conditions, 90% of atmospheric emission is derived from the first 1 km of atmosphere – which is why air temperature index can perform remarkably well as a melt proxy. I am not sure that LW modification by GHG drawdown can be ignored entirely.

Ohmura (2001) explains the surprisingly robust physical basis for the surface air temperature driven approaches to surface melt used in many models, highlighting the fact that surface air temperature is strongly correlated with lower atmospheric temperature and hence downwelling longwave (LW) radiation. Against a backdrop of elevated GHGs Solar geoengineering would cool the surface and lower atmosphere and so there will be a significant reduction in downwelling longwave (LW) radiation compared to a case with elevated GHGs and no solar geoengineering. The examples we highlight suggest that all-else-equal offsetting GHG forcing by a reduction in incoming sunlight would produce a greater reduction in melt than an equivalent reduction in CO_2 forcing.

We note the insights of the Ohmura (2001) paper at the start of the surface mass balance section, changing the tone to be less critical of positive degree-day models of surface melt. The new quantitative analysis of the GeoMIP results, which we add at roughly the point the reviewer refers to, describes the changes in surface energy budget that bears out our intuition that solar geoengineering would be more effective at changing the surface energy budget than an equivalent reduction in GHG forcing.

P6L20 – "The effect will be greatest for glaciers and ice sheets that are presently in negative mass balance and have the greatest amount of incoming solar radiation, that is glaciers at low latitudes such as in High Mountain Asia." My understanding is that the stratosphere is several km lower polar areas than at mid latitudes, so the majority of solar geoengineering proposals have advocating for injection aerosols into the polar regions. If this is indeed the case, I am not sure why low latitudes would benefit more from injected aerosols than high latitudes.

The optical depth of the aerosol cloud will determine the fraction of light that it scatters and hence the reduction in sunlight that reaches the surface below. Simply injecting aerosols into the equatorial stratosphere can produce a fairly evenly distributed global aerosol cloud with effects similar to a reduction in incoming sunlight (Niemeier et al. 2013, 10.1002/2013JD020445) though fine-tuning can produce a much more even cloud (Kravitz et al. 2018, 10.1002/2017JD026874). This means that all regions should experience a similar fractional change in incoming sunlight. The fact that the aerosol layer is at a lower altitude at high latitudes should not affect this.

We have edited this section to make clear that a fractional change in incoming sunlight at the ice surface will have a greater effect in sunnier places, i.e. lower latitude regions.

P2L29 – Scalable to 4W/m2. The potential magnitude of SW modification is never compared with characteristic magnitude for SMB components. Fausto (2016; GRL) presents a straightforward radiation balance associated with extreme melt events in Greenland. The article would benefit from a simple thought experiment, whereby a plausible magnitude of SW RF suppression is applied to a summer melt season. The Fausto2016 values, for example have daily mean incoming SW around 100W/m2, with several instances of daily mean sensible heat flux exceed 50 W/m2. Without the authors saying what range of SW modification scenario they deem feasible, it is tough to gauge how that will ultimately effect melt.

The significance of this quoted figure was perhaps not clear so we have added a note in the text that 4Wm-2 is roughly equal to the forcing from a doubling of CO_2. We believe the new quantitative surface mass balance analysis of the GeoMIP ensemble addresses the reviewer's concern here.

Bioalbedo – If 4 W/m2 decreased incoming SW on a total incoming radiation of 150 W/m2 daily mean is being proposed, that is something like a 2.7% decrease in incident radiation. Emerging mechanisms are highlighting much larger changes in melt season albedo. For example, bioalbedo feedback (darkening of the glacier surface due to snow algae) can lower melt season albedo by 13% (or five times as much as the plausible SG mentioned in passing). This sort of contextualization of solar geoengineering is critical but absent from this paper. In jest, one could ask if cryospheric experts would better combat climate change by finding a "cure" for snow algae.

We thank the reviewer for highlighting this omission, the darkening of snow by pollution and by snow algae is an important factor to consider. Snow surfaces with a lower albedo would exhibit a greater sensitivity to changes in incoming sunlight than brighter snow surfaces. This suggests that our quantitive results which focus on the responses over the entire ice-sheet may be underestimating the efficacy of solar geoengineering to reduce melt. We've added some text to explain how the response over darkened snow differs from that over fresh snow and clean ice.

P3L10 – This discussion of the multifaceted effects of aerosol injection seems somewhat cursory/inferior to the tabulated pros and cons of Robock et al. (2009; GRL). I would also note a general absence of comparison with that study, which, for example, yields very different costs estimates of placing 1 Tg S in the stratosphere, and is generally much, much, more negative about the side-effects of geoengineering than presents here.

We didn't believe that a full discussion of the pros and cons of solar geoengineering would be appropriate in a short-format article focused on the cryosphere response, and so included only a brief description of the major side-effects. In introducing the side-effects of stratospheric aerosol geoengineering, we now point the reader to a more up-to-date review of the full effects (Irvine et al. 2016, Wiley Interdisciplinary Reviews). We do not agree with the reviewer's assessment that we have underplayed the side-effects of stratospheric aerosol geoengineering, we believe the text adequately described most of these side-effects. For the shift from direct to diffuse light we've added a brief note on the implications of this shift for plant productivity and concentrating solar power. In terms of the costs of deployment, we refer to more recent estimates than that of Robock et al. (2009) and note that in personal communications Alan Robock agrees with the newer estimates of the costs (personal communication between David Keith and Alan Robock).

Section 5 – This section seems mislabeled as "sea-level rise engineering". One would expect that discussion to move towards how many mm sea-level equivalent may be associated with each geoengineered W/m2, instead this is rather a rehash candidate aerosols with the only tangential brush with sea level being discussion of seasonality of SMB modification.

We have removed this section.

Section 5 – This section is introduced as highlighting why it is "critical to introduce solar geoengineering into such analyses [of future sea level rise]" (P3 L30) – but does seems to miss that mark. Pointing to an IPCC/EGU/EGU community statement on the value of solar geoengineering may serve to anchor the "critical" assertion, but my sense is that international reports generally do not advocate for the inclusion of solar geoengineering as "critical" (i.e. https://eos.org/agu-news/revised-agu-position-statement-addressesclimate-intervention) Perhaps an analogy is a small group of permafrost researchers saying the potential for an Arctic methane bomb is vastly more important than judged by the IPCC. OK, but why? Expand.

The description of the section in this paper that appeared on page 3 was from an earlier draft and did not reflect the structure of the piece we submitted. We no longer make this specific claim.

P6L33 – "As solar geoengineering would lower temperatures and reduce the intensity of the hydrological cycle it would reduce, perhaps even reverse, the negative contribution of Antarctic Surface Mass Balance to sea-level rise." May I highlight his sentence a microcosm of the paper? Unabashed praise for the promise of solar geoengineering with no apparent source for this tremendously speculative statement, and also glazes over/ignores a good deal of cryospheric research that highlights East Antarctic's SMB (the majority of the continent) is net positive, meaning it already draws down sea level today.

Our text here was perhaps not as clear as it could have been. We were indeed referring to the net positive SMB of Antarctica today (which is a net negative contribution to SLR, as we noted) and suggesting that solar geoengineering could potentially offset or even reverse that. We make the same claim in the revised surface mass balance section:

"These results suggest that the negative contribution to sea-level rise of the positive surface mass balance response of Antarctica to global warming would decline roughly in line with temperatures if solar geoengineering were deployed though more work is needed to explore this issue."

P7L20 – This discussion of ice dynamics should more clearly articulate the concept of committed mass loss. I suspect a quantitative assessment of solar geoengineering SMB buffering potential would find that committed loss from Antarctica is substantially larger. It may also be disingenuous to say that SW engineering could counter some of the ice dynamics trends now underway. The major mass loss contributors like Thwaites Glacier do not have ice shelves (i.e. Joughin et al., 2014; Science). The physical basis of committed mass loss purports that once it is triggered, it is only the density difference between ice and water, along with the gradient in bedrock slope, that determines when retreat will stop.

We accept the reviewer's criticism on this point, we perhaps overstated the potential of solar geoengineering in this regard. We have completely rewritten the section (now section 3.3) and end with a more complete discussion of ice dynamics changes that stresses the committed mass loss.

P11L9 – "Solar geoengineering could be deployed to not just reduce sea-level rise but to halt or even reverse it (Irvine et al. 2012)." This sentence is quite problematic. Irvine et al. (2012) only discuss the potential to stop sea-level rise, not reverse it as is being implied by this (self) citation. Keith and Irvine (2016) previous characterize the same study (Irvine et al., 2012) as demonstrating feasibility of solar geoengineering to limit sea-level rise "…by around a quarter". Highlighting these differences in self-characterization of previous studies makes me uneasy, as it seems the current manuscript could be used as a vehicle for expanding, without new foundation, the implications of earlier studies. Here, I caution the editors that it is difficult for me, or perhaps any reader, to comfortably separate conjecture from fact.

The reviewer is right that we make two different statements about the potential of solar geoengineering to change sea-level rise based on the results of a single paper. However, both are appropriate as they refer to different scenarios of solar geoengineering deployment. Irvine et al. (2012) analyzed solar geoengineering scenarios built off the RCP 8.5 emissions scenario with reductions in radiative forcing at 2100 ranging from 2.75 to 9.5 Wm-2, i.e. ranging from scenarios that reduce the warming by around a third (and sea-level rise by around a quarter) to scenarios that reduce temperatures below the pre-industrial mean (reversing recent sea-level rise in these simulations). However, in the revising the text we no longer make this specific claim.

Summary: I might summarize this article as 60% non-cryosphere, which I am familiar with from previous studies, and 40% cryosphere, which I feel is not robust or up-to-date with the present literature. An idealized surface energy budget with and without solar geoengineering modification seem like a minimum requirement to highlight precisely why solar geoengineering is "critical" for the cryospheric community to consider. I get the slight sense that the Brief Communication format here being used more like a popular opinion piece than a substantive review of the subject.

We have revised the paper substantially based on the reviewer's suggestions and hope that these changes address the concerns raised.

Reviewer 2

This article reviews the links between solar radiation management (SRM) and the dynamic and surface mass balance (SMB) of ice sheets. However, there is no effort to understand or even reduce the uncertainty on the ice sheet component under SRM, except to provide an action plan to do this. The focus of climate modelers is on making future scenario based projections of sea level rise with new coupled ice sheet components. There is a long way to go before we can attempt to understand paleo-simulations much less SRM. Since the influence of SRM on ice sheet dynamics is unexplored, I would suggest the paper focus on SMB and should ideally include an analysis, however brief, of the GeoMIP model simulations. The article is bloated in comparison to what can be concluded from the small number of relevant simulations. In addition I find some of the assertions at odds with the references omitted from this review, and these are commented on below.

We thank the reviewer for their suggestions and have made several major changes to address the concerns raised and to improve the manuscript:

- We've added a quantitative analysis of the factors driving surface mass balance changes for the GeoMIP climate model ensemble.
- We've restructured the main section of the paper. Sections 3 and 4 from the original paper are now sub-sections of a broader section which frames the issues we address more clearly and also briefly addresses thermosteric sea-level rise.
- We've removed the "sea level rise engineering" section
- We've rewritten the recommendations for research.

P1:L28. You are referencing 'Expert Judgements' here, which do not really quantify projection uncertainty. The uncertainty should be expressed from model projections as described in AR5 (Ch 13). This is relevant since the next sentence refers to two such projections.

Bayesian statistics is widely applied in the Earth sciences and in sea-level rise projections and provides a framework in which expert judgements can be used alongside other inputs to estimate uncertainty in projections. We believe that it is appropriate to refer to studies which draw on expert judgements of projection uncertainty in this case due to the fact all models miss certain processes which are known to be critical to the future contribution of ice-sheets to sea-level rise. To rely on the spread in model projections alone would be to severely under-estimate uncertainty in ice-sheet contributions to sea-level rise. As our point in this paragraph is to highlight the large uncertainty in sea-level rise contributions from Antarctica we believe it is appropriate to cite studies that illustrate this point using a range of approaches including expert judgement.

P1:L29. Remove 'both of which were published in Nature'. This is a judgement statement implying quality of the referenced research (although this is not the use here, the commonality in source of the papers is irrelevant)!

We’ve rephrased this as follows:

> “For example, two recent high-profile publications made conflicting estimates of Antarctica’s contribution to sea-level rise by 2100 with a best-guess of 10cm (Ritz et al., 2015), and of around 1m (DeConto and Pollard, 2016).”

P1:L29-30. State the period at which these estimates of sea level equivalent apply. 2100?

See last response

P1:L32-35. Evidence required. AR5 (Ch 12 & 13) provides this as does Bouttes (2013) below. Bouttes, N., J.M. Gregory, and J.A. Lowe, 2013: The Reversibility of Sea Level Rise. J. Climate, 26, 2502–2513, https://doi.org/10.1175/JCLI-D-12-00285.1 P2:L1. Carbon removal (e.g. Jones CD et al, 2016, Environ. Res. Lett. 11, 095012).

We thank the reviewer for this useful suggestion which we cite elsewhere, though in this case we have cited Clark et al. (2016) which points out the millennial sea-level rise implications of fossil-fuel emissions (without CDR or solar geoengineering).

P2:L29. RF and GHG not previously defined

We have removed RF as this was the only usage and defined GHG here.

P4:L2-3. This is not self evident. Kravitz et al (2013) suggest that a polar warming might occur with over-cooling in the tropics, when compared against the reference state (Preindustrial). Kravitz, B., et al. (2013), Climate model response from the Geoengineering Model Intercomparison Project (GeoMIP), J. Geophys. Res. Atmos., 118, 8320–8332, doi:10.1002/jgrd.50646.

We have made it clearer in the text that we are referring to the effects of solar geoengineering alone, which cools everywhere, not the combined effect of elevated CO_2 and solar geoengineering. The relevant comparison in that Kravitz study is the abrupt4xCO_2 experiment, not the pre-industrial.

> “As solar geoengineering would reduce temperatures across the world, **offsetting some of the warming from elevated GHG concentrations,** it is clear that to first order it would reduce both the thermal expansion of the oceans and the melting of land ice.”

P4:L9-15. Simple models do not show Greenland ice sheet decline for the strong climate mitigation scenario RCP2.6 either.

We’ve clarified that we are referring to high-CO_2 scenarios here.

P5:L3. Precipitation is decreased except for over the ice sheets (see fig 7 in Kavitz et al., 2013).

We pick this issue up in the revised section on surface mass balance.

P5:17-10. This is definitely not true. Nearly all modern Earth System Models now have a dynamic Greenland ice sheet and a few have mountain glaciers, and they are always, of course, driven by the ESM coupled fluxes (e.g. Lipsomb et al., 2013) . ISMIP6 is NOT using PPD for its offline models. Lipscomb, W.H., J.G. Fyke, M. Vizcaíno, W.J. Sacks, J. Wolfe, M. Vertenstein, A. Craig, E. Kluzek, and D.M. Lawrence, 2013: Implementation and Initial Evaluation of the Glimmer Community Ice Sheet Model in the Community Earth System Model. J. Climate, 26, 7352–7371, https://doi.org/10.1175/JCLI-D-12-00557.1

We thank the reviewer for the correction, our view on this was shaped by our analysis of CMIP5-era models which, as IPCC AR5 WG1 Ch13 p1169, makes clear did not include coupled ice sheets: "Goelzer et al. (2013) and Gillet-Chaulet et al. (2012) suggested that SMB and ice dynamics cannot be assessed separately because of the strong interaction between ice loss and climate due to, for instance, calving and SMB. The current assessment has by necessity separated these effects because the type of coupled ice sheet-climate models needed to make a full assessment do not yet exist."

We have reworded the paragraph as follows:

"Many ice-sheet and glacier models use a simple parameterization of surface mass balance, using a positive degree-day factor to estimate the amount of melt per degree above freezing at the glacier surface (Ohmura, 2001). Degree day factors are determined empirically and vary due to surface albedo, meaning that a weathered ice surface such as the Greenland ice margin are rather dark and have high degree-day factors, while pristine snow cover has a low factor. This degree-day approach has been used in all studies of solar geoengineering's effect on surface mass balance to date, but it has some important limitations."

P6:L34. Actually, the hydrological cycle under SRM is increased over ice sheets (Kravitz et al., 2013).

We have rewritten this section and include results that support the reviewer's assessment.

P7:L13. Need to briefly state what "marine ice sheet instability" actually is. E.g. Grounding-line retreat leads to larger ice mass flux through the grounding-line generating further retreat.

This section has been completely rewritten (now section 3.3) and we include a brief description of marine ice sheet instability.

P7:L17 More precision, perhaps "They suggest that the atmospheric warming that led to the break-up of some Antarctic Peninsula ice shelves would, if the warming continued, destabilize the larger southern ice shelves in the future (Liu et al., 2015). The process is through the hydrostatic head of melt-water filled crevasses which results in "hydrofracture" and the rapid disintegration of the ice shelf." Though actually it is the Ice Cliff Instability (ICI) that is the killer in DeConto and Pollard but the ice shelves need to go first and in any case SRM will never stop ICI. Stick to the key point from this paper is that air temperatures are perhaps important for ice sheet collapse and these can easily be reversed. You are spending too much time on in DeConto and Pollard given the uncertainty they themselves express in the paper. You can be much briefer here.

We have revised this section considerably, reducing the amount of material on the DeConto and Pollard paper and focusing on the potential significance of surface air temperature on ice-shelf stability. We have adopted the phrasing suggested by the reviewer for those sentences.

P8:L3-9. This whole discussion belongs back at the first paragraph of this section. Putting it here leads to a disjointed argument and repetition. Getting circumpolar water up on to the shelves depends on the Ekman pumping which is a function of the circumpolar winds. If the winds shift because of SRM or associated ozone depletion then the basal melt will be different. I have not seen any study of changes in the southern ocean winds under SRM. Intermediate waters are not going to cool significantly on the timescale SRM might be deployed.

We thank the reviewer for this useful suggestion. We have restructured and rewritten this section, brining this point up nearer to the beginning of this section.

P9:L25. Bouttes et al., 2013 is relevant to this discussion.

We thank the reviewer for this suggestion and cite Bouttes et al. (2013) on the reversibility of thermosteric sea-level rise in the new sub-section (3.1) devoted to this issue.

P10:L15-30. A few coupled global climate models are now including an interactive Antarctic and Greenland ice sheet components. Such models would enable a more complete understanding of the impact of SRM on ice sheets, than the doggy offline components.

We thank the reviewer for this suggestion and in the fully revised research recommendations sections, this is our first recommendation.