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Interactive comment on "Brief communication: Understanding solar geoengineering's potential to limit sea level rise requires attention from cryosphere experts" *by* Peter J. Irvine et al.

Anonymous Referee #1

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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.

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Discussion paper

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.

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.

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.

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.

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.

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.

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.

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-addresses-climate-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.

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.

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.

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.

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