Interactive comment on “The climate effects of increasing ocean albedo: An idealized representation of solar geoengineering” by Ben Kravitz et al.

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I am surprised that in discussing an experiment that finds:

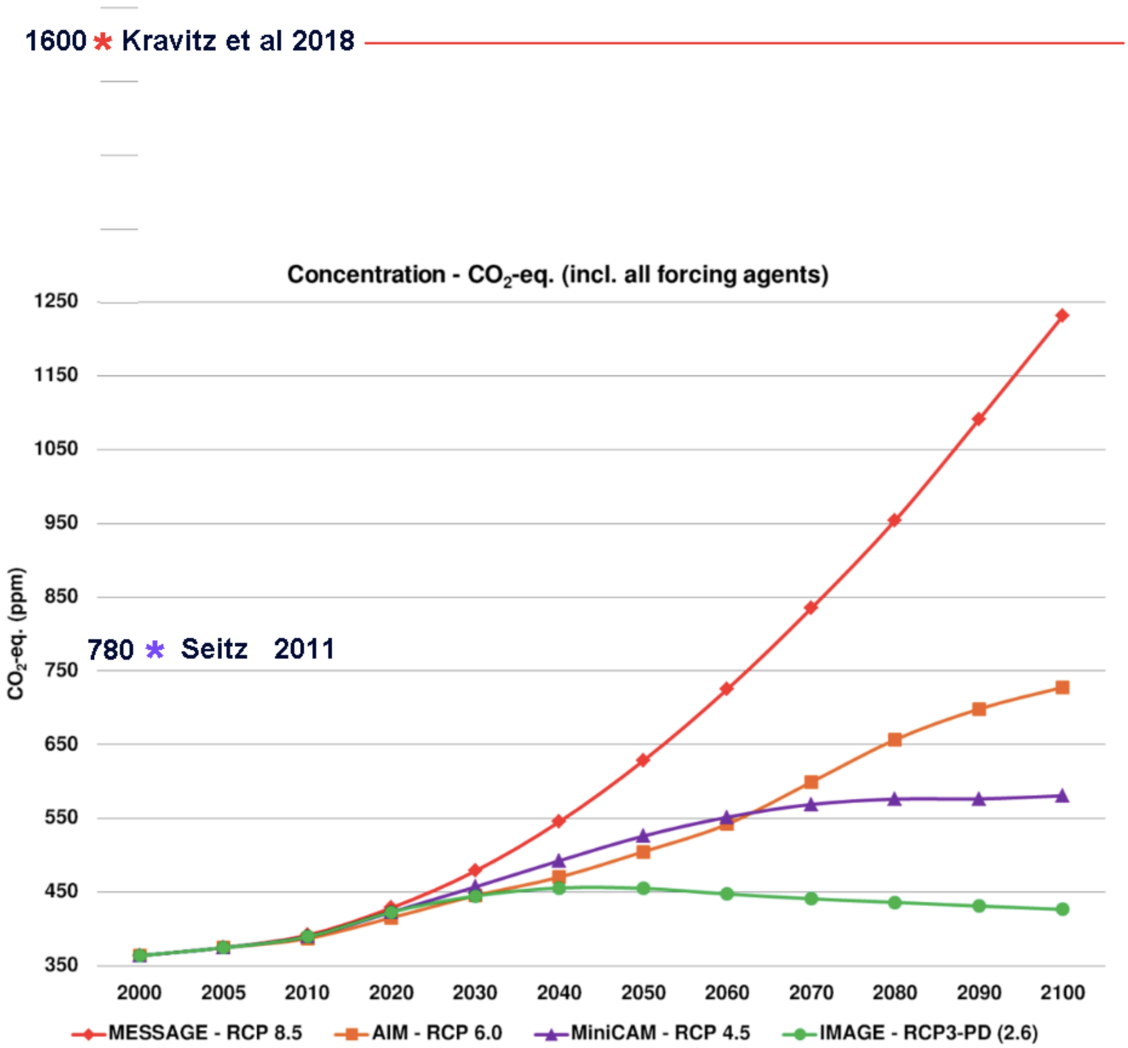
> Eleven Earth System Models are relatively consistent in their temperature, radiative flux, and hydrological cycle responses…

GeoMIP should elide:

> an abrupt quadrupling of ocean albedo to maintain approximate net top-of-atmosphere radiative flux balance “

with the doubling of CO_2 in one of the references it discusses. Seitz 2011 reported the initial CAM3.1 modeling of the combined effect of higher ocean albedo and CO_2 doubling to 780ppm, approximating the forcing in IPCC Representative Concentration Pathway 6.0

In contrast, Kravitz et al 2018 instantaneously quadruple CO_2 to 1600 ppm, considerably outside the IPCC envelope, which only extends to ~1230 ppm CO_2 eq. in RCP 8.5, as seen in the accompanying graph.



While Seitz 2011 is primarily concerned with fresh water conservation, it used the CAM3.1 model to quantify the coupled climate impact of increased sea surface albedo and doubled CO2 forcing ,and found substantial continental cooling in such a global case, using a carbon forcing well within the IPCC parameter envelope- 780 PPM C02 eq. , which approximates the RCP 6.0 projection for 2100.

In contrast, *The climate effects of increasing ocean albedo: An idealized representation of solar geoengineering* considers a more dystopic future.

As the authors candidly note:

> "The results obtained for G1ocean-albedo were to some extent by design. The objective of G1ocean-albedo was to achieve net top-of-atmosphere radiative flux balance, which resulted in warming.
> Conceivably, one could define an objective of no global temperature change, implying a net negative radiative flux at the top-of-atmosphere, or no global land temperature change, requiring adjustments over the oceans to make up the imbalance. It is unclear whether, unlike G1ocean-albedo, such alternate approaches would result in transient behavior that lasts longer than a few years. Such an experiment could be accomplished using feedback methods that have been introduced to geoengineering research in recent years (e.g., MacMartin et al., 2014b; Kravitz et al., 2016)."

It is natural to assume that model intercomparison experiments involve models with comparable forcings, use reasonably physical feedback parametrizations, and rely on the objective description of cited works. This is not evident in the following passage:

> "G1 ocean-albedo may be more apposite to the impact of geoengineering via "ocean microbubbles," whereby surfactants are added to the ocean surface, promoting the formation of microscopic, highly reflective bubbles (Seitz, 2011; Robock, 2011).

This does not represent the content of Seitz 2011, which speaks for itself -- neither its title: *Bright Water: Microbubbles, water conservation and climate change*, or its text contains the word 'geoengineering.'

It discusses the physics of reducing solar forcing in the hydrosphere, not the atmosphere, and while discussing the relationship between microbubble lifetime and variable ocean biochemistry that gives rise to natural albedo effects like undershine, does not presume adding surfactants to promote their formation- none were used in the (physical) experiments in albedo modification it describes.

The authors continue:

> An area of investigation we did not undertake, yet one that repeatedly emerges in discussions of microbubbles is the resulting effects of surface albedo increase on the ocean mixed layer. By reflecting more solar radiation, microbubbles have the potential to inhibit vertical mixing and available light in the euphotic zone, which could have profound effects on marine biota. This implies that another useful future area of investigation for the G1 ocean-albedo simulation is an analysis of the marine carbon cycle.

This reflects Robock 2011, an Editorial Comment in *Climatic Change* entitled ‘Bubble Bubble Toil and Trouble’ and these issues are addressed at some length in in Seitz 2011, which called for their investigation by ecologists and systems biologists. Readers should also note that as modeled with CAM3.1, water brightening reduced peak water surface temperature, which could promote, rather than inhibit, diurnal mixing, by reducing thermal stratification.

I appreciate the utility of idealized simulations, but fear this article invites something best avoided—policy analysts may confuse idealizations with models of the real world. It would clearly be desirable to apply approximations of water reflectivity and temperature less naive that simple Mie theory, or unmixed slab models, to the complex evaporative response of both fresh water reservoirs and the sea surface, with a view to achieving less idealized and more realistic representations of solar radiation management, especially on local scales.

Once again my thanks to Ken Caldeira for the CAM-1 modeling that informed Seitz 2011.