

Expert judgements on solar geoengineering research priorities and challenges

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Introduction

Figure S1 shows similar results as Figure 3 in the manuscript but for a different level of funding and Figure S2 shows a break-down into sub-categories of research from Figure 3. Figure S3 illustrates the online implementation of our survey and Figure S4 shows a plot that appeared as part of the notes for Question 11.

Text S1 presents the survey questions in full. Text S2 presents the additional text comments that survey participants provided for the quantitative questions 2-6, those for which we allowed comments. Texts S3, S4, S5, S6, S7 presents the full text responses to the qualitative questions 7, 8, 9, 10 and 12, respectively.

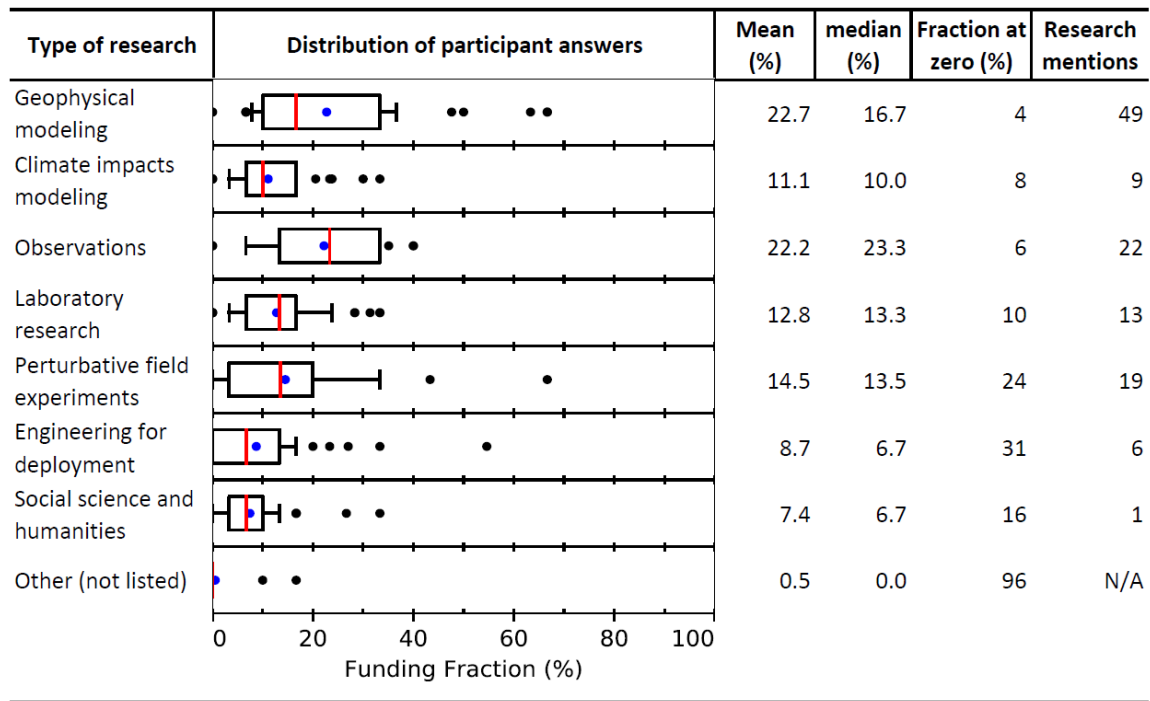


Figure S1. As Figure 3 but for \$30M budget. Box-Whisker plots showing the distribution of participant answers to Question 4: “Please allocate funding across the below types of research in a way that reduces the overall uncertainty about efficacy and risks of deploying solar geoengineering. Please assume a budget of \$30M for the US over 10 years.” 49 participants completed this question. This is plotted in the same way as figure 1. For each research type we report the median and mean value for relative funding, the fraction of participants who gave each objective zero priority, and we show the number of mentions in specific research proposals (Question 7).

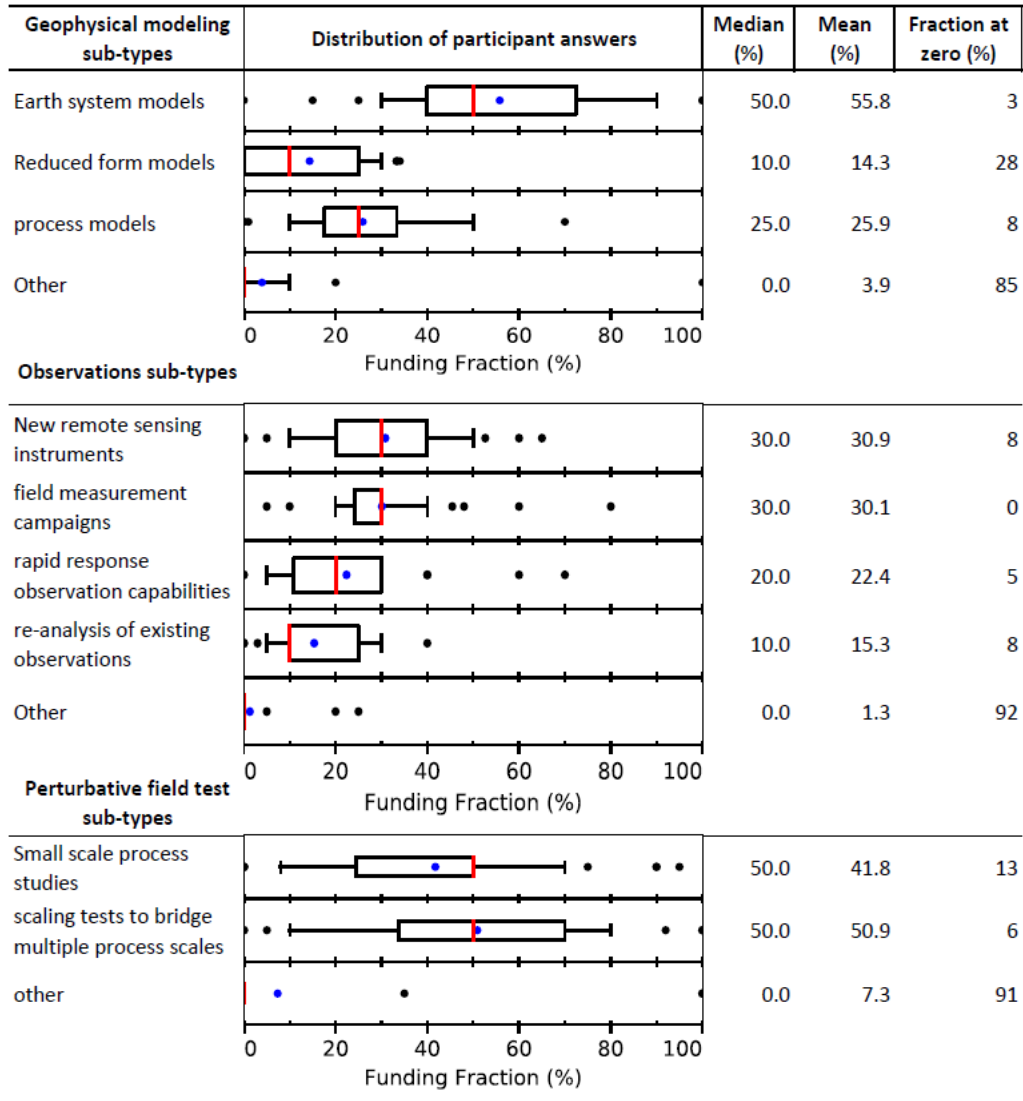


Figure S2. Box-Whisker plots showing the distribution of participant answers to Question 4: “Please allocate funding across the below sub-types of research in a way that reduces the overall uncertainty about efficacy and risks of deploying solar geoengineering. Please assume a budget of \$300M for the US over 10 years.” 32 participants completed this question. This is plotted in the same way as figure 1. For each research sub-type we report the median and mean value for relative funding, and the fraction of participants who gave each objective zero priority (Question 7).

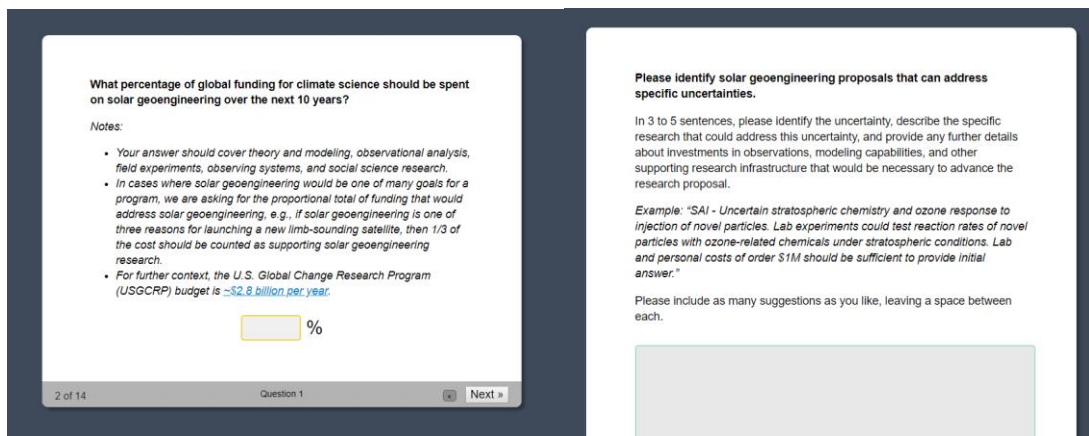


Figure S3. Screenshots of Questions 1 and 7 as participants saw them on the online survey platform.

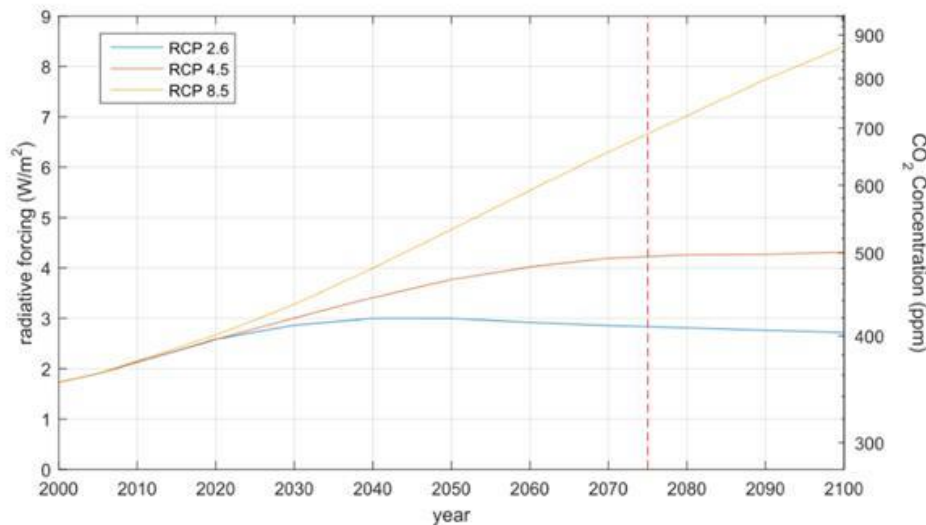


Figure S4. This plot was included as part of Question 11 of the survey with this caption: 'The radiative forcing estimated by the IPCC "Representative Concentration Pathways" (RCPs) are plotted for your reference. Values plotted here are total radiative forcing from long-lived greenhouse gases, short-lived gases (each accounting for net positive radiative forcing), aerosols and precursors (which account for net negative radiative forcing), and other relatively small changes (surface albedo and solar irradiance changes).'

Text S1. Survey questions in full

Introductory page

Objective – The goal of this survey, and the discussion which will follow, is to identify research priorities for solar geoengineering (also known as albedo modification, solar radiation management, etc.) from the natural science community working on this topic.

We would like you to assume that there is funding over the next 10 years to advance understanding of the feasibility of solar geoengineering, its potential biophysical consequences, and its potential impacts on ecosystems and society.

The scope of this imagined research agenda is deliberately very broad, including everything up to the point of deploying solar geoengineering. This excludes climate response tests, which we consider a form of deployment.

Importantly, we fully recognize that many experts (including those on our own organizing team as well as those taking this survey) may disagree with the scope of this imagined research agenda. We chose to use such a broad scope so that we could learn experts' positive and negative views on a range of topics.

Solar geoengineering proposals – Throughout we focus on leading solar geoengineering proposals with the potential to exert a large-scale cooling effect on the climate. The leading proposals we address are:

- Stratospheric Aerosol Geoengineering
- Marine Cloud Brightening
- Cirrus Cloud Thinning
- Land Surface Albedo Modification
- Ocean Albedo Modification
- Space-Based Methods

In the following questions, there will be the option to identify other geoengineering proposals not listed above. However, please exclude carbon dioxide removal proposals as they are beyond the scope of this exercise.

Thank you for your participation!

Question 1: What percentage of global funding for climate science should be spent on solar geoengineering over the next 10 years?

Notes:

- *Your answer should cover theory and modeling, observational analysis, field experiments, observing systems, and social science research.*
- *In cases where solar geoengineering would be one of many goals for a program, we are asking for the proportional total of funding that would address solar geoengineering, e.g., if solar geoengineering is one of three reasons for launching a new limb-sounding satellite, then 1/3 of the cost should be counted as supporting solar geoengineering research.*
- *For further context, the U.S. Global Change Research Program (USGCRP) budget is ~\$2.8 billion per year.*

____ %

Question 2: How would you prioritize the following research objectives to best support decision-making on solar geoengineering over the next 10 years?

Notes:

- *Objectives marked with [*] are defined further in the optional follow-up question, where respondents are asked to consider specific sub-categories.*
- *Please indicate priority by listing a percentage. Please use the “scale your allocation” button on the bottom left to ensure your answer sums to 100%.*

Please scale your allocation to 100%:

- Understanding and predicting the Earth system response to solar geoengineering [*]
- Understanding and predicting the physical consequences of solar geoengineering-driven climatic changes on human and ecological systems [*]
- Understanding human social dimensions of solar geoengineering [*]
- Developing or improving methods for solar geoengineering [*]
- Developing weather control applications of solar geoengineering
- Developing counter-geoengineering
- Other(s) (please specify below)

If other(s), please explain (optional).

Optional: How would you prioritize the following research sub-objectives to best support decision-making on solar geoengineering over the next 10 years?

Notes:

- *Please indicate priority by listing a percentage. Please use the “scale your allocation” button on the bottom left to ensure your answer sums to 100%.*

Optional Question 2A: Understanding and predicting the Earth system response to solar geoengineering

Please scale your allocation to 100%:

- Predicting radiative forcing from solar geoengineering
- Predicting climate response
- Predicting oceanic response
- Predicting biogeochemical response (including carbon cycle)
- Predicting cryospheric and sea-level response
- Predicting atmospheric chemistry response (troposphere and stratosphere)
- Other(s)

If other(s), please explain (optional).

Optional Question 2B: Understanding and predicting the physical consequences of solar geoengineering-driven climatic changes on human and ecological systems

Please scale your allocation to 100%:

- Human health (heatwave mortality, vector-borne disease, etc.)
- Agriculture and forestry
- Hydrology (water resources, drought, etc.)
- Storm damage (storm surges including sea-level rise, wind damage, etc.)
- Terrestrial ecosystems
- Fisheries and oceanic ecosystems
- Other(s)

If other(s), please explain (optional).

Optional Question 2C: Developing or improving methods for solar geoengineering

Please scale your allocation to 100%:

- Evaluating feasibility of solar geoengineering proposals
- Exploring alternate materials and means for solar geoengineering proposals
- Developing approaches to monitor and control solar geoengineering
- Advancing engineering research to understand deployment systems
- Developing novel ideas for solar geoengineering
- Other

If other(s), please explain (optional).

Optional Question 2D: Understanding human social dimensions of solar geoengineering

Please scale your allocation to 100%:

- Public perception and engagement
- Policy, law and governance
- Ethics and other implications
- Economics
- Other(s)

If other(s), please explain (optional).

Question 3 and 4 Description:

In the following questions we will ask you to allocate funding to different types of research for a hypothetical US research effort.

We would like you to consider how you would allocate funding at two different US budget levels over the next 10 years:

- Smaller Budget - \$30M
- Larger Budget - \$300M

These budgets should not be interpreted as recommendations. We offer two distinct funding levels because we recognize that some (though not all) respondents may allocate funding differently for larger and smaller budgets.

We provide cost estimates for certain types of research to help inform your allocation.

We also provide a text box for you to comment on your answer.

Question 3: Smaller Budget

In this question we will ask you to allocate funding to different types of research for a hypothetical US research effort into solar geoengineering. We would like you to allocate funding for a smaller budget of \$30M over the next 10 years. We will repeat this exercise for a larger budget in the next question.

Below, we provide cost estimates for certain types of research to help inform your allocation.

Satellites: between ~\$10M and ~\$800M; Adding a sensor to planned satellite: between \$1M and \$100M

- *Example: The cost cap for small satellites funded by a particular NASA program (ESTO-InVEST) is \$1.5M/year for 3-4 years ([link](#)). NASA has not funded a stratospheric sensor yet via this mechanism but a sensor (e.g., a step or two before the satellite) of the size class was funded to measure stratospheric aerosol ([link](#)).*
- *Example: The cost cap for larger projects is higher. The 2017 Decadal Survey establishes some new cost categories: "Continuity" \$150M, "Explorer" \$350M, and other cost caps, such as \$800M, for observing aerosols, clouds, convection, and precipitation ([link](#)).*
- *Example: An additional sensor to a satellite could cost between \$1M and \$100M, depending on the sensor size and complexity.*

Small Scale Field Experiments: between ~\$3M and ~\$30M

- *Example: The NASA Earth Venture Suborbital program ([link](#)) has a cost of \$30m for "large" investigations and \$15m for "small" investigations.*
- *Example: The engineering flight for SCoPEX is expected to cost ~\$3M ([link](#)).*

Laboratory Experiments: between ~\$100k and ~\$10M

- *Example: NASA's Atmospheric Composition: Laboratory Research provides ~\$250k/year for 3 years ([link](#)).*
- *Example: NSF's Major Research Instrumentation (MRI) funds up to \$4M for a multi-investigator large program. Track 1 MRI proposals are those that request funds from NSF greater than or equal to \$100k and less than \$1M; and Track 2 MRI proposals are those that request funds from NSF greater than or equal to \$1M up to and including \$4M ([link](#)).*

Please allocate funding across the below types of research in a way that reduces the overall uncertainty about efficacy and risks of deploying solar geoengineering. Please assume a budget of \$30M for the US over 10 years.

Note:

- *Research types marked with [*] are defined further in the optional follow-up question, where respondents are asked to consider specific sub-categories.*
- *Please use the “scale your allocation” button on the bottom left to ensure your answer sums to \$30 Million.*

Please sum your answer to \$30M:

- Developing and applying geophysical models [*]
- Developing and applying models of the societal and ecological impacts of solar geoengineering and climate change
- Observations including field experiments, remote sensing, and reanalysis, but excluding perturbative experiments [*]
- Laboratory research
- Perturbative field experiments [*]
- Engineering research on deployment systems
- Social science and humanities research
- Other(s)

Please use this text box to comment on your answer, e.g., to explain what research would fall under the “other” category, to briefly explain your answer, to highlight research types or foci not included in our list, and to comment on the question (optional).

Optional: Please allocate the percentage of funding that should go to the below sub-types of research in a way that reduces the overall uncertainty about efficacy and risks of deploying solar geoengineering.

Notes:

- *Please bear in mind the total funding you have allocated to each type of research above and the estimated costs for different research types.*
- *Please use the “scale your allocation” button on the bottom left to ensure your answer sums to 100%.*

Optional Question 3A: Developing and applying geophysical models

Please scale your allocation to 100%:

- General Circulation Models / Earth system models
- Reduced form models (e.g., Earth system models of intermediate complexity)
- Process models (e.g., Lagrangian models, Chemistry-Transport Models, etc.)
- Other(s)

If other(s), please explain (optional).

Optional Question 3B: Observations including field experiments, remote sensing, and reanalysis, but excluding perturbative experiments

Please scale your allocation to 100%:

- Developing and deploying new remote sensing instruments (e.g., satellites)
- Field measurements campaigns (e.g., aircraft missions)
- Rapid response observation capability (e.g., volcanic eruption observation)
- More thorough analysis of existing observational data, including developing new data analysis methods
- Other(s)

If other(s), please explain (optional).

Optional Question 3C: Perturbative field experiments

Please scale your allocation to 100%:

- Small-scale process studies (e.g., SCoPEx experiment which will release between 100g and 1kg of material)
- Scaling tests to bridge gaps across multiple process scales (e.g., marine cloud brightening test spanning microphysics, large eddy simulation and mesoscale models)
- Other(s)

If other(s), please explain (optional).

Question 4: Larger Budget

As in the previous question, we will ask you to allocate funding to different types of research for a hypothetical US research effort into solar geoengineering, but we would like you to consider a larger budget of \$300M over 10 years.

Below, we provide the same cost estimates for certain types of research to help inform your allocation.

Satellites: between ~\$10M and ~\$800M; Adding a sensor to planned satellite: between \$1M and \$100M

- *Example: The cost cap for small satellites funded by a particular NASA program (ESTO-InVEST) is \$1.5M/year for 3-4 years ([link](#)). NASA has not funded a stratospheric sensor yet via this mechanism but a sensor (e.g., a step or two before the satellite) of the size class was funded to measure stratospheric aerosol ([link](#)).*
- *Example: The cost cap for larger projects is higher. The 2017 Decadal Survey establishes some new cost categories: "Continuity" \$150M, "Explorer" \$350M, and other cost caps, such as \$800M, for observing aerosols, clouds, convection, and precipitation ([link](#)).*
- *Example: An additional sensor to a satellite could cost between \$1M and \$100M, depending on the sensor size and complexity.*

Small Scale Field Experiments: between ~\$3M and ~\$30M

- *Example: The NASA Earth Venture Suborbital program ([link](#)) has a cost of \$30m for "large" investigations and \$15m for "small" investigations.*
- *Example: The engineering flight for SCoPEX is expected to cost ~\$3M ([link](#)).*

Laboratory Experiments: between ~\$100k and ~\$10M

- *Example: NASA's Atmospheric Composition: Laboratory Research provides ~\$250k/year for 3 years ([link](#)).*
- *Example: NSF's Major Research Instrumentation (MRI) funds up to \$4M for a multi-investigator large program. Track 1 MRI proposals are those that request funds from NSF greater than or equal to \$100k and less than \$1M; and Track 2 MRI proposals are those that request funds from NSF greater than or equal to \$1M up to and including \$4M ([link](#)).*

Please allocate funding across the below types of research in a way that reduces the overall uncertainty about efficacy and risks of deploying solar geoengineering. Please assume a budget of \$300M for the US over 10 years.

Note:

- *Research types marked with [*] are defined further in the optional follow-up question, where respondents are asked to consider specific sub-categories.*
- *Please use the “scale your allocation” button on the bottom left to ensure your answer sums to \$300 Million.*

Please sum your answer to \$300M:

- Developing and applying geophysical models [*]
- Developing and applying models of the societal and ecological impacts of solar geoengineering and climate change
- Observations including field experiments, remote sensing, and reanalysis, but excluding perturbative experiments [*]
- Laboratory research
- Perturbative field experiments [*]
- Engineering research on deployment systems
- Social science and humanities research
- Other(s)

Please use this text box to comment on your answer, e.g., to explain what research would fall under the “other” category, to briefly explain your answer, to highlight research types or foci not included in our list, and to comment on the question (optional).

Optional: Please allocate the percentage of funding that should go to the below sub-types of research in a way that reduces the overall uncertainty about efficacy and risks of deploying solar geoengineering.

This question had the same text as questions 3A-C

Question 5: Indicate the relative priority of research into these solar geoengineering proposals.

Note:

- *Please indicate priority by listing a percentage. Please use the “scale your allocation” button on the bottom left to ensure your answer sums to 100%.*

Please scale your answer to 100%:

- Stratospheric Aerosols Geoengineering
- Marine Cloud Brightening
- Cirrus Cloud Thinning
- Space-Based Methods
- Land Surface Albedo Modification
- Ocean Albedo Modification
- Developing Novel Proposals
- Other 1
- Other 2
- Other 3

If other(s), please explain (optional).

Question 6: Please rank these proposals in terms of the likelihood that they can achieve >2 Wm-2 of radiative forcing at an acceptable economic and environmental cost. You can also choose, “not at all likely.”

- Land Surface Albedo Modification
- Space-Based Methods
- Cirrus Cloud Thinning
- Marine Cloud Brightening
- Stratospheric Aerosol Geoengineering
- Ocean Albedo Modification

If you added new card(s), please explain (optional).

Question 7: Please identify solar geoengineering proposals that can address specific uncertainties.

In 3 to 5 sentences, please identify the uncertainty, describe the specific research that could address this uncertainty, and provide any further details about investments in observations, modeling capabilities, and other supporting research infrastructure that would be necessary to advance the research proposal.

Example: “SAI - Uncertain stratospheric chemistry and ozone response to injection of novel particles. Lab experiments could test reaction rates of novel particles with ozone-related chemicals under stratospheric conditions. Lab and personal costs of order \$1M should be sufficient to provide initial answer.”

Please include as many suggestions as you like, leaving a space between each.

Question 8: What finding would cause you to abandon research into solar geoengineering in general or into a specific method?

Imagine that after a decade of research you found out something that caused you to abandon research into a specific solar geoengineering proposal or solar geoengineering generally.

Identify the solar geoengineering proposal and the show-stopping finding (don't be afraid to include out-there suggestions!). If you can, please describe specific research that could discover whether this concern is valid in one or two sentences, and briefly provide any further details.

Example: "General - The climate response to solar forcing is fundamentally mischaracterized by current climate models. Alternative types of climate models (cloud-resolving, etc.) should be employed to validate the findings of conventional GCMs."

Please include as many suggestions as you like, leaving a space between each.

Question 9: What common misconceptions about solar geoengineering should be understood as one puts together a research agenda?

Please identify common misconceptions about solar geoengineering that could result in the misallocation of resources in a solar geoengineering research agenda.

Identify the specific misconception, and if you'd like, describe approaches to addressing it.

Example: "General - solar geoengineering would inevitably weaken the hydrological cycle."

Please include as many suggestions as you like, leaving a space between each.

Question 10: Please identify novel challenges for solar geoengineering research in general—or for specific solar geoengineering proposals—that would need to be addressed in a research program.

Solar geoengineering poses several novel challenges for climate research and climate policy.

Please identify any novel challenges for solar geoengineering research and suggest approaches for how they could be addressed through a research agenda.

Example: “General – solar geoengineering can be tailored to achieve different objectives; thus scenarios of deployment depend on objectives to be pursued.”

Please include as many suggestions as you like, leaving a space between each.

Optional Question 11:

The radiative forcing estimated by the IPCC "Representative Concentration Pathways" (RCPs) are plotted for your reference. [See Figure SX]

Optional Question 11A: What is your best estimate of the radiative forcing in 2075? Please first include the 90% upper bound and 10% lower bound of your subjective probability, and then indicate its expected level.

Notes:

- *Please use your own best judgement of the political and economic realities of global climate change, accounting for your judgments about changes in climate science and energy technology.*
- *Values plotted here are total radiative forcing from long-lived greenhouse gases, short-lived gases (each accounting for net positive radiative forcing), aerosols and precursors (which account for net negative radiative forcing), and other relatively small changes (surface albedo and solar irradiance changes).*
- *Assume solar geoengineering is not implemented.*

90% upper bound: ____ Wm⁻²

10% lower bound: ____ Wm⁻²

Expected level: ____ Wm⁻²

Optional Question 11B: What is your desired anthropogenic forcing in 2075? If this answer is lower than your estimate of likely climate forcing in 2075, please indicate how much forcing is from solar geoengineering.

Assume:

- *You are charged with designing a climate policy that mitigates risks caused by climate change.*
- *You may achieve your goal through emission reduction and solar geoengineering.*

Notes:

- *Please use your answer from above as a baseline for your answer here.*
- *You should consider realistic trade-offs between climate goals and social/economic impacts.*
- *You can choose whether or not to implement solar geoengineering to achieve your goal.*

Desired anthropogenic forcing: ____ Wm⁻²

Anthropogenic forcing from solar geengineering, if any: ____ Wm⁻²

Question 12: Do you have any final comments? What else should we have asked? What should be raised in our discussion on December 11th?

Please make any final comments and describe any issues you feel we should have included in this survey and that we could address in our discussion.

Please include as many suggestions as you like, leaving a space between each.

Please press "submit elicitation" in the lower right corner when you have completed the survey.

Text S2. Full text responses to Questions 2-6.

Questions 2-6 were primarily quantitative questions, but we gave the participants the option to add comments.

Question 2: How would you prioritize the following research objectives to best support decision-making on solar geoengineering over the next 10 years?

Developing the observational capacity necessary for evaluating solar geoengineering. If this is to happen, we need to be able to monitor the global system in much more detailed ways than we currently do. This might be under "developing or improving methods for solar geoengineering", or under "understanding... the earth system response to solar geoengineering", or perhaps even under "understanding... the physical consequences of solar geoengineering climatic changes on human and ecological systems", but I think it is important enough to be its own category. We need to understand what impact our actions are having in order for any "engineering" to happen (i.e. to assess the situation and modify the plan). Similarly, we need to know the baseline variability in the systems of interest. Again, to have any control, we need to know where we are starting and what changes are attributable to the intervention vs. natural variability. As such, the percentage filled out in Optional Question C) below, for "Developing approaches to monitor and control solar geoengineering" refers only to control. Monitoring is too big to be a subcategory.

Troposphere aerosol application of environmentally benign materials for SRM has been neglected. The priority should be given to identifying such materials and appropriate methods for distribution.

The virtue of tropospheric intervention is the concept of 'Primum non nocere'
The experiment can be stopped in the event of unexpected consequences.

Geoengineering research in developing countries and Spreading the awareness among the public about geoengineering

Other counts as something we haven't considered or that your definition of some of these terms differ from mine (for example human social dimensions). I am wondering for

example, where one might place "compensation for negative consequences of geoengineering or geoengineering research"

Identify localized applications (e.g., reduce loss of mountain snow pack/glaciers) and local benefits to build support

Developing and evaluating plausible scenarios and preparing for initial deployments for regional intervention to limit significant impacts, such as to limit polar warming, extreme tropical cyclone intensification, coral reef protection

Understanding economic, legal and political dimensions of solar geoengineering

First, develop a robust, enforceable, and international governance regime for both the RESEARCH and DEVELOPMENT of geoengineering. Before any IMPLEMENTATION occurs, create a viable international treaty organization on geoengineering that both: 1) significantly promotes the use of mitigation if any geoengineering is implemented, and 2) significantly reduces the risk of international geoengineering-related conflict. Such a treaty organization must have the full participation of the United States, China, and most other large and medium national powers.

Others: contingency-- "unknown unknowns"

Optional Question 2A: Understanding and predicting the Earth system response to solar geoengineering

Most of these impacts questions should be addressed by normal global change research, so it is only the delta for solar geo that needs to be funded.

Cryosphere response

Again, I feel there may be research areas that are not covered by current categories. Where for example do implications of geoengineering on energy availability fall?

Polar region impacts

If you are evaluating stratospheric aerosols, you must also consider the effects of suddenly stopping the treatment on all the above in question B. And include the human health effects on respiratory function - increased asthma, for instance.

Optional Question 2B: Understanding and predicting the physical consequences of solar geoengineering-driven climatic changes on human and ecological systems

As far as I am concerned oceanic response is a climate response, the carbon cycle is a climate response. I have answered the question as if it were "atmospheric circulation, temperature, and precipitation response (including extreme events)"

See response to 1 above

(Answerer mentioned: Troposphere aerosol application of environmentally benign materials for SRM has been neglected. The priority should be given to identifying such materials and appropriate methods for distribution.)

Predicting societal response to geoengineering

Again, there are probably components that may not be adequately covered by the current categories. For example a better understanding of the role of component processes (e.g. aerosol/cloud interactions) in the response, or the importance of climate feedbacks in the coupled response, or how earth system components interact. It is also not clear to me why oceans and cryosphere have been singled out, while the atmosphere has not.

Impacts on human and natural systems

Local impacts

Extreme weather amelioration

Some of these questions are hard to answer as I have different mental models of what solar geoengineering is. with SAI i don't care much about predicting radiative forcing, but MCB maybe I would invest more. So, I am thinking of SAI.

Wild fire response

Be sure to include effects on ozone layer, effects on ocean acidification, and effects on crop yields.

Optional Question 2C: Developing or improving methods for solar geoengineering

I'm not sure what fits into some of these categories. E.g., understanding what latitude or season or altitude to inject aerosols and how those affect outcomes (or the corresponding variables for MCB), are those "alternate means"?

Same. I am trying to allocate some flexibility in the program for unanticipated issues

Exploring natural analogs and field testing localized approaches and impacts

Evaluating analog situations, such as volcanic injections, changes in sulfate distributions due to coal-fired power plant emissions, etc.

What constitutes a solar geoengineering proposal?

Research on gaps between lab/small scale test and large/global scale deployment.

Optional Question 2D: Understanding human social dimensions of solar geoengineering

I think social dynamics of how solar geo could influence mitigation efforts, etc, would be worth looking into. Does economics mean 'cost'? That is how I am answering it. Is unclear that there is much pure economics research for solar geo."

Integration studies. These four topics are highly intertwined, so there needs to be some room for studies of how to integrate the competing priorities.

Understanding how implementation of geoengineering would influence future CO2 emissions

Same. I am trying to allocate some flexibility in the program for unanticipated issues

Economics should include assessments of prospective costs of liability for loss and damage and geopolitical conflict.

Co-development of proposals (emphasize that engagement should be more than one-way)

"Ethics and other implications" is what? That makes Ethics hard to assess and leaves "others" as what? This list leaves a lot to be desired. I think could've benefited from a more detailed list just like the past two questions. Maybe reflects biases of the research team. Others: Behavioral and psychological drivers, cultural dimensions

There are social dimensions that are basically embedded in the natural science consequences, but not covered by this schematic "that lie somewhere between this and optional question B. Let's call it "coupled human-natural systems" research for short, which is something the NSF can also parse.

Note: my policy, law, and governance is a separate "other" category of 15% in the first question above. In that first question, I'm thinking in terms of CREATING viable governance systems. In this question D I am thinking of how to better understand human-geoengineering interactions, make them more democratic and cost effective if implemented.

Military strategy, International relations

Question 3: Please allocate funding across the below types of research in a way that reduces the overall uncertainty about efficacy and risks of deploying solar geoengineering. Please assume a budget of \$30M for the US over 10 years.

I had some problems in thinking about the different research objectives/disciplines (in this question and other question) as I think that research on solar geoengineering requires an interdisciplinary approach, for example, (perturbative) field experiments are most likely be prepared by laboratory research, conducting the field experiments should be complemented with studies how people perceive this kind of field experiments and the insights of the field experiments (about cost etc) provide input for improvement economic analysis. Furthermore, in allocating the 30 Million budget, I thought about in which field we have already a somewhat good understanding and where not, therefore I allocated a higher amount to the second category compared to the first category so that this topic can catch up, however, in general I consider the first category at least equally if not more important than the second category.

Identify potential natural analogs and develop readiness to deploy

Your question is not clear if you are talking about \$30M over a decade or over a year for 10 years (which is what I am assuming for this case based on you asking for the percentage out of the annual USGCRP budget). \$30M to cover a decade would be ridiculously small given the predicament the world is in.

I think combining social and natural research questions under the same budget is a bad idea. First of all, few people knows what it takes to do research in social sciences and natural sciences. The examples provided did not cover social sciences. It inevitably leads to ranking one science over other, while it really reflects on the needs of the widely different research programs. For example, when I allocate 5 million to social sciences I am increasing current funding 10 fold.

The geophysical models should be developed based on more accurate field observations, especially stratospheric aerosol observations. Observation system should be ready for the next volcano eruption and gain more valuable information. The development of ecological model should be coupled with the climate model, as ecosystem feedbacks on climate system.

I think that large scale research on deployment should not begin before we have functional global mitigation governance. If we can't get us on track to net zero emissions in a coordinated way, it's too risky to create SRM capability.

Most of the funding should be dedicated to developing the observational network (which would be useful with/without geoengineering) and to small-scale field experiments. The social and natural science research should be allowed to continue in earnest.

Small scale field experiments could probably go ahead, but only after the robust governance system described above is in place. Most learning about the physical processes may likely come from satellite observations. Social science research could result in novel ways for society to mitigate much more as it learns about the full ramifications of geoengineering, the technocratic management of the global environment.

I argue for little funding for impact studies because I consider that in this area a lot of basic research is needed that is not specific to geoengineering. While it is true that also numerical modelling and climate observations for

geoengineering will benefit from more general climate research developments, I think that in particular further stratospheric research is strongly needed which is currently less in the focus of climate research beyond the geoengineering issue, see e.g. the scarcity of future limb sounders.

I think, first, it is important to increase understanding of the climate system by small scale perturbation experiments and laboratory research before doing engineering and deploying research.

For a \$30m a year 10 year programme I think the priority would be to try out marine cloud brightening and get some sense of stratospheric chemistry responses to SAI, hence hardware heavy spending choices

It's too early to do engineering research on deployment systems with such a small investment. This budget distribution recognized that field activities and remote sensing are more expensive than modeling; however, the modeling aspects should be considered a strong focus throughout.

The amount for observations is based on my expectation that the development of AirCore technology will go smoothly and continue to be funded by NASA irrespective of its benefit for solar geoengineering measurements. Once it is validated for relevant measurements (e.g. aerosol, strat. ozone, etc.), it holds great promise for a cheap worldwide observing system. A large number of AirCore launches would establish the baseline far better than we have so far--an Argo program for the atmosphere. With such a small budget, I think the social science and humanities research around ethics and governance would be my next priority behind establishing an accurate baseline. As for a comment on the question, 3M/year is a small number to think about for the scope of geoengineering research.

Laboratory research is prioritized in this allocation to establish the size, morphology, particle separation, phase function of aerosol candidates

> You fail to define ""perturbative"". Having read the question below, I realize that you think Scopex is perturbative, which is not what I would have called it. It is a process experiment that results in no perturbation to the climate system.

> There is little reason to explore research on development systems when the funding is insufficient to carry out a experiment that can actually result in a perturbation to the climate system.

- > "Other" includes research on detection. One of the most difficult problems is how to detect and monitor changes.
- > While I think that modeling impacts is important, we know far more about that at this point than we do about the actual means of doing geoengineering, so it has a lower priority.

I don't think, I can comment on the \$\$ for different areas, this required an assessment of research needs and requirements in different areas.

I think that aside from some already defined activities (SCoPEX and MCB tests), pretty much everything else is IMO still dependent on actually doing some more representative modeling than has been done to date. But, being somewhat prepared for future volcanic eruptions seems pretty important too.

This amount of funding is completely inadequate to produce a credible assessment of the viability, potential or implications for geoengineering. I have chosen to allocate the token funding to support some research into the theoretical implications of geoengineering based on existing knowledge, and that gleaned from relevant research being done for other research (e.g. fundamental climate research), or and to support token funding for a few feasibility studies of processes in the lab, and exploration of implications to society.

Three comments:

- (1) The premise that research investments at this or the larger scale will ""reduce overall uncertainty in the efficacy and risks of deploying solar geoengineering"" is an assumption that is not inherently accurate.
- (2) In my view, perturbation experiments intended to have an impact on radiative forcing at a level that would yield a detectable climate response should not go forward in the foreseeable future.
- (3) In my view, field experiments in solar geoengineering should only be funded by governments and other entities that are themselves committed to supporting deep reductions in carbon emissions as the primary means to limit warming. In this context, I don't accept the premise that the US government should be funding such research. If, hypothetically, the US government changes its position in this regard, I would, hypothetically, be inclined to answer this question.

Question 4: Please allocate funding across the below types of research in a way that reduces the overall uncertainty about efficacy and risks of deploying solar geoengineering. Please assume a budget of \$300M for the US over 10 years.

I think that a larger budget makes the prospect of imminent implementation more likely, and therefore a greater proportion of the research budget should be allocated to the practical precursors to implementation (field experiments and deployment systems). In contrast, developing new computer models becomes less important, since those models have been continuously improved for several decades now, and non-geoengineering science funding also goes towards improving those models. Note that every individual category still increases relative to the 30M scenario though. Also, social science becomes more important as actual implementation is approached, since a successful implementation will have to navigate the social environment just as well as it navigates the physical environment.

> I have allocated about half the money to field tests. My expectation is that even with this greater funding, it will still take a decade to understand how to do solar climate engineering and whether it is feasible at a control level. This militates against funding deployment.

> Funding for modeling is hard to define because substantial modeling needs to be done to support "perturbative" experiments and it is unclear where this fits.

> I certainly see the need for remote sensing and instrument development but the funding levels here are actually inconsistent with doing much in that area.

ethical and geopolitical implications

See previous answer.

Expand observations, including staging for natural analogs (e.g., volcanoes), test localized interventions

Again, the question is not really clear on if \$300M total for 10 years or if this is a per year budget. Given the need to not only stay below 1.5 C but push the warming back to less than 0.5 C, the program needs to be \$300M/year.

The optional questions below do not address a detailed distribution for social science and humanities research. Again, reflects on biases of the team but also I am starting to perceived this as lack of genuine interest on these topics by the research team. Maybe get a group of people to design an independent survey that focuses on social sciences.

With a Large Budget, a new satellite system might be able to be developed, launched and maintained. This system could be designed to monitor and answer specific questions from geoengineering.

All forms of research are important. Above, I have allocated more to improving the observational network, performing perturbative field experiments and engineering as they are comparatively more expensive, and will have additional utility outside of the solar geoengineering agenda.

To be honest, I would like to just copy/paste my allocations from the previous section, except for the allocation to field experiments, which I think should stay at a level below \$5 million.

Again, I think, the most important research need at the moment is to fully understand the climate system first. Therefore, it is essential to develop and implement measurement techniques for stratospheric aerosols and its radiation effects. Furthermore, laboratory sundries and small scale perturbation experiments could also contribute.

The field testing should include and feature technologies to restore lost ice and snow. This is the single largest restorative lever we have on climate change. By restorative, it is meant that it builds back a natural system, such as ice in the Arctic, that until recently was there.

Again with the larger budget, modeling should be a strong focus throughout. We will soon have a paper arguing why feedbacks will dynamically alter deployment strategies. However, a large investment is required to "instrument" and prepare for any action, and this level of commitment warrants initial investment in perturbative field experiments.

Question 3A: Developing and applying geophysical models – smaller \$30M

Theory (or extremely simple models) can still be valuable.

Regional models (domain size of a few thousand km). These studies are needed to understand the implications of regional forcing such as that produced by MCB or cirrus thinning. GCM's have insufficient resolution to address these issues.

See comment above.

(Answerer mentioned: I don't think, I can comment on the \$\$ for different areas, this required an assessment of research needs and requirements in different areas.)

I'm not convinced there's much more to learn from intermediate complexity models. But MCB needs non-GCM, as does plume mixing for H₂SO₄ injection (e.g.)

Statistical downscaling techniques (other) and other diagnostic tools (e.g. regional climate models) will also be useful. Also, I would concentrate on developing Integrated Assessment Models (also other).

I think it is important to do apply all kind of models, because each of them has advantages and disadvantages when looking at various processes with different time and spatial scales.

Put some money into terraforming research for other planets -- it is more likely to find unknown unknowns and generate new thinking

Question 4A: Developing and applying geophysical models – Larger \$300M

~one postdoc for 2 years on theory.

Same comment as earlier. There is a need to model processes at the large regional scale to understand effects of non-uniform forcing.

Identify tipping points and feedbacks in climate system for prioritizing climate intervention development (allocation decision based on which models best able to accomplish this)

Maybe use machine learning to mimic human decision, and couple that with the Earth System Models

Question 3B: Observations including field experiments, remote sensing, and reanalysis, but excluding perturbative experiments – smaller \$30M

AirCore (see above) could become nearly as ubiquitous as ozone sondes. It seems like NASA and other agencies are likely to have the limb measurements we need for continuity. If that continuity is in danger (e.g. the upcoming missions get

canceled or ACE-FTS or MLS die), then all of the observational money (and any additional money) should be spent immediately to get a new MLS up ASAP. We need to preserve our long continuous record of the stratosphere to properly characterize the variability. Of course, developing a plan for rapid response observation would be fantastic, but with this budget, we'd have to mostly rely on NASA HQ for that. Additional analysis of the existing observational data, especially ACE-FTS data, holds great promise for understanding the baseline and our current ability to monitor geoengineering. I would suggest a number of OSSEs could be included with analysis of global climate model output.

The share of 10 percent for the first category is restricted to "developing" as with the low overall funding it is not feasible to deploy satellites.

Identifying and testing sites for deployment of tropospheric and surface approaches

Question 4B: Observations including field experiments, remote sensing, and reanalysis, but excluding perturbative experiments – Larger \$300M

AirCore, lidar.

Maintain and expand current monitoring networks

Surface observing systems need enhancement

Field measurements using satellites wherever possible and practical should be used rather than airplanes. Less GHG emissions, less logistical challenges.

Question 3C: Perturbative field experiments – Smaller \$30M

Intervention in the troposphere with environmentally benign materials must be explored further. Field testing even in limited form must find acceptance to a skeptical public.

Our community needs to develop a rational response to the most severe critics (e.g. Klein, N., 2014. This Changes Everything. Chap 6 pages 256-290. Simon and Shuster, New York, ISBN 978-1-4516-9738-4.

We need to put the cards on the table rather than presenting ourselves as a small group of elitists who are playing god

Note: these categories are not mutually exclusive and it is difficult to arrive at a clear distinction of the difference between these two categories.

Short-term terrestrial and marine ecosystem responses to geoengineering (e.g. photosynthetic rates and atmosphere-land carbon and moisture fluxes.)

I ran into some inconsistency problems with this question. I consider both categories equally important. Given my overall budget share I allocated to perturbative field experiments I am not sure whether it would be possible to achieve anything meaningful in the second category (scaling tests to bridge...). Anyhow, I decided to express that I consider both categories equally important.

I remain unconvinced that SCoPEX will be able to do what it is trying to do, it being just too small a study (injection of a kilogram is minuscule)--larger experiments will still be minuscule in a global context and need to be pursued.

I put \$0. I don't think any money should be spent on either of these.

Field experiments require permitting. Most likely to receive permits are localized surface albedo modifications. These are very high-impact, to rebuild brightness, esp ice and snow, where until recently they were present. This should be prioritized.

Question 4C: Perturbative field experiments – Larger \$300M

Same comment as before. These are distinctions without much difference and therefore hard to differentiate.

Long-term terrestrial and marine ecosystem responses to geoengineering (e.g. photosynthetic rates and atmosphere-land carbon and moisture fluxes.)

Allocate based on prioritized interventions to address feedbacks and tipping points

The "other" field testing meant here is really not perturbative - it should include and feature technologies for localized albedo modification, such as that to restore lost ice and snow. This is the single largest restorative lever we have on climate change. By restorative, it is meant that it builds back a natural system, such as ice in the Arctic, that until recently was there.

Question 5: Indicate the relative priority of research into these solar geoengineering proposals.

This is not really a separate proposal per se, but I think that more research should go into targeted interventions, rather than interventions that aim to fix the entire planet's climate all at once. Nobody actually experiences the global average temperature, after all. An intervention targeted towards minimizing deadly heat waves or ice sheet mass loss might be more prudent than an intervention that aimed to fix all of our problems at once. It may well be that some of the other methods are also useful for targeted interventions; this is more a change of goal rather than a new type of intervention per se.

I do not know the state of the science on the efficacy and practicality of these various methods.

I've heard the experience with ARPA-E is experts put too much allocation into their first choice, and often it is the 2nd or 3rd choice that ends up the winner.

Other 1: biogeochemical feedbacks from the terrestrial ecosystems
Other 2: biogeochemical feedbacks from the marine ecosystems

Ocean pipes to bring cooler deep water to the surface

Research needs to be performed in all of these areas, but it should not take away any funding from climate research in general.

I have allocated funding based upon my intuition about viability (thus supporting stratospheric aerosol research), scientific importance (supporting MCB research which may be effective, and research would certainly contribute to critical fundamental research for climate science), and "novel proposals" which me is shorthand for "other". We need to keep looking, but I don't think any of the other strategies mentioned are sufficiently promising to name them explicitly

Marine cloud brightening research is worthy of support in the context of better understanding cloud feedback in the climate system, rather than as a priority approach to intervene in the climate system.

1) Other: Combinations of the above selected methods.
Furthermore, I consider Land surface Albedo modification an important topic for

regional climate management (i.e. with respect to the mitigation of heat waves), however, I do not consider it to have a significant contribution on the global level.

Identify climate feedbacks and tipping points and use available observations and models to identify which methods map to prioritized climate interventions

My additional area of investigation would be clear sky albedo modification (similar to sulfate effect at present

Again, what constitutes a geoengineering proposal? How do I answer these if I do not know the intent and scale?

No other, but I want to justify my choices. SAI is by far the most researched and well understood. MCB is inherently dangerous and may not be efficacious (I do not trust GCM cloud schemes). CCT is unproven and unfeasible. Space-based methods may offer more control over the distribution of forcing and should be further researched. Land albedo modification may produce localized cooling effects. Novel approaches should have some form of funding.

By novel, I would focus primarily on improving stratospheric aerosols methods to lower the risks of ozone depletion, regional disparities, acid deposition, and others (with different particle types and deployment patterns) -- which is currently being done.

Question 6: Please rank these proposals in terms of the likelihood that they can achieve $>2 \text{ Wm}^{-2}$ of radiative forcing at an acceptable economic and environmental cost.

I do not feel comfortable answering this question. I have no idea what is an "acceptable environmental cost", since those ethical and moral judgments are not my expertise.

Strat aerosols is really the only game in town. All the rest are just ornaments.

I am not an expert in all these areas. I know that global reductions of solar radiation can archive this, but if this is technical possible is not clear at this point.

Whether or not the economic and environmental costs (and geopolitical costs) are "acceptable" depends greatly on who one might be asking. Absent

some shared understanding of the premise, I don't see how this question can be answered.

There is considerable potential for brightening clear sky areas over remote ocean regions that have very low albedo, so a large relative influence. Sulfate aerosols now are generally concentrated over relatively limited land areas; what needs to be considered are low loading of bright aerosols out over remote dark ocean areas.

I don't think any can produce an acceptable environmental cost.

please disregard my check marks for the first two (I can't undo them)

Sea ice albedo modification

Comment: 2 Wm^{-2} is quite a lot!!

Text S3. Full text responses to Question 7.

Question 7: Please identify solar geoengineering proposals that can address specific uncertainties.

Uncertainty in biogeochemical feedbacks from the terrestrial and marine ecosystems. These two largest carbon pools may either become a source (more effort in geoengineering) or a sink (less effort in geoengineering) of carbon depending on different regions. Fully-coupled Earth System modeling development and simulations can help quantify the strength of these feedbacks and thus reduce the uncertainty in how much effort to be put in geoengineering. Lab and personal costs of order \$150K should be sufficient to provide initial answers.

Stratospheric aerosols
understanding the creation, transport and removal of particles. Modeling and process observations.

N/A

SAI - uncertain decision-making process. Modelling results that show how uncertainties impact policy outcome. Modelling outcome can be used to address various impacts, that contribute to this process. Modelling work would cost ~0.5 M (?) for some initial results

SAI - main uncertainty is on its impact on the Ozone layer. There is also uncertainty in how the upper level clouds would be affected because of the heating from aerosols when they are closer to the tropopause. About \$ 10 M could be invested to provide some initial answers on both of these uncertainties.

All SRM proposals - there is larger uncertainty on how much the productivity of the terrestrial ecosystems would be reduced. This would need larger investments (\$50M) in earth system modeling and process modeling

Marine cloud brightening: Very large uncertainty in precipitation response. I do not recommend this proposal at all.

Ocean pipe: The feasibility of cooling the surface by bringing deep water should be explored. Initial \$10 M investment may be needed. I do recognize that this is not an SRM proposal.

Using more sophisticated earth system models to evaluate and study the impacts of solar geoengineering. Carrying out more lab experiments to calibrate the solar geoengineering related model parameters and processes.

The development of Earth System Model, as well as process scale models is very important to reduce uncertainties in our understanding of impact of climate change and geoengineering. Confidence in models can be gained by have sufficient observations that help identify the state of the climate system, as well as changes to forcings, for example after volcanic eruptions. Improved design of future scenarios would be helpful that take into account CDR and SRM methods. Experiments need to be well supported by model simulations and lab studies. At this point, a lot can be learned from models, in particular whether geoengineering can be used for the common good.

SAI - uncertainty in response to changes in the partitioning of diffuse/direct radiation. Lab and field measurements of radiation fractions and productivity could help constrain responses, assuming the measurements were across different seasons, plant types, ecosystems, biomes. Investigate trade-offs between light stress and other plant stressors (temperature, drought, nutrients) and how SAI compares to climate change. What are the implications for crop yields, phenology, vegetation dynamics? All solar geoengineering proposals - uncertainty in ecological responses as it relates to biodiversity. Would solar geoengineering help or hurt biodiversity relative to climate change? What about possible co-benefits with carbon uptake (land and ocean)? Need observations and modeling to answer these questions. Better satellite and in situ observations of terrestrial hydrology (soil moisture, runoff, transpiration), vegetation productivity would help constrain models more broadly, not just for solar geoengineering applications. More research into how we model changes in biodiversity (e.g., climate envelopes or dynamic ecosystem models).

SAI/MCB - Assess sensitivity to different uncertainties (e.g. through perturbed-physics simulations), while controlling to manage desired outcomes (e.g., change injection rate as needed to maintain global mean temperature) so that one isn't comparing apples and oranges. A few \$M would provide vastly more objective knowledge than we have today about which uncertainties matter (which today is primarily subjective).

SAI/MCB - Better quantify how uncertain different parameters are by more carefully constructed model intercomparison studies (that get at why models respond differently vs simply pointing out that models respond differently), and assess whether model differences remain consistent with observations. Clearly not a panacea, but a few \$M, combined with the above, would help better define which uncertainties are priorities for being reduced - rather than simply looking for our keys under the streetlight (i.e., rather than conducting the experiments we know how to do).

I won't provide concrete proposals for SAI, but I suspect that proposals in the range of \$5M could provide some useful information any of the following topics, each of which would be relevant:

- 1) technologies for producing aerosol particles with specific size and composition;
- 2) the scattering and absorption of said particles as a function of wavelength;
- 3) the reactivity of particles, and the surface chemistry that might take place on those particles;
- 4) the time evolution of particles when exposed to specific environmental conditions (changes in size, number, etc)

There is a pretty carefully formulated experimental design for MCB studies offered by Wood, Ackerman, and colleagues, that ramps up from 1) design of sprayers to produce sea spray; 2) tracking the evolution of these particles; 3) determining the buoyancy of particles after emission; 3) tracking the mixing of particles with their environment (e.g. mixing with plume); 4) tracking the mixing of the particles between surface and top of boundary layer; etc. These phased series of experiments and accompanying modeling calculations have already got \$ associated with them. The justification for the experiments are offered in the recent paper by Wood et al, 2017

Ecosystem functioning - Uncertain consequences for ecosystem processes under atmospheric conditions with high CO₂, cooler temperature, shifted precipitation, and more scattered light. Manipulative field experiments (cf. FACE).

Discontinuation syndrome - Uncertain ecosystem/agricultural production responses to discontinuation of geoengineering measures. Increases systems understanding.

Improved understanding (and modification) of cloud microphysical properties-- requires ESM improvements, improved observations (i.e. use the IMO sulfur regulation

from 2020 onwards as natural experiment), and field tests. I assume at least USD15 M are required to make sufficient progress.

Regional climate response to SAI--requires, both improved ESM simulations to project changes in temperature and precipitation on shorter time-scales than a year. General equilibrium model analysis is required to translate these regional changes into economic effects. An integrated research project with 5 years duration and about USD 3 M should significantly improve our understanding

Model testing of potential analog situations, such as volcanic eruptions, present sulfate loading (changing amounts, locations, and distributions over time--so reductions in SO₂ emission in North Atlantic basin and into Arctic (might it be that reduced sulfate is contributing to faster sea ice loss than climate existing climate models are projecting). Notion is to evaluate the aspects of models that will be perturbed through climate intervention approaches. Would likely require \$5-10M for personnel and computational time) spread over several years.

Stratus cloud modification--moving from one to fleets of dispersing ships to get much better understanding of what is possible. Probably \$10M/yr for 5 years.

Clear sky aerosols-- evaluation of the potential for surface introduction over remote ocean regions. Model evaluation if it would have the desired effect on climate and not unduly disturb the weather, as well as field testing of creating the layer, evaluation of potential supplies of aerosol materials and various injection mechanisms. Likely \$5M/yr for 5 years.

Evaluation of potential for application of available techniques to moderate the most significant climate change impacts, including moderating Arctic warming, reducing warmth of ocean areas where heat is supplied that leads to tropical cyclone intensification, coral bleaching, potential modification of storm tracks in favorable years to moderate aridification of areas where the subtropics is expanding. Probably \$8M/yr for 5-10 years--focusing on modeling initially and then moving into perturbative field experiments.

SAI - uncertain political response to research and development. Surveys and psychological/behavioral economics laboratory studies. Collections of observational data that can reflect the issues involved with high-leverage technologies (e.g. nano-tech, nuclear, etc)

The biggest uncertainty in SAI right now is in the microphysical parametrization of SO₂ to sulfate aerosol oxidation. Mostly because all our models are tuned to Pinatubo AOD, and to obtain it every model uses a different amount of SO₂. So we are not sure that the AOD we simulate in our different injection scenarios would be correct. To improve this, our best option is to be ready for the next Pinatubo-like event in terms of

all possible measurements, from SO₂ amount to plume evolution.
So balloons, LIDAR, satellites all over the globe.

SAI - Uncertain carbon cycle response to temperature, radiation and precipitation. Development of earth system model could test the response of terrestrial and oceanic ecosystem (including wildfire changes), carbon flux exchange between atmosphere, land and ocean, and their feedback on climate. Costs of order \$2M should be sufficient to provide initial answer.

Uncertainty: Has any human society ever achieved a consistent programmatic intervention / infrastructure project over a 200-year span, and if so, what were the features of success? \$500k for historical analysis, case studies, and expert workshops.
Uncertainty: Once solar geoengineering is begun, how can it be linked policy-wise with CDR and mitigation? \$500K for serious, worldwide, multi-stakeholder research on plausible linkages.

We need many more studies on the impacts on agriculture and ecosystems of already proposed schemes. We need more climate model simulations of overshoot scenarios. And we really need governance (of research and possible implementation) and ethics research.

cirrus clouds - need field observations to monitor physical and chemical state of clouds across full latitude and seasonal ranges - no real idea of the aircraft cost or if a satellite could do the job

Land albedo - how sustainable is an initial growth in desert environments - how much water or nutrient needs to be supplied annually - models disagree widely on these numbers - small scale field tests could determine these quite well, with added ecological service benefits, but potentially high political risk in Saharan countries. Could be maybe combined with/subsidized by solar power facilities.

Regional SAI - how well could regions e.g. polar sea ice & permafrost be cooled in spring/summer without risk of aerosol spread or remote climate impacts. Better process models of ocean-sea ice-Antarctica and Greenland could do these. Main limit is the need for much more data on Southern Ocean, Greenland shelf and sub-ice shelf processes and mass and energy exchange between ocean/sea ice/atmosphere. Observing buoys and autosubs could do this for <10 million

Generally I think that natural laboratory experiments have not received enough attention. Degassing volcanic eruptions can provide much large scale guidance as to the efficacy of cloud brightening. Current cloud brightening proposals are severely hampered by the divergence in GCM fast responses ie the second indirect effect.

SAI/Cirrus seeding - Take the novel particles proposed for SAI and measure their ice nucleating abilities in the lab to evaluate their freezing properties under lowermost stratospheric/upper tropospheric conditions. Run GCM simulations with the particles as new tracers and simulate their ability to modify cirrus clouds. Costs: <200 K USD.

Cirrus seeding - Study natural analogues to cirrus seeding in form of Saharan dust events, which frequently bring dust over Central Europe. Do optical properties of cirrus change compared with background conditions? Use available satellite and ground-based lidar and radar data to start with, together with some idealized modelling. Costs: 100 K USD. Aircraft campaign follows as its stage 2 to in-situ sample cirrus clouds and analyze ice crystal residuals to understand how they formed. Costs: 2 M USD.

Albedo modification both over sea ice, land ice and land could be field tested, simulated using realistic climate modeling, studied for impacts over near term as well as in the long term. Budget may be about 5 million for initial testing and simulations.

Marine cloud brightening.

Ocean albedo enhancement by introducing bubbles

Regional changes in temperature, precipitation, aridity and other relevant variables are very uncertain at the moment. It is not well understood which regional changes are linked to 'side effects' of geoengineering in general (such as stratospheric heating) and which could be avoided if the strategy of where stratospheric injections are placed for example is altered. Multi-model studies could address this issue IF there were enough models capable of capturing all the relevant processes: interactive aerosols, chemistry, ocean, etc. Investment in improving climate models would aid this issue. Cost \$1M - 5M. Progress on this question can be made with one existing model by altering geoengineering strategies, however that will not expose uncertainties due to parameterization of basic physics (clouds, convection, etc) in that particular model. Progress could be made with \$1 - 2 M.

Evolution of aerosols, chemistry, and heating in the stratosphere at varying levels of injections (especially high) is a large uncertainty: different models predict a very different amount of stratospheric heating for the same amount of injection (of SO₂ for example). Magnitude of changes in the troposphere depends on the amplitude of stratospheric heating, hence it is very important to understand how much the stratosphere would heat with injections. A combination of observations and model studies would likely be most useful here, with collaborations from multiple modeling groups working on increasing certainty in aerosol and chemistry parameterizations. Not sure about amount of funding, but given that observations would be needed in the stratosphere, I would estimate this would be at least \$10M.

Related to above, changes in stratospheric heating change the dynamics in the tropical stratosphere, including the QBO. At the moment there is uncertainty in how the QBO will change even without geoengineering: some models predict shortening, some predict

lengthening of the QBO cycle. These changes in turn affect tropospheric variability. The uncertainty here comes from parameterized gravity waves in GCMs and this is a huge 'black hole' in models with no obvious solution. Mesoscale numerical modeling studies along with observations could make progress, but not fully answer question as observations into the future won't be available.

Uncertainty: Global and local radiative effect of widespread, thin tropopause cirrus clouds and their response to increasing aerosol number and increasing LS temperature. A comprehensive approach requires in situ (aircraft) observations of formation, remote (satellite, lidar) studies of frequency and lifecycle, and validation of process and global models that may then predict responses to perturbations.

SAI - uncertain atmospheric chemical response. Could be addressed with lab or perturbative experiments.

"SAI - impact of stratospheric aerosols on vegetation - multiple ways of approaching this, including lab experiments and latest land surface models

SAI - diminishing returns - aerosol coagulation/condensation/nucleation rates could be further measured in labs and field experiments e.g. scopex. Impacts on ozone chemistry of solid aerosols? May also be tested in lab and with models

SAI - aerosol optical properties - specifically refractive indices for dry and coated solid particles are poorly constrained (even sulphate) - probably why Kleinschmitt (2018) gets a different answer to Niemeier and Timmreck (2015). A lab-based project with full-spectra refractive indices would be very useful

SAI - deployment schemes - some suggest modified aircraft, others bespoke aircraft, technology is unproven and will require investment in passive/active emission and what substance to emit/carry (S, SO₂, H₂SO₄, SO₃, etc)

Land albedo modification - how useful is LAM at addressing urban heat islands? Perhaps consider current albedo of cities and determine temperature magnification? What are the global implications of regional LAM applications, specifically on hydrology - use climate models

SAI - impact of SAI on storms - Use high-resolution climate models to model storms - specifically intensity, location and number changes. Does clustering frequency change?
TBC

I ran out of time to answer this question, sorry.

CCT - Recent research indicates anomalous ridging along the coast of N. America (related to increasing wild fires) may be related to increased cloudiness at high latitudes,

especially cirrus clouds. It is possible that CCT could reduce this ridging. Climate modeling experiments involving at least two GCMs could provide a sufficient initial answer for about \$ 2-3 M over 3 years.

Airborne/satellite field campaign to improve knowledge of ambient aerosols. It's not clear that current knowledge of sulfate aerosol precursors can explain observed sulfate aerosol distribution. There is significant organic carbon containing aerosol in the stratosphere, and its budget is not well understood. Detailed in situ measurements and global satellite measurements are necessary to address this uncertainty. This pertains to SAI.

A key uncertainty is the socio-economic response. If the "ideal" situation is to use SRM as a temporary measure over a few decades to get the world past this emissions "hump," how do we ensure that 1) mitigation really does take place simultaneously, and 2) SRM really is ramped up and then ramped down in a way that is managed internationally and cooperatively. It is hard to think of other examples when global powers have cooperated on a 30-40 year global project of such magnitude and with such uncertainty. A team of policy scientists, political scientists, and economists should be able to lend insight.

There are of course tons of uncertainties for a multitude of suggested methods. My suggestions are based on the consideration that SAI (with sulfur) is the most likely method to be chosen simply because the natural analog of volcanoes exists. Hence I'm less inclined to suggest research in other methods although questions may be even larger. I'm somewhat favorable towards MCB and cirrus thinning research because this may provide scientific advancements in the understanding of clouds that is very welcome anyhow.

SAI - Development and distribution of substances like sulfur in the stratosphere is still fairly uncertain (as can be seen from model spread), however important to define resulting forcing. I do see options to advance stratospheric modeling (e.g. going to gravity-wave resolving resolutions) and the necessity to have well resolved stratospheric trace gas observations to evaluate models.

SAI - Effects on tropical hydrology are still fairly uncertain (as again can be seen from model spread). My hypotheses is that this is related to imperfectness of convection parameterizations. The only way to possibly improve this is to go to convection-resolving model resolutions, which is not only an issue for research of SAI effects, of course, but if one considers solar geoengineering seriously I think such an approach is necessary.

SAI - aerosol evolution and chemical effects maybe researched in ScoPex-like experiments

Cloud studies (both cirrus and marine stratocumulus) - There are still large uncertainties in cloud microphysics which may be tackled in perturbation experiments. Cloud microphysics may also turn out to be a crucial source of model spread once other parameterizations (convection) become more and more obsolete at higher resolutions.

Climate consequences of stratospheric aerosols. This will require greater investments in both climate models and response models.

Tradeoffs between increased temperatures and increased ocean acidification under different CO₂ mitigation / solar geoengineering scenarios in terms of impacts on marine ecosystems and climate feedbacks. Coupled earth system modeling experiments including ocean biogeochemistry and plankton communities. Uncertainties still remain in plankton community dynamics in response to climate and CO₂ change - could be supported by laboratory or field experiments. All in all ~\$5M effort.

Uncertain terrestrial and oceanic biosphere response to stratospheric aerosol injection, marine cloud brightening, cirrus cloud thinning, etc. Further development of process-based models and model-intercomparison could help reduce the uncertainty.

Model inter comparison studies could identify the uncertainty in negative side effects such as stratospheric ozone depletion, stratospheric warming, impacts on clouds, and on the climate response of different solar geoengineering methods. This could be done by international collaboration of different climate modelling groups across the globe. Funding of \$500K would be sufficient to conduct such studies

How well if at all does marine cloud brightening work, and over how wide a range of clouds? Needs to be tested in the field, with moderately large diffusers, first static, then at sea, and a lot of observation. As mentioned, this should be big theme of \$30 programme

Effects of SAI on atmospheric chemistry, much as discussed above, with assessment of a range of particles.

Health effects of non-sulphate particles. Comparative toxicology I guess.

Effects of SAI on tropopause height and permeability, and secondarily on stratospheric dynamics. This I guess has to be modelling.

Hydrological cycle--modelling and maybe palaeo

No real idea of costs -- don't think I can add value there.

Climate modeling resources need to be sufficient to model worldwide impacts of various solar geoengineering proposals, with an eye to benefits and risks to human populations and the ecosystem worldwide - and any impacts of stopping the proposed treatments as well.

SAI - Lab experiments and modeling are required to better understand uncertainties associated with stratospheric chemistry, injection strategies, changing circulation and deposition rates, ecological impacts, and feedbacks to the climate system. Needed are more observations and remote sensing monitoring capabilities that could better utilize natural injections from volcanoes. \$10M would provide an initial seed activity coordinated through a mission-oriented agency (e.g., US DOE) that would fund both university and Lab research.

How the stratospheric aerosol layer would respond, and importantly how it would vary year-on-year, across different stratospheric dynamical regimes, is a key “science unknown”, that needs to be identified carefully for the different hypothesised stratospheric SRM methods. I am particularly interested in the identified concept of a “target aerosol distribution” for the perturbed stratospheric aerosol layer which seems to me to be an important research question, which has only recently started to be identified in geoengineering research.

A key caveat within current review articles and assessments such as the 2015 “Climate Intervention: Reflecting Sunlight to Cool Earth” report from the US National Academy of Sciences and other related US national science agencies involves how the particle size distribution of the stratospheric aerosol layer would vary (spatially and temporally) in such a scenario. A major step forward in model capability in recent years has been a new generation of composition-climate models (CCMs) which transport aerosol particles (and their precursors) and represent microphysical processes, enabling to investigate much more realistically how the stratospheric aerosol layer responds to the hypothesised continuous injection of sulphur or particles, compared, for example, to a one-off volcanic injection of sulphur. By combining predictions with the interactive stratospheric aerosol CCMs and climate/earth-system models being applied for CMIP6, there is the potential to identify important new understanding in this area.

In the next 5-10 years I think we can expect several of the CMIP6 models to have a configuration aligned to CMIP6 also with interactive stratospheric aerosol modules, which then provides a more flexible and realistic basis for fully integrated assessment of the effects on when future climate warming targets and tipping points could potentially be delayed with geoengineering scenarios within CMIP6 scenario runs.

SAI- uncertain ice sheet response to geoengineering. In order to understand the effectiveness of solar geoengineering in limiting ice sheet mass loss, we need better understanding of surface mass balance, along with better understanding of ice sheet surface (and near-surface) hydrology, better understanding of how solar geoengineering may change the winds and ocean circulation near ice sheet margins, and a better understanding of the relationship between surface melt and crevassing, fracture, and iceberg calving. These questions require a variety of models, including regional atmospheric models, coupled ice-ocean models, surface hydrology models, continuum ice flow models with damage representations, and discrete element models of calving and fracture processes. These questions also require a variety of observational data, including field measurements of surface mass balance and hydrology, oceanographic measurements from the sub-ice cavity, high-frequency measurements of ice velocity, and better remotely sensed characterization of the melting and fracture state of the ice surface.

As a social scientist, I can only comment on social uncertainties. These are difficult to reduce, but some research activities can contribute. Public opinion studies, including controlled experiments and more open dialogues, can increase in questions' depth and participants' breadth. Furthermore, studies of elite decision-makers (for lack of a better phrase) are currently missing. Likewise, systematic developing and game-testing of socio-political scenarios can reduce governance uncertainties, at least to a modest degree. These two examples require modest financial investment.

Uncertain natural variability of stratospheric circulation and lower stratospheric ozone. Analysis of MLS and ACE-FTS data could examine natural variability of the stratospheric circulation, the natural variability of lower stratospheric ozone, and the response of circulation and ozone to volcanic aerosols. Chemistry-Climate Models might be necessary as well, since the observational record is so short. \$250k for personnel and computing

Uncertainty in observation system necessary to enact solar geoengineering. Once goals are defined for the geoengineering to control (e.g. Arctic and Antarctic temperatures less than xK for mean and yK for extremes) and the tradeoffs are established (e.g. a maximum x% decrease in mean midlatitude stratospheric ozone), an observing system needs to be developed that can measure the appropriate variables to within an acceptable uncertainty. This is a complicated problem that will involve a control component and an (or a series of) Observation System Simulation Experiment(s). A minimum of three Chemistry-Climate Models should be used, since current models differ significantly in relevant dynamics. This modeling effort would be order \$1M for personnel and computing.

Uncertainty in lower stratospheric mixing with the upper troposphere and in the stratospheric overworld circulation, thus uncertainty in stratospheric lifetimes of injected particles. This problem can be solved with additional in situ measurements and continued limb satellite coverage measuring nitrous oxide and methane. The satellite measurements are insufficient to determine dynamical variability because changes in concentrations could also be due to changes in the chemistry. To establish whether changes in chemistry matter, or whether these are purely dynamical variables, new in situ measurements from a range of latitudes (including the tropics) are necessary. The campaign would be around \$35M, including instrument costs, personnel, and 7-10 launches of the high-altitude balloons with CSBF.

Plastic sheeting could be put in forests to test effects of diffuse radiation.

More work can be done on marine ecosystems to study effects of diffuse vs direct radiation.

Experiments can seed cirrus clouds to assess whether cloud seeding would really work at thinning or dissipating cirrus clouds. These could be both physical experiments and model simulations.

Experiments could be done in the field to test effects of aerosol plumes on the remote marine atmosphere. Is there a substantial albedo increase including the areas that surround the plume? These could be both physical experiments and model simulations.

Experiments could be done to look at particle dispersal in the stratosphere. Is there a good way to avoid clumping of particles? This could be both laboratory and in situ.

Climate dynamics response to various patterns of forcing. Model intercomparisons to test the robustness of responses versus natural variability. \$100M needed to make significant advances in a few good models.

Uncertainty in cloud response to aerosol injections. Field trials seem necessary because this field of science seems to be stuck without progress. \$100M needed for field trials, multi-scale model intercomparisons, satellite analysis etc

Effect of novel particles injected into the stratosphere on cirrus nucleation. Lab experiments are needed to test INP properties and substantial advances in modelling is needed to evaluate results. Field trial may also be needed. \$10 would cover the lab experiments and \$50M to advance the modelling and analysis sufficiently.

The greatest uncertainty is public acceptance.

Research must identify an environmentally benign material appropriate for SRM.

Deployment mechanisms must be immediately reversible.

Research must first identify the material and a candidate deployment mechanism.

This must garner public acceptance. only then can we proceed with development, testing manufacture and deployment

SAI - Uncertain ice sheet and sea level responses to stratospheric aerosol injection or other solar radiation management techniques. Development of ice sheet models and multi-model large ensemble simulations using chemistry enabled GCMs can be performed to quantify the uncertainty in the stratospheric response to SAI and its influence on the hydrologic cycle. Follow-on effects on ocean salinity and circulation, and melt rates of ice sheets/shelves would then be better understood. The number of climate models capable of this type of assessment is growing.

Validation of the model responses to volcanic eruptions would be important. Improved reanalyses of the Pinatubo eruption would be very important in model evaluation (previous reanalyses do not explicitly resolve them) but the science is getting to the point where explicit representation of these effects, in both models and reanalysis systems, is possible. Costs are difficult to estimate but would involve both modeling and analysis system investments and amount to several million \$.

MCB - Can additional boundary-layer CCN be generated in sufficient amounts to impact cloud microphysics? Need to develop sprayer and test dispersion in marine boundary layer to compare with large eddy simulation model results. \$3M

MCB - Can we measure the effect of additional CCN on marine cloud microphysics? Need to have a ship-based deployment of sprayer and aircraft observations of outcomes. \$10M

Cirrus thinning - Insufficient understanding of the relative importance of heterogeneous vs. homogeneous nucleation of mid-latitude cirrus. Requires enhanced modeling effort of cirrus clouds on scales from large eddy simulations to large regional models and comparison. May require some field observations (later). \$2 for detailed modeling program.

Text S4. Full text responses to Question 8.

Question 8: What finding would cause you to abandon research into solar geoengineering in general or into a specific method?

This is a very good question. Forcing scientists to state their dealbreakers up front is a very good method to anticipate potential problems in geoengineering research. In future surveys, this sort of inquiry could be improved even more by doing a "pre-mortem": where you ask the participants to imagine a future scenario in which a plan was implemented and failed spectacularly, and then you ask them to write a brief narrative describing how it failed. The question, "what would cause you to abandon research" is a much higher bar for potential problems to clear than, "what could cause geoengineering to fail".

At the most basic, any finding which demonstrated conclusively that it would not be possible to improve the human condition by a particular method of geoengineering should cause us to abandon that method. For example, if it were shown that it is impossible to use SAI to minimize changes in temperature and precipitation in such a way that humanity is better off than the no-geoengineering scenario.

In addition, any finding which demonstrated that a different method is much more likely to be effective than a particular favored method would cause me to abandon research into the ineffective method and switch my efforts to the method that was more likely to work.

Potential justifications of solar geoengineering rely on both climate change's risks and solar geoengineering's expected net impacts, both physical and social. It is possible that the former turn out to be low. Dramatic reductions of net GHG emissions could cause this in principle, but I almost exclude the possibility. Relatively more likely are significantly lower climate sensitivity and second-order climatic impacts, and humans' adaptive capacity. However, not do I consider even these to be unlikely, but because we will largely not learn about them until after-the-fact, their late revelation would probably not undermine the case for solar geoengineering research.

It is also possible that solar geoengineering's physical impacts could be unacceptable. I am a social scientist, so I here must speculate. It seems to me that a relatively understudied area is the ecosystem impacts of sunlight's availability and diffusivity under stratospheric aerosols. Thus, if I had to choose a potential physical deal breaker, this would be it.

As I noted in a previous question, reducing social uncertainties is difficult. It is therefore unlikely that social research could conclude that, for example, deployment would probably be unilateral and premature or that solar geoengineering research would greatly reduce emissions abatement. Real-world events could perhaps greatly increase my skepticism. For example, consideration of solar geoengineering governance in an international forum, such as the UN General Assembly, could reveal that nuclear states would be willing to use force over decision-making.

General - Solar geoengineering will disrupt a natural cycle (e.g. ENSO or QBO) so that it is always in one state or the other. Models do not represent all QBO and ENSO processes correctly, and so this is a very real concern. Improvements in GCMs--including in warm pool/cold tongue biases, ITCZ biases, resolution, gravity waves, tropopause processes--could help to test this in a more reliable way.

Specific - SRM with stratospheric aerosols will dramatically decrease the ozone layer. Lab studies and small field perturbation experiments could address this concern.

General - The natural variability of the system is such that an intervention would have to be in place for a decade or more before determining whether critical negative effects (like midlatitude lower stratospheric ozone depletion) were caused by the solar geoengineering. Constraining the variability in the current climate system and attributing variability to processes (MJO, QBO, ENSO, SSWs) is necessary so that these large signals can be removed from the observations during a geoengineering deployment. Because of nonlinearities, this attribution might be impossible with current observations and models.

General - Solar geoengineering will cause such a global increase in Seasonal Affective Disorder that rates of alcoholism, depression and suicide will skyrocket and outpace the negative mental health effects associated with global warming itself.

If it was clearly demonstrated that any solar geo deployment would inevitably lead to world war or other similar strife.

If we were rapidly transforming our energy system so that solar geo would not be necessary.

If it was found that, for example, while improving mean states, an increase in extreme events would nullify any benefit.

If it was found that, for example, atmospheric chemical consequences would mean that solar geo would do more harm than good.

If solar geo research became so contentious and such a distraction that it seemed likely to undermine other scientific research and/or deployment of clean energy systems.

NOTE: I do not think any of the above situations would be likely.

Local or regional changes in climate (precip, T, ice etc) were substantially outside what would occur without geoengineering. I think we would have no alternative but to allow time for much deeper understanding of climate response through modelling and analysis

1. Our climate model is not robust enough to provide sufficiently accurate predictions
2. Global geopolitics tend to a direction that underates the climate change threats and I pessimistically feel climate scientists have lost its voice in both political leadership and public support.
3. Global economic/political inequality harmed the international cooperation to fight climate change, therefore I predict human could only make limited efforts to develop some adaption technologies.
4. Geoengineering research so far has failed to trigger business revenue into this field. Without capitals and markets, too few stakeholders will participate hence it is gradually diminish from publics' agenda.

All research is best done by international groups of researchers so as to avoid creating the public perception of self-serving research activities that continue to feed a small group of elite researchers who are part of one set of elite institutions of one nation state

WE CAN NOT WALK AWAY FROM THE MESS WE HAVE CREATED. THIS RESEARCH SHOULD ONLY STOP AFTER THE LAST HUMAN HAS BREATHED THE LAST BREATH OF AIR

The mismatch in radiative forcing distributions between CO₂ and all SRM approaches is problematic, though generally I view this as a motivation FOR research rather than a rationale against it. That said, it limits the investments that I believe should be made into simple specific methods where that mismatch goes unaddressed.

General - Large regional (sub-continent scale) responses to solar climate engineering forcings are fundamentally unpredictable (not just uncertain). Address by

carrying out detailed ensemble modeling experiments with GCMs at high resolution.
General - Detection of impact of climate engineering forcings on earth radiation budget is not possible on time scales shorter than about a decade. Address by additional ensemble studies with high-resolution GCMs that include an adequate representation of climate noise and a more detailed assessment of the feasibility of engineering feedback approaches.

MCB - Impact of adding CCN to the marine boundary layer on marine cloud microphysics cannot be predicted within some reasonable level (TBD what this means) of uncertainty. Address by field experiments in conjunction with LES models - models used to make predictions and predictions verified by experiments.

MCB - Regional climate responses, particularly of the hydrological cycle, to regional forcing patterns produced by marine cloud modifications cannot be predicted within some reasonable averaging period (TBD but perhaps a decade). Address with a combination of modeling at regional and global scales as well as with field campaigns.

Cirrus thinning - Cannot reliably predict the impact of adding additional ice nuclei to cirrus cloud properties. Address with modeling across scales from LES to global models. May require observations of cirrus clouds.

The general public misunderstand/misrepresent the stratospheric aerosol geoengineering (without controlling anthropogenic CO₂ emissions). The public should understand that climate mitigation cannot be done solely by aerosol geoengineering. CO₂-control strategies are mandatory as well.

Feasibility (technology and cost) of SRM and ability and impact of termination

1) Work to mitigate climate change and adapt to it have advanced to the point that the threat of climate change has diminished, esp. relative to the uncertainty about, or risks of geoengineering.

2) Science indicates that the pace or scale of climate change are highly likely to be less dramatic than currently believed.

3) Research indicates that a particular form of geoengineering is likely to cause significant harm (including indirect consequences) to human health or ecological systems, or to significantly exacerbate regional political tensions.

Policy/ Economics - IAM-type outcome that shows SAI has a high probability (>50%) of causing social unrest/ war/ economic depression/ international institution breakdown/ complete lack of equity in final outcome even if all proposed physical benefits are valid. Perhaps a serious study that works out the fate and risks of any technology resembling solar geo could give some answer.

Also if current lack of intellectual diversity of the field persists into the future: vast majority of decision-makers and scientists represent the interest of a very small portion of global population (not that current scientists are selfish, or are only looking to benefit any particular country)"

SAI - What if the aerosol-induced heating near the tropopause stabilized the tropical atmosphere and led to huge reductions in rainfall? This scenario is likely if the aerosol size increases to 0.5 micron or more. I am also more concerned about the major disruptions to stratospheric circulations (e.g. QBO) and the consequences.

Fundamental negative impacts of solar geoengineering.

General - the temperature cannot be maintained by solar geoengineering, there are too many side effects (admittedly hard to objectively judge "too many")

"MCB - ""works"" over sufficiently limited regions or times of year so that the resulting climate response is sufficiently spatially heterogeneous, so that there are guaranteed to be substantial numbers of people substantially ""worse off"" no matter how it is deployed. Probably hard to learn that without both experiments to understand the cloud conditions under which it works and climate modeling.

SAI - a nonlinear stratospheric response to aerosol heating results in greatly increased aerosol lifetime making it impossible to turn off the cooling once some threshold is passed, but we don't know the threshold. Ok, that's a bit too far out there (but hey, it was a prof at Harvard who suggested it once!)

SAI - aerosol microphysical growth is vastly underestimated when injecting into a stratosphere that already has aerosols vs the volcanic case... resulting in a maximum practical cooling capability of only a half a degree or so. Maybe we can see how far we can change parameters in a sectional aerosol model before they simply are no longer reconcilable with the volcanic case?"

if clouds were found by field and modeling studies to be much less susceptible to aerosol modification that would serve as a reason to abandon that research track

"I would abandon research into solar geoengineering if it is found that civil society broadly rejects solar geoengineering as an acceptable climate response.

Whether or not this concern is seen as ""valid"" on the part of the solar geoengineering research community may not be relevant. "

Very low feasibility as currently assessed for space mirrors

NA

"Extreme and non-predictable remote climate anomalies (far away from the place of solar geoengineering deployment--however, I do not believe that this could be detected by ESMs"

"Destruction of ozone layer by SAI--lab and field experiments could provide insights in how far this concern is valid".

"Stratospheric injection approaches -- delay recovery of the ozone hole by over 10 years
Potential for weaponization"

"It will depend on how long it might be before actual implementation might start. There are some strategies, like marine cloud brightening, that could be useful in early intervention scenarios in conjunction with strong mitigation and working to get the global warming to below 0.5 C, but would not be adequate if deployment is held off until warming is over 2.5-3 C and the desire is to get back to 1 C warming or below. For MCB, aside from the question of whether it can actually have a net negative effect on forcing, the key issue is the potential for undesired and unpredictable regional responses. At low levels, the meteorological response is likely within the noise of interannual climate and weather variability--if deployed extensively and widely, the regional impacts on precipitation patterns may be larger than desired if used persistently (one could perhaps deploy it with different patterns and/or intensities in different years, perhaps depending on ocean SST patterns, etc., so a lot to look at. On SRM, I'm not sure there would be a situation where GHG warming with SRM could ever be worse than GHG warming without SRM. The real issue is when one gets started and how far back one wants to try to go. Waiting until there is 4 C warming (with huge sea level rise projected) before starting SRM might not make all that much sense--we need to start early and keep the change from being much at all."

More Model-based sensitivity analyses are required, for example, grand ensembles, high-resolution model simulations. It would help the detection and attribution solar geoengineering. Other important areas are the proper understanding of the effect on the hydrological cycle and its implications of the ecosystem, side of solar geoengineering and its termination effect.

General - We found out climate change is not really that bad.
Specific methods - Not cost effective or scalable to at least 1W/m²

When geoengineering heads to a direction of military weapon, we should stop. If CDR develops and works very well, then there is no need of solar radiation management.

"This is outside of my expertise. I imagine they would have to do with (a) robust evidence that solar geo would shut down key circulations, or (b) impacts on productivity of ecosystems. The best showstopper would be a breakthrough in energy that suggests reasonable mitigation in short order obviates the need for solar geoengineering research."

"The climate response to solar forcing is fundamentally mischaracterized by current climate models.
Alternative types of climate models that are consistent with the unexpected observations

should be developed, and no further research undertaken until that has been achieved, and then tested by new observations of the same character as those that identified the problem in the first place."

SRM would dÃ©stabilise climate system and cross some significant tipping points.

It is hard to imagine abandoning research, as new questions always come up. But for me scenarios include, more interesting and important research to do, or my retirement.

If different approaches/models all suggest that solar geoengineering only yields a negligible cooling

Any fundamental flaw in physics that is assumed to apply being found to be plain wrong in sign of climate response.

I donâ€™t think that there are any that I foresee. Climate models continue to improve and integrate our physical understanding of weather and climate. I guess that the only aspect would be if the models prove so inaccurate that they are shown to be unrepresentative of climate change. For example several years of continued hiatus.

"Several projects using several tools end up finding the maximum theoretical radiative forcing that one could get from a certain (undefined) geoengineering method to be less than 1 W/m² in global average.

New research from a combination of LES modelling and field studies show GCM representation of boundary layer clouds is flawed. GCMs are simulating approximately right clouds for wrong reasons. => Drop GCM idealized experiments of MCB."

Need more cloud resolving climate models.

Any and all proposed methods should be evaluated show extensive climate modeling and impact assessment.

"None! A finding could lead to abandoning a given strategy but finding out what doesn't work is as important as finding out what does.

Heating of the stratosphere (and then the cascade of tropospheric impacts) is a potential show-stopper for SRM, hence research on chemicals that heat less (or not at all) needs to be continued. However, there will be no perfect method to geoengineering the planet - it will just be a question of what impacts we can and which we cannot live with. "

"The uncertainty in cloud feedbacks cannot be characterized effectively.

Increasing The projected abrupt temperature increase upon unintended lapse of climate intervention is more intense than currently understood.

Carbon capture technology or carbon emission reduction proceeds much faster than currently projected."

SAI - a finding that it causes a significant change to the atmospheric or ocean circulation.

"SAI - long-term impact of reduced sunlight on oceanic biogeochemistry - e.g. less algae and consequent alterations to DMS emissions - oceans are not well constrained in climate models so an improved marine observation network will be important in the future

SAI - impacts of aerosol layer on near-surface chemistry (e.g. by photolysis) and concomitant air pollution concerns me. How well constrained is this in climate models?"

"Fundamentally - the (highly unlikely) finding that there is a very inexpensive and accessible renewable energy source that will lead to a radical reduction of CO2 emissions due to economic reasons in the near future, and/or the (even more unlikely) finding that I'm completely wrong about the infrastructure needs and costs of CO2 removal, and that some CDR technique would be ""cheap, easy and safe"".

General - The climate response to solar forcing is fundamentally mischaracterized by current climate models. Alternative types of climate models (cloud-resolving, etc.) should be employed to validate the findings of conventional GCMs. [copied...sorry, but you've already put it perfectly and I agree and can't put that better]

Specific - for any technique, clear evidence (probably based on field experiments) that there would be very detrimental, environmental side effects, particularly on ecosystems (e.g., something really unanticipated like enhanced insect extinctions due to shifts in the color spectrum due to specific types of aerosol injections), which are comparable to or outweigh the effects that will anyway occur due to global warming (without climate geoengineering).

Sociopolitical - the (also very unlikely) emergence of such a heated discussion that significant violent reactions (societal or military) to the steps towards implementation of any technique (along with good social science survey and interview-based research to determine that there is a causal link that would cease if research were stopped."

"Since my research on cirrus cloud thinning (CCT) is fundamental to climate science, discovering that CCT is not viable is not likely to terminate my research on cirrus clouds. But in regards to CCT research: GCMs indicate cirrus cloud seeding will unavoidably increase cirrus cloud coverage, resulting in a warmer climate. A limited area field and satellite remote sensing campaign might be needed to verify this result."

"General - the spatial and temporal inhomogeneity of radiative forcing produced by solar geoengineering is amplified by the climate system, leading to greater seasonal and interannual variability, and extreme weather.

SAI - on small scales, the inhomogeneous nature of stratosphere-troposphere exchange causes small but significant local events from concentrated transport of perturbed stratospheric air into the troposphere."

"We find that DAC is suddenly very cheap and we can just suck all of the excess CO₂ out of the atmosphere quite quickly.

We find that SRM causes some catastrophic effect, like global infertility, widespread genetic mutations, etc.

It is discovered that the CMIP and GeoMIP people forgot to convert from English to Metric, and all we thought we knew about the climate is wrong. It turned out that climate change was fake news.

If it appears that SRM has become managed and controlled by an international corporate consortium of Amazon, Facebook, and Google, and they will become our new corporate overlords, if they aren't already."

"General - Convincing evidence that a method can't be effective. I can imagine that e.g. for the suggestion of cirrus thinning.

General - Convincing evidence that a method can't become cost-efficient.

General - Convincing evidence of massive side effects (e.g. massive ozone depletion through SAI, which I currently wouldn't expect).

General - Convincing evidence from climate modelling that climate effects are of similar magnitude as the changes that solar geoengineering intends to compensate. "

Based on volcano data, stratospheric aerosol injection has a large probability of causing unacceptable droughts in particular areas. If thorough model analysis provides confirming data, I will be convinced that we should abandon it as a potential solution.

General - reduction in solar insolation and increased cloudiness causes widespread and insurmountable human health problems (e.g., mental health issues; vitamin D deficiency). This could be examined with reanalysis of existing and further investigation into mental and physical health data from high latitude communities.

General - Some side effects of one or more solar geoengineering schemes are expected to impact a sizable portion of the global population.

Your example is a very good one:

"General - The climate response to solar forcing is fundamentally mischaracterized by current climate models. Alternative types of climate models (cloud-resolving, etc.) should be employed to validate the findings of conventional GCMs"

I think it is very important to look closer on cloud-resolving models!

General solar geoengineering impact (marine cloud brightening, cirrus cloud seeding, stratospheric sulfate geoengineering) on clouds is way bigger than originally thought. Laboratory studies and field observations should first investigate the cloud-aerosol effect.

SAI: Unexpected and/or second order effects of stratospheric particulates on ozone chemistry, resulting in changes in either formation or depletion on scales larger than perturbation by CFCs

SAI: Chaotic effects on jet stream dynamics beyond any plausible control
SAI: Generalised finding that descending particles have countervailing warming effect on high level tropospheric clouds
SAI: demonstration of trivially easy countergeoengineering with no adverse effects
Cirrus thinning: to be abandoned if it doesn't work...
MCB: effects on cloud lifetime cancel all effects on cloud albedo

If implementing a certain method of geoengineering results into significant reduction in agricultural yield and severe health effects, I would definitely consider stopping research on that particular method. I am particularly concerned about the stratospheric sulfate geoengineering effects on health.

If a major shift in rain distribution, floods, droughts, or violent storms were found, that should be a show stopper.
Negative effects on the ozone layer should be a show stopper.
Increased ice melt in the polar regions should be a show stopper.
Increased cancer risk from some of the nanoparticle sized aerosol materials being considered should be a show stopper.

"For any method, if models and experiments indicate catastrophic outcomes for natural ecosystems or human habitability, efforts should be abandoned. If the costs for mitigation begin to approach alternative energy or CO₂ removal strategies, geoengineering should not be considered.
Space-based - the expense and potential for various failure modes makes this option a poor candidate."

Text S5. Full text responses to Question 9.

Question 9: What common misconceptions about solar geoengineering should be understood as one puts together a research agenda?

Sorry, I don't think there are any genuine misconceptions. All questions are legitimate, otherwise we would be accused of hubris.

The greatest misconception is that a capitalist economic system will self moderate. In response researchers must call for a social intervention involving international teams of researchers from all types of research institutions.

I'm not aware of widespread misconceptions within the scientific community but in the general public's discussion of geoengineering I often find that recognition of unintended consequences goes under appreciated and the political hurdles that would be involved in implementing geoengineering is understated.

Solar geoengineering could prevent the climate warming so anthropogenic CO2 emissions need not to be controlled in industries and business. Terrestrial and marine ecosystems remain carbon sinks with aerosol geoengineering.

That the main thing is the global temperature response.

Policymakers and the public need a clearer understanding of what is known and not known now about the potential capabilities and risks of geoengineering.

"General - solar geoengineering could be easily weaponized
General - solar geoengineering does not preserve sea ice
General - solar geoengineering can only be done in a very limited number of ways (at the tropics, or a single location on earth)"

SAI - acid rain would increase

Solar geoengineering exerts perturbations to climate system. We have no clue on its real consequences and no way to revert.

"General - solar geoengineering does ""X""", without specifying what type, how much, where, when, etc.
General - assuming large termination effect built into scenarios
SAI - that it has to be sulfates"

"General - hydrological cycle changes guarantee that there will be ""winners and losers""
General - it will be impossible to govern solar geo. (To which my response is that solar geo is conceivably easier to govern than not doing solar geo.)
General - the climate system is too uncertain (to which my response would be, that not messing with the climate system isn't one of the options on the table anymore)"

Briefly:

One unhelpful misperception is that modeling research to date has largely alleviated concerns that solar geoengineering deployment might produce stark winners and losers. (e.g. Rahman et al Nature 2018 <https://www.nature.com/articles/d41586-018-03917-8>)
Another is that reduction in uncertainty about efficacy and risks of deployment will necessarily result from a scale-up of solar geoengineering research.
Another is that solar geoengineering deployment is "low cost"

Solar geoengineering is necessarily a bad solution under whichever conditions.

Conspiracy theories -- e.g., chemtrails.

"Solar geoengineering is proposed/pushed by fossil fuel rich countries to avoid deep reductions in current emission trends" => show limitations of solar engineering and that it can only be part of a mitigation strategy if complemented by emission reduction and CDR.

"Solar geoengineering leaves the carbon problem unresolved" => improved ESM modelling to quantify the impact of solar engineering on the carbon cycle response
"Solar geoengineering needs to be maintained for several thousand year"" => analyze phase-out scenarios (under the inclusion of CDR and emission reductions).

"General - Potential for weaponization

General - that only large-scale solutions capable of affecting global average temperature should be investigated"

"General: That uncertainties about climate outcomes are more uncertain with solar geoengineering than without. In that the intervention technologies have natural analogs or similar type situations that have been experienced and are modeled, the notion that there are greater uncertainties and likelihoods of surprises when we are keeping the climate similar to the present than for conditions without SRM and the climate heads to 2-3 C above present seems to me hard to justified.

General: That SRM would be able to reverse more than the temperature. Once the temperature goes up and there are consequent ecological, biodiversity, cryospheric and societal impacts that are simply not going to be reversible. It is for this reason that intervention needs to start in the near term rather than waiting for decades and some high level temperature increment.

General: That we can wait to intervene while the Arctic melts, thinking a warmer Arctic is not going to have a very serious consequence on the rest of the world, so paying no attention to rough estimates of sea level sensitivity from paleoclimatic data are something like 15-20 METERS per degree, and that decay of land ice has always occurred far more rapidly than the build up of ice on land.

General: That mitigation can be done in time to keep the warming below levels, whether 1.5 or 3 C, that would have very detrimental consequences for the environment, sea level, society, coastal communities, etc.

General: That the IPCC practice of considering all forcing equivalent, independent of their latitudinal and seasonal patterns, can all be lumped together and will give the net change in global atmospheric temperature. Were this the case, the Milankovitch orbital changes would not have caused glacial-interglacial cycling. Given it is not the case means that it might well be possible to use available technologies to affect particular regions--so, for example, to reduce Arctic warming and thus reduce the impact on NH winter weather.

General: That we have to know everything possible before we start deployment rather than viewing the deployment a period when there can be iterative learning underway. We clearly don't know everything for the GHG increase going forward, yet have, based on what information we have, agreed that the fossil fueled energy system must be given up. Model results have been used to decide to forego fossil fuels--why not apply the

same criterion for considering SRM, especially in that we'll be running models for cases and situations that are within our observational experience instead of outside the range."

Termination effect, the effect on the ecosystem, and its socioeconomic impact.

"General - solar geoengineering is purely a natural science/technology issue.

General - solar geoengineering is less risky than unabated climate change.

General - solar geoengineering is inexpensive."

General - solar geoengineering would distract people from emission reduction

"Geoengineering serves as weather modification.

Sulfate stratospheric geoengineering would cause ozone depletion even in the far future.

"

General - That the hard thing is deciding to do it - rather than everything that happens after the decision (maintaining a program and institutional knowledge / infrastructure for multiple decades).

General - That it is so incredibly different from CDR that you don't need to address them at the same time. In reality, from a programmatic perspective, you should be thinking about CDR as soon as you start thinking about solar geo, because CDR is perhaps a bigger challenge.

General - That governance happens on the level of nation-states, and that nation-states represent the people in them, and that this is a sufficient framework through which to discuss "governance issues". Rather, that notion is divorced from the experiential reality of most people. Most of the governance discussions taking place pretend it is this 1990s world of rational, globalist actors. The discussion has not updated to a context of rising authoritarianism and populism worldwide, much less non-state actors and a new media context.

"Solar geoengineering would

(a) inevitably weaken the hydrological cycle:

(b) adversely affect monsoons

(c) make things worse than climate change would

(d) be a wonderful and risk-free solution (!!)

I think all of these can be addressed by better interpretation and explanation of the results we already have from modelling studies: but we need better communication tools (and especially better ways to deal with people who are not interested in evidence if it conflicts with their preconceptions)"

SRM could work before there is a governance system in place to regulate emissions

The solar geoengineering will be a "solution" to global warming. If the research soon finds major problems with any proposed scheme, this will hasten the reduction of

greenhouse gas emissions. Rather than say, "we will be forced to do this at some point," it is crucial to say, "this is so dangerous we can never do it, so we need to agree to leave all the carbon in the ground now."

"There is a simple choice between business as usual and returning to pre-industrial conditions. - In fact a return to pre-control is entirely impossible, so the real choices available need to be explained clearly

CCR is a feasible as a way of mitigating temperature rises regardless of emissions, or even to bridge the gap between Paris and 1.5 or 2 degree limits. - The limits of carbon removal mean that it can play essentially no role in limiting temperature rises this century hence the real choice is between exceeding the targets or doing some kind of geoengineering - whether SRM, or targeted GE to reduce specific impacts."

- 1) that we will be able to hit the 1.5/2c targets using conventional mitigation/CDR.
- 2) that the side-effects of geoengineering are worse than those of global warming.
- 3) that we will be able to achieve net negative emissions

Make clear which methods are more and which less mature, for example by giving the estimated total time spent on researching them.

(some GE ideas are often presented as equivalent to SAI, despite their very dubious feasibility and limited maximal climate responses)

The world with GE will be more unequal than a (high CO₂) world without GE.

Solar radiation management is not synonymous with SAI. Research should be done with an open mind.

Uneven cooling (over-cooling the tropics and under-cooling the poles): this mainly happens due to Equatorial injections - strategically placed injections shown in recent studies can alleviate this issue.

It is a misconception that any form of stratospheric radiation management can be as low risk for potential feedbacks and unintended consequences on the atmosphere system than solar occultation or other space-based methods.

It is a misconception that the effects of stratospheric aerosol perturbations on upper tropospheric clouds can currently be predicted with any quantitative accuracy.

General - it can offset all climate change, need to make clear what it can and cannot offset.

General - that it could be a substitute for emissions reduction, and that it's only really risk reduction.

General - that some massive change would suddenly be turned on, but that it would be a gradual rate of introduction. "

"General - solar geoengineering would lead to more droughts (might be true for some regions, but will need to be investigated)
 General - solar geoengineering is a conservative technocrat's dream
 General - solar geoengineering would be cheap (possibly true but the engineering is in its infancy)
 General - the greatest barrier to deployment is uncertainty over climate risks - most GCM research to date has found that solar geoengineering would reduce climate risk. In reality, the greatest barrier is the political and social side
 General - solar geoengineering is currently prohibited under international law - obviously not true which needs to be addressed to cover large scale deployments
 SAI - would lead to significant acid deposition
 SAI - would weaken the Indian monsoon
 SAI - is currently being deployed by UK/US governments (chemtrails)
 General - that all forms of solar geoengineering are equally risky- not true, MCB is inherently riskier than SAI in terms of local thermodynamic changes"

There are very many...the one mentioned about the hydrological cycle is a common one.

Others include

- that it would not be possible to design an approach that would not have significant ""losers"" (i.e., where the climate situation gets worse than unabated climate change),
- the misconceptions about ""lock-in"" (if we start it we ""have to"" continue),
- the likelihood of a ""termination shock"" (an abrupt stop),
- the idea that work on radiative forcing geoengineering automatically leads to a ""moral hazard"" response (in some contexts it can go the other way around)
- and the wide range of misconceptions about the motivations of researchers working on the topic.

1. Solar geoengineering may produce a more hostile climate and ecology than would be produced by a climate resulting from ""business as usual"" global warming. Our best tools for addressing this concern are climate models and paleoclimate data.

For governance and social science, research is not separated sufficiently from deployment.

Climate quantities on large space and time scales (seasonal, annual, global, zonal) do not represent impacts and risks adequately.

Researchers are addressing this, but the perception still exists that geoengineering is "all or nothing." Instead -- if implemented -- it should be understood in terms of "doses" or "applications." A light touch is needed.

Not sure I understand the question well, but I don't think there are very clear common misconceptions, maybe rather mischaracterizations or oversimplifications.

General - Termination of solar geoengineering would inevitably cause catastrophic climate consequences.

Many people think, solar geoengineering aims to create a postindustrial climate again. But that must not be necessarily the goal. It could be use to cut peak warming.

"That it is possible to be engaged in general climate change discourse without understanding and, where appropriate, addressing issues surrounding solar geoengineering. The fact that no one ever pays a price for not talking about solar geoengineering biases the discussion on which decisions are made without any of those concerned having to face up to the role their silence plays in such biasing. That geoengineering should always be understood as a Plan B for use after mitigation and adaptation have in some sense failed, the "in case of emergency break glass" approach.

That geoengineering needs to be, should be or is, by those researching it, viewed as an alternative to mitigation strategies.

That geoengineering researchers are geoengineering proponents

That geoengineering researchers cannot or should not be geoengineering proponents

Also: termination shock, hydrology, yadda yadda yadda"

"I think people have misunderstood about the termination shock, i.e. sudden warming after stopping the geoengineering. We should make other to understand that sudden warming effect can be minimized by gradually stopping the geoengineering. Geoengineering would result into more climate catastrophes than doubling of CO₂ or global warming.

Geoengineering is impossible to implement due to cost, governance, and lack of understanding about its effects. I think all these issues can be resolved if continue improving current geoengineering methods, developing new geoengineering methods, and developing consensus among countries about governing issues. "

"General misconception - the climate situation is serious, and stratospheric aerosols are thought to be a quick way to bring down temperature - thus, we should use them before fully assessing all the risks, and perhaps even without obtaining permits to do the testing, because of the urgency.

To address this, the side effects and downsides of starting, or discontinuing, treatments must be considered, permits must be obtained, and innovative work must continue until solutions that are truly benign are developed."

"The most common misconception seems to be that the government is already conducting such geoengineering strategies.

There definitely is concern that precipitation regimes will be altered in an unknown way; however, there is reason for caution given the large uncertainties in precipitation modeling."

That moral hazard is inherent in geoengineering research. The actual observational support for the moral hazard hypothesis is mixed (Burns et al., 2016). The notion that geoengineering is somehow a substitute for emissions reductions constantly plagues the public discussion of this research, when the reality is that each year without meaningful global action to address climate change increases the odds that we will need BOTH emissions reductions and geoengineering.

"Misconceptions are pervasive in the solar geoengineering discourse. I could list ten to twenty of them: Solar geoengineering would compound climate change's uncertainties. It would be irreversible. It would necessarily deplete stratospheric ozone. Its researchers and advocates thereof are motivated by a desire to continue the use of fossil fuels. Etc.

The further one moves away from core natural science research, and as the speaker/writer is less familiar with the evidence, misconceptions become more pronounced. I am thus not too concerned about a public funding body misallocating significant resources due to misconceptions. A more important question is how to reduce misconceptions among natural scientists who are new to solar geoengineering, social scientists, journalist, and ""opinion makers.""

I doubt that mere ""more research"" would reduce misconceptions, as we've seen in the climate change discourse more generally. Nevertheless, more research on the impacts of optimized or moderate use of solar geoengineering could help. Allocating a portion of funding for communication and outreach, both project-specific and general, would hopefully reduce misconceptions. And psychological work into why misconceptions arise and persist may also be beneficial in this regard. "

"The stratosphere is relatively quiescent and its dynamics are well-characterized, so it is possible to effectively ""turn down the sun"" with stratospheric aerosols. The geoengineering program would need to continue forever."

"Solar geoengineering necessarily means increased drought.
That once you start solar geo, you can't stop.
That there is a substantial risk from early termination of solar geo.
That there will necessarily be winners and losers in any solar geo deployment.
That there is a substantial risk from a rogue non-state actor (i.e., the evil billionaire).
Solar geo can't prevent ice sheet melting or sea level rise.
That ozone loss makes strat aerosol geoengineering be a non-starter.
That a solar geo deployment by a state actor, absent global consensus, would be likely to generate military conflict."

Text S6. Full text responses to Question 10.

Question 10: Please identify novel challenges for solar geoengineering research in general—or for specific solar geoengineering proposals—that would need to be addressed in a research program.

General - ultimately the coordinated minimization of climate impacts in a geoengineering application relies on being able to comprehensively estimate climate changes (likely in an Earth system model), quantify the economic and related impacts of such changes, and agree upon the importance/costs of such impacts (perhaps more of a political challenge). Doing so is a tremendous modeling challenge and would require a means of adequately identifying and estimating numerous sources of uncertainty. Comprehensive uncertainty estimation through each stage of this process will be a novel (and immense) challenge.

"General - solar climate engineering represents a paradigm shift in atmospheric research since it uses direct intervention in the atmosphere. This raises concerns about the effect of that intervention, even on small scales. A research program must include research on how to communicate with the public to alleviate these concerns. General - solar climate engineering involves both science and engineering research in a unique way. The research program must be designed with to both foster open science research and to drive the program towards an engineering result. This is very difficult to do.

General - corollary to previous. The research program must have clearly articulated objectives as it moves from one phase to the next. "

What is the acceptable level of risk that un-intended consequences will be larger than the benefits?

General - how solar geoengineering deployment can be implemented under uncertainties (both scientific and political/economic)

SAI - In spite of the modeling results that solar geoengineering can be tailored to achieve different objectives, I am concerned that the climate system is too complex. It has too many degrees of freedom. The recent modeling studies basically met only 3 large scale objectives - global mean T, inter-hemispheric T gradient and equator-pole T gradient. This does not guarantee regional metrics would not be severely altered. In fact there was a recent research paper in Nature Geosciences that showed that the sinking in North Atlantic increased under SAI. More research is needed to assess how geoengineering would alter a large number regional indices (e.g. monsoon circulation in different part of the planet)

Regionally targeted methods, no global scale uncontrollable consequences.

General - how do we communicate solar geoengineering research to fellow climate scientists and the general public? how much do we rely on it in scenarios work?

How about, researchers with specific agendas are borderline dishonest in their press releases, resulting in a vast cloud of deliberate misinformation?

Solar geoengineering may be applied at the regional scale.

"Ethical - few individuals have the power to change the course of history potentially positively affecting some areas and negatively affecting others.
No-analogue conditions - unclear what knowledge we have to understand how ecological systems will respond."

"Solar geoengineering will be tailored to satisfy regional needs and for portfolios of technologies need to be investigated".

"Define criteria when it would be too early/risky to deploy solar geoengineering--I can imagine a situation of public/societal pressure to use these technologies after a series of heat waves/storms etc even though these might not be necessarily caused by climate change"

"Investigate the prospects of regional solar geoengineering--requiring to understand the remote climate response".

"General - to what extent can climate interventions be tailored to address specific feedbacks and tipping elements in the climate system?

General - are there positive economic and industry implications for specific approaches? (For example, using solar sails for shading that also generate electricity)"

"General: On a wide range of issues, society has had trouble acting until the crisis is quite severe. If that is done with GHG warming, not nearly all the consequences can be reversed, even if the climate can be taken back to its previous state. So, if we really want to protect the world, there is a need for deployment well before the consequences and impacts are affected. So, there will be a need for society to be supportive of taking an action going forward based only on model simulations. That is being done, reluctantly, with respect to moving away from fossil fuels--can it happen with climate intervention?
General: There is the notion that we will have to have near perfect information before acting. This is not the case in any other major decision field as uncertainties are present all the time. The scientific tradition of requiring two-sigma significance on findings is likely to hold off scientific consensus about the consequences of deployment until well after the optimal time for its application, negating the potential for applying what may be the only way to avoid an existential crisis for humanity.

General: If deployment does go forward, basically every adverse weather event is going to be said to be due to the intervention or that the intervention was not properly done, and so the intervenors are to blame. Getting around the climax of potential responsibility and liability would seem likely to be quite a challenge to address.

General: The time constants involved cover a very wide range of time, making the

problem and potentially reversing the impacts quite problematic. So, while simulations show that the atmospheric climate is largely reversible, reversing the melting of land ice is likely to involve degrees of response that might well lead to climatic adjustments that are larger than desired.

General: On how much offsetting to apply, different aspects of society and the environment are adjusted to different conditions, so cities were built based on something close to preindustrial sea level, while agriculture has likely largely adjusted to the climate of the last few decades and going back to the 19th century climate would likely cause greater impacts than staying where we are. So, choosing the level to return to and the timing of doing so will have no single right answer, requiring that lots of tradeoffs be made. How this is to be done globally is a real challenge, which may be why it might work best to start with a regional focus of revising a key impact might pose a more workable governance challenge than trying to agree on a global offset.

General: Climate change is not the only issue affecting humankind, and there are large numbers of people in all sorts of climate situations. How to work out the necessary inter-nation tradeoffs is likely to prove quite problematic."

The main challenge would be the testing of solar geoengineering technology.

"General - Climate impacts analyses can not be easily applied to climate+solar geoengineering analysis because of natural systems and social systems react different. General - How do different solar geoengineering techniques and deployment scenarios interact with each other? e.g. is a portfolio of solar geo technologies more or less risky than a single technique intervention?"

General - tailoring SG to objectives that are both climate and policy related. How many degrees of freedom do we have, what is the maximum?

"Identify climate disasters due to solar geoengineering and natural weather variation.

How to reach one objective fulfill everyone's interest? "

I really think the novelty here is the temporal duration of a solar geoengineering intervention. It is not easy to address in research, because most methods of research deal with things that have already happened or are happening, not things that will happen in the future.

How to research deployment without creating a lobby for deployment

Can the world ever agree on a scheme to control our climate? Who will be in charge? How do we decide what climate we want? How do we stop the technology from being used for nefarious purposes?

"Targeted geoengineering - how to tailor it meet specific objectives, e.g. seasonal warming of permafrost/sea ice/ice sheets.

Cocktail geoengineering - possible interactions between different methods - remote impacts of targeted schemes, ecological impacts of those interventions."

"Scrambling facilities and effort (eg monitoring aircraft, balloon borne instruments, focussing satellite efforts) etc after a stratospheric or tropospheric injection of so₂ is a challenge because of the scheduling logistics.

Removal of the myths surrounding the relative impacts of srm versus global warming. That's not to say that a cautious approach to physical research to point out the potential side effects is not warranted as this research points out what should not be done (ie unilateral geoengineering deployment)."

All ideas need to be thoroughly vetted through extensive modeling and detailed high resolution spatial and temporal analytics.

One of the obvious challenges is what is the desired outcome of geoengineering beyond global mean cooling. Once that is set, the novel aspect of this research is arriving at a solution - which is as much of an engineering problem as a science problem, and approaching it with an engineering mindset would be of benefit: how can all the available geoengineering approaches be best combined to produce a strategy that brings the climate closest to present day? How do we effectively span the parameter space and optimize of all the combinations of stratospheric injections, cloud brightening, cirrus thinning , etc to come up with a best solution to the problem at hand? How does that strategy need to be adjusted at different times into the future?

Stratospheric solar management strategies rely critically on basic aerosol and cloud microphysical processes that are not well quantified. A comprehensive approach involves intensive in situ observations, intensive and sustained remote sensing observations, development of new space-based instrumentation, and process and large scale modeling of natural phenomena, including volcanic eruptions.

SAI probably needs to be structured to have as uniform effects on especially precipitation changes across the planet as possible, but perhaps also other metrics. The more there are winners and losers the less politically feasible it is as a risk reduction option, even if the scenario with winners and losers was better on a global basis. Some kind of effort towards uniformity of effect and perhaps risk could be an objective. Or some kind of feedback mechanisms that could ensure that.

"SAI - alternative aerosols are preferable to sulphate. I'm unconvinced by calcium carbonate - especially the impacts on polar stratospheric clouds which may enhance ozone depletion. Additionally, coagulation rates for candidate aerosols are poorly constrained. Small-scale field and lab experiments should investigate alternative aerosols (coagulation, chemistry and optical properties after becoming coated with stratospheric

sulphate)

SAI - finding an injection mechanism that optimizes efficacy - requires engineering and field tests

General - public perception, governmental awareness, media interpretation - how to remove the universal lay stigma that solar geo is inherently bad (promulgated by the left and right in the UK). How to disseminate results without personal agenda or bias? "

"A major quasi-novel challenge is that going from the model, lab and experimentation phase to actually evaluating the climate impacts skips over the ""test"" phase - as indicated earlier in this survey, that would already be considered a form of small-scale deployment. Knowing we would not be able to test without deploying poses a significant challenge in public acceptance.

I have named this one since it is thought of less often and might not be listed by other respondents. But this is of course shared with a few other novel technologies, and most other challenges are also not entirely novel, e.g., broadly expressed ethical concerns also apply to stem cell research, genetic engineering and cloning."

"The viability of CCT may rest on a deployment strategy that results in a fairly uniform distribution of ice nuclei (IN) at cirrus cloud levels under clear-sky conditions. A field campaign could be designed to test proposed seeding strategies. Another challenge is whether CCT can reduce cirrus cloud coverage on a broad scale (i.e. not resulting in more cirrus coverage due to increased IN concentrations). This could be tested through (1) the development of a geostationary satellite remote sensing method for cirrus cloud properties, and (2) application of the method to observe the evolution of cirrus cloud properties over the Rocky Mountains during the onset of an Asian dust plume. A reduction in cirrus cloud coverage coupled with lower concentrations and larger sizes of ice crystals would indicate CCT viability."

Climate modeling scenarios have policy implications and complicate the standard incremental approach of looking at a physically simplified scenario. This paper is worth a look Talberg, Anita, et al. "How geoengineering scenarios frame assumptions and create expectations." Sustainability Science (2018): 1-12.

As mentioned previously, how to manage a multi-decadal and global project on such a scale. This would require cooperation by the United States and China over a long period, which seems very unlikely. No single country is powerful enough to force this upon the globe.

General - I think the major novel challenge is of political/societal nature. Climate change effects are unequally distributed globally. Hence, climate targets that e.g. different states may have likely differ strongly with consequences for any global attempt to engineer the climate, which may turn much more into a design question than into an attempt to reduce climate change.

Otherwise, I think the main open issues I'd like to see researched are not necessarily

specific to SAI but very important in this context. I've mentioned them earlier:

- stratospheric transport
- aerosol microphysics
- tropical hydroclimate
- cloud microphysics

General - Solar geoengineering can be deployed in different regions of the world. Thus, optimizing regional climate could make the climate in other regions much worse.

I think once research goes on and effects and feasibility of solar geoengineering methods become more clear, there might be different parties with different goals (e.g. cooling goals) which they want to achieve. These goals might contradict each other which could lead to conflicts.

I'm sorry, I'm dry

The biggest challenge is developing a cost effective, and environment friendly method for geoengineering. We should focus and invest on developing novel methods. Another challenge is complete understanding its effects on climate, health, agriculture, ecosystems etc.

General - avoidance of the harms, such as sea level rise, drought, and temperature rise worldwide, that can be prevented by restoring surface albedo in the arctic by restoring ice, are disconnected from the costs of doing so. Financial mechanisms for connecting positive outcome benefits financially to the cost of r&d and implementation of solutions need to be developed.

"Human social and ecological feedbacks must be better understood, so they represent little-discussed challenges.

Methods for delivery, monitoring climate responses, and accounting for complexity in the Earth system all represent current research challenges."

I think that your example is very good. There needs to be more research into ways that geoengineering could be targeted at specific goals. As I said in a previous text box, no one actually experiences the global average temperature. It is the heat waves, sea level rise, storms, droughts, etc that actually damage human lives and human societies.

"This might be my view as a social scientist, yet I see most challenges to solar geoengineering research as regarding perception and understanding. As such, public opinion studies (broadly defined) and communication & outreach are critical. Related to this, there is a risk that each outdoor project, especially those that somehow increase in scale, will instigate a political debate that is a proxy debate for the entire solar geoengineering research and development endeavor. Because of this, public opinion studies and communication & outreach should include program-wide activities, not take place simply on a project-level basis.

In terms of natural science, the greatest concerns seem to concern spatially and, to a lesser degree, temporally heterogeneous impacts. Research on solar geoengineering optimization through, among other means, more degrees of freedom, could indicate how to reduce heterogeneity. "

"The scope of earth systems that could change from a long-term continuous solar geoengineering project is beyond that from just a volcano. Once the variables that are deemed ""important"" to control are determined, then there is the challenge of designing a global observational system that quantifies these with enough precision (and to a lesser extent, accuracy) that a control approach is possible. That is truly a novel challenge.

The moral philosophy questions raised by solar geoengineering design are extraordinarily difficult, since regional effects can be tuned and there will always be tradeoffs. Any comprehensive research program about solar geoengineering has to address the moral quandaries and should include local and indigenous participation from the beginning."

It seems the main novel challenge is the threat to the ability to do relatively benign outdoor research posed by small yet vocal organizations.

Detectability against natural variability. The length of the experiments required. Detection of unintended consequences.

"The most novel challenge is for the community of elites who tend to dominate the research budgets needs to be persuaded that they can not solve this problem among themselves.

Emphasis must be placed on

1. International research activities involving rich and poor nations, and
2. Domestic research activities that involve the full spectrum of scientific talent from all capable institutions including universities and community colleges"

Text S7. Full text responses to Question 12.

Question 12: Do you have any final comments? What else should we have asked? What should be raised in our discussion on December 11th?

I think that a ""pre-mortem"" question would be very helpful, as I stated in a previous text box (a pre-mortem is a question where respondents are asked to visualize a hypothetical future in which the proposed project has been a spectacular failure, and then write a narrative describing how that happened). I also think that the reverse- in which respondents are asked to write a short narrative describing the events that made a successful geoengineering intervention possible- would also be a very helpful question. I also think that, in general, we should broaden our focus beyond purely solar

geoengineering proposals. I think that interventions targeted at specific climate goals might be better from a harm minimization standpoint than an intervention that tries to balance the entire planet at once. My own personal research has been on targeted interventions in ice sheets. We need more research into the ways that solar geoengineering will affect the ice sheets; however, it is also the case that solar geoengineering may not be a particularly effective means to prevent ice sheet collapse, particularly if dynamic feedbacks such as the Marine Ice Sheet Instability are activated. Other localized/targeted interventions might be more effective for this goal. And more generally, other forms of interventions (as in, other than solar geoengineering) might be more effective at addressing other specific targeted goals. The argument that, ""humanity needs to take over management of the entire climate system"" may be harder to justify than, ""we can reduce tangible harms to society from this one specific risk"".

I skipped a few questions that I felt unqualified to answer.

Question 1 has an anchor of 1/3 in the question, and therefore the results are not valid. I have no idea what I would have answered if that number hadn't been listed in the question. Because that seemed higher than my initial reaction, I adjusted my number even lower, to try to account for the anchoring.

The main contribution we can make is anything that would help others to develop research priorities and/or budget allocations.

Also advice on how to structure research (e.g., which agencies should do what) could also be helpful.

Examples of specific possible research projects are also likely to prove useful.

Sorry for not thinking through the concrete research projects more .

Thank you for your interest in my opinion.

Is geo-engineering distracting from making the energy choices that the world needs to make?

"To state the obvious, a 10-year program would not involve doing the same kinds of things (or spending the same amounts) each year over a decade. How would the size and contours of a program usefully be expected to shift over 10 years? What would determine that? Is it useful and possible to lay out some if/then scenarios?

What kinds of research need to go on in the U.S., regardless of what is being conducted elsewhere -- either for political reasons or regional variations that matter for the science and impacts? In the other direction, what kind of research would the U.S. need to stop or limit even if it were going on elsewhere?"

"I have a very broad concern:

Whatever SRM method is considered, the scale of intervention would be most likely

global as the climate change problem is a global problem. Already humanity is grappling with a global change problem and the concomitant global scale degradation and damage to our planet. The quality of life is already worse in many poor countries and is getting worse in many countries because of ""development"".

Should we add another global scale disruption? I understand that the intention of SRM is good but a very careful and thorough investigation of environmental degradation and damages that are associated with the implementation and the after effects is badly needed. I am afraid that the magnitude of the environmental degradation & damages associated with either SRM or CDR could be as big as the damages that has been inflicted already on the planet by our current fossil fuel driven energy system."

"There are a lot of good questions in this survey, however, it is not possible for me to answer those in 30 minutes. It will be very useful to produce some answers of the questions in an assessment. I don't think, it make sense to estimate how much funding should go where at this point (therefore I have not answers those questions). The discussion on Dec 11th could focus on identifying pressing questions that should be answered by the planned assessment."

"Thank you for the opportunity in participating in this questionnaire which help me a lot in thinking about solar geoengineering and the associated research challenges. Regarding the last ""optional"" question--I missed there the opportunity to identify the contribution of CDR. The difference between my desired level of RF and the expected is only partially filled by SRM because I expect/hope that the missing part if filled by stronger CDR deployment than in the expected case. My expectation for RF in 2075 are rather low as I expect a strong technological shift (not just because of climate policy but just because of economic reasons, i.e. there will be no new coal power plants being build from 2030 onwards). Accordingly, right now I consider for the Solar Geoengineering debate the peak shaving argument more relevant compared to the argument that we might need Solar Geoengineering towards the end of the century to mitigate extreme climate change. "

"Iterative learning through early deployment. We really do not have time for 20 years of research to reduce uncertainties somewhat with only theoretical and laboratory levels of investigation. We need to realize we will need to be trying and adjusting, working with models, and working in a risk framework for decision-making. A comparative risk analysis needs to be conducted, and the public needs to be convinced of its validity.

Getting scientists to agree to using a relative risk evaluation process (so GHG increase with and without SRM) rather than treating SRM on its own and out of the context of ongoing GHG forcing, as has been the case in many analyses, both of scientific aspects and of governance and societal consideration."

"Like I have responded in the previous questions, conducting this exercise just for social sciences is better than trying to accommodate very different research programs

and research needs under the same program.

It is not clear what a solar geoengineering proposal is and thus it is hard to answer those questions. The word "proposal" is super vague and can be interpreted in many different ways.

In the question regarding stopping rules, doesn't it depend on how much research has been done? How much do we know about other aspects before we learn about one single, drastically negative outcome?

I responded to the questions, but as it is always the case, there was a lot of assumptions going on behind. I could also detect a bias towards natural sciences. Which makes sense, but was not explicitly stated at the beginning of the expert elicitation. "

If we deployed solar geoengineering, when should we stop it? How to decide?

"I think it could have been better to crowdsource the research priorities and budget items. Doing it with the categories pre-defined probably induces the respondent to allocate funds to things they wouldn't have otherwise thought were important, just because they were on the list. Though I guess if it's not for a peer-reviewed paper, the reliability doesn't matter as much.

I really think there should be an "if-then" programmatic flowchart that specifies what will be done in the event a showstopper is indicated."

"Allocating percentages and providing numerical estimates (etc) is hard work and time-consuming. It'd have been much better just to ask people to assess options as High/Medium/Low/Zero fractions of whatever is being allocated...

I left some of the free-form pages blank 'cos it'd take too much time to complete them, and life is short... "

We need a lot more social science and political science involved. We also need biologists and ecologists and agronomists. The physical science side is well represented already, but how will the decision to implement ever be made?

The questions on US funding seemed very isolationist. It would have been appropriate to ask for example how much funding from the US should be dedicated to including other nations. Global warming is GLOBAL and so are the impacts of srm. GCMs all differ and give different answers, but the ensemble had been shown to be a better representation of reality. Funding just USA institutions is isolationist.

I think one of the keys to this research field being successful is not only excellence in the science done but also coordination and everyone involved moving towards a common goal. Individual proposals are 'fine', but only if they fit into an overall coordinated strategy. There needs to be a continuous collaboration between those studying impacts and those designing the details of individual geoengineering strategies - and research on individual strategies can not occur in isolation from others, as incomplete side effects and consequences will be arrived at. Going back to what I said on

a previous question, this is very much an engineering/optimization problem which requires teams of people to come together and work towards a common goal (which is not that easy to define). The design of the geoengineering strategy dictates the side-effects, hence for geoengineering to be implemented one day, an iterative, coordinated approach is very much needed. Perhaps this seems obvious to do, but I don't think is trivial to achieve.

I would consider many of the questions asked in here as being research questions. I could speculate, but I chose not to because I don't find much value in guessing.

The field has a bit of an image problem within the scientific and climate community, and this needs to be addressed. If anything I think this may have gone backwards in the last few years.

Thanks and good luck pulling it all together!

In regards to CCT (again), two GCM studies argue that CCT is not viable since cirrus clouds at all latitudes and seasons form through heterogeneous ice nucleation (i.e. het) or through advection from deep convection (based on the ice nucleation processes employed and various other processes). Trude Storelvmo and colleagues have shown in GCM (CESM) experiments that, if cirrus clouds often form through homogeneous ice nucleation (i.e. hom) at high latitudes (and at mid-latitudes during months when the sun is relatively low in the sky), then the net cooling produced by seeding under these conditions should be comparable to the cooling produced by seeding everywhere globally. A satellite remote sensing study by Mitchell et al. should be published any day now (final ACP page-proofs were sent to Peter Irvine and David Keith) that shows arguably strong evidence that high latitude cirrus clouds are often formed through hom, and that they often form through hom during winter in the mid-latitudes. This is essentially what Trude assumed, and her work shows a global mean net cloud forcing of $\sim -2 \text{ W/m}^2$.

This survey was very thorough and well done; it took me much more than 30 minutes to complete. My mind is empty - no further comments.

It seems that the vast majority of researcher studying geoengineering do not ever want to geoengineer, and in fact find the idea a bit abhorrent. However geoengineering researchers also seem to be coldly rational, realizing that if or when we fail to mitigate, it will have been good to have researched geoengineering for the time when it appears to be the only way to prevent dangerous climate change. It would be interesting to think of other examples in history when people thought very hard about an idea (most) everyone was opposed to. Nuclear planners during the Cold War come to mind, but there have to be other examples that fit more closely. How do we as researchers simultaneously promote our research without implicitly encouraging the subject of our research?

"The survey was good. We might further discuss monitoring strategies and remote sensing needs. Integrating these needs into existing/planned missions may be a good approach.

It is interesting that the media are reporting, with some alarm, that Harvard will begin deploying geoengineering strategies soon."