Expert judgements judgements on solar geoengineering research priorities and challenges

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Abstract

Solar geoengineering describes a set of proposals to deliberately alter the earth's radiative balance to reduce climate risks. We elicit judgements on natural science research priorities for solar geoengineering through a survey and in-person discussion with 72 subject matter experts, including two thirds of all scientists with ≥10 publications on the topic. Experts prioritized Earth system response (33%) and impacts on society and ecosystems (27%) over the human and social dimensions (17%) and developing or improving solar geoengineering methods (15%), with most allocating no effort to weather control or counter-geoengineering. While almost all funding to date has focused on geophysical modeling and social sciences, our experts recommended substantial funding for observations (26%), perturbative field experiments (16%), laboratory research (11%) and engineering for deployment (11%). Of the specific proposals, stratospheric aerosols received the highest average priority (34%) then marine cloud brightening (17%) and cirrus cloud thinning (10%). The views of experts with ≥10 publications were generally consistent with experts with <10 publications, though when asked to choose the radiative forcing for their ideal climate scenario only 40% included solar geoengineering compared to 70% of experts with <10 publications. This suggests that those who have done more solar geoengineering research are less supportive of its use in climate policy. We summarize specific research recommendations and challenges that our experts identified, the most salient of which were fundamental uncertainties around key climate processes, novel challenges related to solar geoengineering as a design problem, and the challenges of public and policymaker engagement.

1 Introduction

Solar geoengineering (also known as climate intervention, solar radiation management, or albedo modification, among other terms) describes a set of proposals to reflect light or increase the amount of outgoing thermal radiation, with the goal of reducing some of the impacts of climate change. The idea that the climate could be artificially cooled emerged in the 1960's at the same time as the potential risks of climate change were being taken seriously for the first time (PSAC, 1965). In the following decades, the topic was included in all of the major climate assessments (e.g., NAS, 1977; 1983; 1992), and occasional articles explored the technical possibilities (Latham, 1990; Keith & Dowlatabadi, 1992; Teller et al., 1997). However, as climate change grew more visible and the transition away from fossil fuels became the focus for policy action, there was a relative decline in research and debate around solar geoengineering. In 2006, Paul Crutzen argued for the end of this de facto taboo on solar geoengineering research, noting that solar geoengineering may be necessary given the limited progress on emissions cuts and the limited prospects of a sufficient reduction going forward (Crutzen, 2006).

Crutzen's worries about emissions cuts were well-founded; emissions have risen over 20% since 2006 (Quéré et al., 2018), and his call for research seems to have been heeded. Prior to 2006 there were only ~50 solar geoengineering publications but from 2006 to 2018 around 1200 were published (Burns et al., 2019). Over the decade ending in 2018, cumulative global government funding directly addressing solar geoengineering (including both research and policy work) amounted to approximately $30 Million, with another $20 Million from philanthropic sources (Necheles et al., 2018). There have also been several major assessments by the UK Royal Society (Shepherd et al., 2009), the US National Academy of Sciences (McNutt et al., 2015), and others (Long et al., 2011; Schäfer et al., 2015).

This research has shown that several solar geoengineering proposals have potential to reduce some risks (Boucher et al., 2013; Irvine et al., 2016) and that stratospheric aerosol geoengineering in particular seems technically feasible, with relatively low direct deployment costs (Smith & Wagner, 2018). Research has clearly demonstrated both the dangers of using SRM as a substitute for emissions cuts, and that it could substantially reduce many climate hazards compared to a case without solar geoengineering (Boucher et al., 2013; Irvine et al., 2019; Keith & Irvine, 2016). Most researchers in the field recognize that major challenges for solar geoengineering lie outside of the scientific domain and researchers in other fields have highlighted several concerns here (Anshelm & Hansson, 2014; Shepherd et al., 2009). For example, there are the governance challenges of cooperating to develop and deploy these proposals (Bodansky, 2013; Virgoe, 2009), there is a danger of this idea discouraging near-term action on emissions cuts (a concern referred to as moral hazard) (Keith, 2000; Lin, 2013), and there are ethical concerns about whether it is right to intervene in the climate in this way (Gardiner, 2011; Jamieson, 1996).

Because there is little broad support today for deployment of proposed solar geoengineering schemes, the primary question facing policymakers at this stage is whether and how to engage in a strategic research effort to assess these proposals. Such an assessment would be critical for informed decision-making on the questions of whether and how to develop and deploy solar geoengineering. Thus far, solar geoengineering research has been largely exploratory and ad hoc, with some notable exceptions such as the geoengineering model intercomparison project (GeoMIP) (Kravitz et al., 2011). An important step towards reaching a strategic research agenda is a structured approach to identifying research needs, priorities and challenges. This paper is a first attempt to identify these needs using a structured survey and discussion with many of the natural science researchers working in this field.

Most reports into solar geoengineering have outlined some specific research gaps (e.g. (Long et al., 2011; NRC, 2015; Schäfer et al., 2015; Shepherd et al., 2009)) and several have made specific recommendations regarding funding (NRC, 2015; Shepherd et al., 2009), or on the potential organization for research efforts (Long et al., 2011; USGCRP, 2016), but most of these recommendations have not been very detailed. Several peer-reviewed articles have discussed research priorities in greater depth, for example: Caldeira and Keith and (2010) discuss potentially high-value solar geoengineering research and the challenges of getting it funded in the US, Keith et al. (2014) discussed potential field experiments for solar geoengineering, MacMartin et al. (2016) reviewed research gaps and priorities for stratospheric aerosol geoengineering, and MacMartin and Kravitz (2019) outlined a mission-driven approach for stratospheric aerosol geoengineering research. Many more papers have also explored research priorities for specific areas, such as sea-level rise (Irvine et al., 2018), climate impacts (Irvine et al., 2017) and ecosystem impacts (McCormack et al., 2016). The most substantial effort to outline a research agenda for solar geoengineering at the time of writing is at the National Academy of Sciences, currently underway and scheduled to be completed in mid-2020.

There have been several studies which assess the views of solar geoengineering experts (Mercer, 2014; Merk et al., 2019; Winickoff et al., 2015), the views of climate experts on solar geoengineering (Bellamy & Healey, 2018; Dai et al., under review; Dannenberg & Zitzelsberger, 2019; Himmelsbach, 2018), and many studies of the public perceptions of solar geoengineering (Burns et al. (2016) provide a recent review of this literature). However, only a few studies have applied expert assessments to assess solar geoengineering and identify research priorities.

Bellamy et al. (2013, 2014) elicited expert and public views on solar geoengineering alongside a broader range of climate policy options, applying a multi-criteria mapping approach to open up the assessment. In contrast to previous assessment exercises which considered a smaller set of criteria, e.g. the Royal Society Report's assessment of geoengineering methods (Shepherd et al., 2009), they found that stratospheric aerosol geoengineering performed poorly compared to other options when their wide set of criteria were considered. Sugiyama et al. (2017) asked experts to submit a wide set of transdisciplinary research questions for solar geoengineering research then convened a workshop with both scientists and stakeholders to discuss and narrow down these inputs into a list of 40 prioritized research questions.

In this paper, we report the results of a survey and discussion which elicited views from solar geoengineering researchers on the needs of and challenges for a solar geoengineering research agenda. The focus was upon developing a strategic research agenda that could be implemented 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, and which would best support decision-making on solar geoengineering over this same period. Our focus was upon natural science research priorities and most of our participants were drawn from the natural sciences, though the survey and discussion do touch on other areas of research and broader concerns. Section 2 describes the views of our participants on high-level priorities for solar geoengineering research and funding. Section 3 provides an overview of the specific research recommendations and Section 4 covers the technical challenges for research that were identified by our participants. Section 5 presents results which demonstrate a notable lack of consensus amongst our experts on several key issues related to the development and potential use of solar geoengineering. In section 6, we review our findings and provide an outlook on the challenges for developing a research agenda.

2 Methods

We used a two-step process to elicit expert judgments about solar geoengineering research priorities from a group of natural science researchers working on this topic (Keeney & Winterfeldt, 1991; Morgan, 2017). The first step was a structured formal survey with quantitative questions as well as free-form text responses including a request that participants provide specific research proposals. The second step was a structured meeting at which participants first reviewed results from the survey and then discussed research priorities in plenaries and breakout groups. The rationale for the two-step process was that elicitation of individual anonymized judgments prior to group discussion is expected to reveal a broader spectrum of opinions and so reduce the group-think that tends to occur in meetings. The two-step process also allowed the collection of a large number of detailed responses and specific research ideas that could not have been developed in a group discussion at a single meeting.

We received 61 survey responses, 46 of which were complete, prior to the meeting. All survey responses and meeting notes were anonymized. We received a total of around 24,000 words of response to the free-form questions. We estimate that typical completion time for the survey was around an hour.

The meeting was held on the 11th of December in Washington D.C., during the American Geophysical Union (AGU) Fall Meeting, the largest annual gathering of geoscientists (more than 20,000). The meeting was held near to the AGU meeting site, during a dinner paid for by the organizers. 36 people attended the meeting, including organizers, participants, and observers.

The goal of the meeting was to discuss the priorities and challenges for a solar geoengineering research agenda. The meeting consisted of several short plenary sessions and two break-out sessions for smaller group discussions. The meeting opened with a presentation of initial results from the survey for the participants to reflect upon. In the break-out sessions, the participants were prompted to consider a set of explanatory questions, such as: “What factors or principles should guide decisions around research objectives?” The full list of discussion questions can be found as Supplementary Text S1. The meeting ended with all participants being asked to give one final message on the topic. Notes were taken by the organizers during the plenary sessions and by a rapporteur for each group during the break-out sessions.

Recruitment process and participants

We adopted a semi-structured approach to recruiting survey and meeting participants. All authors of AGU abstracts that mentioned solar geoengineering, whose contact details could be found, were contacted to see if they would be interested in participating. In addition, the organizers sent emails to their more senior personal contacts in the field of solar geoengineering as well as to other researchers working on climate science whose expertise would be applicable to solar geoengineering. All who were interested were invited to complete the survey, however we received more expressions of interest to attend the meeting than we could accommodate. To narrow this down, we selected the more senior researchers and attempted to bring in researchers with a balance of research expertise.

We recruited a total of 72 participants, 61 of whom participated in the survey and 36 of whom attended the meeting. While our recruitment approach was not systematic, we comprehensively sampled the most senior solar geoengineering researchers having recruited 18 of the 27 living authors with ≥10 solar geoengineering publications according to a 20th May 2020 Web of Science search (TOPIC: ("solar climate engineering" or "solar geoengineering" or "solar radiation modification" or "solar radiation management" or "stratospheric aerosol geoengineering" or "marine cloud brightening" or "marine sky brightening" or "cirrus cloud thinning" or "geomip")). Throughout the results section we report results for the whole sample, highlighting in the text any notable differences between the group with ≥10 publications and the rest.

Table 1 lists all 72 of our participants. 61 solar geoengineering natural science experts participated in the survey, 25 of whom also attended the in-person discussion. There were 36 meeting participants in total, 11 of whom did not participate in the survey most of whom were social science and policy experts with familiarity with solar geoengineering. 16 of 61 survey participants were female, and 13 of 36 meeting participants were female. 37 of our survey participants came from US institutions, 6 from the UK, 6 from Germany, 4 from China, 4 from Switzerland, and one each from Norway, India, France, and Finland. For our meeting, 33 participants were from the US and 3 were from China. While the solar geoengineering research community is majority male and predominantly based in Western countries (Buck et al., 2014), our participants are not representative of the field as they were drawn from participants at the 2018 AGU conference (hosted in Washington DC) and from the organizers' personal networks (all of whom are based in the US).

Table 1. Full list of participants, listing name, affiliation, whether they have 10 or more solar geoengineering publications, and whether they participated in the survey, discussion or both.

Name	Affiliation	Participation
Alan Robock (≥10)	Rutgers University	both
Cheng-en Yang	University of Tennessee, Knoxville	both

Daniele Visioni	Cornell University	both
David Mitchell	Desert Research Institute	both
Doug MacMartin (≥10)	Cornell University	both
Duoying Ji (≥10)	Beijing Normal University	both
Forrest Hoffman	Oak Ridge National Laboratory	both
Gabrielle Dreyfus	Institute for Governance & Sustainable Development	both
Hannah Horowitz	University of Washington	both
Jadwiga Richter (≥10)	National Center for Atmospheric Research	both
Jean-Francois Lamarque	National Center for Atmospheric Research	both
John Dykema	Harvard University	both
John Fasullo	National Center for Atmospheric Research	both
Karl Froyd	NOAA and the University of Colorado	both
Kate Ricke	University of California San Diego	both
Ken Caldeira (author, ≥10)	Carnegie Institution for Science	both
Lili Xia	Rutgers University	both
Long Cao (≥10)	Zhejiang University	both
Marianna Linz	University of California Los Angeles	both
Michael Wolovick	Beijing Normal University	both
Mike MacCracken	Climate Institute	both
Peter Frumhoff	Union of Concerned Scientists	both
Peter Irvine (author, ≥10)	Harvard University	both
William Lauenroth	Yale University	both
Zhen Dai	Harvard University	both
Alex Wong	Unaffiliated	discussion
Cynthia Scharf	Carnegie Climate Geoengineering Governance Initiative	discussion
David Goldston	Massachussetts Insittute of Technology	discussion
Elizabeth Burns (author)	Harvard University	discussion
Frank Keutsch (author)	Harvard University	discussion
Janie Thompson	Cassidy and Associates	discussion
Joseph Majkut	Niskanen Center	discussion
Katherine Thomas	National Academy of Science	discussion
Laurie Geller	National Academy of Science	discussion
Sarah Doherty	University of Washington	discussion
Simon Nicholson	Forum for Climate Engineering Assessment	discussion
Abdul Malik	Imperial College London	survey
Andreas Oschlies	Helmholtz Centre for Ocean Research Kiel	survey
Anthony Jones (≥10)	UK Met Office	survey
Antti-Ilari Partanen	Finnish Meteorological Institute	survey
Barbara Vogel	Forschungszentrum Jülich	survey
Ben Kravitz (≥10)	Indiana University	survey
Blaž Gaspirini	University of Washington	survey
Daniel Schlaepfer	Yale University	survey
David Keith (author, ≥10)	Harvard University	survey
Dipu Sudhakar	University of Leipzig	survey

Ellias Feng	Helmholtz Centre for Ocean Research Kiel	survey
Gabriel Vecchi	Princeton University	survey
Govindasamy Bala (≥10)	Indian Institute of Science	survey
Graham Mann	University of Leeds	survey
Hauke Schmidt (≥10)	Max Planck Institute for Meteorology	survey
Helene Muri (≥10)	Norwegian University of Science and Technology	survey
Jim Haywood	UK Met Office	survey
John Moore (≥10)	Beijing Normal University	survey
John Shepherd	University of Southhampton	survey
Katie Dagon	National Center for Atmospheric Research	survey
Ken Carslaw	University of Leeds	survey
Leslie Field	Stanford University	survey
Mark Lawrence (≥10)	Institute of Advanced Sustainability Studies, Potsdam	survey
Olivier Boucher (≥10)	Institut Pierre-Simon Laplace	survey
Paul Wennberg	Caltech	survey
Philip Rasch (≥10)	Pacific Northwest National Laboratory	survey
Robert M. Nelson	Planetary Science Institute	survey
Robert Wood	University of Washington	survey
Sandro Vattioni	ETH Zurich	survey
Simone Tilmes (≥10)	National Center for Atmospheric Research	survey
Sonia Seneviratne	ETH Zurich	survey
Steven Barrett	Massachussetts Insittute of Technology	survey
Subarna Bhattacharyya	Climformatics	survey
Thomas Ackerman	University of Washington	survey
Ulrike Lohmann	ETH Zurich	survey
Varun Mallampalli	Duke University	survey

Survey Instrument

Here, we provide a brief summary of the questions included in the survey. The full question text, which included important explanatory notes and assumptions, as well as the list of options we provided, can be found as Supplementary Text S1. The survey was implemented on an online expert elicitation platform developed by NearZero (http://www.nearzero.org/), Supplementary Figure 1 shows 2 screenshots of the survey on this platform.

Quantitative Questions. These questions were mandatory, except where otherwise stated. Space was provided after many questions for the participants to provide additional text input.

- Q1. What percentage of global funding for climate science should be spent on solar geoengineering over the next 10 years? *Box to specify number.*
- Q2. How would you prioritize the following research objectives to best support decision-making on solar geoengineering over the next 10 years? *Constrained sum with specified options, "other" option, and text box.*
    - Q2a-d. (optional) How would you prioritize the following research sub-objectives to best support decision-making on solar geoengineering over the next 10 years? *Constrained sum with specified options, "other" option, and text box.*
- Q3. 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. *Constrained sum with specified options, "other" option, and text box.*
    - Q3a-c. (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. *Constrained sum with specified options, "other" option, and text box.*

- Q4. *As question 3 and its sub-questions but for $300M*

- Q5. Indicate the relative priority of research into these solar geoengineering proposals. *Constrained sum with specified options, "other" option, and text box. Here we listed stratospheric aerosol geoengineering, marine cloud Brightening, cirrus cloud thinning, land surface albedo modification, space-based methods (e.g. space mirrors), ocean albedo modification.*

- Q6. 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. *Card-sort with same options as Q5 and option to add additional cards.*

- Q11a. (optional) 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. *Boxes to specify numbers. A figure*

accompanied this question showing the radiative forcing evolution of RCP 2.6, 4.5 and 8.5, see Supplementary Figure 2.

- Q11b. (optional) 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. *Boxes to specify numbers.*

The results for questions 1 and 11 have been previously published in Dai et al. (under review) who used identical questions in their survey of US and Chinese climate experts.

Qualitative Questions. With these questions we provided the participants with a text box and asked them to provide a few sentences to describe each of their suggestions. Additional instructions were provided along with these questions which can be found in Supplementary Text S1.

- Q7. Please identify solar geoengineering proposals that can address specific uncertainties.
- Q8. What finding would cause you to abandon research into solar geoengineering in general or into a specific method?
- Q9. What common misconceptions about solar geoengineering should be understood as one puts together a research agenda?
- Q10. 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.
- Q12. Do you have any final comments? What else should we have asked? What should be raised in our discussion on December 11th?

Coding specific research recommendations

In Question 7, we asked participants to describe a specific solar geoengineering uncertainty and the research that could be undertaken to address that uncertainty. We received 81 specific research recommendations from the 39 participants who answered the question. We coded all the specific research recommendations to quantify how frequently the different solar geoengineering technologies, research types, and research themes were mentioned in these proposals. Some answers were as short as a sentence identifying an issue to be addressed, whereas others were over a paragraph long. Our categorization was not exclusive, some of the brief proposals did not specify a type of research, whereas some longer proposals covered multiple technologies, research types and research themes. Figures 1 and 2 show the number of mentions of each technology and research type, respectively, and in Section 3 a summary of the content of these proposals is made, in part based on this coding.

3 Research prioritization and funding allocation

In defining a strategic solar geoengineering research agenda, decisions about the overall spending level and the allocation of funding across different priorities would need to be made. In this section, we provide an overview of the views of our participants on these issues from our survey and discussion (See Methods). In framing these survey questions, we asked our participants to consider what natural science research would best reduce the overall uncertainty about efficacy and risks of deploying solar geoengineering and best support decision-making on solar geoengineering in the near term.

"Q1: What percentage of global funding for climate science should be spent on solar geoengineering over the next 10 years?" (See Survey Question 1 in the Methods section, Q1 from hereon). Over the decade ending in 2018, approximately $30 Million of government funding targeted solar geoengineering (Necheles et al., 2018). This means that less than 0.1% of the government-funded climate research budget targets solar geoengineering (in 2017, the US alone spent ~2.8 Billion through its Global Change Research Program ("US Global Change Budget," 2017)). In their response to our survey question asking this, the median and mean response from our survey participants was 5% and 7.7%, respectively, with most answers lying between 1% and 10%. None of our participants allocated 0% funding. The ≥10 publication and <10 publication groups had the same median but mean responses of 6.0% and 8.5%,

respectively. While it seems reasonable to assume that our participants would be more supportive of solar geoengineering research than the broader climate science community, similar results were found when the identical question was asked of US and Chinese climate experts. Only one of 26 experts suggested no research should be done while the median and mean response was 4% and 5%, respectively (Dai et al., under review). To put our funding results in perspective, the median response of 7.7% would correspond to $140M per year in the US alone, roughly 50 times the annualized global government spending over the past decade (Necheles et al., 2018). Some participants may have interpreted this question to imply that funds for solar geoengineering research should be considered to be zero-sum with funds available to support climate science and this could have influenced answers. For example, a question on what fraction of global funding for national security should be spent on solar geoengineering research may have produced substantially different results.

There are several solar geoengineering proposals, which should receive the highest priority? Figure 2 shows the participants' views on this question and contextualizes that by also showing their views on whether the proposals are likely to achieve a global radiative forcing >2 Wm^{-2}, and the number of specific research recommendations that were made for each proposal (these specific research recommendations are discussed in Section 3). We asked the participants to explain their reasoning and the following quote from the survey captures several points raised in the discussion:

> "I have allocated funding based upon my intuition about viability (thus supporting stratospheric aerosol research), scientific importance (supporting MCB [marine cloud brightening] research which may be effective, and research would certainly contribute to critical fundamental research for climate science), and "novel proposals" which [for] 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.”

The participants reported that their views on the feasibility, efficacy, and risks of the different solar geoengineering proposals were central for their prioritization, with many noting that the value of the research for climate science more broadly was also a critical factor and some considering public acceptability as well. Stratospheric aerosol injection was widely agreed to be the most feasible, and it received the highest average prioritization at 34%, with 75% of participants giving it a higher priority than any other technology. Among the ≥10 publication group the emphasis on stratospheric aerosol geoengineering was greater, with an average prioritization of 43.6%. Many participants noted the substantial potential “co-benefits” of research into marine cloud brightening and cirrus thinning for understanding aerosol-cloud interactions, a key uncertainty in climate science (Wood et al., 2017). Most participants felt that it would be important to research a range of promising ideas and continue looking for novel ideas at this early stage, and this is reflected in the broad distribution of prioritization across the proposals. Many participants also argued that a research agenda should not overlook the potential benefits of smaller-scale, regional solar geoengineering proposals, e.g. land albedo modification and ice albedo modification (Field et al., 2018; Seneviratne et al., 2018), nor targeted interventions to address specific feedbacks, e.g. buttressing marine outlet glaciers (Wolovick & Moore, 2018). Several participants suggested that such smaller-scale interventions may be able to avoid some of the governance and other challenges of global-scale interventions as effects could be assumed to be localized. These claims were contested by other participants who argued that local interventions may carry larger risks per unit of climate benefit because the climate response is more uneven due to teleconnections; and also, because cloud-manipulation has a short timescale (hours) which enables the socially risky possibility of weather control and which in-turn increases risk of termination.

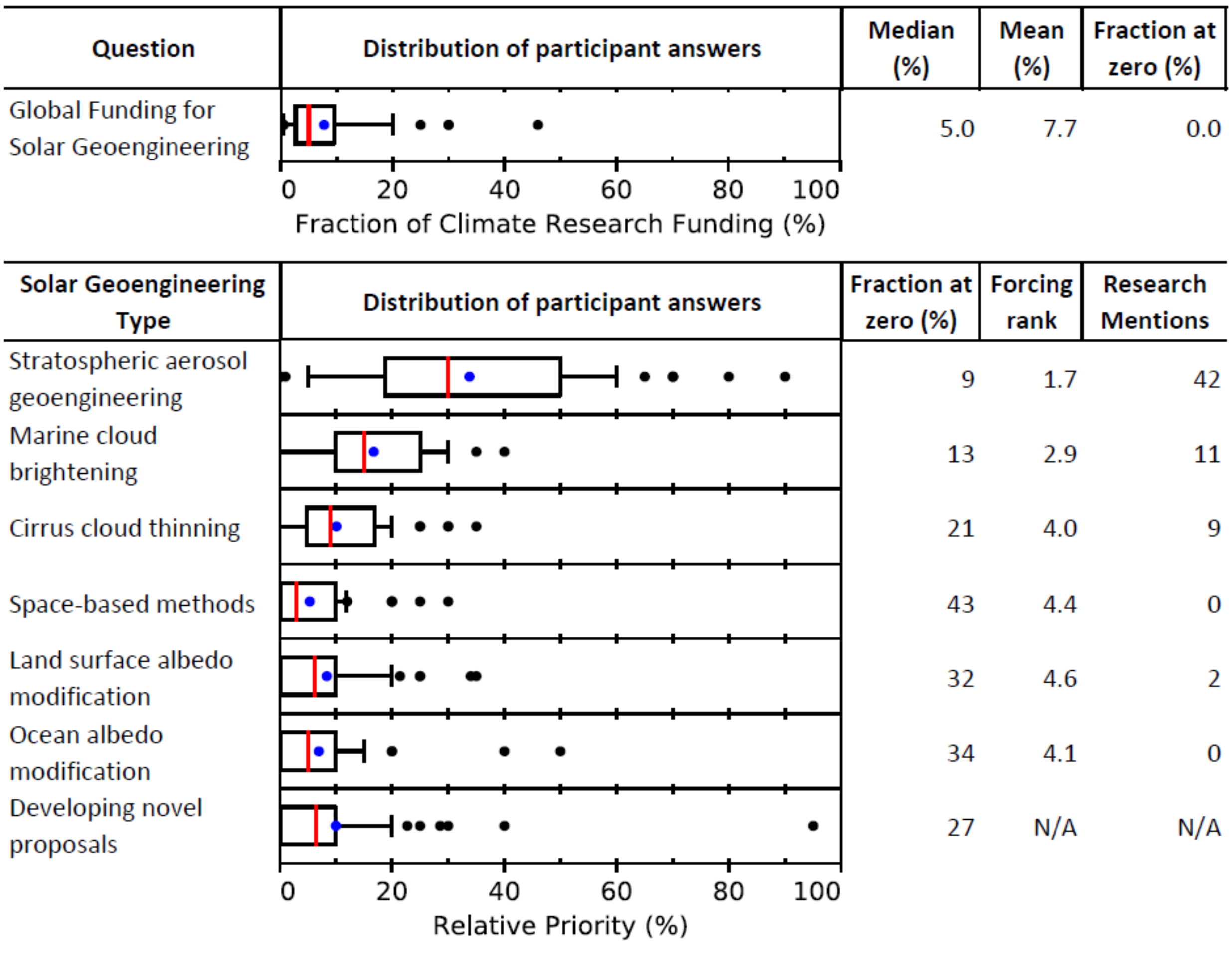


Figure 1. Box-Whisker plots showing the distribution of participant answers to Question 5: "Indicate the relative priority of research into these solar geoengineering proposals." 56 participants completed this question. The entries are listed in the order in which they appeared in the survey question. In the box-whisker plots, the red vertical line indicates the median, the blue dot shows the mean, the box shows the inter-quartile range, the whiskers show the 10-90% range, and points beyond the whiskers are marked with black dots. For each solar geoengineering technology, we report the fraction of participants who gave each technology zero priority

(Question 5), the mean rank that participants gave when asked to rank the likelihood of achieving >2 Wm^{-2} with each technology (Question 6), and the number of mentions in specific research proposals (Question 7).

Which high-level research objectives should be pursued to best support decision-making on solar geoengineering? Figure 2 shows that in their answers, the participants' put the greatest emphasis on advancing the understanding of the Earth system response to solar geoengineering proposals and on the impacts that they would have on society and ecosystems. Despite the predominance of natural scientists, participants gave substantial weight to the social dimensions of solar geoengineering (Anshelm & Hansson, 2014). Most participants also gave considerable weight to the goal of developing or improving methods for solar geoengineering. The goals of developing weather control applications of solar geoengineering and developing counter-geoengineering (ideas to counteract its cooling effect (Parker et al., 2018)) were given no weight by most participants. The prioritization of research objectives for the ≥10 publication group were very similar to those of the <10 publication group.

Research objective	Distribution of participant answers	Median (%)	Mean (%)	Fraction at zero (%)
Earth system response		30.0	32.9	0.0
Impacts on society and ecosystems		30.0	27.4	0.0
Human and social dimensions		15.0	16.9	1.7
Developing or improving methods		14.7	15.1	8.5
Weather control		0.0	3.6	62.7
Counter-geoengineering		0.0	2.4	69.5
Other (not listed)		0.0	1.7	84.7

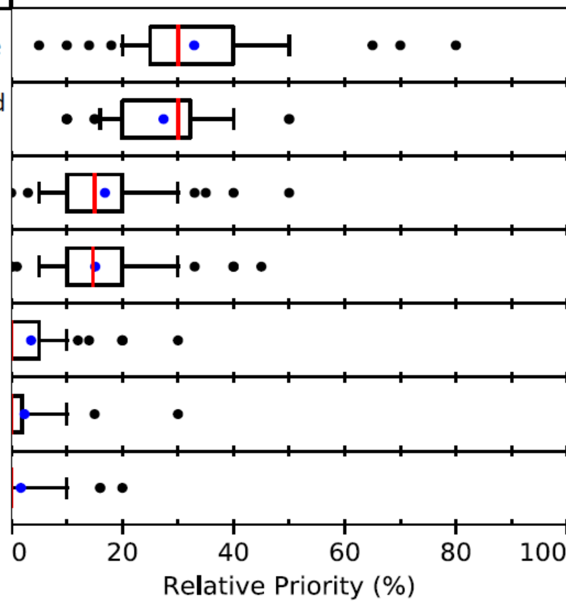
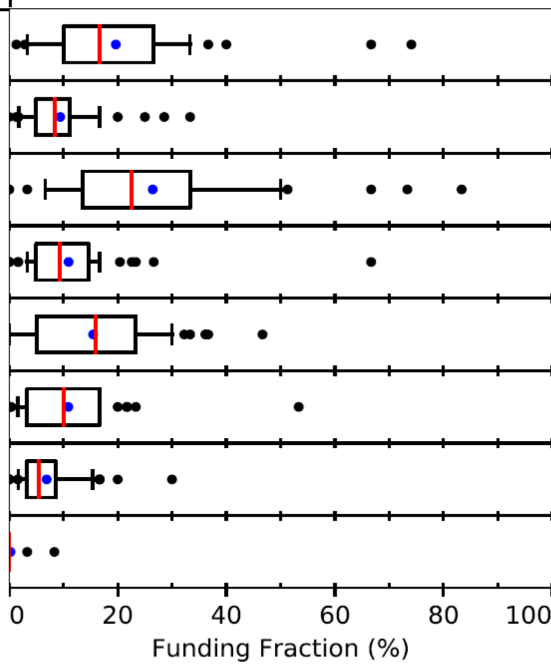


Figure 2. Box-Whisker plots showing the distribution of participant answers to Question 2: "How would you prioritize the following research objectives to best support decision-making on solar geoengineering over the next 10 years?" 59 participants completed this question. This is plotted in the same way as figure 1. For each research objective we also report the median and mean value for relative priority, and the fraction of participants who gave each objective zero priority.

How should funding be allocated across different types of research to best reduce the overall uncertainty about efficacy and risks of deploying solar geoengineering? Figure 3 shows that observations (of current conditions and natural analogs rather than solar geoengineering deployment) received the greatest funding overall, followed by modeling, then perturbative field experiments, with the others all receiving some funding from most participants. We asked the participants to imagine they were allocating funding in the US at two different funding levels $300M (see Figure 3) and $30M (see Supplementary Figure 1), and our notes invited the participants to consider the costs of individual budget items such as aircraft observation missions, which can cost many millions of dollars. Our results show a shift in priorities towards research areas essential to supporting decisions around deployment for the larger $300M budget, with the fraction allocating no funding to perturbative field experiments and engineering research on deployment systems dropping from 24% and 31% (respectively) at $30M to 15% and 9% at $300M. A break-down of these results into sub-categories of modeling, observation and field test research for the $300M budget can be found in Supplementary Figure 2. As for the research prioritization, there were only small differences between the results for the ≥10 and <10 publication groups which had results for each category that were within a few percentage points.

In our discussion, we learned that participants made sharply different assumptions about this possible run up to deployment when prioritizing research. Some believed that climate change was so pressing a risk that demands for action on solar geoengineering could be near at hand, some believed that such demands likely wouldn't come for decades, and others that they would never materialize. Different assumptions about timeline affected their prioritization of research, with a longer timeline implying that a better general understanding of the idea would be the goal of the research effort and a shorter timeline implying a need to address all aspects that could inform a decision to deploy. When asked about a larger funding effort ($300M over 10 years, rather than $30M) many assumed this implied a more imminent decision to deploy and adjusted their allocations accordingly. While there was no agreement on a likely timescale on which practical decisions about deployment might be made, there was general agreement that assumptions about this timeline would play an outsized role in determining a research agenda.



Type of research	Distribution of participant answers	Mean (%)	median (%)	Fraction at zero (%)	Research mentions
Geophysical modeling		19.6	16.7	0	49
Climate impacts modeling		9.4	8.3	2	9
Observations		26.4	22.5	4	22
Laboratory research		10.9	9.3	7	13
Perturbative field experiments		15.5	16.0	15	19
Engineering for deployment		10.9	10.0	9	6
Social science and humanities		6.9	5.5	9	1
Other (not listed)		0.3	0.0	96	N/A

Figure 3. 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 $300M for the US over 10 years.” 46 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).

4 Specific research recommendations

A solar geoengineering research agenda will require decisions about the allocation of funding across different priorities and an understanding of the major uncertainties that research could address. In this section, we provide a brief summary of the specific research needs identified by our participants. Those who wish to know more can read the full response to this survey question (Q7) and the others in Supplementary Texts S3-7.

In their research recommendations, our participants focused primarily on stratospheric aerosol geoengineering, which received 42 mentions, with marine cloud brightening and cirrus cloud thinning receiving many fewer, 11 and 9, respectively (see Figure 1 and Methods). The only other solar geoengineering proposals to be mentioned in research proposals were land surface albedo modification (2), ocean pipes (1) (Lovelock & Rapley, 2007), and sea-ice albedo modification (1) (Field et al., 2018).

Many research proposals addressed uncertainties specific to certain solar geoengineering technologies, and others were more general. Most marine cloud brightening research proposals (8 of 11) and all cirrus cloud thinning (9) research proposals focused on uncertain aerosol-cloud processes, whereas for stratospheric aerosol geoengineering a few uncertainties received particular attention: stratospheric aerosol processes (12 mentions), stratospheric chemistry (11) and stratospheric dynamics (6). Here we list specific stratospheric aerosol geoengineering uncertainties that were raised by multiple participants:

- The limited understanding of the background state of the stratosphere.
- The impact on stratospheric dynamics, including stratosphere-troposphere coupling and the Quasi-Biennial Oscillation which could be addressed through further model development.
- The dynamic, chemical and aerosol feedbacks that would shape the forcing response to different levels of aerosol injection.
- The impact of sedimenting aerosols on cirrus and other cloud types (See, e.g. Cirisan et al., 2013).
- The radiative and chemical properties of novel aerosol types and their broader impacts (Keith et al., 2016).
- The impacts of increased diffuse light on ecosystems (Mercado et al., 2009; Proctor et al., 2018).

Most marine cloud brightening research proposals stressed the value of field tests for resolving the aerosol-cloud uncertainties (7 of 11). For cirrus cloud thinning, field observations were more frequently (5 of 9) mentioned than perturbative field experiments (2), perhaps reflecting the greater uncertainty around the current state of cirrus clouds. In our discussion of stratospheric aerosol geoengineering, several participants suggested that perturbation experiments (6 of 42) ought to be a low priority as there is much more that could be learned through observation (14), modeling (27), and lab research (11), a view borne out in the survey responses (number of mentions in brackets).

In addition to proposals addressing the various specific uncertainties associated with each type of solar geoengineering there were many general proposals addressing uncertainties around the consequences of solar geoengineering in general. The most frequently raised uncertainties in these research proposals were:

- the climate response (20), including the hydrological response (6)
- the human and ecological impacts (18), in particular terrestrial ecosystem impacts (12)
- biogeochemistry and carbon cycle changes (13)
- the ocean ecosystem response (6)

In our discussion, some participants suggested that evaluating the impacts of solar geoengineering may in some ways be more challenging than for climate change, as a world with high CO_2 but low temperatures is unprecedented in the more recent paleo-climate record, though the Cambrian may provide an analogous case (Lunt et al., 2008).

5 Technical challenges for solar geoengineering research

The most fundamental scientific challenge that our participants identified was that many of the processes through which solar geoengineering proposals would modify the climate are poorly understood. The feasibility of both marine cloud brightening and cirrus cloud thinning are particularly uncertain as they both rely on poorly understood aerosol-cloud interactions, though there was more confidence in the feasibility of marine cloud brightening. While our participants judged that stratospheric aerosol geoengineering was likely feasible, its efficacy depends on uncertain stratospheric aerosol microphysics and dynamics amongst other factors. For all solar geoengineering proposals, their consequences would depend on uncertain, and in some cases poorly understood, climate processes, that can only be simulated in climate models.

Models are thus critical for the study of solar geoengineering and participants agreed that improving modeling capability and addressing model limitations should be a key part of a solar geoengineering research agenda. Some of these model limitations could be addressed or better understood by applying process-level models and cloud-resolving models whereas others will require developing new capabilities, e.g. to represent sub-gridscale processes related to deployment. And in other cases, improved observations and understanding would be needed to guide future model developments.

Observations are thus also critical for advancing understanding of the uncertain processes on which solar geoengineering depends. Most of our participants stressed the value of observations and analysis of natural analogues which could provide an excellent test of our understanding of these uncertain processes. For stratospheric aerosol geoengineering, past, major volcanic eruptions were widely recognized as a valuable test of model performance. For marine cloud brightening low-altitude volcanic eruptions (Malavelle et al., 2017) and changing aerosol pollution patterns (Samset et al., 2019), including the upcoming change in international maritime organization standards for sulfur emissions, would provide natural analogues that could be studied in greater depth. For cirrus cloud thinning, large forest fires and Saharan dust storms could provide valuable information about the response of cirrus clouds to ice nucleating particles. Our participants agreed that a key challenge for a solar geoengineering research agenda would be defining the observational tools (e.g. which satellite instruments) and targets (e.g.

volcanic eruptions) that could best constrain key solar geoengineering uncertainties and anticipating the observational needs for monitoring and guiding large-scale deployment.

6 The challenges of climate control

Solar geoengineering is not a fact about nature to be studied, or the applied scientific challenge of estimating the climate response to human perturbations, but a perturbative technology that would be designed to meet some goal. That is, solar geoengineering is a design problem (Ban-Weiss & Caldeira, 2010; Keith, 2013; Kravitz et al., 2016). To implement solar geoengineering, an objective must be defined and a strategy to achieve it developed. This poses several linked technical and policy challenges that any solar geoengineering research agenda will need to tackle.

Our participants highlighted some technical challenges climate control poses. First, there is the need to develop appropriate control strategies. Several papers have developed approaches for climate control (Kravitz et al., 2014; MacMartin et al., 2014), and these approaches have been applied in climate model studies, for example in the Geoengineering Large Ensemble (GLENS) project (Kravitz et al., 2018). However, the broader solar geoengineering modeling community has yet to integrate these feedback approaches. Second, the observational needs for realizing climate control effectively need to be identified and the limitations of current and possible observational capabilities understood. Third, there are a wide range of possible climate objectives and means for achieving them, as each solar geoengineering technology can be deployed in different ways and different approaches could be combined (Cao et al., 2017). Our participants noted that it’s impossible to map out all of these options as there are many climate impacts to address and uncertainties to account for.

The goals or objectives for solar geoengineering deployment are (for the purposes of climate modeling), and will remain (for any potential deployment), socially constructed and contested. A key challenge then is that defining objectives for solar geoengineering deployment and the measures by which to evaluate its performance are not simply scientific questions. Solar geoengineering deployment would shape diverse risks affecting diverse actors with diverse interests and views. However, to date, climate modelers have been the only ones to articulate potential objectives for solar geoengineering, through the scenarios they simulate, and have defined the metrics by which to evaluate it. As climate modelers have been framing the objectives and analysis of solar geoengineering, their views and values have played a critical role in shaping the debate on solar geoengineering and will continue to do so.

An important question then is the extent to which solar geoengineering researchers share views on policy that would guide deployment. We asked participants (the majority of whom are climate modelers) to imagine that they "are charged with designing a climate policy that mitigates risks caused by climate change" and to "consider realistic trade-offs between climate goals and social/economic impacts" when making their choice (see Q11 in Supplementary Text S1 for the full question text). We told them that they could choose to achieve their goal through emission reduction and (optionally) solar geoengineering. The distribution of responses to this question is shown in Figure 4. Figure 4 also shows our participants' best estimates of the estimated radiative forcing in 2075, assuming solar geoengineering is not deployed. Of the 38 participants who answered this question 22 (or 58%) included solar geoengineering alongside emission cuts to achieve their chosen radiative forcing outcome. While the estimated radiative forcing results for the ≥10 and <10 publication group were nearly identical, only 40% (6 out of 15 who answered the question) of the ≥10 publication group included solar geoengineering in their chosen policy compared to 70% (16 out of 23) <10 publication group. This, rather counter-intuitively, suggests that those who have shown greater interest and invested more time in solar geoengineering research are less supportive of its use in climate policy. The same question was asked of US and Chinese climate experts in Dai et al. (under review) and only 4 of 26 experts included solar geoengineering in their chosen climate policy.

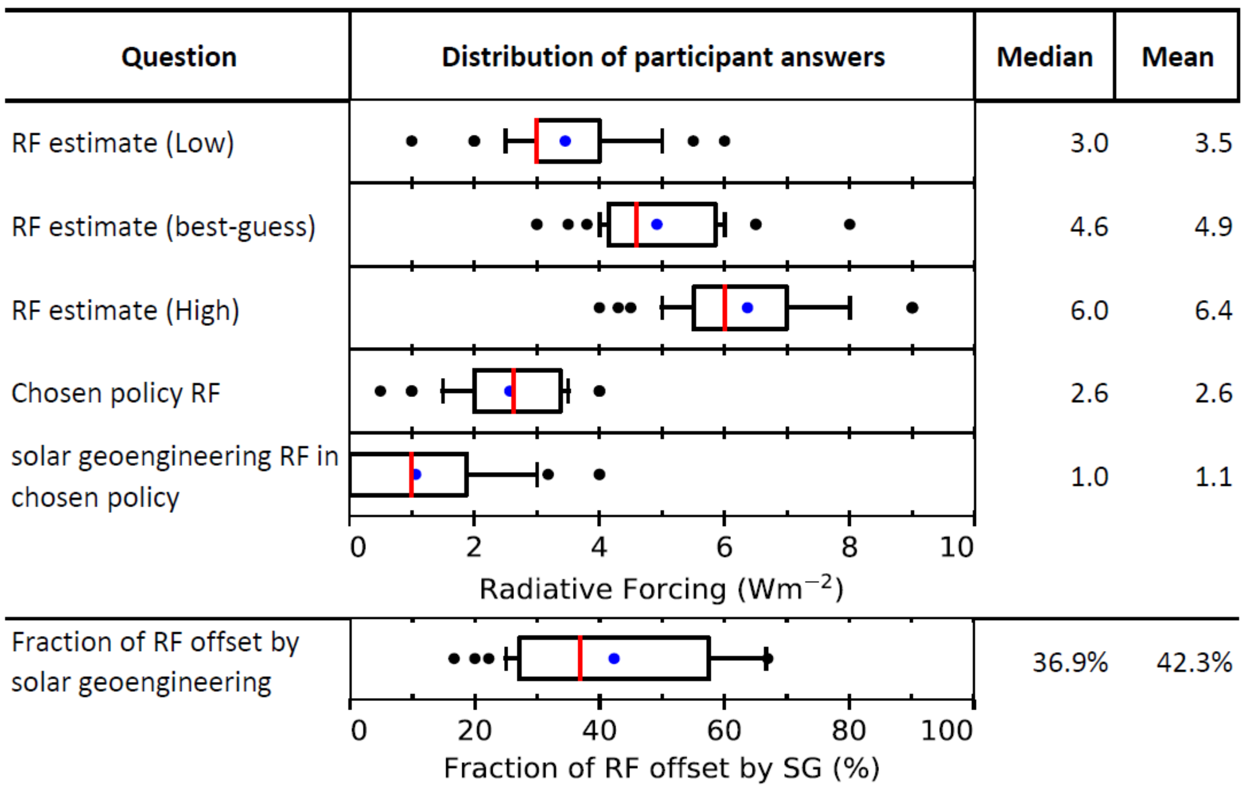


Figure 4. Box-Whisker plots showing the distribution of participant answers to question 11 addressing participants' estimated and desired radiative forcing in 2075**.** 58 participants completed question 1 and 38 completed all parts of question 11. In the box-whisker plots, the red vertical line indicates the median, the box shows the inter-quartile range, the whiskers show the

10-90% range, and points beyond the whiskers are marked with circles. The mean response and the fraction of participants who gave answers of zero are also reported (where applicable). For those participants who included solar geoengineering in their chosen case, we include results for the fraction of radiative forcing offset by solar geoengineering (note that this is relative to the pre-industrial level).

While our results do not allow us to determine what led our participants to make their choices, our survey results suggest a couple of possible explanations for this split. First, some of our participants expressed the hope that emissions cuts would be sufficient to obviate the need for solar geoengineering (See responses to Q8 in Supplementary Text S4). Second, our participants could have reached different judgements about the benefits, risks and uncertainties of solar geoengineering (see the responses to Q9 in Supplementary Text S5). Beyond these possibilities the broader literature suggests several other concerns that could have motivated their answers: there is the widespread concern that developing or even discussing solar geoengineering could discourage near-term emissions cuts (a concern that is often referred to as “moral hazard” in this context) (Lin, 2013); some oppose solar geoengineering as they think to deploy it would mean humans are intervening in a system that they should leave alone (Jamieson, 1996); some are concerned that adequate governance arrangements for managing solar geoengineering deployment may prove difficult to reach (Bodansky, 2013); some are concerned that such a failure to govern this technology could raise international tensions and potentially lead to conflict (Fleming, 2012); and finally, others are concerned about the long-term commitment and risk of termination shock that large-scale solar geoengineering deployment would bring (Parker & Irvine, 2018).

Climate researchers agree more strongly on climate science and they do on climate policy (Cook et al., 2016). Similarly, we find that solar geoengineering researchers agree more strongly on science and on research prioritization than they do on policy choices related to the application of these technologies.

Given the importance of values in determining the objectives for solar geoengineering and in shaping policy on this issue, there was broad agreement among our participants that some form of public and policymaker engagement would be important for the development and conduct of a strategic solar geoengineering research program. Such engagement was understood to be important both for guiding its direction and establishing its legitimacy (Carr et al., 2013). However, many of our participants were concerned that solar geoengineering faces considerable challenges related to public and policymaker perception and understanding. One concern was that it could be seen as an alternative to emissions cuts (the moral hazard concern) and another was that perceptions of its risks would bear no relation to the science. However, empirical public perception analysis suggests that publics, particularly those in countries more vulnerable to climate change (Sugiyama, Asayama, Ishii, et al., 2017), cautiously support research though are wary about potential deployment (see Burns et al. (2016) for a review of public perception research on this topic). Another concern our participants raised was the widespread chemtrails conspiracy which may complicate engagement on this issue considerably (Tingley & Wagner, 2017)**.**

7 Summary and outlook for developing a solar geoengineering research agenda

Through a survey and discussion with a total of 72 solar geoengineering experts, including two thirds of experts with ≥10 publications in the field (according to a web of science search), we elicited views on the research priorities, specific research recommendations and challenges for a strategic solar geoengineering research agenda.

Our participants expressed a range of views on the priorities for a solar geoengineering research agenda but when taken together some patterns emerge (Summarized in Section 2). When asked to allocate priority across the different technologies, stratospheric aerosol geoengineering received the highest average prioritization (34%, Figure 1) and was ranked highest by 75% of participants, with marine cloud brightening second (17%), though our participants generally supported researching a broad range of proposed technologies and continuing to develop new ideas (49%). In our discussion, some participants argued for more attention on regional solar geoengineering approaches (which were not addressed in our survey) as compared to global-scale approaches, though the group was divided on the relative merits of these approaches. For research objectives, our participants put the highest priority on research which would advance our understanding of the Earth System response to solar geoengineering (33%, Figure 2) and its climate impacts (27%) but also supported research that developed or improved methods for solar geoengineering (15%) and which explored its human and social dimensions (17%). However, most of our participants did not allocate any priority to developing counter-geoengineering or weather control applications of solar geoengineering (N.B., this judgment is shared by the authors.). Overall, we found only small differences between the prioritization of research by the more experienced solar geoengineering experts, i.e. those with ≥10 publications, and the others, with the only notable difference being that stratospheric aerosol geoengineering received a greater mean priority of 43.5% from the more experienced researchers, compared to 29.1% from the others. While all academics have specific research interests and biases, we are not aware that any of these researchers have any relevant financial or related interests outside their particular research.

One issue that this exercise revealed was that the priorities for a solar geoengineering research agenda would seem to depend on the assumed timeline to a potential decision to deploy. For example, many of our participants thought that a shorter timeline would demand a greater focus on the practicalities of deployment but there was little agreement on what would be a reasonable timeline to assume. This poses a challenge for those developing the goals for a research agenda as the choice of assumed timeline carries significant political baggage.

We also found some common suggestions when we asked participants for specific research recommendations (summarized in Section 3), and when we asked about specific technical challenges for solar geoengineering research (Section 4). Our participants generally agreed that modeling and model development would be a central part of any solar geoengineering research agenda but that a major challenge was the deep uncertainties around processes that would be central to predicting the effects of solar geoengineering. They stressed that observations of the current climate state and of natural analogues for solar geoengineering would be essential to addressing these uncertainties. On perturbative field experiments the views were mixed, some stressed their value for addressing uncertainties around the aerosol-cloud interactions central to marine cloud brightening, though others questioned their value for stratospheric aerosol geoengineering, suggesting much could be learned by other means at this stage. In addition, our participants noted the challenge of evaluating the likely impacts of solar geoengineering given the wide range of possible ways that it could be deployed, the potential to design deployment to achieve particular ends, and the difficulty of comparing the complex mix of risks across the range of scenarios including and excluding solar geoengineering.

Solar geoengineering poses some unique research challenges as unlike climate change it is a design problem. Solar geoengineering would require defining objectives and developing a control strategy to achieve those objectives. While developing a control strategy is a technical challenge, albeit a novel one for climate science, defining objectives for solar geoengineering is not simply a scientific challenge but also a political challenge. It is thus important to understand what values and judgements researchers working on this field bring with them and whether a consensus view is emerging. We found that our participants did not have a unified position when asked whether they would include solar geoengineering alongside future climate policy when asked to choose their desired outcome (see Section 5 and Q11 for more details). On this point we found the starkest differences between the more experienced (≥10 publications) and less experienced researchers, with only 40% of experienced researchers including solar geoengineering deployment in their chosen scenario compared to 70% of less experienced researchers. One interpretation for this result is that more experienced natural science researchers better understand the risks of solar geoengineering and so are more reluctant to recommend it. However, in the authors' view, our results and the broader literature suggest that researchers' values, political judgements and views on the policy implications of solar geoengineering may be more diverse than views on the science of solar geoengineering. Future research which makes a more detailed analysis of the views and judgments of solar geoengineering researchers will be needed to resolve this issue.

The people we assembled were predominately expert on various aspects of the natural sciences and our questionnaire emphasized issues in the natural sciences. Our focus on research needs in the natural sciences should not be interpreted as undervaluing the potential for important contributions from the social sciences. Indeed, many of the most important risks of solar geoengineering may derive from how political and social systems respond to proposed solar geoengineering rather than from the direct physical effects of solar geoengineering deployment (Shepherd et al., 2009). Furthermore, our participants broadly agreed that public and policymaker engagement would be important for guiding the direction of solar geoengineering research and building its legitimacy. There were also concerns that communication and engagement on this issue would be challenging, especially given the wide-spread chemtrails conspiracy (Tingley & Wagner, 2017). Similarly, while our survey and discussion focused on a natural science research agenda for the USA, our participants generally agreed that solar geoengineering research is an international concern.

Research to date suggests that solar geoengineering may have the potential to substantially reduce many climate risks but there remain significant scientific uncertainties about its feasibility, potential, limits and arguably deeper uncertainties around its broader political ramifications. A strategic research agenda that aimed to tackle these scientific uncertainties would seem to be a necessary, albeit not sufficient, condition for wise decision-making about whether and how to deploy solar geoengineering. Here, we have identified some broad areas of agreement on the technical content of such a strategic research agenda and highlighted some of the broader difficulties that would need to be overcome in framing such a strategic research agenda. We hope that this work will be helpful for those working to develop a strategic research agenda for solar geoengineering and those working to develop research policy on this topic.

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Availability of data and material – The full survey responses can be found in the supplementary materials, and copies of the original files will be made available on request.

Code availability – The code used to produce the figures will be made available on request.

Authors' contributions – PI and DK conceived the project and led the writing of the article. PI conducted the analysis and produced the figures. PI and EB developed and implemented the survey. EB organized the in-person meeting. All authors contributed to the interpretation of the results and the writing of the article.

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