Mission-driven research for stratospheric aerosol geoengineering

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The last decade has seen broad exploratory research into stratospheric aerosol (SA) geoengineering, motivated by concern that reducing greenhouse gas emissions may be insufficient to avoid significant impacts from climate change. Based on this research, it is plausible that a limited deployment of SA geoengineering, provided it is used in addition to cutting emissions, could reduce many climate risks for most people. However, "plausible" is an insufficient basis on which to support future decisions. Developing the necessary knowledge requires a transition toward mission-driven research that has the explicit goal of supporting informed decisions. We highlight two important observations that follow from considering such a comprehensive, prioritized natural-science research effort. First, while field experiments may eventually be needed to reduce some of the uncertainties, we expect that the next phase of research will continue to be primarily model-based, with one outcome being to assess and prioritize which uncertainties need to be reduced (and, as a corollary, which field experiments can reduce those uncertainties). Second, we anticipate a clear separation in scale and character between small-scale experimental research to resolve specific process uncertainties and global-scale activities. We argue that the latter, even if the radiative forcing is negligible, should more appropriately be considered after a decision regarding whether and how to deploy SA geoengineering, rather than within the scope of "research" activities.

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Reducing human emissions of CO_2 and other greenhouse gases (mitigation), while essential, is unlikely to occur fast enough to avoid significant risks from climate change (1, 2). This concern has prompted initial research into solar geoengineering (3, 4): the idea that one could deliberately cool the planet with approaches such as putting aerosols (or their precursors) into the stratosphere (5) or "brightening" marine clouds (6). This cannot be a substitute for mitigation, both because it would not address all climate impacts (e.g., ocean acidification) and because the forcing would need to be sustained for as long as the CO_2 remains in the atmosphere, of the order of centuries or longer (7). However, solar geoengineering could become an additional element of an overall integrated strategy for addressing climate change (8–10), as shown in Fig. 1. Climate modeling to date suggests that it is plausible that a limited deployment of stratospheric aerosol (SA) geoengineering, used as a supplement to mitigation and atmospheric CO_2 removal, might reduce many climate impacts (11) including temperature and precipitation extremes (12), sea-level rise (13), and hurricane frequency or severity (14), among others.

However, the current state of knowledge is insufficient to conduct a thorough risk assessment. Solar geoengineering would not affect the climate the same way as reducing greenhouse gas concentrations; it thus poses both physical climate risks and risks arising from societal processes, such as the potential for affecting the commitment to mitigation or the risk of abrupt termination (15, 16). Ultimately the risks of deploying geoengineering will need to be weighed against the risks of not deploying geoengineering.

Decisions about whether and how to deploy solar geoengineering will require appropriate governance,

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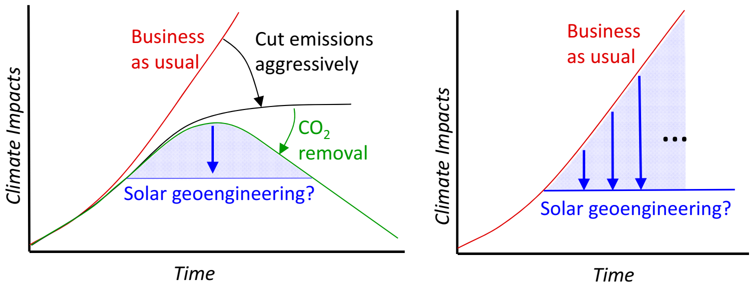

Fig. 1. (*Left*) Reducing greenhouse gas emissions, combined with large-scale atmospheric CO$_2$ removal, may lead to long-term climate stabilization with some overshoot of desired temperature targets. There is a plausible role for temporary and limited solar geoengineering as part of an overall strategy to reduce climate risks during the overshoot period. Adapted with permission of the Royal Society, from ref. 8; permission conveyed through Copyright Clearance Center, Inc. (*Right*) Geoengineering, instead of reducing emissions, would require extremely large forcing to be sustained for millennia, and is thus not realistic.

the form and function of which are active areas of research (17–21). Natural science research provides information about how one can design a strategy to meet some set of chosen climate objectives, what the impacts of deploying different strategies might be, and what the uncertainties and risks are. Addressing these natural-science questions is essential both to inform future decisions and also to inform what governance is needed to make these decisions. For example, geoengineering governance may depend on the projected distribution of benefits and harms, or the degree of uncertainty in projected outcomes.

Developing the required knowledge demands a mission-driven research program (22), which is defined by its explicit end goal of supporting informed future decisions regarding deployment (see also arguments for research in refs. 23–27). In contrast, while the curiosity-driven research model that has been used to date is appropriate for initial exploration, it lacks a systematic research approach; mission-driven research is designed to be comprehensive, ensuring that the important questions are being asked and addressed, and it explicitly incorporates prioritization, ensuring that limited funding is used efficiently in support of the goal. This prioritization will be different from that of a research program aimed at advancing understanding of the climate system.

We focus herein on SAs rather than marine cloud brightening (MCB), as the two have distinctly separate research needs in many aspects. From large volcanic eruptions, it is known that adding aerosols (such as sulfate) to the stratosphere is certain to provide some cooling on a global scale (28). Moreover, this method of geoengineering is nearly certain to be technically possible (29, 30).

The next sections describe the goals of research, distinct research phases, and natural separations in fundamental decision points.

Research Goals

Three Interconnected Questions. The first step in defining mission-driven research is articulating the mission: What does it mean to support informed decisions about geoengineering? There are three overlapping natural-science questions that will need to be addressed.

i) How would one deploy to meet specified objectives? Given some objectives, what options (including defining choices such as the latitude or season of aerosol injection) exist to meet them? Which climate outcomes are achievable, and which are not? What are the observational requirements to assess outcomes and conduct attribution, ensuring that any hypothetical deployment is achieving what is intended?

ii) What are the projected climate impacts of different deployment options? Which climate change impacts would be reduced? What additional impacts arise directly from the aerosols (e.g., ozone, health, etc.)?

iii) What is our confidence in predicting outcomes? What is the range of plausible outcomes? What is the justification for our confidence? Are there deployment strategies that minimize the impact of uncertainties? What research would be needed to further reduce uncertainty?

Curiosity-driven research to date has tended to focus almost exclusively on the second of these three questions. However, while climate-change impacts can be studied as a pure "science" problem (e.g., specifying emissions and understanding the response), a key feature of geoengineering is that it also involves a design element (31–34). The impacts of geoengineering depend on choices such as the latitude of injection (35, 36), the season of injection (36), and the type of aerosol (37). For example, recent simulations (38) combine injections at multiple latitudes both to avoid shifts in tropical precipitation that would arise from overcooling one hemisphere relative to the other (39) and to manage the pattern of overcooling the tropics relative to the poles seen in many earlier simulations (40). Design choices also affect which uncertainties matter. For example, equatorial injection of SO$_2$ leads to influences on the stratospheric quasi-biennial oscillation (41) but off-equatorial injection does not (42). Similarly, equatorial injection may lead to greater nonlinear effects on aerosol growth than off-equatorial injection (35). One might deliberately choose one strategy over another because its outcomes are less sensitive to uncertainty.

This potential for design means that research into these three questions must be integrated; neither impacts assessment nor uncertainty assessment is meaningful in isolation from particular deployment options, while those deployment options cannot be isolated from the climate objectives they are chosen to meet.

Uncertainty. Nonetheless, the bulk of the long-term research effort will involve reducing the uncertainty in predicting the effects of geoengineering. Uncertainty is at the heart of informing the risk–risk trade-offs in decisions regarding whether to deploy or not deploy geoengineering; this includes understanding which uncertainties are most critical, how those uncertainties can be reduced or managed, and a plan for any potential consequences of a decision around deployment.

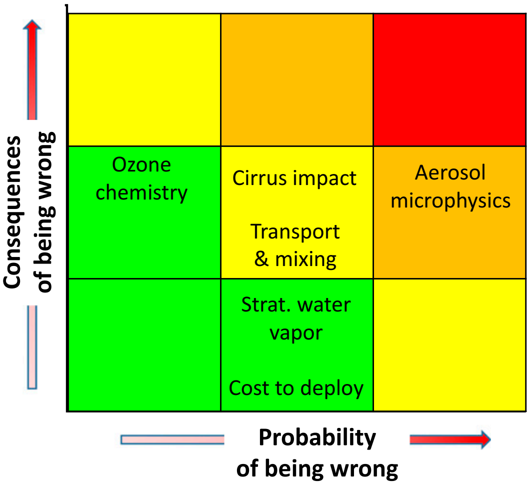

Fig. 2. Hypothetical risk register for SA geoengineering. Each uncertainty is categorized in terms of the probability of occurrence and the severity of occurrence. For each uncertainty, strategies need to be defined to reduce either the probability (through more knowledge) or the severity, or both. The placement here is illustrative only; there is insufficient knowledge today to support where each uncertainty falls on either axis. One of the goals of the initial phase of strategic research should be to make better assessments for where different uncertainties fall in this space.

An efficient research process should also focus on retiring the most critical risks early. If there are reasons to stop further research or define fundamentally different strategies, those should be identified as early as possible.

One tool that can help foster a conscious, explicit approach to uncertainty is to develop and maintain a risk register, a standard tool in managing engineering projects. An illustrative example is given in Fig. 2. For any uncertainty (e.g., in predicting aerosol size distribution) a qualitative judgment can be made on two axes: the probability of being (significantly) wrong and the consequences of being wrong. The combination of these two describes the risk associated with each uncertainty.

The intent is not quantitative accuracy but rather to foster dialogue, identify priorities, and motivate necessary research. For those uncertainties that are likely, severe, or both, one should identify risk-reduction options in both axes: (*i*) what steps can improve knowledge (thus shifting the uncertainty to the left of the diagram) and (*ii*) what steps might reduce the severity (shifting the uncertainty downward). For aerosol size uncertainty, for example, small perturbative stratospheric experiments (43) might reduce uncertainty, while a feedback strategy (33, 38, 44) might make climate outcomes less sensitive to errors in estimating the size distribution. Note that uncertainties are not restricted to scientific ones (e.g., the cost to deploy is also uncertain).

The placement of individual uncertainties in Fig. 2 is only illustrative, because there is not any objective basis today for choosing where to place them. One of the most important initial goals of research should be to provide a better basis for this chart and hence a better basis for prioritizing which uncertainties need to be resolved first. This need for prioritization illustrates the importance of transitioning to a mission-driven approach.

Phases of Research

Given the societal concerns, likely higher costs, and possibly physical risks associated with field experiments, whenever possible addressing a question through modeling should precede field research, observational campaigns should precede perturbative experiments, and process-scale perturbative experiments should precede larger-scale experimentation. This progression is illustrated in the qualitative timeline sketch in Fig. 3. None of these phases should proceed without two conditions not explicitly shown in the figure: that the results from the prior phase of research justify continuing research, and that governance is in place appropriate to the nature and scale of research. Our Fig. 3 is similar in principle to figure 2 of ref. 45 and overlaps in message with figure 1 of ref. 46, but with some important different emphases. We describe high-level characteristics of each phase in Fig. 3 first; *Research Versus Predeployment* will focus more attention on the dividing line between "research" and "operations."

Current Status. To date, solar geoengineering research has been almost exclusively model-based. Early geoengineering research often simply "turned down the sun" in a climate model to explore how the climate responds differently to one mechanism of radiative forcing compared with another (40). However, not only would SAs affect the climate differently from a solar reduction, but simulations with a solar reduction cannot resolve some effects of design choices such as the latitude of aerosol injection or choice of aerosol.

It is only recently possible to simulate SA geoengineering in climate models that simultaneously include many essential

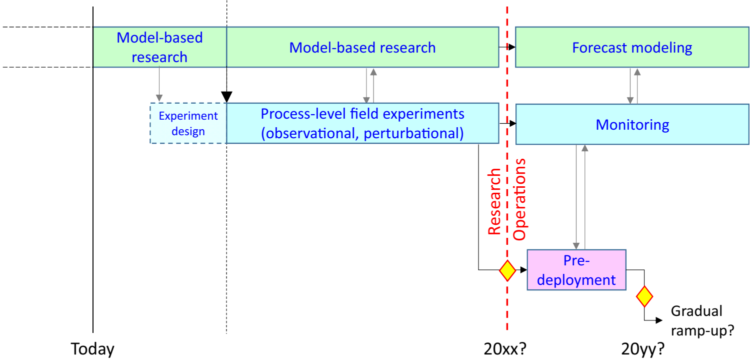

Fig. 3. Potential research phases that would support an informed decision regarding deployment. Progression and timing would depend on both prior outcomes and parallel development of appropriate research governance. A critical decision point precedes "predeployment" activities: global-scale validation tests that we recommend not be started until after a deployment decision, with the expectation that deployment would follow if outcomes matched expectation. This marks a hard boundary between the focus of activity.

processes, including aerosol microphysical growth (how injections of SO_2, for example, oxidize into H_2SO_4 and form sulfate aerosols through condensation and coagulation), stratospheric chemistry (including changes to ozone and water vapor concentrations), a sufficiently spatially resolved stratosphere to capture important dynamic processes influencing aerosol transport and variability, and full coupling with land, ocean, and sea-ice models. While substantial uncertainties remain (47), these models have been validated against available observational data, including data from the period after the 1991 eruption of Mt. Pinatubo (48, 49).

Next Steps: Model-Focused Research. Building on this current modeling capability, the next phase of a mission-driven SA geoengineering research program should address all three of the research questions listed in *Research Goals*. Modeling can be used to help understand how different design choices lead to different climate impacts, which in turn can help define plausible strategies; these can then serve as a basis for providing the best current assessment of projected impacts. This information would be valuable today to provide a better assessment of whether SA geoengineering is likely to reduce climate risks and to support development of governance. In particular, the use of sophisticated climate models, which have only become available within the past few years, is critical for capturing nonlinearities and impacts of SA geoengineering that might be missed when using less-comprehensive models.

In addition to defining strategies and projecting impacts, one of the most critical research activities today is to assess uncertainty in model projections, including (see Fig. 2) both how uncertain the representation is of any specific physical process and what the sensitivity of simulated outcomes is to that uncertainty. Climate modeling informs both of these: Model intercomparison can provide better guidance than currently available on the former, while perturbed-physics simulations can address the latter. For example, do different models yield different conclusions for the aerosol size distribution under geoengineering, while still being consistent with observations after the Pinatubo eruption? If the parameters influencing the modeled size distribution are changed, does that significantly influence conclusions regarding SA geoengineering?

It is reasonable to expect that some uncertainties can only be adequately reduced with additional data from dedicated observational campaigns or experiments that introduce deliberate perturbations (43, 45, 46). However, it is currently unclear which uncertainties have the most potential to alter decisions about geoengineering, and hence are the most critical to be reduced through experiments. Thus, while some experiments may be justifiable today if the uncertainties they reduce are already known to significantly affect conclusions, the next phase of a well-designed and prioritized strategic research program into SA geoengineering can be expected to remain primarily model-based.

Reducing Process Uncertainty Through Experimentation. Assuming that nothing has been identified during the first model-based phase that would lead to stopping or redirecting further research, the second phase of a strategic research program will likely involve field research. There are three broad observations relevant to defining this stage of research.

First, and what differentiates a research strategy for SA geoengineering from MCB, is that unlike MCB (50) it is not possible to construct an SA geoengineering test that is "full-scale" in radiative forcing that is not also hemispheric or global. This is a result of the timescales involved (or more precisely, the fact that the aerosol lifetime in the stratosphere is much longer than the transport timescales). For MCB, a single experiment conducted over a small area but at deployment-scale radiative forcing can in principle simultaneously resolve many of the relevant cloud-aerosol uncertainties (50–52). The same is not true for SAs. Separately designed experiments may be required to address uncertainties in aerosol microphysics (particularly coagulation and condensation processes affecting particle size), stratospheric mixing, stratospheric chemistry (principally ozone), aerosol radiative heating and its effects on water vapor concentrations and on stratospheric circulation, the influence on cirrus, and so forth. Appropriately designed observations after a large volcanic eruption may be sufficient to resolve many of these uncertainties. The research plan cannot rely on there being a Pinatubo-scale eruption, but research should be prepared to take advantage of any natural analogue that does occur (53), and even relatively smaller eruptions may provide invaluable observational data that could significantly reduce the need for perturbative experiments.

Second is that the equations of motion describing large-scale circulation are well understood; the main source of uncertainty in climate modeling arises from modeling across scales, which necessarily involves the parameterization of sub-grid-cell processes. It is the aggregation of these uncertainties that leads to emergent uncertainty at larger scales. This large-scale uncertainty can be conceptually divided (similar to ref. 46) into uncertainty in stratospheric processes, which we discuss in *Research Versus Predeployment*, and the uncertainty in the resulting (tropospheric) climate response.

The third observation is that uncertainty in the climate response to solar geoengineering, such as regional precipitation changes, cannot be experimentally reduced without significant radiative forcing and time (54, 55); this would be true for either SAs or MCB. Indeed, it would be difficult to reliably quantify the regional climate response to solar geoengineering even during the initial decades of a gradually ramped-up deployment. Thus, there can be no responsible geoengineering experiment focused on reducing uncertainty in the climate response. Unlike Keith et al. (45) or Lenferna et al. (46), we do not include these tests in our Fig. 3.

Observational or perturbational atmospheric experiments might need to be repeated at different latitudes, altitudes, or times of year to capture a broader range of atmospheric conditions. However, experiments designed to reduce process uncertainties do not need to be larger than the model grid scale (presently less than ∼100 km) at which those processes are described. There is thus a natural scale separation between experiments to resolve sub-grid-cell process uncertainty and the large-scale/global consequences of this uncertainty in the stratosphere, with the latter being more appropriately framed as model validation. This natural gap is also evident in figure 1 of ref. 46.

Research Versus Predeployment

Uncertainties in the climate response to aerosol forcing cannot be resolved without imposing substantial radiative forcing, but validating model predictions of large-scale aerosol transport and aerosol microphysical growth would not require a large enough radiative forcing to yield a detectable surface climate response. For example, the eruption of Mt. Pinatubo in 1991 increased the SA optical depth by a factor of 60 above the ambient background levels (56) while cooling the climate by an order of 0.5°C or less

(28); an aerosol burden a factor of 20 to 30 smaller could lead to detectable aerosol properties but with no detectable impact on surface climate. Such a test would be useful to validate whether the models are properly representing aerosol changes or whether further process-level research is needed. [This is a refinement of the "albedo response test" category introduced by Lenferna et al. (46)]. Because the aerosol lifetime in the stratosphere is on the order of a year, validating aerosol transport predictions is at least hemispheric in scale.

We argue that such activities are better conceived of as operational tests (what we refer to in Fig. 3 as "predeployment"), validating models and the ability to detect aerosols through satellite observations, rather than as a final stage of research. While the goal of such tests would not be to actually affect the climate, they are fundamentally different in character from process-level research experiments, foremost because they represent model validation rather than uncertainty reduction, but also because they require a level of operational capability (both for aerosol delivery and for observations) more commensurate with deployment.

Furthermore, while the radiative forcing from such tests would be small, the decision to conduct them could be nearly as challenging from an international governance perspective as a deployment decision, due to both concern over a potential "slippery slope" toward deployment and the clearly transboundary nature of the test. Thus, one advantage of this delineation between research and operations (Fig. 3) is that it has the potential to simplify research governance by moving such issues into the realm of deployment governance, which must already manage transboundary issues.

There is little reason to engage in these global-scale tests unless there has already been a decision that deployment would follow if the observed results were sufficiently consistent with model projections. Of course, if such tests did reveal substantial differences between observations and model projections, that revelation would indicate the presence of fundamental uncertainties that would need to be resolved before any deployment of SA geoengineering.

Discussion

Limiting global mean temperature rise to the 1.5 or 2°C targets in the 2015 Paris Agreement without the use of solar geoengineering will require a near-immediate transformation of the global energy system on a massive and unprecedented scale, along with large-scale deployment of currently unproven "negative emissions" technologies (2, 57). Furthermore, the warming for a given increase in atmospheric CO_2 concentrations remains uncertain, as do the impacts that might result from a given warming (58). Since future emissions-reduction and negative emissions are not guaranteed to be sufficient to avoid significant climate impacts, we believe it is essential to also pursue research into solar geoengineering to understand whether and how it can contribute to reducing impacts. There is some urgency in conducting this research, as 1.5°C of global warming might be reached within 20 y (58); even an aggressive research effort to inform decisions could take longer than that.

A comprehensive, prioritized, mission-driven research effort should address three intertwined questions: how one might deploy SA geoengineering to meet climate goals, what the best estimate for the resulting impacts would be, and assessing and (where possible) reducing the risks posed by uncertainty. There are several observations worth highlighting:

i) Near-term research in SA geoengineering will likely continue to be primarily model-based, both to provide initial answers to these questions based on current knowledge and to identify and prioritize which field experimentation may be needed (including both perturbative and purely observational).

ii) We expect a need for field experimentation to reduce uncertainties, but we also expect that any perturbative experiments will always be at relatively small scale. Uncertainty in model projections arises primarily through sub-grid-cell parameterizations. We expect that experiments to resolve these uncertainties will not need to be larger than model grid resolution (~100 km), leading to a natural scale separation between these and hemispheric or global-scale validation tests.

iii) We suggest that it is more appropriate to conceive of any hemispheric or global-scale test, even at negligible radiative forcing, as a "predeployment" operational validation of model projections and observational capability, placing them as a follow-on from a global deployment decision rather than as part of the research activities needed to inform such a decision.

This framing of the overall research effort enables prioritization of essential near-term research. Clarification of the goals and characteristics of research is also needed to better define broader issues such as what governance is needed for geoengineering research, and what institutional design and research processes could be appropriate to execute this research. For example, the observations above suggest that a significant research program does not need to wait for the development of appropriate governance for field experiments, and further that the development of such governance may not need to address concerns across all spatial scales. Processes will need to be put in place to assess priorities, and there are challenging questions to address such as how to codevelop these priorities with different publics and ensure their input in which impacts and risks are assessed by research (59), or what research outcomes might justify stopping further research.

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