Title: When Are Small-Scale Field Experiments in Solar Geoengineering Worth Pursuing?

Authors: Ryan J. O'Loughlin[1,2] and Daniele Visioni[3,4]

[1]Philosophy Department, Queens College, City University of New York (CUNY), New York, USA
[2]Department of Earth and Environmental Sciences, CUNY Graduate Center, New York, USA
[3]Department of Earth and Atmospheric Sciences, Cornell University, Ithaca, NY, USA
[4]Atmospheric Chemistry, Observations, and Modeling Laboratory, NSF National Center for Atmospheric Research, Boulder, CO, USA

Correspondence to: Ryan J. O'Loughlin (ryan.oloughlin@qc.cuny.edu)

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Abstract

We propose a set of heuristics—scientific rigor, safety, usefulness, and transparency—for assessing the pursuitworthiness of small-scale field experiments in solar geoengineering research. Rather than offering a fixed logic of pursuit, we emphasize that these heuristics should operate as part of a dynamic and iterative evaluative process within the solar geoengineering research community, responsive to changing modeling priorities, new data, and shifting ethical and political landscapes. We argue that such experiments must be understood within the broader context of climate modeling research, where their primary role is to improve model components and identify further uncertainties. As debates about "moonshot" research and urgent science continue to evolve, our heuristics offer a way for the community, and for potential funders, to evaluate field experiments without abandoning the standards that guide responsible inquiry. Although our heuristics presuppose the pursuitworthiness of solar geoengineering research as a whole, they provide a structured framework for evaluating which field experiments are worth undertaking and why.

1. Introduction

The question of which scientific ideas are worth pursuing is a fundamental challenge in science, particularly in fields where the stakes are high, and resources are limited. When the research is also time-sensitive, then the challenge becomes even greater. Philosophers of science have analyzed the pursuitworthiness of science from multiple perspectives, on topics ranging from whether there is a logic of pursuit (Feyerabend 1975; Shaw 2022), whether scientific standards ought to be relaxed in times of "fast science" (Friedman and Šešelja 2023; Stegenga 2024) as well as the role of criticism in evaluating scientific pursuits (DiMarco and Khalifa 2022).

These philosophical questions are not merely abstract. They take on particular urgency in emerging areas of research where the scientific stakes are entangled with social, ethical, and political considerations. Against the backdrop of the ongoing climate crisis, solar geoengineering has emerged as a controversial but increasingly discussed avenue of research. A particularly pressing question is: *when is it worth pursuing small-scale field experiments to inform solar geoengineering research*? This is the question we pursue in this paper.

Solar geoengineering involves intentionally reflecting small amounts of sunlight back into space to cool the earth, and potentially reducing some risks arising from global warming. As of now, all research on solar geoengineering is done via climate models. However, climate models contain significant uncertainties—many of which are directly relevant to assessing the feasibility and risks of solar geoengineering. For example, in the case of stratospheric aerosol injection (SAI), which involves releasing sulfate particles or alternative aerosols into the stratosphere to reflect a portion of incoming sunlight, a key source of uncertainty lies in aerosol microphysics: how these particles form, evolve, interact with radiation, and affect cloud dynamics. While current climate models are informed by observations of the natural world (such as how aerosols emitted by volcanic eruptions evolve), such opportunistic experiments are not necessarily a one-to-one equivalent to a real SAI deployment. Therefore, small-scale field experiments have been proposed to reduce some of these uncertainties and improve confidence in the climate model projections.

But whether these field experiments are worth pursuing—that is, whether they are pursuitworthy—is far from straightforward. Solar geoengineering is highly controversial; many object to it on moral or political grounds. As a result, there is little hope of reaching widespread agreement on general criteria for pursuitworthiness in this domain. Indeed, people disagree in terms of their ethical commitments, arguing, e.g., that geoengineering is a moral hazard, is inherently or practically unjust, or, conversely, is morally obligatory (Gardiner 2011; Svoboda et al. 2011; Hale 2012; Preston 2013; Horton and Keith 2016; Morrow 2020; Whyte 2020). There's additional disagreement about the risk that research will automatically lead to deployment (the so-called "slippery slope", see Callies 2019; Andow 2023) and disagreements about different research institutions' priorities besides (e.g., see Tilmes et al. 2024). (See Flegal et al. 2019 for a recent review of some of these disagreements).

Even amongst those that agree in general about the need for more research, there are different opinions about how best to move that research away from the purely theoretical realm and into that of real-world testing. We therefore set aside the broader question of whether solar geoengineering research should be pursued at all, and instead examine the pursuitworthiness of field experiments within the context of ongoing solar geoengineering research. In other words, we assume a research context in which scientists are already committed to investigating the uncertainties, risks, and potential impacts of proposed solar geoengineering methods (perhaps in response to a call for a global assessment of solar geoengineering made by a governing body), but in which no agreement has yet been achieved over the necessity or prioritization of field experiments.

One of our central claims is that judgments about the pursuitworthiness of field experiments in solar geoengineering cannot be adequately made in isolation—they must be understood in the context of iterative climate model development. We argue that model uncertainties both shape the design of such experiments and determine their potential value. Judgments that field experiments will inevitably lead to full scale deployment often fail to recognize the foundational role played by climate models.

To assess pursuitworthiness in this setting, we look at how climate scientists themselves talk about prioritizing field experiments. Specifically, we build on Visioni et al.'s "living assessment" approach, proposing a dynamic and evolving set of heuristics rather than a fixed logic of pursuit. These heuristics include: assessment of scientific rigor, safety, usefulness, and transparency. These heuristics are informed by ongoing conversations within the solar geoengineering research community, including perspectives from those directly involve in field experiment planning and model development. We assess some limitations of our heuristics as well, most notably that they presuppose rather than show that the mission of solar geoengineering research—to provide a reliable scientific basis for future decisions about deployment—is itself pursuitworthy. We bring this last point into conversation with Shaw's (2022) discussion of moonshot research and urgent science.

The paper proceeds as follows. In Section 2, we provide background on solar geoengineering research, focusing in particular on stratospheric aerosol injection (SAI) and the uncertainties that surround it. In Section 3, we examine the role that field experiments could play in addressing these uncertainties, especially in the context of iterative model development. We also distinguish between scientific experiments and feasibility tests. Then, in Section 4, we present a framework for evaluating the pursuitworthiness of such experiments, drawing on recent proposals for "living assessments" in solar geoengineering research. In Section 5, we consider how the time-sensitive and politically contested nature of solar geoengineering complicates pursuitworthiness judgments. In Section 6, we conclude.

2. Solar geoengineering research: some background

As global temperatures continue to rise and the window for limiting warming to 1.5°C or even 2°C rapidly narrows, it is becoming increasingly unlikely that emissions reductions alone will be sufficient to meet international climate targets (IPCC 2023). Indeed, some prominent scientists believe we've already passed the 1.5°C threshold or will in the next few years (e.g., see Hansen et al. 2023). This state of affairs has led some scientists to research solar geoengineering, a type of climate intervention method which seeks to reflect a small portion of incoming solar radiation back into space to artificially cool the planet.

Two solar geoengineering methods have attracted the most sustained scientific attention: stratospheric aerosol injection (SAI) and marine cloud brightening. In this paper, we focus on SAI. The basic idea behind SAI is to inject sulfate aerosols—or their precursor gas sulfur dioxide (SO_2)–into the stratosphere, where they would disperse globally and reflect a fraction of incoming solar energy. This process mimics the natural cooling effects observed after major volcanic eruptions, such as Mount Pinatubo in 1991. Because aerosols gradually settle out of the stratosphere over time, any cooling effect would be temporary, requiring regular re-injection over time to maintain the cooling.

There is broad scientific agreement that SAI is technically feasible (Duffey et al. 2025). However, the exact outcomes of such a potential intervention remain deeply uncertain. To date, nearly all research on SAI has relied on climate model simulations. Therefore, the strengths and weaknesses of climate models bear directly on scientists' knowledge of SAI.

There are many different types of climate models but the state-of-the-art are generally called Earth System Models (ESMs). Variations of these models simulate large-scale patterns of atmospheric and oceanic circulation and are used in both weather forecasting and long-term climate projections. ESMs are run on supercomputers and encode mathematical representations of physical laws such as Navier–Stokes equations governing fluid dynamics. These fundamental equations describe how mass and energy move through the Earth's atmosphere, oceans, land, and ice systems. However, not all climate-relevant processes occur at scales that can be explicitly resolved by these models. To address this limitation, many physical processes are parameterized, i.e., ESMs implement idealizations or mini-models which represent the effect of small-scale processes “at the grid scale of the model” (Gettelman and Rood 2016, 46). Parameterizations come in varying degrees of complexity and often have empirical support or are derived from theory (Lloyd 2015; O’Loughlin and Li 2022).

ESMs often simulate climate as part of a coordinated research activity, known as the Coupled Model Intercomparison Project (CMIP) and its various subprojects. One of these subprojects is the Geoengineering Model Intercomparison Project (GeoMIP). GeoMIP has been going on since 2009. As with other modeling comparison projects, GeoMIP has revealed both robust conclusions and key uncertainties.

In a review of solar geoengineering research, climate scientists Kravitz and MacMartin (2020, 64) say that, based on modeling studies, if solar geoengineering were implemented it would be “virtually certain to reduce global mean temperature, offsetting, at least partially, changes associated with rising CO_2 concentrations.” Modeling studies also show that in a solar geoengineered climate “nearly all regions are predicted to experience a climate closer to the historical baseline” than they would in a warming world (Kravitz and MacMartin 2020, 64). One robust conclusion from models, for instance, involves the pattern of warming: multiple generations of models from different institutions all show that uniformly reducing the solar constant overcools the tropics and undercools the poles (Kravitz et al. 2021). This conclusion is robust in that the models all represent the same key (albeit idealized) causal process that leads to the same outcome despite some differences in model assumptions (O’Loughlin 2021). Similarly, different patterns of aerosols in the stratosphere (for example, uniform distribution over all latitudes, or all aerosols only in one hemisphere) produce similar patterns of cooling in different climate models (Visioni et al. 2023).

GeoMIP has also revealed many key uncertainties. Climate models diverge significantly on many outcomes of solar geoengineering—particularly at regional scales and in projecting side effects such as changes in precipitation (Ricke et al. 2023), stratospheric heating, or ozone chemistry (Tilmes et al. 2022). These divergences reflect not only differences in model inputs and scenarios (which are a source of uncertainty on their own, but not one that can be resolved through climate science analyses), but also deeper structural features of climate models themselves, i.e., parameterizations. In particular, processes such as aerosol nucleation, coagulation, stratospheric mixing, and aerosol–cloud interactions occur at scales far smaller than the model grid and are therefore modeled using empirical or theoretical idealizations. Because these parameterizations differ between models, and because many of the underlying processes remain poorly understood or are hard to observe, results often vary widely. Since the ultimate fate of the aerosols (i.e. where they are transported, which impacts how they reflect sunlight) is impacted by the aerosol size distribution, the small scale differences result in large scale divergence in model projections (Visioni et al. 2021). Might small-scale field experiments help resolve some of these uncertainties?

3. Proposed small-scale field experiments

In this section we specify which types of experiments count as small-scale field experiments for our purposes. There are a variety of experiments and other activities that could be done (or are being done) that relate to solar geoengineering (see https://srm360.org/outdoor-experiments/). However, given the scope of our analysis, we are focused on a subset of such activities.

As Kravitz and MacMartin (2020) note, our current understanding of SAI's effects is limited by the structure and assumptions of climate models. Reducing uncertainty will therefore require not only more simulations, but also better constraints on the parameterized processes that drive model disagreement. Such constraints can come from theoretical insight, observations, field experiments, or the removal of a need for parameterization if computational power increases. Not all parameterized processes can be resolved solely through small-scale observations. For example, models differ significantly in how they represent large-scale stratospheric transport (Dietmüller et al. 2018), which plays a crucial role in the behavior of aerosols from SAI. In such cases, improving model convergence will require a better understanding of the large-scale physical mechanisms involved, as well as increased horizontal and vertical resolution to more accurately capture transport processes.

Some recent reviews (e.g., Eastham et al. 2025; Haywood et al. 2025) of solar geoengineering research highlight major knowledge gaps and how to address them. For example, for SAI, some uncertainties about the behavior of microscopic processes stem from the lack of observation of sulfates in the stratosphere at scales that are much smaller than those from the large volcanic eruptions that are usually used as "proxies" for SAI climatic impacts. Because it is difficult to replicate stratospheric conditions in the lab, some outdoor experiments have been discussed and, in some cases, proposed. Ideas have ranged from balloon-borne releases of a few hundred grams of alumina to study its effects on atmospheric chemistry, to short-duration tethered balloon experiments designed to study plume dispersion or sulfate particle formation in the lower stratosphere.

These types of outdoor experiments are the small-scale field experiments we focus on. Proposals for such experiments generally involve the controlled and monitored release of some quantities of a sulfate or its precursor, and would be "conducted on the smallest possible length and timescales required to validate, with statistical confidence, that the approaches being tested can affect the parameters under investigation" (Symes 2024, 6). Our definition of a small-scale field experiment here strictly considers only those scientific experiments whose main aim is to reduce specific climate modeling uncertainties, and also whose findings can be used to determine what further research needs to be done, which could involve prioritizing additional small-scale field experiments.

Therefore, our question (*when is it worth pursuing small-scale field experiments?)* is framed squarely within the context of climate modeling research. To be clear, most large-scale climatic uncertainties can't be solved or reduced with small-scale experiments. So, these experiments must be understood as part of an iterative process of climate model improvement. For example, our confidence in how SAI could affect the monsoons is a question that has fundamentally to do with our understanding of the climatic processes as represented in climate models. If SAI were implemented, then it would affect global climate, and climate models are the best tools we have for researching global climate scenarios. In the near term, though, small-scale experiments can help scientists gain knowledge to improve their models, identify new areas of uncertainty, propose additional small-scale experiments, and so on, to ultimately provide reliable information for SAI decision-making.

One may be tempted to ask about the pursuitworthiness of these small-scale field experiments in the abstract, i.e., without thinking about the climate modeling research framework, climate change itself, or the political context surrounding it. However, in our view it's not really possible to evaluate the pursuitworthiness of small-scale atmospheric experiments in the abstract. While there is interesting scientific knowledge that can be gained about small-scale processes (knowledge for knowledge's sake), as a practical matter framing the gained knowledge in the broader context of providing robust modeling assessments avoids the pretense that this knowledge is being produced in a vacuum that is disconnected from climate change, climate mitigation, and climate interventions research. (We return to this point in section 4.2 when we discuss transparency of intent, and in section 5 when we talk about the time sensitivity of SAI research).

Some public critiques of small-scale experiments appear to overlook the central role of climate modeling in solar geoengineering research, often due to ethical or political concerns. For example, in a 2025 letter to *The Guardian*, climate scientists Raymond Pierrehumbert and Michael Mann write:

> The [UK's Advanced Research and Invention Agency] ARIA programme thesis document on 'cooling the Earth' makes for chilling reading. The project goes all-in on the supposed need for field trials, without making a case that such trials could answer any of the really important questions about what would happen with a sustained global-scale deployment. That the trials are described as 'small scale' is little comfort, because even small-scale trials risk developing the technology somebody else (think Musk, Trump or Putin) might use for a large-scale deployment.

While these concerns are ethically and politically salient, they risk obscuring the narrower scientific rationale for certain small-scale experiments, namely, that they can help resolve specific modeling uncertainties within a broader, iterative research framework. We see no problem with critiquing solar geoengineering research on ethical grounds, but we think the critiques should not mischaracterize the research or its aims.

We are not concerned with determining the pursuitworthiness of other activities concerning solar geoengineering. For instance, there are questions of *feasibility* that could be explored through outdoor testing and that do not reduce modeling uncertainties directly, but instead would inform what a deployment would realistically look like. We regard these as feasibility tests, not experiments. For example, testing the ceiling of a plane to understand whether a higher altitude of SAI is achievable does not reduce climatic model uncertainties, but it does inform the feasibility of a specific simulated strategy. Admittedly, the boundaries between feasibility tests and scientific experiments are sometimes fuzzy. For example: for marine cloud brightening, whether a nozzle can spray sea salt aerosols at the size that is theoretically more conductive to brightening is a feasibility test. However, if a specific size is unachievable, this may inform the size distribution of aerosols simulated in a model (see Wood 2021) and so it may count as an experiment.

We do not claim that feasibility tests would not be useful per se, but simply that their justification can't be found in the pursuitworthiness criteria we will outline in section 4 below. Separating scientific experiments from feasibility tests and only discussing the pursuitworthiness of the former also helps us avoid discussing potential "lock-in" risks that may relate to the latter but not to the former (Royal Society 2009).[1] Feasibility tests might create the impression that SAI is definitely going to happen and we need only figure out how to implement it. In contrast, we are assuming a framework in which scientists are committed to

[1] However, one may still worry about "cognitive lock-in" (Cairns 2014) and other types of slippery slopes (Tang 2023).

providing a reliable scientific backing to inform future decisions about deploying or not deploying solar geoengineering.

One final comment. There are also activities that are neither an experiment nor a feasibility test but might mistakenly, and unfortunately, be thought of as connected to SAI research. E.g., the Make Sunsets company which allows customers to buy “cooling credits”, i.e., a small biodegradable balloon filled with hydrogen gas and sulfur dioxide that the company releases into the atmosphere (see here: https://makesunsets.com/). Such activities are not an experiment because no data is collected and no observations are made. Indeed, it does not appear to be connected to scientific research of any kind. It is a start-up business that aims to intervene in the climate. (As a side note: these activities do not satisfy 3 of the 4 pursuitworthiness criteria we outline in section 4.2 below. Make Sunsets’ interventions are neither scientifically useful nor scientifically rigorous, and the decision to release each balloon (and where) are not the result of a community-based decision.).

4. A Living Assessment

So, given the research context, which for now is largely model-driven, how shall we determine when to pursue specific field experiments?

Let us start from the assumption that the point of broader solar geoengineering research is to provide a reliable scientific backing to inform future decisions about deploying or not deploying solar geoengineering. That is, we will assume that there could be a future point at which increasing risks from climate change, such as an early warning system reporting the risk of approaching a climatic tipping point in a certain amount of time unless further warming is halted or current warming is reduced, would force governments to consider solar geoengineering. In such a case, a hypothetical government could decide not to deploy solar geoengineering if i) its natural risks are overall deemed greater than the risks of tipping points or ii) public perception about such or other risks (Beckage et al. 2025), however unfounded, renders a government-led operation deeply unpopular. For the latter, a recent illustrative example is the widespread general electoral losses suffered by governments in charge during the COVID-19 pandemic, especially where strict measures were implemented (Su and Rashkova 2024). In this sense, a “mandated” intervention to redress climate change might be perceived badly (Bardosh et al. 2022), especially if accompanied by a low trust environment around solar geoengineering (e.g., see Baum et al. 2024; see also Adhikari et al. 2022 for a review of research on trust in vaccines).

4.1 Risk Register

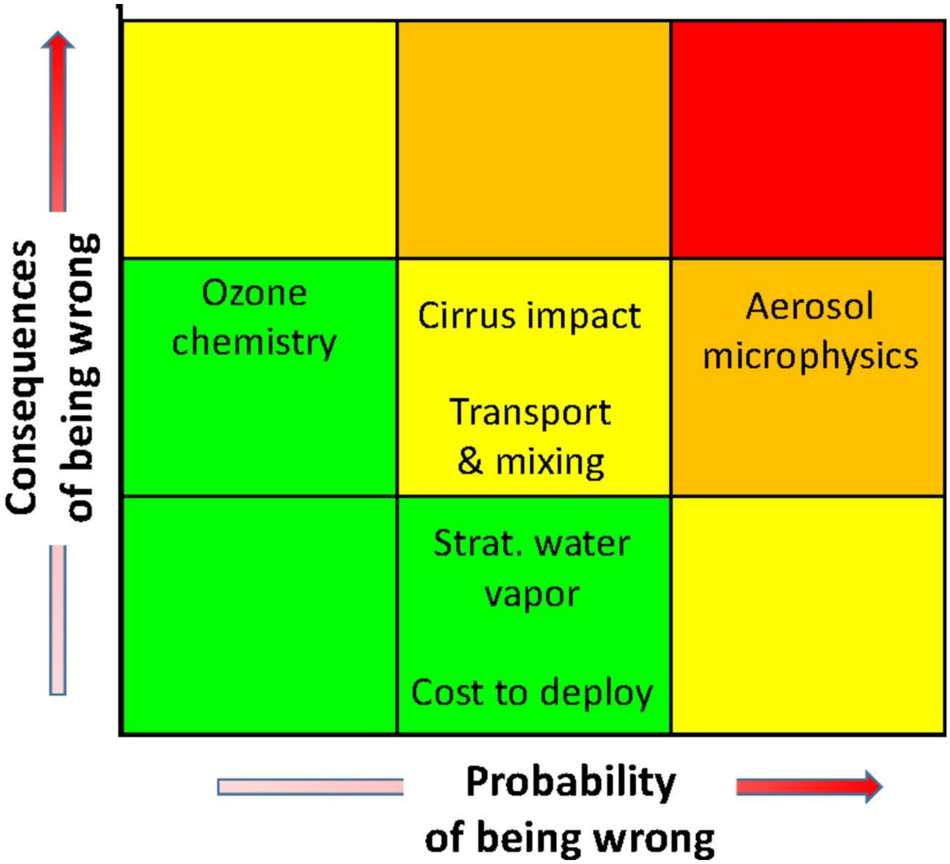

Figure 1. From "Mission-driven research for stratospheric aerosol geoengineering" (MacMartin and Kravitz, 2019, Proceedings of the National Academy of Sciences of the United States of America 116 (4): 1089–94. https://doi.org/10.1073/pnas.1811022116). Note that this figure was done with only sulfate-based SAI in mind, whereas we expand to multiple potential materials (see section 4.2 below).

With this basis, we can identify two approaches that would help determine the pursuitworthiness of a given field experiment that could take into account both the need for more robust scientific information about solar geoengineering and the potential for increased transparency to build trust with the public (Petersen et al. 2021). Proposals for field experiments should be explicit and upfront about risks and about the potential for experiments to improve scientists' and public understanding.

The first of the two approaches is a "risk register." A risk register is a management tool in which the risks of a specific endeavor are compiled and classified in terms of their probability of happening, potential impact if they happened, mitigation measures, contingency plans in case they happen, and other additional information about the various risks. They are routinely used by many projects and companies. MacMartin and Kravitz (2019) proposed a risk register framework in the context of what they call “mission driven research” for SAI. In this context, they note that their figure (see figure 1 above) is hypothetical, at best, as current theoretical research was not extensive enough to allow one to populate an actual register.

In the MacMartin and Kravitz register, specific risks or uncertainties of SAI were ranked across two axes: the probability of being wrong about a specific aspect of SAI (i.e., its uncertainty based on current knowledge) and the consequences of being wrong. An aspect of SAI that is highly uncertain, and where the consequences of being wrong are also high (top right corner), is to be prioritized more than an aspect of SAI that is not uncertain, or whose consequences would not be high anyway. For instance, in Fig. 1 “Aerosol microphysics” is ranked as having a high probability of being wrong, while the consequence of being wrong is medium. Specifically, aerosol microphysical processes involving nucleation, coagulation and condensation which in turn affect particle size, are not well understood, in their mutual interaction, in the stratosphere. So, scientists do not know how to best represent aerosol microphysics in climate models in a way that is both accurate and computationally inexpensive. The consequences of this lack of knowledge include inaccurate estimates of forcing (i.e., how much aerosols affect the energy balance of the planet) as well as heating and water vapor effects on the stratosphere (Tilmes et al. 2022). MacMartin and Kravitz (2019) rank the consequences of this uncertainty as “medium” though they acknowledge that this is a qualitative and potentially revisable judgment.

Since the register in Fig. 1 is intended to illustrate the broader conceptual framework rather than offer definitive rankings, these labels can be seen as provisional. For instance, while aerosol microphysics currently occupies the top spot among known uncertainties in SAI, new and potentially larger uncertainties could emerge. To accommodate this, the top-right corner of the register was intentionally left open to signal that the framework is designed to evolve with future knowledge. (Kravitz and MacMartin (2020) rank aerosol microphysics as having both a “high” probability of being wrong and a “high” consequence of being wrong.)

Assuming such a register could be actually populated and agreed upon (something that would require its own theoretical research effort and possibly also a governing body or decision-making framework), such a register would offer an evaluation tool to help determine the pursuitworthiness of a field experiment.

However, we should be clear that the register is just an initial step to determine priorities, rather than an attempt to capture the entirety of the picture related to uncertainties and risks, otherwise we risk falling prey to the “Illusion of control” (Drummond 2011; Budzier 2011). That is, as Drummond (2011) discusses, we risk mistakenly treating the risk register as a perfect representation of the world that captures every possible uncertainty and unknown. A risk register does not do this. Ultimately, a risk register is a way to model your priorities, but it is only based on available knowledge which is incomplete and imperfect, as are estimates of uncertainty. All models are wrong, so all risk registers are wrong. But some are useful (Box 1976).

We believe that a risk register offers a valuable starting point for evaluating pursuitworthiness. In the next section, we situate it within a broader and more complete evaluative framework.

4.2 Heuristics to Judge Pursuitworthiness

Let's think through three potentially proposed[2] field experiments based on how they would be evaluated using the risk register alone, and to highlight further questions that arise in assessing their pursuitworthiness. The three experiments are:

- (**top right corner**) a very small-scale experiment that aims to measure reaction rates at the interface of a novel non-sulfate-based compound, such as calcium carbonate or alumina. The idea would be to conduct the experiment and use its results to improve a climate model chemistry module to simulate ozone reaction rate. Currently, due to the lack of observations, this is a large uncertainty, with a high risk of bad consequences in case ozone depletion is large (Vattioni et al. 2023). Therefore, based solely on the risk register in light of our current knowledge, this experiment appears to be pursuitworthy. However, there are other crucial factors to consider, for example: (1) whether there is a clear explanation of why the non-sulfate compound is being chosen, based on evaluations of efficacy or safety; (2) whether there are serious reasons to consider laboratory chamber experiments untrustworthy, or largely uncertain, due to an inability to reproduce actual atmospheric conditions; (3) whether there are reasons to believe that climate modelers actually would model solar geoengineering scenarios using this non-sulfate-based compound, i.e., that the knowledge gained from the experiment would be taken up and productively used by scientists.

- (**bottom left corner**) an experiment in which marine clouds are seeded from above with sulfate aerosols to emulate the potential impact on cloud coverage of SAI aerosols settling down from the stratosphere. Modeling suggests that such a seeding effect could change the overall radiative efficacy of cloud forcing (i.e., (Lee et al. 2023; Gristey and Feingold 2025) by a small amount. Note that our knowledge about this sensitivity could be improved through direct observations of aerosol-cloud interactions after small volcanic eruptions (Peace et al. 2024). There are no indications that the experiment would materially affect our knowledge of the overall outcomes of SAI, but it could change estimates of the overall amount of sulfate needed to cool by a certain amount. Based on the risk register alone, then, the experiment should not be deemed pursuitworthy. If another determination had been made, i.e., if this had been placed up

[2] The first example is largely based on the Stratospheric Controlled Perturbation Experiment (SCoPEx) (https://www.keutschgroup.com/scopex). The other two haven't been concretely proposed but are experiments that solar geoengineering scientists have discussed (e.g., see Eastham et al. 2025).

and to the right in the risk register, there would still be other factors that would need to be considered. For example, whether the researchers obtained consent from the local community and whether they clearly communicated the purpose and expected outcomes of the experiment during the approval process. Indeed, a recent marine cloud brightening experiment off the coast of California was shut down due, according to critics and reporters, to a lack of transparency and because the research team did not engage with the community (Flavelle 2024; Jinnah et al. 2024). It is possible that a public framing of such an experiment within an agreed upon risk registry decided beforehand could have helped with its public acceptability.

- (**top left corner**) a field experiment that would release 1000 kg of SO_2 in the polar stratosphere during March using three different technical setups (a reminder to the reader that this proposal is hypothetical). We suppose here that some preliminary engineering research indicates that the specifics of the release of compressed SO_2 gas from an aircraft in the stratosphere will heavily affect the feasibility of SAI, and that some details (the temperature of the released mixture affecting its evaporation, the turbulence on the wake of the aircraft) are demonstrated to be crucial to actually result in plume spreading. We further suppose that multiple peer-reviewed publications conclude that understanding these details is much more important than previously thought to determine the overall feasibility of SAI. The proposed field experiment would address these uncertainties. From the risk register alone, since the consequences of being wrong are "high", this experiment should be deemed pursuitworthy. However, additional considerations are important. For example, will the experiment's results be productively used in future research? For this experiment, the answer would be a clear "yes". The outcome of the experiment, together with understanding technical feasibility, is also designed to inform small-scale mixing parameterization in climate models, improving projections of efficacy and helping constrain the overall sulfate amount that would be needed to cool the planet by 1ºC. Another question: is the experiment safe? Here, again, the answer is "yes" because the proposed quantities would be small relative to some present sources: the Holuhraun eruption in Iceland in 2014 released (in the troposphere) over 100,000 kg of sulfate in a few hours. Finally, scientists must engage with the relevant community or decision-making bodies to obtain approval. This experiment may face public or political resistance for the same reasons that make it scientifically valuable: it is of a size that, independently from comparisons with other sources, might be considered "large", and would be perceived by many as a field trial for an eventual deployment. While the experiment would yield critical scientific and technical information, it may also be seen as a step onto a slippery slope toward deployment.

What should be clear from these examples is that the risk register itself is a necessary, but not sufficient, condition to determine pursuitworthiness. Aside from the limitations of risk registers mentioned in section 4.1 above, we also remind readers that we are envisioning this process at a time where there is no agreement over potential large-scale deployments, so outdoor experiments must also be designed to enhance public trust and meaningful engagement. Other relevant factors concern whether the experiment is safe and whether its results would be taken up and used in climate modeling studies. Thus, it is important that the pursuitworthiness of field experiments are also evaluated following criteria similar to that suggested in Visioni et al. (2024), criteria which involve safety, usefulness, and transparency.

Therefore, we propose that four guiding criteria should be used when considering research pursuitworthiness:

1) **Scientific rigor.** Experiments need to be well designed, as determined by a rigorous process of scientific peer-review. While this criterion may seem completely obvious, it is worth including in case researchers or decision makers are tempted to suggest that we lower the bar for rigor to speed up the research process and defend against climate impacts "sooner rather than later" (so to speak). Such thinking is ill-advised. Even if we regard SAI research as "fast" science (see section 5 below), we agree with Stegenga (2024) that scientific norms–the "reliability enhancing features" of routine science–should still be sought to the fullest extent feasible.
2) **Safety.** Experiments must be deemed safe, environmentally, through a process that should be not dissimilar to other environmental assessments. Clearly, a definition of "safety" will itself depend on specific values and tradeoffs (Oreskes 2004). E.g., in the US, Environmental Protection Agency regulations about specific pollutants usually become more stringent with time, but this has changed with the most recent Trump administration. Similarly, procedural norms are sometimes sidestepped in case of national emergencies (Edgell et al. 2021), with all the risks that that entails (Whyte 2021). In this case, however, we are restricting ourselves to a present in which such an emergency is not yet felt. At least, such an emergency is not yet felt to such a degree that any government is seriously considering deploying SAI in the near term. Instead, we are asking: *should we pursue field experiments <u>now</u> to help inform future decisions about SAI deployment?* Given current circumstances, the safety of geoengineering field experiments should not be considered differently from other experiments. However, balancing trust-building through participatory processes (Christopher et al. 2008), which help assess safety, may take on added importance in the context of SAI research.
3) **Utility.** The experiment must be deemed sufficiently useful. Here, two broad considerations are relevant. Evaluating a proposed experiment for usefulness should involve:
    a) justifying the experiment in terms of the risk register, identifying the uncertainty in it, explaining the potential to move it left/down.

b) probability of uptake – how likely it is that the knowledge gained from the experiment would be put to use, e.g., via a parameterization update or revision of a modeling assumption or newly designed experiment to address a newly discovered uncertainty. There is also an institutional aspect to consider. The utility of new knowledge depends on where a given field experiment falls within research timelines, funding priorities, and infrastructural readiness. For instance, climate modeling intercomparison projects have fixed simulation schedules, and research that feeds into those cycles is more likely to have tangible influence (Touzé-Peiffer et al. 2020). Thus, an experiment's pursuitworthiness can hinge not only on what knowledge it yields, but also on whether the institutional context allows that knowledge to be acted upon in time.

4) **Transparency.** The experiment design and its outcomes are fully transparent, and its planning involved sufficient engagement with the relevant communities. Such transparency should also include transparency of intent: the fact that the experiment is being performed in order to improve simulations of solar geoengineering is a fundamental step to ensure trust. This transparency can also alleviate worries (no, the experiment is not pre-deployment or on a slippery slope to it; rather, the experiment is to improve the modeling). Community engagement may also involve coming to an agreement about what is useful (e.g., based on the community's views of climate harms as featured in the risk register).

In general, field experiments that are determined to be sufficiently scientifically rigorous, safe, useful, and transparent should be deemed pursuitworthy. This framework could be also used to judge if one proposal is more worth funding than another (in the context of limited funding), assuming multiple proposals clear all predetermined thresholds on all four criteria, but here we are outlining these criteria with the idea of judging their overall pursuitworthiness individually. Critiques of proposals should speak directly to our criteria and should be precise, e.g., by being contrastive (see DiMarco and Khalifa 2022).

We should note that the relative importance of each criterion can be disputed, and there can be disagreement about whether a given experiment sufficiently satisfies each of these criteria, so we see our criteria as heuristics rather than a strict logic of pursuit. The notion of heuristics we have in mind is reminiscent of Longino's (2008) discussion of the values that guide theory choice. Longino says that "Heuristics come into play earlier in research, when one is trying to formulate models or make choices among directions to pursue" (2008, 79). Longino further says that the notion of "heuristics" rather than "traditional values" pushes back against the idea that there's only one set or one way of judging a theory. For our purposes this means that other workable criteria could be theorized; what we're presenting here is an approach that we believe best reflects the broad solar geoengineering scientific community's current values, aims, and standards (Talati et al. 2025; Visioni et al. 2023; Táíwò and Talati 2021; Whyte 2020; Rahman et al. 2018; American Geophysical Union. Ethical Framework Principles for Climate Intervention Research 2024).

As ChoGlueck and Lloyd (2023, 16) describe it, "a heuristic is an *active* framework—at least partially subject to community-wide empirical evaluation—held by a community for building models that answer their research questions, not merely a passive set of personal beliefs or idiosyncratic schemes." The community's questions will likely shift as research progresses and as the community itself evolves. The heuristics that guide pursuitworthiness may have to evolve as well.

It's perhaps worth emphasizing this point further: we are not presenting a silver bullet solution to determining pursuitworthiness. Indeed, the four guiding criteria can still lead us astray – peer review can fail us, estimates of safety can be flawed, and the determination of usefulness is vulnerable to criticism such as Feyerabend's, i.e., that we would need to know the results of research ahead of time to *really* know whether it had been worth doing (e.g., see Feyerabend 1981 and discussion in Shaw 2022). In general, these criteria say nothing of the unconceived alternative lines of inquiry that were never pursued but may have been very useful had they been pursued (Stanford 2010). Perhaps there's a more scientifically rigorous, safer, more useful, and more transparent field experiment that we simply haven't thought of. Finally, when it comes to transparency and public engagement, there can be deep disagreement between scientists and a given community, so a project deemed overwhelmingly pursuitworthy by some may nonetheless be shut down or rejected.

Nevertheless, we propose that these criteria can serve as heuristics to determine pursuitworthiness and that they function best if they operate as part of a dynamic evaluative process. Scientific knowledge evolves, and no assessment of SAI research is ever final. As new data from already-pursued experiments come in, or as modeling priorities shift, our understanding of what is "useful" or even "safe" may evolve. For example, an experiment initially thought to reduce uncertainty may end up complicating model projections or revealing additional unknowns—moving a risk *up* and to the *right* in the register rather than down and to the left. Such is the nature of science–you can't predict exactly what the outcomes will be, if you could, then you wouldn't need to do the research anyway (recall Feyerabend). Indeed, it is notoriously difficult to determine how a climate model projection will change when a single component is updated or added (Lenhard and Winsberg 2010; O'Loughlin 2023). This underscores the value of comparing model outputs before and after updates, both to identify priority areas for future modeling and to inform the pursuitworthiness of targeted field experiments. For these reasons, we echo Visioni et al.'s (2024) call for a "living assessment"—one that evolves as new data, questions, and priorities emerge. Within such a framework, our proposed heuristics can help determine which field experiments are worth pursuing.

5. The time-sensitivity of solar geoengineering research

Pursuitworthiness judgments do not unfold in a vacuum. They evolve with the perceived urgency and stakes of the research.

In 2006 the nobel prize winning chemist Paul Crutzen famously suggested that scientists might explore “artificially enhancing earth’s albedo and thereby cooling climate by adding sunlight reflecting aerosol in the stratosphere…[to help] counteract the climate forcing of growing CO_2 emissions” (Crutzen 2006, 212). At the time, or perhaps a few decades earlier when climatologist Mikhail Budyko suggested the idea (Budyko 1974), SAI research could have been viewed as a case of luxury science. As Shaw describes it, luxury science “has no expected timeline for returning particular results” and during luxury science our decision to pursue one research direction or another is “based on nothing but a free choice” (2022, 108). In Feyerabend’s words, “anything goes”.

Shaw (2022) contrasts luxury science with urgent science. A research proposal is urgent just in case “there is a practical or moral reason to need a result within a specified timeline and the research can realistically be carried out within that timeframe” (2022, 108). We can view luxury and urgent science as falling along a spectrum. The growing threat of climate impacts and the continued failure of world leaders to address climate change arguably pushes SAI research closer and closer to the “urgent science” side of the spectrum.

However, the urgency of SAI research is itself debatable. On the one hand there are moral arguments against SAI. E.g., if you believe that having SAI as an available tool will lead to moral corruption (e.g., see Gardiner 2020) then you would likely disagree that any particular SAI research result is needed in a timely manner (or ever, for that matter). On the other hand, there are disagreements about the feasibility of timely climate mitigation.[3] To complicate matters, such disagreements are not always outright stated: proponents and opponents of SAI research alike tend to make inaccurate or inconsistent *assumptions* about the political feasibility of both climate mitigation and potential SAI governance (Clark 2023). Even amongst SAI researchers there can be disagreement over urgency.

We might instead regard SAI research as a case of moonshot research. As Shaw puts it, “Moonshot research programs contain an overarching *telos* which is intentionally vague allowing for a great amount of latitude for how that *telos* should be interpreted and achieved (2022, 108).” Solar geoengineering research does seem to exemplify moonshot research, also known as mission-driven research. Indeed, the mission of solar geoengineering research can be described as follows:

MISSION: to provide a reliable scientific backing to inform future decisions about deploying or not deploying solar geoengineering.

[3] Compare the optimism of Hannah Ritchie's "Not the End of the World" (2024, Ch. 3) to the pessimism articulated by a leading voice in climate science, scientist and former NASA GISS director James Hansen (Hansen et al. 2025).

Of course, how this research mission plays out differs across organizations. E.g., GeoMIP aims to inform the scientific community, policy makers, and the public based on analyses of climate model consensus and disagreement. So GeoMIP is entirely model-focused. On the other hand, the U.K.'s Advanced Research and Invention Agency (ARIA) aims to fund research that helps answer the "most critical technical and fundamental questions on the practicality, measurability, controllability, and likely (side-)effects of approaches that might one day be used to actively cool the Earth" (ARIA 2024, 5). ARIA is open to funding field experiments if certain conditions are met (conditions pertaining to scale, safety, whether the information sought could be achieved by other means, and more; see ARIA CFP pp. 8-11). However, these experiments will, should they be funded, aim to improve physical understanding that will feed into climate models.

Within the context of mission-driven research there is still ample room for disagreement which creates challenges for determining pursuitworthiness. In a recent correspondence published in *Nature Climate Change*, Mike Hulme argues that the moonshot model of scientific research (specifically as pertains to funding) is inappropriate for climate change research:

> …climate change is not well framed as a 'crisis' or 'emergency' that demands 'moonshot' technologies; it is not like an approaching asteroid, as allegorized, for example, in the movie *Don't Look Up*. Climate change will not be arrested, nor its challenges managed, through one-off breakthrough technologies that ARIA has been designed to incubate.
>
> …Rather, climate change should be understood as an emergent risk embedded in long-run socio–cultural–technological systems. (2025, 339)
>
> …Allocating very large amounts of money to researching single-shot techno-scientific solutions to climate change, as done in ARIA, misreads the nature of the climate challenge and offers a false prospectus for research. What society needs more is the integrated, interdisciplinary and incremental research facilitated by the small steps funding model. (2025, 340)

Hulme raises several legitimate criticisms, e.g., that ARIA wields more autonomy than is typical for public funded research and cannot be subjected to freedom of information requests. (Therefore, to satisfy our heuristic of transparency, ARIA should be transparent about the funded research projects even if it is not required to by the FoI).[4] But in the above quote Hulme also mischaracterizes solar geoengineering research: it's not "single-shot" and no solar geoengineering scientist thinks it is a "solution" to climate change. At the same time, we can also see that Hulme simply disagrees with the *telos* in question. That is, Hulme is opposed to

[4] Note that ARIA has published a list and info about all funded proposals even though they weren't required to. See here: https://www.aria.org.uk/opportunity-spaces/future-proofing-our-climate-and-weather/exploring-climate-cooling

solar geoengineering research altogether.[5] Determining pursuitworthiness within a moonshot research program does not tell us anything at all about whether the research program is worth pursuing to begin with.

Coming from another direction, Morrow (2020) urges solar geoengineering research to become *more* of a mission-driven effort. Whereas Hulme critiques the moonshot ethos as incompatible with climate science, Morrow (2020, 635) argues that

> Creating an international, information-oriented, mission-driven research program on solar geoengineering could provide several benefits over the current, investigator-driven framework. First, it would provide a more effective way to identify and answer the questions that policymakers would need to answer to make wise, responsible decisions about solar geoengineering. Second, it would improve the efficiency, effectiveness, legitimacy, and justice of research governance. Third, it would reduce the tendency for solar geoengineering research to exacerbate international domination.

The discrepancy between Hulme's and Morrow's respective views reinforces a broader point: determining pursuitworthiness within a mission-driven research program assumes the mission itself is acceptable. However, that assumption is often precisely what is contested. The heuristics developed in Section 4.2 presuppose acceptance of the mission; they are not themselves arguments for adopting it.

Finally, let us consider whether the urgency surrounding SAI might someday justify relaxing the standards proposed in our heuristics.

Presumably what is considered "useful" would change. To take an extreme case: if a climate tipping point were found to be approaching so rapidly that there was no time to improve models or conduct coordinated simulations, then many field experiments currently seen as valuable would lose their practical relevance.[6] A risk register is of little help if every item on it requires more time to address than scientists actually have.

But might we also relax scientific and safety standards? Would scientists be justified in engaging *less* with the public so they can speed up research?

Let's assume that at least some standards might be relaxed. This could be worrisome. One way to help assuage these worries is to ensure fair representation in the scientific (and relevant SAI research decision making) community. In section 4.2 above we mentioned that our proposed heuristics best represent what we take to be the interests of the SAI research community. But as time progresses and climate change continues to deal severe impacts, it will become

[5] See the Solar Geoengineering Non-Use Agreement website and blog here: https://www.solargeoeng.org/

[6] In an extremely time-limited case, perhaps machine learning (ML) could be of use (de Burgh-Day and Leeuwenburg 2023), however there are severe limitations to using ML to project novel climates (e.g., see Li 2023).

increasingly important to broaden the community. Indeed, a recent United Nations report on solar geoengineering says the relevant community is "everyone on earth" (United Nations Environment Programme 2023, 2023). Therefore, it is imperative to support endeavors like the DEGREES initiative (https://www.degrees.ngo) which aims to fund researchers in the Global South to assess specific potential impacts of solar geoengineering. Including more non-scientists through developing solar geoengineering governance efforts is crucial as well (American Geophysical Union. Ethical Framework Principles for Climate Intervention Research 2024). These activities are important in their own right, here and now, to increase the representation of laypeople, marginalized and climate-vulnerable communities, and, indeed, everyone on earth. But such activities may take on added importance as climate change impacts worsen and the sense of urgency increases.

6. Conclusion

In this paper, we proposed a set of heuristics—scientific rigor, safety, usefulness, and transparency—for assessing the pursuitworthiness of small-scale field experiments in solar geoengineering research. Rather than offering a fixed logic of pursuit, we emphasized that these heuristics should operate as part of a dynamic and iterative evaluative process within the solar geoengineering research community, responsive to changing modeling priorities, new data, and shifting ethical and political landscapes. We argued that such experiments must be understood within the broader context of climate modeling research, where their primary role is to improve model components and identify further uncertainties. As debates about "moonshot" research and urgent science continue to evolve, our heuristics offer a way for the community, and for potential funders, to evaluate field experiments without abandoning the standards that guide responsible inquiry. Although our heuristics presuppose the pursuitworthiness of solar geoengineering research as a whole, they provide a structured framework for evaluating which field experiments are worth undertaking and why.

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