



When are small-scale field experiments in solar geoengineering worth pursuing?

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Abstract

We propose a set of heuristics—scientific rigor, safety, usefulness, engagement, 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. Our analysis assumes, rather than argues for, the pursuitworthiness of solar geoengineering research as a whole. However, we conclude this work by revisiting the overarching question of whether solar geoengineering research itself is worth pursuing; we argue that accounts that focus on how research governance can reduce bias and build stable scientific norms offer a more promising route than appeals to urgency or “moonshot” missions. Overall, we provide a structured framework, situated within scientific practice, that can help determine which field experiments are worth undertaking and why.

Keywords Pursuitworthiness · Heuristics · Solar geoengineering · Climate modeling · Fast science

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 (Feyera-

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bend, 1975; Shaw, 2022), whether scientific standards ought to be relaxed in times of “fast science” (Friedman & Šešelja, 2023; Stegenga, 2024) as well as the role of criticism in evaluating scientific pursuits (DiMarco & 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. 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. Importantly, research on solar geoengineering is in principle distinct from any future decision about deployment: one can conduct scientific research on this topic without thereby endorsing its eventual use.

Setting aside, for the moment, the question of whether solar geoengineering should ever be deployed, it is worth noting that disagreement extends to the pursuit of research itself and to the relationship between research and deployment. 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 & 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 review of some of these disagreements).

Despite these disagreements, most observers accept the legitimacy of theoretical research, which has continued for more than two decades. To date, nearly all research into solar geoengineering has been conducted “in silico”, using climate models that, unavoidably, 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, there have been some suggestions that small-scale experiments might be useful to reduce some of these uncertainties and improve confidence in the climate model projections. For example, the National Academies of Science, Engineering, and Medicine “Reflecting Sunlight” report (2021) seems to support them:

Recognizing both the philosophical/ethical and the technical/scientific dimensions of this issue, it is the committee’s judgment that, subject to appropriate governance and oversight, outdoor experimentation could feasibly be pursued in a balanced manner that is sufficient in scale to acquire critical observations not available by other means.

It is this segment of the scientific community that we want to speak to (i.e., that we want to identify heuristics of pursuit for). We want to start developing the conceptual tools that would allow one (scientist, public, or policymaker) to understand if, given ongoing solar geoengineering research, a proposed outdoor experiment should move forward or not. Our guiding question is, *when is it worth pursuing small-scale field experiments to inform solar geoengineering research?* Indeed, there is currently a lack of agreed upon standards to evaluate experiments in the solar geoengineering research community.

By contrast, when it comes to the general question of whether *any* research into solar geoengineering is pursuitworthy, we believe there is no general logic or guiding principles of pursuit that can deliver a decisive answer. On the one hand, we broadly share Winsberg's (2021) view that opposing research *in general* rests on an overextended reading of moral hazard and that the expected benefits of supporting public research into solar geoengineering outweighs the expected costs and harms. On the other hand, such reasoning is unlikely to sway those already convinced that the very act of research constitutes a moral hazard or those pessimistic about the prospects of solar geoengineering governance (Solar Geoengineering Non-Use Agreement, n.d.; Clark, 2023). Alternatively, attempting to frame solar geoengineering as a case of mission-driven urgent science or "moonshot research" (e.g., see Shaw, 2022), while interesting, will not persuade anyone not already sympathetic to the urgency of SAI or to the mission itself. (See section 5 below for a fuller discussion).

Therefore, we focus on the 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. Correcting this misunderstanding requires attending to scientific practice.

To assess pursuitworthiness in this setting, we look at how climate scientists themselves talk about prioritizing field experiments. Specifically, we build on Visoni 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, transparency, and engagement. These heuristics are informed by ongoing conversations within the solar geoengineering research community, including perspectives from those directly involved 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. Moreover, the heuristics involve inevitable tradeoffs about which reasonable people can disagree (e.g. an experiment determined to be useful may be perceived to be environmentally unsafe).

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 return to the question of whether solar geoengineering research *in general* is pursuitworthy. 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 (SO₂)—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. That is, in order to understand why scientists want to conduct small-scale geoengineering field experiments, we need to know some details about climate modeling.

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 parameter-

ized, i.e., ESMs implement idealizations or mini-models which represent the effect of small-scale processes “at the grid scale of the model” (Gettelman & Rood, 2016, p. 46). Parameterizations come in varying degrees of complexity and often have empirical support or are derived from theory (Lloyd, 2015; O’Loughlin & 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 CO₂ 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 & MacMartin, 2020, p. 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. 2023a, b).

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).

Short of deployment, climate models are the only way for scientists to learn how SAI would affect Earth’s climate. To improve the models and thereby give scientists *better* projections of what SAI’s effects might be, some small-scale field experiments have been proposed.

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 various solar geoengineering strategies (see <https://srm360.org/outdoor-experiments/>). However, given that we are focusing on SAI, 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.¹ 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. Crucially, while some model improvements related to SAI research can be achieved through observations and improvements in theory, others will only be achievable via experiments (e.g., see the discussion of the hypothetical alumina experiment in section 4.2 below).

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 confi-

¹The knowledge gaps in these reviews may also help inform how a “risk register” would be populated (see section 4.1 below).

dence, that the approaches being tested can affect the parameters under investigation” (Symes, 2024, p. 6).²

It is crucial to understand that most large-scale climatic uncertainties can’t be solved or reduced with small-scale experiments. So, these experiments must be understood—and, indeed, only make sense—as part of an iterative process of climate model improvement.³ 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

² To see just how small these experiments are compared to other activities that aren’t scrutinized, consider Visioni and Gruener’s (2025) description of a relatively large small-scale experiment: “Phase one would involve releasing approximately ten tonnes of sulfur dioxide (*for comparison, South Africa’s Matimba coal power plant emits over 270,000 tonnes per year, or around 750 tonnes a day*) in one location, one kilometer or more above the tropopause, and carefully measuring what happens to it with aircraft and ground-based instruments. This amount would be far too small to affect climate, but this would allow researchers to study how aerosols form, disperse and persist at this altitude in real-world conditions” (emphasis added).

³ By “iterative”, we mean that the models can be improved with information from experiments and then used to produce new simulations. The simulations can reveal new uncertainties which in turn can inform the need for new experiments. And so on.

the technology somebody else (think Musk, Trump or Putin) might use for a large-scale deployment.

While these concerns may be 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. Here, the language of “field trials” seems to suggest that a miniature version of SAI is being funded so we can examine its effects on the local climate (or some such) when that is patently not the case. 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.

One complication that arises concerns the distinction between scientific experiments on the one hand and feasibility (or technology) tests on the other. That is, 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 climate model uncertainties, but it does inform the feasibility of a specific simulated strategy. The distinction is like the difference between asking “what effects would SAI have?” vs. “do we have sufficient technology and know-how to practically deploy SAI?” 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 conducive 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).⁴ 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 providing a reliable scientific backing to inform future decisions about deploying or not deploying solar geoengineering.

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

⁴ However, one may still worry about “cognitive lock-in” (Cairns, 2014) and other types of slippery slopes (Tang, 2023).

activities do not satisfy 4 of the 5 pursuitworthiness criteria we outline in section 4.2 below. Make Sunsets' interventions are neither scientifically useful nor scientifically rigorous, the decision to release each balloon (and where) are not the result of a community-based decision, and there is little to no transparency).

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 & 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).

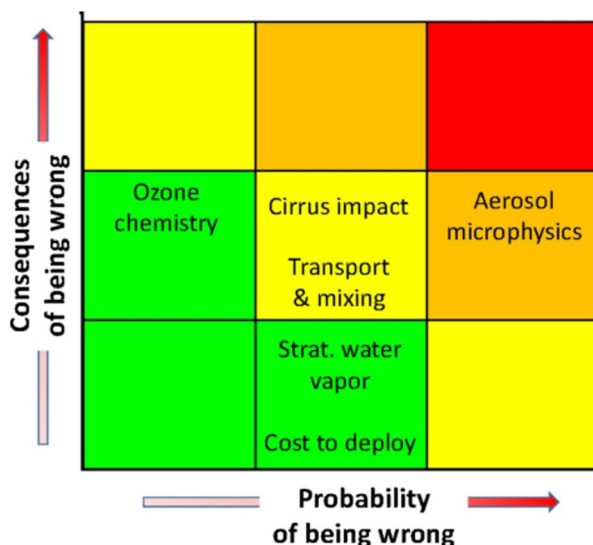
4.1 Risk register

With this basis, let's discuss how one might determine the pursuitworthiness of a given field experiment. We begin here with the risk-register approach as a useful starting point, but—as we show in section 4.2—it is only one part of one element within a broader set of heuristics.

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 & 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 (Fig. 1) 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

Fig. 1 From “Mission-driven research for stratospheric aerosol geoengineering” (MacMartin & 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 consider multiple potential materials (see section 4.2 below)



being wrong are also high (top right corner), is to be prioritized (according to the risk register) more than an aspect of SAI that is not uncertain, or whose consequences would not be high anyway. Note that “aspect of SAI” refers not to experiments or research but to SAI itself. The risk register tells us nothing about how risky or safe particular experiments or research avenues are – the risk and uncertainties are all about what would happen if we start deploying SAI based on current knowledge.

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. In plain language, this means that if we were to deploy SAI now using the projections given by climate models, the models may get key details of aerosol behavior wrong (uncertainty is high) and the consequences would likely be bad. 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 & 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 (cf. Kravitz & MacMartin, 2020).

Populating such a register is a substantial endeavor, but is somewhat already underway in the field: together with some academic literature trying to categorize

solar geoengineering uncertainties (Eastham et al., 2025; Haywood et al., 2025) within large consortia of scientists, there is also an ongoing effort to build a bottom-up database of SAI uncertainties that can include iterative feedback from scientists about specific ones, and that includes, amongst others, similar axes as those outlined above (e.g., Degree of uncertainty/Degree of consequence).⁵ Furthermore, in other fields of science, expert elicitation activities have been used to better define and quantify specific uncertainties (Buma et al., 2025); this methodology could potentially be applied to the risk register to provide more legitimacy and robustness to it, making sure the register could serve as a representational tool to help determine the pursuit-worthiness of a field experiment.

As one reviewer of our manuscript pointed out, the risk register merely represents research priorities based on available knowledge and does not by itself serve as an evaluative tool for pursuitworthiness. Indeed, the very use of the risk register involves a normative assumption that research into high-uncertainty, high-risk aspects of SAI should be pursued. This is a conditional claim: given that SAI research is already underway, the register represents how the research community currently structures the scientific uncertainties that matter for modeling and for future SAI-related decisions. Our aim is to evaluate how particular experiments should be judged within that context. The justification for the normative assumption is straightforward but not unassailable. Scientists want to eliminate uncertainties associated with the most detrimental negative consequences as early as they can, and the model-based research framework (recall from section 2 above that, barring actual SAI deployment, the largescale effects of SAI can only be learned about via climate models) informs how such uncertainties should be ranked. One could object by saying that easy-to-resolve uncertainties should be prioritized or that apparent ease of resolving the uncertainty should serve as an additional axis in the register. We accommodate this objection in our “usefulness” criterion below.

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 determining pursuitworthiness. In the next section, we situate it within a broader and more complete evaluative framework.

⁵ <https://uncertainties.reflective.org/>.

4.2 Heuristics to Judge Pursuitworthiness

Let's think through three potentially proposed⁶ 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. It will quickly become clear that the risk register and its underlying normative assumption are insufficient for determining pursuitworthiness. The following evaluations are illustrative and are intended to show how the risk register might be applied in practice and to surface additional considerations relevant to 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 & 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 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 bright-

⁶ 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).

ening 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 register decided beforehand could have helped with its public acceptability.

- **(top left corner)** a field experiment that would release 1000 kg of SO₂ 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 SO₂ 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 (again, hypothetically) 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 likely 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 1 °C. 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 not sufficient 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 Visoni et al. (2024), criteria which involve safety, usefulness, transparency, and engagement.

Therefore, we propose that five 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.⁷ 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. 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. Experimental proposals should undergo pre-registration (their aims, rationale and methodology should be accessible beforehand) and pre-review (some form of independent assessment of the scientific validity should be provided). There are other fields in which pre-registration and pre-review are standard practice (see Nosek et al. (2018), especially in fields in which credibility and public trust are a broader issue than just the robustness of the underlying science (as an example, neuroscience, see Chambers et al., 2014). Potentially, an open peer-review of the experiment can help highlight gaps or oversights in a proposed experiment. A closer example is perhaps the way in which NASA selects very expensive satellite-based observational missions, in which concept studies go through various peer-review stages, and only after a few years is the final selection made (<https://www.nasa.gov/new-s-release/new-proposals-to-help-nasa-advance-knowledge-of-our-changing-climate/>) and feedback is integrated throughout.
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 now 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 & Watts, 2008), which help assess safety, may take on added importance in the context of SAI research.
3. **Usefulness.** The experiment must be deemed sufficiently scientifically useful. Here, two broad considerations are relevant. Evaluating a proposed experiment for usefulness should involve:

⁷ In our view, privately funded small-scale experiments whose planning and results are not subject to peer review are not pursuitworthy.

- (a) justifying the experiment in terms of the risk register, identifying the uncertainty the experiment purports to address, and 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 must be sufficiently transparent, including funding sources, data produced, and publications or reports. 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).⁸
5. **Engagement.** The experiment's planning needs to involve sufficient engagement with the relevant communities. 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 or other climate harms; the community's views in turn may inform what new model simulations need to be prioritized, e.g., at a regional scale). Note that engagement itself should also be transparent in the above-described sense. What is a "relevant" community would be, of course, up to debate, and can be considered in more or less expansive ways, from simply the scientific community to that plus the local communities that would be affected, to a broader meaning including vulnerable groups that might particularly be affected were solar geoengineering deployed – or not (Taiwo & Talati, 2021; Sovacool et al., 2024).

In general, field experiments that are determined to be sufficiently scientifically rigorous, safe, useful, transparent, and engaged 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 pre-determined thresholds on all five criteria, but here we are outlining these criteria with the idea of judging their overall pursuitworthiness individually. Critiques of propos-

⁸ However, some have raised a worry that transparency can "bite back" (Thi Nguyen, 2022), e.g., that scientists' expertise may be undermined if they are required to explain *all* aspects of their decisions to the general public when some such aspects may not be easily explicable to the general public.

als should speak directly to our criteria and should be precise, e.g., by being contrastive (see DiMarco & Khalifa, 2022). There may also be a worry that, because of the fuzzy boundary between experiments and feasibility tests (see section 3 above), the “usefulness” criterion could lead us astray. That is, it’s possible that a field experiment would be determined to be overwhelmingly pursuitworthy based on our criteria but that the field experiment also ends up producing data that is relevant to solving a feasibility or technical problem about SAI deployment. To help guard against this worry, such possibilities should be disclosed to relevant communities as required by our “engagement” and “transparency” criteria. Being highly useful does not, by itself, determine pursuitworthiness.

One may be tempted to impose a hierarchy wherein some of the more “purely epistemic” criteria (i.e., rigor and usefulness) are prioritized over others. However, we think imposing a hierarchy is ill-advised at the general level. One reason we think this is because the criteria are not independent of one another. For example, greater transparency can make it easier to assess whether an experiment is rigorous⁹, while meaningful engagement with diverse communities can bring to light new research questions and associated uncertainties, thereby shaping the experiment’s usefulness. A broader reason we oppose the hierarchy is that if the aim is for research to proceed over time, then neglecting considerations such as safety or engagement may jeopardize future inquiry and ultimately undermine epistemic gains. Pursuitworthiness concerns whether an experiment is *worth* pursuing, and part of that *worth* lies in sustaining a research trajectory capable of generating knowledge over the longer term, not merely in producing short-term epistemic returns. The broader institutional and social context therefore matters, and each of our heuristics plays a necessary role. For these reasons, we do not propose a fixed hierarchy among them.

Even without a formal hierarchy, 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; Visions et al., 2023; Táiwò & 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 ques-

⁹ A more transparent experiment may also be more likely to *be* more rigorous since more scientists and other experts could weigh in, thereby enhancing its interactive objectivity (Longino, 1990).

tions 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 five 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, better engaged, 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.¹⁰

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 & 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 Visoni 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.

An instructive real-world case that fits our framework well is the Woods Hole Oceanographic Institution's Locking Away Carbon in the Northeast Shelf and Slope (LOC-NESS) project, a small-scale ocean alkalinity enhancement field trial designed to quantify both the uptake of CO₂ following a controlled addition of sodium hydroxide and any associated biological, chemical, or physical responses. Although this experiment falls within carbon dioxide removal rather than solar geoengineering, it is similar enough for present purposes (i.e., it is controversial and is a type of geoengineering). First, the experiment was **rigorous**. The researchers submitted

¹⁰ A case in point here is the SCoPEx experiment (Jinnah et al. 2024). On the one hand, some have critiqued the experiment for being unclear in its goals and for only engaging with communities in a post-hoc way. On the other hand, some have suggested that the SCoPEx experiment was shut down partly because the researchers were *too* transparent and so next time they "won't be open in the same way," (David Keith quoted in the NY Times, Gelles, 2024).

their project proposal to the EPA and during the public comment period many scientists evaluated the rigor of the experimental design.¹¹ Moreover, the proposal for the experiment was informed by peer-reviewed research (Jiang et al., 2023; Guo et al., 2025). In the course of the experiment itself, the perturbation was tracked for days using ship-based measurements, CTD [Conductivity, Temperature, and Depth] casts, plankton and neuston net tows, autonomous vehicles, and drifters, producing a multi-platform data set designed to resolve physical transport, carbonate chemistry, and biological response. Second, while the researchers did not use a risk register, it is clear why the experiment was **useful**. Field data of this kind are currently the main source of uncertainty in model representations of ocean alkalinity enhancement, and independent researchers repeatedly emphasized the need for precisely this sort of trial in order to constrain projections of carbon uptake and ecological impact. Moreover, the researchers described how the experiment is situated in their broader project, i.e., it occurred after extensive analysis of already-available data and laboratory experiments, and its results are intended to inform coupled physical–biogeochemical ocean models used to evaluate the scalability and broader impacts of ocean alkalinity enhancement. Third, the experiment was deemed **safe**: the experiment proceeded under a U.S. Environmental Protection Agency permit that imposed explicit environmental thresholds and operational constraints, including real-time monitoring of seawater pH that required the flow rate to be reduced when the allowable limit was approached. The field trial also exemplifies **transparency** and **engagement**. Over the course of several years, the research team held numerous meetings with regional fishing communities, responded to pointed criticism, modified the experimental plan in light of stakeholder concerns (including relocating the release site and initiating additional ecological testing), and invited a compensated fishing-industry observer to witness the deployment.¹² All resulting data are made publicly available. Taken together, these features make LOC-NESS a case in which rigor, safety, usefulness, transparency, and engagement were clearly satisfied – and indeed, the experiment was carried out successfully.

5 The pursuitworthiness of solar geoengineering research more broadly

At this point a critic may object that we have only provided very *local* pursuitworthiness criteria that themselves presuppose the pursuitworthiness of solar geoengineering research overall. This objection is correct. We argue that pursuitworthiness in the solar geoengineering domain is primarily a local, purpose-relative notion: it concerns the evaluation of particular research steps within an ongoing program rather than the justification of the program as a whole. Our analysis in section 4 above is directed

¹¹ Some scientists’ testimonials can be found here: <https://locness.who.edu/about/scientific-testimonials/>. E.g., one scientist writes that “The rigorously designed experiments proposed by the LOC-NESS team are vital to understanding the feasibility of these approaches. The outcomes of this work will have global significance, the risks to local marine life will be minor and highly ephemeral.”

¹² The observer wrote up their thoughts on the whole experience here: <https://www.nationalfisherman.com/gulf-of-maine-sees-first-open-water-ocean-alkalinity-enhancement-field-trial-in-us>.

at practicing scientists who are already committed to investigating the uncertainties, risks, and potential impacts of proposed solar geoengineering methods but who have not yet reached agreement concerning the necessity or prioritization of field experiments. We are skeptical that anything new or fruitful can be said about whether solar geoengineering research is *in general* pursuitworthy. Below we survey two attempts, one inspired by Shaw's (2022) notions of "urgent science" and "moonshot research" and one following Winsberg's (2021) "modest defense of geoengineering research."

5.1 Urgency and moonshot 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 CO₂ emissions" (Crutzen, 2006, p. 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 Feyereabend'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 doubts about the feasibility of timely climate mitigation.¹³ If you think that timely climate mitigation is a lost cause you may think SAI research and eventual deployment is morally required (Horton & Keith, 2016). 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.

So, while the distinction between urgent science and luxury science could help determine pursuitworthiness in some research contexts, it currently appears to be of little use here.

¹³ Views on this possibility of timely climate mitigation vary considerably. E.g., compare the optimism of Our World in Data's Hannah Ritchie (see her *Not the End of the World* (2024, Ch. 3)) to the pessimism articulated by climate scientist and former NASA GISS director James Hansen (Hansen et al. 2025).

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.

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.¹⁴

Note, though, that the mere existence of a mission-driven research program does not imply broad acceptance of the mission. 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 technoscientific 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)

¹⁴Therefore, the missions of these different organizations reinforce the point made in sections 2 and 3 above that the solar geoengineering research effort is primarily model-driven (which, again, has consequences for how we should think about the pursuitworthiness of small-scale field experiments).

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).¹⁵ 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. Perhaps the best explanation for this mischaracterization is that Hulme simply disagrees with the *telos* in question. That is, Hulme is opposed to solar geoengineering research altogether.¹⁶

Coming from another direction, Morrow (2020) urges solar geoengineering research to become *more* of a mission-driven effort. Whereas Hulme critiques the moonshot ethos by saying it is 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 surfaces 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 above presuppose acceptance of the mission; they are not themselves arguments for adopting it.

Given these tensions, attempts to ground pursuitworthiness in urgency or framed as moonshot research seem inconclusive. Winsberg offers a different route.

5.2 Winsberg's defense of solar geoengineering research

Winsberg (2021) argues in defense of solar geoengineering research and his argument may be leveraged to defend the overall pursuitworthiness of the solar geoengineering research **Mission**. Of particular interest are two premises Winsberg considers:

Bias reduction Public support for ...SAI research will bring it out into the open and promote the creation of inter-coordination projects that will establish conventional standards which reduce the possibility of preference bias. This effect would more

¹⁵ 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>.

¹⁶ See the Solar Geoengineering Non-Use Agreement website and blog here: <https://www.solargeoeng.org/>.

than offset the extra legitimacy conferred on projects that did continue to be affected by preference bias.

Research builds norms Public support for ...SAI research will bring it out into the open and promote the creation of scientific communities that will establish shared ethical norms and best practices regarding what are acceptable experiments and implementations....

The “preference” bias that Winsberg refers to is the tendency for industry to make methodological choices (or fund researchers who make methodological choices) that accord with their values, such as Coca Cola funding researchers who investigate the link between lack of exercise and obesity (rather than, say, the link between sugar and obesity) (Wilholt, 2009; O’Connor & Freeborn, 2025). For SAI research, the worry could be that industry funded research will make SAI look more promising (or less risky) than it actually is or than it would otherwise appear if other (equally defensible but different) methodological choices had been made.

Winsberg’s basic point is that doing more research into solar geoengineering is more likely to yield net positive effects, provided that the research is publicly funded and that his premises of **Bias Reduction** and **Research Builds Norms** hold—premises for which he considers the evidence “quite strong.” If research can indeed reduce bias and build shared norms, then its pursuitworthiness depends less on the technology’s ultimate desirability and more on how inquiry is structured and governed (and, perhaps, incentivized, see (Currie, 2018). In this respect, Winsberg’s argument aligns nicely with Morrow’s call for a more mission-driven, coordinated research framework for solar geoengineering (see section 5.1 above).

Overall, we find Winsberg’s argument convincing.¹⁷ Strictly speaking, however, one could object by saying that we do not know how humanity’s response to climate change will play out and, under some plausible potential futures, it would have been better had SAI research not been pursued at all. It’s easy to imagine such counterfactuals. E.g., take the often-stated worry about moral hazards. In the solar geoengineering context, a moral hazard is the concern that merely researching & developing SAI as a potential option will weaken political will for emissions mitigation by creating the perception of an easier technological fallback. Some social science research presents evidence that moral hazard worries are unfounded, but other evidence pulls in the opposite direction (Merk et al., 2016; Raimi et al., 2019; Andrews et al., 2022). Given the stakes, we would want absolutely conclusive evidence to know for sure that the moral hazard worry is unfounded but social science can provide no such evidence.

¹⁷Note that Winsberg’s analysis is founded on a result from Good’s (1967) theorem which shows that acquiring cost-free evidence maximizes expected utility. Kostić (2023) objects that this need not hold for risk-averse agents whose preferences are represented by risk-weighted expected utility, since evidence can introduce a chance of misleadingly favoring a high-downside option. However, this objection assumes that agents can avoid entering risky informational states by declining to gather evidence. In the present context this assumption is false. Climate models are continuously improved for independent scientific and policy purposes, and the physical climate system itself is generating new evidence about the consequences of mitigation failure. These developments alter both the probability distribution over states and the payoff structure, regardless of whether dedicated SAI research is pursued.

If the worry about moral hazards *does* turn out to be well-founded and SAI research completely undermines climate mitigation and thereby leads to large net negative effects, then field experiments and indeed all of solar geoengineering research will turn out to seem very much NOT to have been pursuitworthy from the perspective of future people. This possibility is both very real and completely unhelpful for practical purposes (i.e., given that solar geoengineering research is ongoing and decisions have to be made concerning the permissibility of field experiments). We further note that second-guessing what research is useful based on potential future knowledge or shifting situations is difficult, and can lead to precluding meaningful scientific advances that might turn out to be useful: the example of mRNA research, that was highly unpopular and often rejected, now having been awarded a Nobel prize, comes to mind (Bansal, 2023).

Therefore, while we believe that solar geoengineering research overall is pursuit-worthy and that moral hazard worries may be overstated, one can always find reasons to disagree. Instead of mounting a defense by offering a general account of pursuit-worthiness we have decided to instead zoom in on the research context where solar geoengineering research is already being conducted.

6 Conclusion

In this paper, we proposed a set of heuristics—scientific rigor, safety, usefulness, engagement 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 we have seen, attempts to characterize solar geoengineering research as urgent or moonshot research, while interesting, do not help to establish general pursuitworthiness due to disagreements about the urgency or the very pursuitworthiness of the mission. In contrast, Winsberg's (2021) arguments concerning how research governance can reduce bias and build stable scientific norms seem more promising. Perhaps they may be further developed to alleviate the worry about moral hazards and violations of norms in times of urgent science. Overall, we have provided a set of heuristic criteria, situated within scientific practice where solar geoengineering research is ongoing, that we believe can help determine which small-scale solar geoengineering field experiments are worth undertaking and why.

However, important questions remain. One concerns whether perceived urgency surrounding SAI might someday justify relaxing the standards embedded in our heuristics. What counts as “useful” would presumably shift under extreme time pressure. If, for example, a climate tipping point were imminent and there was no time to refine models or coordinate simulations, many experiments currently regarded as valuable

would lose their practical relevance.¹⁸ A risk register is of little help if every item on it requires more time to address than scientists actually have.

More troubling is whether urgency might be invoked to relax standards of rigor, safety, or engagement. A parallel can be seen in the early COVID-19 pandemic, when some research initiatives bypassed or minimized ethics review in the name of speed, leaving questions of consent, oversight, and public accountability unsettled. As one case study documents, a door-to-door symptom-survey was launched without submitting a protocol to the national ethics committee, despite the potential for involuntary reporting, loss of privacy, and military presence during data collection (see Voo & Mastroleo, 2024, especially case 4.3). The survey team viewed ethics oversight as an obstacle to timely information gathering, but the absence of review meant that informed consent procedures, participant protections, and the boundaries between research, surveillance, and state enforcement were left unclear. The result was a situation in which urgent public health aims eclipsed safeguards ordinarily seen as non-negotiable.

Let's assume that in the SAI case too, in a situation of urgency, some standards might be relaxed.¹⁹ 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. This suggestion aligns well with Winsberg's **Research Build Norms** premise). 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 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). 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 lay-people, 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.

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¹⁸In an extremely time-limited case, perhaps machine learning (ML) could be of use (de Burgh-Day & Leeuwenburg, 2023), however there are severe limitations to using ML to project novel climates (e.g., see Li, 2023).

¹⁹The picture becomes even more complicated when the private sector becomes involved. E.g., there are early examples of startups doing some form of small-scale SAI and justifying it through the "urgency" framework; scientists have argued (Keith & Visoni, 2025) that their main risk is the potential to lose public trust in solar geoengineering.

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Declarations

Competing interests None.

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