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Toward a Responsible Solar Geoengineering Research Program

The seriousness of the risks posed by climate change demands that we examine all possible means of response, but how we do so makes all the difference.

A combination of greenhouse-gas emission cuts and solar geoengineering could keep temperatures under the 1.5-degree aspirational target of the recent Paris Agreement. It could make the world cooler at the century's end than it is today. Emissions cuts alone cannot achieve either objective with high confidence. The climate's inertia along with uncertain feedbacks such as loss of permafrost carbon mean there is a small but significant chance that the world will continue to warm for more than a century after emissions stop.

Solar geoengineering is the process by which humans might deliberately reduce the effect of heat-trapping greenhouse gases, particularly carbon dioxide, by reflecting a small fraction of sunlight back to space. The most plausible solar geoengineering technology appears to be the addition of aerosols (fine droplets or powder) to the stratosphere, where they would scatter some sunlight back to space, thus cooling the planet by reducing the amount of heat that enters the atmosphere.

Solar geoengineering could also limit global warming's predicted side effects such as sea level rise and changes in precipitation and other weather patterns. Because these changes would have their most powerful impact on the world's most vulnerable people, who lack the resources to move or adapt, one can make a strong ethical case for research to explore the technology.

Climate risks such as warming, extreme storms, and rising seas increase with cumulative emissions of carbon dioxide. Solar geoengineering may temporarily reduce such climate risks, but no matter how

well it works it cannot eliminate all the risk arising from the growing burden of long-lived greenhouse gases. We can draw three important conclusions from these two facts. First, net emissions must eventually be reduced to zero to limit climate risk. Second, eliminating emissions does not eliminate climate risks, because it does nothing to address emissions already in the atmosphere. Third, the combination of solar geoengineering and emissions cuts may limit risks in ways that cannot be achieved by emissions cuts alone.

Solar geoengineering is not a substitute for cutting emissions. It is—at best—a supplement. We can't keep using the atmosphere as a free waste dump for carbon and expect to have a safe climate no matter what we do to reflect away some sunlight.

The potential benefits of solar geoengineering warrant a large-scale international research effort. Economists have estimated that global climate change could result in worldwide economic damage of more than a trillion dollars per year later this century. A geoengineering project large enough to cut the economic damage in half could be implemented at a cost of a few billion dollars per year, several hundred times less than the economic damage it would prevent. Furthermore, a modest research effort can yield rapid progress because the technological development of solar geoengineering would be largely an exercise in the application of existing tools from aerosol science, atmospheric science, climate research, and applied aerospace engineering. Of course, any exploration of geoengineering would also have to consider how its deployment would be governed, and governance research can build on decades of climate policy work across fields as diverse as economics, international law, environmental ethics, and risk perception.

Yet despite this promise, there is very little organized research on the topic, and there is no US government research program.

For much of the past few decades the topic has been taboo in climate research and policy. This is surprising when one considers its history. What is now called solar geoengineering was—remarkably—in the very first report on climate change to a US president, which reached President Lyndon Johnson's desk in 1965. Over the next few decades the topic was covered in major reports on climate change, including the US National Research Council (NRC) reports of 1977, 1983, and 1992. But as climate change reached the top of the environmental agenda with the 1992 Rio Framework Convention and the formation of the Intergovernmental Panel on Climate

Change, discussion of solar geoengineering went quiet.

However, there are now signs of rapid change in the politics of solar geoengineering. Perhaps most important, Janos Pasztor, who served as senior climate adviser to UN Secretary General Ban Ki-Moon, is now leading the Carnegie Climate Geoengineering Governance Initiative, a major effort to develop international governance for climate engineering. There are also modest research programs in Europe and China. Over the past few years, environmental advocacy groups such as the Environmental Defense Fund and the Natural Resources Defense Council have released formal statements supporting research. In 2015, the NRC released a report on geoengineering, recommending a broad research effort. Most recently, in January 2017, President Obama's US Global Change Research Program (USGCRP), a coordinating body administered by the White House's Office of Science and Technology Policy, explicitly recommended research.

With these changes in the political environment, the time is right for launching a substantial international research program on solar geoengineering, one that has strong norms of transparency and open access and that embeds solar geoengineering inside global debates about climate governance. How might a responsible US government research program be constructed as part of this effort? Does the arrival of the Trump administration change the picture?

Reasons for reluctance

Before examining how a research program might be established, much can be learned from reviewing the deep concerns that have held back previous efforts: uncertainty, slippery slope, messing with nature, governability, and moral hazard.

Uncertainty. The central purpose of research is to reduce uncertainty; so although there is much that we don't know about solar geoengineering, that cannot stand alone as an argument against research. A related argument is that because of the uncertainty inherent in predictions about the climate's response to solar geoengineering, it cannot be meaningfully tested short of full-scale deployment. But this argument fails to address the uncertainty on both sides of a hypothetical decision to (gradually) deploy solar geoengineering.

The term of art for the human force driving climate change is radiative forcing (RF), the heat energy added to the atmosphere measured in watts per square meter (W/m^2). Accumulated greenhouse

gases produce a positive radiative forcing, whereas solar geoengineering produces a negative forcing. These RFs can be known with some confidence. The deep uncertainties lie in predicting the climate's response to the forcing; the higher it is, the more uncertain and dangerous things become, but we don't know exactly how the climate dice will roll.

As a concrete example, assume that emissions can be cut to near zero soon after mid-century, yielding a peak RF of 4 W/m^2 in 2075. Suppose deployment of solar geoengineering starts in 2030 and is gradually increased to produce an RF of -1 W/m^2 in 2075 and then slowly reduced thereafter as greenhouse gas concentrations decline. The central question is, which version of 2075 is more dangerous? A world with 4 W/m^2 of RF from greenhouse gases or a world with a net RF of 3 W/m^2 but with additional risks from solar geoengineering. No one knows. Uncertainty is baked into either path. But a reasonable first guess is that because RF is the central driver of climate change, there is less climate risk in the world with less RF. Uncertainty is real, but it speaks louder as an argument for rather than against research.

Slippery slope. If the slope from research to deployment is slippery because research reveals that solar geoengineering works better and with less risk than we now expect, that slipperiness is not itself an argument against research. The basis for concern about slippery slopes is the socio-technical lock-in that arises when technologies coevolve interdependencies with other technologies and when they develop political constituencies that encourage continued use even against the public interest. This is a legitimate concern for many technologies, from cars to Facebook. All else equal, socio-technical lock-in is a particular concern for technologies that drive profitable businesses or that require structural changes that are difficult to abandon.

Solar geoengineering seems unlikely to generate strong lock-in because it appears to have very low direct cost, a fact that poses deep challenges for governance but likely reduces the chance of a substantial concentration of economic power. If it's true, as some estimates suggest, that the direct cost of large-scale implementation would be a few billion dollars per year, then it will be hard to develop concentrated economic power. Moreover, there is no obvious way to collect a fee for the reduction in climate risk because the benefits to solar geoengineering are, in a useful piece of economic jargon, "non-excludable." It therefore seems likely that the deployment would be struc-

tured as some form of fee-for-service contract to governments. Moreover, because the technology is primarily an application of existing aerosol science and aerospace engineering with little apparent basis for strong intellectual property claims, governments could likely procure deployment services from many vendors. This combination of factors suggests that concerns about socio-technical lock-in seem relatively small.

Messing with nature. In *The End of Nature*, Bill McKibben argues that carbon dioxide-driven climate change makes climate itself a human artifact rather than a natural process and thereby eliminates the capital "N" of Nature. Use of solar geoengineering would indeed cement the climate's status as a deliberate product of political decisions. Yet the move by humanity to take deliberate responsibility for managing the climate should, I think, be seen a progressive step, a step beyond the sharp dichotomy between "Civilization" and "Nature" (itself a particularly North American nineteenth-century view) and toward a stance of deliberate responsibility. Disagreement about the environmental ethics of solar geoengineering arise, in part, from two alternate views of the same action. If solar geoengineering reduces the climate and ecological effects of accumulated carbon dioxide, is its implementation a step toward cleaning up our mess in a process of ecological management at planetary scale? Or is it yet another step toward the subjugation of nature for human ends? Which analogy fits best: reintroducing wolves to Yellowstone? Restoration of the Florida Everglades in arguably the world's largest and most costly environmental engineering effort? Reviving woolly mammoths? Or is geoengineering more akin to indoor ski slopes in Dubai, the creation of artificial environments to suit human whims?

The combination of emissions cuts, solar geoengineering, and negative emissions gives humanity the ability to (roughly) restore preindustrial climate. Such deliberate restorative planetary management would take centuries, but I see it as a worthy organizing goal for environmental advocacy—a goal that cannot be achieved by emissions cuts alone, even an immediate elimination of emissions.

Governability. How is it possible to govern a technology for which unilateral action is easy? One for which costs and benefits are uncertain and globally distributed? One for which it's hard to confidently attribute a specific impact such as a hurricane or drought to the intervention? How can governance of geoengineering accord with the ideal that decision makers consult with people whose lives

will be affected? Some theorists have concluded that no governance system can meet all the criteria. A fair argument, but a claim that perfect governance is impossible does not amount to a proof that no system of governance is practical. The empirical challenge for these claims is that the arguments apply with equal or greater force to technologies for which there is, in practice, some level of functioning, though imperfect, global governance.

Suppose a technology were invented that allowed significant control over the economy to be exercised rapidly by a single small committee in each major country. Suppose further that the impacts of these committee decisions were unpredictable and that there were strong nonlinearities that interconnected the effects of committee decisions in different countries, so that a single decision by the US committee could throw people out of work in Bolivia. This technology exists in the monetary policy of central banks. Yet despite the challenges, there is some governance of global central banking, and perhaps evidence that bankers are better at managing business cycles than they were in the first half of the twentieth century. Similar arguments could be made about the Internet, infectious diseases, or global air traffic control. All of these involve high-consequence systems with significant uncertainty. In no case is management perfect, but it is by no means obvious that these examples are inherently more difficult than governing solar geoengineering. Indeed, if it is true, as current research suggests, that the benefits and risks of solar geoengineering would be distributed relatively evenly around the globe, then governance of solar geoengineering may be easier than for other high-consequence global technologies. Moreover, if solar geoengineering is a form of public good, albeit one with a higher level of uncertainty and risk than many, it's a public good for which the political challenge of agreeing about who pays is relatively small because the costs are relatively small.

Moral hazard. Perhaps the most salient concern is that by making geoengineering seem more plausible, an active research program in this area will weaken efforts to control emissions. The fear is that opponents of climate action will make exaggerated claims about the effectiveness of solar geoengineering, using them as a rhetorical tool to oppose emissions

cuts. Although there is little evidence of this today, I share this fear. Indeed, writing in 2000, I may have been the first to highlight this dynamic as the moral hazard of geoengineering.

Political conflict over climate policy is long-standing. It's reasonable to expect that all available arguments will be deployed in political battle. In evaluating concern about geoengineering's moral hazard the question is not whether the technology will be over-hyped to argue against climate action? But rather, how much might this argument alter the political balance of power? There are reasons to suspect the impact may be small. The power of the environmental advocacy forces that fight for climate action will not evaporate if research makes solar geoengineering more visible any more than the power of the fossil-fuel lobby evaporated in the face of record temperatures.

The impact of geoengineering as a rhetorical tool against climate action may be smaller than feared because it can serve both sides of the climate policy battle. The very existence of solar geoengineering, along with its uncertainties and risks, can serve as a powerful argument in favor of accelerated action on emissions. The effectiveness of these arguments will depend on how knowledge of solar geoengineering alters people's perception of climate risks. The common assumption is that concern for climate risk as measured by an individual's willingness to pay for emissions cuts will be reduced. But learning about solar geoengineering may increase the salience of climate risks and thereby increase one's commitment to reduce emissions. One might imagine two extreme reactions to solar geoengineering: Great! A technofix! Now I can buy a big truck and ignore the environmental extremists. Or, conversely: Damn! If scientists want to spray sulfuric acid in the stratosphere as a last-ditch protection from heat waves, then climate change is scarier than I thought. I should pony up and pay more for an electric car. We cannot know yet which response would prevail, but experimental social scientists have begun to explore public reaction to solar geoengineering, and results from all experiments to date suggest that the latter reaction dominates: information about solar geoengineering increases willingness to pay for emission mitigation.

The worst case would be if the Trump administration vigorously funded research on solar geoengineering, promoting it as a substitute for emissions cuts. In that case, the solar geoengineering research community's best option might be to refuse funds and adopt a stance of active resistance.

Each of the concerns described above has merit. One must weight them, however, against the evidence that solar geoengineering could avert harm to some of the world's most vulnerable people. These concerns do suggest some specific ways in which research programs might be managed to minimize risks; they do not, individually or collectively, amount to a strong argument against research.

Program design

Guidance for establishing a US research program on solar geoengineering is available from the 2017 US Global Change Research Program report, the 2015 National Research Council report *Climate Intervention: Reflecting Sunlight to Cool Earth*, and a 2011 report from the Bipartisan Policy Center's Task Force on Climate Remediation Research, to cite a few prominent examples. I do not attempt to describe a research program here, but rather suggest a few crosscutting principles that might be useful in developing such a program in the United States or elsewhere.

Separate solar geoengineering from carbon removal. The term *geoengineering* is often used to describe both solar geoengineering and a group of conceptual or emerging technologies that remove carbon dioxide from the atmosphere. Solar geoengineering and carbon removal both provide a means to manage climate risks; both deserve a much greater research effort; and both are inadequately considered in climate policy analysis and debates. They are, however, wholly distinct with respect to the science and technology required to develop, test, and deploy them; their costs and environmental risks; and the challenges they pose for public policy

and governance.

The sharp distinction between the two is evident in the distribution of risks and benefits. The most plausible means of solar geoengineering appear to have global risks and benefits while having minimal risk and low direct cost at the point at which they are deployed (for example, the airfield from which aircraft distributing aerosols might fly). In sharp contrast, carbon removal offers the globally distributed benefit of reduced atmospheric carbon burden but with comparatively high localized costs, and some methods pose significant environmental and social risks at the point of deployment. Ocean iron fertilization is the one form of carbon removal that shares many of the characteristics of solar geoengineering, and its prominence in early analysis explains much of the desire to lump solar geoengineering and carbon removal, but hopes for that technology have faded so that it is no longer prominent in discussions.

Of course, decisions about the use of solar geoengineering or carbon removal make sense only in the context of a larger analysis and debate about climate policy that includes mitigation and adaptation. Yet because these technologies have little in common, decision makers have a better chance to craft sensible policy for each if research into their effectiveness and risks is managed separately. If there is a case for unifying research and development programs, it makes more sense to combine carbon removal with emissions mitigation than to combine carbon removal with solar geoengineering.

Focus on risks. Research should be concentrated on finding ways that solar geoengineering could fail. Unknowns are perhaps the largest concern, so we need a diverse exploratory research effort to examine a wide range of low-probability, high-consequence risks or failure modes. This research effort needs to be coupled to the emerging results of the systems engineering effort (discussed next) so that effort is directed at identifying problems with methods that seem most likely to be deployed. Otherwise, effort may be wasted finding problems with solar geoengineering technologies or deployment scenarios that are so ineffective or ill-considered that they are unlikely to be deployed.

As well as searching for physical risks or limitations of good-faith deployment scenarios, research

should examine plausible scenarios for normal accidents and for malicious use. These sharply divergent assumptions are all relevant, but they need to be clearly distinguished. Too much current analysis focuses on mushy middle-ground scenarios that are too suboptimal to be credible as good-faith scenarios yet do not illuminate the potential for malicious use.

Include engineering. Solar geoengineering cannot be seriously evaluated if it exists only as a grab bag of academic papers. Research examining technical and social risks requires coherent scenarios for deployment that are specified in sufficient detail to enable critical analysis. Developing such scenarios requires work that straddles the engineering science boundary and is guided by a systems engineering approach. Such an approach should start with a clear set of goals, such as reducing specific climate risks, while minimizing risks of deployment and minimizing the regional variation of climate response. Scenarios for deployment need to specify the material to be deployed, the means of deployment, and the means of monitoring the climate and ecosystem response, as well as the means of adjusting the deployment to achieve some specified goal, such as limiting the increase in precipitation or keeping global average temperatures under some threshold. Such physical and operational scenarios can serve as a basis for critical examination of physical risk and efficacy as well as for analysis of governance, including mechanisms for managing liability and for enabling collective decisions about deployment.

The 2015 NRC report recommended that geoengineering research be limited to “dual use” research that has a clear relevance to both general climate geosciences and to geoengineering. Although research on solar geoengineering must be embedded in a vigorous portfolio of atmospheric and climate-related geoscience, an overarching dual-use standard would be a poor guide for a geoengineering research program, for two reasons.

First, this criterion would rule out the kind of systems engineering analysis required to define deployment scenarios. Yet without such scenarios, analysis of geoengineering may focus on implausible scenarios, such as the all too common assumption that stratospheric sulfate aerosols would be used to offset all anthropogenic warming.

Second, there is a strong anti-correlation between research most likely to yield interesting climate science and that mostly likely to be relevant to solar geoengineering. One of the interesting new ideas for solar geoengineering is the possibility of thinning high cirrus clouds by seeding to reduce their

tendency to trap the Earth’s infrared heat. Cirrus thinning works directly to counter the heat-trapping effects of greenhouse gases, so it might work better than the older idea of reflecting sunlight with stratospheric aerosols. But the technique is highly uncertain. There are many meteorological conditions under which cirrus seeding will be ineffective or even counterproductive. Field experiments with cirrus seeding could improve understanding of its potential as a geoengineering measure while also improving understanding of these clouds in ways that might be used to improve the accuracy of climate and weather models. In contrast, there is no reasonable scientific doubt that if aerosols such as sulfuric acid or calcium carbonate could be successfully introduced to the stratosphere, they would scatter sunlight back to space with a cooling tendency. Because the scattering from stratospheric aerosols is relatively well understood, there is less chance that research would yield interesting spin-offs for climate science than there would be for research on cirrus thinning.

If the dual-use recommendation of the 2015 NRC study were adopted, research might be directed away from methods with low technical risk that would be the natural focus of a systems-engineering driven program, and instead directed to methods with deeper scientific uncertainty. Some consideration of dual use makes sense, and study of cirrus thinning is certainly justified, but a science-first dual-use criterion should not drive the whole research program. Geoengineering research requires an engineering core.

Avoid centralization by encouraging research diversity. The biggest risk of solar geoengineering research is overconfidence. Research programs with strong central management tend to produce a single answer while downplaying facts that don’t fit the core narrative. This tendency can be combatted by distributing research across distinct clusters with substantial independence. It is simply not possible to work creatively to develop a technology while at the same time thinking critically about all the ways it could fail. The ideal structure would be a red-team/blue-team approach in which some groups work to develop systems engineering approaches and best-case deployment scenarios while most of the effort is spent in independent research clusters searching for risks and failure modes.

Political realities

Development of a successful and sustainable research effort on solar geoengineering is a political challenge. If solar geoengineering research is part of a coherent

climate policy agenda that includes vigorous support for climate science, increases efforts to cut emissions, helps the most vulnerable to adapt, develops negative emission technologies, and renews a commitment to growing international governance, then based on the political evolution of this topic over the past half-decade there is a good chance that such a research program could have sustained support from major environmental organizations, lawmakers concerned with climate, and the public. These are big “ifs” under any circumstances, and doubly so now.

The Trump administration may make deep cuts to climate science or gut policies for climate action, or both. If so, these actions will be challenged in court, but at the very least they will introduce major uncertainty.

Consider two scenarios. Under a pessimistic scenario, a Trump administration might gut climate and related geoscience research, eliminate the USGCRP, make deep cuts to Department of Energy renewable energy programs, kill the Clean Power Plan, eliminate the federal renewable production tax credit, and withdraw from the Paris Agreement. It would be counterproductive to establish a formal federal solar geoengineering research program under this scenario because the likely result would be that forces lobbying for climate action would single out and attack research on solar geoengineering, labeling it as an excuse for inaction. This could fracture the delicate political coalition that now supports research, making it harder to sustain an effective research program even after the Trump administration is replaced by some future administration committed to climate action.

Some US-based research could continue under this pessimistic scenario, but it would be best to avoid strong links to the administration. Individual program managers in agencies such as the National Science Foundation could fund individual projects, in accord with the USGCRP recommendations. Research could also be funded by private philanthropies, particularly those with a strong track record of funding climate research and advocacy. Last, under this scenario, it would be appropriate to redouble a focus on international engagement.

The worst case would be if, under this pessimistic scenario, the Trump administration vigorously funded research on solar geoengineering, promoting it as a substitute for emissions cuts. In that case, the solar geoengineering research community's best option might be to refuse funds and adopt a stance of active resistance.

Under an optimistic scenario, the Trump administration might appease its political base with pro-coal, anti-climate messages but would maintain the federal climate science research portfolio, even if under a different name. It would also pursue some actions on emissions mitigation. It might still gut the Clean Power Plan while increasing support for low-carbon power under the guise of a stimulus-driven push for manufacturing of renewables and electric vehicles. Under this scenario, rhetoric would be more nationalistic. There would be much less talk about climate, yet the overall effect on emissions might differ little from the current path. Under this scenario, it would be appropriate to begin development of a modest solar geoengineering research program along the lines suggested by the USGCRP report, though it would be best if the effort was decentralized and decoupled from direct high-level connections to the administration.

Two guiding principles apply in either case: First, solar geoengineering research should be embedded in a broader climate research portfolio on mitigation and adaptation action. Second, physical science and engineering research should be linked to governance and policy work. Only an integrated research program can hope to achieve the multiple objectives instrumental to making the science, policy, and politics of solar geoengineering work.

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Recommended reading

Oliver Morton, *The Planet Remade: How Geoengineering Could Change the World* (Princeton, NJ: Princeton University Press, 2015).

David Keith, *A Case for Climate Engineering* (Cambridge, MA: MIT Press, 2013).

Gernot Wagner and Martin L. Weitzman, *Climate Shock* (Princeton, NJ: Princeton University Press, 2015).

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Mike Hulme, *Can Science Fix Climate Change: A Case Against Climate Engineering* (Cambridge, MA: Polity Press, 2014).